GREAT THINKERS IN ECONOMICS
Series Editor: A. P. Thirlwall

MILTON FRIEDMAN

James Forder

Great Thinkers in Economics

Series Editor
A. P. Thirlwall
School of Economics
University of Kent
Canterbury, UK

The famous historian, E. H. Carr once said that in order to understand history it is necessary to understand the historian writing it. The same could be said of economics. Famous economists often remark that specific episodes in their lives, or particular events that took place in their formative years attracted them to economics. Great Thinkers in Economics is designed to illuminate the economics of some of the great historical and contemporary economists by exploring the interaction between their lives and work, and the events surrounding them.

More information about this series at
http://www.palgrave.com/gp/series/15026

James Forder

Milton Friedman

palgrave
macmillan

James Forder
Balliol College
University of Oxford
Oxford, UK

Great Thinkers in Economics
ISBN 978-1-137-38783-7 ISBN 978-1-137-38784-4 (eBook)
https://doi.org/10.1057/978-1-137-38784-4

Library of Congress Control Number: 2018966687

Cover illustration: Robert Clay/Alamy Stock Photo

This Palgrave Macmillan imprint is published by the registered company Springer Nature Limited
The registered company address is: The Campus, 4 Crinan Street, London, N1 9XW, United Kingdom

Also in the *Great Thinkers in Economics* series

Forthcoming

Alan Kirman
Vilfredo Pareto

Esteban Perez
Roy Harrod

Ramesh Chandra
Allyn Abbott Young

Harald Hagemann
John Hicks

Available

Robert Dimand
Irving Fisher

David Reisman
Thomas Robert Malthus

Peter Boettke
F. A. Hayek

David Reisman
James Edward Meade

David Cowan
Frank H. Knight

Nahid Aslanbeigui and Guy Oakes
Arthur Cecil Pigou

David Reisman
James Buchanan

Robert Scott
Kenneth Boulding

Robert Dimand
James Tobin

Peter E. Earl and Bruce Littleboy
G. L. S. Shackle

Barbara Ingham and Paul Mosley
Sir Arthur Lewis

John E. King
David Ricardo

Esben Sloth Anderson
Joseph A. Schumpeter

James Ronald Stanfield and Jacqueline Bloom Stanfield
John Kenneth Galbraith

Gavin Kennedy
Adam Smith

Julio Lopez and Michaël Assous
Michal Kalecki

G. C. Harcourt and Prue Kerr
Joan Robinson

Alessandro Roncaglia
Piero Sraffa

Paul Davidson
John Maynard Keynes

John E. King
Nicholas Kaldor

Gordon Fletcher
Dennis Robertson

Michael Szenberg and Lall Ramrattan
Franco Modigliani

William J. Barber
Gunnar Myrdal

Peter D. Groenewegen
Alfred Marshall

Acknowledgements

I am grateful for help and comments of Oscar Alexander-Jones, Bee Boileau, Conor Callaghan, Luke Chester, Tim Congdon, Sarah Duffy, Christina Laskaridis, Andrew Lin, Hugo Monnery, Scot Peterson, Cerian Richmond-Jones, Tony Thirlwall, and Caroline Thurston.

Contents

Notes to the Reader

1. The following are identified by their last names only: Martin Anderson, William J. Barber, Gary S. Becker, John Maurice Clark, Benjamin Graham, J. Daniel Hammond, Seymour Harris, Harold Freeman, Milton Friedman, Christopher Gilbert, Harry Johnson, Jack Johnston, John Maynard Keynes, Lawrence Klein, Oscar Lange, Abba Lerner, Mervyn Lewis, Wesley Mitchell, Edward Nelson, Paul Samuelson, Thomas Sargent, Henry Schultz, Jim Thomas, Thomas Wilson, A. B. Wolfe, David McCord Wright. Others sharing those names are further identified where necessary.
2. Of those featuring in these pages, I was taught as a graduate student by David Hendry, and supervised in doctoral studies by Peter Oppenheimer, and wrote a book debating the merits of the euro with Christopher Huhne.
3. Many of Friedman's works were reprinted, some of them several times. I cite the original publication and give only its date where it seems reasonable to suppose it is readily accessible and use the form 'Friedman (1951/1953)', indicating an original and a reprint date

only when the reprint seems much more accessible than the original. In those cases, page numbers refer to the indicated reprint.

4. Since Friedman wrote so much, and I have referred to a wider range of it than most discussions, there are many instances where I discuss multiple items from the same year, distinguished as 'Friedman (1974a)' and 'Friedman (1974b)', etc. To try to make keeping track of the various sources a little easier, I have tried to make the 'a' reference for any year identify any outstanding work from that year. It is not a perfect mechanism, but I hope it helps.
5. I am nowhere near offering a complete coverage of the secondary literature on Friedman, but I have very much avoided commenting on works which are not in their final form. It seems to me best to allow authors to commit themselves to a final version before we start publishing responses to their thoughts.
6. *The Times* and *The Sunday Times* are published in London, and Cambridge is in England.

1
Introduction

There can hardly be an economist of the twentieth century who is more intensely revered than Milton Friedman. It is easy to see why. He was author of *A Theory of the Consumption Function* (1957) and, with Anna Schwartz, *A Monetary History of the United States* (1963)—each of which is a better, and more important book than most economists ever write. That is just two books. There are plenty more, and dozens of scholarly articles as well, many of which most economists would be proud to have written. But there is so often seen to be a moral force to his work as well in overturning the postwar 'Keynesian' consensus and thereby promoting what came to be called the 'monetarist', or sometimes, 'Friedman's monetarist' revolution. And then there was *Capitalism and Freedom* (1962). That was not just a best seller, but also a book widely credited with explaining to millions the benefits of the free market. That was in due course followed by another of the same temper—*Free to Choose* (1980)—which was written with his wife, Rose Friedman, and which became the basis of a television series broadcast in many countries, and certainly a source of enormous fame for Friedman as well as giving publicity to his views.

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_1

On the other hand, it must also be true that there is no economist of the twentieth century who is more reviled. Though his standing as an economist of great importance in shaping the development of the subject is not often doubted, the proposition that he shaped it for the better certainly is. For his admirers and supporters he was not just a genius of economics, but also the intellectual powerhouse of the turning of the tide against statism and the suppression of the market, and whilst short, bald, and seemingly inconspicuous, was a hero of the cause of freedom. His detractors see something else. For them, he was a leading figure in the destruction of the postwar social democratic consensus, the principal catalyst of the abandonment of full employment policy, and the leading, one of the most vociferous, and probably the cleverest and most important, and very possibly the most cunning, counsel of rapacious global capitalism, as well as, sometimes, a charlatan. But for the cape, he appears as the Höllenzwang's perfect Mephistophiel.

It is an extraordinary thing that a figure of such undoubted importance, and such controversial moral standing has never been the subject of a major biography, and certainly no dispassionate one. There is Ebenstein (2007) and Ruger (2013), but they hardly fit the bill. The former is full of praise for Friedman, but really does not give much of an analysis of his ideas; the latter is specifically focussed on Friedman as a proponent of freedom and that is all very well, but it is no way to get at the substance of his work, nor for that matter, more than a part of the substance of the man. Butler (1985a), 'Milton Friedman', is a more serious attempt to present Friedman's economics. That, though, is very much a guide to Friedman's economic thought as Friedman would like it to be seen. His ideas are presented; sometimes critical lines of thinking are put, and then there follows an account of Friedman's response, and a summing up in his favour. Its subtitle could just as well be 'an explanation of why he is right' as 'a guide to his economic thought'. Then there is also Frazer (1988a, b)—two very large volumes which really cover only parts of his work and, as Wicker (1990) noted, are full of detail but without nearly enough critical evaluation.

Reviewing Ruger (2013) and Butler (1985b)—a very much abbreviated version of Butler (1985a)—but also taking the opportunity to make a brief comment on Ebenstein (2007), Nelson (2012, p. 1108) said that there were plenty of pieces authored by Friedman that they did not discuss and in various ways the discussion they did offer was not up to much. The character of his remedy to that is indicated by the online draft of Nelson (book draft), 'Milton Friedman and economic debate in the United States, 1932–1972', with mention of a further book to cover the following 33 years. It appears to be a giant undertaking—something like 600,000 words in the first book, excluding the bibliography. That will surely offer an account of Friedman's work in that period which will be as full as anyone could want.

In what follows, there is obviously no pretence that I am trying to match Nelson in the pursuit of his goals. I do though share with him, if not for exactly his reasons, the view that there is a great deal of value in attending to a much wider range of Friedman's works than is the norm. It would have been possible for me to pick six or eight works and treat them in more detail, and to aim at giving a good picture of Friedman's ideas and modes of thought, and perhaps even his influence. But instead I have tried to offer a much wider account of his printed, published work than that.

I could have gone further in enquiring into the facts of Friedman's life, or an even wider range of his works and correspondence. The collection of his papers and various sorts of recordings at the Hoover Institution is huge; its online listing of his published works omits almost nothing. And its links to the text of, or information about how to acquire, most of those works is extraordinarily close to being complete. Full investigation of all that would be a very substantial undertaking. No doubt it would be rewarding in its way, but rather than pursue that, I have tried to identify principal themes arising mainly from his publications, but from a wide range of them.

One reason for seeking that breadth is that some of the lesser-noted works are little gems, well deserving greater note—Friedman (1951a) on the impact of unionization on wages is one; Friedman (1969a) on the optimum quantity of money, concerning the idea that the price level should be made to fall, is probably not as well-known as it deserves to be, and there are plenty more. Another is that it serves to correct the common misperception about Friedman that his monetary economics was, if not quite the only story, very much the dominant one. Far from it—an appreciation of his importance as well as his brilliance really comes from seeing the range of issues about which he wrote, and how much there was that was nothing to do with money.

There is a further point, and perhaps a more important one which is parallel to a view that motivated much of Forder (2014). There, the thought was that if one wants to know what the great mass of economists thought, at a certain time, it was just as well to read plenty that the great mass of economists wrote, rather than just the three or four things that continued to be cited much later by those whose interests were anyway not historical. The presumption that one sees best Friedman's lines of thinking from a certain time by attending only to the works that continue to be cited much later raises the same issue. As I hope will become apparent, there has been much said about which works of Friedman were his most original that entirely lacks historical foundation. So if one focusses attention on those which are commonly said to be his most important, then the result must be a misrepresentation of his importance, as well as a reinforcing of that erroneous picture.

As things turn out, this wider attention brings a significant further benefit in revealing patterns of various sorts in his work. One is his habit of repeating certain messages, and rather simple ones at that. In some cases, the curiosity is just that he never sees the need to improve a story, or even change it in the light of developments. In others, there is interest in the way that later appreciations have seen such depth of thought in what is in fact just a rehash of things he had been saying in lesser-noted sources for years. There are also, though, those cases where he did change what he said, and there, against the background of repetition, there can be a special interest in them. There are other kinds of pattern too which are rarely noted—perhaps in some cases have never been noted in print—but which do throw light on developments in his thinking, and are suggestive also about his motivations. These patterns explain much of the organization of the book.

Part 2 concerns most of his economics up to and including Friedman (1957a). In this period, one notable characteristic of his work is its variety. Although Friedman (1956a)—his 'restatement' of the Quantity Theory of Money—just falls within it, there is otherwise relatively little of lasting importance on money. There is that little, and certainly work for Friedman and Schwartz (1963a) was underway. But as regards his publications, his work on money appears merely as one theme amongst several. It is in 1956 that this changes. Then, except for his book on consumption the following year, and work following up on it over the next few years, and a few oddities like Akerlof et al. (2002), almost all his academic writing concerned monetary economics. His monetary economics from the period starting with Friedman (1956a) is therefore the subject of Part 3.

Since his first paper was in 1935, the same year as Knight (1935), a collection of essays Friedman co-edited, the period in which he was publishing up to 1957 is just more than 20 years; and as it happens, a period about as long after that date saw almost all the rest of his serious research. Friedman and Schwartz (1982) is the only major work that falls a few years later, though most of the intellectual input to that was much earlier, before its publication was delayed. Otherwise, there is really only a thin scattering of publications after, say, 1980. Roughly speaking, then, his career as an academic economist divides into two—in the first half he is very much a generalist, in the second, very much a monetary economist. It frustrates my picture that Friedman (1956a) and Friedman (1957a) come in the wrong order, but their chassé-croisé marks the change in his orientation as far as his publications are concerned.

In both periods there are further patterns. In the variety of work in the earlier period, one can often see continuous lines of thought between one item and another and between one topic area and another. That is also not a very surprising finding, of course, though it is the sort of point rarely made about Friedman. Slightly more weighty, perhaps, is that across much of this work there is also an identifiable methodological unity—not all aspects of it cover all his work, and the exceptions are interesting too, but the pattern is there. His famous essay, *The methodology of positive economics*, Friedman (1953b), falls in this period and although I shall say that essay is grossly over-rated, I shall also argue that a discernible methodological motivation is visible in his work. That comes properly to light only by attending to a wide range of it, but then is clearly visible and his distinctive methodology can be appreciated.

In his work on money two large books—Friedman and Schwartz (1963a) and Friedman and Schwartz (1970)—and particularly the first, dominate the picture. But amongst the rest of this work, there are again distinct themes. There is one around the Quantity Theory, one around rules, on which Friedman (1960a) was particularly important; then one on the explanations of ongoing inflation—the explanation, the way Friedman looked at it, that is, of why as a matter of political economy it was that the money supply was allowed to grow too fast. The vast bulk of his work again falls into clearer, and more revealing, patterns than is usually noticed.

In Part 4, I consider a range of his popular writing. The two principal books—Friedman (1962a) and Friedman and Friedman (1980), *Capitalism and Freedom*, and *Free to Choose*—are sometimes said to be about 'politics' or the character of 'freedom'. That is definitely a misdescription of the second, but as concerns the first, so I argue, Friedman's supposed analysis of the idea of freedom is far too juvenile to be taken seriously. The development of his statement of it—as one again learns from investigating much less-often read works—does throw some light on his intellectual temperament, perhaps the more so when it is put alongside other instances of his telling a similar story over a succession of years. But as regards the content of it, his story about the nature of freedom is not of interest. The rest of the argument of these books, and their reception, however, certainly is of interest. His *Newsweek* columns, which I consider between the two books, very much repay reading, and they must be important because, since there were so many of them, they are in fact a significant aspect of his engagement with the public, particularly in the United States. Then there are other matters he considered, and others he did not consider, which deserve a little attention, before finally, I consider some of his publicly aimed responses to the inflation problem in the mid-1970s.

Before all that, though, in Part 1, I give attention to aspects of Friedman's biography. As elsewhere, my starting point is what he wrote. For the purposes of Part 1, much the most important source is Friedman and Friedman (1998a)—the joint autobiography he wrote with his wife, Rose. It, of course, provides an outline of his life. My selective summary of it, along with a few other details I have thought it worth adding, provide the biographical background to the discussion of his work in the rest of this book. But more importantly—more interestingly too, I hope, I suggest that his book throws up some particular points of interest about the working of his mind, and to some extent his motivations. These matters and points following from them are therefore explored in the later chapters of Part 1.

In all this, it is Friedman and his thinking which are the centre of attention. I consider his ideas, and the arguments he puts, and I try to consider the context of the times in which he puts them. I also consider contemporary responses to them since they are certainly part of the picture of Friedman's intellectual world, and in any case, his counter-responses are often part of the story, and where there are none, that may be interesting too. Something I have tended not to do is pursue very far the question of how the arguments Friedman started ended up—that is to say, the question of what professional consensus eventually emerged about the issues he was addressing. Although it seems to fascinate many, much of the time there would be an artificiality about that question anyway, since the relevant debates have been remoulded by later hands, and arguments over the answers to those remoulded questions are not truly arguments about the historical Milton Friedman at all. But in any case, such things would take me into territory different from that of describing and considering the arguments he made. So on the big questions of whether the velocity of money is a stable function of a small number of variables, whether the supply of money is independent of demand, fixed exchange rates are better than floating, or what the value of the marginal propensity to consume might be, with or without credit constraints, I am offering neither a very firm opinion, nor a digest of the latest literature. I have likewise not gone extensively into later historical commentary on Friedman. I am trying to focus on Friedman, those who responded to him, and the way that he continued, or terminated, the debate. As well as making it possible to give a more rounded picture of the scope and themes of his work as a whole, I hope this approach also leads to a suggestion as to why it is that views of him differ so widely. It is by studying the arguments as they were conducted, and as Friedman conducted them in particular, that one can start to see how such divergent opinions emerge.

In considering those arguments, there is again no pretence of offering a full assessment of the arguments even as he made them. Quite often, there is not even a pretence of giving a full description of them—there are too many of them, and some are too complex for a complete account to be practicable. What I am trying to do is indicate the character and direction of the arguments, and how he responded to the difficulties he faced, and to put others' responses to them into some kind of context. Sometimes that results in my pointing to what I believe to be strengths of Friedman's arguments that seem to go unnoticed. In a few cases, it results in quite extensive criticism. The reason for that is that I believe the weaknesses of them have not had the attention they should, and in a couple of cases, his work has been seriously misassessed as a result. I imagine some will feel I am too hard on him. But the point is to arrive at an assessment of him, his work, and the reception of his work. It defeats the purpose if the weaknesses in the work are to be ignored simply because they have been ignored by others. And the same is true, of course, of the strengths of his work where they have been little-noted.

On the other hand, the question of the context in which Friedman puts his arguments is a different one. In much of what is written about him and indeed many other great figures, there is something of a tendency to present what the author in question said and move very swiftly to declarations about its influence. The influence of ideas is anyway far from the only interesting thing about them, but the point seemingly not recognized is that it is impossible to determine the influence of an idea from what was written *later*. One can only discern how it changed things—its influence—through acquaintance with what went before. That claim about common practice, I feel, is instantiated and powerfully illustrated by the nonsense that has been written about Friedman's (1968a) supposed influence on the Phillips curve literature. Such influence, if any, simply cannot be ascertained without the study of the Phillips curve literature before 1968. Such investigations can be big jobs of course and I cannot claim here to have made full investigations. But there are a number of points at which I have taken the opportunity of pointing out that—contrary to what appears to be widely believed—things Friedman wrote would have been to him, ordinary pieces of background knowledge, not great breakthroughs at all. On a number of matters, therefore, he was not original in the ways he is often said to be.

In some of those cases, he is original in some other way, and that too is a worthwhile finding, and in others, his work was just much less important than is commonly supposed.

Some, no doubt will be appalled at that piece of blasphemy. Not only is every word a word of genius, with never a false step taken, but once he has been declared to be original on some point, there is no rowing back. Well so be it. I hope it will also be clear that I take the larger issue to be that there is so much in Friedman's writings, so much that is so clever, so immediate and well-directed at the issues of concern, so much indeed that is original, even though mixed with things that are not, and so much which surely has had a profound effect on later thinking, that even throwing the buckets of cold water that I am, it must remain beyond doubt that he was truly one of the greatest of the great thinkers in economics.

Part I

Friedman’s Life and Autobiography

2

Part I Introduction

Friedman lived a long life, and enjoyed a long career, but little has been written about his life in relation to his career. Nearly everything there is has come from Friedman and the information is notably patchy. There is certainly a project, hardly attempted here, to be undertaken in seeing how many of the gaps can be filled. But there is interest in seeing what Friedman has chosen to say about his life, and very much in observing the way he chose to say it. There are a number of little oddities about his memoirs which must reveal something about him, and I describe some of these. Then there were also a number of controversies in which he became involved, but where the controversy was not principally about the results of his research. The way in which he handled them—or did not handle them—seems to offer some insight. And there is the notable point that amongst the things hardly discussed, though the book is very long, is his engagement with British affairs, especially in the 1970s. Much of that is related to his involvement with the Institute of Economic Affairs, but another branch arises from the controversy linking his visit to Chile in 1975 and some remarks relating that country to Britain. Very notably, I think, no one seems ever to have

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_2

commented on his failure to discuss his experiences in Britain, but since it is such a substantial gap, on that question, I have written my own account, entirely from sources in the public domain, and speculated a little on why the Friedmans omitted it.

3

Two Lucky People

Near the end of their lives together, in 1998, Milton and Rose Friedman published a joint autobiography—Friedman and Friedman (1998a)—called *Two Lucky People*. It is a book of something like 300,000 words printed over more than 600 large pages, and was a distressingly long time in the writing, as Friedman told Snowdon and Vane (1999, p. 144) when interviewed in January 1996. Parts of the book are jointly written, parts by one author or the other, sometimes giving an account of events in only one of their lives, and sometimes passing the baton of narrative from one to the other. It is not quite the only source of autobiographical information about the couple since Rose wrote ten articles about their lives together in the Oriental Economist from May 1976 to February 1977, and an eleventh in April—each of some thousands of words; and as well as giving a large number of interviews, the most notable of which was an interview by Lawrence and Norman (1973), in *Playboy*, Friedman also wrote Friedman (1985/2005) with the title 'My evolution as an economist', and his autobiographical essay for the Nobel Foundation, Friedman (1976c), with a later update in Friedman (2005). Friedman and Friedman (1998a) is though, not only by far

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_3

the most substantial, but also contains nearly all the biographical information that is in any of the other sources.

It is, however, a very curious book. Although so long, much of it seems to have been written without much regard to what interested and knowledgeable readers might have been interested to learn, or of what others might have been expected to find interesting about the Friedmans. Certainly, there are important ways in which it is incomplete. I therefore take from it an outline of Friedman's life, adding a few things along the way, where it seems worthwhile and the information is available. But in what it says, what it does not say, and sometimes the way in which it does either of those things it offers an insight into Friedman's outlook and inclinations that are not straightforwardly part of his autobiographical intent. Pursuing those themes, I consider a few of the events in Friedman's life—his life beyond the narrow matter of writing and arguing about economics—to see what light his treatment of them might throw on his attitudes.

1 Two Lucky People

The book tells of Friedman's parents both being born in what was then part of Austria–Hungary and emigrating separately to the United States, Friedman's mother initially working in a 'sweatshop', but regarding it as creating an opportunity she otherwise would not have had. Friedman, born in 1912, was their fourth and last child and first son. When he was very young the family moved to Rahway, NJ, where his mother ran a retail store whilst his father continued to work in New York. The family was poor, but they had enough to eat and it was always taken for granted that he would go to college. His father died when he was fifteen, he graduated from high school when sixteen and won a scholarship to Rutgers University where, subsidized by the scholarship, he supported himself with a variety of jobs. He intended to study mathematics with the idea of becoming an actuary, but fell under the influence of Arthur Burns and Homer Jones. So, when he graduated in 1932 and was offered a scholarship to cover tuition in economics at Chicago or mathematics at Brown, he chose the former. Chicago, where he was taught by Jacob Viner, Frank Knight, Henry Schultz, Lloyd Mints, and Henry Simons was a revelation for its intellectual atmosphere. He met Rose Director, and they later married. For a year he studied at Columbia and learned from Harold Hotelling, Wesley Mitchell, and John Maurice Clark, then returned to Chicago working as research assistant to Schultz (in 1934) and publishing his first academic paper—Friedman (1935a)—criticizing something Pigou had said about empirical approaches to the analysis of demand. In the same year, he followed his friend Allen Wallis to Washington, DC to work at the National Resources Committee, where he did his first work on consumption with Hildegarde Kneeland.

In 1936 Simon Kuznets organized the first of what was to become a very long-running series of annual conferences known as the 'Conference on Research in National Income and Wealth', proceedings of which were published not quite so regularly as '*Studies in Income and Wealth*'. In 1937 he moved to New York and the National Bureau of Economic Research to start working as Simon Kuznets' assistant on Friedman and Kuznets (1945) which—after a controversy over its publication—became Friedman's doctorate, submitted at Columbia. It is not a greatly studied book, but it is certainly not a negligible one and it can be added that Mincer (1958) built on it; Gary Becker commented on its influence on his work on human capital, and Teixeira (2007) noted its importance in that area more generally.

In the year 1940–1941 Friedman taught at Wisconsin. Whilst there, he wrote a report on their teaching of statistics, saying that it was not sufficient for a student to be able to do independent work in the field. That may have ruffled some feathers and amid some controversy, Friedman's appointment was not renewed. The matter has attracted some attention, with Mark Perlman (1976) describing the decision as the outcome of 'blatant anti-Semitism', although without seeming to have much to back up the claim and neither Lampman (1993) nor the more detailed study by Cronon and Jenkins (1994) reached such a conclusion. The Friedmans hinted at anti-Semitism, and elsewhere in the book (p. 58) the point was put more strongly, but their chapter on Wisconsin was called, more neutrally, 'Victim of campus politics'. That is probably what it was—Friedman annoyed some people, his appointment did not fit in with the plans of some others, and he lost out. Still, they took the opportunity to describe Harold Groves—Friedman's principal advocate—as 'a man of great personal force and the very highest character' (p. 91) whilst Walter Morton, one of his opponents, was 'regarded as anti-Semitic and strongly pro-German at the time' (p. 101).

What is distinctive, though, as compared to much of *Two Lucky People*, is the detail with which the matter was treated, which is quite untypical of the book, and the fact that it is very clear that the Friedmans were hurt by the events or the outcome. Apart from the long discussion of the incident itself, there are several later occasions where something else is compared to their Wisconsin experience ('there were none of those bitter personal fights we had experienced at Wisconsin' [p. 192], and the like). It was only one year in Friedman's life, but it is never allowed too far from the readers' minds, presumably because it is never far from the authors'.

Leaving Wisconsin, from 1941 to 1943 he worked at the Treasury on tax policy, including the development of the withholding tax, and was seconded onto the project that led to Shoup et al. (1943) on the question of how much taxation would be required to prevent inflation as war expenditures increased. Then from 1943 to 1945 he worked at Columbia with Hotelling and Wallis on statistical problems with direct military applications. He taught for a year at Minnesota, sharing an office with George Stigler with whom he had been friends since they were at Chicago, and who, as Friedman said, 'like myself, lived, breathed, and slept economics' (p. 149).

At that time they wrote Friedman and Stigler (1946) for the Foundation for Economic Education, arguing against rent control by analysing what would later be called a 'natural experiment'. They noted that after the San Francisco earthquake of 1906, the free market had dealt with the resultant housing shortage much better than the price controlled market of 1946 could. It was a very controversial little book for three reasons. The one that seems most to have exercised Friedman and Friedman (pp. 150–151), and to judge by letters in Hammond and Hammond (2006), also Stigler, concerned the point that Friedman and Stigler, whilst opposing rent control, had expressed approval for the promotion of greater economic equality. That approval apparently upset Leonard Read, the director of the Foundation, and a footnote was added to the text (p. 10), without the authors' approval, drawing the inference that 'even from the standpoint of those who put equality above justice and liberty, rent controls are the height of folly'. Friedman and Stigler apparently felt this accused them of having that standpoint, which it does not quite do, although perhaps that is the natural reading. As a result, 'for some years' Friedman and Stigler 'refused to have anything to do with the foundation or with Leonard Read' (p. 151).

The other sources of controversy seem to have exercised them much less. The book received a very hostile reception from Bangs (1947) and Bloomberg (1947), who criticized Friedman and Stigler over their analysis and lack of acquaintance with the facts. Perhaps more notable, though was a point concerning their use of the word 'rationing' to describe various forms of allocation of housing, such as 'Rationing by chance and favouritism' (p. 13), but also, 'Price rationing' (p. 9). To judge by the discussion of Levy and Peart (2017, pp. 52–56), the idea of 'rationing' by price was a novel usage and provoked ire in some quarters, including from Rose Lane Wilder and Ayn Rand, for suggesting a moral equivalence between the price mechanism and overt quantity rationing by the state. Friedman may have been unaware of that issue at the time and certainly made no comment on it, but it was to be, of course, terminology that became entirely routine in economics with the various kinds of 'rationing' being simply alternative ways of dealing with scarcity.

Then in 1946 he returned to Chicago, taking the position vacated by Jacob Viner. He was then there for thirty years and became surely the most famous of the Chicago economists, as well as very much the figure head of 'Chicago economics'. It is quite a thought then, that Kirk Johnson and Marianne Johnson (2009, p. 212) say that Viner himself had hoped John Hicks would be appointed; Stigler (1988, p. 40) told of how he lost the job in a final interview; and according to the archival research of Mitch (2016), Friedman ended up selected as a compromise between the followers of Frank Knight and the Cowles Commission.

In 1947 he made his first trip abroad to attend the inaugural meeting of what became the Mont Pèlerin Society, saying of it, 'Here I was, a young, naïve provincial American, meeting people from all over the world, all dedicated to the same liberal principles as we were' (p. 159). He did not attend again until 1957 but became one of its more notable members and in 1970, its President. As President, reasoning that liberal intellectuals were by then in much closer contact than they had been in 1947, and that the Society had therefore served its purpose, he proposed to wind it up. But, as Friedman and Friedman (1998a, p. 334) describe, the institution proved resistant to its own abolition, and he was able only to bring agreement on limiting its size. Friedman continued to attend, and present papers, with a number of them being published.

In 1948 he began leading the NBER's study on the role of money in business cycles.[1] Friedman and Friedman (1998a, pp. 227–228) said that happened in 1950 and made the rather odd claim that this offer, and Friedman's acceptance, showed the interest in money he had developed and which was evidenced in articles reprinted in Friedman (1953c). Actually, of papers in that book that had been published even by 1950, there was only Friedman (1948a) that could be said to be much about money. One wonders why Friedman thought it mattered whether he had already manifested an interest in money just as much as how he managed to have the facts so wrong, but the whole matter is made odder still by the point that Friedman and Friedman (1998a, p. 213) said the concentration on money in that book was the *result* of his rejoining the NBER.

He spent the autumn of 1950 in Paris, as a consultant to the United States Economic Cooperation Administration, considering the Schuman Plan for European integration,[2] and writing on the benefits of floating exchange rates. In 1953–1954 he was a visiting fellow of Gonville and Caius College, Cambridge, having been encouraged to go there by Stanley Dennison when he was in London to lecture in 1952. *Two Lucky People* does not say so, but during the visit he was a guest at the Oxford

[1]This is apparent from NBER (1948, p. 22).

[2]The detail of his appointment comes from Friedman (1953d, p. 157) rather than *Two Lucky People*.

Political Economy Club, introduced by Edward Hugh-Jones[3]; and he must have spoken at University College, Hull, because Kaufman (2013), said that Ronald Dearing, when a student there, 'demolished' him.[4]

In 1955 the Cowles Commission left Chicago and on some accounts—Ebenstein (2007, pp. 57–58), for example—Friedman's antipathy to their approach to econometrics and their attitude to that of the National Bureau was an important reason. Sometimes, as it is by Boumans (2016), the dispute was put in terms of the difference between the simple econometrics and 'Marshallian' approach of Friedman and the NBER, and the 'Walrasian', simultaneous equation approach of Cowles. Friedman's preference for his approach, despite his statistical skill, was to be one characteristic feature of his work. Antagonism, there certainly was, and it is often exemplified by Koopmans (1947) which was a pro-Cowles criticism of the approach of the NBER; and sometimes by Friedman (1940a) or Friedman (1951f), pointing in the opposite direction. As Ruger (2013) noted, it is hard to believe that economists of the calibre of those involved with Cowles were really shooed off by Friedman, and Friedman and Friedman themselves say that Chicago was the worse for its departure, but it could not realistically be prevented. Christ (1994), similarly, was not in doubt about the contribution the Commission made whilst at Chicago. How much Friedman really regretted their departure is, of course, another question, with Epstein (1987, p. 108) making it clear he disapproved of their approach.

The narrative of Friedman and Friedman records that in 1955 Friedman was a government-sponsored adviser to the government of India; 1957 saw the publication of *A Theory of the Consumption Function*—Friedman (1957a)—which some, with much justice, were to come to regard as his best book. In 1957–1958 he was at the Center for Advanced Study in the Behavioral Sciences at Stanford and in

[3]The Society Minute books shows he was there on 14 November 1953 at the same meeting as David McCord Wright (as the guest of Harrod). Friedman would have had something to say since Sir Donald MacDougall who had been instrumental in preventing the floating of the pound in 1952, introduced a discussion on 'Is the dollar problem soluble?' The minutes however, do not describe the discussion.

[4]Kaufman was a long-time Labour politician and had been in the government from 1974 to 1979, so his objectivity may be incomplete. Dearing was at Hull University from 1952 to 1954.

1962–1963 the Friedmans visited 21 countries, and in later years more, and some of the same ones again. In 1964–1965 he was Wesley Clair Mitchell Visiting Professor at Columbia, and in 1967 he was visiting professor at UCLA, and then in 1972 at the University of Hawaii.

His involvement with public policy debate took a great step with the publication of Friedman (1962a)—*Capitalism and Freedom*. It originated in lectures he had given at the Volker Foundation summer school in 1956, and became a landmark of American libertarianism. In a different way, his involvement in public policy must also have been increased by the publication of Friedman and Schwartz (1963a)—*A Monetary History of the United States*—which is surely the single most important work in making his scholarly reputation. He was then an adviser to Barry Goldwater during his campaign in the Presidential election of 1964 and to Richard Nixon before and after his election in 1968, though he differed publicly with him over the imposition of price controls in August 1971. Friedman had heart surgery in 1972 but recovered sufficiently by the following year to campaign with Ronald Reagan, who was governor of California, in favour of an amendment to the state constitution to limit taxes.

Meanwhile, in 1967 he was President of the American Economic Association, delivering the famous Friedman (1968a), and initiating the establishment of the Journal of Economic Literature with Mark Perlman as editor. Friedman noted that he was the son of Selig Perlman who had been one of those favouring Friedman's reappointment at Wisconsin, but thought that appointing him as editor was his greatest contribution as president. The only other matter from his Presidency to receive much attention from Friedman and Friedman was the question of how to react to the imprisonment of Andreas Papandreou by the military government that took power in Greece in 1967. Friedman observed that having been an economist and chairman of the Department at Berkeley, Papandreou had become a socialist politician in Greece and Friedman described himself as having been 'pressured' to support his release, saying 'After some hesitation, I did so, sending a cable on behalf of the AEA to the 'Colonels'' (p. 237). The relationship of the Perlmans is a strange thing for Friedman to highlight, but was he really meaning to indicate that the imprisonment of socialists by military governments was something he thought might be acceptable, and if not, why did he advertise, but then say nothing to explain his reluctance to protest?

He served as a member of the 'Gates Commission' in 1969, on the all-volunteer army, saying of it that, 'No public-policy activity that I have ever engaged in has given me as much satisfaction as the All-Volunteer Commission. I regarded the draft (i.e. military conscription) as a major stain on our free society' (p. 381). It was a rare moment of emotion coming through in the book. Between 1975 and 1977 he made visits to Chile, South Africa, Australia, and Israel, offering advice on economic policy and meeting with political leaders. His meeting with, and advice to Augusto Pinochet in Chile attracted a great deal of criticism, then and later—much of it apparently in the belief that Friedman had had a more substantial or formal role than he did.

In 1976 he won the Nobel Prize, the ceremony being disrupted by a protest about his Chilean connections, he retired from Chicago the following year, became a senior research fellow of the Hoover Institute at Stanford and moved to California. In retirement he became an adviser to Reagan in his 1980 Presidential campaign, and then joined his President's Economic Policy Advisory Board, and made more trips to other countries, including three to China, leading in due course to the account of his visits in Friedman (1990). During the second of them, in 1988 he met and was impressed by the economic insight of the pro-market reformer Zhao Ziyang who held a senior position in the government at that time, later being ousted at the time of the Tiananmen Square massacre in 1989. In retirement he made the television series *Free to Choose*, which was shown in many countries. The book of the same title, written with Rose, was in many respects a follow-up to *Capitalism and Freedom*. And he completed, after much delay, Friedman and Schwartz (1982). He died in 2006.

2 A Sample of Missing and Forgotten Matters

There is much more in Friedman and Friedman (1998a) than that, of course. It is 300,000 words, after all. But a great deal of it is uninteresting, being a collection of recollections of minor incidents and sometimes just travelogue. On the other hand, despite its length, there are some things which are very notably not in it.

Amongst the omissions are much in the way of discussion of some of Friedman's achievements. If we are at the level of learning how his lip was damaged in a car accident (p. 22), then we might have expected to learn of his winning the Bradley Mathematics Prize at Rutgers. Even in the discussion of the Nobel Prize, nothing is said about why he won it. It was 'for his achievements in the fields of consumption analysis, monetary history, and theory and for his demonstration of the complexity of stabilization policy' according to the citation, but one cannot learn even that from *Two Lucky People*. Still, the authors did discuss the award of the Prize, and they can barely be said to have done that in the case of his winning of the John Bates Clark medal in 1951. At the time, that was awarded only in alternate years (with none in 1953, as it happens) for significant contributions by an American economist under the age of 40. It would have been entirely unmentioned in the book except for a footnote in a section written by Rose (p. 98). She said that when Friedman left Wisconsin, Edwin Witte had said that he had not achieved enough to be reappointed, but implied that the award of the medal showed this was incorrect. Since he left Wisconsin ten years before that award, the claim seems a very peculiar one. Similarly, there is no mention of his appointment, joining C. W. Guillebaud as 'General Editor' of the Cambridge Economic Handbooks and amending the introduction to them in 1956.

Just as notable as this sort of thing, though, is the lack of any worthwhile conscious self-revelation by the authors, or indeed for the most part, any sort of emotion at all. So, Friedman was 15 when his father died, and the sum total of what the reader learns about how that affected him is that it drew his attention to how lucky he was that cardiology advanced so much before he had his own heart attack. Of his three sisters, we similarly learn next to nothing, except that none was alive when the book was written (p. 20). At age 20 as a student at Chicago, the reader is told, Friedman faced his financially most difficult year and borrowed money from one them, paying it back the next year (p. 34). But more interesting is the point that after that, there was no further mention of the sisters in *Two Lucky People*. The last mention of his mother is to say that by 1932, her store had had some difficult times, but she had managed (p. 28). So, in the 66 years between then and the publication of the book, there is nothing more to be said about any of them, nor even what became of them—and there is no explanation of why not. Rose's family had only slightly more attention.

Or, to move the point onto lighter matters, nothing is said about Friedman's surely being the model for Rosten's (1966) 'infuriating man'—infuriating because he actually listened to what people said and gave reasoned responses. Nor was there mention of his plainly being the model for the fictional detective, Henry Spearman, in *Murder at the Margin* (Jevons 1978) and other books by 'Marshall Jevons'. Not just the scansion of the name, but the fact that Spearman is short, bald, with horn-rimmed glasses and thinks of everything—including the solution of murders—in terms of economic theory, whilst denying the importance of the realism of assumptions, make him unmistakable. He even, like the real Friedman, had a long-time personal assistant called 'Gloria', although Spearman did once admit the usefulness of the theory of monopolistic competition—a bête noire of the real Friedman. The Friedmans very probably learned about these books from the authors since 'Marshall Jevons' was actually William Breit and Kenneth Elzinga, and it was Breit who invited Friedman to give the lecture that became Friedman (1985/2005). In any case, they certainly knew about the books since Friedman wrote an endorsement for the cover of *The Fatal Equilibrium* (Jevons 1985). In their emotionless, humourless memoir, there is no mention of such things—nor even of Friedman's passion for the circus, previously disclosed in *Time* (19 December 1969, p. 71), nor his undergraduate regard for the work of Horatio Alger mentioned, for what it is worth, in *The Sunday Times* (12 December 1976). Friedman said he admired him for never wavering from his free-market principles, though how he came to know his work whilst at Rutgers is an interesting matter.[5]

[5]According to Scharnhorst (1980) Alger's work was far out of fashion in the 1930s, and his books out of print. They did though come to wider attention shortly before Friedman's interview, through their dystopic recall in Thompson (1972).

3 An Unintellectual Work

Whilst such things as a disinclination to disclose his feelings become apparent to the reader, on a couple of other points the authors go out of their way to assert a character trait of Friedman. But in two very notable cases, whatever may have been true of his life, the content of Friedman and Friedman (1998a) does nothing at all to substantiate the assertion—indeed, quite the opposite. One striking case is over Friedman's supposed unrelenting intellectualism. On this point, the authors—Rose, specifically—chose to quote *Time* (19 December 1969, p. 71).

> Friedman is a man totally devoted to ideas – isolating them in pure form, expressing them in uncompromising terms and following them wherever they may lead. His basic philosophy is simple and unoriginal: personal freedom is the supreme good – in economic, political and social relations. What is unusual is his consistency in applying this principle to any and all problems, regardless of whom he dismays or pleases, and even regardless of the practical difficulties of putting them into effect. (Friedman and Friedman 1998, p. 410)

In contrast, though, the book shows nothing of this devotion—there is hardly an idea it, much less one followed anywhere. The authors did say that their objective was not to describe scientific results, but to give an account of their life and times, but the book is much more thoroughly unintellectual than that suggests.

To take one pertinent example, it would not have been an exposition of scientific results if something were said about the formation of Friedman's free-market views, though surely it would be material for his memoirs. As near as he came to that, really, was to say (p. 32) that at Rutgers, Homer Jones had introduced him to the 'Chicago view'. But Friedman revealed nothing about how he reacted immediately or shortly afterwards to those ideas. Even making the point he did, for some reason, he adopted the device of quoting himself, from Friedman (1976d). That piece was a tribute to Jones, so the remark served its purpose. The fact that he had no more to say in his autobiography is extraordinary. Robert Samuelson (1998) reviewing *Two Lucky People*, also finding it disappointing for saying so little on this point, said that in conversation Friedman had said that at Rutgers he had been 'mildly socialistic'. That perhaps should be considered in the light of his telling *The Sunday Times* (12 December 1976) that there he had remained 'immune from that particular virus'—viz 'left wing ardour'. But then in Friedman (1976e) he said he learned his liberalism (i.e. views on freedom) as a graduate student at Chicago. And then there is Hammond (2003), basing his view on archival sources that go beyond anything in *Two Lucky People* who also suggested he may have been much more of an institutionalist before he joined the Chicago faculty—but that was long after he first attended the University. It is a pity Friedman chose the approach of disclosing nothing offering any worthwhile insight, but also rather puzzling.

Something of the same could be said about how little was said about his students or his teaching. There is mention of notes from Friedman's course on price theory being worked up by two students—David Fand and Warren Gustus—into what became Friedman (1962b). As the preface to that book makes clear, Friedman had been reluctant to see it published, and as Miller (1963) noted, it is a bit scrappy in some respects, although notably rigorous in parts. It was also, Miller said, 'certainly provocative enough to be a very useful supplement to other texts' (p. 467). There is also mention of the point that Friedman's teaching of monetary theory led him to set up the Workshop in Money and Banking in 1953. There, graduate students presented work to each other, and in due course more senior scholars joined or visited. The output of the Workshop eventually led to four books: Friedman (1956b), Meigs (1962), Morrison (1966), and Meiselman (1970). But beyond that the book records almost nothing.

Much more information about Friedman's teaching, and his students' reactions to it is available from the work of Hammond (1999). He recounts that Friedman taught undergraduates at Chicago only from 1946 to 1951, and whilst at Chicago otherwise only in Hawaii (Hammond could have added Cambridge, where Friedman taught six students, tutorial style, one of them being Samuel Brittan, the future Financial Times columnist). Friedman was Committee Chair for 75 Ph.D.s—the first being Warren Nutter, and the last Gerald Dwyer (who finished after Friedman had retired). His price theory course was attended by James Buchanan, Gary Becker, Robert Lucas and in the early years, Marshall's *Principles* was the main text and he told students that it was the best book in economic theory, attributing to Marshall the idea of seeing economics as a means of studying the system as it actually works. Friedman always taught it as partial equilibrium and as concerning problem-solving. In 1963 he moved from teaching price theory to money—teaching one course focussed on the Quantity Theory and one on the 'income-expenditure theory'—or Keynesian theory. He also did a great deal of work on reforming the Ph.D., to which Hammond attributes the eventual spread of the 'three essays' form of Ph.D. around the world, meanwhile also lamenting that Friedman's emphasis on clear writing spread less well. He gave his doctoral students very frank comments, advertising them as such, and often criticized students' exposition rather than their economics. The Ph.D. students are a very distinguished group. Hammond concludes 'In his thirty years at the University of Chicago, Milton Friedman created a legacy as a teacher to match his legacy as an economic scientist' (p. xxxv). Quite right—it is a gigantic intellectual legacy, but there is so little hint of it in *Two Lucky People*.

Of the Workshop on Money and Banking itself, there must be more that could also have been said since between Friedman and Schwartz (1963a) and Friedman and Schwartz (1970) a dozen unpublished doctorates were cited—eleven from Chicago, and presumably the Workshop. That is testimony not just to Friedman's seriousness as a teacher, but also the coherence of the wider project he was leading through the Workshop, and incorporating his own research. And the 'Workshop system', though not invented by Friedman was surely promoted by this success, and in itself is argued by Emmett (2011) to be a key element in the development of economics at Chicago.

And the same absence of engagement with the intellectual question is apparent in the ten pages of discussion of the economics department at Chicago. On his arrival there as a student, Friedman said he was exposed to 'a cosmopolitan and vibrant intellectual atmosphere of a kind that I had never dreamed existed' (p. 35), but there is nothing to develop the point. Then there is a little on the points of view of various members of the department—although on that score there is more attention on former members than those who were there with

Friedman. And what there is shows some signs of scores being settled at least as much as revealing to the reader in any degree what it was like. This lack of discussion is extraordinary considering that Friedman's student awe was only the very beginning of his association with the University and he is so widely regarded as having become the leader of the Chicago School. Yet, there it is. In this case particularly, the superficiality—even the triviality—of the Friedmans' account is all the plainer when it is set beside Reder (1982) or Patinkin (1981a). Patinkin, in particular offers a personal portrait of the department just up to Friedman's arrival which is lively, and vivid, and intelligent, and in every way a superior product to the Friedmans'.

4 On Not Judging Motives

Along with Friedman's asserted, but unillustrated intellectualism, another overt claim in the book, actually in a section authored by Rose quoting herself from Rose Friedman (1976, p. 21), was what she called Friedman's 'characteristic generosity in being unwilling to attribute different motives to others than to himself' (p. 218). Later on, commenting on reactions to Friedman (1977b)—one of his *Newsweek* articles, Friedman himself observed that of letters from readers protesting about one particular proposal, 'few of the negative letters offered reasoned arguments; most simply consisted of diatribe and questioning of my motives. As I have often said, it is frequently easier to question an opponent's motives than to meet his arguments' (p. 361). That was quite something because of what the article said. It had been about a proposal Friedman made for a sophisticated market-based water rationing scheme for California and asked why a scheme like it had not been adopted. Friedman gave the answer that affluent residents 'have become corrupted by the collectivist sentiment of our time, which reveals by its actions that it prefers orders by bureaucrats to voluntary exchanges by free individuals' (p. 361). Enough said, one might feel.

In fact, despite Rose's avowal, and whatever may be true of his outlook more generally, the book itself is full of Friedman's judgements of his opponents' motives, sometimes in insulting terms. To take one case, discussing the level of wartime taxation required to prevent inflation, Friedman raised the point that at one stage the Treasury's estimate of required tax increases was much higher than that of the Office of Price Administration. But then in February 1942, the OPA revised their estimate to an amount higher than the Treasury's. Friedman said, 'in retrospect the explanation was simple' (p. 111). It was that initially the OPA had been seeking legislation to empower it to fix prices and so did not want that made unnecessary by tax increases, but once the powers were granted, they wanted higher taxes to make their task easier, so they changed their estimates. Seeming to forget that he was talking about what he had understood in retrospect, Friedman said 'the manipulation of the estimates seemed to me then, and still does, dishonesty pure and simple' (p. 111).

A clearer case of judging motives would be hard to invent. And there is no doubting that Friedman meant to turn the blade when he said the explanation was 'simple'. He could have been right, of course, and it is clever argument, presenting the matter as a 'confirming instance' of a theory of bureaucratic self-interest, but the point is that he leapt to a judgement of motives, whereas other explanations are clearly available.[6] On the brink of war, it should not be surprising if spending plans change rather suddenly.

Another remarkable case concerns Arthur Burns. Amongst other points, he was described as, 'my mentor, guide and surrogate father' who 'played a major role in shaping my scholarly activity' (p. xi). And then later, that Friedman's experience of being taught by him,

> imparted standards of scholarship – attention to detail, concern with scrupulous accuracy, checking of sources, and above all, openness to criticism – that have affected the whole of my subsequent scientific work. (p. 30)

[6]William Wilson (1947, pp. 191–192) offers a much more serious account, saying 'When the Revenue Act of 1941 was under consideration in April and May of that year, the need for taxes to absorb excess purchasing power was not yet apparent, although Henderson did warn that when production had been expanded up to the limit of our resources increased taxes would be necessary in order to avoid inflation'. He also cited Henderson and Donald Nelson (1941) who called for 'substantial' increases in taxes.

But when Friedman came to discuss his advice to Nixon to float the dollar as soon as he was elected (subsequently published as Friedman [1988b]), he said Burns disagreed, and won the day with Nixon because of his seniority as an adviser. Then, concerning price controls, Friedman said first that Burns had been 'one of the strongest and most outspoken critics' of them (as was Friedman), but that after becoming Chairman of the Federal Reserve, his position changed and that he gave an 'influential talk' (p. 385) suggesting that voluntary price controls might be useful.[7] Friedman was very critical of that in Friedman (1970a) but in *Two Lucky People*, rather than discuss the issue, he offered his explanation of Burns' change of view: 'A striking example of the old saying, "Where a person stands depends on where he sits"' (p. 385). In other words, it was the fact of becoming Chairman that made Burns resile from the view that inflation was the responsibility of the monetary authority.

On both these issues Friedman had strong views, but on both other views are clearly possible. They each, therefore, offer an ideal opportunity for a man devoted to ideas to explain his thinking. The fact that nothing is said to do so is one of the most powerful signs that *Two Lucky People* has nothing at all of the intellectual about it. Having declared Burns to be such an admirable character, to whom Friedman said he owed so much, Friedman had not a single sentence on Burns' ideas on either point, and on the second, straightforwardly attacked his motives.

That is very odd, as well as very poor, but it may also be a clue to Friedman's strength of feeling on the particular question. Certainly Friedman was very much opposed to price controls, and there is no doubting that he had made some very serious, and often inventive and clever arguments against them—in that, Friedman and Stigler (1946) was only the beginning. But one wonders whether there is also an emotional reaction, perhaps showing in his attitude to the OPA, as well

[7]That was Burns (1970/1978a). In the later literature perhaps his similar sentiments in his 'Pepperdine College' address, Burns (1970/1978b) a few months later are better known. Burns' change of position may also have been less dramatic than Friedman implied since in Burns (1957, p. 84) he had apparently contemplated direct controls. There is further discussion of Burns' reasons for his change of view in Hetzel (2016) and Nelson (2016).

as Burns. That may also be suggested by the fact that after this discussion, mention of Burns disappeared from *Two Lucky People* almost completely. He also avoided the issue in his Memorial to Burns—Friedman (1987b, p. 10)—where he said of Burns' time as Chairman of the Federal Reserve that 'Those who follow me at this podium are far better qualified than I to comment on that phase of Arthur's life'.[8]

A different kind of oddity arises over the award of the Nobel Prize to Friedman in 1976. Friedman's main comment on winning it was that say that he was more concerned that his work was favourably judged over a longer period, and to question whether the centralization of so much power in the Nobel Committee made for a good system (pp. 442–443). Rose's discussion was rather different. She cited the comment of *The Financial Times* that the award to Friedman was overdue, and observed that in the first two years of the prize (1969 and 1970), press speculation had focussed on Samuelson and Friedman as likely winners. The first year it went to Frisch and Tinbergen and the second to Samuelson and she wrote, quoting Rose Friedman (1977, p. 28),

> When my husband was passed over for the next five years, it seemed obvious to me as it did to many colleagues, that there was something in addition to the contribution to economic science that was being weighed in the scale. (p. 445)

and followed up by reporting some journalistic views implying that the award had been delayed because of Friedman's involvement in public policy debate, and alleging that there had, unusually, been an argument over the award amongst the broader committee of the Swedish Academy of Science, which normally approved the economists' recommendation as a formality. 'The non-economists on the committee knew

[8]However, in Friedman (1974b) he also defended Burns against the allegation that he manipulated policy to aid Nixon. That allegation was made first by Rose (1974), then repeated by Paul Lewis (1974) in terms that may have suggested Friedman agreed. Although he disagreed with his policy Friedman found it 'inconceivable' that Burns would do such a thing. It seems though, from Abrams (2006) and Butkiewicz and Abrams (2012) that Friedman may have been wrong about that.

only of his more publicized role as a political gadfly out of sympathy with the dominant socialist philosophy of our time', she wrote (p. 446).

Perhaps she was right. But it would have been interesting to know whether Friedman shared her view, or had some other explanation. It is also of interest, though, and rather sad that if this was amongst her responses to the award in 1977, it was this kind of thing, grudging as it was, she wanted to quote twenty years later. Somehow, even in 1998, the wait from 1971 to 1976 was more important than any other reaction. But again, nothing is said, and readers can only make their own guesses as to what might generate that reaction.

5 Friedman's Economics

There is little discussion of Friedman's economics in *Two Lucky People*—only about 25 pages, three of them a 'digression' on 'why economists differ'. There is slightly more intellectual content in these discussions than the rest of the book, but the chapter is of a very variable character, with the outline of Friedman's theory of consumption well explained, and a couple of isolated remarks capturing some sense of other arguments. The apparent goal, though, is not really to convey the sense of his thinking, but rather to show that it was on the one hand controversial, and on the other, that his view ended up being widely accepted. But there was not much attention even to making those points with any care.

So, on Friedman (1953b)—his essay on methodology—he quoted a working paper version of Hammond (2009), saying Friedman's paper was a classic and had been many things to many people, and the abstract of Mayer (1993) saying, 'Friedman's essay is broadly consistent with the methodology that most economist now affirm, at least in principle'. That, said Friedman, was one reason he thought the paper had become controversial, the other being that he had never commented on the debates about it. Why either of those should have that effect is none too clear, but Friedman summarized the methodology itself with the quotation,

> the ultimate test of the validity of a theory is not conformity to the canons of formal logic but the ability to deduce facts that have not yet been

> observed, that are capable of being contradicted by observation, and that subsequent observation does not contradict. (p. 216)

It is not a notably good piece of drafting and his insistence on quoting himself rather than writing an account afresh does him no favours. But more notable than that is that it is not even a quotation from Friedman (1953b), but rather from Friedman (1946).

Of Friedman (1953d), on the case for floating exchange rates, he actually said very little, beyond the fact that it initially persuaded few, before suggesting there had been a rapid change at least of some official views when floating was adopted, and seeming to drift into general reflections on matters concerning exchange rates, with really no connection to his paper at all. The discussion of Friedman (1957a) was much the longest in the chapter, and does much more to explain the ideas underlying the book. Still, even there the point about its being controversial, before becoming broadly accepted is made twice (pp. 223, 227).

Then there is a discussion of 'Monetarism versus Keynesianism'. Friedman said that the 'counterrevolution in monetary theory' (p. 228) began with Friedman (1956b) and progressed through Friedman and Meiselman (1963), Friedman and Schwartz (1963a), Friedman (1968a), and a debate between Friedman and others in Gordon (1974a). At the beginning of the discussion, the issue was characterized as being about the relative importance of monetary and fiscal policy; at the end most of his discussion was about the Phillips curve, though the change was not explained. He said that Friedman and Meiselman (1963) was about the relative stability of the multiplier and the demand for money and that it came about after the Commission on Money and Credit invited him to address them, but only after dinner since he was not to be taken seriously, though they gave him $10,000. The controversy over his paper, he said (p. 229), 'dominated an entire issue of the American Economic Review' (more precisely, it took 100 pages out of about 270 of the September 1965 issue).

Friedman and Schwartz (1963a) was at this point treated much more briefly. All that was said about what it argued was that it presented evidence of a consistent relation between monetary change and subsequent economic change. Tobin (1965), described as a 'leading

Keynesian' and Nobel Prize winner, was said to be strongly critical of some of the book's conclusions, but was quoted as saying that the argument of the book was not controversial, and certainly that it was important. Then Friedman introduced Friedman (1968a) and said it questioned the validity of the Phillips curve and proposed the idea of the natural rate of unemployment. He said his ideas 'by now' had become conventional wisdom, but notwithstanding that, Tobin (1995) was quoted, this time described as 'still an unreconstructed Keynesian' (p. 231), blaming acceptance of the natural rate for economic stagnation at that time. Friedman annotated that comment with the remark 'all because of the dreadful influence of my article!', which as Cross (2001)—who happens to have been the editor of the volume in which Tobin's comment appeared—noted whilst reviewing *Two Lucky People*, showed that Friedman preferred sarcasm to an intellectual response to the point. Quite so—that was precisely a moment where Friedman might have been expected to make a case with economic analysis.

Then came the discussion headed 'The monetary volumes'—clearly suggesting Friedman and Schwartz (1963a, 1970, 1982). In fact, there was discussion only of the first, which had already been mentioned. It was said that it showed the Federal Reserve had, contrary to previous views, been responsible for the severity of the Depression. This was because it had allowed a very large decline in the money supply between 1929 and 1933. Other than that, Friedman commented only on what he had learned about internal Federal Reserve reactions to the book.

Finally, note should be taken of a really remarkable aspect of this discussion. In their digression on 'Why economists differ', Friedman observed 'I have repeatedly experienced attacks on what I regarded as scientific findings by economists who seemed driven more by their values than their objective judgment' (p. 219). That of course was Friedman questioning other's motives but as presented, it reflected a movement in his view towards that of Rose's. She, it was said, had always believed ideology had an important role in determining people's scientific views.

Elsewhere, a little more to that story comes into view. Holden (1980) reported her as writing 'a biography of her husband which relates the world's recent economic crises quite simply to the rejection of his ideas' and quoted her view that the one area where they disagreed was over 'why his views took so long to win acceptance'. A couple of years later, a report in *The Times* (28 September 1982, p. 10) said that publisher Harcourt Brace wanted Rose to write a book based on the Oriental Economist pieces and quoted her saying,

> It is a project I have dreamed about for many years. The general theme is combination of the biographical about the two of us, but with particular emphasis to pursue the hypotheses that Milton has developed, primarily from the point of view of "Why does it take so long to persuade fellow economists?"

The answer, surely, is that fellow economists have what they regard as good reasons for their views. But somehow it does not seem likely that that was to be Rose's conclusion. It is almost as if they have it in mind that all controversy really arises from the ill will of Friedman's opponents. But in the second place, there is something extraordinary about moving the discussion onto the possibility that other economists allowed ideology to affect their policy advocacy, without it ever entering contemplation that someone might say that about Friedman. The Friedmans might dispute the fact, and the question is considered by Cherrier (2011). But the possibility that the suggestion might be made is not something a reasonable person would be able to deny. Surely Friedman should have realised that of all figures of twentieth-century economics, he was one who others felt was ideologically motivated. But the authors of *Two Lucky People* present themselves as if they are entirely oblivious of the possibility.

6 Conclusion

The unintellectual character of *Two Lucky People*, and the absence of any sign of self-awareness on the part of its authors are much the most striking features of this long and uninformative book. On the basis of what they said about Friedman—that he was totally devoted to ideas—it should have been impossible for him to write this book. Sure enough, some of its weaknesses may arise from the difficulties the authors had in completing it. But that would be a poor way to explain its length, and its intellectual failings are not really limited to certain parts—they are a consistent feature. Even on the basis of just those observations it seems a mysterious book, and one unbecoming of the Friedmans as well.

4

Three Controversies

An interesting aspect of Friedman and Friedman (1998a), also noticed by Cross (2001), is that although Friedman's treatment of controversy over his economics offers nothing in the way of worthwhile scientific insight, he gave a much more earnest consideration to some of the other controversies in his life. Various minor ones come up throughout the book—the one over Leonard Read's footnote is an example; the situation at Wisconsin another; I suppose the question of the OPA's estimates of required tax increases is another—but there are three in particular warranting further attention since they all feature in discussions of Friedman, and in each case there is more to be said than is commonly recognized. And two of them are considered at some length in *Two Lucky People.*

1 The Publication of Friedman and Kuznets

One of these arose over the publication of Friedman and Kuznets (1945). This was the study of income from professional practice that Kuznets had underway, and which Friedman joined, eventually using

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_4

the research as his doctoral dissertation. Rose Friedman (1976, p. 19) said that publication was delayed by the war, but in fact there was rather more to it than that, as revealed in Friedman and Friedman (1998a).

Friedman explained the matter as arising from the authors' suggestion that the monopoly position of the American Medical Association had the effect of restricting entry to the profession and thereby raising doctors' salaries and the cost of care. The rules of the NBER required books for publication to be considered by a 'special reading committee' of board members, and that one of the members of the committee for his book, as Friedman put it, was 'C Reinhold [sic – should be 'Reinold'] Noyes, who was in the pharmaceutical business' (p. 74). He said Noyes,

> recommended strongly against publication on the grounds that in the part of the book 'about which economic theory has speculated,' i.e., the part dealing with the reasons for differences of income in different occupations, 'the authors have allowed that theory to blind them.' In particular, he wrote, 'I suggest that the subject of freedom of entry is a hot poker and be dropped'. (pp. 74–75)

The dropping of the hot poker is obviously rather more limited than the bald recommendation against publication suggested at the beginning of the quotation, so Noyes' precise position is not clear. The reader is then told that Friedman and Kuznets wrote various memos and incorporated qualifications into their text, so said Friedman and Friedman, 'replying to successive blasts from Noyes' (p. 75) before the book was published.

The nearest Friedman came to explaining the basis of Noyes' objection is the allusion to the pharmaceutical industry. Thin though the connection is, the point certainly has no relevance except as to imply a bias in favour of the medical profession. On the other hand, he did not point out that Noyes' role in the process arose from his status as a Director of the National Bureau, of which he was later Chairman, nor, as is revealed by the front matter of Fabricant (1942), that he was a director as a result of his nomination by the American Economic Association, not any business interest, nor that he was no mere pharmacist, but the author of two substantial scholarly works himself—Noyes

(1936, 1948). And to judge by his obituary in *The New York Times* (6 July 1954, p. 23), he had left the pharmaceutical business in about 1929.

In any case, the NBER procedures allowed directors to publish comments in a book, and Noyes did so (Noyes 1945, pp. 405–410 of Friedman and Kuznets). Friedman's frustration is perhaps understandable since the weight of much of Noyes' comment was that the evidence adduced by Friedman and Kuznets did not definitely entail their conclusion though he admitted that it would probably be impossible to make the case fully. However, he did also question Friedman and Kuznets' assumption that medicine and dentistry required equal ability—an important step in making out the case that medical incomes were artificially inflated—and pointed out that the difference in mean incomes of doctors and dentists was accounted for by a small number of very high earners amongst the doctors. He suggested that might be due to exceptional success by those doctors, and therefore not be explained by the restriction of entry. They were all good points, calmly made and with no resemblance to any 'blast' alleged to have occurred earlier in the process. And whatever the ultimate importance of the points, reviewers generally shared concern about whether too much was inferred from limited data, and three of them—Lazarus (1946), R. L. Anderson (1946), and Barna (1947)—specifically noted Noyes' points with approval. And it might be added that Noyes made comments in other NBER publications, including Dean (1941), NBER Committee on price determination (1943), and Barger and Landsberg (1944), each time making worthwhile points, with some sense of questioning whether the evidence presented by the authors was sufficient for their stated conclusions. All this makes Noyes seem a sincere commentator.

It is not clear just what changes Friedman and Kuznets were required to make, but some indication comes from Friedman and Kuznets (1939), which was a preliminary report on the work towards Friedman and Kuznets (1945). Presumably because they thought it the most interesting part, the authors focussed on the explanation of their data and the reasons for the differences in income of dentists and doctors. It is not clear that the changes visible here are sufficient to account for the length of delay that occurred, but the earlier paper does indeed show some tendency towards more specific claims, and certainly a clearer implication that the American Medical Association was seeking to restrict entry into the profession. So, it seems natural to suppose that the difference came from Noyes' intervention and greater caution was forced on the authors.

There is another twist to the story though. Friedman ended up praising Wesley Mitchell, the Director of Research, saying that in three years of argument he tried to mediate whilst constantly supporting the scientific freedom of the authors. In a letter now available from Mitchell to Burns, though, Mitchell praised Friedman's ability, and noted that Burns himself had said he thought Friedman had 'more to contribute to economic science than any man of his generation'. But he also said that they had all been too quick to accept Friedman's response to Noyes' first comments but 'Noyes' second set of criticisms forced a more searching examination'. And after a month of study himself, Mitchell said he had concluded that Friedman had 'misused his data in several ways' and 'reached an indefensible conclusion' (and said that Friedman admitted his mistakes). Mitchell's conclusion was that Friedman was so sure of his view that he was led to 'accepting at face value any statistical evidence that pointed in the direction he knew was right',[1] and that Friedman lacked appreciation of the non-rational factors affecting behaviour. Clearly, with those views, Mitchell had needed to be diplomatic. But equally, it is clear that Friedman and Friedman's presentation of the case involves a pretence that nothing was wrong with the original work.

2 Friedman in Chile

A second issue is rather a well-known one and concerns the matter of Friedman's involvement in Chile and in particular his visit there and meeting with Augusto Pinochet in 1975. The case is notorious and often discussed, nearly always to Friedman's detriment, and often

[1] Arthur Burns Papers held at Duke University Box 2, Correspondence W. C. Mitchell 1911–1945, which I am quoting according to Collier (2017).

savagely. In Friedman and Friedman (1998) there is a chapter devoted to the question along with a further 12 pages of letters and documents reproduced in an appendix. The chapter was written by Friedman alone and in fact gives no hint that Rose was even in Chile, though that is revealed in passing when discussing their visit to Australia (p. 427).

The situation was that in 1970 Salvador Allende had become President of Chile, and the first democratically elected Marxist head of government anywhere in the world. After a brief period of economic success, growth turned substantially negative and inflation rose. Amid deteriorating economic performance and accusations of unconstitutional behaviour, in September 1973 Pinochet took control in a military coup, with Allende dying violently in the process. There were, at this time, a number of Chileans who had graduated in economics from Chicago, were generally of free-market views, and who in due course came to have substantial influence over policy, and to be known as 'the Chicago boys'. At the suggestion of Arnold Harberger, who had taught many of them, Friedman visited Chile in 1975 and participated in various meetings and discussions, including two seminars, had a meeting with Pinochet, and at his request, on returning to Chicago, wrote to him with advice on economic policy.

A sharp anti-inflationary monetary policy was implemented and the immediate effects were, of course, very painful. Friedman then came in for criticism over his alleged or presumed influence over policy-formation. That criticism took a variety of forms, but of it all, Letelier (1976) was the most significant both for its content and the following events. Letelier had served in the Allende regime, and described Friedman as 'the intellectual architect and unofficial adviser for the team of economists now running the Chilean economy'. The article was erudite and powerful and also pointed out that power structures within the Chilean economy remained, so that landowners in particular were beneficiaries of the policy, whilst workers' rights had been curtailed. The key goal of the regime, it was implied, was to restore the old hierarchy, threatened or changed by Allende. Free-market economics, cloaked in technical language by the Chicago boys, abetted by Friedman, was the means to this, and—crucially—political repression was necessary to the implementation of the economic policy. A consequence was that the fundamental link between the politics of repression and the economics of inequality made it impossible to argue—as Friedman tried—that his technical advice was separable from the question of his support for the regime.

Letelier's article was published in *The Nation* on 28 August. The following events were that less than a month later, on 21 September, he was murdered by agents of the Pinochet regime in Washington, DC (on which: Freed and Landis [1980]) and less than a month after that, on 14 October, Friedman's Nobel Prize was announced. There was an immediate outcry. Amongst other things, four previous Nobel laureates (none of them economists) denounced the award in letters to *The New York Times*. Much of what was said drew on the idea, using Letelier's word, of Friedman as the 'architect' of the Chilean policy, but suggested he had a much closer involvement in policy design and implementation than he did.

Some of these are quoted in Friedman and Friedman, including a *New York Times* article which said he was 'the guiding light of the junta's economic policy' (p. 401) and a student campaign to 'Drive Friedman off campus' (p. 401). He also cited a 'long article on Chile' that appeared in *Business Week*, which he said was generally highly critical of Harberger and himself and 'included the utterly fallacious allegation that we had 'uncomfortably close ties' to the CIA' (p. 402). As a result of the investigations of the Church Committee (1975) the CIA was known at that time to have its own uncomfortably close ties to Pinochet. Actually, the article is quite a balanced consideration of the mix of economic and ethical questions the situation raised, and Friedman's view was clearly put. In particular, it is not true that he was accused of having any ties with the CIA—rather, Friedman was quoted saying 'I have never had any knowing relationship with the CIA' (p. 70) and there was no indication of a reason to doubt him.[2]

Still, Friedman's point that there was a great deal of abuse of him, much of it factually inaccurate is quite right—there are plenty of other cases he could have cited. Alvaro Bunster, former Chilean Ambassador

[2]Perhaps in an interview it was put to Friedman that he had these ties, and in his recollection Friedman converted a question into an allegation?

to Court of St. James, for example, wrote to *The Times* (1 November 1976) saying,

> Professor Friedman has been a mentor of the economic policy implemented in Chile by Pinochet's regime over the last three years. From time to time Professor Friedman has even travelled to Chile to check the patient's progress.

Whatever one makes of the word 'mentor', it seems Friedman had at that time been to Chile only once. And Gunnar Logberg, of Sweden's 'Chile Committee', apparently made no distinction at all between the politics and economics, being quoted in *The Sunday Times* (12 December, 1976, p. 17), calling Friedman 'an architect of the Chilean junta'.

Much later, he was being accused of the same sort of thing in even more exaggerated terms. One example from the same year as *Two Lucky People* would be Larmer (1998), who said the Pinochet had reshaped the economy 'By squelching protest and bringing in the Chicago boys—led by University of Chicago guru Milton Friedman'. 'Under the direct guidance of Friedman and his followers, Pinochet set out to implement a "free market" program…', wrote Beams (2006). Most remarkable of all, is Naomi Klein (2007) who, making use of very short quotations from *Two Lucky People*, but never going far down the road of explaining the details of Friedman's stated position, depicted a long-term conspiracy in which the Chilean crisis was, for the free-market school, a building block of a plan of economic transformation around the world, with Friedman's ideas a key part of the programme.

Despite saying 'I never could decide whether to be more amused or more annoyed by the charge that I was running the Chilean economy from my office in Chicago' (p. 400), Friedman was clearly upset. There is simply no sign at all of his being amused. He argued that his involvement with Chile was very limited; that he never indicated support for the Pinochet regime; and to some extent sought to distance himself from the Chicago boys, saying that Harberger had been much closer to them—though in choosing to cite Rossett (1984) as authority he made an interesting move, since she had no evidence beyond what must simply be Friedman's assertions.[3] He also published for the first time his letter to Pinochet (and the reply), dated in April and May 1975, and republished a letter from Harberger to Stig Ramel, then President of the Nobel Foundation which had been printed in the *Wall Street Journal*,[4] along with a collection of correspondence in that and other papers between Friedman and his critics; and said that in his two lectures at universities he addressed 'The fragility of freedom' (p. 400).

Friedman's letter to Pinochet, as he said in the book, concerned the sort of advice Friedman gave elsewhere (except that since inflation was so high, he favoured its rapid rather than gradual reduction), and it is very much limited to that. Harberger's letter said that they had no official position in Chile, that their visit showed no approval of the regime, and that Friedman had turned down two honorary degrees from Chilean universities, but that they did not apologize for their involvement because they did not believe there would be a restoration of democracy unless there was improved economic performance. Friedman raised the question of whether the Nobel Prize ought to be determined according to the political views of potential recipients, and whether it is appropriate for people with little knowledge of economics to criticize his support for the economic policy of the regime. He expressed the view, which was also in Harberger's letter, that good economic advice, which would benefit the citizenry, should not be withheld because a political regime was objectionable.

Friedman (1976b) was described by Friedman as the published version of *The fragility of freedom* (p. 631 n7), and the similarity is confirmed by Montes' (2015) inspection of Chilean newspaper reports of Friedman's speech. As Friedman said (p. 400), it argued that the development of the welfare state led to growing state expenditures, and hence, eventually, the necessity of inflationary finance, and consequential failure of democracy. Friedman seems to have meant to present it as showing that his concern was only with freedom, not with supporting Pinochet, but actually the argument could well be read as saying

[3]She said simply, 'They are often described as disciples of Friedman, but Friedman did not know any of them well and had little direct impact on what the Chicago boys did in Chile' (p. 25).

[4]10 December 1976.

his coup was somewhere near inevitable, and to that extent, excusing it. From Friedman's point of view, the objective of the argument was certainly to suggest that welfarism should be avoided because freedom was much more important, and of course Friedman's general attitude to freedom is not in doubt and there seems to be nothing to suggest he would have excepted freedom in Chile.

So, on his own terms, Friedman won this argument. He held no official position in Chile and he did not condone repression. He was surely entitled to the view that ordinary people would benefit from good economic advice, as well as the view that his own advice was good advice—although others doubted it. The idea that the control of inflation was an essential preliminary to the restoration of democracy is certainly not a foolish one. So if we grant only that each must decide for himself when exigencies demand that one sup with the Devil, then he had nothing for which to apologize, and plenty about which to complain. Indeed, whatever view is taken of whether such advice should be given, it seems he was libelled many times.

On the other hand, that is not quite the end of the matter since there are three other points to be noted. One is that although Friedman was quite right about the issues he regarded as the key ones, in *Two Lucky People*, he did not actually tell the whole story, nor the same one as he told elsewhere. Interviewed by *The Sunday Times* (12 December 1976, p. 17) he had said, 'There are old students of mine down there and I'll be goddammed if I'm going to turn my back on them'. That suggests a rather closer association with the Chicago boys than his later attempts to distance himself from them. In the same article, it was reported that he claimed 'the only thing' he did in Chile was to give two lectures in Santiago in March 1975. But that was simply not true. For a start, he met Pinochet. But furthermore, in *Two Lucky People*, he also said that he had seminars with,

> government officials, representatives of the public, and members of the military. The talks were planned to enable us to inform the public about our interpretation of the situation and our recommendations for action. All in all, the result was, as I wrote in my notes on the visit, 'a hectic and continuous schedule'. (p. 399)

He also answered a question in Friedman (1977d, p. 37) with a lie. He said 'I spent six days a year ago (in April 1975) in Chile and have had no contact since with anybody in Chile'. But he had had an exchange of letters with the President. Had Friedman been intent on being entirely straightforward he would also not have allowed Holden (1980, p. 35) to believe, as he reported that, 'When the Chilean junta of General Pinochet ousted President Allende, Friedman happened to be lecturing in Santiago'. He would have made it clear that the visit was planned after the coup. And he would have had to have offered a little more elucidation of his claim in *The Observer* (12 December 1976, p. 11) that he had 'stressed' that his advice on policy in Chile, 'assumed the existence of effective welfare programmes'. His letter to Pinochet did advocate the relief of 'acute distress' and relief of 'cases of real hardship', but gave no indication that these were necessary conditions of his support of disinflation and marketization, which was the clear impression in *The Observer*. Had he wanted to give an entirely full and frank account of his exploits, he would surely also have reported his second trip to Chile in 1981—still during the Pinochet period—when he and Rose attended a Mont Pèlerin Society meeting and he again spoke publicly about his views about economic policy. That visit is considered in some detail by Montes (2015), but goes entirely unmentioned in *Two Lucky People* (or, I believe, anything else by Friedman, except that Friedman (1995) was described as a version of the paper he gave). It is also interesting, that later stories were not quite the same—in Friedman (2000/2012, p. 250), discussing *The fragility of freedom*, he was asked 'So you envisaged, therefore, that the free markets ultimately would undermine Pinochet?', and replied 'Oh, absolutely. The emphasis of that talk was that free markets would undermine political centralization and political control'. But to judge by Friedman (1976b), it was not about that at all, but about how excessive public expenditure leads to the failure of democracy. So, although he was asked a leading question, and followed the lead, his answer was rather far from giving the correct picture.

Secondly, there is Friedman's lack of sensitivity over the conduct of the regime. It is all very well to think that there was no alternative to shock therapy in the circumstances that arose. And if he felt the policies of Allende made a military takeover inevitable, he was entitled to argue the case. But to emphasize only those things suggests brutality of vision which did him no good. And pro forma disavowals of support for repression take him no further.

Along these lines, if he thought it important to draw attention to the idea that there was a Soviet-inspired international propaganda campaign to blacken Pinochet, he also might have thought to find a more broadly respected authority than Lasky (1975).[5] Elsewhere—Friedman (1992a)—he said of the protests against himself, 'I firmly believe that these demonstrations were orchestrated by the international communist apparatus'. This, he substantiated by asking why there were no similar protests when he visited Communist countries, and gave the answer, 'The reason is clear. Those protests were instigated and organized by outside forces'. Then in Friedman (2000/2012, p. 249) he said that the Communists were determined to damage 'anybody who had anything to do with' Pinochet, and this time seemed to think the point demonstrated by his claim 'I remember seeing the same faces in the crowd in a talk in Chicago and a talk in Santiago. And there was no doubt that there was a concerted effort to tar and feather me'. Since all that was required was people to abuse Friedman over Chile, the local Rent-A-Crowd would have offered the Communist conspirators a more economical deal than flying people from continent to continent, and one can only wonder who Friedman thought he was kidding.

That sort of thing is foolish and not to Friedman's credit, but there is more than that. For him to tell his readers that during their discussion of shock therapy, Pinochet was 'clearly distressed at the possible temporary unemployment' (p. 399) does no service to Pinochet, or Friedman, or his readers, unless they are astonishingly gullible. And concerning Letelier, his discussion had an extraordinary tone. He said (p. 402),

[5]The author was a Watergate apologist and something of a conspiracy theorist; the publisher was an organization set up to promote Pinochet's interests in the United States, and the book or pamphlet seems to have become unobtainable. There may have been such a conspiracy of course, but the point being made is about how much weight Friedman could reasonably have expected this source to carry.

> Orlando Letelier, who had served as Chilean ambassador to the United States and minister of foreign affairs in the Allende government, published an article in *The Nation* titled 'Economic Freedom's Awful Toll.' Not long after the article was published, he was assassinated in Washington, creating a great furor and scandal, and he promptly became a martyr.

Indeed—all that happened. Friedman did not bother to say that Letelier had very much pointed the finger at him for being a principal inspiration of the policy, nor that the assassination was carried out by agents of the Pinochet regime. But if that statement is all the emotion Friedman could summon, even in the context of declaring his disapproval of the regime, perhaps it is just as well.

Some, or perhaps all of these, might seem to invite dismissal as carelessly put remarks, but it is too easy to find other times when Friedman showed the same deficiency of empathy. One came up in connection with China, when, in Friedman (1989a), he wrote of the killing of hundreds or thousands of protesters by the Chinese army in Tiananmen Square, that, 'among the most disturbing of the consequences is the heightened inflationary pressure that the massacre will generate'—and mentioned no other consequences at all.

He also made some remarkable comments during his apartheid-era visit to South Africa in 1976. Friedman and Friedman (1998a, p. 436) said that the visit, 'only reinforced abhorrence of the apartheid policies imposed by the Nationalist government. Yet it made us recognize, as we had not before, how complex the actual situation was'. In the record of the visit provided by Feldberg et al. (1976), Friedman (1976f, pp. 48–49) said,

> The great discrepancy between the average income of the Whites and the Blacks in South Africa is at the root of your fundamental political difficulties with the rest of the world… I am enormously impressed that everybody I have talked to in South Africa has this problem at the top of his head.

That was not all he said, but still it was 'the' root of the difficulty and there was nothing there to suggest he mentioned political rights as being part of the picture. Then having just said that the disparity in income between the two groups was a factor of ten, he said it was important to create equality of opportunity, and suggested that rather than giving free schooling to whites whilst charging blacks, as was the case when he spoke, both should pay the same fee.

That book also contains a list of remarks he made, presumably selected by the editors, and presented without context, including one described as a comment 'On reading Verwoerd's speeches'. Friedman is quoted as saying, 'You know, his theory makes a hell of a lot of sense. Something has gone wrong with its implementation'.

Hendrik Verwoerd was Prime Minister of South Africa from 1958–1966, was an 'arrogant visionary' and purveyor of 'accomplished sophistry' in the view of Hepple (1967, p. 186)—a biographer as well as one of his political opponents. But he was certainly a principal advocate of apartheid. We are not told which speeches Friedman had been reading, but amongst the published ones, Verwoerd (1966a, p. 24) said,

> My point is this that, if mixed development is to be the policy of the future in South Africa, it will lead to the most terrific clash of interests imaginable. The endeavours and desires of the Bantu and the endeavours and the objectives of all Europeans will be antagonistic.

And Verwoerd (1966b, p. 16) said,

> I want to state here unequivocally now the attitude of this side of the House, that South Africa is a white man's country and that he must remain the master here.

What sensible theory it was that Friedman found, one can only guess.

It is possible that the quotation of Friedman is misleading, but on the other hand notable that he seems to have made no complaint about it. He complained about his treatment over Pinochet, which might be said to be a larger issue, but he also complained about Leonard Read's footnote, which hardly can, and one of the editors of the South African volume was described as a friend in Friedman and Friedman (1998a, p. 571). Probably he was just out of his depth in understanding the country and what to say about it, and made a foolish and naïve remark, and there is no real reason to go further than that.[6] But his obtuseness about this kind of thing is clearly marked.

But concerning Friedman's discussion of Chile in *Two Lucky People*, there is a third and most notable point. In his discussion of his visit, Friedman really does make an argument. It does not quite present all the awkward details, and it is an argument made very much on Friedman's terms. It is inconsistent in some important details with earlier explanations, so they seem to have been deficient. But still, his objective being what it was, he pursued it properly—presenting the letters, giving an accurate sense of what others said about him, and stating his case. If we compare that with his treatment of the dispute with Reinold Noyes, or the falling out with Burns over price control, or even his accounts of his own research, it is clear that although there is something of the same in the discussion of events at Wisconsin, it is over the events in Chile that Friedman for the first and only time in the book sets himself really to make his case.

3 *Monetary Trends*… in the United Kingdom

And the third case concerns the reception of Friedman and Schwartz (1982) in the United Kingdom. This was the third volume of Friedman's collaboration with Anna Schwartz. It was a long-awaited follow up to Friedman and Schwartz (1963a) and was initially written as a study of monetary trends in the United States. In that form it had been ready in 1966 but, like Friedman and Kuznets (1945) it was an NBER book, and as Friedman and Schwartz explained, one of the readers had suggested that it would be interesting to conduct a parallel study of those trends in the UK. The authors had accepted that, but it had

[6]Naivety is surely there, but Slobodian (2018, p. 177), discussing attitudes to southern Africa, had Friedman being one of the neoliberal movement's 'outspoken critics of universal suffrage for the region, with a focus on Rhodesia in particular'. That seems to rest entirely on Friedman (1976g)—a *Newsweek* column in which Friedman opposed sanctions against Rhodesia and said economic damage would probably follow from universal suffrage. Slobodian was surely going too far.

taken them much longer than anticipated whilst, they seem to suggest, delivering results they thought not worth the effort and the wait (1982, pp. xxviii–xxix). They shortly had further reason to regret the delay because when the book was published, it appeared during the debate over the Thatcher government's policy of monetary targeting and was therefore a book of potentially great policy significance, and hence of great political importance. Recognizing these things, the Bank of England commissioned two studies of it—initially published by the Bank as Brown (1983) and Hendry and Ericsson (1983). Brown, about the same age as Friedman, was by this time a grand old man of British economics, and an expert on twentieth-century world inflation, having studied it closely in Brown (1955) and being about to produce Brown (1985). Hendry on the other hand was 30 years younger, the outstanding econometrician of his generation, and leading figure of the 'LSE School' of econometrics, who was by then at Oxford.

Neither found much of importance in Friedman and Schwartz' book, and Hendry and Ericsson in particular made it rather clear that they thought it was rubbish. They framed their discussion with a quotation from Friedman (1953b) to the effect that the proper test of a theory was a comparison of its predictions and experience, and concluded that Friedman and Schwartz had failed to present 'evidence pertinent to their main assertions' with the result that they were left 'devoid of credibility' (p. 82). They went on to say that some of the propositions Friedman and Schwartz said they had 'corroborated' could be refuted using the same data and finished up by saying that falsificationism had its limits but that 'rigorous evaluation of empirical claims seems a necessary first step towards taking the con out of economics' (p. 82). Despite the propitiatory efforts of Matthews (1983, p. 6), the Chairman of the Panel, who said Hendry and Ericsson had emphasized that they were 'not attacking the monetarist position as such', but merely arguing that evidence for it was not presented, it is no surprise that the press seized on the matter.

The Financial Times, *Observer*, and *Guardian*, all reported Hendry and Ericsson's conclusions, all quoting the expression 'devoid of credibility'. The report in the *Observer* (18 December 1983, p. 8) went further, saying that Hendry and Ericsson had 'totally destroyed the one academic study on which Friedman's reputation rested'. That was the paper's judgement, and may have been based on the idea that it was Friedman and Schwartz (1963a) that was under discussion, though the claim would still be wild. Hendry was quoted as saying that Friedman and Schwartz had resorted to 'simply incredible' data manipulations and that 'almost every assertion in the book is false' (in context, he would have been understood as referring only to assertions about the UK). The paper stated without elaboration, 'And Professor Friedman has not exercised his right to reply'.

The quotations from Hendry may have come from *The Guardian*, three days before. There, Christopher Huhne, who was active in the SDP/Liberal Alliance,[7] and an opponent of Thatcher's policy, authored two pieces on the same day. In the longer one, which was reprinted in *The Guardian Weekly*, Huhne described Hendry and Ericsson as having dropped a bombshell on Friedman and that 'the emperor of international monetarism, is roundly declared to have no clothes on' (p. 19). Emphasizing Hendry's credentials as a leader in his field, he said the authors 'succeed in destroying the evidence Friedman offers brick by crumbling brick'. Huhne also said of one piece of reasoning, but expressing his own judgement, that it was 'very circular, and very naughty'.

The shorter piece was on the front page of the paper and had the headline 'Monetarism's guru "distorts his evidence"'. Huhne said Friedman had 'effectively been accused of distorting evidence for his theories in a devastating critique to be published by the Bank of England'. The expression 'distorts his evidence' was not in fact said to be a quotation from Hendry and Ericsson (or anyone else), although the headline surely gave the impression that it was. Shortly after, there was a further editorial in *The Guardian* mainly concerned with saying that Hendry and Ericsson had shown monetarism to have no foundation, but also saying their study 'showed that' Friedman and Schwartz 'had substantially and suspiciously manipulated the official British data in their book'

[7] In the General Election earlier in the year he had been a candidate and would be again in 1987, losing both times before, later being elected to the European and Westminster Parliaments and serving in the Government formed in 2010.

(*The Guardian*, 4 January 1984, p. 10). And shortly after that, Huhne presented a television discussion in which, purporting to summarize Hendry's findings he said,

> In other words, the monetarist experiment was based on a misrepresentation of the facts. Those findings were published by the Government's own bank, the Bank of England. We asked Milton Friedman to respond on this programme but he refused. So we asked Geoffrey Wood, a leading exponent of Friedman's ideas, to defend him against the charge that Friedman has unfairly manipulated official data.[8]

In all this there were three kinds of criticism of Friedman. One concerned the matter of whether his econometric technique was appropriate; one whether his adjustments to the data were, and a third one as to whether they were even properly honest. Friedman seems to have been reluctant to respond on any of these levels and in this connection some correspondence in the Hoover Institution archive of his papers is interesting.

First, H. H. Gissurarson, then a student at Pembroke College, Oxford wrote to Hendry and received a reply saying he and Ericsson 'nowhere make any suggestion or innuendo that Friedman and Schwartz have in any way practiced trickery, deception, cheating or anything of that nature', but that he did not agree with their changes to the raw data, nor accept their econometric claims, and that being aware of the risk of their intentions being misrepresented, he and Ericsson had written their paper as carefully as possible.[9] Gissurarson must have sent this to Friedman who wrote to thank him, saying, in perhaps unusually self-revealing terms, 'I cannot tell you how much I appreciate your sending me a copy of the letter from David Hendry to you. It is the first indication I have had that Hendry in any way dissociates himself from the campaign of slander and libel that has been pursued against Anna and myself'. He also said he had written to Hendry, and accepted

[8]'Diverse Reports', broadcast by Channel 4 on 15 February 1984. The transcript of part of the programme is in the Hoover Institution Archive, Milton Friedman Collection, Box 155 file 2.

[9]Hendry to Gissurarson, 19 January 1984. Hoover Institution Archive, Milton Friedman Collection, Box 155 file 1.

Gissurarson's invitation to visit and speak in Iceland.[10] Friedman then wrote to Hendry rather curtly asking whether he had written to *The Guardian* to distance himself from its claims. Hendry replied that his paper contained no innuendo, if Friedman felt he had been libelled by *The Guardian*, he would have a legal solution, and said he looked forward to any substantive comments Friedman had on the arguments.[11]

Next, in March 1984, Friedman wrote to Ralph Harris, Director of the Institute of Economic Affairs and a friend of Friedman, who had evidently sent him a transcript of the Diverse Reports programme. Friedman said that he had been willing to go on the programme until he had learned that Huhne would be presenting it and that he had then refused because of what he regarded as the 'disgraceful and libellous' column Huhne had written in *The Guardian*, but had recommended Geoffrey Wood as a replacement.

In that letter, Friedman went on to comment on the controversy more generally describing it as 'hilarious', saying the Hendry and Ericsson paper was 'unreadable' and that it 'consists of criticizing Anna and me for not using econometric techniques which one of the authors, Hendry, either has not yet published or has published only in 1983 although our book was published in 1982'. Indeed, Hendry and Ericsson's paper is self-consciously one of advanced econometric methodology and well over half their citations were to publications of 1981 or later. Friedman said that he had been asked to respond to the discussion, but that 'it seems to me that that would be counterproductive' as the issues were all technical and abstract and contained nothing that was 'highly relevant to the significance or meaning of our work'. He then said,

> I have long adopted the view that it is a mistake to try to reply to criticisms, that a book is like a child who has grown up: it must be sent out in the world and stand on its own feet. I would prefer to have others defend it rather than do so myself.[12]

[10]Friedman to Gissurarson, 14 May 1984. Hoover Institution Archive, Milton Friedman Collection, Box 155 file 1. The trip to Iceland, which took place later that year is mentioned in Friedman and Friedman (1998a, p. 570), where Gissurarson is described as 'a rather lonely, and highly effective, defender of free markets and limited government in Iceland'.

[11]Friedman to Hendry, 14 May 1984 and Hendry to Friedman, 13 July 1984. Hoover Institution Archive, Milton Friedman Collection, Box 155 file 1.

[12]Friedman to Ralph Harris 12 March 1984 Hoover Institution Archive, Milton Friedman Collection, Box 155 file 2.

The letter to Gissurarson is interesting because it so clearly suggests that Friedman was hurt by this episode. The letter to Harris raises a different point. Friedman knew, and he must have realised that Harris knew, that there was more to the criticisms emanating from *The Guardian* in particular, than that he was not up to date with his econometric technique. And what is more, to the extent that the scientific question of the relation of money to nominal income is the point at issue, he could hardly refuse to consider the possibility that better technique, even if unknown to him at the time he wrote, showed that he was wrong. Friedman, then, gave no answer on any level, and if anything might appear to be trying to confound the various sorts of criticism.

The picture of Friedman as being a man who avoided engaging in controversy will no doubt appear peculiar—not least in the light of the way *Two Lucky People* revealed that he relished the controversial character of so much of his work. Indeed, what he told Harris was complete nonsense—he led a life full of argumentative responses to critics.[13] Even if one insists that only responses to comment about books be considered, there would be Friedman (1958b), a response to four published

[13]Friedman (1936b) was a rejoinder to Pigou's response to Friedman (1935a), Friedman (1949a) was a reply to Neff (1949); Friedman (1960b) to Phipps on the welfare effects of taxes, Friedman (1953e) to Oliver (1953) on the political constraints on economic advice; Friedman and Savage (1952) was a reply to Baumol (1951); Friedman (1954) on the Marshallian demand curve, was a reply to Bailey, Friedman (1955a) to Robertson, Friedman (1955b) was labelled a 'comment' but was entirely a response to Ulman's criticisms of Friedman (1951a, e), Friedman and Becker (1958a) was a reply to Kuh (1958) and Johnston (1958b). Friedman and Becker (1958b) to Klein (1958). Friedman (1961a) was a reply to a comment by Wolf (1961) on Friedman (1958d), Friedman (1961b) and Friedman (1964a) were both responses to comments made on earlier work about the length of the monetary policy lag. The criticism made by Benishay (1962) of Friedman (1961c) was directly answered in Friedman and Schwartz (1963a, p. 620 n16). Friedman (1963c) was described as a 'comment' on Brimmer (1962); and Friedman (1964b) as one on Rieber (1964a), but both of those papers criticized remarks Friedman had made in Congressional Evidence, so really they were replies (and Rieber [1964b] was a rejoinder to the second). Friedman and Meiselman (1964, 1965) were both responses to criticism of Friedman and Meiselman (1963); Friedman (1970c) was a comment on Tobin (1970a), eliciting Tobin (1970b) but Tobin (1970a) was very much a criticism of Friedman's work, and particularly Friedman and Schwartz (1963a), so Friedman's response is equally much in the character of a defence of it. Friedman (1970d) was a response to criticism of Friedman (1970e) by Kaldor (1970). Friedman (1974c) was a reply to Ulmer (1974), and Friedman (1982b) was a reply to a comment by Levin and Meulendyke (1982) on Friedman (1982a). Friedman (1987c) was a general response to rather anti-market views of Mishan (1986), but also correcting him on a specific point about Friedman (1962a).

comments on a summary of Friedman (1957a), Friedman (1957b) was a response to Fisher (1956), which offered an empirical assessment of Friedman's book. Friedman (1958c) was a comment on a review of the book by Houthakker (1958a), Friedman (1958e) was a response to comments on Friedman (1957a). And there is also Friedman (1963b) which was ostensibly a response to Archibald (1961) also incorporating comment on Archibald (1959), but in fact all Friedman did was defend Friedman (1953b)—the lead paper in *Essays in Positive Economics*. That one earned a response from Archibald (1963). Friedman (1963d) was a response to Bodkin (1960), which was a conference paper (to which Friedman also responded at the conference in Friedman [1960a]). Then there would be Friedman (1970f) which was described as 'largely a response' to criticisms of Friedman and Schwartz (1963a), and became part of Friedman's contribution to Gordon (1974a), which was also presented as a response to criticism of Friedman and Schwartz, and there was Friedman and Schwartz (1986a), replying to what Lucia (1985) said about their account of the failure of the Bank of United States in 1930. Who knows how many times he responded to criticisms of *Capitalism and Freedom* and *Free to Choose*?

And in the end, of course, Friedman and Schwartz (1991) was published as a reply to Hendry and Ericsson (1991)—a revised version of their 1983 paper, arguing along generally similar lines and in a similarly didactic style to the earlier one, with 'devoid of credibility' replaced by 'lacking in credibility' (p. 32), and the line about taking the con out of economics being dropped. Friedman and Schwartz' response was principally to argue that there could be different approaches to empirical work and that they had presented more evidence—some of it non-econometric—than Hendry and Ericsson recognized, so that they were not merely 'corroborating' their theory with their data, but seeing the data as part of a broader array of evidence. Even that was not the end of it as in due course Ericsson et al. (2016) alleged more, previously unnoticed, problems with the data manipulations of Friedman and Schwartz (1982).

In *Two Lucky People* very little indeed is said that would even give a hint of these events. As already noted, there was no discussion of Friedman and Schwartz (1982) in the chapter on Friedman's scholarly work—notwithstanding that chapter's emphasis on the controversial character of his views. Nor was it discussed anywhere else in the book, but Hendry and Ericsson did get a mention—just. That was not in relation to the 1980s at all, but in a discussion of Friedman's wartime work at the Statistical Research Group. There, having recounted a failed experiment on metal alloys, Friedman said that it had caused him to be sceptical of multiple regressions and, in a footnote, that he had written a postscript to Friedman and Schwartz (1991) when 'responding to an attack by a couple of statisticians' who had 'attacked us for using insufficiently sophisticated econometric analysis in our analysis, in particular, for not using highly complex multiple regressions, along lines that David Hendry, one of authors, had elaborated' (p. 143 n).

Neither that remark, nor anything else in the book gives any sense of the reaction Friedman and Schwartz (1982) evoked in Britain, or of Friedman's reaction to that. His avoiding of that issue, though, makes the intention to insult, as well as dismiss, Hendry and Ericsson is all the more apparent. The clearest intention though is to avoid the issues they raised.

Friedman was obviously outgunned by Hendry and Ericsson. He could not respond at their level. He would not be alone at any time. Nevertheless, certain aspects of merit in his position should be noted. One is that although his story about the wartime experiment has a juvenile feel, his scepticism about complex econometrics was genuine, being the explanation of a substantial part of his distance from the Cowles Commission, and his later views remained the same. His claim to thinking that statistical analysis was only one kind, and questions are best addressed with it and other forms of analysis together were also perfectly genuine and made at other times. In Friedman (1971a, p. 149)—a discussion of Laidler (1971)—he said that the question of the exogeneity of money needed statistical work in conjunction with historical studies. And indeed, one notable feature of Friedman and Schwartz (1982) is that the statistical analysis was, as the authors said, only a part of the argument and was constantly presented as pointing in the same direction as other considerations. Indeed, it had been the same thing earlier, since Friedman and Schwartz (1963a, p. 686) said 'A great merit of the examination of a wide range of qualitative evidence, so essential in a monetary history, is that it provides a basis for discriminating between these possible explanations of the observed statistical covariation. We can go beyond the numbers alone and, at least on some occasions, discern the antecedent circumstances whence arose the particular movements that become so anonymous when we feed the statistics into the computer'. So, outgunned as he was, and controversial though these other views may be, his argument was not disingenuousness.

4 Conclusion

These three controversies were obviously important to Friedman. In two cases, that is clear from the attention they get in the book. In the third, the matter is so plainly important that doubt about it does not arise. But then the fact that it does not get attention in the book reveals something else.

There clearly was some problem with his work on Friedman and Kuznets (1945), and when enough of the facts are considered, his attempt to imply that Noyes was unreasonable, or even biased, fails. His account of the facts in the case of Chile, in *Two Lucky People* seems to be accurate and fairly full, though in being so it discloses that other things he had previously said were not. On that one, by his own standards, he had nothing to hide and it seems entirely reasonable to suppose that this is what made the difference, in both the care and clarity that he brought to the issue. There is nothing much to be said about the matter of *Monetary Trends*, except that, of course, in so many other instances, Friedman clearly relished controversy, but here he neither engaged it, as he had done throughout his life, nor declared himself at the centre of it, as he was happy to do in other parts of *Two Lucky People*. It was a real controversy, and because of the descriptions of his work in the British press, it was more than just an argument about economics—it shared with the Chilean question an aspect of calling Friedman's conduct much more broadly into question. But on this, in sharp contrast to his handling of the Chilean question, he did not even let his readers know there was an issue, and simply insulted his critics in the course of the discussion of another matter entirely.

5

Friedman in Britain in the 1970s and 1980s

As is already clear, *Two Lucky People* is a strange book in many ways, but perhaps its oddest aspect is the nearly complete avoidance of a discussion of British politics and policy in the 1970s and 1980s, or of Friedman's many visits to the country and media appearances in those years. It is extraordinary because there is so much discussion in the book about the Friedman's visits to other countries. There are whole chapters centred on each of a visit to Paris in 1950, India in 1955, Chile in 1975, various visits to Israel, and his three to China. There is another chapter on the Friedmans' round the world journey of 1962–1963, and numerous other passages about travel here and there. There is a chapter on Britain too, but that is the one focussed on his year at Cambridge in 1953–1954. Of Britain in the 1970s and 1980s, when he became quite a notable media personality, there is practically nothing, and one must wonder why.

1 Friedman and Thatcher

The issue is complicated by the fact that there is a widely repeated story of Friedman having had a particularly powerful influence on the actual policy of the Thatcher government, and his relationship with Thatcher is

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_5

sometimes paired with that with Reagan and sometimes it is said that she was someone to whom he was personally close. Of course British policy thinking, and no doubt that of Thatcher and members of her government were affected by Friedman's research and advocacy, and Friedman was in addition, very much a figure in policy debate. But it is argued in Forder (2016) that there is no sign of the kind of direct, personal influence on Thatcher that is sometimes claimed, nor of his being any kind of adviser to the government, nor of his views being particularly valued by them.

Nevertheless, Friedman and Friedman (1998a) clearly meant to suggest a close connection of that kind in a single paragraph about a meeting at Downing Street which said (p. 566),

> During our visit to London in February 1980 to film the discussions for the British version of *Free to Choose*, Margaret Thatcher invited us to meet with her and some of her ministers at 10 Downing Street. The meeting generated an interesting and spirited discussion, especially after Mrs. Thatcher left, asking me to instruct some of the "wets" in her cabinet. As on earlier and later meetings with Mrs. Thatcher, it was impossible not to be impressed with her intellect, character, and force of personality.

This paragraph seems unlikely to be a good guide to Friedman's relationship with the Prime Minister. The day after the meeting, Thatcher told the House of Commons she had been present only 'right at the beginning' of it, so there cannot have been much time for her three admirable characteristics to make their impression on Friedman. As to 'earlier and later meetings', there seems to have been exactly one of each. There was a 'hastily arranged' meeting in 1978 (before Thatcher was Prime Minister) described by Frost (2002, p. 106), where she stepped in to meet Friedman when Keith Joseph became unavailable at the last moment. Then, five years after Thatcher lost office, she visited the Hoover Institution and there is a poorly composed photograph of her with the Friedmans in between pages 372 and 373 of *Two Lucky People*, but no discussion of the meeting. As to Friedman's impressions of her, after the 1978 meeting he had written to Ralph Harris saying, that she was 'attractive and interesting', but that 'Whether she really has the capacities that Britain so badly needs at this time, I must confess, seems to me still a very open question'.[1] Cockett (1994, p. 173), reported him saying, in 1991, that he had formed 'an extremely high opinion of her'. But still, his judgement seems to have been a later one than implied in Friedman and Friedman.

As to his being asked to instruct the wets, the claim could hardly be less plausible. There was only one present and in any case,[2] the term was only just coming into use and although later adopted as a badge of pride, was initially demeaning. Thatcher's position in the early days of her government was weak, and the threat of her being removed by the Conservative Party was real. So it is most unlikely she used that word of any of her senior ministers in their presence and that of guests. That is all the more so since the reason she left the meeting was that she was facing a vote of No Confidence the following day and had to prepare for it.[3]

There is one other hint in the book of a close relationship with Thatcher. In the course of discussing a recording for a radio broadcast in which he appeared with Edward Heath on the occasion of Reagan's victory in the 1980 Presidential election, Friedman said that Heath was drunk, and being as upset about the result as he had been about Thatcher's success and 'launched into a vitriolic and libelous attack on Thatcher and her advisers, including me—utterly unrestrained and totally lacking in civility' (pp. 390–391). That little allusion to

[1]Letter from Friedman to Ralph Harris, 4 December 1978. Thatcher Foundation Archive, document 117139.

[2]The 'wets' were Conservatives who opposed to or were reluctant about the severity of Thatcher's financial policy. *The Times* of 28 February 1980 reported that Geoffrey Howe, John Biffen, Nigel Lawson, Ian Gilmour, and Patrick Jenkin were at the meeting. Gilmour was emerging as the leading wet. Biffen might later be regarded as one, but not at that time. He was Chief Secretary to the Treasury with a major responsibility for cutting expenditure and as recently as 20 January, the front page of *The Sunday Times* had reported him as incautiously remarking in public on the necessity of 'three years of unparalleled austerity'. Not very wet.

[3]Hansard, 28 February 1980, column 1562 has Thatcher's answer to a question about what she had learned in her meeting with Friedman, where her answer conveys that it was 'nothing' because she left the meeting so soon. On the same day, column 1580 reports the moving of No Confidence—presumably that had not been anticipated when Friedman was invited to the meeting.

Friedman's role with Thatcher has no substance but apart from the paragraph in the text, it is all there is. Campbell (1993, p. 723) did confirm that when Heath 'woke up' he 'let fly', saying that he thought Reagan was too intelligent to accept Friedman's ideas.

In the fuller consideration in Forder (2016) of the question of Friedman's influence, one point that became apparent is that the Conservative Party regarded Friedman's visit to Downing Street as a problem or even an embarrassment, and their position was entirely one of trying to avoid him making damaging remarks in the press, and not at all to hear what he had to say. Perhaps Friedman did not realize that and really thought his advice was valued, or perhaps in *Two Lucky People*, as in the interview reported in Middlemas (2010, p. 150 n11), apparently describing the same meeting, he was just trying to make his role sound larger than it was.

2 Friedman at the Institute of Economic Affairs, and Elsewhere

But although Friedman and Friedman obviously exaggerated his influence on Thatcher, of his wider activities in Britain, which were numerous, and had substantial impact, they said next to nothing. It is difficult to know what a full account of his activities would be, and impossible to give one, but their extent can be indicated just by considering some of his more prominent presentations. The Institute of Economic Affairs published five pamphlets authored by him, between them of some considerable note. The first was Friedman (1970e) describing the monetarist counter-revolution, in the inaugural Wincott Memorial Lecture, and provoking Kaldor (1970) and also leading to Friedman (1970g). Friedman (1974d) described his views on indexation and was reviewed by Oppenheimer (1974), and he took part in another discussion of that matter at the IEA, published as Robbins (1974); Friedman (1975a) was his first clear statement of the idea that policymakers had been misled by the idea of the Phillips curve. Then there were Friedman (1977c), which was his Nobel lecture, also published in the *Journal of*

Political Economy of course, but helpfully republished in the United Kingdom by the IEA; and Friedman (1977e), which is perhaps less noted than the others, but offered a response to some of the views of J K Galbraith. All of those attracted press comment and discussion. In September 1982 he also attended a lunch given in his honour by the IEA—even that was reported in the press.[4]

That was only the IEA. One other notable media appearance was on the 'Controversy' Programme in 1974 debating the causes of inflation in a panel of five economists, of whom he was the only monetarist.[5] Another was his appearance in 'The Jay Interview' with Peter Jay on 17 July 1976. He attracted a great deal of attention towards the end of 1976 for saying that because of her excessive government spending, Britain was on the same path as Chile, and democracy might not survive. In April 1978 he gave the first Hoover Foundation Lecture at the University of Strathclyde. It is a rather badly organized piece expressing guarded optimism that views were shifting towards favouring smaller government. That was discussed on *The Money Programme*[6]; excerpted in *The Listener* (27 April 1978, pp. 526–528) and published as Friedman (1978a).[7] The following year he gave the inaugural Harry Johnson lecture, which was then published as Friedman (1980a). That one, delivered during the 1979 election campaign, was on the relatively neutral topic the law of one price.

In 1980 he first commented in the press about, and then gave written evidence to the Treasury and Civil Service Committee on the Green Paper, *Monetary Control*, which described the Government's plan for controlling the money supply. When it was published, Friedman was

---

[4] *The Times*, 21 September 1982, p. 8.

[5] It was broadcast on 23 September 1974. The other panel members were Geoffrey Maynard, Robert Neild, Peter Oppenheimer, and David Worswick, and the Chairman was Andrew Shonfield. Michael Parkin—a monetarist—had apparently been planted in the studio audience and made a contribution. The transcript of the discussion is in the Milton Friedman Archive at the Hoover Institution, Box 55 file 13.

[6] http://genome.ch.bbc.co.uk/8c40c3c86f2f41faa2b84d73bde13b2d.

[7] *The Listener* was a BBC publication established in 1929 to publish the text of broadcasts, and promote intellectual and cultural events generally. In the 1970s it also published discussions of broadcasts and letters about them.

interviewed from Paris and pronounced it as 'an incompetent piece of work' and said that if it had been written by a student, he would have failed in it.[8] Subsequently, the written evidence to the Committee—Friedman (1980b, p. 57)—he expanded slightly on that saying,

> I could hardly believe my eyes when I read, in the first paragraph of the summary chapter, "The principal means of controlling the growth of the money supply must be fiscal policy – both public expenditure and tax policy – and interest rates". Only a Rip Van Winkle, who had not read any of the flood of literature during the past decade and more on the money supply process, could possibly have written that sentence. Direct control of the monetary base is an alternative …

In January and February 1980 six episodes of *Free to Choose*, were broadcast simultaneously with the British publication of the book. Five of them were followed by broadcast discussions of Friedman's views. That was discussed in Friedman and Friedman (1998a, pp. 499–500), although in just one paragraph, where Friedman also rather strangely said that he had no list of the participants in the discussion but remembered that they included 'at least one minister in the Conservative government and one former minister of the prior Labour government'. No doubt he was recalling, or half-recalling, the discussion broadcast on 22 March with Geoffrey Howe and Denis Healey, the then Chancellor of the Exchequer and his immediate Labour predecessor.[9] The fact that he could not—or perhaps pretended he could not—remember their names is itself very notable, and amongst other things, quite a comment on the idea that he had been any sort of adviser to the Thatcher government.

There were plenty of other, less-noted interventions from the 1970s, and a few in the 1980s, including a televised discussion with Hayek the day before the 1981 budget and some interviews in which he expressed

[8] *The Times*, 20 October 1980, p. 19.

[9] From newspaper TV listings it can be gleaned that the series started on Saturday 16 February 1980 and in addition to Healey and Howe discussing the last episode, the following took part in the discussions, all of which were chaired by Peter Jay: 23 February—Eric Heffer, Lord Kearton, Bob Rowthorn; 1 March—David Ennals, Jack Jones, Sir Hector Laing; 8 March—Nigel Lawson, Neil Kinnock, Maurice Peston; and 15 March—Roy Hattersley, Charles Medawar, Saxon Tate.

his admiration for Thatcher whilst blaming the left of the Conservative party or the obstructionism of the Civil Service for her failure to achieve more.[10] After that his name continued to appear fairly often, though the heat quickly went out of the debate over monetarism, and notable interventions were less frequent. But there were a few. In Friedman (1997) he opposed European Monetary Union, and in an interview with Pringle (2002), Friedman stuck to that view, but admitted—as he had to, of course—to having been wrong in predicting that it would not happen. He said it would be a fascinating experiment, but that if he were British, he would be against joining it (p. 20). That was still his view when interviewed by London (2003) in California—when he added another incorrect forecast, saying, 'Within the next 10 to 15 years the eurozone will split apart' (p. 12). Not everything stays the same, though, as he also said, 'The use of the quantity of money as a target has not been a success' and, 'I'm not sure I would as of today push it as hard as I once did'. As his interviewer observed, it showed that, at 91, he was still engaged, but he wondered what would have happened if Friedman had said the same thing twenty years earlier.

Clearly, there is material there for Friedman and Friedman (1998a) to have said much more than they did about Britain, and much more interesting material than they chose to present about their visits elsewhere. Even that, though, is only half the story, because their not discussing Friedman's relationship with the IEA stands in very clear contrast to what he said about them elsewhere.

Cockett (1994, p. 149), studying the IEA pointed out that Friedman was their 'most celebrated exponent of monetary stability, or what came to be called "monetarism"'. That made him valuable to them. But he also quoted Friedman from an interview in 1991 saying, 'Without the IEA, I doubt very much whether there would have been a Thatcherite revolution' (p. 158), and, of Anthony Fisher, who is usually regarded as its founder, that he was the 'single most important

[10]An interview in the *Observer*, 26 September 1982, p. 24. The headline writer suggested Friedman graded Thatcher as 'four out of ten', but no such comment appears in the interview; *The Guardian*, 12 March, 1983, p. 17 reported a television appearance the day before where he said these sorts of things.

person in the development of Thatcherism' (p. 122). That was a few years before Friedman and Friedman (1998a); a few years after it, there was Friedman (2001), a contribution to an IEA volume incorporating a conversation between Ralph Harris (by then, Lord Harris) and Arthur Seldon, and commentaries on it. Harris and Seldon had been General Director and Editorial Director respectively for more than twenty years and the commentators were asked to assess the 'IEA revolution'. Friedman partly repeated the sentiment from his interview with Cockett, but also made a much more personal comment, saying,

> I owe a great personal debt to Harris and Seldon. For decades they have provided me at the IEA with an intellectual home away from home. Through them I have been able to meet and communicate with individuals in the political community, the journalistic community and the academic community… Under their sponsorship I have been able to talk and publish and to reach the intellectual community in Europe. (p. 71)

And he said that conversations with them,

> altered my own views, and enabled me to clarify some issues better than I otherwise would. (p. 72)

In *Two Lucky People*, there is nothing like that. Harris was described as 'one of the founders of the IEA' which was 'the preeminent free-market think tank in Britain' (p. 475), but except for a footnote about his being Secretary of the Mont Pèlerin Society and being ennobled by Thatcher, the only point of interest was that he had ideas about producers for the British version of *Free to Choose*. Fisher was mentioned once, being described as having 'started' the IEA, but the reason he fitted into the narrative was that on one occasion he was on the same train as Rose—and that is all (p. 271). Still, that is more than what was said about Seldon, who was not mentioned at all, and in the whole book there is nothing else about the IEA in the role of 'home away from home' or any other. One must wonder why that is.

3 Friedman in the British Public Eye

Nor is it just that these activities occurred. They were very widely noted as well, and taking the period up to, say 1990—the year Thatcher lost office—there are probably thousands of British press mentions of him and his views. A clear impression of the intensity of references to him can be taken from Nelson (2009) who identified a very large quantity of discussion of Friedman's views in the press in the course of analysing his attitudes to British policy and some of the people involved in making or debating it up to 1979. The transmission of monetarist ideas, particularly by Samuel Brittan—Friedman's student from Cambridge—at *The Financial Times*, and Peter Jay at *The Times* has been considered and emphasized by Parsons (1989). As Nelson (2009) noted, Parsons' account of attention to Friedman in the press is incomplete. It is a particular oddity that he gave so little consideration to anything in *The Economist*, which, page for page probably gave even more attention to Friedman than either of the others, but nevertheless, even he made it clear that Friedman's impact was considerable.

It might be said that only occasionally were there really in depth discussions of his position. There were some, and there were also numerous reports on what he said in his various presentations, including at the IEA. But in any case, from the point of view of assessing Friedman's standing in the public debate, it is apparent from mentions of him that readers were expected to know the general direction of his thinking. Consequently it is clear that his name was associated with both monetarist, and more generally, pro-market ideas. His name was frequently used simply to characterize such positions, or when discussing some issue such as negative income tax or education vouchers, to add a point of interest by mentioning his support for it. And his status as a personality, as having a recognizable name, is confirmed by more light-hearted mentions of him. *The Financial Times* once carried the headline 'The other Professor Friedman' when reviewing Irving Friedman (1973)—a book on the evils of inflation, which, but for providing the opportunity for that headline, hardly deserved its mention. Friedman's celebrity was also evident in 1990 when the distillers of Knockando paid for an advertisement in *The Financial Times* where Friedman was lined up with Mohammed Ali and the murderer Gary Gilmore—all of them having said memorable things in 1976, and all of which were quoted to emphasize the antiquity of the 14-year old whisky.

It is also surely not a pure coincidence that Friedman (1970e, 1978a, 1980a) were the *inaugural* Wincott, Hoover Foundation, and Harry Johnson lectures. Even though he missed out on giving the inaugural Denis Robertson lecture, organized by Harry Johnson, that became Patinkin (1972), it is clear that he was both very much valued as a speaker, and very willing to speak. That too is a comment on his standing as a celebrity figure in the United Kingdom, and his interest in visiting and making his views known.

So, it is clear that there are important matters missing from Friedman and Friedman (1998a). It is as if there is a whole chapter that has been left out. Indeed, it is a possibility that precisely what happened was that it had been intended that there would be another chapter, but it was forgotten, or as the writing of the book dragged on longer and longer it was just left out.

An alternative explanation may start to emerge from a consideration of the character of what was said about him: it is notable that a good part of the press attention he received was hostile, and sometimes viciously so. The reports arising from Hendry and Ericsson (1983) offer one sample, but there was also an earlier case. It concerned the remarks he made giving rise to the Knockando advertisement. They were remarks to the effect that the British fiscal position, with government expenditure over 60% of national income, was so dire that her situation resembled that of Chile under Allende and as he put it in Friedman (1976b, p. 9), 'I fear that the odds are at least 50-50 that within the next five years British freedom and democracy, as we have seen it, will be destroyed'. That version came from November, but Friedman's views came to attention slightly earlier since he said on *Meet the Press* on NBC on 24 October that Britain offered 'another horrible example' of fiscal crisis and 'government spending has reached 60 per cent of the national income. Britain is on the verge of collapse'. In Britain, that was reported on the front page of *The Times* the following day.

The reason he gave for expecting collapse was that such levels of government expenditure were too high to be financed without inflation, and that uncontrolled inflation would lead to some form of coup. He specifically drew a parallel with Chile, saying that the welfare state had been created there at about the same time as in the United Kingdom, and that it had grown inexorably in both countries. Chile, being poorer had reached the point where it could not be financed by taxation at an earlier stage but he said that what he had observed in Chile was similar to and reminded him of what he had observed in the United Kingdom.

The claim that government expenditure was 60% of national income arose from Official statistics, but was very quickly shown to be incorrect. There had been inconsistent methods of accounting, inclusion of some inappropriate items, and some double counting. The corrected figure was 46%. Although the error, if not the precise calculation was very quickly reported,[11] Friedman repeated his view in a BBC interview (recorded in Chicago), broadcast of 9 November, and then summarized in *The Listener* (18 November 1976, pp. 632–633); and again at the end of the month on *60 Minutes* on CBS, he said Chile offered 'a very pertinent example' for what was happening in Britain and that if she continued on the same course 'it will mean the end of democracy'.[12] Those views were again discussed when he appeared, debating with Lord Balogh on *Panorama*, in early December,[13] and when he appeared on *The Money Programme* on 10 December.[14]

The whole incident occurred at just about the time of the announcement of Friedman's Nobel Prize, and that also brought attention to his role in Chile. So the Prize, the controversy over Chile, and his choosing to compare that country with Britain were all more or less simultaneous. It is very clear from Friedman's repetitions of the point about Britain that he welcomed the attention it earned him and it is possible

[11]There was a sophisticated discussion of it by Peter Jay in *The Times*, 28 October, 1976, p. 23 and fuller one, with the corrected calculations in Pliatzky (1982, pp. 161–168).

[12]CBS, *60 Minutes*, from the previous night, reported in *The Times*, 29 November, 1976, pp. 4, 25.

[13]*The Guardian*, 7 December 1976, p. 8.

[14]Both programmes were high-quality current affairs programmes, the latter specializing in economic and financial matters.

that it was this, as well as what he said about his involvement in Chile, that led Paul Samuelson to say to *The Sunday Times* (12 December 1976, p. 17) that 'Milton is the most naïve fellow in the world'. In any case, the stance he took did him no good at all.

Despite the fact that several others had questioned the sustainability of British democracy,[15] the reaction to Friedman's remarks was very hostile. *The Times* (3 December 1976) reported on one of the American broadcasts and commented that, 'Enlisted as a one-man Greek chorus, Professor Milton Friedman, the current Nobel laureate in economics, portrayed a nation headed for anarchy or some unspecified form of despotism. Chile, he suggested—Chile! Is the minatory example… was there not a note of satisfaction in his tone?'

Amongst further press comments on these remarks, three from *The Guardian* are notable. First, Jenkins (1976a) described him as a 'Nobel economist and prize political fool', then Jenkins (1976b) called him 'the Chicago charlatan', and then a Guardian editorial (30 November 1976, p. 14) very possibly written by Jenkins, said, that like a jack-in-the-box,

> if you put that cantankerous old bigot, Professor Milton Friedman, on America television and invite him to talk about Britain, he will reel off the doom and gloom with unflagging enthusiasm until he is put back in the box.

That was extreme, and resulted in some letters to the paper saying that they had fallen below their usual high standard, etc., but the paper's attitude to Friedman is clear enough.

Commonplace as the idea of a coup in Britain may have been, Friedman's enthusiasm for making the point can hardly have won him friends and the fact that he put it in terms of the incorrect '60%' figure exposed him further. Dennis Healey, Chancellor of the Exchequer at

[15]Friedman (1976b) himself said Eric Sevareid had compared Britain and Chile. Sandbrook (2013, Chapter 6) devoted a whole chapter to the matter, though as is his way, much of it was taken up with discussions of fictionalizations of the idea, such as Deighton (1978). McIntosh (2006, p. 176) discussed it; Lord Robens said to *Newsweek* (21 October 1974) that Britain was heading the way of the Weimar Republic, and the consequence would be a dictatorship of right or left.

the time said, 'So the picture of a profligate public expenditure as ignorantly presented by Professor Milton Friedman and fatuously echoed by the Conservative Party bears no relation to the facts'. And Reginald Maudling, one of his Conservative predecessors in that Office, said Friedman had been 'been dancing on the grave of Britain with all the uninhibited freedom of the truly ignorant'.[16] They were right, of course.

Further insight along the same sort of lines comes from reflections on another debate in the newspapers. Hahn and Neild (1980a) criticized monetarism in *The Times*; Friedman (1980c) responded, taking the opportunity to advertise *Free to Choose* by saying it contained evidence on the relationship of money and prices. That allowed Hahn and Neild (1980b) to reply to criticize—quite justifiably—the quality of evidence in that book. Noting the absence of sources and the inadequate econometrics, they drew attention to Friedman's own complaints that others failed to state their evidence and sources properly. That debate was something of a prelude to the letter to *The Times* (30 March 1981) the following year, organized by Hahn and Neild, and signed by 364 economists, saying there was 'no basis in economic theory or supporting evidence' for the monetarist policy then being followed. The letter itself did not mention Friedman, but Neild (2014, p. 4) reflecting on it, said that one of the reasons so many had agreed to sign was that 'Friedman was behaving as a charlatan'. That, although not quite said, was indeed the implication of Hahn and Neild (1980b) and Neild, as a principal academic opponent of Thatcher's policy was probably right in how he judged the motives of those who signed the letter, and hence how Friedman was perceived in Britain.

The response to *Free to Choose* was not so hostile as that, but there were some signs of the same thing. Numerous letters appeared in *The Listener* protesting about it. Robert McKenzie, who had chaired discussions after the American broadcasts, but was based at the LSE, wrote that although Friedman was 'a most skillful proponent of his own ideas, he is also as dogmatic in advancing these ideas as any Marxist', and 'He seeks out and uses facts and illustrations which he believes demonstrate

[16] Hansard, 30 November, 1976, Columns 715 and 742.

the validity of his own ideas… he simply will not entertain any evidence which appears to conflict with his own a priori judgment', and citing one particular example of facts contradicting his view, 'he simply declined to consider the evidence'.

Just before that, Jay (1980) had been written to address the controversy over whether *Free to Choose* should have been broadcast at all being, apparently, deeply ideological. He made a number of points, including that the discussions after the programmes created an excellent opportunity to challenge Friedman's views. On that, he said that many economists had refused to appear after the 'merciless, if not wholly fair, drubbing' Friedman had given the participants in the Controversy programme in 1974 and the result was that their discussions were with people who knew less economics, but were better debaters. Of that, he said Friedman was 'taken aback by the vigour of the debating attack on him' and 'As a superb debater himself, he came to see what an easy ride he had had in the American debates and that it was not unfair to be confronted with some of his own medicine.'

4 Conclusion

It is not clear how much Friedman ever knew of the abuse he received in 1976, but it would be surprising if he had no idea. And in any case, he had made himself foolish with the '60%' claim about government expenditure, and the suggestion that there was a 50-50 chance of a failure of democracy might have been a good way to attract attention, but it was not the outcome of an intelligent analysis, nor did it reflect a sympathetic understanding of the subject matter on which he chose to opine. Whatever he knew of the abuse, quite possibly he knew he had made himself foolish. Then there seems every reason to think he was surprised by the hostility and vehemence with which *Free to Choose* was greeted. Jay was more diplomatic than McKenzie about Friedman, but he made evident Friedman's surprise. And then just a couple of years later, there was the issue over Hendry and Ericsson, and that was certainly unpleasant for Friedman.

So one might speculate that by 1980 or 1981, Friedman would not have been quite so happy to say, as he did in Friedman (1970e, p. 7), that ‘Coming back to Britain, as I am fortunate enough to be able to do from time to time, always means coming back to a warm circle of friends or friendly enemies’. He might have felt the environment much more hostile to him, and hostile in something of a belligerent and insulting way. It may be reading a lot into a detail, but when asked in an interview by Snowdon and Vane (1999, p. 143) about the need to be thick-skinned when advancing controversial views, Friedman replied,

> I don’t think the question is one of having a thick skin. I think the question is one of belief in what you are doing. Conviction is strong. I have never been bothered by intellectual attacks … With very rare exceptions, I never had any personal problems.

Did he mean to contrast ‘intellectual attacks’, with which he had no problems, and those rare personal attacks? Perhaps what he meant was that eager as he was for controversy, personal attacks were another matter, and there his skin was not nearly as thick as it needed to be if he were going to let the readers of *Two Lucky People* in on the story of what the Jenkins, Healey, Neild, McKenzie, Huhne, and even David Hendry thought of him.

Added to this, there is of course the point that whilst there certainly is a story to be told about Friedman’s exploits in Britain, it is not the story he would surely have liked to have told. That would have been a story of his closeness to Margaret Thatcher, of her hanging on his every word, and—preferably—of following his advice. But he was not a close adviser to Thatcher. One need only recall the earnestness with which he sought to distance himself from Pinochet. But it was Pinochet, not Thatcher, who asked him for a letter of advice at the one meeting he had with each whilst they were in Office. Perhaps, for the Friedmans, it was easier just to ignore the whole matter and write about other things instead.

6

Part I Conclusion

Biographies sometimes leave the reputations of their subjects much more tattered than they were, but it is rare for autobiographies to do that. It is all very well that the authors said they were not setting out to write an intellectual book, but it is far more deadeningly unintellectual than that suggests. And in any case, even that story was not consistently told, since in Friedman and Friedman (1998b, p. 11)—an interview presumably intended as marketing for the book—Friedman described it as 'a book starting out as a love story that will end up as a treatise on social science'. It ended up as nothing like that.

It harms its authors' reputations first of all because it is not by any stretch a good book. As a document of the authors' public lives, it is unreliable; as an account of their inner lives, it is a desert; as an entertainment, it fails through being consistently humourless, occasionally mean, and inconsistent as to pace and temperament. There is far too much of it generally, and far too much of what there is turns out to be utterly inconsequential. The authors' prejudices come through, but not anything to give them intellectual sustenance; and most of all it is utterly lacking in self-awareness. Set it beside Galbraith (1981) or Stigler (1988)—to take two obvious comparators—and it will not fare well on any score.

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_6

Indeed, in its meanderings, it is not really a memoir at all. It is as if was composed as verbal scrap book to aid the authors' private reminiscences. Their adventures together are there, and their reminders to themselves about how everything would have been if it had been as it should have been. In its propagandizing, it hardly scores a point, but for the most part does not look as if it is meant to be read. Perhaps it is meant to be more like a display trophy for Friedman's fan club; something for them to own and declare to be the encapsulation of the great man's life and work. It is a campaign medal, for those who fought Friedman's campaign against big government. No need then for Hendry or Huhne, no need to recall Britain. Perhaps even his reluctance to support Papandreou would fit in.

Whilst there is desperately little in the character of an argument about any point of economics, it is not the same with certain difficult issues in Friedman's life. The case of Chile is outstanding. Not everyone will agree with his position, but the account given is a full one, and from Friedman's point of view, the facts are entirely satisfactory. That is not quite true over the publication of Friedman and Kuznets (1945). The facts seem not quite satisfactory from Friedman's point of view, and his presentation of them is not from the reader's. Noyes was not fairly presented, and we now have the testimony of Mitchell to say that his arguments were seen in the NBER, and by Friedman, to be important. Nevertheless, there was more indication here of a willingness to treat there as being an issue to discuss. That was also true of the matter at Wisconsin.

And then there is the matter of Hendry and Ericsson. Whatever the answer may be, Friedman does not let his audience in on the question at all. There, the deception is of a different kind, and Friedman's readers are not allowed to see the difficulty he was in. And of the whole of his engagement with British debate, there is nothing. Surely the facts that he did not get the reception he wanted, and could not present himself as the battling hero there that he thought he was in America must be part of the reason. The hostility, the tone of challenge, and the outright rejection of his views were, I suppose, not things he was well-equipped to handle, however much he presented himself as at home in controversy.

There is one more aspect which deserves comment, and that concerns the response of the reviewers to *Two Lucky People*. They see its stylistic weaknesses, though they are not all as blunt as they might have been about it. But they also ignore the book—or ignore everything after the first 150 pages or so—and simply tell their own story of Friedman's brilliance. Breit (1999) and Schwartz (1998) are both admiring, but though they make some kind of remarks about the book, they are admiring mainly of the author. Actually, they give very good short accounts of Friedman's life and work, but in each case, some of their material simply could not have come from the book. Brooks (1998) was kind too, whilst showing a degree of greater acquaintance with the book, when he said it was like receiving a Christmas card from superachieving friends: 'Met some wonderful people in Japan this past summer, found mice in the vacation house, transformed the nature of economic thought'. But he was too kind, twice over. For one, superachieving friends care about what happened to their sisters; for another, whilst the first two parts are there, the third is really not. The reader is supposed to know that Friedman transformed economic thought; and anyone who doubts it is not expected to be a reader at all.

Cross (2001), then—a British reviewer, as it happens—stands apart from these in seeing the intellectual limitations of the book, and the fact that almost throughout, Friedman did nothing to argue his case, but rather presumed it was evident. Cross, thought that the result would be that others would 'pound their laptops with greater energy for what is revealed and unrevealed in the Friedman memoires' (p. 55). It is surely a point of interest, and rather a mysterious one, that no one else even saw the limitations of the book, let alone fulfilled that prophecy. Here, somehow, it seems as if Friedman's statement of the position was for some all that was wanted; and for others, presumably, not of any interest at all.

Part II

Milton Friedman's Economics, 1935–1957

7

Part II Introduction

In the first twenty years or so of his publishing career, Friedman wrote some papers on statistical theory; an assortment of sometimes penetrating book reviews; two books already noted—Friedman and Kuznets (1945) and Friedman and Stigler (1946)—papers on the theory of demand and some contributions to utility theory, including most famously the 'Friedman-Savage utility function'; an idiosyncratic essay on Marshall's analysis of demand; a small collection of more theoretical work in the shape, for example, of (Friedman 1951/1953, 1952a, 1953f), a little-noticed, but powerful and distinctive essay on the effect of trade unions on wages; a famous essay on flexible exchange rates, and another one on methodology, and then a revolutionary book on the consumption function the year after his famous essay on the Quantity Theory. It seems something of a miscellaneous collection, although impressively wide-ranging. Indeed, it is impressively wide-ranging—quite possibly more so than is often recognized. On the other hand, it is not quite so miscellaneous as it seems. If we look at his work broadly, rather than to what have become his famous works, notable patterns emerge—also, very probably, more than is often recognized. In these years he pursued clear lines of enquiry, in most cases it is easy to see

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_7

why he became interested in those areas and at least to reach conjectures about how one thing led to another, so that the areas of interest are to some extent more closely linked in his thinking that they appear.

Another point, though, is that much of it exhibits a methodological alertness which is itself of some interest. Sometimes he declares it plainly; sometimes he keeps quiet, but nonetheless it is plain, and very often there is some clear hint that methodological questions were in his mind as he wrote. The methodology essay itself—so I shall argue—is worthless, but dramatic as was his demonstration that he could not develop his ideas in philosophical terms, it does not follow that he had no clear programme of scientific action, or that he could not explain the steps of that programme as he took them.

So, I first consider the not-so-miscellaneous works in Chapter 8, Friedman (1957a) in Chapter 9, and although it is slightly earlier, since methodology is so much a unifying theme of Friedman's work in the period, Friedman (1953b) comes last.

8

An Early Miscellany?

1 Statistics

Friedman's own principal contribution to statistical theory came in the shape of a test of the similarity of alternative rankings in Friedman (1937), and consideration of its use in Friedman (1940b).[1] That test came to be called the 'Friedman test' in textbooks and software packages and much later merited an encyclopedia entry in the form of Jensen (1985). During the War, he was evidently a leading member of the Statistical Research Group. Some of the fruits of their work were the seventeen papers by nine authors published in Eisenhart et al. (1947)—amongst them were Friedman (1947a, b), and Friedman and Savage (1947). Following his controversial report on the teaching of statistics at Wisconsin, he joined a committee which produced Hotelling et al. (1948), and then he was, along with Allen Wallis, one of the four authors of Freeman et al. (1949). That was really just a textbook, or even a practical manual, as its preface said, and Condon (1949) and H. D. Wolfe (1949) confirmed, whilst also expressing their appreciation

[1]Along with an important typographical correction to the former in Friedman (1939a).

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_8

of it. Later there was T. W. Anderson and Friedman (1960), developed from some of the work he did at the Statistical Research Group, and a few other papers where some standard piece of statistical thinking was put to work in making a sharp—and largely unnoticed—point in economic analysis.[2]

There is more to his contribution to statistics than just his own publications, though. One is that with Wallis, he devised the idea of sequential analysis, which came to fruition in Wald (1947). The point of the approach is that when repeated testing of a hypothesis is expensive (in time or resources), it can be wasteful to have a predetermined sample size. It might be that early runs of the test provide decisive results. The problem was therefore to devise tests providing results with the same statistical properties as existing tests, but also with a rule determining when the process could be stopped. Early development of the idea, including the role of Friedman and Wallis, is described in Wald (1945), which also reports that the idea was seen as having such military importance that the National Defense Research Committee restricted its publication. Their role is made clear again in Wald (1947) and in Wallis (1980), who also said he and Friedman were nervous about getting credit for it, also saying (p. 325) that Friedman thought neither of them might ever have as big an idea again—another bad forecast! Still, the importance of the idea is clearly indicated.

To add to that, there is also a remark of Savage himself. As well as being a collaborator with Friedman, he was a noted statistician, particularly for Savage (1954)—always highly regarded and sometimes thought revolutionary. His standing is plainly apparent even in the measured language of Lindley (1980). Mosteller (1981) referred to Friedman's influence on him, but what is most notable is Savage's (1976, p. 441) own comment when he called Friedman and Wallis 'my statistical mentors'. It is quite a tribute coming from one of Savage's standing.

So although statistics turned out to be no more than Friedman's second string, if it was even that, in twelve years near the beginning of his

[2]The most notable being Friedman (1951/1953), to be discussed below (p. 265); but there were Friedman (1974e, 1992b) as well.

career, he made quite a mark, with several well-respected publications, a test that carried on being used decades later, an initial insight producing research of great importance, both in its immediate military context, and after that, and clearly earning the respect and appreciation as a teacher of one of the renowned figures of the time.

2 The Theory of Demand

Friedman (1935a) was actually his first publication and as such takes some colour from its being a criticism and correction of Pigou (1910). Pigou was accused of ambiguity in expression, with one interpretation leading to error, the other to redundancy. Pigou (1936) responded, saying Friedman had misunderstood him, to which Friedman (1936b) replied that he had not, and meanwhile, Georgescu-Roegen (1936), stimulated by the exchange, commented at more length to the effect that Friedman's position was unconvincing in various ways. It was also as Friedman said (p. 159 n9) the outgrowth of the work that became Schultz (1938), on which Friedman was employed as research assistant, and very possibly more specifically inspired by Schultz' own commentary on the work of Pigou and others in Schultz (1933). The substance and true objective of the paper concerned the possibilities for using budgetary data to estimate demand elasticities and whilst it was a mathematical argument (one of some sophistication for its time), it aimed firmly at establishing the possibilities for using data to answer empirical questions about the responsiveness of demand for commodities to changes in price and income. He later wrote Friedman (1936c), an entirely mathematical note on demand and Friedman (1938), rejecting an idea of Broster (1937) on the estimation of demand curves.

An article on a clearly related matter, but of a rather different type is Wallis and Friedman (1942), a contribution to the memorial volume produced after Schultz' early death in an accident just months after the publication of his book. The book was very highly regarded—Bowley (1939, p. 213) made that clear saying it was 'necessary' that students became familiar with it; Hotelling (1938a, p. 744) said its goal of synthesizing theory and empirical studies 'so necessary for the development of economic science, is accomplished beautifully'. Friedman had worked on it as research assistant, but he and Wallis made it the occasion for making an important theoretical point. Drawing attention to Schultz' deployment of the theory of indifference curves, they began their paper saying (p. 175),

> Although the indifference function has become the keystone of the theory of consumer choice, empirical workers in the field, even those most thoroughly familiar with the niceties of indifference analysis, have ignored it or, in a few instances, dragged it in as a more or less irrelevant afterthought. Thus Professor Schultz, despite his brilliant presentation of indifference analysis in The Theory and Measurement of Demand, finds almost no occasion to employ it in the statistical portions of the book.

That point was noted by others—Bowley (1939, p. 214) also questioned whether the theory was what was really motivating the statistics, but Wallis and Friedman had much more than that to say. Their point was that though it might seem indifference curves could be estimated either experimentally, by survey-based enquiries, and the like; or statistically, neither was in fact successful. This led, in a section of the paper principally attributed to Friedman, to a consideration of indifference curves as a framework for empirical analysis. The authors said that the difficulties in estimating indifference curves arose, not from limitation of data or technique, but from the character of the problem. Conceptually, indifference curves brought together three elements—the consumption bundles that might be chosen; the consumer preferences; and 'opportunity factors'—their budget. But empirically, they could not be separated. They said (p. 186),

> From one point of view, family composition seems to be an opportunity factor, somewhat similar to income. A husband and wife with a given income, for example, are 'better off' than a husband and wife with six children and the same income. From another point of view, however, family composition is a taste factor. Baby carriages, to cite an illustration, play a different role in the indifference systems of childless couples than in those of families with infants. Finally, family composition may even be treated as a good; for the satisfactions of an additional child are likely

to be compared with the expenses incurred, and considerations of 'price' may play a not inconsiderable role.

The three kinds of consideration, could not, in other words, be empirically separated. Yet, observed the authors (p. 187), precisely the appeal of the indifference analysis was that it appeared to separate those things. It is a good point, much neglected. On the other hand, the authors clearly thought that the limitations of the indifference curve analysis presented no fundamental problem because the questions concerning the relation of expenditure to income, prices, family size, location, and the like could readily be addressed without them. It is also an interesting early highlight in Friedman's biography, both because arguments with that kind of motivation—the motivation of seeking to identify the empirical matters—would feature again and again, but also because here he was—age 30 at the time—stepping aside from a major work of one of his mentors, and making a profound criticism of the basis on which the work had been presented.

3 The Marshallian Demand Curve

Friedman's essay on 'The Marshallian demand curve'—Friedman (1949b)—was a much more determined foray into the history of economic thought than almost anything else he wrote. Its ostensible objective was to argue that in Marshall (1890) and later editions, the 'demand curve' was intended to be understood as showing the relationship of price and quantity demanded of a good, holding real income, not the prices of other goods, constant. It was a 'compensated demand curve' as Hicks (1956) called it slightly later.

It was a long and detailed essay—23,000 words, including footnotes and appendices. Most of Friedman's analysis was based on a very close reading of Marshall in its various editions, and resulted in some finely drawn points. Whatever the ultimate merits of his conclusion, there is no denying the precision of Friedman's thinking about the questions, nor the quality of his analysis of just what Marshall said and might have meant at various points. It is a difficult essay too, with its constant attention to detailed theoretical reasoning and textual sources, but also a very well written one.

The crucial issue concerned what Marshall had meant by the assumption of 'unchanging monetary conditions'. Friedman relied on Keynes (1925) to argue that despite the publication dates, the monetary theory in Marshall (1923) was complete well before Marshall (1890). That meant, he inferred, that Marshall had meant that the overall price level had not changed, so that if one price had, others must have moved in the opposite direction, and the demand curve was drawn on that assumption. That was not the only courageous conjecture that had to be made along the way, but the two big difficulties were dealing with the introduction of Giffen Goods in Marshall (1895), the third edition, and the fact that Marshall himself seemed to accept the account of his thinking that Friedman said was incorrect.

The former is a difficulty because their existence depends on the Hicks/Slutsky 'income effect', which Friedman was in effect saying could not be represented in Marshall's theory. Friedman conceded that there was at least a line in a mathematical appendix of the third edition that would be incorrect on his interpretation. Of that, he said that the point Marshall was addressing there was a subtle one, and so its inconsistency with Friedman's view should not be given much weight; whereas the alternative interpretation would force one to see Marshall as in error on more basic points, and hence on balance the matter clearly favoured Friedman's view.

In addition to his textual analysis, Friedman also raised the point, quoting Marshall (1885, p. 190) that he had thought economic theory should be 'an engine for the discovery of concrete truth'. That led him to argue, for example, that the compensated demand curve was generally more useful. That was because the constancy of real income made for a simpler analysis than one in which the demand function for a particular good would have to include the prices of all other goods, or where shifts of the demand curve would have to be introduced to account for changes in real income.

The combination of the two lines of reasoning led to a certain degree of ambiguity in exactly what Friedman was saying. The close discussion of the text has the feel of arguing that Marshall intended his position to be the one Friedman described. But elsewhere, the implication seems to be that Marshall had not clearly appreciated that there were distinct possibilities. When Friedman came to deal with the question of Giffen goods, part of his response was that Marshall had failed to recognize the contradiction because he had himself been influenced by others' incorrect interpretation of his work (pp. 487–488). Although Friedman sought to explain that by the long lapse of time between the original drafting of the basic theory and the introduction of the Giffen good, it must point at least to a certain fogginess in Marshall's mind about the point, and therefore that he had no clear intent, one way or the other.

A further point of note, though, comes immediately after, when Friedman pointed to what he regarded as 'Further circumstantial evidence' that Marshall did not see the contradiction between the two views. That was that the one Friedman regarded as incorrect, was made explicit as early as Edgeworth (1894), and that Marshall made no effort to correct the misinterpretation of his theory. Why would he not have corrected it, asked Friedman, if he had seen the contradiction? Indeed, but another question would be whether his failure to 'correct' it might not perfectly well be evidence that Edgeworth's reading was the correct one. But the thought that this circumstance might be evidence against Friedman's view was not considered—it was treated just as evidence in favour of a subsidiary hypothesis Friedman introduced to avoid a difficulty with his theory.

For the most part, though Friedman's scholarship was intense, he does not seem to have persuaded many. The idea of the superiority of the compensated demand curve was not really new—Friedman quoted Knight (1944), from whom he must have learned it, saying the same sort of thing, and Patinkin (1973, p. 794) said it was also a view taught by Viner. Patinkin also seems to say that Knight (1944, 1946) had also argued that Marshall should be read as describing such a curve, and that seems not to be correct. On that idea, though, Stigler (1950, p. 389) pointed to an inconsistency raised by Friedman's view, and Alford (1956) criticized it in detail. In the longer term, whatever the merits of Friedman's case, nothing much came of it, as the 'Marshallian' constant money income demand curve continues to be contrasted with the 'Hicksian' compensated demand curve—as it is, for example, in no lesser authorities than Gravelle and Rees (1992), or Mas-Colell et al. (2005).

4 Book Reviews

Early in his career, Friedman wrote a number of book reviews, many of them being entirely worthy but, naturally enough, not terribly weighty. There are others, but as a sample, in Friedman (1935b) he approved Kuznets (1933); in Friedman (1936a) he criticized the statistics in Blodgett (1935) and in Friedman (1939b) said that there was nothing new in Leven and Kathryn Wright (1938) but that it was quite suitable for beginners. Later in Friedman (1948b) he evidently and correctly regarded Dewey and Dakin (1947) as quackery. Friedman (1961d) savaged Wilson (1961) for his neglect of monetary issues in the explanation of inflation, particularly in his discussion of cost-push considerations. Later, there were fewer—Friedman (1987d), expressed interest in and appreciation of Sargent (1986), whilst being more optimistic than the author about the consequences of Reaganomics, and there was Friedman (1996), an inconsequential review of Groenewegen's (1995) 800-page biography of Marshall, consisting mainly of remarks about what Friedman had long believed, mainly about Marshall's private life and views, rather than his economics, and long quotations from the book.

That is all routine enough, but there were other reviews of much more importance from the early part of his career. One was Friedman (1941a) in which he reviewed Triffin (1940) and two very slightly later ones. One was Friedman (1946), a long review of Lange (1945), and the other was Friedman (1947c), a review of Lerner (1944). The latter two were reprinted in Friedman (1953c).

Triffin's book was the publication of his doctoral thesis and intended to carry forward the line of thinking of Chamberlin (1933), his supervisor, on imperfect competition. So it set out to achieve realism in the theory of the firm, and to bring imperfect competition together with the idea of general equilibrium. Its emphasis on the various sorts of industrial structure that might occur meant that 'monopoly' and 'perfect competition' were both devalued as descriptions of any actual industry. And, as Triffin saw it, since the closest competitors a firm faced were not those producing something with 'similar' physical characteristics, but rather whichever things consumers might buy instead of its product, analysis should dispense with the idea of an 'industry' and focus just on the firm. The taxonomic aspect in Triffin was sometimes criticized—it was taxonomy for the sake of taxonomy thought Knight (1941), invoking Veblen (1898) or possibly Veblen (1908). Kaldor (1942) saw merit in Triffin's project but also said that Triffin never moved beyond definitions and classification—'Of a theory of monopolistic competition there is very little to be found…', he said (p. 412).

Those thoughts are consistent with Friedman's, but he went much further. He said that Triffin's book was valuable and thought-provoking, and he accepted as inescapable the conclusion that the theory of monopolistic competition offered no tools for analysis of the 'industry'. But, instead of Triffin's conclusion that that concept should be eliminated and analysis focus either on the firm or on general equilibrium, Friedman questioned his whole project, saying of himself, 'The reviewer deduces that monopolistic competition adds little to our box of tools other than a refinement of Marshall's monopoly analysis' (p. 390). Real world problems, he said, relate to industries, although the exact definition of an industry would depend on the context of the enquiry. In Friedman's view, the theorists of monopolistic competition were mistaken in criticizing classical economists for being concerned with perfect competition, whereas they were concerned with 'the kind of competition that prevails in the real world'. In connection with that he cited seven separate pages of Marshall (1890) and in conclusion said that the absence of a concept of the industry was a limitation of monopolistic competition and the Marshallian tools had more value.

At about 700 words, that was not a long review. Friedman's reviews of Lange and Lerner a few years later were much longer, but similarly concentrated more on presenting Friedman's view of the proper approach to the problems than assessing the authors' work on their own terms.

Lerner was certainly concerned with practical problems of economic management and has often been regarded as a fine logical reasoner and innovative theorist, albeit that, particularly in his earlier work, he had something of a utopian attitude to the implementability of his ideas. All those things are evident in Lerner (1944), and they were all clearly identified by Friedman. The aspect of Friedman's review that subsequently attracted most comment was that he said Lerner was wrong to argue that moves towards income equality were more likely to raise than lower overall welfare. Lerner had no basis, said Friedman, for presuming an equal capacity of individuals to enjoy consumption—some might be more efficient 'pleasure machines' than others, in which case the objective of maximizing total utility would point to the desirability of greater inequality. It was, Friedman observed (pp. 410–411) possible to think that achieving equality was actually being treated as a more fundamental objective than the maximization of total satisfaction.

In terms of the content of the review, though, Friedman devoted much more space to a discussion of how hard it would be to treat Lerner's ideas as creating a programme for action. Arguing more fully but to similar effect as the much briefer review by Stigler (1945), Friedman saw that on a theoretical level Lerner had laid out the conditions of optimality but on the practical level of what was to be done about it, Friedman thought the book amounted to little or nothing more than a series of admonitions to do the right thing. For example, he accepted that optimality required the equalization of marginal costs and benefits, as Lerner argued, more innovatively that might later be imagined,[3] but said that Lerner gave no real indication as to what was to be done to achieve this in non-competitive industries. For example, Lerner suggested that government intervention could be organized to induce marginal cost pricing, but as Friedman noted, the information requirements were prodigious. Similarly, Lerner reprised his discussion of 'functional finance' from Lerner (1943). That idea was lauded by some as a balanced approach to government borrowing. Indeed, Lerner might say on this and other points, that part of his objective was to shift views away from an ignorant insistence on principles, such as that of the balanced budget. He equally wanted to discard anti-market dogmas of the left. The book was intended as a rational tract for an ideological time—its central problem was to describe the best balance of the market and control. Friedman does not seem to have

[3]Lerner had stated the arguments in the 1930s, but restated them in this book. Surprising as it seems, Samuelson (1964) said that until Lerner explained it, in the 1930s no one at Harvard or Chicago could give him a clear account of why marginal cost pricing was socially desirable; and Scitovsky (2008) also had him giving the first clear statement of this point.

appreciated and did not acknowledge the value of that objective. But in any case, he was one step ahead of the argument. Acceptance of the point that different fiscal positions are appropriate to different circumstances takes us nowhere in terms of determining what circumstances exist and what policy is appropriate to them. And on those rocks, said Friedman, Lerner's ideas foundered.

It is an interesting review because Friedman clearly admired Lerner's analysis. Most of his appreciation was crammed into the last couple of paragraphs of the twelve pages of the review, but it was there, and there is no sign of its being insincere. Except for the question of what could be said about the desirability of equality, his concerns about the book were entirely with the question of whether it indicated actual policy responses, or pointed to ways in which policy could in practical terms be designed—it was a pragmatically motivated criticism of what he thought an insufficiently pragmatic book.

Lange's book was for the most part an attempt to establish the conditions under which price flexibility would ensure full employment—a crucial question in Keynesian economics. In a sense it is a further consideration and elaboration of Keynes (1936, Chapter 19), and sought to be as general as possible. With that objective, unsurprisingly, the answer turned out to be that sometimes there would be full employment, sometimes not. The answer depended mainly on monetary effects, the conditions of competition, and the formation of expectations, and Lange took the view that it was only in rather special cases that full employment would emerge.

The book was generally admired for its determined tackling of what appeared to be an important problem, and its author's detailed analysis. So criticism, for the most part, such as that of Harrod (1946) or Timlin (1946), engaged with the book on its own terms, questioning specifics of the assumptions made. Friedman's article-length review, however, took a different approach. He too admired Lange's technical proficiency, but he also felt the project misconceived on methodological grounds. Lange, he said, was clearly a master of 'taxonomic theorizing', and because the book was so good in that way it was 'a good text for a methodological sermon' (p. 613). And what a sermon it was.

Friedman argued that a scientist could proceed in either of two ways. One was to start with observed data and seek to make generalizations about it, test that theory against other data, and revise the theory in the light of those tests. The other, which was the approach of Lange, was to dispense with the matter of assembling data, and instead focus on the logical interrelations of the model. The crucial difference, said Friedman, was that in the first, but not the second case, the issue arose as to what observations would contradict the theory and so models like Lange's provided 'formal models of imaginary worlds, not generalizations about the real world' (p. 618).

As Friedman saw it, Lange's ambition was to construct a completely general account of the macroeconomy and thereby identify the conditions under which full employment would be achieved. The project failed because the impossibly large number of cases that would have to be considered forced theoretical simplifications on Lange and this meant he did not in fact achieve generality. More than that, though, the need to simplify the analysis led him to make apparently empirical claims for which no evidence one way or the other could be presented.

So, for example, Lange could not consider discontinuities or lags; he ended up with ideas such as that of a 'neutral' monetary response, which was defined as one which had no effect on employment in response to a shock, but beyond that, could not be empirically described. He was forced into simplifications based on what Friedman called 'casual empiricism' (p. 624), such as declaring that certain outcomes, such as multiple equilibria, were unlikely to occur and so could be disregarded. Friedman pointed out that Lange had no evidence on that point and emphasized that in all such cases the problem was not that Lange's claims were incorrect but that 'there is no way of telling whether they are right or wrong' (p. 624). Similarly, Lange disregarded mathematical 'special cases'. Again, Friedman said that it might be that of all the functions satisfying Lange's assumptions, only a small proportion gave rise to certain outcomes, but that was no basis for disregarding the possibility that they were the empirically relevant ones.

Lange's ambition to achieve generality, however, confronted a desire also to be realistic. That, thought Friedman, led him to further errors. An exemplar was the introduction of 'friction', which had no true place in the theoretical system, but was put to use in making arguments appear plausible. A second was his treatment of uncertainty and the formation of expectations. Here, said Friedman, he abandoned generality and made more specific claims which could be confronted with data. And, thought Friedman, if Lange had considered a wider set of implications than he did, he would have found them contradicted. That possibility of contradiction was, according to Friedman, an essential of worthwhile theory, and although when it came up, it was to say that Lange's approach would be contradicted, the wider point was that most of his theory, being taxonomic, did not face that possibility.

Johnson (1951) seems to have picked up Friedman's criticism of the 'taxonomic approach' and used it as one strand of very critical review of Meade (1951). Day (1955) responded to Friedman and Johnson, though focussing particularly on Friedman, as the better-executed version of the argument. Noting the quality of Friedman's argument, and accepting that he had made the case that there were significant dangers in taxonomic analysis, Day argued there was still a role for it. He distinguished the taxonomic approach per se from the point that hypothesized relationships may not have empirical counterparts and argued on various instrumental grounds that elements of taxonomy could be productive of scientific progress. In particular, Friedman's idea of beginning by determining which facts were to be explained and then theorizing about them could well be less productive than an approach of passing back and forth between fact and theory. One might have expected that particular point to be one with which Friedman would have agreed, though he did seem to say otherwise in Friedman (1946, p. 631). More broadly, though, whilst Day said nothing of significance to suggest Friedman was wrong about the particular case of Lange's book, the wider case for a thoroughgoing rejection of taxonomic theorizing was called into question and there was a clear case made that Friedman's approach confined analysis too narrowly.

Still, it is clear in all three of these reviews that Friedman was presenting fairly specific, and well-articulated methodological points. They are also points which are distinctive to Friedman. Particularly in the cases of the reviews of Lerner and Lange, and with the exception of Stigler whose ideas were surely formed jointly with Friedman's, he argued along quite different lines from other reviewers or other extended comments on the works. Others accepted the projects of Lerner and Lange as their authors saw them—some applauded, some condemned, all found faults of one sort or another. But Friedman's view of them was quite different—it was that they were barking up altogether the wrong trees. In both cases, too, the argument is executed with poise and precision, and great clarity. It looks a bit odd at first sight to see two book reviews republished in Friedman (1953c), but in this, Friedman's judgement was sound—they are two very important pieces of economics.

In these three reviews, then, a collection of related attitudes to the conduct of economics comes through very nicely. It is also visible in Friedman (1949b) and Wallis and Friedman (1942). Marshall's goal of addressing 'concrete' problems is at the origin of it. Triffin's failure arose from his emphasizing an industrial structure which offered nothing to that cause, so that any extra 'realism' imported by monopolistic competition led to no new solutions of problems. The typical indifference curve analysis did not quite have that failing, but purported to offer empirical opportunities that it could not. Lerner's book led nowhere because his analysis would not deliver actionable plans, and so in so far as it provided an 'understanding' of macroeconomics, it had no use. And Lange was if anything further away from providing useful insight because he described too much—he described too many worlds that might or might not exist, with nothing to tell one from the other. What was wanted, then, one might infer, is theory which provided a plan of action, stateable in terms of things that could be ascertained, or measured, or simply in terms of things that could be done.

5 Choice Under Uncertainty

The same empirical orientation is also very much in evidence in Friedman and Savage (1948). The primary objective of the paper was to argue that expected utility theory, of the kind which had recently been formalized by von Neumann and Morgenstern (1944), was compatible with observed behaviour involving both gambling and insuring. Friedman and Savage quoted several authors alleging that the theory was incompatible with gambling, and observed that they all presumed the marginal utility of income must be declining. In that case, even a fair gamble, which returns an expected value in money terms equal to the stake, is undesirable in utility terms since money which might be won has less marginal value than money which might be lost. Their response was the utility function might be convex—and thereby exhibit risk aversion—at low and high levels of income, and be concave in between. They rationalized this possibility saying (pp. 298–299) one might,

> regard the two convex segments as corresponding to qualitatively different socioeconomic levels, and the concave segment to the transition between the two levels. On this interpretation, increases in income that raise the relative position of the consumer unit in its own class but do not shift the unit out of its class yield diminishing marginal utility, while increases that shift the unit into a new class, that give it a new social and economic status, yield increasing marginal utility.

It would be possible to treat that as being primarily of theoretical interest, and perhaps that is how the 'Friedman-Savage utility function' has tended to be regarded. The authors themselves, though, very clearly considered it an empirical hypothesis. As they put it, they wished to know whether it was consistent with observed features of behaviour they had identified but which were 'not used in deriving it' (p. 299). When, responding to challenges by Baumol (1951) they returned to the issue in Friedman and Savage (1952) they were even more emphatic on this point. They said that Baumol had criticized the theory on the basis that he could imagine cases where it would not be true, and that 'casual observation and introspection' (p. 465) suggested it was false. Their response was that the last point was relevant evidence against the hypothesis. But the point that the hypothesis was not obviously true, that falsifications of it could be imagined was, they said, precisely what gave the theory its scientific status. It was, therefore, a desirable feature, not, as Baumol appeared to them to suggest, a flaw.

Clearly there is a resemblance between this point and the one made in Friedman (1946) that if the taxonomic approach succeeded in describing all the possibilities it does not also have empirical content. They are not quite the same because the point that Baumol was making was not that it was a weakness of the theory that he could think of a way it could be refuted, but that he believed he had devised a case where it probably would be refuted. That was not a test of the theory, but it was a warning that it was not as plausible as von Neumann and Morgenstern, or Friedman and Savage made it seem.

Friedman and Savage gave two examples of their own of what they did think would be a definite refutation of the theory and each is interesting in its own way. One was if an individual was willing to pay more for a gamble than the maximum it was possible to win. Indeed, although if that kind of thing were found to be happening, there would probably be many more serious questions about economic theory generally. That example is hard to treat as a serious-minded contribution to the assessment of the theory. The second one was that it might turn out that individuals' preferences over complex gambles were not consistent with those over simple gambles, and that would contradict the theory. That is an interesting one because it is exactly what Allais (1953) believed he showed. That generated an enormous amount of argument, of course, as many economists proved reluctant to give up the theory. One view of it, though, is that it is a refutation at least of the universal applicability of expected utility theory. Friedman had very little to say about EUT after 1953, and I suppose it is possible that he did think Allais had called the approach sufficiently into question as to make him stop pursuing it.

That point about the theory having substantive empirical content was not the only methodological one made with force. There was, at the time, an ongoing controversy over the relationship of 'ordinal utility', 'cardinal utility' and what was to be said about the measurement of utility. The approach derived from Slutsky and Hicks had, by treating preference as ordinal made the concept of utility, strictly speaking, redundant. Households could be said to prefer one option to another, and a third to either, and to be indifferent between that and a fourth, etc., but the relations of preference and indifference were all that was required, and on the ordinalist views, all that should be hypothesized. There being no room for 'utility', though, meant there was none for marginal utility or diminishing marginal utility either. That made the analysis of risk aversion impossible. The reintroduction of what was sometimes called ‘cardinal’ utility allowed for ‘countable utils’ of wellbeing but amid some confusion, addressed by Alchian (1953), raised controversy because it seemed to make utility measurable in psychologically implausible ways. Friedman and Savage (1948, p. 297) responded with the question,

> Is it not patently unrealistic to suppose that individuals consult a wiggly utility curve before gambling or buying insurance, that they know the odds involved in the gambles or insurance plans open to them, that they can compute the expected utility of a gamble or insurance plan, and that they base their decisions on the size of the expected utility?

The answer to that would, of course, become very familiar: Individuals do not do any such thing. Rather, the hypothesis asserts that individuals behave ‘as if’ they had this knowledge and made those calculations, and (p. 298),

> The validity of this assertion does not depend on whether individuals know the precise odds, much less on whether they say that they can calculate and compare expected utilities … or whether psychologists can uncover any evidence that they do, but solely on whether it yields sufficiently accurate predictions about the class of decisions with which the hypothesis deals … the test by results is the only possible method of determining whether the *as if* statement is or is not a sufficiently good approximation to reality for the purpose at hand.

They then went on to give the example of the hypothesis that an expert billiard player makes his shots ‘as if’ based on lightning-fast calculations, saying their confidence in the approach would not be dented by the discovery that a player had never studied mathematics, and that unless he could achieve approximately the same results as such calculations he would not be ‘expert’ (p. 298).

They also made the point that the axioms giving rise to the expected utility theory made it seem about as plausible as the theory of choice in conditions of certainty, and in Friedman and Savage (1952, pp. 466–467) they were much more explicit. They corrected a formal error in the earlier presentation,[4] but firmly restated the intuitive appeal of their approach. They said the 'very real appeal' of the hypothesis arose from its coherence with other theory, and was 'provided by the plausibility of a set of postulates that are sufficient for the derivation of the hypothesis and are themselves derivable from it and so are an alternative statement of the hypothesis'. The part about the postulates being derivable from the hypothesis is mysterious since it seems to be plainly incorrect. The axioms entail the theory, the theory does not entail the axioms, but leaving that aside, the emphasis on the plausibility of the postulates is clear. And that plausibility, they said, was established by the fact that they had implications of their own which survived casual testing.

Viewed as an empirical paper, it is easy to see that the hypothesized shape of the utility function would explain simultaneous insurance and gambling over appropriate amounts. A low-income household, for example, has diminishing marginal utility of income for small changes and so might insure. But the prospect of a large increase in income would still be worth a gamble. For middle-income households, on the other hand, small gambles would always be welcome. High-income households would generally be averse to risk, with the possible exception of small risks of large losses, and perhaps—if the hypothesis were augmented with a second concave and third convex section, to the possibility of large gains.

All this, the authors noted (p. 301), meant that it was not certain that there would be a premium for the bearing of moderate risks. On the other hand, they also noted that in fact there is such a premium. This led to the view that relatively few households found themselves in the concave section, so that most households would be risk-averse over small wealth changes. Of this, they said that if the concave section were interpreted as being 'a border line between two qualitatively different social classes' (p. 301), they would expect few households to be in that zone. And further, households with opportunities to gamble would not be expected to stay in that zone, since, win or lose they would move out of it.

[4]The error in the formal statement was pointed out by Samuelson and accepted by Savage (1950). When the paper was reprinted as Friedman and Savage (1953), the point was corrected, though mysteriously it was not in the later reprints in Hamilton et al. (1962) and Page (1968). The second is particularly notable as it is a specialist book on utility theory.

That last point showed a characteristic cleverness that would often appear in Friedman's arguments. It handles what appears as a difficulty for Friedman's argument by focussing on the characteristics of equilibrium rather than a looser impression of what would be typical. Put in terms of 'social classes' it is rather odd, since it seems to suggest rather sharp division of classes with few individuals in a middle class. The point might have been better put in terms of the attractiveness of life-changing increases in income, to which many individuals might be said to be attracted even at unfair odds. That, of course, threatens to make utility depend on outcomes relative to some reference value, and that is a different theory from the Friedman-Savage kind, where, however many wiggles there are, utility is a function simply of income.

Although Friedman and Savage considered various other implications of the hypothesis, suggesting tests that would be available, they did not seek to conduct any. There are also some implications they might have considered which seem to draw the hypothesis into question. As Markowitz (1952) indicated, a pair of households that did find themselves in their middle sections would want to make large bets with each other on a coin toss. That too seems wildly implausible, but just as importantly, it is something that should be easy to test. Alternatively, Bailey et al. (1980) suggested that, subject to time separability of the utility function, households would wish to have periods of high expenditure and periods of low expenditure. There are elements of that in observed behaviour—the taking of occasional expensive vacations fits the picture—but not on the scale that would be suggested.

Nevertheless, Friedman (1953f) treated the Friedman-Savage function as 'interesting and empirically relevant' (p. 282) in considering the extent to which the behaviour to which it would give rise might explain observed inequality. There is the point that overt choices about risk might explain the distribution of incomes; and Friedman also made the point that some people setting themselves up as 'employers', offering a 'wage' might be seen as an endogenous, market-generated reaction to what would otherwise be the unavailability of low-risk careers. He did not push the point, but that does create a counterpoise to any presumption that the interests of employers and workers are opposed.

He argued generally that observed inequality might be substantially explained by choices rather than differences of endowment or unavoidable aspects of risk and luck. Naturally he made the point that to the extent that was so, it raised different normative issues from inequality arising from other sources. Perhaps more interestingly, he expanded on the point that high-risk activities might be more socially productive than low-risk ones, in which case, a society of risk-averse individuals would want to create redistributive mechanisms so as to approach as closely as possible the maximization of product whilst limiting inequality. The conclusion from that was 'many common economic and social arrangements' might be 'devices for achieving a distribution of wealth in conformity with the tastes and preferences of the members of society' (p. 290). In a sense there is nothing surprising about that conclusion, though the intricacy of the way in which he traced it to the riskiness of options, rather than differences in capabilities is of interest, as is, of course, Friedman's sympathy with the redistributive arrangements themselves.

Another point of some importance arises from a criticism of the outlook of Friedman and Savage by Robertson (1954), who said that, supposing for the sake of the example, the marginal utility of money were constant, a person might still prefer a certain £75 to an even chance of £50 or £100, and that such preferences could arise from 'the pleasures or pains of uncertainty-bearing per se' (p. 674). Friedman (1955a) evidently thought he had not understood, objecting that if someone did have such a preference, that would mean that the marginal utility of income was not constant. He said 'within the terms of reference of the expected utility hypothesis, utility is nothing else than that quantity the expected value of which individuals seek to maximise' (p. 406). He commented that the confusion between 'utility' as a 'neutral concept' and as a 'value-charged concept that has some direct bearing on social policy is in the main simply a modern example of John Neville Keynes' observation' that confusion between positive and normative sciences and art was the source of much error (p. 407).

The remark about Keynes has no relevance to the discussion, and its appearance has a mysterious aspect, but perhaps just as interesting is the insight on Friedman's view. It is not quite clear whether he does not accept the conceptual possibility of uncertainty-aversion—the pain of bearing uncertainty per se—as Robertson called it, or that he thought it would be impossible empirically to distinguish that from diminishing marginal utility. There is a hint there, that he thought these two responses the same thing since he said,

> 'Utility' is that property of a thing for a person to which a number is assigned by one or another set of operations. It cannot be too strongly emphasised that so long as we restrict ourselves to the interpretation of observable phenomena, no such concept has any meaning aside from such a definition. (p. 406)

That is interesting for coming so very close to invoking the 'operationalism' of Bridgman (1927) and indeed there are other signs of that influence. Nevertheless, it was Friedman who had not understood. Robertson was objecting to expected utility theory. The point that within its confines there was no room for uncertainty-aversion was the complaint. The question Robertson raised was whether it might be that there are preferences as to the process by which an individual comes to receive the money—that is, in this case, specifically, whether it was by being given a certain amount or a lottery of the same expected utility. There is a temptation to say that such a preference would be 'irrational'. That though is an inappropriate response. In the first place, that feeling only arises from supposing that 'rational behaviour' is whatever the theory describes; but in the second it is irrelevant anyway—the question is not whether such a preference would be rational, but whether it might exist. Clearly it might. Interestingly, Friedman's implication that it would be impossible to distinguish the cases empirically was not only off the point, but incorrect. It is just a matter of devising an appropriate experiment—as Ellsberg (1961) did, only slightly later, and seeming to confirm the existence of the pains of uncertainty, or what came to be called 'ambiguity aversion'.

6 Labour Unions

Friedman (1951a) was Friedman's principal contribution to Wright (1951), a volume on the role and regulation of labour unions in the United States. It was a commonplace of American discussion of the time that price stability, full employment, and collective bargaining either were or might well be incompatible. That was invariably seen as a problem, possibly requiring a compromise of free wage bargaining. Friedman, though, took a much more optimistic view, seeming to relish the contrarian position in which it put him. As was argued by a few others such as Morton (1950) and in relation to the steel industry, by Rees (1951), he doubted that unions often raised wages by very much.

Whereas others took it for granted that in a unionized sector, the monopoly power of the union would raise wages relative to ununionized sectors, Friedman argued that the effect would be significant only if the demand for labour was somewhat inelastic and unions could control either the wage or the supply of labour. To address the demand for labour he invoked Marshall (1920, pp. 385–386), saying 'The theory of joint demand developed by Marshall is in some ways the most useful tool of orthodox economic theory for understanding the circumstances under which the demand curve will be inelastic' (p. 207). 'Joint demand' in Marshall described the situation of more than one commodity being demanded for the purpose of producing another commodity. In the case in question unionized labour was in joint demand with other factors of production. Friedman noted that Marshall listed four considerations making for inelasticity of demand—that there be no good substitute for the input in question, that its cost be a small part of the total cost of production, that demand for the final product be inelastic, and that the supply of other factors be fairly inelastic—and said that for the question of the effect of unions the first two of these were the most important.

First, he said that unionized labour would be more essential in the short run than the long. If the union raised wages, then in due course alternative means of production would be implemented, and since the price of the final product would have risen, demand for it would fall. On the second point, Friedman noted that it suggested unions would be most powerful when their wages accounted for only a small portion of total cost, and therefore that the situation was most likely to arise in the case of unions of skilled workers.

Friedman then offered a rather rough and ready consideration of some cases of union action, arguing that for the most part where they had appeared to achieve wage increases, these would have happened anyway. Of the medical profession, drawing on Friedman and

Kuznets (1945) he took a different view. In that case he said that the professional licensing rules gave it power to control entry and so it could be treated as if unionized, and that wages were perhaps 15 or 20% higher than they otherwise would have been. In the light of the presumed inelasticity of demand for medical care, he thought this a small amount, and explained it in part by the emergence of substitute products in the shape of 'chiropractors, osteopaths, faith healers, and the like' (p. 212). Some other back-of-the-envelope calculations suggested to him that the overall impact of unions was that something like 10% of workers had had their wage rates raised by 15%, and the other 90% had therefore had theirs lowered by between one and four per cent, and speculated that this was a smaller effect that was commonly believed.

Friedman then rounded off this part of the discussion with an assertion that he did not regard it as proving anything. That would take much closer analysis, he said. But what it did do was provide a 'crude tests of the general order of magnitude… If unions have a vastly greater effect on wages rates than I have estimated, this effect should show up even in so crude an analysis … The fact that it does not by no means shows me to be right; it does give reason for somewhat greater confidence in the suggested order of magnitude of effect' (p. 221).

Then he moved to explanations of why the effect of unions tended to be exaggerated, suggesting three reasons. One was that changes in wages in unionized sectors are regarded as occurring because of unions whereas they would have occurred in any case. Second was that unions were newsworthy so that their activities tended to attract attention. On the other hand, the forces tending to undermine union power—such as gradual changes in production technique—went unnoticed.

On the question of the relation of full employment and inflation, Friedman began by saying it was often argued that strong unions made them incompatible, but that he did not believe it was correct.[5] Here,

[5]Friedman said the best case had been made by the (recently deceased) Charles Hardy, but unfortunately did not say where. Hardy (1946) seems the best candidate. Even so, Hardy stated it as a 'fact' (p. 24) that these things were incompatible, and did not really argue it at all. Perhaps what Friedman found appealing was that Hardy had taught at Chicago while Friedman was there and the rest of his paper was very much in Quantity Theory terms.

Friedman made another clever argument. Unions, he said, although seeking to exert general upward pressure on wages, also induced rigidity, both by resisting wage cuts, and because of that, leading employers to resist increases. In circumstances where policy was set on full employment, there would probably be inflation arising from policy, and in that case, the effect of unions, if any, would be to retard wage increases. Unions might still get credit for the wage increase that occurred because of the policy and might thereby gain members. The position could arise, thought Friedman, where they became so powerful as to cause inflation, but at the time he was writing, the danger was that inflation would cause strong unions rather than, as popular wisdom had it, the other way round. That in itself made a case for controlling inflation, though there was a danger, thought Friedman, that a misperception of the power of unions would lead to a centralization of state power to control them. He did, however, express some guarded optimism that the tide had turned against state intervention in economic matters.

In due course, Ulman (1955) responded to Friedman. He questioned many of Friedman's ideas and inferences, pointing to gaps in the reasoning, and cited a large number of articles coming to conclusions contrary to Friedman's, and some directly challenging him. He took up the theme of Marshall's analysis directly, acknowledging its value, but pointing to specific limitations in it as well as details of how he believed Friedman had misapplied it. Then he moved to consider whether there could be groups of workers whose labour was jointly demanded with those in the union, considering the effect of their unionization, their response to the wage cut that might be implied by the other union seeking a wage increase, the question of the elasticity of substitution between the two groups and the effect of that on the elasticity of demand for the already-unionized group. And then he invoked Marshall as perhaps having had 'these complications in mind' (p. 389) when he remarked on how much could be learned from the relations of unions in different but related trades, and continued with further analysis of recent history of his own, again casting doubt on Friedman's conclusions. The question of whether Friedman was right to assume that if wages rose in the union sector, they must fall in the non-union sector was considered, with many threads of the argument, including the consideration—omitted by Friedman—that the effects of unionism might show themselves in such things as over-manning, rather than high wages per head.

The whole piece is a tour de force of the state of the art in the economics of trade unions with a huge number of analytical ideas and a mass of sources cited in support of the points put. It is notable that it was so very clearly aimed at rebutting the position specifically of Friedman (1951a). Other sceptics were mentioned, but only just, and as if in imitation of Friedman, or perhaps to make it yet more apparent that it was specifically his argument that was under scrutiny, the lead was taken from Marshall at several points. Then, in a conclusion of about a page, Friedman's name appears ten times—marking nine points of disagreement and one of approval for the proposition that a major research project was required.

Friedman (1955b) responded to Ulman. He said that the 'main point of contention is empirical', which was certainly correct, and that it was not whether unions 'sometimes affect wage rates' (p. 401), which he said was agreed, but rather as to the magnitude of that effect, on which Ulman had presented no evidence. Friedman admitted severe limitations in his empirical work on the topic, but went on to express astonishment that Ulman's criticism of him had involved trying to make the theoretical analysis more 'subtle and complex' rather than by offering clearer evidence. On the theoretical matters, Friedman made a number of further clever arguments, whilst leaving many of Ulman's points undiscussed. His summary of the matter was that Ulman's criticism largely amounted to saying that Friedman's treatment had been too simple and that Ulman had spent most of his effort elaborating qualifications with more sophisticated theory. Perhaps really some were more like direct challenges than qualifications, but Friedman's response was, citing Friedman (1953b), which had been published in the meantime, to say that he would 'only insist' that the test of whether Ulman was right,

> Must be found in an appeal to evidence. Are the implications of the simple theoretical structure contradicted by evidence? If they are shown to be, for example, by demonstrating that industrial unions consistently obtain larger wage increases for their members than craft unions, then the

> simple theoretical structure must be rejected as inadequate by itself… If they are not contradicted, this must mean that the qualifications, while possible, are not quantitatively important.
>
> Friedman (1955b, p. 405)

There is again no doubting the cleverness of Friedman's arguments, though he claimed a bit too much, and dismissed Ulman a bit too quickly, since one clear implication of what he was saying was that testing would be difficult to undertake. That is unfortunate for the empirically minded, but it is not a point that can be dismissed just because it is unfortunate. When it comes, though, to Friedman's assertion that the real issue is about whether the implications of the 'simple theoretical structure' are contradicted by the evidence, there is a subtle rhetorical twist, and one which is, by itself, not quite legitimate. The simple theoretical picture was the one under attack in Friedman (1951a). The simple story is the one that says unions act as monopolists, and like industrial monopoly, raise price. Friedman's theoretical challenge to that—invoking Marshall on joint demand—was by means of considering more sophisticated theory. When Ulman took the theoretical matter further still, Friedman merely declared that to be going too far. But there is no basis for Friedman to say that his degree of complexity is the one appropriate for testing; nor, really, for what seems to be his presumption, that until someone else does the testing and shows that he is wrong, the presumption is that he is right. Notable as this little trick was, there was really no sign that Friedman appreciated what he was doing. The appearance of it all is quite consistent with his feeling that his theory was presumptively the at the right level of complexity and it was up to someone else to show otherwise.

7 Macroeconomics and Money

Although macroeconomic and monetary questions would come to be by far Friedman's principal interest, they formed only a small part of his early work. Indeed, Friedman rather extravagantly drew attention to the point that in Friedman (1942a)—a paper concerned with inflation—he made no mention of money. He noted that first when the paper was reprinted as Friedman (1942/1953). In that version he added a discussion of certain monetary aspects of the issue and commented that in the original he had made 'a serious error which is not excused but may perhaps be explained by the prevailing Keynesian temper of the times' (p. 253 n2). In Friedman (1972a) he pointed to the same piece as one example of the general neglect of monetary matters in the 1940s and there was more discussion of a 'Keynesian' influence in Friedman and Friedman (1998a, pp. 112–113) where, also referring to a statement to Congress on the same matter—Friedman (1942b)—he said,

> The most striking feature of this statement is how thoroughly Keynesian it is. I did not even mention 'money' or 'monetary policy'! The only 'methods of avoiding inflation' I mentioned in addition to taxation were price control and rationing, control of consumers' credit, reduction in governmental spending, and war bond campaigns.

And he shortly continued,

> Until I reread my statement to Congress in preparing this account, I had completely forgotten how thoroughly Keynesian I then was. I was apparently cured, or some would say corrupted, shortly after the end of the war.

This question of the Friedman having once been something of a Keynesian casts a peculiar shadow over the discussion of his early work. Levrero (2018) taking up the idea of Friedman being 'cured', considered the question, finding the illness much less severe than Friedman suggested, though starting from a rather specific understanding of 'Keynesian'. Lothian and Tavlas (2018) on the other hand, seem to have accepted Friedman's view on the basis that during the war, he was in favour of controlling inflation by raising taxes, and in several discussions of the matter, as Friedman later said, made no mention at all of money.

There are, though, other points that should have attention, not least because it is none too clear why Friedman thought Friedman (1942a) worth reprinting at all. It was a comment on Salant (1942), and was rather out of context on its own with a couple of conspicuous loose ends where particular remarks of that author are referenced. Although a discussion of money was added, by 1953 the paper had no current interest at all; and it does not seem to be any more pertinent to issues of 'positive economics' than several other papers he could have included in the volume. It might seem that it was just that he felt the need to correct his 'serious omission' that led to its being reprinted. But the implication of Friedman and Friedman (1998a), is surely that he had been remiss in failing to see the importance of money in the control of inflation. If that was the point needing correction, then Friedman (1943a) was just as much in need of reprinting as Friedman (1942a). That was something of a response to Wallis (1942) but is much more self-contained than Friedman (1942a), and although focussed on the desirability of a sales tax, it was about the steps to be taken to control inflation, so that monetary control might again be relevant.

Friedman (1942a), on the other hand, was really not about the control of inflation at all. Like Warburton (1943)—who was not one to forget about money whatever the temper of the times, but who also made no mention of the kind of argument later introduced by Friedman—it was about the concept of the 'inflationary gap'—that is, roughly speaking, the difference between the total calls on productive resources and their availability. In a few pages at the end, Friedman commented on arcane issues as to the measurement of the gap and consequential policy issues, but it was not in the discussion of policy issues that he added his remarks about money. The original version began by saying that the gap was 'one of those ex ante concepts with which recent theory has made us all familiar' (p. 314), and went on first to note that ex post, no gap would exist, since, somehow, buyers would be matched with sellers. The 'gap' though, was the difference between what he called the 'expected expenditures and the value of goods expected to be available' (p. 315). The question he then addressed was that of whether an increase in the price level in itself would close that gap, stressing that, despite what he thought was the implication of the labelling, it would not. Rather, it would raise incomes as much as costs, leaving demand, and hence the gap unchanged. So if a gap were to be closed by inflation it would have to be as a result of a redistribution of incomes, or by deterring consumption merely by the fact of rising nominal prices, and the like.

The natural reading of Friedman's original argument is that it was intended as an analysis of comparative statics in which the quantity of money was implicitly assumed to be adjusted to various alternative price levels. Understood in that way, if there is a criticism of Friedman's argument it would more naturally be to question why it mattered—the point of gap calculations was to design policy to avoid inflation (or limit it, anyway). Friedman's observation about the effects of inflation is correct, but a little bit peripheral to question of preventing inflation in the first place.

In the revised version, he considered the case of on-going inflation and full adjustment to it. He observed that the cost of holding money would be the interest forgone in doing so, plus the loss of value due to inflation. Inflation would thereby reduce the demand for money by—as he stressed—acting in just the same was a tax on nominal money balances. The way he put it, this forced private agents who wished to maintain the real value of money balances to expend resources in accruing nominal balances whilst the issuance of the money that would become those balances allowed the government to purchase real resources. There was, as he also made clear, a limit to the resources that a government could acquire in this way, and that was determined by the elasticity of demand for real money balances with respect to inflation—at higher rates of inflation, agents would economize on the holding of money, and the scope for inflationary finance would be reduced.

It is an interesting discussion in a number of ways, although arguably more of an addition to the earlier one than a correction of it. For one thing, it really does nothing to show that Friedman had previously been under any kind of Keynesian influence. Even these added remarks made no case that the causes of inflation invariably lie in monetary policy; and nor are they otherwise about the control of inflation. It is perhaps interesting to see Friedman putting the demand for real money balances at the centre of the argument in a way that clearly foreshadows his 'Restatement' of the Quantity Theory in Friedman (1956a). He did not, though, suggest the point had any wider significance, although the theory was to become a very important matter in so much of his later work. It is also, of course, an important piece of theory, and one that seems to have been neglected in the postwar period, although it was shortly to be a central part of Bailey (1956), and to some extent Cagan (1956), both of which were outcomes of doctorates, the second under Friedman's supervision.

So, the remarks in Friedman and Friedman (1998a) about his being Keynesian in the 1940s are probably best regarded just as a cheap shot for the readers of that book. Perhaps that is what it was in Friedman (1942/1953) as well, although there it might be seen as also providing a kind of explanation of why the paper was reprinted. The real reason for that, though, might be just that Friedman was looking for an outlet for that particular piece of analysis. The content of the argument does fit the discussion of the inflationary gap better than it would have fitted as an addition to Friedman (1943a), but it is nothing to do with Keynesianism, monetarism or, fundamentally, the Quantity Theory. And of course the argument was not actually new, since it was made—including the points that there is a limit to the finance that the government can raise in this way, and that such financing can be thought of as a tax—by amusingly, one might think, Keynes (1923, Chapter 2).

If there is any particular piece of writing which shows how far Friedman had to travel on specifically monetary matters, it would be his contribution to Shoup et al. (1943). Along with the likes of Warren (1942), Crum et al. (1942), and Fellner (1942), the book was one of a number on the question of financing the war, and Friedman was credited in the Foreword by Shoup with having provided many of its ideas, but his principal contribution to it was its Chapter 3—Friedman (1943b). There, Friedman considered various ways of forecasting inflation, one of which was gap analysis and another of which was that suggested by Angell (1941). It is all sharp and insightful—and challenging of others' ideas, but the point that stands out in longer perspective is one comment on Angell. Friedman quoted him as saying that the velocity of circulation of money had been stable over the thirty years up to 1929, and described him using this fact as one component of his method of forecasting inflation. Friedman criticized the conclusion saying,

> Angell bases his conclusion primarily on a chart on which national income is plotted against the stock of money… it seriously misrepresents the

> relationship between the year-to-year changes in the two variables… The long-time upward trend of both national income and the stock of money is bound to give a close correlation between the two totals, no matter how loose the relation between year-to-year changes in them. (p. 119)

Friedman went on to observe that it was more appropriate to investigate the correlation of year-to-year changes in the two variables, and that when this was done, their ratio turned out to be 'extremely unstable'. Friedman found plenty more weaknesses in Angell's analysis, but that one is perhaps the most interesting, since the making of that mistake was to be a charge levelled against Friedman in the debate over inflation in the 1980s.

That is an interesting item, but a much better all-round perspective on Friedman's early postwar outlook on macroeconomics comes from considering wider-ranging works than these. Three stand out—Friedman (1948a), the report of Despres et al. (1950), and perhaps Friedman (1950a).

The substance of the first of these was the making of a proposal for an automatic, or rules-based, rather than discretionary stabilization policy, incorporating both monetary and fiscal aspects. The elements of that package were 100% reserve banking; fixed rules of taxation and expenditure, including transfer payments, so that the government budget would balance at an appropriate high level of employment, with actual expenditures varying only because of cyclical changes; a monetary policy that would be entirely passive in the sense that the money supply would rise and fall only as the counterpart to fiscal deficits and surpluses; and a progressive tax system, relying primarily on income tax. That left no role for interest-bearing government liabilities, which Friedman said should not be issued, nor for open market operations in those or any other security. Under these arrangements, a downturn in activity would result in market adjustments, but also a fall in government revenues, an increase in expenditures, and hence a budget deficit, and thereby an increase in the quantity of money. So there would be 'defense in depth' (1948, p. 261) in that after a shock to demand, the changes in transfer payments, price adjustments, and thirdly changes in the stock of money would all push in the same, stabilizing, direction.

As Friedman said, none of the elements had much originality, and he noted their similarity to various proposals of Henry Simons, particularly Simons (1934) and Simons (1936), who advocated 100% reserve banking and emphasized rules-based monetary policy; and in fiscal policy he noted the idea of what came to be called 'automatic stabilizers' featured in Committee for Economic Development (1947). It was also in Hart (1946) and Musgrave and Miller (1948).

The interest of the paper, though, lies not in any appearance of striking originality in the general shape of the proposals, but in aspects of the argument he made about it. One point concerns his advocacy of rules-based policy. As he put it himself, Friedman's objective was to design a workable system to secure long-run objectives—as distinct from those concerned with stabilization policy—and then to consider how it would affect economic behaviour. Those long-run objectives, which he said he felt were widely agreed were 'political freedom, economic efficiency, and substantial equality of economic power'. The advocacy of rules then flowed—really without any further argument—directly from these objectives. In this, he was again in the footsteps of Simons, though unlike Simons, he had nothing to say to justify the principle he was advancing. The case for rules, then, was practically axiomatic.

On the other hand, another point is that Friedman clearly accepted the desirability of policy being designed with a view to active stabilization. He observed that price flexibility would be sufficient to bring stabilization, arguing along the lines of, and citing, Pigou (1943), Pigou (1947), and Patinkin (1948). But rather than rely on the processes they described, Friedman said that the other forces set in motion by his policy would speed the return to equilibrium and achieve it with a lesser price fall than would otherwise be required—that was just the point of 'defense in depth'. He also restated the sentiment about equality that had come through in Friedman and Stigler (1946), saying in passing that whilst he thought a more competitive economy would produce more equality than the existing one, he hoped the community would want to reduce inequality even further.

It is also interesting that he paid quite a lot of attention to the limitations of the proposal. The difficulty posed by price rigidities was clearly noted. He said an increase in wages in one sector would, since other wages do not fall, raise nominal income. That would automatically create a fiscal surplus, and hence a deflationary effect which, again in circumstances of nominal rigidity, could only result in unemployment. He accepted that, saying that other schemes suffered the same limitation, and, said 'The brute fact is that a rational economic program for a free enterprise system … must have flexibility of prices (including wages) as one of its cornerstones' (p. 254). There must be a note of frustration there—the theorist's frustration at a plan that does not quite work because of some awkward fact. He had little more to say about it, except that since the scheme offered security against cumulative deflation, it would help remove restrictive practices, and thereby promote price flexibility.

Secondly, he considered the question of how lags in the system might affect its behaviour. He noted, as he had in Friedman (1947c) that the total lag in policy effect could be divided into the lag in recognizing the need for policy, in taking it, and in its having effect, and noted that it was possible that with his proposal the lags would be such as to make policy destabilizing. That, however, he resisted saying that although his analysis was 'highly conjectural' (p. 257), it seemed likely that the total lag in a regime of discretionary policy would be longer than that under his scheme. Here, he again suggested that the adoption of his proposal would itself enhance stability by making it rational for private actors to take actions which would dampen fluctuations.

At the end of the paper Friedman also mentioned the possibility of a metallic currency, but whilst doubting its desirability, said nothing about it (p. 264). He did though address the commodity reserve currency idea generally and fully, and with some fairly intense scholarship, in Friedman (1951d), addressing ideas from Graham (1937, 1944), and Frank Graham (1942). He rejected the idea for a whole range of reasons. Two, strictly on the economics, were that the scheme had a real resource cost in that the medium of exchange would be costly to mine or produce; and that technological and other developments in its production would become a source of economic instability. On a more pragmatic level, he also doubted that complex commodity-based schemes would win public understanding, noting that if they did not, they would be harder to sustain—and also that on that score, the Gold Standard had something to be said for it.

Another kind of challenge was not to the acceptance of fiduciary money, but the form of the rule governing its supply. Bach (1947), amongst others, had considered a rule for stabilizing the price level, and Friedman might have been expected to comment on that possibility, but did not. And then there was also the idea of a simple money-growth rule which was an obvious competitor with Friedman's idea and advanced specifically in response to Friedman by Warburton (1953). In Warburton (1952), first of all, he argued that the poor performance of the past, particularly since the creation of the Federal Reserve System in 1913, had been due to bad policy, not to the structural arrangements Friedman proposed to overturn. In Warburton (1953) he also criticized Friedman's position on the basis that it was most unlikely that the proposal would lead to changes in the money supply which were appropriate to exogenous cyclical developments, and suggested that if it did not, uncertainty about what changes would be forthcoming could be the source of policy-induced uncertainty that Friedman argued his plan eliminated. The lesson he drew was simply that better policy was required, and in the later paper specified that this could be achieved by the pursuit of a steady rate of growth of the money supply. Friedman did of course come around to that view himself, but as of his 1948 paper, it was not one he gave any serious attention.

Friedman was also part of the team of Despres et al. (1950) which wrote a report organized by the American Economic Association to inform the public about current views on questions of macroeconomic policy. It was obviously seeking consensus and largely found it, albeit at the expense of some generality. Consequently it was rather lacking in specific proposals, and many possible disputes were smoothed over along the lines that 'some economists believe … others believe…'. It did, though, see monetary and fiscal policy both as part of stabilization policy, and suggested that monetary policy was more effective in checking booms than preventing recessions, as well as giving fairly strong support to a role for discretionary fiscal policy, in the form 'Most economists approve …' (p. 522). Friedman—later notorious for his idiosyncratic views was indeed the only one to enter any note of dissent, but that was not on monetary and fiscal policy, but the matter of price control. The dissent was that he disapproved of the 'general tenor' (p. 534 n1) of the group's discussion of it, and one specific proposal. As he saw it, the group saw institutional measures to stabilize prices as a complement for macroeconomic policy, whereas he thought them quite inappropriate to that goal. Macroeconomic policy could achieve broad price stability, and in that case, individual prices should be free to fluctuate to achieve desirable resource allocation. That would certainly be a recurring theme of Friedman's work, but it is notable that at this time, he did not choose to dissent from the others' presentation of views on monetary and fiscal policy.

Friedman (1950a) is a quite different kind of paper, being an analysis of Wesley Mitchell—famous for his assiduous data collection and organization—as an economic theorist. Friedman set about a 'free rendering' (p. 479) of a version of a theory he believed could be read into Mitchell's work. It was notably scholarly in its presentation, being very fully referenced, and Friedman emphasized its point in the title—'Wesley C Mitchell as an economic theorist'—perhaps also intending to imply a challenge to Koopmans (1947). That key point was that Mitchell was much more insightful as a theorist than his reputation suggested. The accuracy, or even the insight, of Friedman's account is not at issue, but what is of interest is that in the last few pages of the paper, he presented a mathematical version of the theory he thought suggested by Mitchell's work. Friedman evidently took his cue from Burns' (1949) remark, 'I venture the prophecy that if Mitchell's homely work of (1913) were translated into the picturesque vocabulary of "propensities", "multipliers", "acceleration coefficients" and the like, it would create a sensation in the theoretical world' (p. 26). In taking that cue, Friedman described Mitchell's work in terms of an income-expenditure framework, starting with '$Y = C + I$', and including a multiplier relation. Mitchell, it was noted (p. 474) accepted that over long periods, prices vary with the quantity of money, but that money was not the primary stimulus to shorter-term changes, and in Friedman's rendering, the argument was even put in the language of 'liquidity preference' (pp. 486–487, 492–493) and to judge by this piece alone, Friedman seems quite content with the Keynesian framework, as well as with its terminology.

In Friedman (1951g) he actually said there were two 'languages' in which policy could be discussed—the Keynesian and the Quantity

Theory—and that time, clearly preferred the Quantity Theory language, and indeed expressed the view that control of the quantity of money should be a 'major instrument for controlling inflation' (p. 188). That preference certainly emerged in Friedman (1952b) which was a study of inflation in three wars—the American Civil War and the two World Wars. Friedman began by saying that data from them was often excluded from consideration because they were presumed to be abnormal periods, but that they could also be seen as offering just the kind of 'critical experiment' that was sometimes hard to find. The paper involved some judgements that might be questioned, particularly over just what counted as a 'wartime period', as well as some frank admissions of doubt about data quality, but one striking thing is Friedman's conclusion. He again said that the two theories could be seen as different languages, but also that,

> A crucial issue in economic theory in recent years has been the relative value of two competing theories of income determination: the quantity theory of money and the Keynesian income-expenditure theory. (p. 621)

The presentation of them as competing theories might be seen in Whittlesey (1948) and Hansen (1949). But on the other hand, there was a strand of literature exemplified by Morton (1950) seeing them as being, deep down, quite compatible, and that is probably the one in which Friedman's earlier discussions, up to Friedman (1950a) and Friedman (1951g) would best be seen. But in Friedman (1952b) he said that the 'major issue' was about the theories as empirical hypotheses and that in that respect they were competitive, with the crucial issue being as to which variable the theories treated as empirically stable—for the Quantity Theory it was the velocity of money, for the Keynesian, the propensity to consume. His conclusion, of course, was in favour of the Quantity Theory. He said of the results of the work,

> If you want to control prices and incomes, they say in about as clear tones as empirical evidence ever speaks, control the stock of money per unit of output. (p. 623)

That is easily recognized as the kind of thing he would say in his later work, but the specific casting of the Quantity and the income-expenditure theories as alternatives was also to become a major feature of his work. It is interesting to see the way in which his view seems to have developed in the early 1950s.

Up to a point, the impression of a rather sudden adoption of the Quantity Theory can be corroborated by reference to other works and comments by Friedman from the period. In two 'University of Chicago Round Table' radio broadcasts from 1951, Friedman made pertinent remarks. In February, Friedman (1951c) said, 'The point I would like to emphasize is, however, that all this rests on two essential pillars. A sound economic programme for the coming period must have, as one part of it, a fiscal policy which involves increased taxation to offset the effect of increased government expenditure. It must also have a monetary policy which prevents civilians from adding fuel to the inflation'. In September, Friedman (1951b) was a discussion of raising taxes to control inflation. Late on Friedman raised the possibility of using monetary policy, and said that if a tight money policy were adopted 'we might be able to borrow from the current savings of the people a sum sufficient to make up for the kind of deficits' in prospect. Then in 1952, Friedman (1952c), replying to Congressional questions, he rejected selective credit controls and said that control over the quantity of money 'should be carried to the point at which inflation is prevented regardless of the effect on interest rates' (p. 1069), and that if interest rates seemed too high, taxes should be raised. Those precisely dated comments do seem to suggest that Friedman became convinced of a much more important role for monetary policy at just the time he may well have been writing on the wartime inflations.

That development has been analysed in detail by Lothian and Tavlas (2018). They pointed to a particular influence of Clark Warburton, showing that his correspondence with Friedman in 1951 seems to have been important in the development of Friedman's views. The change that seems to be visible in Friedman (1952b) is the adoption of the Quantity Theory first of all as a distinct theory, rather than merely a language, competing with the income-expenditure theory, but more substantively, the acceptance of it as providing a powerful explanation, and implied policy conclusion about the control, of inflation. It was further argued by Lothian and Tavlas, however, that Warburton also convinced Friedman of the point that the policy of the Federal Reserve was the cause of the Depression. That last point may be questionable in the light of Friedman's taking that view in his contribution to Director, Friedman, and Wallis (1950), but nevertheless the insight is a fascinating one. As Lothian and Tavlas say, Warburton has been recognized, for example by Cargill (1979) and Bordo and Schwartz (1979) as preceding Friedman and Schwartz in many of their conclusions. Indeed, Friedman and Schwartz (1963a), themselves recognize that and note his precedence on a number of points, and acknowledged significant help from him. He is one of the most cited authors in their book, and distinguished from some of the others by the fact that what they say always conveys approval of his ideas. But on the basis of Lothian and Tavlas' work, and perhaps even more so that of Tavlas (2019) it could well be said that Warburton had a direct, personal influence on Friedman of a depth and range going well beyond what was acknowledged.

Here, clearly, Friedman's views were developing and moving towards the positions that became distinctively his. Naturally enough, then, the question of where he began and how it relates to the idea of his supposed early Keynesianism arises again. It may be that later disputes—those of the 1970s and 1980s—create an impression that 'Keynesian' and 'Quantity Theorist'—relabelled as 'monetarist', exhaust the relevant range of options. Then, it seems that if Friedman was not a Quantity Theorist, he must have been a Keynesian. That is far too much of a caricature of possible positions to be worth anything in a serious analysis. It is clear then that his earliest works, there are distinctive aspects to Friedman's views, but there is nothing making him markedly either Keynesian or anti-Keynesian. So in these things, there is nothing much to be learned about Friedman's attitude to 'Keynesianism'. On the other hand, there are a couple of earlier comments of Friedman's own—both from little-noted book reviews from the 1940s, that throw some light on the matter. One is the hardly noticed Friedman (1944, p. 101 n2) in which he doubted the 'usefulness' of Keynesian analysis because of the factors it assumed exogenous. It was a quick remark, and not developed, but it shows his attitude. And even before that, there was Friedman (1941b, p. 581), published in such a caliginous corner of the American Economic Review that it has apparently never been noted at all.[6] There, he referred to what he called an 'under-consumption or over-saving theory of cyclical and secular unemployment' and said he thought it 'at best, seriously incomplete and, at worst, completely fallacious'. That seems quite a good clue as to his attitude. If anything, he seems to have warmed slightly to a Keynesian approach when between about 1950 and 1952 he was presenting it and the Quantity Theory as alternative languages, but as Lothian and Tavlas (2018) say, it is then that he also put them as competing theories and presented his evidence to differentiate them, and thereafter his description of them in terms of different languages faded away. All that certainly should be seen as development of thinking, but it is something like a growing confidence in the non-Keynesian aspects of his view, or perhaps a developing articulation of what his view was. At no time would it have been sensible to describe Friedman as 'Keynesian' but nor was specific opposition to Keynesianism animating his thinking.

8 Flexible Exchange Rates

Having, in Friedman (1948a, p. 252), very briefly noted that his proposals for macroeconomic stabilization lent themselves to floating exchange rates, Friedman addressed that question much more fully in Friedman (1953d). That was written whilst he was working in Paris, but published for the first time in Friedman (1953c). At the time, although there was some academic and other support for floating, policy presumptions in nearly every country were clearly in favour of fixed rates and Friedman's was an early challenge to the view and was to become, in retrospect, very widely noted.

[6] A measure of its obscurity is that (as of August 2018) it is an extremely rare example of a complete omission from the online listing of Friedman's works at the Hoover Institution.

His starting point, and the basis of the case he made, was that from time to time there would be events which would change the pattern of supply and demand for currencies, and that four responses were possible—a change in exchange rates, a change in prices, direct controls over transactions, or a change in money supplies. He proceeded to consider each, arguing that with floating exchange rates the first effect of a tendency to surplus or deficit would be a correcting movement of the exchange rate. A fixed-but-adjustable system, on the other hand, tended to result in adjustments being delayed, and therefore encouraged speculation whenever one seemed likely. Price changes could in principle bring exactly the same effects as floating exchange rates, but Friedman observed that prices were not nearly flexible enough and wages were the least flexible. If required changes were infrequent this mechanism might serve, particularly since real changes in any case would normally require some prices to change. However, Friedman noted that widespread emphasis on the maintenance of employment meant that many changes were monetary and the advantage of exchange rate flexibility in dealing with these was that then no other changes were required. Similarly, direct controls could in principle work, but in fact the information problems were prodigious, and controls themselves distorted market signals, in some cases worsening or sustaining the balance of payments problem which made them seem necessary. One of the reasons for that was that the existence of controls itself made a currency less attractive and hence to appear weaker than it would in the absence of controls. He suggested that the pound might have been in that position at the time, with the implication that were it to float, it would strengthen. That seems to have been a recurring thought since Erickson (2001, pp. 55–56) has Ralph Harris saying that Friedman made the same argument in relation to exchange controls in the 1970s.

And reserve changes too, could serve the purpose, except that Friedman noted that for disturbances known to be small and temporary, private speculators could equally well change their holdings of various currencies. For larger disturbances or those of longer duration, some other form of adjustment would be required unless countries were prepared to accumulate reserves without limit, and even that mechanism was impaired by the tendency towards sterilization of changes in the money supply. It seemed clear to Friedman that floating exchange rates offered the best adjustment mechanism.

He then turned to objections to floating, considering first the claim that they created uncertainty. He said that any uncertainty would be due to some underlying factor. From the point of view of those involved in international trade, there was uncertainty associated with floating exchange rates but that could be hedged. With fixed exchange rates, the underlying uncertainty would have to show up somewhere else, and the implication was that hedging that might be harder than hedging the currency risk. There was another possible objection in that it was said that speculation in floating exchange rate markets was destabilizing, so that it added to uncertainty. In what is probably the best-remembered argument of the essay, Friedman found that implausible since, so long as equilibrium was eventually restored, it would have to mean that on average, speculators bought currencies destined to fall, and sold those destined to rise. In that case, they would be systematic losers. He augmented that with a discussion of the argument put by Nurkse (1944) to the effect that interwar experience showed speculation to be destabilizing, and that one aspect of Friedman's paper in due course gave rise to its own little debate about the interpretation of the historical facts.[7]

Friedman also considered the argument that the fall in value of a currency was more noticeable to the public than a crisis of a fixed rate and would therefore be likely to induce actions by the public based on the anticipation of further exchange rate change, and this could be a source of increased volatility. That the importance of defending a fixed rate might be an inducement to avoid inflationary policy was recognized, but Friedman suggested that in Britain the fixed exchange rate had allowed pre-election expansion by keeping the inflationary consequence temporarily out of the public view and thereby encouraged poor policy. He contrasted the situation with that, such as under the traditional Gold Standard, where governments were fully committed to

[7]Aliber (1962) raised a question, taking the matter further in Aliber (1970) argued speculation on the franc had been destabilizing. Stein and Tower (1967) on their own account said say the evidence strongly favours Friedman over Nurkse and Lloyd Thomas (1973) responded to Aliber saying he had confused the effects of speculation with those of unsound financial policy.

maintaining a parity without the use of direct controls and would therefore submit to its macroeconomic discipline. He rejected the view that floating exchange rates made it possible for a depreciation to generate a 'wage price spiral' on the basis that there could only be an ongoing inflation if the government allowed the money supply to expand, and dismissed the view that it would be more likely to do so without the discipline of fixed exchange rates on the basis that they would not in any case remove the source of the disturbance, whatever it was, initiating the changes.

Turning to the operation of a floating system, he considered the possibility that there might be government intervention and whilst thinking it generally poor policy, did not rule out the possibility that it could in special circumstances be appropriate. He observed that in a floating rate system one country—but only one, and presumably the United States—could retain a gold parity, but said it would be better not to, and instead to institute a free gold market. And he discussed the situation of the Sterling Area,[8] accepting that one possibility was the maintenance of fixed exchange rates within it, whilst the pound floated against other currencies.

Finally, he made brief comments on three specific issues. He said that in Europe, and particularly in 'England, France, Norway, and some other countries' there was extreme reluctance to allow internal adjustment involving unemployment and for this reason, liberalization of trade with fixed exchange rates would be very difficult, and if achieved, would probably lead to the reimposition of direct controls very quickly. Secondly, he pointed out that floating exchange rates made it possible for countries to pursue independent monetary policies and thereby achieve price stability, even though others inflated. Price stability could be achieved with fixed rates only with a very high and probably undesirable degree of policy co-ordination. And thirdly he said that the rearmament ongoing at the time provided an example of how floating rates could ease adjustment. That rearmament was likely, he said, to result

[8]An arrangement of fixed exchange rates and reserve sharing of most of the British Commonwealth countries, the contemporary operation of which was discussed for example, by J. R. Sargent (1952).

in shifts in the pattern of demand, causing payments imbalances and hence pressure for trade restrictions and hence, amongst other things, interfering with the rearmament effort itself.

Although Friedman and Friedman (1998a, p. 220) are clear that the essay had no influence on policy, it is a remarkable collection of arguments. Some of them are so prescient as to make it hard to remember the date of publication was as early as 1953. The claim, for example, about British policy in relation to the timing of elections was the basis of the line of thinking made well-known by Nordhaus (1975). But even in British politics, 1955 is usually treated (e.g. by Oppenheimer [1970] or Hopkin [1974]) as the election where policymakers first really saw the benefits of adjusting policy. It was not just prescient in that kind of way, though, but also contained a collection of little theoretical insights—mostly presented just in passing—which if they had been noted at all had surely not been clearly and widely perceived. Not only as Cesarano (2006) said, did he recognize that the strength of his case for floating depended on the characteristics of the countries in question, but up to a point, as Dellas and Tavlas (2009) noted, he also appreciated some of the details of the argument later made on the question of optimal currency areas by Mundell (1961).

Even if some of the detailed arguments were not strictly speaking original, the assemblage of them was powerful, and it had distinctive Friedmanesque aspects too. Much discussion of exchange rates was very much conducted in terms of immediate policy goals. The desire to achieve full, or at least greater, convertibility was often in play. That had been an important motive of the then-secret 'Robot' plan to float the pound that was almost implemented in 1952, before being abandoned because of even more immediate goals—on the account of MacDougall (1987) because exports were supply constrained and could not increase if the pound fell. Friedman's approach put the argument in terms of much more fundamental considerations than that. Similarly, his precise reasoning about the long-run or equilibrium characteristics of various outcomes was typical of his way of addressing problems—if speculation was destabilizing, speculators would be losing money. The contrast with the easy, commonsensical approach, for example of Meade (1951) is stark. Meade recognized that floating exchange rates might smooth adjustments, but simply declared the possibility of destabilizing speculation and said that the remedy was stabilizing action by the authorities. It did not sound likely to Friedman that speculators lost money, so speculation would have to be stabilizing, on average. But he also pointed out that since stabilization was being attempted, there was a test available as to whether the private speculation motivating it was actually destabilizing: If it was, policymakers should make money. Other points that would probably be regarded as Friedmanesque are there too, though rather muted and not nearly so emphasized as they would be in later work. One is the consideration of the relation of exchange rate fixing to the freedom of individuals. To the extent that the attempt to fix exchange rates tended to result in other interventions—particularly trade restrictions and currency control, implied Friedman (p. 158 n3)—it restricted freedom. The point was there, but emphasis on it came later—it is more prominent in Friedman (1962a). And the point that policymakers deciding to fix an exchange rate can be expected to make errors about the appropriate level at which to fix it, similarly, was there, but not by any means as centrally as that kind of point was in later work.

One striking thing about it all is how little those who followed seem to have added to Friedman's argument—he made that point himself in Friedman (1988c). Johnson (1969) put the case differently, but added little in substance, and even took Friedman's title as the basis of his own—'The case for flexible exchange rates, 1969'. Equally, one might say it is noticeable how little Friedman found it necessary to add in responding to the counter-arguments. In Friedman and Roosa (1967)—a debate on the issue with a leading advocated of fixed rates—and in Friedman (1969b)—a 'round table' discussion, the advocates of fixed rates had little to say that could not have been answered by the 1953 paper, although of course Friedman rephrased some of the points, and had more examples to present of things going wrong in fixed-rate systems (or, in the case of the Canadian experience of floating, discussed in the debate with Roosa, explaining that floating rates offered no guarantee against poor domestic policy). Similarly, when Kindleberger (1970) adapted Johnson's title to 'The case for fixed exchange rates, 1969', Friedman (1970h) responded making it fairly clear that there was little new that he needed to say to answer him.

Friedman did adapt the presentation of his position in certain ways in the later treatments. One is that—no doubt because it was written while he was working in Paris—the 1953 paper is rather Eurocentric. It is easy to read as a proposal, principally to the British and West Germans to float their currencies. When floating came, the British are probably best regarded as being forced into it, although the decision of the West Germans, albeit 20 years after Friedman wrote, was much more like one taken on the basis Friedman described. As it was put by Emminger (1977), the story was one of the difficulties of central bank operations, but the essence of it was that, at the end of the day, if they wanted their inflation rate to be lower than the American, they needed a floating deutschemark. A consequence of this Eurocentrism, though, is that the question of any special role of the dollar in the system was obscured, and there was even a question as to whether the United States could float if other countries were going to maintain dollar pegs in any case. So, in Friedman (1969b), he accepted this, and said he would not describe his proposal as one to 'float the dollar', but rather as one to cease pegging it: It should cease official gold transactions, remove all forms of exchange control and like policies, and cease foreign exchange transactions except for actual purchases of goods and services. If other countries then chose to arrange their affairs to peg to the dollar, Friedman saw no disadvantage to the United States.

In later statements, he also made a better job of clearly specifying that the advantage of flexible changes rates appears most clearly when external shocks are monetary. Real shocks require real adjustment, and there may be costs, but they are fundamentally unavoidable. Monetary shocks originating in the rest of the world do not in themselves require any more than an exchange rate change. It is when that change is effectively prevented by the institutional arrangements that adjustment becomes painful.

A point that he seems not to have taken seriously is that the political practicalities may be that governments committed to fixed exchange rates act with more determination on the control of inflation. That was often the appeal of what Friedman called 'pseudo gold standards', which were systems with a nominal commodity peg, but less than complete backing of the currency. He might seem not to have understood it in Friedman (1960b) where he considered proposals to raise the price of gold and free the market. How could a policymaker raise a price and free the market at the same time, he wanted to know. But the proposal, surely, was to achieve the price in question by appropriate conduct of macroeconomic policy, not market intervention. That was the point of the idea that the mechanism provided discipline. In Friedman (1961e) he again attacked pseudo gold standards, mainly on the basis of historical examples, and in Friedman (1964c) he tried to argue that the 'discipline' of the pseudo gold standard arose only from the monetary decisions of other policymakers. That is true, of course, but it still might be useful. Interestingly, Howson (2016) revealed that this question came up in correspondence between Friedman and Lionel Robbins, with the latter saying precisely that the British counter-inflation policy just before 1967 had only happened because of the fixed exchange rate. Friedman did not give in, though interestingly in Friedman (1973c) he did recommend fixing the Yugoslav dinar to the deutschmark for this kind of reason. And later on—in Friedman (1985a, 1988d) he seemed to suggest the matter had lost importance since politicians then had much greater incentives to control inflation than previously.

An aspect of the matter that is sometimes obscured by listing notable figures on either side of the debate is the distinctive motivation of Friedman's case in that floating exchange rates. Sohmen (1961) had a position similar to Friedman's, but Haberler (1954) was primarily interested in convertibility and saw temporary floating as a way of discovering equilibrium exchange rates. For Friedman, the idea was very much a means of reducing government involvement—for example in the gold market—and also making unnecessary the range of measures which were in practice implemented in response to balance of payments issues. In America the Interest Equalization Tax was one of them, but as he noted in Friedman (1953d), there was a constant danger of implementation of trade protection. Somewhat later, in more optimistic times, I suppose, Friedman (1966/1969) took the equivalent view that floating would make trade liberalization easier.

Meade (1955), also an advocate of floating rates, also saw the benefit as being to release government from the obligations it gave itself by fixing them. In his case, of course, it was not that floating was a natural accompaniment to a rules-based domestic policy, but just the opposite.

Government was to be freed of its constraint so as to be able to pursue discretionary policy. Again, Friedman's position was different.

In a little twist on the whole story, the main point of Friedman (1960b) is a clever little vignette. Building further on the point that destabilizing speculators must lose money, he looked at financial market speculation as a form of gambling which might deliver utility to those who enjoy it. Then, currency markets, for example, could be viewed as combining a way of setting exchange rates with the supply of gambling opportunities. If other forms of gambling were cheaper to supply, the speculators would leave the financial markets, but if not, then even if speculation were destabilizing, and speculators were paying for the pleasure of gambling, other market participants were benefiting by the same amount (less the real cost of supplying the service). So destabilizing speculation might occur, but would not necessarily be socially costly.

9 Conclusion

So without having considered anything which both proved very important and was in itself a major piece of work, there are already ample grounds for regarding Friedman as an excellent, innovative economist. More than that, much the majority of his work is very much related to practical, empirical enquires. It is clear enough that what he took from Marshall's idea of 'an engine for the discovery of concrete truth' was that theory should be aimed at elucidating the facts. Much of his work is directly seeking to do that—the Friedman-Savage utility function, and his view on trade unions are clear examples. Other pieces do not do that, but argue very much that that is the important thing—the reviews of Lange and Lerner, and his account, contentious though it may be, of the Marshallian demand curve are very much of this kind. And the pieces which are concerned with advocating policy—on rule-governed macroeconomic policy, and flexible exchange rates, for example, if he is not discovering concrete truth, the value of the theory framed with that goal is clearly on display. So he wrote on several topics, there is very much a depth and unity of vision about them, and it is just as marked as the cleverness and variety of his insights themselves.

9
Consumption

1 Theory of the Consumption Function

Friedman (1957a)—*A Theory of the Consumption Function*—is in some estimations, such as that of Walters (1987), his most important work. Certainly it is a very important book, in economics as it is in Friedman's work. In later literature, though, it has been very much misappreciated and seen as having significance wholly or mainly as a rather narrow response to Keynesian theory and being based on a supposedly new theoretical idea devised by Friedman. On that view, the preceding theory was that of Keynes (1936, Chapters 8 and 9), a central point of which was that because of what Keynes called a 'fundamental psychological law', there was a declining average propensity to consume out of income. This supposedly led to a difficulty, supposedly exposed by one or other of a variety of works by Kuznets, in that whereas studies of household expenditure conformed to Keynes' picture, with high-income households having higher savings rates than low income ones, the average savings rate did not increase with generally rising incomes, so that the time-series and cross-sectional data appeared to conflict. Friedman's

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_9

theory, though, handles that point very easily, and on that basis appears as a distinct advance on—as well as a rejection of—Keynesian theory.

That central theoretical idea of Friedman's book concerned the distinction between 'permanent' and 'transitory' consumption and expenditure. The idea of permanent income (or consumption) was never clearly defined in the book but the common sense of it is that it is something like the normal, expected income of a household. Permanent consumption is then equivalently the normal expected consumption. It would be related to permanent income, with taste and demographic factors determining the exact relationship. Then 'transitory income' was Friedman's label for the variation of received income from permanent income, and 'transitory consumption' the equivalent variation of consumption from its 'permanent' level. That variation might be due to unforeseen circumstances, such as medical emergencies, or equally such things as unexpected opportunities to make favourable purchases and the like. In later years the distinction between 'permanent' and 'transitory' on the consumption side has tended to be ignored, or quite possibly forgotten, with all emphasis placed on the difference between measured and permanent income. As the argument was made by Friedman, though, 'transitory consumption' was an important part of the picture.

Concerning income, the first point is that increases in income which are attributable to changes in transitory income—and therefore not expected to be maintained—will not affect consumption as much as increases in permanent income, and possibly hardly at all. Then, the theoretical insight for which the book is probably most remembered concerns the relationship between the observed relationship between income and consumption and its theoretical explanation. The point there was that in any particular period, high-income households would tend to be those with large positive transitory income, and low-income households, those with large negative transitory income. Since both apportion consumption to their permanent income, a 'Keynesian' relationship would be observed—high-income households would have a higher savings rate than low income households. On the other hand, the aggregate savings rate need not change as a result of the growth of output or redistribution. Friedman's theory thereby easily explained the data that posed the difficulty for the Keynesian.

Friedman began with a brief and rather odd introduction. He said that data had initially seemed to confirm Keynes' idea of the consumption function, but that Kuznets (1952) called it into question with data showing that the savings rate did not rise over time. That is already rather peculiar since to the extent that Kuznets' work was crucial, it would be more natural to cite Kuznets (1942) or possibly Kuznets (1946). One consequence is perhaps that Friedman created something of a feeling of his own response being more immediate than it was. He briefly discussed the response to this problem in the form of the 'relative income hypothesis' of Brady and Rose Friedman (1947) and Duesenberry (1949) and said that Tobin (1951) had rejected that view and that his arguments would be discussed later. Then he reverted to the question of Keynes's view and said,

> The doubts about the adequacy of the Keynesian consumption function raised by the empirical evidence were reinforced by the theoretical controversy about Keynes's proposition that there is no automatic force in a monetary economy to assure the existence of a full-employment equilibrium position. A number of writers, particularly Haberler and Pigou, demonstrated that this analytical proposition is invalid if consumption expenditure is taken to be a function not only of income but also of wealth or, to put it differently, if the average propensity to consume is taken to depend in a particular way on the ratio of wealth to income. The dependence is required for the so-called 'Pigou effect'. This suggestion was widely accepted, not only because of its consistency with general economic theory, but also because it seemed to offer a plausible explanation for the high ratio of consumption to income in the immediate post-war period. (p. 5)

And then he immediately cited some studies of the effect of wealth on consumption. That too is rather peculiar since in his discussion of the matter, Keynes had recognized the importance of wealth, the Pigou effect is in any case a very special form of wealth effect,[1] probably of

[1] A 'wealth effect' is an effect arising from a change in wealth. Wealth can change in many ways—capital appreciation, capital destruction, loss of market power, inheritance, etc. The Pigou effect is a change in wealth due to a change in the real value of money (or nominally denominated financial assets) consequent upon a change in the general level of price—a very special case.

no practical significance, and one which had really not featured in the discussion of the consumption function before Friedman dragged it in. But Friedman ended the discussion, with the rather optimistic observation that, 'This brief sketch may convey something of the flavor of the work that has been done' (p. 6), and said that his book would offer another theory.

In Chapter II, he then described a basic indifference curve analysis of a consumer existing for two periods, first considering conditions of certainty, then of uncertainty. The former was surely familiar, and became very routine in later years. It gives rise to the presumption that, on natural simplifying assumptions about preferences, households will seek to smooth consumption and, when their income is not smooth, credit market conditions permitting, they will borrow or lend to achieve this. This, as Friedman said (p. 10) is all eminently sensible, but clearly pointed to the fact that treating current receipts as 'income' for the purposes of determining consumption was not quite appropriate. He briefly noted a similar problem in relation to 'consumption', which as a matter of theory should refer to the planned or actual consumption of services, rather than to expenditure. This led to the definitions of 'permanent income' and 'permanent consumption' as 'the concepts relevant to the theoretical analysis' (p. 11).

In relation to uncertainty, Friedman raised conceptual problems concerning how opportunities and preferences were to be separated in conditions where future prices and income were uncertain. These worries harked back to the concerns about indifference curves in Wallis and Friedman (1942), but here Friedman chose to resolve the issue in effect by assuming it away—the conceptual lines were blurred, but on the level of analysis being pursued there was no way to judge the consequence (p. 15). Uncertainty did however give rise to an additional reason for holding wealth—namely to provide security against unanticipated fluctuations—and that led Friedman to comment that some forms of wealth performed this role better than others, and that in particular human capital did it rather poorly.

This led to the proposition that 'permanent consumption' in a specified period would be a function of permanent income in that period, with the function depending on the interest rate, 'utility factors'—meaning characteristics of the household in question—and the proportion of permanent income attributable to human capital (p. 17). The terminology is a little confusing of course, because there is nothing very permanent about the consumption in a specified period. In certain respects 'planned' would be a better word, as Friedman himself noted (p. 11 n6). The point, though, was that it was these things, not measured current income that were the determinants of 'permanent' consumption.

In Chapter III, Friedman turned to stating his specific hypothesis. The theory, as he had stated it he said was untestable, but by adding further assumptions, he could arrive at a testable hypothesis. To that end, the transitory components of income and consumption were defined as the measured values less the permanent components, and Friedman discussed the issue of whether the permanent components should be treated as lifetime average values, saying—in a passage obviously unnoticed by many textbook writers and the like—they should not. He said,

> it seems neither necessary nor desirable to decide in advance the precise meaning to be attached to 'permanent.' The distinction between permanent and transitory is intended to interpret actual behavior. We are going to treat consumer units as if they regarded their income and their consumption as the sum of two such components, and as if the relation between the permanent components is the one suggested by our theoretical analysis. The precise line to be drawn between permanent and transitory components is best left to be determined by the data themselves, to be whatever seems to correspond to consumer behavior. (p. 23)

The emphasis on 'as if' obviously picks up the idea described in Friedman and Savage (1948) about consumers consulting wiggly utility functions, and which, by the time of Friedman (1957a) had been further discussed in Friedman (1953b). In the discussion of consumption, the point was that the hypothesis was that consumers were to be regarded as apportioning normal consumption to what they took to be their normal income, but the process by which they determined that numerical value was left open. The question of how household impressions of their normal income were formed was a separate matter, and Friedman illustrated various possibilities he thought reasonable, some of which saw 'permanent' income changing in ways which could be foreseeable (pp. 23–25).

Friedman then made the assumption that in any period, transitory income and consumption are uncorrelated with each other, and with their respective permanent components. The commonsense of the former point would arise from, for example, the idea that adventitious variations in income, and opportunities to make favourable purchases are causally unrelated to each other. A crucial point, as Friedman very clearly noted, was that this lack of correlation gave the theory an empirical content that it did not have merely from his earlier statement of it. It made it possible, as he explained, that data would contradict the theory. Then, calling on a mass of existing data, in successive chapters he considered its consistency with the budget studies, the time series data, its relationship to the relative income hypothesis, and then the question of the relative importance of permanent and transitory income, and 'a miscellany' of other empirical ideas.

There is obviously much deeper thinking in the formulation of this theory that later accounts have suggested, but the chapters testing it are extraordinarily insightful, as well as clever, and it is in these that the real power of Friedman's thinking emerges. They are so full of ideas for tests, and interpretations of the data as to defy summarization, but some examples can be given.

There is, for example, what probably seems a fairly ordinary point that in the time series data, there was a good relationship between aggregate disposable income and consumption, but more importantly, that the years of deviation from that relationship were easily explained in terms of the permanent income hypothesis—depression years showed low saving, and years of wartime inflation or in one case, prosperity, showing high saving. Later, considering the difference between blacks and whites, he observed white incomes are higher, so that at a given income level, black consumption should be lower than white, which it was. At that income level, a greater share of black incomes was transitory. On the other hand, there was no reason to expect different average propensities to consume, or elasticities of consumption with respect to income, and there were not.

In Chapter IV (pp. 97–99), Friedman considered a group of consumers who had the same average measured income in two periods, and supposed that amongst the group, average transitory income was zero in each period. In that case average measured and permanent incomes were equal and the same in the two years. Then he said the consumers could be classified according to whether their individual incomes rose or fell between the first year and the second, and one could then compare cross-sectional consumption and measured income in each year. He then proceeded to argue that the regression of consumption on measured income would be steeper for a group of households which had experienced the same change in income than for a wider group that had experienced a variety of changes.

This is because the members of a group of households which, for example, had experienced a fall in income must have had a lower average transitory component in the current period than the previous one. Since within that group, transitory incomes must predominantly have been low, the variance of transitory incomes would be lower than that of the population as a whole.[2] But then differences between their incomes would have to be predominantly accounted for by differences in permanent income. In that case, there should be a strong correlation within the group between measured income and consumption. QED. Examining various data sets (pp. 101–109) Friedman argued the data broadly conformed to this conclusion, although not entirely so, and he admitted there were anomalies he could not explain.

In Chapter V—concerning the consistency of his theory with the time series data—he argued that over long periods of time, the income elasticity of consumption will be greater than it is over shorter periods. This is because over long periods, the variance in income comes to be statistically dominated by variance in permanent income. His analysis of others' data (pp. 125–129) suggested this proposition was confirmed.

In Chapter VI Friedman noted (p. 157) that much of the evidence he had used had also been cited in support of the relative income

[2]Generally, within an 'income change class'—i.e. a group having a similar change in income, the variance of transitory incomes would tend to be low.

hypothesis of Brady and Rose Friedman, Modigliani (1949), and Duesenberry. Only the first twelve pages of the chapter actually concerned the question of comparing the permanent and relative income hypotheses, with the next twelve comparing the relative and Keynesian 'absolute' income hypotheses, mainly for the purpose of questioning the conclusion of Tobin (1951) that the absolute income hypothesis was to be preferred between them.

In the former discussion, Friedman noted that considering a single group of consumers, the regression of consumption on measured income could be converted into one of the ratio of measured consumption and measured income to the ratio of measured income to mean income. For that reason the permanent and relative income hypotheses could not be distinguished by studying a single group of consumers. Friedman observed that the permanent income hypothesis was more 'fruitful' because the parameters of the regression depend on the particular functional form of the consumption function and the extent to which the variance of income was accounted for by the variance of the permanent component, whereas the relative income hypothesis had nothing further to say.

On the merits of the two theories, Friedman first interpreted the apparent successes of the relative income hypothesis in terms of permanent income, finding nothing there problematic, then briefly considered ways of differentiating the theories. For example, he argued that if emulation effects were a driver of consumption, there was no strong reason for their effects to be different in farm communities from others; and yet, the data showed there were such differences.

The second part of the chapter, discussing Tobin (1951) was, Friedman said, motivated by the intrinsic interest of the matter, and by the fact that whereas his argument had suggested the superiority of the permanent income hypothesis over the relative income hypothesis, it had not addressed the absolute income hypothesis. The implication seems to be that Tobin's finding of superiority of the absolute over relative hypotheses, left open the possibility that the absolute hypothesis was to be preferred to Friedman's theory. On the face of it, that is rather peculiar, since one would have thought that Friedman's discussion of the time series data had already established the superiority of his theory over the Keynesian one. In any case, he presented doubts about the power of various aspects of Tobin's argument and presented additional data tending to weaken or reverse some conclusions.

In Chapter VII, Friedman argued that further evidence for the hypothesis was available from consideration of the proportion of the variance of consumption attributable to variance in its permanent and transitory components. That, he estimated from others' data (pp. 184–189). Then he noted that this was also equal to the elasticity of consumption with respect to income, on which he had separate, consistent data. The two methods of measuring the same concept therefore provided evidence for the hypothesis, and Friedman argued that this was particularly important since it was a relationship that had not played a role in constructing the argument, and used quite separate data. Having established the general agreement of the two analyses, he then noted that the relationship was closest when the elasticity data was one and 'permanent' was taken to be three years, and this provided his estimate of the planning horizon of the household.

Chapter VIII consisted of a further collection of shorter arguments with the same kind of feel. They concerned the regression of income on consumption, the application of the theory to particular categories of consumption, the analysis of inequality, the relationship between wealth and income, and to suggest further tests of the hypothesis, some of which were just as clever as the ones he had performed.

2 Reactions to *A Theory of the Consumption Function*

Reactions to Friedman's book came thick and fast—it is an extraordinary testament to the impact of this book that it attracted so much comment so quickly, and all of it saw very much merit in the book, although it will be no surprise that it was also thought challenging and provocative. That was the view of Hoffman (1957, p. 198); it was said to be 'extraordinarily stimulating' by Nerlove (1958, p. 164); and then Charles Schultze (1958, p. 243) despite some substantive criticisms, said 'His brilliant and subtle exploitation of the data to bolster his hypothesis at many points border on sheer genius', and pointing particularly at Chapters IV, V, and VII, Farrell (1959, p. 689) said it was 'a most brilliant and fascinating economic argument'. Champernowne (1958) found fault with some of Friedman's treatment of the formal utility theory, but clearly saw that it did not much matter and highlighted the value of theory which could be tested against other theories and might be clearly rejected by the data if it were sufficiently far from the truth. Tobin (1958, p. 447), memorably, called the book 'one of those rare contributions of which it can be said that research and thought in its field will not be the same henceforth'.

Johnston (1958a) wrote a longer and more critical review, though he also noted the 'striking features' of the book were the specification of a very simple theory, the fact that it suggested a relationship between quantities which are unobservable, and 'the great statistical and theoretical ingenuity that the author displays in attempting to show that his simple hypothesis is consistent with an extensive array of cross-section and time series evidence'. However, he suggested that some of Friedman's arguments were really much less than true tests of the hypothesis—Friedman sometimes assumed one thing was explained along the lines of his theory in order then to 'prove' another thing was consistent with it, for example. To take an example used by Johnston, data showed that income elasticities of consumption were higher for Britain and Sweden than for the United States and Friedman said 'On our hypothesis', such differences reflected 'differences in the relative importance of transitory factors in producing differences in measured income' and that these factors therefore appeared to be of less importance in the UK than in the US (p. 54). This, he said, meant that the need for a reserve for emergencies was less in the UK than in the US, so that the average propensity to consume should be higher in the UK—which it was (p. 57). But Johnston (1958a, p. 433) said the greater importance of transitory factors was something deduced by assuming the theory true, not a piece of data. The finding of a further fact which, according to the theory 'explains' that one shows a consistency of the picture being presented, but that is all. Lydall (1958) took that kind of criticism further, more or less dismissing the conclusions of the book because of it, whilst still noting, though, how clever it was.

Other early reactions fitted the pattern very well—few sought to dismiss the argument; everyone seems to have admired Friedman's cleverness; and there were plenty of grounds for doubting some of his arguments, or that the whole collection was decisive. Just as important, though, is that there was so much interest. Fisher (1956) wrote 75 pages to test Friedman's theory against Modigliani's 'Life Cycle Hypothesis' (favouring Friedman's by a narrow margin). That even appeared before Friedman's book, Fisher having seen a pre-publication version. Then the following year there was a symposium on Fisher's paper to which Friedman (1957b) was a contribution along with others by very distinguished authors, including Modigliani, Klein, and Sargan.

A symposium edited by Lincoln Clark (1958) saw substantial comments by Tobin (1958), Friend (1958), and Orcutt (1958), and a reply by Friedman (1958b). Friend thought the theory failed to explain high savings in the early 1950s, and Friedman (p. 464) acknowledged that as a proper concern. Tobin in particular drew attention to the point that sometimes Friedman's clever responses to the data led him into ad hoc moves which raised questions about the interpretation of the same data in other contexts where those moves were not needed and apparently forgotten about. So, Tobin (1958, p. 452) argued that generally Friedman assumed that for groups of families, mean transitory income and consumption were both zero, but when a test seemed to reject the theory, he pointed at the failure of this assumption as the explanation, but he was not then led to question the assumption generally. Tobin also suggested that unusual expenditure requirements might lead to increased income, for example by secondary household workers taking more work, and in that case transitory income and consumption would be correlated—Friedman simply admitted he had not thought of it (p. 467). It is perhaps interesting that Tobin's comments were all about doubts in Friedman's way of looking at things. Tobin did not himself offer treatments of the data which seemed to call Friedman's view into question, but merely made mostly very plausible arguments that Friedman might not be right. It is sceptical, rather than constructive, but interesting too to see someone who had really grasped the character and many of the details of Friedman's argument, setting himself, with the same sort of skill as Friedman, to look at the matter with doubt, rather than with the aim of showing the theory in action. When that was done, doubts simply could not be excluded.

A critical comment by Houthakker (1958a) is notable for a number of reasons. He expressed, like so many others, his admiration for the 'skill and insight' Friedman displayed in the empirical chapters, and continued 'Much of what he has to say is debatable, but all of it is thought-provoking and intelligent. In his ability to relate observations to hypotheses Friedman is without peer' (p. 399). But he also tried to implement a test of the theory that Friedman had suggested in the book, but not performed, finding it contradicted it. That earned a comment by Eisner (1958)—approved by Friedman (1958c)—to the effect that Houthakker had misunderstood, and the test actually supported Friedman's view. Houthakker (1958b) conceded defeat on the substantive point whilst trying to insist that Friedman's theory was nevertheless inadequately supported.

A point that really deserves attention in assessing the impact of the book though, is that so many serious discussions of the book appeared so quickly. In addition to those so far considered, a particular strand was started by Bodkin (1960)—a similar version of which was also published as Bodkin (1959). In 1950, certain veterans had received what Bodkin thought clearly unexpected insurance payouts. Friedman had mentioned these payments, but they raised opportunities for further testing, which Bodkin undertook, feeling they led to doubts about the theory. Friedman (1960c) responded to Bodkin, setting out to show that 'his results can be rationalized in terms of the permanent income hypothesis' (p. 192). That was achieved, in Friedman's view, with more clever arguments of the kind that fill Friedman (1957a). He felt there were reasons to think that the payment of the windfall may have created expectations of further payments, and that those who had the insurance policies might have high permanent income so that at given measured income, they had negative transitory income (excluding the windfall) and would therefore spend the windfall. There was then further attention to windfalls, notably by Kreinin (1961, 1963), and Reid (1962, 1963), all seeming to support Friedman's view, as well as attracting critical comments from Bodkin (1963) and Bird (1963). The matter, though, reached something of an unusual culmination with Bird and

Bodkin (1966) reassessing the matter and concluding they could not be sure Friedman was wrong!

That was only one other strand, though. Bodkin's piece appeared with two others—Tobin and Watts (1960) and Modigliani and Ando (1960). The first found rational behaviour in the management of household accounts in a way broadly consistent with the permanent income hypothesis. Nevertheless, Friedman (1960c, pp. 191–192) said, 'The use of multiple correlations with so many independent variables as to render interpretation and comprehension almost impossible' meant that their work should be treated as a 'interim stage', pending 'some method of compression into more meaningful terms'. That was one of his many objections to complex econometrics—this time it was in circumstances where the work was supporting his theory. Of the second, just as interestingly, he said that they added more evidence supportive of the permanent income hypothesis to the already impressive amount, and 'The failure yet again to contradict the implications of the hypothesis strengthens the confidence we can place in its at least tentative acceptability. However, it can do so only to a minor degree'.

Friedman himself returned to the matter in Friedman (1963d), refining his thinking about the concepts and measurement of time horizons relevant to the theory (and in Friedman [1963e], in the same volume, commented on some more empirical work on the hypothesis from Liviatan [1963]). Even when support for the hypothesis was very week, as in Friend and Kravis (1957a), or evidence even seemed to contradict it, as did Friend and Kravis (1957b), the importance these authors saw in the book is clear.

Much more could be said, of course, but stopping here highlights the impressive point that practically all this debate arose within just six years of the publication of Friedman's book, and the arguments described started within three. The immediacy and power of its impact is palpable. Few admitted to unqualified acceptance of his theory, doubts about some of the points, or a general scepticism at Friedman's way of proceeding were nearly universal. But so was admiration of his cleverness, and recognition of the significance and power of his argument. And considering the number of responses to his book that appeared so quickly, its extraordinary impact is plain to see.

3 Aspects of the Theory of Consumption Before *A Theory of the Consumption Function*

It should also be clear, though, from a consideration of those reactions, that the interest and importance seen in the book was really not at all attributable to what the later literature presumes. There are few authors who thought there was anything important about resolving a paradox from Kuznets. Asimakopulos (1959), writing a review of the book, would be one, but all he really did was report what Friedman had said, rather than offer much criticism, and he reported what Friedman had said about the problem from Kuznets as well. There are not many who take that kind of line, nor even who really see the sharp point of the book as being to challenge Keynes.

Indeed, the novelty of the book is, as it happens, nothing much like what is usually supposed at all. As is often noted, it appeared just after Modigliani and Brumberg (1955) which also addressed the question of household consumption by considering the matter as one of longer-term planning or intertemporal maximization and to that extent was similar to Friedman's work, as well as just prior to it. There was also the then-unpublished, but widely known, Modigliani and Brumberg (1954/1979). If the idea about long-term planning were really all there was to it, or even if introducing it was intended as a major achievement of Friedman (1957a), then there might be a question as to what Friedman really added.

The study of individual households' consumption generally and of particular goods had a very long history, being the basis of the idea of the Engel curve. Williams and Zimmerman (1935) produced a bibliography of hundreds of such 'budget studies'. These did generate data suggestive of a 'Keynesian' consumption function, in that consumption rose less than proportionately with income. Even at that time, the point that aggregate saving did not rise was noted. Hansen (1932, p. 373) saw that, saying 'As incomes rose all around, the whole manner of living changed'. That reveals that he did not think the point raised a problem then, and the same thing is very much true of studies of aggregate consumption after 1936. Hansen (1941, Chapter 11), addressing this question clearly thought nothing of any difficulty posed by the fact of consumption rising with national income. Citing a pre-publication version of Kuznets (1942), he simply stated the point and moved on. Samuelson (1943) commented on the same thing, distinguishing the cyclical and secular aspects of the problem, and treating the Keynesian consumption function as appropriate to the former. For the latter, he said that the theory did not imply increasing savings, noted data from Kuznets (1941) showing saving had not in fact risen, and said 'the most plausible explanation of this is to be found in the hypothesis that our enlarged scale of wants was causing an upward shift in the consumption function at about the same rate as improvements in our production potential' (p. 33). Similarly, none of Mack (1952), Ferber (1953), and Hagen (1955), each surveying the economics of consumption, showed any sign of thinking the data on the long-term constancy of savings raised any particular difficulty. Modigliani and Brumberg (1955) did not even refer to Kuznets.

The explanation of this attitude is presumably that the Keynesian analysis was seen as addressing the specific problem of understanding business cycles. It was that role that had led Hansen (1947) to the view that Keynes' invention of the consumption function was the most important contribution of *The General Theory*. Sure enough, when attention turned to the question of analysing the possibility of postwar recession, the issue of how secular growth would affect the position did arise, but was straightforwardly handled by Smithies (1945) who simply added a time trend to the basic consumption function. Explaining that trend was not his main concern, but he said it was due to urbanization as migrants to the cities consumed more, equalization of incomes, and the increasing expectations as to what were necessities. His approach obviously did not offer much of a fundamental explanation, but for him, it served its purpose and for later readers shows that the fact of the long-term growth of consumption was perfectly well appreciated. Or as Samuelson (1948, p. 420) briefly put it, 'Throughout all our history the Consumption-income schedule has been shifting upward'.

Modigliani (1949), pointed out the limitation of Smithies' approach, noting that its predictive accuracy depended on the coincidence of the rate of growth of income remaining the same as the apparently exogenous time trend of consumption. It is a sound point although not one that allows Smithies any leeway for having been addressing a more specific question. For Modigliani, though, that was not a preliminary to offering any better theory. He merely pointed out (p. 385) that economic growth takes the form of new commodities becoming available rather than merely of a greater quantity of the old ones being produced. He seems, very reasonably, to have taken the view that the fact that aggregate consumption rises as national income grows entirely unproblematic. If anything, it was the explanation of the data from the budget studies that he found more difficult, expressing sympathy for the idea that household consumption was determined by relative income position, so that households which were poor relative to their group or community consumed more than the richer ones. The main goal of his paper was to analyse cyclical behaviour, and he suggested that downturns tend to make incomes more equal amongst the rich, maintaining consumption; that the unemployed produce nothing, but certainly consume something. Then in upturns, new consumption habits are acquired which are not entirely shaken off in downturns; and that 'income receivers are inclined to look upon a fall in income as temporary and therefore be less willing to make any further painful adjustments' (p. 387).

Modigliani took the idea of the importance of relative position from Brady and Rose Friedman (1947). They offered next to nothing in the way of theory, and drew only very limited conclusions, but did present a good amount of data suggesting that the richest in any group—such as 'southern white farmers', or 'residents of New England mid-size cities'—saved more than lower-income members of the same group, but not more than lower-income members of other groups. That line of thinking was developed much more in a book by Duesenberry (1949). He specifically pointed out that thinking about microeconomic behaviour would guide theory—a point also made by Mack (1952) and Ferber (1953, p. 4). He offered a very serious consideration of the matter and adopted many lines of thinking that would become commonplace in later years, as well as engaging closely with the details of the available evidence. He advanced the view that a household's consumption expenditure depends on that of others with which it has contact, with those with closer contact having greater weight. In a manner obviously suggesting the idea of 'keeping up with the Joneses', there was a 'demonstration effect' of one household's consumption on another's. He took the problem of confronting the theories with the data very seriously, earning the admiration of Ackley (1951) for doing so, and presented a number of imaginative uses of others' results—he may not have been as impressive as Friedman, but it was analysis of the same general type, and also skilful. On the question of the long-term trends in saving, he considered data on the stated income aspirations of individuals; and in the manner of Brady and Rose Friedman, noted the comparison of relative savings rates of members of different groups and residents of different cities. He remarked (p. 47) that although the data could never prove a theory true, it could show it false, but in the end found it consistent with his theory and considerably more difficult for a Smithies-type, Keynesian consumption function with a trend, and he specifically considered the arguments of Kuznets (1942). Then, in what became the best-known part of the book because of its separate publication as Duesenberry (1948) he considered the cyclical behaviour of saving and introduced the idea that households resist reducing consumption below a level it had recently reached, again alleging consistency with plausible psychology concerning habit-formation, and arguing that the data supported his view whilst being hard to explain on any other. Here one of the points that he made was that the tendency of econometricians to presume a hypothesis correct and worry about estimating parameters had left unused a great deal of microeconomic data that could indicate whether a hypothesis was correct, and that research would be more effective if hypotheses were expressed in terms of the behaviour of individuals rather than in terms of aggregate results.

Amid all this, as noted by Hynes (1998), the theoretical points sometimes taken to be Friedman's important insights were widely appreciated. Vickrey (1947) may be the first to consider the high savings rate of high-income households as being potentially explained by the fact that in any sample, a disproportionate number of such households will be enjoying what is for them, unusually high income.

In any case, that point was also seen by Mack (1948), Katona (1949), and Reid (1952), before being very much put to use by Modigliani and Brumberg (1955, p. 409)—all before Friedman. The idea that this insight came from Friedman, or that consequently, the importance of his book that solved a problem raised by Kuznets, is a therefore travesty. Thomas (1989), focussing on econometric studies, rather politely called the usual story 'stylized history' (p. 131 n1). Specific appearances of the idea that household expectations about their income affect current consumption were also commonplace—Gilboy (1938, p. 139) noted the growth of social security had reduced incentives to save, and so must have been imputing a forward-looking understanding to households. Hansen (1941, Chapter 11), discussing behaviour in the course of a business cycle wrote, 'It requires, apparently, an extremely severe deflation, such as that in the early thirties, to bring about aggregate dissaving or disinvestment. In more normal depressions, even at the bottom, income exceeds consumption'. He said nothing about why that was so, and presumably thought that the reasons for consumption smoothing were sufficiently apparent. Katona (1951) considered various questions of expectations in some detail. Duesenberry (1949, pp. 65–67) considered the matter of how saving would be affected by expectations of changes in income, although on that point, he thought the lack of measures of those expectations meant little could be said; and later made the point that those who were unemployed but expecting to be reemployed would maintain their consumption more than others who were permanently in the same income group (p. 82).

It can be seen, then, that at the time Friedman wrote, the theory of consumption was not nearly so benighted as certain later authors have made out. But from the point of view of considering Friedman's contribution, just as important as the wide-recognition of these points is that it can hardly be in doubt that they were very much part of Friedman's own understanding. A large quantity of work on consumption and budget studies had been conducted through the annual meetings of Conference on Research on Income and Wealth, with which Friedman was very much involved; in the Preface to Friedman (1957a), he said that he had stayed in touch with Dorothy Brady and Margaret Reid, noting that the latter had conducted some testing of the same ideas as in the book. Their close connection is also attested by Bronfenbrenner (1958, p. 184) who said that Brady (1956) 'reads like a preliminary sketch of the Friedman 'permanent income' hypothesis'. Indeed, she was another with the idea that those with low incomes would have a tendency to be those with incomes which were abnormally low by their own standards and hence to save little (pp. 140–141). In addition to saying he had a short note of the theory dated 1951, Friedman also said that Brumberg had read an early version of the manuscript (p. x). Since Ando and Modigliani (1963, p. 56 n2) say Brumberg died in August 1954, Friedman must have known about the Modigliani and Brumberg papers well before his book was published. That does make it a little odd that Modigliani (1949) is the only work by that author to which Friedman actually referred, saying (p. 6 n12) only that he became aware of the Modigliani and Brumberg papers 'After completing an earlier draft of this monograph'. That does not make it very clear why a later draft could not have incorporated some comment on them. But the important point is that Friedman was deeply engaged with this literature and there cannot be any doubt that he was aware of the general content of the discussion.

To some extent it can be argued that Friedman's book brings together theoretical ideas, but it simply cannot be that the devising of them, or the solving of a paradox raised by Kuznets, with which he is so often credited were seen by him as the important contributions of the book; and nor should they be seen as such by anybody else. That, though, does not mean it is not an important book.

4 The Importance of *A Theory of the Consumption Function*

Rather than anything like that, the appeal and importance of Friedman (1957a) lies in very much the characteristics Friedman originally identified: It suggests a simple hypothesis, powerfully attractive, and having enormous analytical power, and provides a great deal of empirical support for it. The motivating theory—the Fisherian theory of intertemporal optimization—was not at all original, and not at all surprising.

Nothing very much about particular explanatory insights—such as that low-income groups contain a disproportionate number of individuals with incomes which are unusually low by that individual's standards—was original or surprising either. In any case, Friedman shared these things with Modigliani and Brumberg (1955) which is much more theoretically intense. If it is the working out of the theory that is valued, they are the more important authors.

What was special, though, was Friedman's clarity of vision in the empirical possibilities of simple theory. He might be set alongside Mack (1952)—alongside, but in contrast to her. She had noted the 'baffling complexity' of the findings of the literature—of which she plainly had enormous knowledge—but said that progress would be made by accepting this complexity and addressing it with a greater willingness to see all manner of considerations as important. But Friedman had a very simple theory, and explained so many of those complexities with it.

He equally stands out in seeing the importance of finding ways to test theory, and framing additional assumptions to make that possible. Duesenberry (1949) and Tobin (1951) saw the importance of testing just as clearly, and have something of Friedman's vision and imagination about the construction of the tests as well. But Friedman has 200 pages of it—much more than either of them—and seems to have arguments from every angle, and answers to every problem. Indeed, his arguments have their limitations, as his critics pointed out. There are ad hoc moves which, of their nature, are applied inconsistently; there are generous interpretations; and there are avenues left unexplored, where there might have been trouble if the exploration had been undertaken. Certainly, if it is a fault, then the book can be faulted for being more of an attempt to show that the data can be seen as fitting the permanent income hypothesis than it is an attempt to refute that theory.

But if the matter is taken on the terms Friedman envisaged, the question is not whether some of his arguments are weaker than others, or whether each one leaves room for doubt. The question is about the persuasive power of all his arguments combined—including the theoretical appeal of the approach, its consistency with utility-maximization, its simplicity, and all Friedman's accounts of the data in the light of it. He may have felt he has some knock-out blows against other views, but there is no sign he thought any particular point was by itself a decisive confirmation of his own. He achieved persuasion by the accumulation of points. Friedman's view was that all this meant that the permanent income hypothesis should be preferred to either the absolute income hypothesis or the relative income hypothesis. One can see the point precisely in his summing up on the question of the merits of the relative income hypothesis, where he said,

> The permanent income hypothesis seems to me superior to the relative income hypothesis on three grounds: first, it has a simpler and more attractive theoretical basis in that it uses the same constructs to account for cross-section and temporal results, whereas the relative income hypothesis introduces very different considerations to account for the declining ratio of consumption to measured income in budget study regressions of consumption on income and for the constant ratio of aggregate consumption to aggregate income over long spans of time; second, it is more fruitful, in that it predicts a wider range of characteristics of observed consumption behavior; and finally, the evidence that we have cited seems to fit it somewhat better. (pp. 168–169)

Here, Friedman is clear and decisive as to which theory he feels does better, but cautious as to how sure to be. On that basis he surely made his case.

There is a further point in that comes clearly into view in seeing the permanent income hypothesis more in relation to Duesenberry (1949) than Keynes (1936). A good case can be made that by 1957, Duesenberry's was the better established theory, even if it was not the standard fare of elementary exposition in macroeconomics. What Duesenberry had done was to advocate his theory on the basis that it was 'realistic' in the sense of reflecting an understanding of psychology. It was, in that way, a large step in the complexity-embracing direction Mack had thought it necessary to go.

But Friedman (1957a) triumphantly rejected the view that such realism was necessary to finding theory which gave acceptable results—the assumption that households were maximizing according to standard utility theory was consistent with the data and, indeed explained it better than Duesenberry. For example, Friedman quoted Duesenberry

(1949, pp. 37–38) saying that at low incomes, the desire for present consumption is so strong as to mean there is almost no saving and commented that 'As is shown in Chapter II, this analysis is, to say the least, most unsatisfactory on a purely theoretical level' (p. 167). Indeed, precisely. To any mainstream economist of thirty or forty years later, Friedman's position would seem very ordinary. The notion that the capability of intertemporal optimization is somehow wealth-dependent is 'most unsatisfactory' would be thought a very mild criticism. It is anti-theoretical; something like a denial of economics.

Although Friedman was criticized over this and points like it (by Houthakker [1958a], Pettengill [1958], Steissler [1960]), it is the outlook which creates precisely the strength of his thinking. He did not begin with 'plausible psychology', but with rational behaviour. It is possible, I think, to fail to appreciate both how unusual that was, and how surprising, as well as, to some, shocking, it was. Indeed, it seems Patinkin (1972, p. 9) did fail to appreciate this when he argued to the effect that before Friedman, the full implications of the Fisherian intertemporal analysis had not been appreciated. There is no evidence of that. Rather than there being any problem about understanding the implications of the Fisherian theory, it seems those implications were just not thought terribly important amongst those who thought that an understanding of 'realistic psychology' was of the essence of a theory of consumption.

Seen in this way, Friedman's achievement was a gigantic one. Friedman (1957a) is certainly only one component of the change, but the moving away from organizing thinking around prima facie psychological presumptions, and towards the explanation of data in terms of more or less rational behaviour is probably the crucial transformation in economics of the mid-twentieth century. Because it was in an area so dominated by psychological thinking, reaching its highest point in Duesenberry (1949), and because he brought so much skill as well as so much evidence to the matter, Friedman's book is a crucial one in opening new horizons and bringing that change.

On the other hand, that line of thinking can easily be taken too far. And such later accounts of the matter as Evans (1984, pp. 105–106) do take it too far. He said,

> like Ando and Modigliani, Friedman made the assumption of a homothetic utility function, so that consumption is again proportional to lifetime resources with the constant of proportionality depending on current and expected real interest rates. The main differences between the theories are that, first, Friedman ignores the influence of assets, and, second, he uses the approximation that the consumer's length of life is infinite…

Far from the mark as it is, there is nothing terribly exceptional about that—many others along the same lines could be quoted.[3] But this is every bit as much a travesty as the story that the point of the book was to resolve Kuznets' so-called paradox. It is not that it imputes to Friedman a sophistication and modernism that was not there—though if that is what is meant by 'sophistication', it does do that. It is that it denies to Friedman the true character of the insight he had, and the true strengths of the work. Friedman's book is both much more commonsensical than Evans suggests, and much more inventive. Certainly Friedman's consumers are optimizing, but they are, as it were, optimizing humans; not optimizing supercomputers. They have an idea of their normal economic situation, and spend accordingly. Starting there, the brilliance of the work comes from the analyst's working out of how to represent that behaviour in a way that facilitates the instructive organizing of the data; and its achievement lies in charting that course for economic analysis.

This point perhaps comes through most clearly in relation to the question of the empirical meaning of 'permanent income'. So many critics complained that Friedman was vague about this, evidently feeling that there should be some theoretical postulate that specifies how consumers determine their permanent income. And indeed, in the imaginings of later

---

[3] Here are two from the Journal of Economic Literature. Actually in a survey of the consumption literature, Attanasio and Weber (2010, p. 694) said, 'All these observations clearly contradicted the implications of the Keynesian model and led to the formulation of the life cycle and permanent income models' and the 'main implication of the model that was first stressed in Friedman (1957): consumption depends on the present discounted value of future expected income' (p. 707). Lusardi and Annamaria Mitchell (2014, p. 6) said, that in Modigliani and Brumberg and Friedman the consumer 'is posited to arrange his optimal saving and decumulation patterns to smooth marginal utility over his lifetime'.

writers, that postulate is sometimes said to be that it is an average of lifetime income, or assumed to be determined by an adaptive expectations mechanism, or, sometimes, apparently, both. Friedman's approach was quite different. The theoretical point was that consumers set consumption according to what they regard as their normal income. But it is an empirical matter to determine how that is done. That did mean that Friedman was drawn into, in effect, presuming the theory true in order to estimate the period associated with 'permanence', and that is scientifically unsatisfactory. But the common sense aspect is there too—and that distinguishes Friedman from later authors who adopted much more axiomatic approaches.

On the matter of the book being one of Friedman's attacks on Keynesianism, a reasonable reading points in rather a different direction. Certainly, Friedman said that he did not accept that the income-expenditure theory was well established (p. 236). But there is not much in the book that is an attack on it. The presentation of the Kuznets data in the introduction seems to set up Keynes for knocking down, and that view is certainly encouraged by the gratuitous appearance there of the 'Pigou effect'. But in the rest of the book, nothing of the sort happens. Even the point about Keynes' theory and Kuznets' data looks as if it might have been paraphrased from the introduction to Duesenberry (1949)—where that author, too, was presenting himself as solving a problem, though with considerably more reason than Friedman, years later. Friedman's theory was shown to be able to explain cross-sectional and time series data, but there is no special criticism of Keynes. Indeed, there is never a mention of him, after the introduction, and the word 'Keynesian' appears once in the text. On the contrary, the appearance is very much that in analysis really aimed at understanding consumption, the Keynesian consumption function had been rejected before Friedman came on the scene, and the rival hypothesis to his was Duesenberry's.

In the conclusion the point is made that the permanent income hypothesis suggests the multiplier will be small. It is interesting though that the only point following from that was that it suggested greater cyclical stability than would occur with a larger multiplier. The seemingly obvious point that fiscal policy will be less powerful was not made—no conclusion was drawn about policy. There is an obvious opportunity there to disparage the Keynesians, but Friedman did not take it.

He gave as much attention to the idea of 'secular stagnation'—associated with Alvin Hansen, though he did not name him—as he did to the multiplier. That is rather odd too since, having invoked the Pigou effect, he might have said that Pigou (1943) is easily seen precisely as a rebuttal of the possibility of secular stagnation. Instead, he said the problem arose from the two ideas of declining investment opportunities and rising savings rates, but that his theory removed one of those problems. It is interesting that Backhouse and Boianovsky (2016) argue that in Alvin Hansen's hands, the secular stagnation thesis was entirely about the disappearance of investment opportunities; and was very much more about that than saving in the hands of most of his followers. Only later, did it come to be seen as a concern about rising full employment savings. It may be, I suppose, that Friedman's advertising of it as a problem of rising saving may be a part of this transformation, albeit that he was arguing precisely that the savings aspect of it was illusory.

It is also surely worth noting how Friedman entitled his book. It was, '*A Theory of the Consumption Function*'. Friedman and Friedman (1998a, p. xii) made a mistake about that, referring to it as 'The theory of the consumption function', but the original was more modest. It could have been The '*General Theory of Consumption, Income and Saving*', but it was more modest than that as well. On the other hand, it would have been no pretence to call it, '*A Theory of Consumption*'. Instead, Friedman chose, '*A Theory of the Consumption Function*'. Not only by the testimony of Hansen (1947), but because it certainly plays such a role in the construction of his whole system, the consumption function is a deeply Keynesian concept. So although Friedman may later have presented it differently, when his book was written, he chose to put it squarely in the Keynesian tradition.

10
Methodology

Of all his work, it is Friedman (1953c), on 'the methodology of positive economics', that has attracted the most peculiar, and—so I shall argue—misconceived commentary. It is misconceived not because there is some interpretation of this famous essay that everyone heretofore has missed, and which I am about to reveal, but because it is such a poor, muddled, often facile, incoherent essay. There is no sense in supposing it to have an interesting interpretation. It may well be that it has been historically important for the reason that ideas that readers have drawn from it have framed the practical, working methodology of economists. But that merely reports something about the ideas drawn from it, not the quality or coherence of the presentation. As far as understanding good economic methodology is concerned, it lacks sufficient logic to offer anything, and is just not worth worrying about. So, its interest cannot lie in any methodological principles it propounds, but there may be some in understanding Friedman, and perhaps reactions to him.

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_10

1 Friedman's Argument

Friedman began by citing the 'admirable book' (p. 3) by John Neville Keynes (1891) and describing a distinction he found in it between a 'positive science', a 'normative science', and an 'art'. He noted Keynes had said they were often confused, and that whilst his own essay was mainly concerned with methodological problems of hypothesis testing, since the confusion described by Keynes was still commonplace, he would begin with a discussion of positive and normative economics.

He said first that people were 'inevitably tempted to shape positive conclusions to fit strongly held normative preconceptions' (p. 4). However, differences about economic policy amongst disinterested citizens arose from differences about positive much more than normative questions. That meant that progress depended principally on progress in positive economics. Moving to describe positive economics he said,

> The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e. not truistic) predictions. (p. 7)

He went on to say that a positive science was therefore partly a language, partly a body of hypotheses. He said that 'viewed' as a language, theory had no substantive content. Of the body of hypotheses, which was the main subject of his essay, he said,

> theory is to be judged by its predicative power for the class of phenomena which it is intended to "explain." Only factual evidence can show whether it is "right" or "wrong" or, better, tentatively "accepted" as valid or "rejected." As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. (pp. 8–9)

and that a hypothesis would be rejected if its predictions 'are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted' (p. 10) and elaborating on that last point, said,

> great confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been "confirmed" by experience. (p. 9)

That very brisk observation, and one or two others elsewhere in the essay are the basis on which it is said to espouse a falsificationist approach. Then he observed that the 'validity of a hypothesis in this sense' (p. 9) would leave an infinite number of hypotheses available to explain any set of observed facts. Consequently he favoured the selection of hypotheses exhibiting 'simplicity' and 'fruitfulness', accepting a degree of vagueness in these ideas. Simplicity he characterized in terms of little knowledge being required to make a prediction, and fruitfulness in terms of 'the more precise the resulting prediction, the wider the area within which the theory yields predictions, and the more additional lines for further research it suggests' (p. 10).

He continued that testing hypotheses could be difficult and this had the effect of promoting purely mathematical reasoning, and economics needed to be more than a collection of tautologies. A second and more serious effect was 'to foster misunderstanding of the role of empirical evidence in theoretical work' (p. 12). After saying that some data might be used in constructing a hypothesis, and other data in testing, and perhaps reformulating it, he then said that confusion occurred because it was sometimes supposed that,

> hypotheses have not only "implications" but also "assumptions" and that the conformity of these "assumptions" to "reality" is a test of the validity of the hypothesis different from or additional to the test by implications. (p. 14)

And seeking to elaborate on this said,

> In so far as a theory can be said to have "assumptions" at all, and in so far as their "realism" can be judged independently of the validity of predictions, the relation between the significance of a theory and the "realism" of its "assumptions" is almost the opposite of that suggested by the view under criticism. (p. 14)

Of this, he said that important hypotheses would have descriptively inaccurate assumptions and that,

> in general, the more significant the theory, the more unrealistic the assumptions (in this sense). (p. 14)

From there he went on to say that the relevant question about assumptions was not whether they were descriptively accurate, but whether they were 'sufficiently good approximations for the purpose in hand' (p. 15) and the *only* way to determine this was to see whether the theory yielded accurate predictions. Here, apparently, the verisimilitude of assumptions was a strict irrelevance to model assessment. In this connection he raised the matters of monopolistic competition and marginal analysis. Of the first he said the development of the theory had been motivated by the view that their assumptions were descriptively inaccurate. The second concerned the question of the 'marginalist' analysis of pricing and wage determination. Citing a number of participants in a particular debate that blew up in 1946, he said that both sides had neglected the question of whether the predictions of the marginalist theory were correct, and instead concentrated on the 'largely irrelevant' (p. 15) question of whether businessmen think in terms of the same concepts as the theorist.

Moving to a fuller discussion of the question of the significance of the accuracy of assumptions, he made some of the most memorable arguments of the essay. One concerned the formula for a body falling in a vacuum. Friedman argued that testing the theory by its assumptions would mean determining whether actual air pressure is close enough to a vacuum. But, noting that a ball and feather would behave differently when dropped, said that no sense could be given to the question of whether the atmosphere was near enough a vacuum. The usual formula, strictly applicable only to a body falling in a vacuum, was accepted, he said, 'because it works, not because we live in an approximate vacuum' (p. 18).

Considering the question of whether it might be said that because the assumption of a vacuum is false, the theory does not work for a feather; he said that the correct response was that because the theory does not work, then its assumptions are false for a feather. This led him to the point that, as he said, there is an 'entirely valid use of "assumptions" in *specifying* the circumstances for which a theory holds' but that was not to be confused with the idea that assumptions could be 'used to *determine* the circumstances for which a theory holds' (p. 19).

From here he moved immediately to describe the 'as if' approach to the acceptability of assumptions for which the essay is probably most famous. He first gave the example of the location of leaves on a tree, saying one might hypothesize that they 'are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives' (p. 19); and then repeated the example of the expert billiard player from Friedman and Savage (1948). Of this, he said that a businessman's claim to set prices according to average cost, with allowances for market conditions, is no more informative than if the billiard player says he 'just figures it out', and rubs a rabbit's foot to make sure (p. 22).

In other words, interview data about business practice had no value in theorizing about such things as how prices are set. And from there, he said, it was a short step to the view that firms behave as if maximizing profit in conditions of full information. That view was supported by the 'evidence' (p. 22) of the point that firms which do not behave so as to maximize profit would be forced out of business, and by 'countless applications of the hypothesis to specific problems and the repeated failure of its implication to be contradicted' (p. 22). Those applications, apparently, were scattered and hard to document but Friedman concluded, 'Yet the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony to its worth' (p. 23).

He then considered what roles assumptions did play, adding to the idea that they could specify the conditions under which it was expected to apply, two other ideas. These were that they could be 'an economical mode of describing' the theory, and they might 'facilitate an indirect test of the hypothesis by its implications' (p. 23).

His explanation of the first of these began with the point that the idea that leaves grow so as to maximize their exposure to sunlight could be replaced by a much longer explanation of the same idea, but then he drifted into other points. First came an idea that a theory consisted of an assertion about certain matters being important and others not, together with a set of rules defining the situations in which it was to apply. And then it went to a discussion of the point that those rules could never be precisely defined, and from there to a discussion of the meaning of the expression 'crucial assumptions' during which Friedman claimed that there are usually many ways of describing a model completely—'many different sets of "postulates" which both imply and are implied by the model as a whole' (p. 26). And some assumptions were called 'crucial' because of their convenience or 'intuitive plausibility, or capacity to suggest, if only be implication, some of the considerations that are relevant in judging or applying the model' (p. 26).

In elucidating the latter point, he said, 'what are called the assumptions of a hypothesis can be used to get some indirect evidence on the acceptability of the hypothesis in so far as the assumptions can themselves be regarded as implications of the hypothesis' (p. 28). Then he suggested assumptions could facilitate such indirect testing by suggesting similarities between one theory and another. As he put it,

> a hypothesis is formulated for a particular class of behavior. This hypothesis can, as usual, be stated without specifying any "assumptions." But suppose it can be shown that it is equivalent to a set of assumptions including the assumption that man seeks his own interest. The hypothesis then gains indirect plausibility from the success for other classes of phenomena of hypotheses that can also be said to make this assumption. (pp. 28–29)

This idea, he used to explain systematic differences of opinion between various groups. Economists, more than sociologists, he said, would be inclined to accept a particular theory which was based on the idea of maximizing behaviour because of the experience of dealing with other theories where that assumption led to good results.

After this, Friedman observed that much criticism of economic theory focussed on the unrealism of assumptions and criticized those who said or implied that since the world is complex, economic models needed to be as well. This led him to return to the matter of monopolistic competition. He said that Marshall had supposed it was useful to consider industries and this led to consideration of the ideal types of competition and monopoly. Friedman claimed that firms could not be absolutely classified as one or the other, but that that classification would depend on the problem under consideration. He raised the example of a tax on cigarettes and said the model of perfect competition would give a good account of cigarette firms' behaviour. On the other hand, citing actual outcomes during the Second World War, he said that if the question were about their response to price control, then it would give poor results. So in this case, the firms could not be treated as perfect competitors.

He said that it would be desirable to have a more encompassing theory and that such a theory 'must have content and substance; have implications susceptible to empirical contradiction and of substantive interest and importance' (p. 38). He did not expand on that point, but went on to claim that the theory of imperfect competition was an attempt to construct such a theory, but failed because of the impossibility of giving a satisfactory definition of an industry.

That took Friedman to his conclusion. He reasserted several things he had already said, and expressed his view that monetary dynamics was the area most in need of further development.

2 Responses to Friedman

The paper was quickly noted and there are about 140 citations of it in JStor articles in the first 25 years after its publication. Few of them, though, contain much discussion. The most common reason for mentioning him is to note—with greater or lesser reservation, but usually broad approval—the point that the realism of assumptions is unimportant. Johnson (1968, p. 5) even seems to have regarded this attitude as the defining characteristic of 'positive economics'. Rottenberg (1956) perhaps made a little more of it, in that he specifically defended the approach of a new piece of work in Friedmanesque terms. The idea appeared in a textbook in Lipsey (1963, p. 12 n1) although he just said Friedman offered one view of the character of 'assumptions'. The same idea, attributed to Friedman was also quickly applied outside economics, narrowly defined, in Downs (1957, p. 21) in his seminal 'Economic theory of democracy'.

A general appreciation of the character of responses to Friedman can be taken from a sampling from JStor. It seems that only rather infrequently did brief comments on the essay pick out other points from it—Weston (1955, p. 131) affirmed that assumptions need not be realistic, but also said it was essential that models yielded testable predictions, calling Friedman's essay 'profound'; Louis Dow (1961) mentioned the essay for the distinction between positive and normative questions. Discussing management science, Dale and Meloy (1962) did not actually mention the question of assumptions, but cited Friedman for the view that only the predictions of theory were to be tested.

There was more general approval too—or less specific approval anyway—from Clark Allen (1954), reviewing Friedman's book, who thought the methodology essay should be required reading for Ph.D. students, without indicating in particular why. Oliver (1954) thought it 'pedagogically helpful' despite what he regarded as Friedman's deployment of a straw man in the form of those who object to unrealistic assumptions. Machlup (1955) thought the essay excellent except for its failure to address the issue of the 'understandability' of assumptions, 'in the sense in which man can understand the actions of fellowmen'—a point which, of course, might be crucial. There were also occasional mentions of the importance of refutability, and very occasional ones of such things as the positive–normative distinction, or the advantages of simple models.

A quick criticism was something of a rarity, although Boulding (1954)—a bit like Machlup, perhaps—just said that Friedman may have lain too much stress on forecasting outcomes rather than understanding processes. Katona (1968) agreed with Friedman that a theory could not be tested by testing its assumptions but went on to make the rather powerful point that making assumptions more realistic might nevertheless make theory better.

The situation with longer responses was not quite the same. Koopmans (1957, pp. 137–140) quite rightly felt that Friedman had not taken seriously the question of stating how it was to be determined what would count as a refutation of a theory. Archibald (1959), reviewing Koopmans, criticized Friedman over the question of the testing of assumptions, noting that different assumptions are made for different purposes, and it might well be appropriate to test some, but also specifically acknowledged his 'great debt' to Friedman's 'writings on methodology' (p. 61 n1). And then Rotwein (1959) wrote the first article specifically for the purposes of criticizing Friedman, finding him inconsistent in crucial ways. Samuelson (1963) also criticized him, saying it was absurd that the inaccuracy of assumptions should be said to be anything but a detriment to a theory, and labelling that idea the 'F-twist', with a rather obviously provocative intent. He said, 'Some inaccuracies are worse than others, but that is only to say that some sins against empirical science are worse than others, not that a sin is a merit or that a small sin is equivalent to a zero sin' (p. 233). Melitz (1965) argued the importance of testing assumptions, as one component of the assessment of theory; de Alessi (1965) criticized the combination of views that theory might be viewed as a language and as such had no substantive content. Klappholz and Agassi (1959) sympathized with Friedman's view that much methodological criticism of economics was misconceived but argued, in effect, that he took his own prescriptions too far. There were plenty more along these or similar lines.

In the 1970s, it started to be said or implied, for example by Blaug (1975), or Finn (1979), as well as Boland (1979), and slightly later McCloskey (1983) that Friedman's paper had become very widely read, and the 'as if' approach had often been adopted from him. There continued to be many very brief mentions of the paper, still usually in connection with the reality of assumptions and the idea of 'as if' modelling, but a higher proportion were outside economics, or mentioned him because the methodology was being said to give rise to some incorrect view on a substantive matter. The simple invoking of him as authority for the legitimacy of 'as if' reasoning became less common. So, presumably as a consequence of the wide understanding and acceptance of his view, it became the objectors to, rather than the advocates of, Friedman's view that started to predominate amongst those who specifically cited him.

In shaping the subsequent debate, and establishing the place of Friedman (1953b) in the literature, though, a most important moment came with the publication of Boland (1979). He observed a peculiarity of the literature in that, he said, textbook writers discussing methodology thought Friedman's essay authoritative, but journal articles about it were overwhelmingly critical, and went on to favour the insight of the textbook writers saying, 'Every critic of Friedman's essay has been wrong' (p. 503). He declared that the methodological position taken in Friedman's paper was not 'positivist' as almost everyone supposed, but rather 'instrumentalist', and that when the paper was so seen, it became apparent that it contained none of the errors the critics saw. Boland's argument perhaps does not run quite as far as this suggests because he criticized Friedman's critics for rejecting instrumentalism (e.g. p. 518), and thereby, thought Boland, failing to see the internal consistency of Friedman's essay as an instrumentalist essay. If one is interested in getting the methodology right, though, criticizing Friedman for arguing an incorrect case, even if he is consistent in doing so, would seem to be a reasonable approach.

Boland's intervention surely raised interest in Friedman's paper so that there followed more criticisms of it, and a partly separate strand of attacks on Boland's, and needless to say, responses to both streams. The critical literature arising from Friedman's essay grew enormously. That is probably a sufficient explanation of the essay coming to be so widely seen as important, or revolutionary, and indeed to seem so important as to warrant, in due course, the production of Mäki (2009a), the proceedings of a conference celebrating the 50th anniversary of its publication. Of Friedman's essay, Mäki (2009b, p. xvii) said it had become,

> the most cited, the most influential, the most controversial piece of methodological writing in twentieth-century economics. It is also poorly understood – and indeed hard to understand, given its richness and obscurities. And it remains highly topical for the foundational debates in and around economics in the early twenty-first century.

Another development was that following Boland's discovering that Friedman was an 'instrumentalist', other authors chose to attach other labels to Friedman's methodology so, for example, Hirsch and de Marchi (1990) found him to be a pragmatist in the mould of Dewey; Hoover (2009) saw 'causal realism' in Friedman and Schwartz (1963a); and Mäki (2009c) himself, offered what he called a 'realist reading' of the paper, whilst also picking up a theme from Mäki (1986), saying that there was no single message. Still, apparently, expressing admiration for the paper he explained the absence of a single message as arising from various specific remarks in the paper, the point that the 'as if' approach is compatible with many philosophical outlooks and Friedman argues 'from sets of mutually incompatible premises' (p. 90).

3 Assessing Friedman's Essay

The question of which philosophical outlook best accommodates the position of Friedman (1953b) is not, I believe, worth any more attention. The reason was clearly seen by Helm (1984, p. 121). Whereas others have taken particular remarks from Friedman and treated them as, or worked them into, full representations of his views, it is more appropriate to treat Friedman's ideas as 'simply muddled and confused'. As a piece of philosophical writing, Friedman's essay is incompetent and this can readily be seen without venturing into discussion of what the essay means.

There is the persistent use of 'valid' to mean 'true'—a real philosophical solecism. And this is a notable tendency to put certain words in quotation marks, obviously to suggest there is something problematic about them. So we have 'explain', 'right', 'frequently', 'confirmed', 'predictions', 'assumptions', 'realism' and 'realistic'. But despite persistently pointing to some problem—real or imagined—Friedman hardly seeks, and never succeeds, in giving an indication of what the problem is, and certainly not in describing how one might react to it. Anyone might make use of inverted commas as a quick way of getting a point across when there is an evident difficulty about a word. But in a philosophical work, where there are supposedly fundamental points being made about these things—'assumptions' most of all—care over clarification is required. Otherwise, it is perhaps a trick of a schoolchild, or an undergraduate failing to try to evade the necessity of precise formulation of an idea like that.

There is a notable failure to engage with any relevant literature. The introduction of John Neville Keynes (1891) may create something of an air of learning, but further consideration changes the picture.

A genuine, and competent, contribution to mythological thinking would surely have contained mention of Robbins (1932/1984) and Hutchison (1938). The omission of Robbins is particularly notable since, as Howson (2011) noted, Friedman discussed other matters with him in 1952, when he must have been writing the essay. Robbins and Hutchinson both consider the status of assumptions at some length, and that being one of the central concerns of Friedman's essay, he could have been expected to address their views. J. N. Keynes (1891, Chapter vii) thought the verisimilitude of assumptions an important matter, and that can hardly be ignored, since it was Friedman who praised it as an admirable book. A barely satisfactory essay would have had to have included a response to such a point.

There are numerous specific failings. His claim that the survival only of profit maximizing firms is 'evidence' for the profit maximization hypothesis is clearly mistaken in one way or another. As Simon (1963, p. 230) said, since the investigator has no way to know which are the profit maximizing firms, the claim cannot be tested. Perhaps Friedman did not mean 'evidence' in that sense, in which case his use of terminology is again in question, but anyway his argument seems to be circular. The assertion that there is much evidence on a point but that since it is scattered there is none to be presented is absurd, as is the idea that because a hypothesis keeps being adopted, that somehow shows it is correct. The idea of a theory gaining support because it shares a single assumption with another that has proven successful is, likewise, absurd.

Concerning the argument about the 'as if' approach, in his discussion of the matter of marginal productivity, where Friedman was so keen to dismiss the literature, he seems to have been ill-informed even on the facts of how the argument had been conducted. On the one hand, Machlup (1946, pp. 534–535) who supported the marginalist position and was one of those cited by Friedman—actually deployed the example of a 'theory of overtaking'. That was just like Friedman's idea of the billiard player. Machlup said it would describe a driver's actions in terms of the speeds and distances of vehicles and such like, and that no one would object that the theory could not be correct because the driver was a poor mathematician. So Friedman was wrong to say that both sides had concentrated on the accuracy of assumptions. Lester (1946) was the leading anti-marginalist and was also cited by Friedman. But Lester actually noted that his ideas made it possible to explain some data better than the marginalists could (pp. 75–76). More importantly, though, he was seeking to resolve issues raised by a great deal of empirical work—the sense of which is described in Forder (2013)—which appeared to contradict the predictions of marginalist theory. He was motivated to reconsider the assumptions of the marginalist model precisely by its failure to deliver correct predictions. He did not draw that out in the papers cited by Friedman, but those working in the field—unlike, one supposes, Friedman—would certainly have been aware of it.

Just as markedly, Friedman again showed himself all at sea over the philosophical aspects of his argument. Consider, for example the point he made about leaves on a tree. He said, 'I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives' (p. 19). He went on to say that various observations were consistent with this and that (depending on the purpose of the enquiry) the point that leaves do not in fact deliberate does no damage at all. All this was apparently to affirm the point that unrealistic assumptions are acceptable. The oddity is that Friedman's assumption ('hypothesis') was that leaves grow as if maximizing exposure to sunlight. In so far as that is in fact consistent with the data, it is not unrealistic. The unrealistic assumption he has in mind is presumably 'leaves deliberate over the matter and choose to grow where they will receive most sunlight'. That is unrealistic, but the prediction it embodies is confirmed by data, so one might well say the fact that it is an unrealistic assumption does not matter, because things work out as if it were true. The ideas that the realism of assumptions is irrelevant, and that it is perfectly proper to model on the 'as if' basis are therefore substitutes. Redundancy is not contradiction of course, but if it is philosophy we are doing, one expects the author to show more awareness of such matters than Friedman did. As for the point that the sole purpose of theory is to suggest testable hypotheses—a point made forcefully by Boland—a question might be why one would need theory. Testable hypotheses can, after all, simply be dreamt up. There is no need to write down a list of statements which have the entailment that growth in the money supply determines the rate of inflation before one tests that proposition. Again, if it is philosophy we are doing, these things matter.

Clearly, in the ordinary way of doing economics, it is often the list of assumptions that points to an explanation. But that takes us back to Machlup and Boulding—if it is explanation, and understanding, one wants, somewhere in the picture, their realism is going to be an issue.

Then there is the matter of a model being acceptable if its predictions are 'sufficiently good approximations for the purpose in hand' (p. 15), or whether it 'works "well enough"' (p. 18). One of Boland's moves, defending Friedman, was to emphasize that he sees theories having particular purposes and the crucial issue in assessing a theory is whether it is good enough for the purpose at hand. It sounds a bit of a bodger's charter, though there is an obvious aspect of common sense there too. But in a piece of philosophical writing, something more would need to be said about how one is to tell whether it is good enough. And this problem is acute when Friedman himself condemns the idea of assessing a theory by the realism of its assumptions just because it is not apparent what would be 'approximately true'. He apparently accepts the need for judgement in one place, whilst dismissing it out of hand in another.

Many more such weaknesses could be identified, but one central subset of them concerns various things Friedman said about assumptions. It is easy to agree that strict verisimilitude is hardly, if ever, important; and that there can be a variety of reasons that assumptions which are quite false might be appropriate. That, however, is not what he said when he asserted the strict irrelevance of the matter. Nor was he consistent. Even disregarding the suggestion that the inaccuracy of assumptions might be desirable (and therefore not irrelevant), there is also the point that assumptions are sometimes regarded as 'crucial' since they are intuitively plausible—the truth of assumptions is irrelevant but their plausibility might make them crucial? Really? Then there is the doubt about whether assumptions even exist. That is not something ever clarified and the question of what Friedman might have thought he meant is baffling. But perhaps most of all, doubts about their existence need to be considered in the light of what he called their 'entirely valid use' in 'specifying' when a theory holds.

As to Friedman's idea that the assumptions may serve a useful purpose in suggesting other hypotheses to test, this would seem to raise the question of why one should be interested in testing those particular other hypotheses. The ordinary view would be that it is because those hypotheses arise from a set of assumptions which it is supposed capture reality in some relevant way. It seems most unlikely that any reasonable fleshing out of that 'relevant way' will be consistent with the claim that the realism of assumptions is strictly irrelevant. But certainly one cannot make any progress with that sort of enquiry by trying to learn from Friedman's essay.

Perhaps all this should be seen together with just a couple of other remarks Friedman made directly on the subject of methodology. Frazer and Boland (1983) reported that Freidman had described his views not as being as Boland had said, but rather as aligned with Karl Popper's. They did their best to limit the damage to the view of Boland (1979) with a rather involved argument about the limits of Friedman's instrumentalism.[1] That statement by Friedman obviously needs to be contrasted with the one Boland (2010) reported him making. On that occasion, Friedman said that Boland's account of him as an instrumentalist was 'entirely correct'. Again, one has the impression that Friedman did not know what he was talking about. Then there is Friedman (2009)—a couple of hundred words posthumously published as the last contribution to Mäki (2009a). There Friedman could hardly have said a truer word when he commented that the ongoing debate was

> a severe condemnation of the essay. Surely, if the essay had been really lucid, scholars should not today still be having different opinions about what it says. (p. 355)

[1] It extended only to 'the shorter run, where policymakers reside' rather than a longer time frame 'where some ultimately true theory may reside' (p. 141). Apparently Popper allowed some room for instrumentalism there. It is an oddity of the following literature that when Boland (2016) reprised the debate over Friedman's supposed instrumentalism, he mentioned neither Friedman saying he was Popperian, nor Frazer and Boland (1983).

Well said. But any hope that Friedman might provide some elucidation was quickly squashed when he said that he had decided not to respond to critics of the paper as he wanted to get on with doing economics. 'I feel', he said, 'like a proud father who has a large brood of bright children—all of them right, all of them wrong, and all entitled to his or her own views'.

Some might read that as Conor Callaghan suggested to me, as being as arrogant as anything Friedman wrote—he was getting on with the real work while others squabble about the philosophical puzzles he had set them. All told it is probably better to treat it as a well-judged dive for cover. That view takes strength from the point that what he said recalled his letter to Ralph Harris about not responding to Hendry and Ericsson (1983). In the case of the methodology essay, the impression that he was hoping to conceal an inability to contribute to the debate is even stronger. After all, he would not have needed to respond to critics if all he was doing was giving a clearer statement of his views, or even of his view of 1953. It was his own diagnosis that the essay had not been clear, and that he could have tried to repair.

No doubt appeal will be made to the point that what is required is a sympathetic reading and that I am simply too hard on someone who was indeed not a philosopher, trying to explain himself. One difficulty is that, as already noted, authors have come to quite different conclusions about what the essay means. No doubt they were all trying to be sympathetic, and it led to no revelation. As Helm said, many positions can be supported by the right quotations, but each author adopting one of them is vulnerable to counter-quotation from other parts of the essay. It is not that specific slips can be identified and forgiven, which would make the case for charitable forgiveness; it is that the essay lacks sufficient coherence to know which parts might be slips, and which intended.

And again, no doubt, it will be true in the future as it certainly has been in the past, that clever advocates of one interpretation or another can find ways of giving sense to each part of what Friedman wrote, and with perhaps only modest compromises, even make the parts consistent with each other. The reason though for addressing and highlighting the weaknesses of the essay is not to say that clever advocates cannot produce clever advocacy, but to say that in this case, there is no point. Even if it proved possible to bring sense to the whole essay, someone else would have to bring it. In what Friedman said, there is none, and as a piece of philosophical writing, the essay is a failure.

4 Three Good Ideas and Their Origins

All this notwithstanding, an aspect of Friedman's essay which attracts attention to it, is the appearance that despite its failings it contains some excellent ideas. The positive–normative distinction is certainly important; as is the 'falsificationist' stance. Then there is the question of unrealistic assumptions in connection with which the idea of the 'as if' approach to model assessment clearly has merit, albeit more limited merit than Friedman suggests. The point that Friedman's essay contains these ideas is surely what resolves the apparent paradox of so many short comments seeming to praise Friedman, whilst longer ones were so critical. The short comments cite him only to report such an idea, and praising that, move on. Boland's feeling that 'textbooks' accepted Friedman's position whilst scholarship did not, may have a similar explanation.[2]

Some may feel that the fact that it contains those ideas is enough reason to read the essay despite its other limitations. One answer is that there ought to be better sources. Perhaps some may feel that just because many economists seem to have picked up these ideas from Friedman, that makes the essay important. That may indeed give it some historiographical interest, but it says nothing of the true intellectual value of the essay (cf. the discussion in Forder [2018a] of Lipsey's reading of Phillips [1958]).

In any case, though, one must be aware that interesting as these ideas are, there is no originality in any of them. The positive–normative distinction was very old. The idea that tests of a theory cannot actually confirm it, but only (potentially) falsify was certainly known

[2] It is hard to be sure since although he responded to the arguments of each of its critics, Boland (1979) did not say which textbooks he had in mind as approving Friedman's essay.

before Friedman's essay, having clearly been appreciated by Samuelson (1947/1965, pp. 4–5), without any apparent claim to having invented the idea, and as already noted, Duesenberry made the point in relation to his theory of consumption. Tinbergen (1951) even had 'refutable hypotheses' in the title of a paper. In the case of Friedman, a connection is often made between his ideas and those of Popper (1959) (or Popper [1934] in its German original). On the question of dating, Friedman and Friedman (1998a, p. 215) said that a first draft of Friedman (1953b) was written in 1946, and Friedman met Popper and discussed methodology with him in 1947, acknowledging that Popper's views were more advanced than his own. This seems not to be correct since, interestingly, Hammond (2009, p. 69) said that it appeared from his archival research that the first draft was written 'in late 1947 or early 1948'. Actually, he at least came close to the point in Friedman (1946, p. 618). But in any case, there cannot be any more doubt about the earlier formulation of Popper's views than there is about their greater sophistication since even the brief remarks in Popper (1945, p. 78) contain more of value than Friedman's argument.[3] There, Popper expressed the view that natural and social sciences were basically alike and pursued the method of deducing hypotheses, testing them and rejecting those that failed, so that the resultant hypotheses remained only 'tentative' but tended to be accepted when they survived a sufficiency of 'severe' tests. The point of tests being 'severe' was that a theory that correctly predicted an event which would have appeared highly improbable on the basis of other theories gained much more than predicting less surprising events. Friedman used the language of falsification and 'tentative' acceptance, but showed no sign of appreciating the issue about the severity of tests.

As to the making of unrealistic assumptions, economists had been doing that for a long time. Practically all would surely have accepted that there could be a role for assumptions that were not true to reality,

[3]It is a notable characteristic of many discussions of the 'Popperian' aspect of Friedman's outlook that Popper (1945) is scarcely cited, but rather the views therein are traced to their reprint in book form in Popper (1957). Since Friedman (1953b) was in between the two, that may threaten to create a misapprehension.

though not to the extent as to make the matter a strict irrelevance in all cases. That last point may be original to Friedman, if it is what he meant, but it is a hard one to defend, and he was inconsistent about it anyway.

On the other hand, there is the specific case of assumptions which are certainly false but where matters may work out as if they were true. The 'as if' approach this suggests is surely the idea for which the paper is most famous, and certainly the point that attracted the bulk of the short approving comments in the years after the paper was published, as well as a large share of the disapproving ones later. Indeed, the 'as if methodology' is often enough called 'Friedman's as if methodology', so associated have they become. But that too was quite an old story by 1953. Vaihinger (1911/1924) may be the origin of it, and the idea of 'als ob' theory was sometimes put in those words in English-language publications, such as by Wolfe (1936). It is not too hard to see that many authors had something like it in mind as justifying their assumptions, although when it was actually remarked on, it was with a warning that such an approach had to be treated carefully, as it was by Solomon (1947) reviewing, as it happens, Friedman and Kuznets (1945).

Friedman, though, surely learned it from Frank Knight. In Knight (1923a, p. 344), he said 'men do behave much "as if" they are trying to maximize something', and then in Knight (1923b, pp. 286–287), specifically defending economics against an attack on its common methodology, said,

> there is room for question as to how essential after all the psychological assumptions, or any psychological assumptions, really are for the substantial body of economic theory … It may be suggested that the truth of our assumed psychology is not vital as long as men in the mass behave "as if" they were actuated by motives of the character described; this would be analogous to the treatment of force in mechanics.

Friedman may perfectly well have learned that directly from Knight, but in any case the idea also appears in Knight (1922, p. 475) and Knight (1925), both of which were reprinted in Knight (1935), of which Friedman was one of the editors. He cannot possibly be thought to have had a claim to have originated the idea.

Dispensing then with the view that Friedman's paper is an intelligent piece of philosophy, and also with the idea that, despite its gigantic philosophical failings, it is nevertheless original in presenting these ideas, there seems to be not much left. Like an It Girl, the paper is famous for its celebrity, and its celebrity comes entirely from its fame.

5 Other Methodological Issues

Apart from Friedman (1953b) and occasional short remarks related to it, there are three or four other topics of broadly methodological content that featured in Friedman's work, and are perhaps worth considering alongside the methodology essay. In one case—his discussion of 'Marshallian' and 'Walrasian' approaches—it is perhaps possible to see that he introduced some confusion about his own work. The question of the use of the word 'cause' is not something on which he published much himself, but his few remarks, and perhaps his tendency to avoid using the word, are part of a pattern. Combined with one particular letter he wrote, they certainly warrant discussion. And his views on the distinction between positive and normative economics, which did feature briefly in Friedman (1953b), and the matter he seems to have treated as connected to it, of explaining disagreements between economists, is also interesting.

5.1 Marshallianism and Walrasianism

A point which Friedman evidently regarded as methodological and on which he sometimes placed some emphasis was the distinction between his own 'Marshallian' approach and the 'Walrasian' one he felt too influential in economics. The matter came up in Friedman (1949b) where he specifically rejected the idea of that distinction being between general and partial equilibrium, and drew it instead between the Marshallian view of theory as an 'engine for discovery' by the interpretation of facts and its being a more purely logical enterprise. 'The most reckless and treacherous of all theorists is he who professes to let facts and figures speak for themselves', said Marshall (1885), favourably quoted by Friedman. He said of the Walrasian influence, that it led to a view that,

> Abstractness, generality, and mathematical elegance have in some measure become ends in themselves, criteria by which to judge economic theory. Facts are to be described, not explained. Theory is to be tested by the accuracy of its 'assumptions' as photographic descriptions of reality, not by the correctness of the predictions that can be derived from it. (p. 490)

On the other hand, in the Marshallian view, economic theory provided organized methods of reasoning, and a body of substantive hypotheses based on factual evidence. It was because he saw this as being recognized, but not as persuading as many as it should that he asserted, 'We curtsy to Marshall, but we walk with Walras' (p. 489). In a similar vein, in Friedman (1955c)—a review of Walras (1874/1954), its first translation into English—he drew attention to the limitations of the approach and to the kind of mistake into which he said it led Walras, and concluding that to form substantive hypotheses, 'we must turn to other economists, notably, of course, to Alfred Marshall' (p. 908). Rather later, he mentioned it again in Friedman (1974f, pp. 145, 159) in responding to criticisms of his *Theoretical Framework for Monetary Analysis* (Friedman 1974a) by Tobin (1974) and Patinkin (1974). There, it was something of a dismissal—their comments were not making contact with his ideas because they were too much pursuing the objects of generality and abstractness. He deployed it again, in Friedman (1976a, p. 311)—something of a rematch from the 1974 encounter—again to explain, or explain away—his differences with Tobin.

The general sense of the distinction between seeking broad explanation of all aspects of a situation, and Friedman's approach of identifying narrower questions for analysis is in outline clear enough, and Hammond (1996) observed that it was appreciated in the 1940s before Friedman mentioned it. On the other hand, the extent to which the approaches discussed by Friedman are usefully characterized in terms of these labels is another matter. As an illustration of the difficulty, one might note that Hammond (1996, p. 147) also described Friedman (1974a) as putting his 'entire monetary project right in the middle of the neo-Walrasian paradigm' by expressing it in terms of the IS-LM model. Hammond observed that Friedman must have understood that this approach brought 'a tremendous amount of methodological baggage' (p. 147). Perhaps it did, but is that baggage really of abstractness, generality, and mathematical elegance? Would it not be better to regard it as rather a rough and ready model, seeking to capture in some simple relationships, the key components of reality? It was, after all supposed to be a representation of the Keynesian system, and Keynes' methodological approach was praised by Friedman (1974a, 1986a).

On the matter more specifically of Friedman's views on the question, one point is that for all the fuss made over it by later commentators, Friedman's discussions of the matter are rather few and not terribly substantial. Certainly Friedman (1941a) criticized Triffin by arguing the quality and insight of Marshall's analysis, but there was actually no mention of alternative methodologies as such. When he deployed it in Friedman (1974a), the matter is slightly reminiscent of his (1955b) response to Ulman (1955). There it was that Ulman made the matter too complicated; in responding to Tobin and Patinkin, it was that they were seeking too much generality. In both cases, Friedman asserts that it is he who pitches the matter at the right level, and on that basis discharged any obligation to respond closely to the points made. Although the matter must be one of judgement, and one could certainly agree with Friedman, the labelling of his approach as 'Marshallian' really does nothing to fortify it. Neither in Marshall nor elsewhere is one likely to find an account of the uniquely appropriate level of abstraction at which to conduct analysis. And since so much of the debate with Ulman was about Marshall's analysis, it is something of a surprise that he did not find a way to work it in there to Ulman's disadvantage as well—but he did not. More notable than that, is the non-appearance of the distinction in Friedman (1953b) itself (or, for that matter in Friedman (1946) or Friedman (1947c)). If it really is central to Friedman's methodological outlook, these things take some explaining.

There are further indications of this distinction not really being important in Friedman's thinking. When the matter came up in an interview published as Hammond (1992), it was because Hammond raised it, and when asked, Friedman first responded that he did not know 'how I first came to make the distinction or why I said it was important' (p. 11), and then when pressed said he thought he picked it up from Burns, but clearly had nothing to say about it. Then there is the point that amongst Friedman's few mentions of Walrasianism in any form, surely the most famous is that from Friedman (1968a) where the natural rate of unemployment was overtly defined as the solution of a system of Walrasian equations. It ought to be a puzzle that he adopted that terminology, without comment, to describe his own theorizing. All in all, it might well seem that Friedman actually saw little in the distinction, and its appearance in Friedman (1974f), after it had been so absent from Friedman's work would be naturally interpreted as making it simply a device to dismiss his opponents. There do not really seem to be grounds for thinking it much more.

There is then the question of his understanding of Marshall and Walras. On this, Friedman was rather taken to task by de Vroey (2009b) who doubted that Friedman's view of the undue influence of Walrasianism was at all well founded, that he knew much about Walras himself, or that his characterization of the difference between Walras and Marshall was sound. As de Vroey pointed out, 1954 being the date of the first translation of Walras into English, it is a good bet that few American economists had read him in 1949. Indeed, notwithstanding that Friedman (1955c, p. 906 n6) described himself as rereading it, de Vroey (2009a, p. 324 n24) also reported that he had elicited from Friedman the information that until asked to review Walras (1874/1954), he had not read the book himself. On the question of the differences in their approaches, de Vroey (2009a) described his own views in some detail, with the effect of calling various aspects of Friedman's rather limited account into question. Certainly though, as de Vroey noted, there cannot be doubt that Friedman's association of Walrasianism and the idea that assumptions should be photographic descriptions of reality is very peculiar indeed. Just what is notable, one would have thought, is the crashing unrealism of the 'Walrasian system', and Friedman's association of the two seems very strange, and clearly shows he cannot be treated as giving a serious account of Walras.

Friedman's picture was further criticized by de Vroey in relation to the claim that economics generally was overly Walrasian—'we curtsy to Marshall, but walk with Walras'. As de Vroey said, Friedman's remark came too early for it to have anything to do with Arrow and Debreu (1954) or Debreu (1959). But a possibility must be that Friedman's remark came from what he picked up in reviewing Triffin (1940), who was a Belgian, and surely had read Walras. His idea that monopolistic competition brought realism to the theory of the firm, even as he sought to follow Walras' inspiration in analysing general equilibrium, may provide the link that confused Friedman. Then for Friedman the abstractness and generality under fire was that exemplified by Lerner and Lange—Friedman's reviews of which predated the discussion de Vroey criticized. The idea of 'photographic' modelling also naturally takes its life from Friedman's view of Triffin's book. He was in a position to criticize that kind of theorizing, and as we know, he had no sympathy at all with it, even though, surely he had no real idea of Walrasianism at the time.

5.2 Causation

Another case arises over the word 'cause', over which Friedman sometimes expressed some particular views. In Robbins (1974, p. 101) he declared it to be 'a tricky word' which he liked to avoid, but there is a much longer and much more revealing treatment of it in an interview with Hammond (1992). As Hammond explained, the discussion arose from Friedman (1985b) writing to Hammond to comment on a pre-publication version of Hammond (1986), and saying he had been,

> stuffed with straw and attacked. I have little quarrel with your substantive conclusions; I have a considerable quarrel with the rhetoric

> I have always regarded 'cause' as a very tricky concept. In my technical scientific writings I have to the best of my ability tried to avoid using the word.

Indeed, as Hoover (2009, pp. 306–308) showed, whilst also noting that they are much less reticent about 'effect', in Friedman and Schwartz (1963a) the word does seem to be avoided, with various circumlocutions used instead. That would also be true of other works.

However, in the interview, Friedman said that the problem was that causal statements lead to an infinite regress, in that one can always ask what caused the cause, and indicated that this was in his mind somehow comparable to the problem of distinguishing fact from theory. What that similarity might be never became clear and after about five pages of further discussion, Friedman declared the matter about cause to be a semantic one and that he had no interest in such matters. Hammond very politely pressed him twice and was firmly told the matter of whether to use the word 'cause' was a 'semantic choice' (he meant 'lexical' of course) and had no further significance. Friedman's comments were probably not consistent even in those five pages, but that last claim makes a nonsense of his original objection to Hammond's paper.

Even on the basis of his 'semantic' choice, Friedman's commitment to his position seems to have been very thin, since in Friedman (1965a), his Foreword to Cagan (1965), he made a different choice, saying,

> 'Probably the most important issue to which the monograph contributes is the long-standing dispute about the causal relation between money and prices' (p. xxiv), and 'Originally, we did not expect the examination of the supply of money to provide evidence on such general issues as the causal relation between money and prices … But research lead a life of its own…' (p. xxvii)

In any case, Hammond (1996) then examined the matter of causation in Friedman's writings on money in some detail. But whilst he brought much to the understanding of Friedman's economics and the arguments about it, he does not seem to make anything out of any interesting ideas Friedman might have had about the notion of causation. As to his avoidance of the word, it is just another juvenile error to suppose that periphrasis solves philosophical problems, and even if it is just a matter of infinite regress, that remains after the words are changed. Here, then, despite the tone of his initial response to Hammond, it seems very much as if Friedman has picked up the notion that there is some philosophical problem one should acknowledge, but given every chance to elaborate, he had no idea what moves to make.

5.3 Positive and Normative Economics, and the Explanation of Disagreement

Further issues arises from Friedman's various discussions of the question of confusion between 'positive' and 'normative' economics, and what seem to be his sometimes-associated discussions of the explanation of disagreement between economists. Here again, Friedman seems unable even to sort out his own ideas in a consistent way, although the picture is not an easy one to digest.

His best-known discussion of both issues is probably that of Friedman (1953b), which has already been mentioned. Apart from the oddity of choosing J. N. Keynes as his vehicle for making the positive-normative distinction, rather than the more natural Hume (1739) or Robbins (1932/1984), there is the point that the ostensible reason for including the discussion was that confusion was commonplace. But Friedman gave no examples, nor even an indication of how such confusion would be manifested. Rather, he went on to doubt that there were many important normative disagreements in economics. If anything, that might suggest that any conceptual confusion that did exist would be harmless, but Friedman used it as the basis for saying that 'progress of normative economics' had little to offer policy formation and hence that achieving policy agreement would be facilitated by distinguishing 'sharply' between the two kinds (pp. 6–7).

As Hammond (2009) noted that discussion is peripheral to the essay as a whole, and he also discovered that it had not been part of the first draft, but had apparently been added in a draft towards the end of 1952. That is interesting because there are three other discussions by Friedman of matters he seems to have thought related, from about the same time. First there was Friedman (1951g)—a contribution to a debate about monetary policy, but mainly a comment on Harris (1951a); then Friedman (1952d) which was a comment of just two pages on a discussion by Ruggles (1952), and then Friedman (1953g) commented on Calkins (1953). Of these, the substance and much of the wording of Friedman (1952d) was simply incorporated into Friedman (1953b) and it is therefore the source of the additional remarks Hammond found added.

Harris' piece was an introduction to a symposium on monetary policy and it had an aspect of scene-setting, but was also pusillanimous about what policy might be followed, notably saying both that the Federal Reserve had the power to stop inflation and that it was unlikely to use it because of various likely negative consequences such as, in particular, very high interest rates. He thereby exposed himself to a logical dismembering by Friedman, and that duly occurred. To take one point, Friedman said that Harris was arguing that there was a discontinuity in the effect of rising interest rates—they could be too low to stop inflation, or too high to permit economic prosperity. On the contrary, said Friedman, if interest rates needed to be very high to stop inflation, that showed that at lower levels, they could not be doing the damage Harris feared.

Before setting about that sort of matter, though, Friedman criticized Harris' stance, saying that he moved from what he thought the Federal Reserve 'likely' to do, to what it 'should' do. Friedman did not use the words, but there is an aspect of accusing Harris of adopting a normative view according to his positive conclusion as to what was likely. Expressing his own view, Friedman said,

> The role of the economist in discussions of public policy seems to me to be to prescribe what should be done in the light of what can be done, politics aside, not to predict what is "politically feasible" and then to recommend it. (p. 187)

That is rather odd because on the face of it, he is demanding that a normative position be taken. Perhaps he would have said that in the area under discussion, there were no normative disputes, so that it was safe to draw policy conclusions. It is not clear that is right since in his concluding remarks, Harris (1951b, p. 199) briefly put his point differently, to the effect that raising interest rates had distributive consequences, with the implication that they were a proper concern of policy, and accused Friedman of dismissing those concerns. But in any case, the position in Friedman (1953b) was that normative disagreement was unusual, and that had not stopped him calling for a 'sharp' distinction. So the absence of normative disagreement would presumably not be a reason to proceed to normative conclusions.

Ruggles and Calkins both described approaches to economic research, and both described there being an aspect of assessment of results which involved value judgements. For Ruggles, there were four steps in research and the last was this evaluation of conclusions which required 'an aggregation of value judgments derived from some social or individual system of ethics and tastes' (p. 411). He was perfectly clear that general ethical considerations do enter into the evaluation of conclusions. But that equally clearly meets Friedman's point, since it is an evaluation of conclusions otherwise reached that is being discussed. Calkins similarly saw the knowledge and tools of the economist used analytically, and then policy advice in government needed 'a proposed course of action or, better, several courses of action, and for each an indication of how it is expected to work and what its consequences and costs are expected to be' (p. 433). Both authors therefore saw normative decisions as needing to be taken, but neither showed any sign of being confused about the matter. Still, Friedman took exception to both and denounced what he thought was their confusion and in Friedman (1953g) said again that there was a need for 'a sharp separation' between positive and normative economics, but also said, 'I do not deny that both are appropriate fields of study and can be part of the science of economics' (p. 448).

Taking the three together, it seems fairly clear that Friedman is not altogether in control of the argument. A plea for sympathy would arise in the case of the latter two since Ruggles did not offer much for Friedman to get his teeth into, and Calkins's piece was yet more amorphous. Friedman would not be the last to 'comment' on a paper by riding a none-too-relevant hobby horse of his own. Still, in all three of these cases, there is some sense of Friedman demanding a separation of aspects of thinking, but he does not seem to be clear what objective he is pursuing. One wonders whether it was encountering Keynes at around this time that gave Friedman the impression that he had a clear idea, and one important enough to add to the methodology essay.

A later discussion of issues that seem to be at least partly related occupies the first few pages of Friedman (1968b), much of which was lifted into Friedman and Friedman (1998a, pp. 216–219). The issue was that of why economists disagree. In Friedman (1953b) the suggestion was that it was because of differences of scientific view but more ideas were presented in 1968. One was that economists adjusted views according to their perception of political feasibility. That is one view of what he had objected to Harris doing and in responding to Oliver (1953), Friedman (1953e) elaborated slightly on that, noting that Keynes (J. M. Keynes, this time) had recommended a tariff because he thought it impossible for Britain to abandon the Gold Standard. That example was cited again in Friedman (1968b) and he clearly thought that decisive in making the case for recommending the best policy irrespective of the chance of its being politically acceptable, although he was not quite consistent on that himself.[4] A second idea was that economists would sometimes keep quiet about the consequences of policies for fear of appearing hard-hearted—their failure to oppose the minimum wage was suggested. A third example was that of economists declining to advocate floating exchange rates because of official resistance. Notwithstanding all this, in due course he moved to say that he nevertheless believed most differences between American economists arose from different scientific views rather than normative judgements but that there were two qualifications.

One was that 'any scientific judgment' (p. 6) involved uncertainty and different scientists will resolve the resulting questions according to their 'basic values', so that in Friedman's case, he said himself, since he was a believer in freedom, he would resolve doubts about the precise effects of any proposal 'in favour of policies relying on the market' (p. 7). Here he seems to convict himself of the error he alleged others to make when he was criticizing Ruggles and Calkins. But this time, he did not seem to have recognized the problem.

[4]Friedman (1989c) recalled that in thinking about exchange rate policy during his 1952 visit to Paris, he had ruled out a single European currency 'on political grounds'.

Then he said, 'A second way that basic values enter into policy choice is through differences in time perspective', with some people being long-termist, and others short-termist. That would certainly explain differences in policy preference, but Friedman went on to say that liberals such as himself tended to be long-termist, whereas interventionists were short-termist. This was because, on the one hand, the market works slowly whereas central control can operate quickly, on the other hand, interventionists would 'be disposed to have a shorter time preference' (p. 8). That sounds as if it makes one time preference a matter of personal choice. What Friedman said, though, was first that such a person believes that since central government can achieve things rapidly, if the long-term consequences are adverse, they can be addressed with more government action. Secondly, he said that because of electoral demands, 'he will have a short time perspective because the political process demands it', and Friedman contrasted that with the business entrepreneur who 'can afford to wait' (p. 8).

Clearly his factual claims may be doubted. The idea that business entrepreneurs tend to be in a position, or have the inclination, to await returns might well be questioned, though perhaps that was less apparent in 1968 than later. But the proposition that it is a general characteristic of free marketeers to be long-termist and interventionists to be short-termist seems ill-founded. It is hard to see that those who argue for greater investment in public infrastructure, or education, or to take a case that was just beginning to be argued when Friedman was writing, those calling for greater environmental protection, were led to their proposals by their short-termism, whilst their non-interventionist opponents took a long view. And as to his last point, it was merely that those holding elected office, whatever their political views, need quick results, and so nothing to do with the point he was trying to make.

More fundamentally, though, the question would be what room Friedman's qualifications leave for his original hypothesis. He seems to have meant that the presumption that disagreements are over positive rather than normative matters applies only where there is no scientific uncertainty. In economics, therefore, that would not be very often. And amongst matters that pass that bar, we still have to except those where differences in time preference might be relevant. It seems unlikely to leave much.

Friedman (1968b) was a piece of popular writing, but the muddle it exhibits does not seem to be due to any simplification because of that. Surely what we see here is Friedman's ideology forming his positive views. He has convinced himself that interventionists are short-termist and is propagandizing against them, confident that their short-termism should be seen as another of their failings. He has thereby fallen precisely into the trap which, without much merit, he had claimed ensnared others. And to add to that, he has apparently even convicted himself, as a result of his liberalism, of mixing the normative and the positive, but thinks nothing of it.

So none of these discussions is really satisfactory and Friedman appears to be in a tangle about the whole area. It could be said that his views on the positive–normative distinction, or on whether politics should constrain policy recommendation, or for that matter, the merits or ethics of concealing one's hard-heartedness, are pretty harmless. Indeed, they are unimportant remarks without much discernible connection to Friedman's economics. But a different point is this—there are a collection of loosely-speaking methodological or philosophical issues on each of which Friedman seems quite lost. In each case too, part of the pattern seems to be that he is convinced that he had profound insights that needed sharing. But in each case, including one where he took himself to an interview because he was so upset about how he had been misrepresented, he was simply unable to hold up his end of the discussion.

6 Friedman's Essay, and Friedman's Methodology, 1935–1957

Just because Friedman had no facility with the argument, it need not follow that at the most general level, one cannot see the intellectual motivation of the methodology essay. That is the point that motivates treating the essay by means of a 'soft reading', as Mayer (1993) put it, winning approval from Laidler (2017). That, I think, goes rather beyond a 'sympathetic reading', which would seek to correct mistakes to find a good essay hidden by them, and ought rather to be one which accepts that it is a poor essay from which one can gather only a very vague idea of what the author had in mind. Mayer's soft reading was that Friedman wanted to emphasize the importance of testing theory. He was strangely at pains to present the failings of the essay as intentional accommodation of less philosophically aware readers, although the sorts of errors and inconsistencies in the paper are no way of doing that. But nevertheless, one can see that the importance of testing was a key aspect of the essay.

Much more important than that, it is quite easy to see significant elements of methodological purposefulness in Friedman's economics—there is a consistent methodological practice there, and he clearly had a good idea of what he was doing. As Hammond (1996) said, at that time, relatively few had sought to assess Friedman's methodology in the light of his practice—noting Hirsch and de Marchi (1990), Hoover (1984), and Hoover (1988) as exceptions; and later, Hoover (2009) was very much of that kind, as was Mariyani-Squire (2018). Indeed, seeking to understand a single essay on methodology written by someone who wrote so much economics without seeing the two together seems very peculiar. What might be doubted, though, is not whether the methodology essay should be read alone, but whether it should be read at all. Friedman's methodology can indeed be learned by looking to Friedman's economics. But the idea that one then needs to, or there is any point in trying to interpret the methodology essay is quite a different claim.

In seeking to describe Friedman's practice, one might consider the character of 'positive economics' as exhibited in Friedman's work. Here, the beginning of the methodology essay may be actively misleading. The title of the collection was *Essays in Positive Economics*, and the lead essay was *The Methodology of Positive Economics*. Nor was it merely the lead essay, and the only one newly written for the book. It is a little noted point, but it appears on the contents page as the 'Introduction'. It really seems that it is intended to describe the approach to economics adopted in the book.

The discussion of Keynes at the beginning of the essay is suggestive that the subject matter is 'positive' rather than 'normative' economics. If that is correct, though, it does not at all provide an introduction to the book since it is full of normative argument. Oliver (1954) questioned the appropriateness of the book's title on that basis, and Hutchison (1954) made a similar point. There could hardly be, for example a 'case for flexible exchange rates' in a work that eschewed the normative. On the other hand, we know that this part was not part of the original draft of the essay. It seems a good speculation that some combination of Friedman's muddled ideas developed in writing about Harris, Ruggles, and Calkins, perhaps along with a sense of mischief offered by the opportunity to quote a Keynes who was not the usual one, led to its inclusion.

On the other hand, the title of the book may well have been decided before the discussion of positive and normative matters was added—it seems unlikely that the late addition of a few paragraphs to the introduce was allowed to determine what the book was to be called. It seems very possible, then, that 'positive economics' is not to be contrasted with 'normative economics' at all, but rather that it is a book of essays written according to the dictates, or anyway, perhaps in the spirit of 'positivism'. That is a problematic term, with a variety of uses and nuances of the central ones. But it is broadly speaking the doctrine that knowledge comes from investigation of the world, rather than being innate, or intuitive, or God-given. It proposes observation and experimentation, and rejects *a priorism*. There are many and various shadings in what it is taken to mean, and that made it possible for Caldwell (2001, p. 142)—not really intending any particular judgement on Friedman—to say 'everyone from socialist planners to Milton Friedman' adopted the label. There is certainly no need to suggest that Friedman ever formed a detailed idea of what it is to be a positivist, but the idea that he picked up the vocabulary as indicating a concern with acquiring knowledge from the data does not require that.

That understanding of the word fits the content of the book much better. This is perhaps particularly so in relation to the two critical reviews—those of Lange and Lerner. The fault that Friedman found was their failure to produce testable hypotheses, or practical guidelines for action, not that they engaged in normative disputation. Of the other essays, there is rather little aimed squarely at the matter of testing, but most of them provide analysis based on observation, and proposals which are actionable. Where this is not true, such as in Friedman (1949b) on the Marshallian demand curve, the argument was nevertheless about the question of which theoretical picture would give rise to useful analysis. In Friedman's other work from the same sort of period, the point comes through just as clearly—or more so, perhaps. The argument in Friedman and Savage (1948) where indeed the 'as if' view was specifically deployed is certainly intended to provide testable hypotheses, and a good deal of data was considered. In Friedman (1951a), on the effect of trade unions, Friedman's emphasis was on the empirical, and that came right to the fore in Friedman (1955b) when he criticized Ulman for not seeing the importance of empirical work. Most importantly, it is apparent throughout Friedman (1957a), and most powerfully apparent too. There, the theoretical discussion set up the theory precisely to make it 'susceptible of contradiction by a wide range of phenomena capable of being observed' (p. 27).

On the question of the realism of assumptions, it is plain that, despite what he wrote in the methodology essay, he was in general very much concerned with it. That point has been noted, for example, by Mäki (1986) pointing at the way Friedman (1977e) criticized Galbraith for making unrealistic assumptions, or by Caldwell (1980) over his views on the development of the theory of the Phillips curve. Hoover (2009) argued the closely related point that throughout Friedman and Schwartz (1963a) the authors are concerned with establishing causality (notwithstanding their avoidance of the word). He too concluded, quite rightly, that Friedman certainly cared about the realism of assumptions. But examples are clear and central in his earlier work as well. In Friedman (1957a, p. 26), when he introduced the crucial assumption that transitory income and consumption are uncorrelated, he said,

> The ultimate test of its acceptability is of course whether such phenomena are in fact observed and most of what follows is devoted to this question. It is hardly worth proceeding to such more refined tests, however, unless the assumption can pass—or at least not fail miserably—the much cruder test of consistency with casual observation of one's self and one's neighbors, so some comments on the intuitive plausibility of the assumption are not out of order. (pp. 27–28)

and then,

> The purpose of these remarks is not to demonstrate that a zero correlation is the only plausible assumption… Its (sic) purpose is rather to show that common observation does not render it absurd to suppose that a hypothesis embodying a zero correlation can yield a fairly close approximation to observed consumer behavior. (p. 29)

So he was not even going to move to testing theory until he had established the plausibility of the assumption. So much for the irrelevance of that consideration. Similarly, in Friedman and Savage (1952), it was 'the plausibility of a set of postulates' that was cited as something giving rise to the 'very real appeal' of the theory. In such matters—quite rightly, of course—Friedman gave no room at all to the thought that he might say things are 'as if' transitory components are uncorrelated, or anything of the kind.

Then another point seeming often to escape attention concerns the application of the 'as if' approach. In Friedman's economics, it is deployed when the matter at hand relates to something in the character of an agent's decision-taking. The billiard player is paradigmatic, and the leaves on the tree are close enough. The managers of firms who have no actual knowledge of their marginal cost function offer an application in theory. The only place in Friedman (1957a) where the expression was used is the one already quoted above (p. 137) where it was said that consumers were treated as if they regarded their income and consumption as consisting of permanent and transitory components; in Friedman and Savage (1948) it arose in connection with the authors' response to the issue of how consumers determined their maximizing action on a utility function. It is then only in relation to cases of this kind that the realism of assumptions is ever actually treated as an irrelevance. Again, the methodological approach exhibited in Friedman's work seems consistent and reasonable. One is invited to see that the economic theorist is not concerned with such things as the ontology of 'utility'. *That* is an analytic device, a way of interpreting, the medium of the 'as if' methodology. But there is no question of arguing that the world is such that it is all 'as if' the quantity of money determines the price level—that really is what happens. So there is no sign at all from his economics that

Friedman thought realism of assumptions generally irrelevant, and certainly not that there is some sense in which more significant theories have less realistic assumptions, despite the claims of the methodology essay.

On the other hand, it is not easy to see Friedman, as he sometimes seems to want to be seen, or as some of the commentators suggest he should be, as under any important influence of Karl Popper. A clear reason for not accepting that is that Friedman offers almost nothing that could be a severe test. He did make a suggestion on those lines in Friedman and Savage (1948), and as I suggested the Allais paradox may have provided that test. But in Friedman (1957a), there is nothing of the kind. There are *differentiating* tests—or there are arguments that there are, at least—that serve to show the superiority of the Permanent Income Hypothesis over other specific theories. But that is not the same thing. There is nothing in the spirit of a Popperian severe test—a test the hypothesis is expected to fail. There hardly could be because Friedman's modus operandi—as well as his brilliance—is very much to devise ways of seeing the data as fitting the theory. It might be said that the theory is in principle capable of refutation, but the fact is that with Friedman's skills in construing the data, that will not happen. And if Friedman had meant it to be possible, he would have lain out criteria of failure, rather than taking each piece of data as it comes, and explaining how it fits. So Friedman's work, his exhibited methodology, was empirical, and sincerely so, but it is not Popperian. It confronted theory with data, and it did so energetically. But the confrontation was to allow the data to be explained, and came nowhere near being designed on Popperian lines.

That Friedman had a coherent approach to his work, and even that he was in certain respects innovative in his insight about it therefore need not be doubted. But it does not follow that any worthwhile account of those ideas can be gleaned from Friedman (1953b). That essay is intellectually impoverished. If it is methodology we are interested in, then there is much more to be learned from studying Friedman's actual approach to his economics. That being done, though, there is nothing more to learn about methodology from the essay. All we can learn from there is that Friedman had no skill, or competence, really, in that kind of argument. So Friedman's *Methodology* is nonsense, but the point that should seem much more important is that the methodology of Friedman is something else entirely. Though it is limited, it was well-motivated, and it was most consistently applied.

7 Conclusion on the Methodology of Positive Economics

It is a strange beast, then, the methodology essay. As Hammond (1996, p. 29) quite rightly said, the attention given to it is 'inordinate'. The reason is that as Dennis (1986, p. 637), admirably unperturbed by the fuss about it said, it is 'a dog's breakfast'. Surely in part it is so venerated because it contains some crucial ideas which have become associated with Friedman's name so as to make them seem original to him. But the 'It Girl' metaphor is quite appropriate—it receives so much attention because it is highly cited. Yet for all that is said about it, it is not even about 'positive' as distinct from 'normative' economics, and it is hard to see that Friedman was well-equipped to contribute to such a discussion anyway. It does contain some important ideas, including the importance of testing and falsification; at least the possibility of wholly unrealistic assumptions being entirely appropriate, enlivened through the billiard player and the leaf examples; and the more general idea of the 'as if' methodology, and its invitation to disregard what economic agents say or think they are doing. They are not just important, but colourful, and in some aspects, profound. They are not in any way Friedman's ideas, and the rest of the essay—the argumentation that should join these ideas into a piece of philosophy, is a failure. I suppose most economists pay no attention to that point—they either never read the essay, or they read it blinkered by its reputation. It seems memorable, and profound, because only those colourful ideas are remembered, and they are remembered by people who have probably never considered the possibility that those ideas are not original to Friedman.

11
Part II Conclusion

Although almost none of his work on money has yet been considered, it is clear that if Friedman had written no more, he would have been an important economist, and an economist who wrote on a wide variety of issues. Certain other characteristics of this body of work are worthy of note. One is that although it imaginative, his work was not markedly anti-Keynesian, nor for that matter terribly much pro-laissez faire. *A Theory of the Consumption Function* is misjudged as being principally an attack on Keynes, and all his work on stabilization policy accepts at least the possibility of useful policy, even if it would be best for it to be rule-governed; and none of it gives the impression of being intended principally as a criticism of Keynesian economics specifically. Starting with Friedman (1948a) he sought to make policy automatic, but many a Keynesian would have no objection to that in principle if the right automatic rules could be devised.

A Theory of the Consumption Function is certainly the most important work of the period. That is not because it overturned a key component of the Keynesian theory. It is because it pointed the way to an empirically acceptable account of consumption based on more-or-less rational behaviour. That was not even mainly pointing a road away

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_11

from Keynes—it was pointing away from Duesenberry. The crucial Keynesian concept was the consumption *function*, and that survived Friedman's treatment. The book was important in another way, of course, because it so fully displayed Friedman's brilliance. That point is nothing much to do with whether faults can be found in the work—certainly they can. It is to do with the insight and the skill, and the peerless way he put the data into an order described by the theory.

And the methodology essay, although horrible and horribly overrated nevertheless does seem to flow from a practice adopted by Friedman which was aimed squarely at theorizing the data; and understanding what was going on by devising theory which explained the observations. If we throw away the methodology essay and look at the economics he wrote, from a methodological point of view, a consistent picture emerges. Some of his arguments are a little optimistic in terms of how much reliance he feels can be put on the theory, but that is not to question the objective he was pursuing. Viewed simply as trying to devise theory which data should be shown to fit, and not trying to make the description of methodology more sophisticated than that, it can be seen that he was doing that, and that there was a very notable methodological consistency in his work.

Part III

Friedman on Money

12

Part III Introduction

Starting with Friedman (1956a), and obviously excepting Friedman (1957a) and the work following up on it, almost all the later economics Friedman wrote was about money. It is striking because the variety of the earlier period disappears and there is a great concentration in just one area. Friedman and Schwartz (1963a) is quite rightly seen as the centrepiece. Nearly everything he wrote on money up to the early 1970s needs to be seen in relation to it—some things are preliminary, some derivative, and some further exploring its themes, but it is the hub of his work. Intellectually speaking it is perhaps most closely related to Friedman and Schwartz (1963b) and Friedman and Schwartz (1970). The former was an exploration of the relationship between money and business cycles, the latter another book which was being written at the same time as Friedman and Schwartz (1963a), and which although published later, is in fact foundational. I consider the two books first. Works related to them, all of which are also in one way or another explorations of the Quantity Theory of Money, but falling into—as I see it—six strands of thought, are then considered in Chapter 14. Chapter 15, moving into the 1970s, brings a different set of issues as Friedman turns to the question of how to explain why it is that inflation

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_12

occurs despite—so he says—being so easy to control, and from there, to Chapter 16 and the Phillips curve. His discussions of that are seen by many as one of his great achievements, but I have a different view, and in any case, a little-noted fact about them is how divorced they are from his work of the 1960s. One can see the ambitions and lines of thinking that took him there, but when one is looking for the patterns in his work, the fissures in it also come to light—as they certainly do here. Finally, in Chapter 17 I consider Friedman and Schwartz (1982)—a long delayed book which in its delay and reception very much provide a warning, as well as a disappointing end—or near-end—to Friedman's research career.

13

The *Monetary History* and *Monetary Statistics*

Though it became much the most noted, Friedman and Schwartz (1963a) was intended by its authors as only one of a series of volumes. Indeed, as they explain, it began as a single chapter of 'analytical narrative' (p. xxi) in a statistical study, to be published as *Trends and Cycles in the Stock of Money in the United States, 1867–1960*—to be accompanied by Cagan (1965), which was a book concerned mainly with the determination of the money supply, but also pointing to a degree of mutual causation between it and economic activity. The Friedman–Schwartz project grew and grew, until in the end it was too large to be completed. The next volume they produced was not *Trends and Cycles*, but Friedman and Schwartz (1970)—*Monetary Statistics of the United States*—in which two further projected volumes were described. One was to be on 'monetary trends' and another on 'monetary cycles'. In the event, the long-delayed one on monetary trends, covering both the United States and the United Kingdom, appeared as Friedman and Schwartz (1982), but no book on monetary cycles ever appeared, with Friedman and Schwartz (1963b) addressing that aspect of the problem.

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_13

1 Monetary Statistics

The volume on monetary statistics turned out to be much less widely noted than the monetary history, and indeed after a fairly short time, hardly noted at all. It is, nevertheless, an extraordinary work. The primary objective was to present the authors' estimates of the quantity of money that were used in Friedman and Schwartz (1963a). After an introduction which did that, there were three parts—a long essay on the question of the definition of money, a discussion of previous estimates of its quantity, and a detailed description of how Friedman and Schwartz arrived at their own figures. All three are impressive pieces of scholarship, and although the second and third—that one alone of nearly 300 pages—are anything but exciting reading, that serves to underline the determination with which the authors pursued their objective. It is difficult to see how the standing of this work as historical scholarship of a very high order could be doubted.

The first of these sections—nearly 200 pages on the definition of money—has somewhat broader appeal and is much the authors' best discussion of the much-deprecated and indeed rather mysterious idea that the definition of money is an empirical matter. Their aim was to explain the approach leading to their preferred definition of money, given at the start of the book—'the sum of currency outside banks plus all deposits of commercial banks—demand and time—adjusted to exclude interbank deposits, US government deposits, and items in the process of collection' (p. 2). They considered many previous definitions and some recent 'a priori' approaches that sought first to determine the essential characteristics of money and then to identify the assets having those characteristics. Friedman and Schwartz found specific fault with the specific measures proposed, but really their position was that an 'a priori' approach was the wrong one and they did not so much argue, as decide that the better approach was to investigate the relationship between various possible measures and other economic variables. They justified this in terms which, even more than Friedman (1955a) seem to be straight from the operationalism of Bridgman (1927, e.g. p. 9):

> "Money" is that to which we choose to assign a number by specified operations; it is not something in existence to be discovered, like the American continent; it is a tentative scientific construct to be invented, like "length" or "temperature" or "force" in physics.
>
> Friedman and Schwartz (1970, p. 137)

That, really, was their position when they started, and not something concluded from the discussion of others' approaches, interesting as that discussion was. They considered various statistical matters, and some theoretical ones along the lines that assets which are near perfect substitutes for each other should either all be 'money' or none should, but what their approach amounted to was considering a range of possible measures of 'money' and determining which one had the most stable, simple demand function.

To some, that has had the appearance of prejudicing the research. Melitz and Martin (1971) said that Friedman and Schwartz were implying that those who preferred a conceptual definition were unscientific, but noted that whereas hypotheses should be testable, there was no corresponding presumption about definitions. Though they noted that Friedman and Schwartz advanced conceptual arguments for their definition as well as empirical ones, they argued that there was no reason any empirical result would necessarily make it irrational to stick with a conceptually sound definition. Mason (1976) was rather harder on them, saying their reasoning was 'casuistic' (p. 531) and that 'An empirical definition of money designed to validate a monetary hypothesis precludes empirical invalidation' (p. 532) and was therefore the antithesis of scientific procedure. For Friedman and Schwartz, the empirical counterpart of the concept was the one 'most useful in making predictions about observable phenomena on the basis of the theory one accepts' (p. 1). For Mason that meant the theory had been accepted and so was not being tested, and the argument was circular—money was by definition that asset which had a stable demand function, although the purported test of the theory was to investigate the stability of demand for that asset.

Aspects of Friedman and Schwartz' position are badly put, and indeed the idea of an 'empirical definition' is itself paradoxical, if not oxymoronic. But the difficulty they face is that there is a range of assets which are more or less like currency. Some of their critics probably had not recalled that Keynes (1936, p. 167 n1) took the same sort of view—'we can draw the line between "money" and "debts" at whatever point is the most convenient for handling a particular problem'. Their *theory* was that the behaviour of the quantity of 'money' had predictive power. If they had announced a whole range of hypotheses, each to the effect that the quantity of money according to some measure or other had a stable demand function, and proceeded to reject all but one of them, they would presumably not be called unscientific. Instead, they approached the problem by first discovering which concept of money led to the best results on that question. Certainly, it was a presumption of the authors that there is some definition of money which will render a stable demand function, and the way they conduct that discussion reveals that. But nothing they did in principle precluded the possibility of finding there was no definition meeting the criterion. What they did not have—surely quite properly—was a strong preconception about precisely which asset or combination of assets would be the one with the most stable demand. So they set about finding out. There is nothing foolish about that.

On the other hand, the Friedman–Schwartz approach does mean there is, as I said of the consumption function, an absence of a crucial test. The list of candidates for being 'money' is limited to the various measured aggregates. Thoughts such as that attention should be given to 'trade credit' of the kind found to be important in monetary policy by Brechling and Lipsey (1963) were not represented. That issue might be addressed by means of an assessment of how useful the most useful definition of money turned out to be, but that would be another step in the argument. The first step was for Friedman and Schwartz to determine the best version of their theory. That was the step they took. Certainly it left other questions open, but in itself, it was entirely reasonable.

2 Monetary History

A Monetary History itself, is of course a much more noted book. According to the authors' striking first sentence, 'This book is about the stock of money in the United States'. It is very much about that, although it is much broader than that simple statement suggests with a large collection of discussions of significant events, such as for example, the discussion of the political consequences of falling prices after the Civil War (pp. 44–50); the argument over remonetization of silver up to about 1900 (pp. 113–119); the central banking activities of the Treasury before 1913 (pp. 149–152); a discussion of the banking panic of 1907 (pp. 156–168); the personalities as well as the policies in the early years of the Federal Reserve (pp. 224–231); the debate about how the Federal Reserve might control speculation (pp. 254–266); the consequences of the British abandonment of gold in 1931 (pp. 380–384); the changes in regulation and administration in the New Deal period (pp. 420–449); the fall in the deposit-reserve ratio from 1933 to 1940 (pp. 534–541); the 'Accord' of 1951 between the Treasury and the Federal Reserve (pp. 623–627); and a whole chapter on the postwar reversal of the long-term downward trend of velocity (Chapter 12).

Those sorts of things make it a much fuller 'monetary history' than a bare account of the development of the stock of money, even combined with analysis of economic causes and effects, would have been. It is a fuller book, a richer book, and a much more interesting one as well, with a huge amount of scholarship on display in many of those discussions. Nevertheless, its backbone is a sometimes detailed account of the course of the quantity of money. Since that quantity is arithmetically determined by the quantity of high-powered money, and the deposit-reserve and deposit-currency ratios, the course of those variables was closely followed. The most important and broadest of the authors' conclusions then follow fairly directly from that analysis. In two world wars, and one other period—from 1897 to 1914—there were substantial price rises, and each was accompanied by a substantial increase in the stock of money; there were four periods of notable economic stability, each being a period of stable change in the stock of money; and there had been six severe contractions, each accompanied by a notable decline in the money stock. In the case of both inflation and contraction, there were no other cases of comparable change in the money stock which were not accompanied by the expected economic outcome. Of the contractions, four were associated with monetary disturbance and banking crisis, and two were caused by actions of the Federal Reserve System. The authors similarly pointed to the long run and general association of exchange rates with prices, the long trend decline in velocity, and the predictability of faster growth of the money supply in cyclical upturns and its slower growth in downturns. They emphasized that these regularities existed despite the changes in metallic backing and international arrangements, as well as the domestic arrangements of the banking system, and despite the variability of the deposit-currency and deposit-reserve ratios. And they said that the record showed that monetary changes were often independent of, rather than a consequence of business conditions, and that their presumption that the economic change resulted from the monetary ones was greatly strengthened by their examination of the Great Depression. Nevertheless, they clearly admitted that changes in income sometimes had consequences for the quantity of money. They also observed that the relationship between both secular and cyclical changes in the money stock and nominal income was much closer than their relation to real income, there having been markedly different rates of money growth and inflation in periods of similar real growth.

Beyond that, they noted what they called the stability of a range of monetary relations, including concerning exchange rates, velocity, and the key ratios accounting for the quantity of money. Actually, in the case of the ratios, as even their summary account shows, 'stability' meant that they could explain the developments more than that there was any particular relationship. The stability of the relations and correlations they found in themselves, as the authors noted (p. 686), showed nothing about the direction of causation. However, they thought that their narrative of events showed it clearly. In such cases as the increase in world output of gold after 1897, their task is easy: An expansion of the money stock followed. It is not quite so straightforward when they consider the debate over the monetization of silver since that was driven, on their own account, by business conditions. But they argued that the creation of the Federal Reserve provided clear examples. Three times it had taken distinct contractionary action, and each time that had been followed by a fall in output. And the fourth case—that of the extreme fall in the quantity of money between 1929 and 1933—or the 'great contraction' as Friedman and Schwartz called it, similarly, they argued, supported their view. The contraction was not caused by the Federal

Reserve, but could have been prevented or reversed by it. It was, as they put it, the 'independence of monetary changes' (p. 686) from those of the real economy that showed the direction of influence.

And this point made, the book ended, a little strangely with an enquiry into whether the Federal Reserve's errors in the Depression were psychologically or politically inevitable, and a little discourse on the 'deceptiveness of appearances'. The first, they hoped, could suggest an explanation of 'how able and public-spirited men could have acted in a manner which in retrospect appears misguided, why there was so notable an absence of economic statesmanship outside the System and hence no steady informed pressure on the System for different action' (p. 692). Concerning the second, it was said that the study of money was full of 'mystery and paradox' (p. 695), and that J. S. Mill had said, money is a useful machine, but has an independent influence only when things go wrong, which the authors described as 'Perfectly true', but liable to be misleading unless it were recognized that little could do more harm than a monetary malfunction. From there, they proceeded to explain that individuals believe they can control their money holdings, but that the total amount of money outside the banking system is not controlled by those who hold it. This was an example of the deceptiveness of appearances. Various misapprehensions about money were then listed, some of which depended on arguments that had been made earlier in the book, but which could hardly be said to be decisive, and which, in some cases, strike no chord with the idea of appearances being deceptive in the sort of way it might be supposed that the issue about individuals' money balances might. And after these rambling remarks, this mighty work, full of so much detail and insight, ended with the warm glow of a bed-time story as they finally concluded,

> One thing of which we are confident is that the history of money will continue to have surprises in store for those who follow its future course – surprises that the student of money and the statesman alike will ignore at their peril. (p. 700)

The extraordinary quality and importance of the book was hardly doubted. Culbertson (1964) identified weaknesses in many of the arguments, but equally regarded it as quite convincing on certain points, including that, as he put it, the Keynesian revolution had been built on a fiction—the fiction of a failure of monetary policy in the Depression. Tobin (1965), not quite using the same words as Tobin (1958, p. 447) described it, (p. 485), as 'one of those rare books that leave their mark on all future research on the subject'. Kemmerer (1964, p. 197) went even a little further, saying, 'This is one of the most important books of our time'. In good measure, the quality is attributable to the detail of the work, and beyond asserting that, it is hard to demonstrate, other than by a recital some details. But the slightly retrospective judgement of Temin (1977) is perhaps worth noting. He, through Temin (1976), had been one of the most determined critics of Friedman and Schwartz' analysis of the causes of the Depression, which he substantially reaffirmed in Temin (1981) and though in Temin (1989) he moderated his position, he never came round to theirs. But he said,

> this is not a book to be cited from afar. It is a book to be read, and reread, dissected, and discussed, both for its general themes and for its detailed footnotes. No reader can fail to benefit from exposure to Friedman and Schwartz's evidence and reasoning.
>
> Temin (1977, p. 151)

It is a magnificent book, but some oddities—some of them very marked—are there as well. There are two which are in themselves perhaps minor points, but have a special interest in considering the development of Friedman's thought. One is the extent to which Keynes is pushed out of the picture. Considering there is a chapter on the development of monetary thought, it is curious that 'Keynesian' ideas and approaches are mentioned only to be treated as erroneous, no idea attributed to Keynes himself, as distinct from his followers, is ever examined, and his name appears only when it is said that he did not influence something. It is a particular peculiarity considering the admiration Friedman would later—sometimes—express for Keynes.

On the other hand, a great deal of effort goes into criticizing the Federal Reserve and analysing its mistakes, particularly in the Depression. The chapter on 'the great contraction'—which was also published separately as Friedman and Schwartz (1965)—takes well over

100 pages to consider the period 1929–1933. That discussion goes far beyond the kind of account in the rest of the book since it enquired into the internal decision-making of the Federal Reserve. For this they drew extensively on the diary of Charles Hamlin, a member of the Federal Reserve Board form 1914–1936, and the private papers of George Harrison, who succeeded Benjamin Strong as President of the Federal Reserve Bank of New York in 1928, and served until 1941. To Hamlin, in particular, they credit the insight of Benjamin Strong's importance in the System in the years before the stockmarket crash of 1929 (pp. 225–226). It is a bit of an odd source for the idea since Galbraith (1954) and Chandler (1958) both clearly recognized Strong's importance. But whereas others, as noted by Kemmerer (1960), attributed to Strong's death the failure of the System to abate speculation and thereby avoid the Crash, Friedman and Schwartz thought their failure lay in the collapse of the money supply. They argued that if Strong, rather than the less effective Harrison had been in post, policy would have been more expansionary. That did not pass without controversy with both Wicker (1965) and Brunner and Meltzer (1968) suggesting alternatives based on the view that Federal Reserve was consistent before and after Strong's death. The former found that in their reactions to international events, the latter in their interest rate policy. Friedman occasionally—such as in Friedman (1985c)—referred back to his own conclusions on the matter, but seems to have taken no notice of these sorts of doubts.

Friedman and Schwartz' narrative of these four years makes for fascinating reading and must have been fun to write. But it is not really part of the story of the quantity of money, and it would have been no less fascinating in a separate paper. What it might have been is part of a wider historical appreciation of policymaking. But this discussion of the Depression is really the only time the authors stray into such territory. As recognized by Nichols (1964), the guiding thoughts are very much thoughts from economic theory. What the passage does do, though, is highlight the fact that making the case that errors of the Federal Reserve which explain the depth of the Depression was an important part of the agenda of the book.

The content of the book, it must be said, is sometimes imagined to be something it is not. It is not primarily a study of the cause of inflation; it does not say 'inflation is always and everywhere a monetary phenomenon'; it does not emphasize the point that control of the money supply is the only or best way to control inflation; it does not point to a precise, closely predictable relationship between the money supply and anything else. Nor is it, in suggesting the importance of the money supply, an original work—of course not, that was an old idea. As Laidler (1993) said, Currie (1934a, b) went as far as arguing that the explanation of the Depression in mistakes by the Federal Reserve and specifically denied (e.g. 1934a, p. 176) that it had been unable to prevent it. Currie, it should be said, goes strangely unmentioned throughout Friedman and Schwartz (1963a), despite all its scholarship. Tavlas (2011) even found in Foster and Catchings (1929) a forecast that policy would cause recession—although theirs is another work unmentioned by Friedman and Schwartz.

Nor, it should be clear, was the book a great triumph of falsificationist methodology. As Clower (1964, pp. 370–371) put it, the 'conceptual framework of Friedman and Schwartz's History is virtually indistinguishable from that of Friedman's earlier '*A Theory of the Consumption Function*' in that they are seeking to understand the statistical relationships between unobservable 'normal' values of money, nominal income, and velocity. There is no serious attempt to find a crucial test in anything with a resemblance to the Popperian sense. In that way, the methodology of the two books is just the same. So when Bordo and Rockoff (2013) said that *A Monetary History* was designed as a compilation of evidence supporting the Quantity Theory, they were of course quite right, but only up to the limited point that it is 'confirmatory evidence'. It is the data arranged in terms of the theory. It is an arrangement that has proven persuasive, and understandably so, but it is not a severe test. Harrod (1964) was precisely right about this, when he said, it offered no refutation of Keynesian ideas of the Depression for the same reason that Friedman and Schwartz argued the Depression provided no evidence of the ineffectiveness of monetary policy. It was not that those ideas were shown to be incapable of explaining the facts; it was that Friedman and Schwartz had not tried them.

Imperfectly scientific as it is, and less original in its theoretical ideas than it is sometimes imagined to be, it is a staggeringly impressive display of scholarship. To point to limitations is never, and particularly not in the case of such a work as this, to damn it, as all its reviewers, and surely every careful reader since have seen. And this time it was Johnson (1965) who captured the point so well when he said it is 'a Quantity Theorist's monetary history of the United States'. That too is precisely correct—it is monetary history told, not with absolute and unqualified faith in the Quantity Theory, but told with that theory as by far the most important organizing structure. It does not demonstrate the truth of that theory; it does not exactly assume it. What is does is show that when one starts with these presumptions, it turns out that a satisfying story can be told. That story put in two or three words would be 'money matters'; or in the longer version, 'money really matters'.

14

Quantity Theory Themes

1 The 'Restatement', and Other Restatements

Friedman (1956a)—which was both his '*Restatement*' of the Quantity Theory, and his introduction to Friedman (1956b), a book of essays by his students—began saying there had been a particular version of the theory taught at Chicago. It was not a specific theory, but a way of looking at things, and was much more versatile and useful than the 'atrophied and rigid caricature' of the theory taught elsewhere. 'Chicago', he said, 'was one of the few academic centers at which the quantity theory continued to be a central and vigorous part of the oral tradition'. He continued that Henry Simons, Lloyd Mints, Frank Knight, and Jacob Viner had 'taught and developed a more subtle and relevant version, one in which the quantity theory was connected and integrated with general price theory and became a flexible and sensitive tool for interpreting movements in aggregate economic activity and for developing relevant policy prescriptions' (pp. 3–4). And he explained that he would present a particular model 'in an attempt to convey the flavor of the oral tradition which nurtured the remaining essays in this volume' (p. 4).

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_14

There, Friedman very much emphasized that what was under discussion was a theory of the demand for money rather than, presumably, a theory of income determination. His presentation perhaps clothed the model with greater formalism than it really needed—in this it is rather like the theoretical chapter at the beginning of Friedman (1957a). Also like that discussion, his version of the Quantity Theory was a model of utility maximization. In this case, money was seen as yielding a return potentially in interest, but certainly in real services of convenience and security. This, and the rates of return on other assets, the ratio of a household's non-human to human wealth, and taste factors, determined its demand for money. The money-holding of business enterprises was considered separately since they are not ultimate holders of wealth, but the recognition that holding money was costly, and that the benefit of doing so depended on technological and business conditions brought the analysis of their behaviour into line with that of households. The volume of transactions, and other matters that might be treated as 'institutional', such as the frequency of wage payments, were excluded on the basis that it is in principle endogenous to the cost of holding money, though Friedman noted that for some problems it might be treated as constant. Those things gave a theory of the demand for money of individual households and firms, and Friedman said it was appropriate to aggregate them unless and until it became apparent that introduced a problem. He noted that there was no need to distinguish 'active' and 'idle' balances, nor 'transaction' and 'speculative' motives for holding money; and that the introduction of money-issuing banks made no fundamental difference to the analysis of that point. Then he noted that the theory would deliver a model of the determination of nominal income only with some specific assumptions about the supply of money, and the assumed constancy of the variables determining its demand, or else an extreme inelasticity of the demand function. Even then the theory would not by itself describe the division of a change into price and quantity components.

That having been said, Friedman continued by saying the general lines of the argument would be widely acceptable. Indeed, as Angell (1957, p. 601) observed, Friedman's account of the theory was not much more than a listing of things that might matter. But, Friedman continued, the Quantity Theorist was distinguished by three further views. They were that the money demand function was in fact stable, so that velocity of circulation would be predicted by a small number of specifiable variables. That was to be a crucial point, of course: The predictability of velocity, or its 'constancy' as it is sometimes casually, though inaccurately put, would be central to his later work. And indeed he observed in passing that the rejection of the Quantity Theory in the 1930s had occurred because of the apparent instability of the demand for money.

Secondly, the Quantity Theorist took it that the supply of money was determined by factors which were to an important extent different from the factors determining the demand for money. A particular case in which this would not be true would be if an increase in the demand for loans led to an extension of lending, so that the same forces determined demand and supply of money. The third point, Friedman did not state explicitly, but evidently concerns the point that the interest-elasticity of the demand for money is not great. What he said (p. 17) was merely that the Keynesian attack on the Quantity Theory had supposed that at low interest rates the demand for money could be infinitely elastic so that there would be a 'liquidity trap', meaning that increase in the supply of money would have no effect in reducing interest rates. This view was evidently rejected by the Quantity Theorist, but whether by intent or poor drafting, Friedman failed here to clarify whether he took the important point of the Quantity Theory to be merely that there is no liquidity trap, or the much broader point that the demand for money is generally insensitive to the interest rate so that its elasticity can be regarded as near to zero.

A characteristic of Friedman's presentation, not always drawn out in the debates that followed, was his departure from Keynes' (1936) approach of analysing separately the motives for holding money. Sometimes this point seems to feature in the description of Friedman as applying a 'capital theoretic' approach to the matter—meaning that he treated the demand for money on the model of the demand for other durables. There is no 'separate' demand arising from each function of a durable. As suggested by Laidler (1969) a significant source of interest is that Friedman's approach leads directly to testing of the theory or estimation of the demand for money equation. The idea of there being separate motives for holding money may lead to qualitative judgements such as for example, that demand for money is likely to be unstable, but it does not suggest a path to quantitative estimation. Friedman's approach, as he must have intended, is the one that facilitates empirical work. He said that the 'proof of this pudding is in the eating' (p. 17) meaning, obviously, that the value of the theory was to be determined empirically, and described the following four essays, written by his doctoral students. Eugene Lerner (1956) he thought showed the power of the Quantity Theory, but John Klein's (1956) study of German war inflation did not.

Friedman said that the study of more wartime inflation might be profitable but, strangely, did not mention Friedman (1952b). It is not too clear what lesson he meant to draw from Selden's (1956) study of the velocity of circulation. Friedman noted an apparent contradiction between the long-term downward trend and its procyclicality, but his comment is opaque and hard to align with Selden's paper. It does not, though, give much indication of Selden's own conclusion that he had been only partially successful in explaining velocity (p. 231). On Cagan's (1956) study of hyperinflation Friedman was clearer, noting the significance for the Quantity Theory outlook of being able to establish stability of the demand for money function in conditions of high inflation by incorporating an appropriate measure of expected inflation. He also commented briefly on the difficulty of measuring that variable towards the end of hyperinflations. Friedman then summed up, rather optimistically, really, by commenting on what he called the 'extraordinary empirical stability and regularity' of velocity (p. 21) and, noting with much clearer justification, the valuable work done by contemporary doctoral students working on money in keeping the Chicago tradition alive.

Friedman wrote many other discussions of the Quantity Theory, sometimes in broader discussions of the history of monetary thought, but also in two notable encyclopedia entries. In the first, Friedman (1968c), he gave an account which was similar in fundamental substance to that of Friedman (1956a), though quite different in presentation. The 'oral tradition' was gone, and the only American economist given an important role in the development of the theory was Irving Fisher (of Yale). The presentation lost nothing by having the veneer of formalism removed and he walked through what was to become rather a routine treatment of the Fisherian 'transactions' approach, and then the Cambridge 'cash-balances approach', before discussing the 'Keynesian attack' on the theory. He described a postwar reformulation of the theory—surely meaning the one in Friedman (1956a)—and said it had incorporated both the old idea that disequilibrium of money holdings would affect spending, and had been 'strongly affected by the Keynesian analysis of liquidity preference' (p. 439), presenting his version of that influence, and citing Johnson (1962) as further authority for its existence.

Here he discussed the 'empirical evidence' too, though not really addressing any of the difficult issues that had come up in Friedman and Schwartz (1963a), but by leaning heavily on an appeal to the data on the relationship between the quantity of money and the price level. Allowing for the trend in velocity, this was 'very close' in the United States. That is something of a shift, because it edges away from the theory being a way of looking at things, and a general organizing framework for analysis, and towards being a theory of inflation. But interestingly, saying that the same close association was not there in some developing countries, Friedman suggested there were either in a special situation because deposits were expanding rapidly, or had unreliable price indexes. Although the discussion was by no means one-sided, with arguments contrary to his own being cited, those two points were really just alternatives to treating the data as calling the theory into question, and Friedman did not really seem to contemplate that. It is almost as if he could not help himself reaching for the story that would deliver the desired conclusion.

The second of the encyclopedia entries, Friedman (1987a), was much longer, and an elegant piece of writing, albeit with some doubtful argumentative moves. It covered the theoretical points from earlier treatments, taking just the same line on them, though it did so for the most part in more detail. There was also a brief discussion of the supply of money, and a longer one of the transmission mechanism—which had been something over which Friedman was much criticized in the 1970s and 1980s. He offered a fuller account of the Keynesian challenge to the theory than he did elsewhere, and introduced international transmission, the Phillips curve and rational expectations to the story. He said that the Phillips curve had been welcomed by Keynesians as addressing a problem created by their assumption of rigid nominal wages, but that it contradicted 'the quantity theory distinction between real and nominal magnitudes' (p. 24)—with an obvious suggestion that this distinction was special to the Quantity Theory. The 'empirical evidence' was there again, although this time there was very much less indication of there being real, contrary, arguments, and the case for the Quantity Theory view was augmented by citation of Lothian (1985) and even a working paper version of Duck (1988), both of which were very supportive of Friedman's views. He could have quoted Hendry (1980), say, for a bit of balance, but nothing like that was there. Turning to policy, he said that there was an obvious implication that the quantity of money must be a key variable in policies designed to control prices or nominal income, but went on to say that it could not be used to achieve precise control of the price level because of lags and the conflicting objectives of policymakers. It was an encyclopedia entry, but it was an account of the Quantity Theory very much as Friedman would like it to be seen, with nothing really there to suggest that there might be controversy.

2 The 'Oral Tradition' of the Quantity Theory

The account of the 'oral tradition' at the start of Friedman (1956a) became notorious, though only after a delay. Following the publication of Friedman (1968c)—the first of the encyclopedia entries—and apologizing for taking so long, Patinkin (1969) wrote a rebuttal of the story. His central historical claim was that the theory Friedman presented as a reformulation of the Quantity Theory was no such thing since central elements of that 'reformulation' had not been part of the earlier thought at Chicago and that what Friedman actually presented was in essential respects derived from Keynes (1936). So Patinkin set out to describe 'the true nature of the Chicago monetary tradition' (p. 47), and in the process he clearly implied that Friedman had sought to conceal the Keynesian origins of his ideas, noting particularly Friedman's avoidance of the terminology of 'liquidity preference', or even the use of the word 'liquidity'.

Friedman surely knew of Patinkin's objections to the 1956 piece before he wrote the 1968 one. They corresponded extensively, and Patinkin (1965, p. 54 n5) contained a brief comment on the matter. In any case, whether he knew of it or not, even before that, Friedman (1964/1969, p. 73) had said the Quantity Theory had been reformulated 'in a way much influenced by the Keynesian liquidity preference analysis'. Friedman (1968c) could well be read as having been written in the light of the concerns about the historical accuracy of the first. Patinkin, though, does not seem to have seen much improvement. He noted that Friedman had accepted various Keynesian influences, but summed up the position, and ended his paper saying that in the light of those points 'one can only regret that Friedman has persisted – even with the confines of an international encyclopedia – in presenting his exposition of the demand function for money as a "reformulation of the quantity theory"' (p. 62).

That way of ending the paper clearly conveys that Patinkin was concerned with more than setting the history straight and wanted his readers to see that Friedman was presenting the material in a misleading way. That objective is interesting in itself, but perhaps more interesting is that Patinkin did not seem to be willing to let go of the issue. Patinkin (1972) was a closely argued, scholarly account of what it was that was innovative in Keynesian monetary theory, but again he chose to advertise it as a rebuttal of Friedman. Patinkin (1974) was part of a debate over Friedman's theoretical approach to monetary questions described in Friedman (1974a), but Patinkin chose to focus on disputing his historical claims. Friedman (1974f) responded to that, but then in Patinkin (1981b), Patinkin (1969) was reprinted, along with a postscript criticizing Friedman (1974f). By that time, there had also been Patinkin (1979), again criticizing Friedman over related matters. Closely considered, these later works might well be thought to land rather few blows on Friedman, but what is clear is that Patinkin was going overboard about the whole question.

And then there is the involvement of Harry Johnson in the matter. He seems simply to have adopted Patinkin's account of the history, and magnified the hints that Friedman was seeking to mislead his readers. In Johnson (1970, pp. 85–86) he asserted that Patinkin had 'shown conclusively' that Freidman was wrong about the history, and then noted that Friedman (1964/1969) accepted the importance of Keynesian thinking, and proceeded to opine that whilst Friedman had become enamoured of the 'Cambridge oral tradition' as allowing wisdom to be attributed to institutions even when the ideas were not published and that he 'unconsciously stole a leaf' (p. 86 n4) to the benefit of Chicago. If that is the kind of explanation wanted, then more likely Friedman took the idea from Simons' (1935, p. 555) description of Currie (1934b) as expounding 'a set of views which, while firmly established in the "oral tradition" of some schools, are meagrely represented in the accessible literature'.

In Johnson (1971b, p. 96), again relying on Patinkin, he said Friedman was all wrong about the history, whilst crediting him with theoretical advances. But then in Johnson (1971a), which was mainly concerned with offering a home-brewed account of how intellectual revolutions succeed in economics, he said that the monetarist counter revolution faced the problem of finding 'plausible linkage with pre-Keynesian orthodoxy' (p. 10), that Friedman (1956a) had attempted this and that Patinkin had 'exploded' (p. 11) his argument. Then, he declared, 'Nevertheless, one should not be too fastidious in condemnation of the techniques of scholarly chicanery used to promote a revolution' (p. 11). Friedman was convicted and pardoned in one breath, and although backhanded, Johnson's intention to insult him is clear, though there was nothing to substantiate the accusation. Friedman may have been in error about the oral tradition, but that is not chicanery.

Very probably no weight at all should be attached to Johnson's assessments of the substantive issues. As is apparent from a careful reading of Moggridge (2008) he was very much prone to curt judgements; and as Matthews (2000) said, he could be acid-tongued. In the case of the history of the Quantity Theory, his own inconsistent attitude to the facts is clear from a comparison of his earlier remarks with those in Johnson

(1972, p. 68 n7) where he credited Friedman (1956a) with clarity in presenting Keynesian economics, made no mention of any chicanery, and suggested it was Keynes who had obscured the issues. Then in Nobay and Johnson (1977), despite merely referring to contributions to the debate in passing (p. 475 n7), he again asserted that Patinkin had won it (p. 478).

Although Laidler (1973) thought the matter resolved in favour of Patinkin, debate over the substance of the issue in due course drew in others as well, though the terms of the debate became far from clear. On one view the question was merely whether the Quantity Theory had been alive and well at Chicago in the Depression, and understood as suggesting remedies for it, in which case Tavlas (1998) seems to show that it was, although only on the basis that it provided a broad view, rather than specific theoretical propositions. That, though, it might be said, was what Friedman was getting at when he called it ‘a way of looking at things’. Or if it is specifically the existence of an ‘oral tradition’, that is perhaps an idea on which Patinkin seems to throw particularly into doubt, though Steindl (1990) and Rockoff (2015) suggest otherwise. On the other hand, if it is the uniqueness of the Chicago view, Laidler (1993) would seem to show that it definitely was not—noting the views of Ralph Hawtrey and Lauchlin Currie in particular. Humphrey (1971), even more so, emphasized that if one were looking for an active role for the Quantity Theory in the 1930s, Chicago was the wrong place—he found Carl Snyder, Lionel Edie, Currie, and Clark Warburton all to be non-Chicago Quantity Theorists of the time. Or again, the debate might be whether the theory was understood as being a theory about the demand for money, in which case, Parkin (1986), ostensibly reviewing Patinkin (1981b)—a collection mainly of reprints—but in fact almost exclusively discussing Patinkin (1969), appears to show that, in some cases, it was. As Patinkin (1986) in effect argued, all Parkin really showed was that there are passages in earlier writing consistent with that view, not that it was an approach emphasized by the authors in question—and he also noted that it was a pity Parkin had not concentrated his remarks on the theoretical aspects of the book, on which he was more expert. There is a warning there a good few others could well heed.

At least as interesting as the debate itself, though, is the point that Patinkin and Johnson seem to have been so exercised about it. There was, after all, no pretence that Friedman was offering an historical study of the matter—it was just a few paragraphs at the start of an introduction. Leeson (2000) ventured that the explanation of their approach was that they were jealous of Friedman, and Patinkin in particular was spurred into action by the success of Friedman (1968a). Jealousy might have something to do with it; the 'success' of that paper hardly can, since, as is apparent from Forder and Sømme (2019), it was far from being the instant success it would have had to have been to bring such a quick response. In any case, had it been that paper which brought Patinkin to life, he could just as well have attacked the paper itself—it is full of weaknesses, as shown in Forder (2018b). And even if that were to explain the timing of the publication of Patinkin (1969), it can hardly explain why it was this one issue to which Patinkin kept on returning, long after his point was made, and long after Friedman had accepted the importance of Keynesian analysis.

Another possibility was suggested by Freedman (2006), also questioning the truth of Friedman's (2003) claim to have been 'baffled' by the dispute since there was so little at stake. Freedman's view was that Friedman's plan was rhetorical and it was to undermine Keynes by writing him out of the relevant history so as to promote the monetarist counter-revolution. Johnson and Patinkin, apparently, were seeking to resist this. Realistically, it is hard to see that as early as 1956 his views were well enough developed to have such a plan. He might have been happy enough to write Keynes out of the history, and take more credit himself, but on the other hand, the accusation was that he was presenting Keynesian theory, and that is hardly a way to change policy in a monetarist direction, and furthermore, he described the Keynesian influence on the theory in Friedman (1968c), before any substantial attack on him was published. The essay itself, it might be said, contains very little that is inimical to Keynesianism, except perhaps in so far as the Keynesians choose to make their case in terms of the instability of the demand for money. And if it were Patinkin and Johnson's goal to resist the monetarist counter-revolution, all they managed was some loud barking up the wrong tree, since one can grant all their arguments and leave the substance of Friedman's economics unimpaired.

There is obviously no reason the same explanation should apply to both Patinkin and Johnson. In Johnson's case, this incident may be just one reflection of a general attitude and approach he had. He had previously made some snide remarks about Friedman, such as in Johnson (1965) where he had said that a full assessment of Friedman and Schwartz (1963a) was difficult because they had not published their statistical work (which appeared only in Friedman and Schwartz [1970]). And he said, 'familiarity with Professor Friedman's style of operation leads one to suspect is not entirely unintentional' (p. 388), though in fact of course, the completion of two books of the kind in question is hardly one to be rushed. And there is Johnson (1976, p. 95) in *The Economist*, when he ended his piece saying,

> Friedman's methodology of 'positive economics' is associated with a 'black box' approach and a reluctance to explain how the economic system he is analysing actually produces the results he gets, and to state all the evidence he had. He has frequently trapped and sandbagged critics of reputation and integrity by the techniques of under-disclosure of analysis and evidence and apparent over-statements of the strength of his results.
>
> Johnson (1976, p. 95)

It is quite a way to comment on the award of Nobel Prize, which is what he was doing. So, notwithstanding that he also made some very complimentary remarks about Friedman in various places, it seems safe to say that he was not generally anxious to applaud his work.

Some part of a better explanation of Patinkin's behaviour may well be simply fastidiousness over points of doctrine—it is a characteristic that is apparent in the 100 pages of Notes at the end of Patinkin (1956). That can hardly be all, though, since a point needs to be corrected only once for the record to be straight. Another part might arise from the apparently systematic way that Friedman ignored that book. Published the same year as *Studies in the Quantity Theory of Money*, it achieved renown in scholarship, but hardly the lasting repute of Friedman's book. Friedman, though, ignored it almost completely. That might be excusable in the original, 1956, *Restatement*. But later, it would not be. Patinkin (1956) does appear in the list of references of Friedman (1968c), but is not actually mentioned in the text (and the second edition—Patinkin [1956/1965] is not noted at all). Similarly, in none of the later works discussed above when Friedman revisited the matter did Patinkin's book get a mention—when he cited Patinkin, it was only Patinkin (1948). As a spur to action after 1968, a long-standing historical error, and an immediate snub as the history is written again without being brought up to date in a way Patinkin could be forgiven for thinking crucial, seems a realistic explanation, and as Friedman continued to ignore Patinkin's book, quite a good explanation of his ongoing fervour.

The oddity, or apparent oddity of Patinkin's attack on Friedman is one thing, but it is not the only oddity of the matter warranting notice. It was said by Leeson (2003), publishing two volumes, mainly of reprints, about the dispute, that it had been most fruitful in increasing knowledge about the history of the Quantity Theory. In a sense that is true, but anyone who wanted to know about that could have set about finding out without Friedman's essay and the more interesting thing is the extent to which the authors concerned seem to have seen finding out about that history merely as a means to the important end of showing that Friedman was either right or wrong as the case may be. That question was just a distraction from sorting out the history. Yet, as he more or less said himself in Friedman (2003), all he did was write three paragraphs at the beginning of an essay, making a rather vague allusion to an oral tradition, on the existence of which nothing much then turned, and which he was quite happy to give up when he returned to the issue. The history of the Quantity Theory could perfectly well be written without reference to such trivia (as indeed some of it was in due course by Laidler [1991]).

The casualness of Friedman's presentation ought to defuse the issue, but there is more to it than that since there seems to be such a temptation to treat the remarks as reflecting—one way or another—some profound thinking. It is as if it is either chicanery, perhaps as part of a plan, running over decades, to undermine Keynesianism; or else, for all its vagueness and lack of specificity, and indeed certainly incompleteness in some respects, it reflects Friedman's great mastery of all the facts, which others set themselves merely to confirm, should they so choose. If one were going to assess those things, the important issue would be one of what Friedman knew. Very likely, it was next to nothing, as Steindl (2004) seems to suggest. In that case, whatever else was going on, he was making up his story. His evident willingness to accept that there was a Keynesian influence on the reformulation of the theory seems in itself to suggest that he had little in the way of real information about what had been said or thought in Chicago before the *General Theory*, and it is a puzzle that others have been so anxious to prove his points for him.

So, sober consideration might suggest that what happened was that in writing an introduction to a book of essays by students at Chicago, that ended saying what fine contributions they were making, he just meant to give their work a bit of a puff with some vague but warm remarks about the tradition of their University, on which Friedman knew no more than what floated to him in the air, and on which he placed no weight. Unsurprisingly, what he said can be construed to have something to it; unsurprisingly, fault can be found. The most notable point is that other people mind so much.

3 The Stability of Velocity

The motivating idea of Friedman (1956a) was that the demand for money is a function of a small number of variables and is in that sense 'stable', but Friedman's own account of what that function might be appeared a little later in Friedman (1959b) and the summary of that paper in Friedman (1959c). That approach, always described as the outcome of joint work with Schwartz, was then incorporated into Friedman and Schwartz (1963a).

The central problem as Friedman treated it in 1959 was that over most of the period velocity was procyclical, and on a very long downward trend until the postwar period, though Friedman and Schwartz (1963a) also emphasized the point that this trend had been reversed after the War. The downward trend was casually explained (p. 639) as the consequence of the services of money balances being a luxury, the demand for which rose with income. Tobin (1965) thought it not at all obvious that the marginal costs and benefits of money balances moved in the appropriate way as income grew and that the evidence of the downward trend was not so convincing, and where it did occur, much of it might be explained by the growth of deposit banking—that was in effect a factor Friedman and Schwartz left out of consideration. On the face of it the procyclicality raised a problem since the demand for money would have been expected to rise in periods of prosperity. In the book, it was discussed only briefly (pp. 642–643), but Friedman (1959d), drawing attention specifically to the similarity of the argument to that in Friedman (1957a), suggested that the demand for money was a function of permanent income. He suggested that since measured income was in excess of permanent income around cyclical peaks, demand for money would appear to be low relative to measured income, and hence velocity would appear high. That was insufficient to account for the observed magnitudes, so he also, in Friedman (1959b, p. 335), introduced the idea of 'permanent prices'—perhaps straining the terminology further than is comfortable—to mean something like 'normal prices'. Then, the data appeared to be well-explained.

The analogy between the roles of permanent income here and in the case of consumption is not as close as Friedman may seem to have suggested. In the case of consumption, households are presumed to have a motive to equalize consumption in different periods, arising from there being a reasonably accurately known limit on consumption over a number of periods and diminishing marginal utility of consumption per period (or some equivalent). In the case of the demand for money there is no equivalent limit on the total liquidity that may be enjoyed in a sequence of periods. To the extent that money is held as a transactions medium, one would therefore expect money balances to respond to transactions, not permanent income, except that there are costs in adjusting those balances. Transactions certainly rise in business upturns, but in any case, it would be those costs of adjusting balances, not an argument about permanent income, that would be the natural place to look for an explanation of the procyclicality of velocity.

Further, on the basis that there was a long-term decline in velocity, Friedman and Schwartz faced a difficulty in explaining the postwar rise in velocity. The question was crucial since as it had been put by Friedman (1956a) the predictability of velocity was the essence of the Quantity Theory. Various explanations had been suggested, including that the change was due to higher interest rates, expectations of inflation, and the development of the financial system. Friedman and Schwartz accepted none of these, suggesting instead that it was explained by increasing confidence in future stability (p. 675). Their idea was that the demand for money had fallen because confidence had risen and there was no longer such desire for an asset of such 'versatility' as money—putting the point, again perhaps, as Patinkin had suggested about Friedman (1956a), so as to avoid the word 'liquidity' (p. 673). There is obviously a rather ad hoc aspect to that, and also a peculiar one since, as pointed out by Meltzer (1965, p. 413), it would seem to require that there was an ongoing increase in this confidence as the postwar period progressed, or that such confidence became more important in explaining velocity.

In their dealing with this issue, Friedman and Schwartz offered some of the clearest instances in the book of the same cleverness as was so evident in Friedman (1957a) in handling the data and urging that it conforms to his theory. But it also highlights the traps that those with that skill confront if they take the argument only as far as required to establish their desired conclusion. Friedman and Schwartz said, for example, that expectations of inflation could not provide the explanation because, using the estimates of Cagan (1956), they could show that those expectations would have had to have changed by a gigantic amount to bring the observed change in velocity. That, someone might argue, was vitiated by the fact that Cagan's estimates related to hyperinflation and their reliability at low rates of inflation must be questionable. Another possibility was that institutional changes in the market for short term government securities had made them more attractive to corporations, thereby reducing their demand for money. One reason for doubting that was that although demand for such securities had been volatile there was no tendency for changes in holdings of them to move in the opposite direction to changes in currency and deposits, 'a movement that might be expected if they were close substitutes' (p. 661). Rather, there was some indication of them moving together. One might have thought that the analyst's expectations on that point would depend on what was causing the overall change in asset holding, and possibly the volatility of demand for government securities. But Friedman and Schwartz had reached the conclusion they wanted, and took the point no further.

Their treatment of the question of the role of interest rates may be the most interesting. They criticized the idea that interest rates might provide the explanation, first of all on the basis that it 'explains too much' (p. 648) because the rise in velocity in the earliest postwar period could be explained otherwise and so for that period an explanation based on interest rates would be 'superfluous' (p. 648), whereas for other periods it did not explain it at all. Although they attributed (p. 646 n6) the argument to Latané (1960), Gurley (1960), and Tobin (1956) most of their detailed response to it focussed on Latané's work. So if, along the lines of Latané's thinking, high interest rates were the explanation, velocity would have to be much more sensitive to interest rates than it had been over the longer period. Perhaps so, though they made no mention of the thought that since velocity had been declining for all those years money holdings were very large, and it might be that in that case they are more sensitive to interest rates. And as Goodhart (1964, pp. 316–317) pointed out, calling their argument 'disingenuous', if comparisons of different periods were to be made, one would expect Friedman and Schwartz to frame it in terms of the real interest rate, not, as they did, the nominal rate. Noting that Latané believed that interest rates could explain the whole of the postwar rise in velocity, they argued that for a longer period, the better single factor was theirs, and seem to regard that as one basis for dismissing interest rates altogether which, plainly, it is not. Then, similarly, after giving their own almost year-by-year account of hypothesized factors leading to changes in general confidence in stability (pp. 673–675), and asserting its tentative character and the need for proper testing, they concluded 'Nevertheless, changing expectations about economic stability seem at the moment a more plausible explanation of postwar movements in the velocity of money than any of the other factors we have examined'

(p. 675). That, obviously, does have the tone of presuming that there is to be one answer, and one answer only.

There is nothing there to say that Friedman and Schwartz were wrong, though as Gregor Becker (2017, pp. 39–40) noted, the pre-War period was not free of anomalies and, in Friedman and Schwartz (1970), the authors were to say that they preferred their definition of money on balance, but that 'for some specific periods one of the others may be preferable' (p. 92). There must be a bit of doubt about what they really meant by that, and in what circumstances they or anyone would think it proper to switch definition. Still, having said that Latané's account worked better for his own, narrower definition of money than it did for theirs, and notwithstanding that the definition of money had been chosen essentially because it provided for predictable velocity, the point was not taken up.

In any case, considering that on their own account, their idea that increased confidence in economic stability provided the explanation could not be shown to be correct, and also could not be shown, even if correct, to be sufficient, their willingness to assume that their arguments dealt with other possibilities is notable. And as Cootner (1966, p. 105), and Tobin (1965, p. 474) noted in various ways, if they were going to argue that greater confidence reduced the demand for money, there needed to be a systematic enquiry into the effects of confidence or a fuller argument as to which periods were those of normal confidence and which were abnormal. Indeed, this is all the more so, considering that one of their points against interest rates was that they might explain 'too much' or 'too little' in different periods.

There are further issues they might have considered. For one, 'economic stability' might mean so many different things that it presents a very elastic explanation. And one might take that point further, since expectations about it might change very quickly, and so if that is a significant determinant of velocity, Friedman's Quantity Theory would be in real difficulty. But that thought, apparently, did not occur. In Friedman (1961f, p. 263), though, the predicted reversal of the post-war trend in velocity was specifically described as a 'critical test' of the explanation in terms of confidence. It is difficult to see that Friedman was vindicated there,[1] but in Friedman and Schwartz (1963a, p. 700) the point was made rather more cautiously, although it amounts to the same thing—the authors expected the downward trend of velocity to be resumed but said, 'We shall have to wait for experience to unfold before discriminating finally among the alternative explanations'.

Friedman and Schwartz' arguments here are not satisfactory on any score and it is perhaps worth noting that the issue could be seen as raising one or two fundamental difficulties for them. To admit the effect of development of the financial system would certainly open a can of worms, since that might matter in any period. Indeed it was to become one of the bases for arguing that velocity should be expected to be unstable. Alternatively, to admit the effect of developing money substitutes might seem to call into question the reliability of the utility of their definition of money—perhaps these, and other, future, 'substitutes' need to be included. And perhaps most of all, considering what was to follow, a role for inflation could hardly be considered if one for interest rates was really to be denied.

Only a little later, though, Friedman (1966a) clearly accepted a role for interest rates in the demand for money. Indeed, he showed great irritation at Friedman (1959b) having been interpreted as denying it. He said 'inability to pin down the elasticity is very different from assigning a zero value to it' (p. 72 n1), despite the fact that the equation he presented contained no variable for interest rates. The impression that Friedman had been denying that the interest rate was an important determinant of money demand is clear and the discussion in Friedman and Schwartz (1963a) is probably more important in creating that impression than that of the 1959 paper. Sure enough, as Friedman (1966a) insisted, nowhere does he assert that interest rates have no effect, but the efforts he went to in order to make it seem unimportant, and his summing up against it, just like the presentation of an equation without it in Friedman (1959b) clearly convey an attitude to seeking to relegate it to irrelevance.

Nevertheless, the effect was clearly admitted in 1966. That issue excited some—Okun (1970, p. 58 n1) said it was enough to 'disprove

[1] Cf.: https://fred.stlouisfed.org/series/M2V.

the Quantity Theory of money', whereas Friedman dismissed the importance of the matter, expressing doubt about the usefulness of seeing the real and monetary sectors as separate and saying that Johnson (1965) (and similarly Meltzer [1965]) were mistaken to think the matter had great theoretical importance. For long run questions, inflation and interest rates mattered; in cyclical analysis monetary changes affected both output and prices (pp. 81–82). He went from there to restating that the key Keynesian proposition related to absolute liquidity preference, and they had been wrong about that, but had they been right, the matter would be of fundamental importance. But the argument merely about whether to include interest rates in the money demand function was not.

It is of course this kind of argument that led to Friedman being regarded as a slippery debater. When Friedman (1959b) could not 'pin down' an effect of interest rates, that was a conclusion. When one is as skilled at extracting the required confessions from the data as Friedman had frequently shown himself to be, if he 'cannot find' an effect, the reader is not invited to think that what was meant was that such an effect was probably there, but merely hiding from detection. Had that been the intention, it would have been easy enough to affirm that the result was implausible and contradicted expectation. Rather, the tone of that paper is one of triumph. What was suggested was that the (supposedly) Keynesian view of an infinite interest elasticity was trounced. It is not necessary to have zero elasticity to contradict that view of Keynesianism, but it is hard to escape the feeling that Friedman thought it desirable. And as Johnson (1965) noted, there is an importance to the inclusion of any effect of interest rates since they certainly have real determinants and there would then be a route for those real determinants to have a monetary effect, whereas, he suggested, Friedman and Schwartz were committed to denying that.

In Friedman and Schwartz (1963a), simply the effort the authors devote to finding another story, its shakiness when located, and their conclusion in favour of it, and it alone, without apparently contemplating the possibility that other factors might contribute, make the direction of their thinking very clear. As to the question raised by Okun's remark—whether the necessity of including interest rates in the equation amounts to the refutation of the Quantity Theory, that really depends on what one means by 'the Quantity Theory'—on the loose treatment of Friedman (1956a), where it is principally a framework for organizing thinking, as Friedman said, little harm is done. But where it is taken to offer specific guidance on the control of inflation by means of the control of the quantity of money, or to be the basis of the Classical dichotomy, it is much more problematic.

4 Causation

Despite Friedman's avoidance of the word, questions concerning causation were crucial to his advocacy of the Quantity Theory, and are a central concern of the analysis of Hammond (1996). Two issues in particular arose. One was that of the direction of causation between money and activity; the other was that of the elucidation of the transmission mechanism—the question of how the effect of changes in the quantity of money came about.

On the question of the direction of causation, Friedman and Schwartz very much felt that principally, it was changes in the quantity of money that brought about changes in activity, but they did equally clearly note a subsidiary role for the opposite effect, and seeing it particularly over shorter periods. They said,

> While the influence running from money to economic activity has been predominant, there have clearly also been influences running the other way, particularly during the shorter-run movements associated with the business cycle… Changes in the money stock are therefore a consequence as well as an independent source of change in money income and prices, though, once they occur, they produce in their turn still further effects on income and prices.

The relative strength of effects was made clear, though, when they continued immediately, saying,

> Mutual interaction, but with money rather clearly the senior partner in longer-run movements and in major cyclical movements, and more

> nearly an equal partner with money income and prices in shorter-run and milder movements – this is the generalization suggested by our evidence.
> Friedman and Schwartz (1963a, p. 695)

Friedman noted the same thing elsewhere, such as Friedman (1964d, pp. 10–11) whilst citing Friedman and Schwartz. Nevertheless the question of the effect from activity to money proved rather a difficult one, with Friedman sometimes seeming to disregard it entirely, or even to suppose that it did not exist. Consequently some formed the view that he was not serious about it. J. M. Clark put it to Friedman directly in correspondence quoted by Hammond (1996, p. 92), saying 'I remain skeptical as to whether your recognition of a two-way causal relationship is real, or a pro forma device for disarming criticism without allowing the recognition to affect your views in any operational way.'

In making their case, a part of the argument certainly concerned the appearance of the timing of monetary and business changes. From the close analysis of the question in Friedman and Schwartz (1963b, p. 63) they summarized their finding, saying, that 'beyond any reasonable doubt', there was a cyclical pattern in the stock of money. Referring to the 'reference' peaks and troughs of business cycles, as calculated at the NBER, they said,

> The rate of change in the money stock regularly reaches a peak before the reference peak and a trough before the reference trough, though the lead is rather variable. The amplitude of the cyclical movement in money is closely correlated with the amplitude of the cyclical movement in general business

The question of the value of this timing evidence attracted some controversy. Brainard and Tobin (1968) and Tobin (1970a) clearly thought it a weak point in the argument. Both those papers were theoretical presentations demonstrating how treacherous the timing evidence can be. The advantage of the models being entirely theoretical was that it was clear what the true causal links were. That made it possible to show, as Tobin and Brainard said, 'In a highly interdependent dynamic system, the chronological order in which variables reach cyclical peaks and troughs proves nothing whatever about directions of causation. Although few people would seriously claim that cyclical lead-lag patterns are a reliable guide to direction of causal influence, believers in the causal primacy of monetary variables have offered the timing order of variables in business cycles as partial evidence for their position' (p. 120). In the latter piece Friedman and Schwartz were specifically accused of relying on the timing evidence, and argument was clearly aimed directly at them. The paper presented a pair of models, one of which Tobin thought 'ultra-Keynesian' the other 'monetarist' but in which the first showed income lagging behind money, and in the second the opposite.

Friedman (1970c) noted that Tobin had said that he thought changes in money the 'principal' cause of changes in money income and complained that this was unscientific language, asking 'What does "principal" cause mean? If there were an unambiguous way to count "causes", presumably it would mean, "accounts for more of the variance of money income than any other single cause"' (p. 319) and continued in that sort of vein also pointing out that if the money supply were held constant, 'Changes in the supply of money would then account for zero per cent of the variance of nominal income. Would Tobin then say that money is of no importance?' (p. 319). Friedman may give the impression of being evasive—of course his opinion was that predominantly, and in the normal case, significant monetary events preceded and caused significant real events. If it is 'unscientific' to describe something as a principal cause, what of calling it 'the senior partner'? He would have objected even more if Tobin had said the idea was that money was the only cause, and presumably would not want it to be said to be a secondary, or minor one, so he should have accepted the characterization of his position that he was offered.

Again, it was the kind of thing to make Friedman look slippery, but in this case it was a peculiar way for Friedman to argue too, since he had a better argument available, which he also used. That was to object to the implication that he relied solely on a naïve correspondence in timing. Friedman and Schwartz (1963b) confronted that point head on by saying that if it were discovered that there was a correlation between the output of dressmakers' pins and economic activity, no one would believe pin-making the cause of the business cycle. They noted that the theoretical case for an important effect from money was much easier to make than that from pins, but also pointed to a variety of other evidence. This included the point that changes in the quantity of money could be independently explained, in connection to which they considered a large number of historical incidents seeming to confirm their view. And they noted the difficult for an alternative explanation in explaining both the business cycle, and why it should be that the monetary changes also occurred when they did.

It is also notable that in the 'summing up' of Friedman and Schwartz (1963a, Chapter 13) they is by no means emphasize matters of timing. But perhaps a more direct treatment of the matter of causation is found in Friedman (1964d) which however includes nothing foreign to Friedman and Schwartz (1963a). There he cited the point that there were cases where changes in the quantity of money had identifiable sources that were not changes in business conditions. He cited Cagan (1965) for the argument that since change in the money supply must be arithmetically attributable to changes in base money, the deposit ratio, and the reserve ratio, evidence on causation could be extracted from their behaviour in relation to business conditions—if it were to be that business conditions determine the money supply, that effect would have to be an effect on one of the three components. Yet for secular changes that was not the case, though there was more indication that it was for cyclical changes. He considered the matter of timing of peaks and troughs of the business cycle and of monetary growth, and argued at length that, though it was far from decisive, there was some support for the view that money was the cause and activity the effect. Another argument was rather deeper. It began with the observation that cyclical peaks and troughs could be seen as pairs of about equal magnitude if the trough were treated as coming first, but not if the peak did. That was an important point in itself since it suggested that the idea that deep recessions have their origin in great booms was incorrect. Friedman also interpreted it as suggesting a picture whereby the economy was normally near to maximum production, but sometimes something would cause it to fall below that level, before returning to it, but not beyond it. The same question was then asked about the money series, and it was found that same pattern of correlations was there and Friedman inferred that this showed that the monetary changes were causing the business cycle, rather than the other way round. Had it been that changes in business conditions caused changes in the quantity of money, he argued, the pairings of equally-sized monetary fluctuations should have seen periods of rapid increase in the quantity of money preceding those of stagnation in it (pp. 17–18). He then cited various pieces of data from other countries: money and income had been on an upward trend in Yugoslavia, except that the trend for each was broken by one year of stability, and that year was one year earlier in the money series than the income series; in Israel, rates of change in money led rates of change in income; in Japan, policy changes were driven by the balances of payments, but the rate of change of money again led the rate of change of income. Although he did not make it very clear, Friedman obviously believed that the combined weight of different considerations was what was persuasive about the direction of causation.

The debate on the direction of causation was of course just one, more or less started by Friedman, that was to go on without definite resolution for decades—the appearance much later of Moore (1988), arguing the opposite case is testimony enough to that. That book became a landmark of the argument for 'endogenous money'.

The further question of the transmission mechanism was also addressed in Friedman and Schwartz (1963b), again perhaps more clearly than in Friedman and Schwartz (1963a). In the shorter piece, this too was linked to the question of the direction of causation because they said (p. 59),

> we shall not be persuaded that the monetary changes are the source of the economic changes unless we can specify in some detail the mechanism that connects the one with the other.

The explanation they gave, very much in concordance with Friedman (1956a) was along the lines that an increase in the quantity of money would first increase deposits, then the price of other assets in a gradually diffusing process. In due course the wealth and relative price effects would lead to the creation of new assets.

There were three published discussions of this paper. First it was greeted with some scepticism by Minsky (1963), who seemed to accuse Friedman and Schwartz of taking an ideological position on the importance of money and doubted whether their account of the transmission mechanism was really distinct from one that would be advanced by a Keynesian. Okun (1963) pointed to a great many doubts and more or less just declared himself unpersuaded. He simply doubted that monetary policy could be so powerful relative to fiscal policy, or that they had described the demand for money correctly, expressing particular scepticism as to whether they were right about its interest-insensitivity. On the transmission mechanism he made a more concrete point in noting that Friedman and Schwartz did not consider the point that monetary expansion might relieve credit rationing. That is an interesting detail, but does not really argue against the power of monetary policy. In summary though, he declared their work stimulating, and said it and other works by Friedman posed ‘a major challenge to the unconverted’ (p. 76) amongst whom he obviously counted himself. And he said that he found many of their results implausible, but could not account for them other than as Freidman and Schwartz did. One imagines both that many readers felt that way, though few actually said so, and that the authors would have found it frustrating, that they still, seemingly, could not win those people round. Then Warburton (1963) was rather at the other end of the spectrum, saying he could not see anything in the results that would surprise anyone who had studied money in relation to the business cycle. Perhaps he was feeling his work had been neglected.

5 The Quantity Theory Versus the Income-Expenditure Theory

Friedman (1956a) is often regarded as the first blow—and presumably as being intended as the first blow—in the monetarist counter-revolution. It is, as perhaps I have implied, something of a stretch to see it that way since even the idea of a ‘counter-revolution’ was rather far in the future in 1956. The idea of Gregor Becker (2017) is more to the point: It was a theoretical paper setting an empirical agenda. Part of that agenda was of course pursued by the following essays in the book; part by the enquiring into the question of the effect of interest rates on the demand for money. A third part, which developed various strands over the following years, related to the utility of the Quantity Theory in understanding or forecasting macroeconomic developments, and particularly its utility in comparison to the empirical insight available from Keynesian theory.

5.1 Friedman and Becker

A preliminary to the main lines of argument on this question came in Friedman and Gary Becker (1957). They did not actually consider the comparison Keynesian system with the Quantity Theory, but merely a difficulty in estimating a consumption function and using it to forecast income. That was that the error in estimated income was dependent not just on the accuracy of the estimate of consumption, but also on the size of the multiplier. The size of the multiplier depended on the form of the consumption function. So it could be that adopting a consumption function leading to a small multiplier would give better forecasts of income even if it gave worse forecasts of consumption. They even showed that treating consumption simply as trending could give a better approximation for income than certain simple Keynesian consumption functions in which investment and the multiplier determining income—in other words it was a plausible modelling strategy to treat the multiplier as zero. Clearly that cast doubt on the value of the income-expenditure model.

Their paper attracted critical comment from Kuh (1958), Johnston (1958b), and Klein (1958). All the commentators noted that Friedman and Becker had used very simple Keynesian consumption functions, and doubted that they were taken seriously, and all found ways in which a more Keynesian view of the matter could be made to deliver much better results—Kuh did it, for example, by making consumption depend on lagged income; Johnston by using a consumption function from Modigliani (1949), incorporating previous peak income; Klein seemed to admit that simple Keynesian models had little practical use, but said it was easy to construct better ones and that the points made by Friedman and Becker were already well understood, and that the statistical ones in particular went back to Haavelmo (1943). He also realised rather more explicitly that the others that what was really at stake was Friedman's approach to modelling consumption and the value of 'simple' models. On this though he felt that others simply did not accept Friedman's view and that progress was much more likely to be made by accepting the complexity of the required theory of consumption.

Friedman and Becker (1958a) replied to the first two, accepting what they said, and denying that the remarks amounted to a criticism, referring to Friedman and Becker (1958b), their reply to Klein for elaboration. There, the authors pleaded that the only substantive conclusion they had reached was that the assumption that the multiplier was zero produced better results than a naïve Keynesian consumption function, and therefore the criticisms, involving more sophisticated functions were misplaced. That, of course, was not the whole story since if it is kept sufficiently clearly in mind that the naïve Keynesian consumption functions had no life beyond elementary explanations of theory, then their own conclusion would be uninteresting. Clearly, the subtext of the paper was that it was Friedman (1957a) that pointed the way to better consumption functions, just as it was that 'better' meant 'more useful for the purpose at hand' rather than as the commentators clearly presumed, being necessarily a more thorough unpicking of the determinants of consumption.

Friedman (1958e) chose to add a further response to Klein, saying, no doubt correctly, that the criticism was really of his book. There, he did not so much argue the pragmatic point that his consumption function worked well, and that was all that mattered, but rather that the permanent income hypothesis offered better explanations. He said that consumption functions based on measured income needed to be augmented with additional explanatory variables because of the inadequacy of the measured income concept. His approach on the other hand created the 'possibility of isolating the influence of some of the variables in question in their own right rather than as proxies for income differences'

(p. 548). And he went on, then, to suggest that some of Klein's further analysis suggested that Friedman had been on the right track.

5.2 Friedman and Meiselman (1963)

Friedman and Meiselman (1963)—the paper that Friedman and Friedman (1998a) said had to be presented after dinner since it was not to be taken seriously—in a sense presents another line of thinking seeking to make the same general point as Friedman and Becker (1957). This time, though, rather than comparing the predictive power of the Keynesian theory to a trend, the authors cast the matter in terms of the stability of the demand for money and the stability of the multiplier—that is, of the reliability of estimates of those two magnitudes. Consumption was estimated alternatively as a linear function of the quantity of money or of autonomous expenditure. Their idea was that these alternative approaches arose from theories which supposed velocity or the multiplier respectively, to be stable. Friedman and Meiselman then showed, to their own satisfaction, that except for the Depression period, the demand for money was much more stable than the multiplier. At one point they concluded that their results were 'strikingly one-sided' and that 'the critical variable for monetary policy is the stock of money, not interest rates or investment expenditures' (p. 166). At another, they said their results could not be decisive, they argued that they created a presumption that further investigation would yield more benefit if based on the Quantity Theory than the income-expenditure theory (p. 174).

Like Friedman and Becker (1957), this paper quickly attracted a collection of rebuttals by authors arguing along broadly similar lines, disputing the view that evidently very simple models could provide useful insight into the direction of future research, with particular criticism focussed on Friedman and Meiselman's definition of 'autonomous expenditure'. The difficulty with that concept was that in simple expositional models, it was easy enough to say that 'investment' was exogenous, whereas 'consumption' was a function of income, and derive the conclusion that income would be determined by investment and the multiplier, in actual application, identification of the exogenous components of income raised a serious problem. To apply the theory, there needed to be identifiable expenditures on final demand which were independent of total income. Hester (1964a) suggested four other ways of capturing this concept, and Ando and Modigliani (1965) in particular criticized Friedman and Meiselman's approach to this as well as their econometrics, and accused the authors of having reached their results by using a 'strikingly one-sided procedure' (p. 714), or to put that more straightforwardly, there was nothing striking about the results at all—other, reasonable approaches generated quite different results. DePrano and Mayer (1965) similarly felt that the superiority of one approach over the other was not established, and that both monetary and fiscal variables played a role in income determination, so that 'it is incorrect to stress either autonomous expenditures or money to the exclusion of the other variables' (p. 747).

The responses of Friedman and Meiselman (1964) to Hester, and Friedman and Meiselman (1965) to the others also resembled the responses to the comments on Friedman and Becker (1957) in that much weight was placed on the point that the critics had misunderstood what was being argued. They said, as Friedman often did, that his earlier exposition must have been unclear to have attracted the comments it did, but in this case it is certainly arguable that Friedman and Meiselman (1965) offered a clearer exposition of their line of thinking than the original. There, they debated some points of statistical approach and argued that their critics criticisms were ill-founded, but also emphasized the value of simple rather than sophisticated models. They said that the issue at stake was very basic and so should lend itself to straightforward analysis, and that multivariate analyses would inevitably have to be confined to short periods, whereas they could consider a much longer period (pp. 761–762). On the other hand, they said it was the avoidance of various ramifications that meant their work could not be decisive.

A crucial question, though, was over the definition of 'autonomous expenditure'. Friedman and Meiselman said that it had become apparent that there was no straightforward way to define it. They argued, quoting their original paper (p. 765), that an approach to determining whether a certain expenditure should be included as autonomous was to check correlations with and without its inclusion. Here, although their statistical approach might be questioned, they were clearly thinking in terms of an 'empirical definition' of autonomous consumption, rather than, as their critics tended to prefer, one based on a priori reasoning about the economic relationships. The reasons were much like those at play in the definition of money: Theory provided a concept, but not a determinate way of measuring it, so the investigation of the theory had to begin by discovering a way of measuring the concept, and the way of doing that was to use the measure that allowed the theory to perform the best.

And in their summing up, they put rather powerfully a point of view which was very much a theme of Friedman's work from around this time. They said that the readers of textbooks could well think that the Keynesian system had brought coherent theory linking concepts with easily identifiable empirical counterparts. But they said, considering 'autonomous expenditure', and including their critics in the remark,

> All of us use words to describe it – like 'independent,' 'uncorrelated with residual error,' 'exogenous' – that are figurative rather than operational. We proposed an operational criterion for choosing its empirical counterpart that we regard as far from satisfactory and that our critics apparently regard as beneath mention. They rely on what is 'beyond reasonable doubt' or 'plausible'. (p. 784)

They admitted that this may have led them to a measure that was unfavourable to the Keynesian theory, and that the 'fishing expedition' of their critics had led to measures which made the theory look better. But, they said, their measure arose from a procedure they specified, and their critics' did not. That is, from the point of view of Friedman's empirically-based approach, crucial. A way of putting it is that it might be that the income-expenditure theory contains much truth, but it remains abstract and—therefore—of little if any use, because 'autonomous expenditure', whilst a conceptually clear idea, cannot in fact be given an empirical counterpart. For that reason, there is nothing to be done with the conceptual apparatus offered by the theory. In principle, the Quantity Theory might suffer the same fate if, for example no worthwhile meaning could be given to the idea of 'money'. But in practice this was not the case.

Friedman and Meiselman do not quite say, though they might have done, that the case for the Quantity Theory was stronger after the criticisms than it was before it. Their own work had shown—or had purported to show—that the Quantity Theory performed better than the income-expenditure theory. In Friedman and Meiselman's view, their critics had demonstrated, without meaning to, that as an operational, empirical tool, the income-expenditure theory could barely get off the ground because the key concept of autonomous expenditure could not be given an empirical measure.

The same methodological issue arose, with much the same outcome when Macesich (1964) presented results for Canada that were consistent with Friedman and Meiselman's findings, noting the interest in the point arising from Canada's being a much more open economy. He was criticized by Clarence Barber (1966), again doubting the definition of autonomous expenditure as well as raising issues about the significance of changing exchange rate regimes and questioning the ability of the Quantity Theory to explain the Depression in Canada. Macesich (1966) responded rather defensively saying he had addressed some of the issues raised, but making explicit the point that advocates of the income-expenditure theory were failing to find clear-cut ways to give empirical content to their theoretical ideas. Then Barrett and Walters (1966) investigating the United Kingdom, argued that the appropriate definition of autonomous expenditure, might vary in different countries, perhaps because of the significance of the trade balance. And Lewis (1967) also addressed the methodological issue, suggesting that Friedman and Meiselman were inconsistent in the application of their procedure, but not seeming to question its value. And after that, DePrano (1968) then restated much the same position as previously, and some time later, Modigliani and Ando (1976) were still fighting the battle.

As to the results of the debate, it is hard to see that they were better than inconclusive from Friedman's point of view, and Laidler (1971) thought it never likely to achieve anything since when the competing theories were carefully considered, it seemed the tests were a poor way of differentiating them. On some assessments, like that of Edge (1967), Friedman and Meiselman were clearly the losers. Even if that is correct, one aspect of their work, discussed by Bias (2014), is that Friedman and Meiselman's way of comparing single equations taken to represent competing theories, rather than trying to compare full macroeconometric models, was taken up in the 'St Louis model' by Andersen and Jordan (1968) and Andersen and Carlson (1970). There clearly is a resemblance in the approach to econometrics and Jordan (1986) for example, said they considered their work 'a sequel' to Friedman and Meiselman. It is interesting, though, that neither of the original papers actually mentioned their paper. Their monetarist conclusions then led to further controversy and methodological argument about whether single equations could or could not represent theories appropriately.

A different issue, though, is the point made in Friedman and Meiselman (1965) and clearly appreciated by Macesich, that the utility of the theories depended on its being practicable to give empirical content to the theoretical concepts. Johnson (1970, p. 86), putting the matter in very Friedman-like terms, said that the crucial test of good theory was the ability to predict much from little. As Friedman and Meiselman themselves said, they addressed that with a stated and replicable approach whereas their opponents theorized about the meaning of autonomous expenditure and allowed themselves to try many different approaches to its measurement, as well as regarding theoretical criticisms of Friedman and Meiselman's preferred measure as damning it. But as Gregor Becker (2017, p. 61) noted, the critics failed to appreciate the point about the practical application of theory. Indeed, at one extreme there was Hester (1964b) who even went as far as to say 'intuition' was essential to the identification of the empirical correlate of autonomous expenditure (p. 377).

5.3 Friedman's *Theoretical Framework for Monetary Analysis*

Friedman (1974a) was then a further attempt to persuade his opponents. That paper was a modified version of Friedman (1971b) which was itself an amalgam of Friedman (1970f) and Friedman (1971c), the latter of which also appeared as Friedman (1971d)—a publication of papers at a conference in Sheffield. The 1974 version appeared in Gordon (1974a) along with criticisms of it and responses by Friedman which had previously appeared in the Journal of Political Economy in 1972. In his introduction, Gordon (1974b) explained the objective as being to allow Friedman to present, and to debate, the theoretical presumptions underlying Friedman and Schwartz (1963a).

That being the purpose of the exercise, Friedman took something of a circuitous route, beginning with fourteen pages of discussion of the variations on the Quantity Theory, along the lines of that from Friedman (1968c), followed by about the same amount of discussion of what he described as the Keynesian challenge to it. This led to what he called a 'simple common model' of seven unknowns and six equations. Three of the equations were investment and consumption functions, and a national income identity summing them; the other three gave the supply and demand for money, and their equality. These, said Friedman, could be accepted by Quantity Theorists, and by income-expenditure theorists, but that between them, they had seven variables—consumption, investment, income, the interest rate, the price level, the demand for money, and the supply of money, so there was, as he put it (first in Friedman [1970f, p. 219]) in terms that became well known, a 'missing equation' in this common simple model. The basic stories of the Keynesian and old fashioned Quantity Theory were said to be that they took the price level, and the level of income, respectively, as being exogenous, though in connection with the former, the Phillips curve was also mentioned.

A third approach to finding the missing equation, though, presented Friedman's 'monetary theory of nominal income' by assuming a unit income elasticity of the demand for money, and supposing the difference between the anticipated real interest rate and the anticipated rate of growth to be fixed. With a little further simplification that gave a model where changes in the money supply explained changes in nominal income rather than in either prices or output. So rather than assume one or other fixed, Friedman treated the combination of the two as the thing to be explained and the Quantity Theory emerged in a different role, as he put it (p. 34), in deriving 'a theory of nominal income rather than a theory of either prices or real income.'

Of those engaged in the debate itself, Brunner and Meltzer (1974) faulted him for ignoring fiscal matters—which, they said, introduced plenty more variables so that there were plenty of other missing equations. As Hammond (1996, p. 152) argued, they might appear to be the friendliest of the commentators, but their criticism was as deep as any. Davidson (1974) pointed to the importance of uncertainty in denying that Friedman offered a reasonable account of Keynesian views; and Patinkin (1974)—discussed above (p. 221)—was rather off the point criticizing Friedman's intellectual history again. Tobin (1974) thought both the monetarist and Keynesian positions had been caricatured, and that it had been implied that the whole economics profession had failed to address the relationship of output and prices, until what he called 'Friedman's ostentations discovery of the problem of "the missing equation"' (p. 83). His irritation with Friedman for mischaracterizing the debate is apparent throughout, but he also chose not to hold back on his criticism of the model. He argued that Friedman had assumed the real interest rate fixed by real factors, and the nominal rate by that and firmly held expectations of inflation, and that had to mean that fiscal policy was very powerful. The model was 'bizarre' and it was 'hard to imagine that it was seriously intended' (p. 82). Friedman (1974f) responded to all the comments but it is difficult to see that he added much of substance to economic understanding. Of Brunner and Meltzer, he more or less agreed that he had omitted discussion of many things, and simply said that he was focussing on others. That entirely avoids the point that, Brunner and Meltzer clearly meant to say these things were crucial. Something of the same is true of his response to Davidson, except that he also took issue with the interpretation of Keynes and Keynesian economics he had offered. Responding to Tobin, he perhaps came closer to discussing the actual economics, considering the reasons for alternative views of the size of policy effects on goods and money markets, for example. But some of that concerned arguments about what had previously been argued, and he shortly moved on to saying that Tobin had not interpreted him correctly, and then his claim that Tobin's turn of mind was 'Walrasian' and his own

'Marshallian', which was supposed to explain the difficulty they were having. And in the case of Patinkin, he had not really had much of the substance of economics put to him.

The substantial part of Friedman's paper which was published as Friedman (1971c) was also discussed at the Sheffield conference. There, Harrod (1971) politely said how much he admired Friedman and how welcome the paper was, before criticizing it on a variety of grounds, some as fundamental as noting that the change of nominal income should not be considered in the absence of a discussion of cost-push inflation, and even questioning the Fisherian proposition, which he said had an earlier statement in Marshall (1887/1926), that the nominal interest rate is determined as the real rate plus the expected rate of inflation. It could not be so, he said, because money and bonds are both money-denominated and so a commonly-expected change in the value of money cannot change the exchange rate between them. Right or wrong, those are not really the sort of criticisms that could have left him feeling Friedman's analysis provided much insight. Wilson (1971) doubted the value of investigating the change in nominal income without investigating its breakdown into price and quantity components; the utility of analysis that excluded cost-push inflation; or one that seemed to say so little about the forces that tended to strengthen or weaken cyclical disturbances. A little point of interest, perhaps, is that in the general discussion, reported in Clayton et al. (1971, pp. 69–71) it was suggested that the problem of the division of changes in income into price and quantity effects might be addressed by means of the Phillips curve and the note of the discussion (p. 70) seems to have Friedman (1971e) denying this, though of course it may be an incorrect record of what was said.

All in all, the presentation seems to have pleased no one (except the unidentified sycophant reviewing the book for Recherches économiques de Louvain in 1976 [volume 42, part 1, p. 70], and the authors of a remark in 'Choice', quoted on the back of the paperback edition). When it was cited, it was less often because it was thought to advance thinking than because it provided something to attack—as it did for Davidson (1978) or Schrock and Smith (1979), for example. Even Friedman made very little of it—only a point from his rebuttal of Tobin appears in Friedman (1987a), it was hardly mentioned in Friedman and Friedman (1998a)—just once to say it was published, and that Keynesianism 'was still dominant' (p. 231) at the time, and once for a long quotation about the difference between the experience of graduate students at Chicago and the LSE. Again, one must suppose that from the point of view of Friedman, its lack of impact was a disappointment.

5.4 A Sequel to *A Theoretical Framework*

Stein (1976b) is in some respects a follow-up to the Gordon volume. As editor, Stein (1976a, p. 3) quoted Tobin (1974, p. 77) saying that if the monetarists and Keynesians could agree on the matter of which parameter values in which equations would support either case it would be possible to move to evaluating the evidence, but the book had made no progress towards that agreement. Stein's own volume, though, may have ended in an even less satisfactory position.

There were four principal papers, and Friedman and Schwartz each made comments on one of them, but there was no major contribution from them. The book ended with a good joke from Christ (1976). That proposed there was reasonable agreement that monetary policy was slow to have effects though operated with agility by the Federal Reserve, whereas fiscal policy, having quicker effects was more suited to management of the business cycle, but was operated very slowly by Congress. Christ suggested it would be better if the responsibilities for the two branches of policy were swapped. As that suggests, the arguments over the issues the book was intended to address were inconclusive—just as much so, in fact, as they had been in Gordon (1974a). Fels (1977) was surely right to think that whilst specialists in monetary matters might be interested, everyone else could safely ignore it. Indeed, but for Christ's comment, the book even ended with an argument between Friedman (1976a) and Tobin (1976). The former said that Tobin and Buiter (1976), on which he was commenting raised three issues, and whereas the authors addressed the third, he would comment on the first two. Tobin responded, wanting to accuse Friedman of irrelevance and inconsistency, and contrasting Friedman's views expressed in *Newsweek* with other things he said and the state of professional opinion. Plus ça change.

It could well be said that the timing of both this book and Gordon (1974a) were unfortunate. The earlier one proposed to leave aside the question of explaining specifically inflation just at the time that it was being recognized as a very serious problem. And as Parkin (1979) said, by the time of the second book, the issue of rational expectations had become a crucial one for many. Nevertheless, considering the fanfare of a republication in Gordon (1974a) of a collection of articles already published in the JPE, the distinguished and diverse group of critics, Friedman's long response, and the point that it was supposed to be describing the theory underlying Friedman and Schwartz (1963a), and considering especially, how much Friedman might have hoped to make out of it, its lack of impact is really its most noticeable characteristic.

5.5 The Quantity Theory or the Income Expenditure Theory?

None of this can be said to have resolved much, except to the satisfaction of the participants, with each side claiming victory. One lesson flows from the assessment offered by Poole and Kornblith (1973, p. 916) of the controversy started by Friedman and Meiselman—'Our findings emphasize the futility of the R^2 game'. Indeed it could be a case study of econometric stand-off for its time.

That notwithstanding, what does come through is that in this period, Friedman made three attempts to bring empirical arguments to the question of whether the Quantity Theory or the income-expenditure theory offered greater practical insight. Those three attempts originated with Friedman and Becker (1957), Friedman and Meiselman (1963), and various versions of Friedman (1974a). In each case it was Friedman who set up a contest; and in each case, he failed to make progress. Perhaps that is as it should be—perhaps the data would not really reveal who was right. It is, though, a little difficult to escape the impression that Friedman's opponents were satisfied with stalemate and did not really feel they needed to win the argument. It is all very well, for example, for Tobin to prove that the timing evidence is not decisive, but one wonders whether he thought he was describing the way things are, or just a way that Friedman could not prove they were not. In any case, the initiative, the striving for progress, was from Friedman, even if he was unable to devise a test that would resolve the matter to the satisfaction of others.

6 Friedman's Accounts of Developments in Monetary Thought

One interesting aspect of Friedman's work is a story he liked to tell about the history of monetary thought. There is certainly a resemblance between this and his restatements of the message of the Quantity Theory, with those restatements having an historical aspect. But his accounts of the development of thinking on money more generally have so much similarity between them, and seem to have been conceived as something different from statements of the theory, that I treat them separately. Apart from the point that he kept on telling this story, and that suggests it had some sort of importance to him; it is notable that it was always free of detail, which suggests he had rather little interest in more than the most general outline. And, often as he told his story, it would be hard to say he refined the presentation. Conspicuous weaknesses of the first telling remained in later iterations, and indeed, new ones were introduced. It is a peculiar thing that he was so anxious to tell this story, and so careless about it.

There is a version of it, perhaps taken slightly more slowly than others in Friedman (1963a), but one slightly later, to which Friedman gave some extra standing by reprinting it in Friedman (1969c) is Friedman (1964/1969). There he discussed both monetary theory, and up to a point policy, though he did not distinguish them clearly. He said at the end of the War, most professional economists took it for granted that 'money did not matter, that it was a subject of minor importance' (p. 69) Since then, theoretical thought had moved back towards the presumptions of the Quantity Theory, modified by Keynesian insight, and in policy away from an emphasis on interest rates and credit market conditions, towards one on the quantity of money, and towards relating internal and external stability. He described the idea of 'absolute liquidity preference', suggested that it explained a belief in the instability of velocity, and implied that it was an important aspect of thinking of the time. He said that the general presumption at the end of the War was that depression was a threat, so that notwithstanding scepticism about the effectiveness of monetary policy, interest rates were kept low in order to achieve high levels of investment. Apparently to substantiate this, he quoted Goldenweiser (1945, p. 117) as saying,

> This country will have to adjust itself to a 2½ per cent interest rate as the return on safe, long-time money, because the time has come when returns on pioneering capital can no longer be unlimited as they were in the past.

Friedman's comment was that this view was shattered when inflation rose, and he went on to say that experience was reinforced by recognition of the Pigou effect by which real cash balances affect expenditure. He said of this idea, that 'The intellectual importance of the forces brought to the fore by Haberler and Pigou was the emphasis they placed on the possibility of substitution between cash on the one hand and real flows of expenditures on the other. This contributed to a re-emphasis on the role of money' (p. 72). Here, the point was that Keynes had considered only the question of substitution between money and bonds whereas the ideas of Haberler and Pigou reintroduced the idea of substitution between cash balances and goods. He said further that disillusion with fiscal policy also promoted monetary policy. Turning to developments in theory, he said that some who used Keynesian modes of thought, 'now say that liquidity preference is seldom absolute' (p. 72) and that changes in the quantity of money do affect income, though only indirectly via an effect on interest rates and investment. He called the question of how money affects income 'a purely semantic' (p. 73) one, which seems rather strange as it was a question at the heart of the theoretical difference, and that he immediately followed up by saying that postwar evidence spoke very strongly on the matter, so that there was a re-evaluation of the role of money.

He said a 'more fundamental and more basic' (p. 73) development came with the reformulation of the Quantity Theory of money (by himself, of course, though he did not say that), taking from Keynes the point that money was an asset. That idea, he said, had led in two directions—one towards studying 'liquidity' rather than money, which Friedman thought a dead end, and one towards an enquiry into the determination of the demand for money in real terms. This theory said that the key determinants were wealth and real rates of return on 'substitute forms of holding money', (p. 74) and this made inflation relevant. It also meant that changes in the quantity of money might affect a whole spectrum of asset prices before having a substantial effect on expenditures. One consequence was the likelihood of long lags between monetary developments and their final effects.

Friedman (1959e) had much the same story, there is some of it in Friedman (1960a). There is a bit of it in Friedman and Schwartz (1963b), and similar remarks appeared at various points in Friedman and Schwartz (1963a), along with a discussion of much more specifically American issues about the operations of the Federal Reserve. The idea that the Depression led to a wide presumption of the impotence of monetary policy, and that 'money does not matter'—a phrase which appears three times (pp. 300, 533, and 626)—are there. There is nothing at all to substantiate the claim on the first two occasions, although on the third, the same ineffective quotation from Goldenweiser (1945) is there in a footnote. It would not look as if it were even intended to substantiate the point except that it is so much clearer that is what Friedman intended in Friedman (1964/1969). In the discussion of the Accord,[2] the Pigou effect was introduced (p. 627) to say it caused the extreme Keynesian conclusions to be called into doubt and thereby reinforced the growing prestige of monetary policy—a claim supported by seven citations in a footnote. There was a very brisk statement of changing attitudes to monetary policy around that time, saying that interest in monetary policy was fostered by the failure of

[2]The Accord was the arrangement by which the pegging of American bond prices ended in March 1951, in circumstances described, for example, by Meltzer (2003, Chapter 7).

the 1948–1949 recession to become serious, the 'apparent' inability of the Federal Reserve to stop inflation whilst it was trying to peg bond prices, and then they said, 'The subsequent subsidence of the price rise and the failure of serious consequences to follow from termination of the support of government bond prices further enhanced the prestige of monetary policy and encouraged a continued shift toward assigning it a greater role' (p. 627).

Friedman (1964d, p. 7) started off with the observation that, 'To the theologian, money is the "root of all evil". To the economist, money had hardly less importance up to the early 1930s. It was then widely accepted that long-period changes in the quantity of money were the primary source of trends in the level of prices and that short-period fluctuations in the quantity of money played an important role in business cycles and might be the major explanation of them'. Then he said that the Depression changed views, and the work of Keynes provided an appealing alternative story, and that attitudes had gradually moved away from his because the predicted postwar recession did not occur, inflation became a problem, the real-balance effect was analysed, and because of evidence on the relationship between money and activity.

Friedman (1965b) dated the hiatus of interest in monetary policy more precisely, saying it lasted 'about twenty years, from about 1935 to about 1955, when there was very little discussion or work in this area' (p. 189) and that in this period it was generally believed that 'it really does not make much difference what happens to the stock of money' (p. 190), but that 'in the last five or ten years' there had been a counter-revolution (p. 190). Apart from that detail, the same basic line of thinking was presented in Friedman (1968a) and Friedman (1968b)—again, in the former 'money did not matter' (p. 2); there cheap money, and disillusion with it, leading to the Accord, were again part of the story. The revival of monetary policy was again fostered by the Pigou effect (although there were no citations this time). The re-evaluation of the effect of monetary policy in the Depression was there, and disillusion with fiscal policy was also mentioned, before the same Goldenweiser quotation was used, although here Friedman also stated that he had said that stabilizing the value of bonds was a policy goal. Friedman (1968a) also deployed various other quotations and summaries from those suggesting monetary policy was unimportant, though it was hardly a thorough treatment of the issue, and it was only Friedman's assertion that, 'These quotations suggest the flavor of professional thought some two decades ago' (p. 4). Essentially the same story was then told in Friedman (1970e), and it appeared again, to the same effect in Friedman (1972a), with the same components, and the addition of discussion of Friedman and Meiselman (1963). Much of that story was then repeated in Friedman (1973f, pp. 11–26), with some additional material on recent research. Even Friedman (1987a)—the second encyclopedia entry on the Quantity Theory—was in many respects rather similar.

The story, it might be said, at least if treated as a very superficial one, has nothing terribly offensive about it. And on occasion, it was written with some style, and certainly an air of authority—Friedman (1964/1969) scores highly on both of those counts. The air of authority perhaps serves to anaesthetize the critical faculties, but looking at the whole collection of these presentations, it does not take much to see that something is wrong. The general outline may indeed be an outline of one aspect of the matter. Generally speaking monetary policy did come to be thought less important after the Depression than it had been before and 1951 was something of a turning point. But those points are superficial—very superficial—and a long way from offering a reasonable picture of the development of monetary thought, which was their ostensible topic.

This is not the place for a rewrite, but it is possible to highlight some evident clues to the poverty of Friedman's treatment. For one thing, his failure to identify, much less digest, the relevant literature is easy to see—there are desperately few references to the work of others who were researching on money but were not Friedman's collaborators or students. Pesek and Saving (1967) went the same way as Patinkin (1956) in being unmentioned; Gurley and Shaw (1960) briefly appeared in Friedman (1964/1969), and Friedman (1967b, p. 4 n1) just managed an allusion to, 'A temporary flurry of interest in non-bank financial intermediaries a few years ago'. The disparagement is pretty clear, and that is all there is. It is a very long search to find any mention of the joint work of Brunner and Meltzer, or even either of them alone. As time went on, there would also be the question of whether there were any responses to the lines of enquiry Friedman described. They should have featured in Friedman (1987a), because it is late enough, and an encyclopedia entry. All he did was mention Hansen (1957) and Tobin (1961), as if they amounted to a summation of Keynesian thought twenty-five years or more years later. There was also work that sought to question his view of the monetary process. As time went on, the reader might have expected to see discussion of Tobin (1963), or Gramley and Chase (1965). If that sort of thing is too esoteric, there were studies of the demand for money that were very much in point: Bronfenbrenner and Mayer (1960), Laidler (1966), or the exchange between Meltzer (1963), Courchene and Shapiro (1964), and Meltzer (1964). There were very many other contributions to the literature, but for all Friedman's repetitions that interest in money had been revived, he reported staggeringly little of the fruits of that interest.

His appreciation of Keynesian economics was, as Ritter (1963) implied, deficient. The appearance and reappearance of the liquidity trap—or the idea of 'absolute liquidity preference' as Friedman preferred to call it—belies the point that it never had, and was hardly thought to have, practical consequences for policy. As Friedman noted, without being able to explain, it is in conflict with his view that policy-makers sought to keep interest rates low to maintain investment. The same goes for the various mentions of the Pigou effect, which of course recall its role in the early pages of Friedman (1957a). But nothing is said to establish it had any practical role on policy thinking, and it would be hard to find any. It does not, for example, seem to have featured in the debates about the Accord of 1951. The fact that Friedman and Schwartz (1963a, p. 627) has a string of citations about it adds nothing since they all concern theoretical questions, none of them policy history.

That string of citations does, though, highlight the dearth of sources for Friedman's claims elsewhere. The Goldenweiser quotation, on which he kept on relying does not even say what Friedman wants it to. It is not about the unimportance of money, but about the likelihood of real forces keeping interest rates low. Only in Friedman (1973f, p. 5) did he adjust his story to make Goldenweiser's errors merely those of poor prediction, whereas the previous implication was that they somehow showed the wrong theoretical attitude. In any case, although

Goldenweiser was a senior figure, the source was a speech to agriculturalists, not a major statement on monetary policy.

Friedman's disinclination, or inability, to find a more suitable authority is an interesting aspect of the whole matter. The impression that he had little idea where to look is reinforced by the point that a draft of Friedman (1968a), evidently intended for circulation and comment, is marked with a request for readers to supply quotations to make Friedman's point that monetary issues were disregarded (Hoover Institution Archive—Friedman Collection, box 49.11, p. 4—'Vintage examples especially appreciated', he wrote). In that piece, the pudding of the perceived unimportance of money was egged with the suggestion that,

> If you wish to go further in this humbling inquiry, I recommend that you compare the sections on money – when you can find them – in the Principles texts of the early postwar years with the lengthy sections in the current crop even, or especially, when the early and recent Principles are different editions of the same work. (p. 4)

But there is no difficulty finding Samuelson (1948) Chapter 13 called 'Prices, money, and interest rates'; and Chapter 15, 'Federal Reserve and central bank monetary policy'. It is true that Samuelson expressed scepticism about the power of monetary policy and even came close to suggesting the possibility of a liquidity trap, though that related to circumstances 'in the middle of a deep depression' (p. 353). A principal point he made, though, was that in such circumstances open market operations increasing bank reserves by reducing their holding of bonds might be ineffective as there might be a lack of safe loans for the banks to make. That is an aspect of a (near-) liquidity trap of which Friedman's accounts gave no indication. Samuelson further argued that reducing interest rates on government debt would not necessarily reduce them on riskier assets if it appeared the rate on government debt would not stay low. In that case, effective lending rates would not fall. He did also say that questionnaire evidence pointed to doubt about the interest elasticity of demand for loans, but that point was again specifically linked to the circumstances of depression. On the following page, on the other hand, he noted that most thought monetary policy would be more powerful in preventing a boom and the conclusion he reached was that monetary and fiscal policy should be used together. Samuelson (1951a, pp. 372–373) had much the same discussion, as did Samuelson (1955, pp. 316–317), although without the discussion of the support of bond prices, which had ended. Those editions also considered the Quantity Theory specifically, along with its 'inadequacies' as Samuelson put it, finding them mainly in the variability of velocity. That, though is not to say that in the earlier editions money was treated as unimportant. If anything, it is in Samuelson (1958), with the monetarist counter-revolution supposedly just starting, where there is rather less said on the question of the power of monetary policy (although the inadequate Quantity Theory is still there). In any case, the discussion is, according to Friedman's clear implication, not supposed to be there at all. And yet it is.

Friedman's main point, it might be argued, was not about the content of textbooks, but about the state of thinking generally. But the same problem arises. One cannot, for example see a case that it was thought that 'money does not matter' from considering the symposium edited by Harris (1948)—Ten economists on inflation—in which Slichter (1948), Mills (1948), Haberler (1948), and Machlup (1948) all gave prominence to monetary considerations. So did Boulding (1948, p. 15), who said, 'There is not much likelihood of serious disagreement among economists about the causes of the present rise in the price-wage level… The inflation is primarily a result of the rise in total stocks of money and other liquid assets'. And the period in question was also the period of Mints (1950)—a Chicago book, as well as a Quantity Theory one—and Shaw (1950) either of which Friedman might have been expected to mention. Or he could have considered other writings even of Goldenweiser, and would have found Goldenweiser (1951, p. 6), the first paragraph of which said,

> Money occupies a strategic place in a modern economy. While there is controversy about the extent of its influence, one thing is certain: the volume of money is not an indifferent but a vital factor in the economy. It is the business of monetary management to increase its supply and make

> it more easily available and cheaper when there is evidence of weakness in the economy, and to reduce its volume and make it less easily available and dearer when indications show that there is excessive expansion.

The book is not a great work, and is mainly descriptive, but clearly not based on the preconception that money is unimportant. Indeed, in discussing the Depression, Goldenweiser said that monetary developments were certainly part of the reason for it (p. 157) and he recognized that subsequent monetary expansion was inadequate (p. 159). And then there is the testimony of earlier remarks by Friedman himself, such as Friedman (1951g, p. 188) where, advocating the aggressive use of monetary policy to control inflation, he said that 'practically everyone' would agree that 'the Federal Reserve even today could impose a drastic deflation on the country'.

Another view of it comes from comparing Friedman's story with others from the same sort of time. Brunner (1970) was also an account of the 'monetarist revolution'. So was Johnson (1971a), although that paper is not greatly to be commended either, but Johnson (1962) is, and that has more insight about the monetarist counter-revolution that Friedman's historical stories. The comparison of the accounts of these authors is considered more in Forder (2018c), but one of the things that makes Friedman's discussions so interesting is just that he kept on with the same general line. With the exception of the additional material brought into Friedman (1972a) and then sometimes retained in later versions, he hardly sought to improve it, nor to keep it up to date, but he did keep on telling it. In a sense the explanation is that they are all stories of what Friedman thought about the issues, and how he and his collaborators treated them. They have no true standing as treatments of the history of thought. On the other hand, that also raises the point that, again with the partial exception of the discussion in Friedman (1972a), he does not tend to push his own name to the forefront. The presentation is much more impersonal—people started to be more interested in the Quantity Theory; new work on the Depression changed the picture of it; etc. It is always Friedman's work, and surely he was supposing that his readers, or many of them anyway, would appreciate that, but it is a curious fact that he does not say so.

15

Stabilization Policy and the Causes of Inflation

1 Rules and Discretion Continued

As noted above (p. 116), a presumption in favour of 'rules' rather than policymaker discretion was an essential of Friedman (1948a), but there he made very little argument specifically on that point. Rather, he had focussed on describing the particular characteristics of the arrangements he proposed. Nevertheless, he did mention the 'uncertainty and undesirable political implications of discretionary action by governmental authorities' (p. 263) and stated amongst his aims the securing of 'political freedom, economic efficiency, and substantial equality of economic power' (p. 246). These points, along with his optimism that his proposal would deliver reasonable results suggest he saw it in part as a response to Simons (1936). That backward-looking and disaffected essay had presented the problem of achieving stabilization without permitting discretionary policymaking to play a significant role as a very difficult one, with the essay being full of discontent with its own proposals. Friedman revealed much less about the moral or philosophical basis for objecting to discretion, but his insistence on the nearly automatic operation of his plan along with the brief remarks on his desire to avoid

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_15

discretion suggest seeing the paper as written with Simons' philosophical outlook in mind, but Friedman's view of the prospects was much more optimistic.

In Friedman (1951g) he reiterated his support for 100% reserves and said that establishing the Federal Reserve was a mistake and in Friedman (1954/1968, p. 96), also seems to have meant to reaffirm the ideas of Friedman (1948a), at least in general terms. He addressed a similar set of questions again in Friedman (1960a)—*A Program for Monetary Stability*, which was a book produced from his Moorhouse I X Millar lectures. That was more of a collection of ideas, and less of a single cohesive proposal than the 1948 piece, but took the argument for several of his ideas more slowly. It also shows the influence of ideas arising from the work towards Friedman and Schwartz (1963a). Most obviously he said that since Friedman (1948a) his further study had persuaded him that a simple money growth rule, rather than the more complex proposal of the earlier piece would deliver satisfactory results. Here, the idea that discretionary policy is destabilizing, which had been little more than a conjecture in Friedman (1948a) was presented as a lesson of experience and the conclusion was drawn that 'the central problem is not to construct a highly sensitive instrument that can continuously offset instability introduced by other factors, but rather to prevent monetary arrangements from themselves becoming a primary source of instability' (p. 23).

He discussed at some length the tools available to the Federal Reserve arguing that those by which credit was allocated to specific purposes should be abolished, since where, if anywhere, such policy was called for, its goals were better achieved by other agencies. He thought that the prohibition of the payment of interest on demand deposits should cease as it was an instance of price control, being ostensibly justified on grounds for which no proper rationale could be given—namely that if they had to pay interest, banks would be driven to riskier investment policy to increase their earnings. The prohibition of interest, said Friedman, could hardly lead them to forgo profitable opportunities, and like Tobin (1960) he thought it better to have banks attract deposits by paying interest than through free gifts of various sorts to depositors. In Friedman's account, the Federal Reserve was no longer needed for its original purpose of preventing widespread banking panics by providing emergency funds since deposit insurance had put an end to such panics. On the other hand rediscounting offered only a poor way of conducting monetary policy: it was hard to predict what change in the money supply would result from any change in the discount rate; changes in the discount rate were invariably discontinuous and so became a source of instability; led to misinterpretations of policy intentions when the rate was changed; and promoted confusion as to the stance of monetary policy by focussing attention on an interest rate rather than the quantity of money. Similarly, variable reserve requirements introduced discontinuities in policy and caused confusion.

These proposals left open market operations as the only tool of monetary policy, but Friedman welcomed that, noting that they could be used with any degree of precision required, and without public announcement, so that no effects arose as a result of announcements themselves; and they operated impersonally and with effects diffused over the whole banking system, amongst other advantages.

He again made the case for 100% reserve banking, this time basing it substantially on the point that changes in preferences as between the holding of currency and deposits were otherwise destabilizing. In the final chapter, Friedman moved to consider 'The goals and criteria of monetary policy'. He first considered international aspects, offering an historical account of the American position, and arguing for an end to the fixed price of gold—a price support scheme, comparable to similar schemes concerning agricultural products, as he regarded it—and just a quick mention of the case for flexible exchange rates, along with a citation of Friedman (1953d).

Moving to internal policy he began by citing Simons' (1936) 'celebrated article on "Rules versus Authorities in Monetary Policy"', for the 'marked and important' (p. 84), if not complete, contrast between rules-based and discretionary policymaking. He said that policy had in practice been conducted by discretion and this was 'highly objectionable on political grounds in a free society' (p. 85), and exposed the authorities to various extraneous pressures and temptations arising from short-termism.

As far as an argument explicitly on the question of the principle of 'authorities' was concerned, that was all he had to say, and it is a pity that there was not more about what those political grounds were. He went on to say that discretionary policy had in fact turned out poorly, and this he said, meant that the urgent need was to provide clearer guidelines for policy and criteria by which to judge it. On the question of the choice of rule, though, a concern with the regulation of the policymaker is again visible. Considering setting the goal of price level stability, he said there was substantial merit in the idea, emphasizing the point that substantial variations in the price level never occurred without similar variations in the quantity of money. But he doubted the desirability of adopting such a target because,

> the link between price changes and monetary changes over short periods is too loose and too imperfectly known to make price level stability an objective and reasonably unambiguous guide to policy. (p. 87)

For this reason he believed that the 'ultimate end of achieving a reasonably stable price level' would be better achieved by 'specifying the role of the monetary authorities in terms of magnitudes they effectively control and for whose behaviour they can properly be held responsible' (p. 88). There is clearly an aspect to that which concerns the principle of delegating discretionary authority, though it is again heavily overlain with pragmatism. The presumption clearly is that policymakers do not know well enough how to control inflation itself to be allowed to try. That aspect is about achieving the goal, not any question of the rights and wrongs of delegation.

In making this case, he emphasized that the link between money and prices related to the average outcome, but that there was variation over short periods. That, he noted, would not matter if the relationship were sufficiently predictable, but he further said that it was not. In particular, he said that the time between troughs in the rate of change of money and of the business cycle had varied between four and 22 months, and for peaks between six and 29. That made it impossible to devise effective policy and, he implied, raised the possibility of attempts to stabilize the price level actually resulting in destabilizing policy on a significant number of occasions. He argued (pp. 93ff.) that although policy had sought to be stabilizing, it was in fact seriously destabilizing in the interwar period and that a rule would have avoided large mistakes. Interestingly, in relation to the postwar period he was able to come to no such firm conclusion, since he accepted there had been no large mistakes, but could not specify satisfactory criteria for judging the quality of policy more precisely. Nevertheless, in so far as he devised criteria, he felt policy had pushed in the correct direction somewhat less than half the time.

The importance of that point in his thinking needs to be seen in the light of the fact that since 1948 he had also written Friedman (1951/1953)—originally published in French, and then republished in translation in Friedman (1953c). It made an important and neglected point about the stabilizing potential of policy. That is that as a matter of statistics, the variance of outcomes in, say, national income, will be equal to the variance of what would happen without policy intervention, plus the variance due to policy, plus twice their covariance. That third term means that in order to be stabilizing overall, policy needs to push in the correct direction much more than half the time, as well as not being too powerful. The point is further discussed in Forder and Monnery (2019), where amongst other things, it is pointed out that this paper seems to have been referenced when Friedman was awarded his Noble Prize. The key aspect of its significance in Friedman's thinking is precisely that on his own reckoning, as of the end of the 1950s or so, policy was not achieving even that standard.

Consequently, although his finding about actual policy was uncertain, the idea that it pushed in the right direction only about half the time described a serious failing. It needed to be *much* better timed in order to have a desirable effect on balance. In this, of course, the question of the length and predictability of lags between monetary actions and their effects is an essential. The idea that monetary policy lags are 'long and variable' became something of a catch-phrase for Friedman. As noted above, issue was analysed in the abstract in Friedman (1947c, 1948a), and some of its important if neglected consequences drawn out in Friedman (1953a). In Friedman and Schwartz (1963a) there was perhaps more attention on lags in policymaker decision-taking than in the effects of policy, but the lag-relationship between money and the business cycle was discussed in Friedman (1958a, 1959a) before having its fullest treatment in Friedman and Schwartz (1963b). In due course, the matter became a constant feature of Friedman's discussions of policy, notably in *Newsweek*, where it was often presented as a reason for uncertainty in forecasting outcomes.

It was the 1959 version that provoked Culbertson (1960), bringing forth Friedman (1961b), and then Culbertson (1961) in another rather bad-tempered exchange. Culbertson ended up saying that whereas be was being accused of casual empiricism, that was much less dangerous that Friedman's pretence at scientific discovery. It is an important point though that if it is accepted that monetary policy matters, in terms of making the case for rules, limitations in Friedman's ability to determine how long are the lags do very little damage to his position. Uncertainty about that, particularly in the light of Friedman (1951/1953) argues very much for his position. Culbertson needed to devise accurate and reliable estimates and he was far from doing that. Kareken and Solow (1963) on the other hand, noted that if monetary policy were perfectly successful in controlling business cycles, one would presumably observe a highly volatile monetary series and a stable business one, but any inference that monetary policy had no effect would be fallacious. In the same way, they said, without knowledge of what counterfactual outcomes there might have been, the relationship of money and business cycles of the kind presented by Friedman, could show nothing. They also questioned the selection of turning points in the rate of change of monetary growth as the crucial indicator—in itself that would generate an apparent lead of monetary change over output change (p. 17). Passing over that question, they investigated the results of comparing the change in money with the change, rather than the level of output. On this basis, they came to the conclusion that the two were about simultaneous. Friedman did not comment on all their arguments but did, in Friedman (1964a), respond to some, arguing that their econometrics could not prove their point.

In any case, Friedman (1960a) concluded that a policy rule should be specified in terms of some variable more directly under the control of the

Federal Reserve than the price level, speculating that this would even be more effective in achieving price level stability. The quantity of money was naturally the variable he suggested, but that raised the question of what target should be set. Here he referred to Friedman (1948a), and described the 'built-in flexibility' of his proposal there. However, he went on to say that his subsequent research had led him to believe that whilst that proposal would work well, the simpler proposal of a constant rate of money growth would work as well whilst having the advantages of facilitating public understanding and requiring less far-reaching reform.

Addressing the question of which measure of the quantity of money to target, he said it was not a matter of great importance. What mattered was that it were 'at least as broad as currency plus adjusted demand deposits' (p. 91) and the definite rate of growth selected be chosen to reflect the long-term behaviour of the measure of money in question.

The proposal for a constant rate of growth of the money supply was, of course, one for which Friedman would become famous, and one which he very much made his own. But he was by no means unique in proposing it, nor in the general shape of his reasons for proposing it. As previously noted, Clark Warburton had advocated it. So had Shaw (1958, pp. 66–68) had been a prominent advocate of it. Before that, Edie (1931) and then Snyder (1935, p. 198) and Angell (1936, p. 163) supported the same sort of plan. Bronfenbrenner (1961, p. 3) even labelled the idea of constant money growth the 'Friedman–Shaw proposals', saying that this terminology was 'sometimes' used, also noting Warburton had taken a similar line. Angell (1960) made a case for rules which acknowledged exceptional cases where discretion would be appropriate. It is interesting that none of those works was mentioned by Friedman and Schwartz (1963a). The nearest those authors came to any mention was to say that Snyder (1924) 'implies the desirability' of such a policy (p. 252 n16). But of the others, or the later work of Snyder, there is nothing.

A Program for Monetary Stability is not nearly so intense, nor so single-minded as Friedman (1957a), but was received—quite rightly, and just like so much of Friedman's work—as one of clarity, lucidity, and clever persuasiveness. Certainly, it had detractors—Ritter (1960) criticized it as unpersuasive in its monetary analysis and for ignoring the effects of technological change and varying investment opportunities in the business cycle. On both points, his case certainly has its merits when this book is considered alone, but Friedman surely had in his mind the fuller analysis that would appear in Friedman and Schwartz (1963a). Ritter also thought that too little attention had been paid to the policy of the Federal Reserve after 1951, although this had been, he thought, the only period where well-informed policymakers were in a position to adopt an effective discretionary policy. Friedman's case against discretionary policy could not be made, unless it was shown to be ineffective in that period. Friedman would perhaps accept that policy was more effective in the first half of the 1950s than at other times, but in later work—particularly Friedman (1972a)—he once again argued its ineffectiveness in the 1960s.

Lerner (1962) was another thoughtful review, and he made a number of interesting criticisms. One concerned the view that the record showed that discretionary policy had been poor. He considered Friedman's instances and argued that for the most part, they were clearly mistakes when the policy was adopted, or that in retrospect enough had been learned that such policy would not be repeated. Rather like Ritter, then, expressing the view that future policy would therefore be better, he rejected Friedman's case for rules.

Another was the point that the paying of interest on deposits might change the character of the demand for money and make it more variable. Selden (1962, p. 345) thought that likely to promote the stability of velocity. That was because when interest is not paid, and a cyclical downturn brings down interest rates on other assets, the cost of holding deposit money falls, and hence velocity falls. But with deposit interest paid, this effect would be mitigated. Lerner, though, took the opposite view, arguing that since interest bearing money is a closer substitute for other investments than is non-interest bearing money, changes in investor sentiment would bring larger changes in the demand for money.

That little dispute is instructive in a couple of ways. Lerner, it might be said, exhibited the Keynesians' concern with the instability in the demand for money arising from changing expectations about other assets, and thinking down those lines saw danger in the proposal. Selden, thinking in terms of the general stability of velocity and of smooth adjustments, saw the proposal bringing an improvement. The second point, though is that Lerner's concern that there might be unanticipated and unwanted consequences of such reform proposals highlights a contrast. Evidently, Friedman's scepticism that policymakers could ever be brought to sufficient understanding to permit effective macroeconomic management was not matched by any doubts about the radical redesign of systems of government—the policymaker cannot learn to make policy, but the professor's office is a fine location for the design of new institutions.

More fundamentally, Lerner also, though, rejected the view that the basic policy problem was to stabilize the business cycle. Of this, he said that what it amounted to was seeking to stabilize the unemployment rate at the level consistent with price stability. But that level was, according to Lerner, too high and not a reasonable measure of 'full employment'. In effect, though not in the language he used, this introduced the idea of 'cost-push' inflation—there were sources of inflation which other than excess demand. Consequently if demand policy were used to control inflation, there would have to be deficient demand and hence excess unemployment. In Lerner's view, other measures were required to control wages, so that full employment could be achieved.

In later work, Friedman addressed the rules and discretion several more times. Once was in Friedman (1962c) which was framed around the question of whether there should be an independent central bank, but much of the substance again concerned the case for a policy rule. Independent central banking was, thought Friedman, entirely undesirable. Having briefly noted in Friedman (1960a, p. 85) that the case for independence (or 'alleged independence' as he actually said) tended to be made in terms of providing an environment of monetary stability, in Friedman (1962c) he said that the political objections were more apparent than the economic, and he asked 'Is it really tolerable in a democracy to have so much power concentrated in a body free from any kind of direct, effective political control? What I have called the "new liberal" often characterizes his position as involving belief in the rule of law rather than of men. It is hard to reconcile such a view with the approval of an independent central bank in any meaningful way' (p. 227). He went on to describe what he had learned of the attitudes of

Emile Moreau, Hjalmar Schacht, and Montague Norman. Of the last, Friedman said, his 'implicit doctrine is clearly thoroughly dictatorial and totalitarian' (p. 229).

Friedman then turned to the economic disadvantages of independent central banking. Again, his attitude apparently arose from a small number of rather specific observations. One was that to make a reality of independence, it would be necessary to concentrate more powers with the central bank than the Federal Reserve had enjoyed. That meant taking aspects of debt management from the Treasury. That, Friedman even said might make for efficiency, but then appealed back to the political argument, saying it would be an unacceptable concentration of power for an independent agency. Secondly, he said that independence resulted in a dependence of policy actions on personalities, and then complained about William Harding's lack of understanding of monetary principles whilst he was Chairman of the Federal Reserve, and that he had 'even less backbone' (p. 234), before praising Strong and disparaging those who held power in the System after he died. And the third 'technical defect' (p. 236) as he called it, of independent central banking, was the tendency to give too much weight to the views of bankers, with again, his argument on the importance of the point coming in the form of specific claims that in the United States the Federal Reserve banks were 'technically' owned by the member banks so that 'One result is that the general views of the banking community exercise a strong influence on the central bank' (p. 238) with the result that too much emphasis was put on interest rates rather than the money supply.

He then said these three points 'constitute a strong technical argument against an independent central bank' (p. 238). That is a remarkable claim—actually they are a collection of particular points mainly about American arrangements, which could be changed if they were thought important, and have no general applicability to the question of central bank independence, along with a little collection of anecdotes. The fact that Friedman apparently could not see how insubstantial this argument was is itself notable. His attempt to make a case in favour of a rule was, however, less successful still. He asserted, in Friedman (1962a)—*Capitalism and Freedom*—the same year, that a general rule in favour of free speech was needed because votes on whether advocates of particular views should be allowed to speak would see many refused, but there was nevertheless wide agreement on the principle. That is not much of an argument, but he followed up saying, 'Exactly the same, considerations apply in the monetary area' (1962c, p. 241), which is clearly not supportable, and is rather naïve. Far from exactly the same, it is not even quite clear what the analogy is meant to be.

In Friedman and Schwartz (1963a), as in NBER publications generally, the authors steered away from actually putting policy proposals, but the point that a simple rule could have avoided serious mistakes certainly came through. Friedman (1965b) stuck to the view that the Federal Reserve had conducted policy poorly, and emphasized the importance of designing systems to avoid that outcome, but hardly engaged with the idea of delegated authority.

The same lines of thinking were then evident in Friedman (1967b), which was a consideration of the monetary views of Henry Simons, and in particular Simons (1936). He had much more developed views that Friedman on the philosophical problems of monetary policy, thought they are more apparent in Simons (1933/1994) and Simons (1948a) than in Simons (1936). But still, Friedman approved Simons' outlook and the motivation leading him to support a price level targeting rule. But Friedman sought to argue that had Simons been aware of the research on the question after he died (i.e. Friedman's research, although he was apparently coy about that again), he would have favoured a money growth rule. In Simons' view, the 'financial good society' required various sorts of reform, including 100% reserve banking but, said Friedman, Simons' 'final position was, roughly, that the price-index rule was the only feasible rule pending a closer approximation to the 'financial good society,' but that the quantity of money rule was much preferable, when and if the 'financial good society' was attained' (p. 3). But later research, thought Friedman, made the case for the monetary rule in the current circumstances.

The case for rules then appeared again in Friedman (1967/1968) in which he repeated many of his views about independent central banking from Friedman (1962c), combining them with arguments from Friedman (1953d) on the benefits of floating exchange rates and Friedman (1961e) on the difference between real and pseudo gold standards. It is a little-noticed piece, perhaps because it was originally in French, though it was reprinted in Friedman (1968d) and is notable for Friedman's declaration, following Harold Wilson's colourful language, of sympathy with the supporters of the British Labour Party who objected to national policy being determined by 'the gnomes of Zurich' (p. 274).

Making the case for a rule was also the objective of Friedman (1968a), though it is frequently said to be about the Phillips curve, and was certainly one important theme of Friedman (1972b), though that seems to be rarely read at all. Those later treatments continue very much to emphasize the practical benefits of rules. On the other hand, after Friedman (1948a) he gave very much less attention to the point that policymakers might be confronted with a dilemma in the form of a choice between generating inflation and allowing a painful period of adjustment with unemployment. In the earlier piece, he had said that price flexibility was a prerequisite for rational policy. That had certainly not been achieved, but he said no more about it. And whilst he incorporated new data, in the form of pointing to what he took to be policy mistakes in the 1950s and 1960s to add extra illustrations to his case, he did not show an inclination to add to or modify his argument at all. Even when challenges to his view that emerged, he made no response. Modigliani (1964) criticized Friedman's proposal, and albeit that in Modigliani (1977, p. 13) he called it 'one of my worst papers', it was prominent enough that Friedman should not have ignored it in later discussions. And there was also the point that the trend in velocity had changed direction after the War, so as Bronfenbrenner (1961) also found a money growth rule based on the earlier data would be expected to perform very poorly. It might be said that the rule could be changed when it became apparent it was failing, but then one must also remember that as late as 1963, Friedman and Schwartz were expressing confidence that the trend would be reversed. It is a notable problem, and Friedman surely should have faced it. But after his switch to a money growth rule in preference to the more complicated arrangements of Friedman (1948a), he seems to have been happy to leave the essential argument just as it was, and ignore other developments.

2 The Optimum Quantity of Money

Although in these discussions of the case for rules rather than discretion, Friedman advocated a money growth rule aiming at price stability, or possibly a low rate of inflation, in Friedman (1969a) he made a case for a falling price level. That was the title essay, and only new work in Friedman (1969c), a collection of essays published between 1954 and 1968, which was something of a sequel to Friedman (1953c), though in this one, all the papers were about money and the volume was described on its jacket as comprehensive account of Friedman's views on that subject.

Friedman (1969a) is famous for the idea of 'helicopter money'—an increase in the money supply achieved by dropping notes from helicopters. The point was that the money supply was increased without any other financial transaction—such as open market operations, or bank lending—occurring. The central idea of the paper, though, was that since the cost of producing money is about zero, efficiency requires that the marginal benefit of money balances also be zero. That was not ordinarily achieved because holding liquidity tended to come at the expense of forgone interest. In Friedman's idea, that problem would be solved if prices were to fall at a rate equal to the real interest rate. In that case, a nominal interest rate of zero would offer an appropriate return. So the change in the money supply should be set to achieve a price level that fell at the real rate of interest (and the title of the essay should have been 'The optimumal rate of change of the quantity of money', but that might have lacked the appeal of the actual one).

Friedman's plan was rather fiercely criticized by Tsiang (1969) and Stein (1970) for leading to instability in the event of certain disturbances, and as Johnson (1971c) said, the objective of the proposal could be achieved by paying interest on money. Friedman noted that, and said his analysis pointed to the desirability of abolishing prohibitions on the paying of interest on demand deposits, which he thought fully justified on other grounds. The direction of his thinking though led elsewhere, towards the much more theoretical point about the character of the optimum arrangements. The essay is a little bit unusual in Friedman's writing, being a long and concentrated theoretical enquiry arising in first principles of monetary thinking, very carefully developed. It is esoteric, but a rather beautifully executed essay, and a fine paper to lead off the collection. It is notable that despite the theoretical tone of the paper, he does seem to have taken seriously the idea that policy might arrange for a falling price level, saying that at least that it would be worthwhile to investigate further how great the benefits might be.

Friedman nonchalantly acknowledged the inconsistency between this proposal and his earlier arguments for rules-based policy saying that he had not at the time formulated all the ideas, and also emphasizing the point that over a wide range, steady money growth was much more important than the precise rate chosen. Indeed, his view changed as he thought about a different aspect of the problem.

He also briefly commented on the difference in motivation between his exposition and that of many other discussions of optimal monetary policy, saying they were often concerned with the possibility of tradeoffs which, in his view, only arose because inflation was unanticipated. His analysis, on the other hand, concerned only anticipated changes in prices, and hence there were no tradeoffs. Those tradeoffs would, however, need to be taken into consideration in forming actual policy, and this suggested to him that a practical compromise might be to set policy so as to stabilize factor payments (allowing goods prices to fall as productivity rose).

3 The Causes of Excess Growth in the Quantity of Money

An issue on which Friedman touched several times was that of what it was that, in practice, caused the money supply to expand at such a rate as to cause inflation. The general idea that a commitment to full employment had a tendency to push in the direction of expansion recurred many times—in Friedman (1954/1963, 1958f)—for example. He considered various ways in which discretionary policy might end up being over-expansionary in Friedman (1960a, pp. 95ff.). He also sometimes hinted that in difficult situation such as wartime, inflationary finance might be more or less unavoidable—Friedman (1963/1968).

But there are also certain ideas about the practical causes of inflation that he pursued that have special interest in understanding the development of his ideas—both for themselves, and for the responses to them he offered. Some show consistent lines in his thinking over long periods of time; some appear only at certain times, either only temporarily, or then becoming a permanent feature of his work—and that too, of course, suggests some reflections.

3.1 Always and Everywhere a Monetary Phenomenon—Cost-Push Inflation

The great debate over macroeconomic policy matters in the 1950s and 1960s, and continuing in the 1970s was not, despite the implication of Friedman's presentations, about the importance specifically of money. Rather, it was about the cause of inflation and whether it should be attributed to excess demand—whether caused by monetary policy or otherwise—or to 'cost-push' factors. Theoretical ideas about cost-push inflation came in many forms that were rarely articulated with much clarity. Perhaps for that reason, the sense of the idea has often eluded the authors of later studies of the period, as well as leaving those who rejected the idea, such as Schuettinger (1978) and Humphrey (1998) well placed to disparage it. Forder (2019) is in part an attempt to recover the central idea and some of its varieties. That central idea was that either firms or unions with monopoly power could raise certain wages or prices and that this might lead to inflation. The point made for example by Friedman (1958f) and Friedman and Schwartz (1963a), that only increasing market power could allow monopolists to raise prices is not the full response sometimes supposed. If it is possible for costs or prices to be raised once, and the result is a general inflation which restores the original relative prices, then the same degree of monopoly power allows them to be raised again. Since it was supposed this could occur when demand was not excessive, it followed that the reduction of demand might be an ineffective remedy. Consequently, as explained by Backhouse and Forder (2013) and Forder (2014, pp. 111–116) other remedies were suggested—very often those in the broad family of 'incomes policy'.

From the point of view of the Quantity Theory, though, there is clearly a question as to what is to be said about the money supply. Some proponents of cost-push theory ignored that, possibly supposing the banking sector created deposits sufficiently freely that inflation could be self-sustaining. Machlup (1960) actually suggested that view. Even if that did not allow inflation to continue indefinitely, it might allow it to go on long enough to be a policy concern. Others took the point and simply treated the idea of cost-push inflation as leading to a policymaker dilemma in that either an increase in the quantity of money, or unemployment would have to be accepted. In either case, there would be a 'cost-push problem' although not necessarily leading to inflation. If the policymaker did respond by causing or allowing an increase in nominal demand, then of course there was inflation, and there would not necessarily be anything to which the monetarist would object—it would simply be that cost-push forces provided the explanation of why the increase in the money supply occurred. On the other hand, the alternative of 'demand-pull' inflation was certainly accepted as a possibility by the non-monetarist, who might then see the origin of excess demand other than in excessive expansion of the money supply. Consequently, in the case of any particular inflation, it would be possible for there to be debate as to which 'type' it was.

Although Friedman occasionally acknowledged the possibility of cost-push inflation arising by unions raising wages and governments responding by increasing the money supply (e.g. Friedman 1951a, p. 227; 1963/1968, p. 29), he tended to place much more emphasis on denying that it was ever more than exceptionally a practical problem. The background to that should perhaps be found in Friedman (1951a, 1955b) where he had doubted that unions had much power to raise wages at all and suggested that if they did, it would be in the union sector, and wages elsewhere would have to fall. Nevertheless, in the second half of the 1950s the question of diagnosing the inflation of the middle of that decade attracted a great deal of attention, including most notably that of Charles Schultze (1959). Of others, though, Friedman's student Selden (1959) argued that it was demand inflation, whilst Morton (1959) thought it was cost-push, that should be met with a firm line preventing the expansion of nominal demand so as to discipline price setters.

Commenting on that paper, Friedman (1959a) said that the price rise of the mid-1950s should not even be called 'inflation' since that term should be reserved for price increases that a large or long-lasting. The one that had occurred, he attributed to an over-reaction of the Federal Reserve to the mild recession of 1953–1954, and he expressed concern that in an environment where such importance attached to maintaining full employment, and with such poorly understood lags in policy effect, such mistakes were likely to recur. That idea obviously coheres with Friedman's advocacy of rules but it is notable that in addition to criticizing Morton for having asserted the occurrence of cost-push inflation without providing any evidence and seeming to treat it as a matter of faith, Friedman himself saw no need to do more than assert his idea, along with an affirmation of its plausibility.

As time went on, the idea of incomes policy as a response to cost-push inflation gained ground, as Friedman (1958f) had anticipated it would, and he was opposed to that as well. That opposition was in part an aspect of his general aversion to price control and in part a natural consequence of his rejection of the view that unions had much effect in raising wages, or that they could do so without an increase in their monopoly power. The best-known statement of his view came in a debate with Solow comprised of Friedman (1966b, c) and Solow (1966a, b). The second of Friedman's contributions is sometimes noted as (supposedly) his first statement of the expectations argument concerning the relationship of inflation and unemployment; and for being the place he first used the word 'natural' to describe the long-run equilibrium level of unemployment—the level prevailing with inflation-expectations adjusted. In fact, except for the terminology of the 'natural' rate, the argument around those points appears in just the same way in Friedman (1963/1968). On the question of cost-push inflation, though, in both cases his conclusions rested on the points that the demand for money is quite stable, and that in practical terms, inflation unaccompanied by an increase in the quantity of money never occurs so that, as he put it in both of them 'inflation is always and everywhere a monetary phenomenon' (1963/1968, p. 39, 1966c, p. 25). That remark became another catch-phrase for Friedman and he was sometimes ridiculed for it, as for example by Patinkin (1981c, p. 31) who, missing the point, said, 'by the same token one can say that the price of potatoes is everywhere and at all times a potato phenomenon.'

The point being made, though, was about what did not cause inflation. It was a summary statement of his rejection of the idea of cost-push inflation. It was that inflation was not a phenomenon of economic development, or of unionization, or monopoly power. It was always a matter of money. One may or may not agree with him but the point, when one stops to understand it, is clear, and rather well put.

3.2 The Interests of the Federal Reserve

The denial of the relevance of the idea of cost-push inflation clearly left a question as to what Friedman did say was the practical cause of inflation—the practical cause of increases in the money supply. And to this he had various answers. One that he advanced intermittently over a long period was that central bankers, or the Federal Reserve in particular, could be expected to act according to the incentives they faced. His general cynicism about central bankers, made explicit in Friedman (1962c), but also evident elsewhere—Friedman and Schwartz (1963a, p. 250), for example.

In slightly varying ways he also used this thought to explain the occurrence of inflation, amongst other things. As already noted, in Friedman (1962c) one of Friedman's arguments was that independent central banks would very probably pay too much attention to the point of views of bankers, and hence, he said, to too much focus on interest rates rather than the money supply, and that this had been shown to be ineffective. It was clearly the same general sort of thinking that lay behind Friedman (1975b)—a *Newsweek* article where he commended a Congressional Resolution for having required the Federal Reserve to publish money supply targets, and Arthur Burns for having accepted their importance. The Resolution would, thought Friedman, improve on the past practice where the Federal Reserve had been able to avoid scrutiny by, amongst other things, adopting different targets at different times. Friedman (1977f) introduced a small variation, saying that the problem was the combination of nominal independence of the Federal Reserve without its being effective, allowed Congress to overspend, whilst the Fed provided the finance, and no one was held accountable.

Friedman (1982a) began as a lecture and in it he presented a litany of Federal Reserve failures, and explained them as the result of the System seeking to retain members. In the published paper Friedman referred to a draft of what became Toma (1982)—an exploration of the capability of the 'theory of bureaucracy' in explaining inflation-bias in Federal Reserve behaviour—and mentioned Chant and Acheson (1972), Chant and Acheson (1973), and Acheson and Chant (1973), saying he had become aware of them since giving the lecture. These in fact were three seminal papers in arguing that central banks could be well-understood as self-serving agencies. It seems likely that Toma either attended or became aware of Friedman's lecture and communicated with him, thereby putting him onto Chant and Acheson. It is quite a thought that for all his interest in this issue—dating at least from Friedman (1962c), but in fact traceable earlier than that—and the fact that he was giving an invited lecture on it, Friedman had not found out that there was a literature on the subject.

Still, what he said was that without its being necessary to suppose that anyone actually pursued such objectives, a good picture arose from treating the Federal Reserve as behaving 'as if' (p. 116) it was seeking to retain members. It is interesting, obviously, that when discussion of a monetary question was put in 'as if' terms—a very rare occurrence in Friedman's work—it was again in relation to a decision-taking process. These efforts led it to behave in ways which generated inflation, Friedman said, and reported some letters he had written to Arthur Burns. This led him to the view that the optimism expressed in Friedman (1975b) had been misplaced, and so inflation continued to occur, in the end because of the structure and incentives of the banking sector and its regulator. Congressional control of policy, he thought, would bring the interests of the electorate—the anti-inflationary interests of the electorate—to bear.

3.3 Development and Inflationary Government Finance

Another idea that Friedman advanced several times was that inflation resulted from the government seeking revenue. That was mentioned in Friedman (1958f, 1963/1968, 1973f). In the latter he even said (pp. 42–43) that the use of funds thus raised to finance development explained the belief that the two were associated—a belief that could perfectly well be true on the basis Friedman was suggesting, though he did not say that. A little later, in Friedman (1974g) he perhaps recognized the severity of the problems facing governments of developing countries more seriously, suggesting that since political pressures to inflate were particularly hard to resist, their best option could be to forgo seigniorage altogether by adopting the currency of a large, stable, developed, trading partner. In that pragmatic context, he was even willing to admit the benefits of fixed exchange rates, though of course the underlying argument—applied to developed as well as developing countries—was commonplace, and always rejected by him.

The particular question of financing government in the context of development was the subject matter of a basically theoretical discussion in Friedman (1971f) which is another of Friedman's lesser-noticed pieces with a clear and valuable idea. The value to the government of the inflationary finance is the value of money issued, but that is the product of the rate of inflation and the existing money supply. At higher rates of inflation, the private sector economizes on cash balances so that one part of that product becomes smaller as the other becomes larger. Friedman observed, following particularly Bailey (1956) and building on the additional paragraphs in the reprint of Friedman (1942/1953), that as a consequence, government revenue from inflation is maximized when inflation is at such a rate that the elasticity of demand for money balances is one. That much was routine. However, he also observed that this applied to the case where there was no economic growth. In a growing economy, there would be an increase in desired real balances as incomes rise. The supply of these balances would therefore be a further source of revenue. But again, the value of that revenue would depend on the quantity of real balances demanded (and its income elasticity). So, with inflation fully anticipated, high inflation would reduce government revenue by two effects. First, as in the simpler argument, it would issue less money to maintain the existing real value of money balances. The second was that it would issue less money to enlarge those balances as income grew. Another way of looking at it was that in the growing economy there was therefore an extra benefit from low inflation. That was that desired real money balances would be larger, so that for a given percentage by which nominal balances could be expanded, the government would acquire more revenue.

It is a clever and important argument, perhaps under-noted even many years later, though it did come with an oddity of an analogy. Perhaps feeling that the point was hard to understand the way he had put it, Friedman drew a parallel between the government issuing money and the private monopolist selling both a principal good and secondary services, of maintenance. He noted that the services might be sold at a price less than that which maximized profit on them in order to sell more of the primary product and hence more services (p. 855). He said the analogy was imperfect, but rather curiously he had misconceived it more fundamentally than that. In the first place, in Friedman's case it is the primary product—money balances—that are being supplied on favourable terms, so as to increase the volume of the secondary product—the increase in those balances. Secondly, though, and rather oddly, what tends to be treated as the usual case of the two-part tariff is also the other way round from the way Friedman put it. In the telling of Harford (2017), for example, it was the razor-blade holders that Gillette sold cheaply, so as to increase sales of the blades. One might almost wonder whether Friedman thought the usual story incorrect, as perhaps it is (cf. Picker [2011]). In any case, Friedman went on to say that it appeared that the observed rate of inflation were in excess of the optimal rate, noting that this was particularly so in developing countries, where the rate of growth was expected to be fast, and wondering why there was excessive inflation said,

> In a sense, the situation is an unstable political equilibrium. Governments tend to look little farther than the next election. If that election is close, an increase in the rate of monetary expansion is sure to provide the government with more revenue. The negative effects… will come later.
>
> That, I believe, is the fundamental explanation why governments so often inflate at a higher rate than the rate that would yield the maximum revenue over a considerable period. (pp. 853–854)

He did not quite say they were doing it to win the election, but that is surely what he meant. One further point he might have considered is how this relates to the question of central bank independence, since his argument here makes just the kind of point its advocates tend to think makes their case. More important than that, and more interesting too, though is the main argument he was making on how economic growth affects the optimal rate of inflation.

3.4 Indexation and Disinflation

In the 1970s, though only briefly, the point that inflation provided governments with revenue became an aspect of Friedman's advocacy of the view that general indexation, and particularly of government debt and tax thresholds, would help to reduce it. Much earlier than that, such as in Friedman (1952c), answering Congressional questions, he had favoured indexing government debt, saying it would reduce the cost of inflation and provide a reliable savings vehicle for lower income groups. He also made the point that indexation would avoid having the Treasury marketing bonds on the basis that buying them was a way of making provision for the future in circumstances where, because of inflation, this was simply not true. There, he also mentioned the point that such a policy might lead to the Treasury paying more attention to the matter of inflation control, but that was as far as he went. After that, he occasionally voiced support for specific indexation, such as in Friedman (1969d) calling for indexation of tax brackets, but in the mid-1970s, as inflation became a more serious problem, the argument took a different turn.

General indexation started to be widely advocated as being a means of reducing the costs of inflation by, for example, smoothing adjustment to it generally, and reducing its tendency to arbitrary redistribution in particular. Tobin and Ross (1971), for example, suggested that the costs of 'living with inflation' might be reduced below the costs of stopping it and it would then be better to allow it to continue. A natural response to this kind of argument, though, was that one way or another the introduction of indexation would worsen inflation. For example, it might perpetuate that inflation which occurred; or, by reducing—or seeming to reduce—the costs of inflation, it might reduce political resistance to it; or the introduction of indexation itself might seem to signal the policymakers had given up the attempt to control inflation, and thereby generate inflation-expectations.

In 1973 and 1974, Friedman took the argument in the opposite direction, specifically arguing for the introduction of indexation on the basis that it would help to reduce inflation. The argument that it reduced the costs of inflation was still there—he put that in *Newsweek* in Friedman (1973e). But in his following column—Friedman (1973h)—he added the points that indexation would ease the 'withdrawal pains' from reducing inflation, and argued that it would help secure counter-inflationary policy by altering the incentives on governments. His idea was that if fiscal drag were eliminated, and an appropriate real rate of interest paid on government debt, much of the fiscal benefit of inflation to government would disappear. Then he made substantially the same argument supporting Congressional moves to indexation in another *Newsweek* article—Friedman (1974h).

In these, the point about 'withdrawal pains' was not clearly addressed, but it was, principally for an American audience, in a *Fortune* article actually entitled 'Using escalators to help fight inflation'—Friedman (1974i)—and for a British one in Friedman (1974d), an IEA pamphlet, *Monetary Correction*. The *Fortune* article began with the simple statement, 'The real obstacles to ending inflation are political, not economic' (p. 94), and in both accounts, the substance of the argument ran along very similar lines. Friedman argued, as before, that indexation would remove the fiscal benefits of inflation. But a point he said was more important was that it would reduce the costs of disinflation. The reason was that during inflation, contracts were set in expectation of its continuation, so that when it was reduced, prices, and in particular wages, were misaligned and so various maladjustments, particularly unemployment resulted. Those costs, he said, made the politics of disinflation very difficult. The addition of an 'escalator clause' to the agreement so that the nominal figures agreed could be adjusted in the light of inflation would remove the need for expectations of inflation to be incorporated in the contracts. Then, as inflation fell, adjustment would be smoother, and the political costs of disinflation much reduced.

Friedman considered various objections to indexation, though in some cases without really seeming to take them seriously. For example, on the point that indexation might worsen the situation if it seemed to signal that the attempt to control inflation had been given up, he said that if the public did not wish to stop inflation, it would be best quickly to adopt ways of living with it. Similarly, he considered a point derived from thinking along the lines of cost-push inflation that inflation was said to serve the useful purpose of reconciling incompatible sectional demands by fooling these groups into believing they have acquired a greater share of national income than they had. To this he responded,

> If this view is correct on a wide enough scale to be important, I see no other ultimate outcome than either runaway inflation or an authoritarian society ruled by force. Perhaps it is only wishful thinking that makes me reluctant to accept this vision of our fate. (Fortune, p. 176, similarly *Monetary Correction*, p. 32)

That is not really a response at all, obviously. What it shows is merely that Friedman rejected the premise of the argument.

Friedman's idea was an innovative one, and also an outstanding example of the Friedmanesque way of standing an argument entirely on its head. The familiar battle lines saw those supportive and opposed to discretionary monetary policy on opposite sides of the indexation issue. Those who were optimistic about the potential of discretion, saw then-prevailing inflation as an unfortunate circumstance, and contemplated a way of reducing its costs. Those who thought discretion dangerous saw indexation as a way of releasing a constraint on its full, detrimental, operation. Friedman, though, conceived it in neither of those ways, but thinking about the specifics of the forces driving policy to inflation or to allowing its persistence, saw indexation as addressing the root of the problem and thereby transformed 'living with' inflation into killing it.

Unsurprisingly, this idea attracted a great deal of media attention and was also very clearly an idea Friedman was keen to push into public discourse. As well as the allusions to it in two of the *Newsweek* articles, and its Fortune and *Monetary Correction* outings, it was also the main substance of his contribution to Fellner et al. (1974) and the *Monetary Correction* version was reprinted in Giersch (1974). Friedman raised it again in Friedman (1974j) and responded to discussion in Friedman (1974k) at another meeting of the IEA, the discussion of which was published as Robbins (1974). From this discussion there emerged two issues which are worth attention in understanding the development of Friedman's views and arguments.

The first arose from the point that the argument Friedman made for indexation has a close logical parallel in one that might be made for incomes policy, which Friedman strenuously rejected throughout his life. The argument would be that starting from a position of ongoing inflation, combined with disinflationary macroeconomic policy, an agreed wage policy could reduce the costs of disinflation in just the way Friedman said indexation would. Friedman (1974k) did—just—accept that possibility, citing a case from Argentina where he said an incomes policy had allowed a disinflation at relatively little cost, but then promptly dismissed the idea of there being a worthwhile general lesson from that case.

Certainly, the indexation approach has advantages. First, it is a mechanism which provides an automatic response to falling inflation rather than being one which can only be implemented on the basis of a commitment to such policy. Second, it allows for free wage bargaining in real terms so that there are none of the temporary price distortions that might arise from incomes policy. There is also the possibility that the implementation of incomes policy leads policymakers to a view that the control of inflation is no longer their responsibility. That clearly might have detrimental consequences. Those are good arguments, though there would be difficulties with indexation as well—there is a question as to how complete it could be made, and of whether incomes policy might not have advantages in terms of public understanding of the policy. Depending on the attitudes of wage bargainers and their organizations it also might well be that incomes policy would be easier to introduce quickly as a temporary measure. Nevertheless, clearly, indexation has the advantage of neither being something a policymaker who was actually inflation-minded would introduce, nor of creating any appearance of being sufficient in itself to reduce inflation. So, on the face of it, they make a good case for distinguishing the two proposals although perhaps not in showing incomes policy to be invariably quite as meritless as Friedman sometimes seemed to suggest. One interesting issue, though, is not so much whether in the detailed analysis indexation may be preferable to incomes policy, but whether, in the light of his arguments on the former, Friedman is entitled to his very strong position against the latter. His case for indexation applied only to exceptional periods. When he made it, then, he probably should have accepted that, in the same circumstances, there could be benefits of temporary incomes policy. But for some reason, he was not willing to do so.

The second issue coming up in the discussion was that of the relationship between indexation and cost-push inflation. On this, a point contra Friedman was powerfully put by Peter Jay. He is correctly described as sympathetic to monetarism, but also took the view—in Jay (1973), for example—that specific difficulties with British policy arose from aggressive trade unions and political intolerance of unemployment. A fuller statement of his views was slightly later—Jay (1976)—but he gave an extremely pessimistic prognosis in Jay (1974a), arguing that policy lurched between seeking to control inflation and to reduce unemployment, and at each turn, the position was worse than at the previous one—each time policy turned to reflation, inflation was already higher than it had been at the last; each time it turned to inflation control, unemployment was higher than on the previous occasion. That made the process explosive, and the consequence, doom.

As Jay (1974b)—in part responding to Friedman (1974j)—characterized the debate, there were two views which were compatible with monetarism. One was that if unions brought wage increases, there would be unemployment, and inflation would stabilize when unemployment was sufficiently high that this balanced the bargaining power of trade unions, making for equilibrium. The other group—Friedman's, as Jay put it—denied that unions ever had power permanently to raise wages so that after an initial recession in which unemployment would rise there would be a reversion to a low rate of unemployment. Friedman (1974k) made a comment on Jay's presentation which did not dispute his characterization of the issue. But he also suggested that one should think of universal indexation, and said that if unions could raise wages in those circumstances, it must mean that society had broken down. This, clearly, was the same sort of dismissal of the argument as appeared in his other presentations of it. But Jay, naturally enough, and with perfect consistency with his earlier and later arguments, said that this was his point—society was breaking down. That view, perhaps, as of 1974, was more plausible in the United Kingdom than in the United States. Certainly, any view that it was literally impossible for unions to raise wages would have seemed absurd in the British context. So the strength of feeling about it may have caught Friedman by surprise, but still, at the IEA, the issue of cost-push inflation and its relation to Friedman's proposal became the critical one, and Friedman's arguments seem to provide no answer at all.

The first oil shock had occurred in 1973 and by the following year was being said to have been a source of inflation. Although such possibilities had not really featured in earlier discussion of cost-push inflation, it would naturally be treated as being of that kind. Friedman might have been expected to accept that the forming of a cartel or a new decision to exercise its power amounted to the increase in monopoly power that made cost-push possible. Actually, he tried (for example, in an interview in *The Guardian* on September 16, 1974) to argue that if the price of oil rose without an increase in the quantity of money, other prices would fall, so there would be no general inflation. That would be a worthwhile observation only if velocity never varied, and the quantity of money was completely exogenous, neither of which was a view Friedman held, so it seems a peculiar position for him to take. In *The Guardian* he again advocated indexation. Had he accepted that oil price increases could cause cost-push inflation, he would have had to confront the thought that indexation might worsen the problem. As Goodman (1975, p. 144), for example, argued, indexation could make adjustment to adverse changes in the terms of trade very difficult—as indeed it would, unless that deterioration had no inflationary consequences at all.

The discussion at the IEA—which, being reported in *The Financial Times* on 19th September, seems to have been a couple of days after *The Guardian* interview—is an interesting encounter in itself, because the appearance from Robbins (1974) taken as a whole is very much that Friedman lost that argument. But even more than that, he seems thereafter very much to have downplayed his advocacy of indexation, and although *Monetary Correction* was reprinted in Murchison (1978) in an apparent attempt to launch an American clone of the IEA, Friedman seems very largely to have stopped advancing the idea that it would actually help in reducing inflation. It is notable, for example, that it disappeared from his *Newsweek* columns; it went unmentioned in his discussion of the British situation as reported in Sinclair (1976). There, he did say that taxation-by-inflation could occur without overt legislation and therefore had an undemocratic aspect, and repeated his earlier accounts of how it finances government. He also made the point that inflation had resulted from the emphasis given to full employment, and gave an account like Jay's of gradually worsening cycles. But indexation went unmentioned. Its desirability did continue to feature in his work occasionally, but as in Friedman (1975c), the case tended to be made entirely in terms of 'equity and representative government', or some such—in other words, it was a much more conventional idea about 'living with inflation'—rather than curing it. And in his travels in Australia, as recorded in Friedman (1975d), he seems to have made nothing of it. That is significant because indexation was widely discussed in Australia at the time and Friedman seems not to have given any weight to it at all—one headline, in *The Bulletin* (12 April 1975), actually read 'Friedman deflates the indexationists' as he gave them so little support. The idea did feature in Friedman and Friedman (1980, pp. 276–280). There, as far as it was applied to the private sector, it was about mitigating the costs of reducing inflation. But there was no sense of the idea being a great breakthrough, and it was evidently being assumed that policy was effective in bringing inflation down, and the matter of the possible breakdown of society was simply ignored. From about the same time, the point was clearer in Friedman (1979a, p. 7) where, though declaring that he had not changed his view he said he regarded indexation not as a cure for but 'only as a way to reduce the harm done by inflation'. It was not, then to reduce the harm done by reducing inflation. The idea also popped up in Friedman (1984b) in the form of the possibility that financial markets might develop inflation futures and thereby allow hedging by private parties, but that was nothing at all to do with the control of inflation.

It is an interesting episode. Having pushed the idea quite so hard for a brief period, and attracted so much—apparently welcome—attention for it, Friedman seems to have downplayed the idea very markedly, and certainly stopped advocating it for the distinctive reason he had. The most likely explanation seems to be that either Peter Jay convinced him that it would not work, or at least convinced him that he had no answer to Jay's concerns that would convince his audience—however far those concerns may have been from his own preconceptions.

16

The Phillips Curve

1 The Phillips Curve Myth

Friedman's supposed contributions on the matter of the Phillips curve are, of course, often amongst those thought to be his most important. But in these appreciations, there is a fundamental and crucial mistake since the wider story of the history of the Phillips curve on which they are based is, in all its principal aspects, a fiction. Despite the story so often told, Phillips (1958) was nothing like the first paper to consider a relationship between wage change (or inflation) and unemployment; Samuelson and Solow (1960) gave no clear indication of thinking ongoing inflation would bring any benefit, and in any case, there is no sign at all of their influencing opinion in that direction in the 1960s; practically none of those estimating Phillips curves and like relations in that decade advocated high inflation as any kind of remedy to unemployment; the argument that expectations would adjust to such inflation and undermine any beneficial effects it might have was very widely appreciated before its supposed discovery by Friedman (1968a), Phelps (1967), or Friedman (1966c) and when their views were debated in the 1970s, the issue was whether it was in fact the adaptation of expectations, or something else, that explained the deterioration

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_16

of the inflation—unemployment relationship at that time, not the question of whether expectations would eventually adjust to continuous price rise; in the 1960s and 1970s there was never any kind of consensus—in academia or elsewhere—that ongoing inflation should be tolerated, much less that it was beneficial; and anyway, even the story containing these fictitious elements, which became almost universally believed by economists, emerged in the academic literature only in the mid-1970s. These points were argued seriatim in the first six chapters of Forder (2014), deploying, as Hoover (2015) kindly noted, quite a lot of evidence. Forder (2015) offered further support for the same point of view by considering how that the story started to come into the textbooks only in the 1970s, and furthermore, it did so with historical claims about what had previously been said which could not be supported by the content of previous editions of even the same textbooks—Shapiro (1978) was one such—though the story also appeared in some new books, such Dornbusch and Fischer (1978) at the same time. Then Forder (2017) showed that Harry Johnson, for all his remarkable reputation as a survey-writer and synthesizer of the economics literature, never had a clear picture of the Phillips curve, and presented plainly inconsistent stories about it. The key thing about that, it was suggested, was that Johnson's reputation was left undamaged. Surely that must be because none of his readers had enough idea about the Phillips curve to appreciate how confused he was, and the implication of that is once again that it was not nearly so important as later stories say. Then in Forder (2018a), I responded to some fairly common misconceptions about the argument which happened to be represented in Laidler (2015). All in all, the usual story of the Phillips curve has no historical merit at all, though of course, as Beggs (2016) suggested, it does have some interest as a foundation-myth of the consensus macroeconomics of the 1990s and early twenty-first century.

2 Friedman on the Phillips Curve Before the Presidential Address

The question of what Friedman said about the Phillips curve, when he said it, and whether and in what ways it was innovative are all slightly different questions. In Forder (2010a) I pointed to a string of historical inaccuracies in Friedman (1977a), though the purpose there was to show some widely believed claims to be incorrect, rather than to consider Friedman's work in much detail. And in the works just described, on the Phillips curve myth, there was relatively little attention to Friedman specifically. A fuller exploration of his own comments on the matter is therefore in order.

In Friedman (1962b) there is an exercise for students about the effects of ongoing inflation on wage bargaining. The students were clearly expected to appreciate that inflation would generate expectations which would shift the relationship. That is yet another data point showing the idea had no novelty later in the decade. Phillips was mentioned there, but the curve itself appeared only in Friedman (1966c), as part of his debate with Solow over incomes policy. Even then it was only in the second round of comments, in his response to Solow (1966a). It was mentioned in Friedman (1967c)—a short, shambolic address to students at Stanford. Other than that, I believe, Friedman wrote nothing of the Phillips curve before 1968. That is quite a thought in itself. Were there any truth in the idea, that after Phillips (1958) the curve had been accepted 'with alacrity', as he said in Friedman (1977a), then one could be fairly sure someone so prolific, and so engaged with policy, as Friedman would have mentioned it more often than this.

There is Friedman and Schwartz (1963a) for a start—it was five years after the Phillips curve, after all. But they give no indication of how thinking about policy was affected by it—they hardly could, since the curve is not mentioned. The point is yet clearer in Friedman (1964d), reporting on the findings of his work with Schwartz and Cagan at the NBER. One finding he noted was that their work gave 'no support to the view, now widely popular, that long-run inflation is favorable to economic growth' (p. 20). There was, apparently, no widely popular view about inflation and unemployment that was worth a comment. A little later in the piece (p. 21) he said,

> The general subject of the division of changes in money income between prices and quantity badly needs more investigation. None of our leading economic theories has much to say about it. Yet knowledge about it is needed for better understanding of the impact not only of monetary changes but also of other factors significant in the business cycle.

One could be tempted to say that it seems he had not even heard of the Phillips curve, though that would be going further than the evidence allows—whether or not he had heard of it, he did not think it important enough to mention, even when denying the existence of work of that kind—just like Johnson (1962), then. Or, as another example, Friedman (1968c, p. 445) discussed conflicts in policy objectives and said that the goal of stable prices can conflict with 'such other objectives as stable exchange rates, stable employment at a high level, and low interest rates on government borrowing; and with the possible desire to use inflation as a means of imposing a tax on money balances'. That comes closer, but in doing so reveals the gulf that remains—stable prices might conflict, amongst various other things, with *stable* employment at a high level. If it is the Phillips curve is at the centre of policymaking, that is a very funny way of putting it.

It would be very easy to add to this list because Friedman wrote dozens of things between 1958 and 1968 that did not mention the Phillips curve. But the point is made. Before 1968, Friedman thought nothing of the importance of the Phillips curve, either by that name, or any other idea of a tradeoff between inflation and unemployment.

3 Friedman on the Phillips Curve: From Presidential Address to 1975

On common understanding, it is of course Friedman (1968a) that marks the great change in macroeconomics. That was his Presidential Address to the American Economic Association, and is often described in terms which suggest it was an enormously influential paper. It is this paper that supposedly launched his attack on policy based on the curve. That cannot be right since the story in which the idea is embedded is made up, but that creates a question as to what view should be taken on Friedman (1968a).

As already noted, Friedman (1968a) was one of the papers where he gave his account of the history of monetary thought, and presented his case for rules, although it is by no means a distinguished example of either. In Forder (2018d) I argued that on the face of the paper itself, making the case for rules very much along the lines he had been arguing since Friedman (1960a) was the primary objective, and the question of which rule to follow more or less the only other one that was discernible at all. This is apparent from the introduction which states the goal of the paper as being to address the conduct of policy, not the tradeoffs amongst its goals. It is also apparent from the conclusion, which was entirely in terms of the benefits of rules. And it is apparent from the pages in between as well, where the curve is hardly mentioned. Seen as a discussion of the case for rules, though, the paper fits very neatly into Friedman's oeuvre, from which much of it is taken fairly directly.

There is only a very small part of the paper that concerns the Phillips curve at all. That part consisted of one paragraph and a long and crucial footnote. The paragraph appeared as a sort of summing up of something he had already said—'You will recognize the similarity', he said, 'between this statement and the celebrated Phillips Curve', (p. 8) before explaining that Phillips had written about a period in which stable prices were expected whereas when, as in Brazil around the time Friedman was writing, there was rapid inflation, the relationship changed. To that, he appended the footnote which said that the Phillips curve would shift according to the experience of inflation and, most importantly, 'That is why students of empirical Phillips Curves have found that it helps to include the rate of change of the price level as an independent variable' (p. 6). That is critical because, quite casually at the end of a footnote, in 1968, Friedman clearly (and correctly) noted that those working on econometric Phillips curves had understood the importance of inflation as a determinant of wage change. It follows immediately that they could not have seen the simple curve as offering a stable 'menu of choice' between inflation and unemployment.

Nor can an attribution to policymakers of some misunderstanding about the Phillips curve be read into his criticism of their actions. What he said was that policy had been too changeable—the volatility introduced by actual policy actions was in effect much of the basis of his case for a rule. He said that the recently past years in the United States,

> would have been steadier and more productive of economic wellbeing if the Federal Reserve had avoided drastic and erratic changes of direction, first expanding the money supply at an unduly rapid pace, then, in early 1966, stepping on the brake too hard, then, at the end of 1966, reversing itself and resuming expansion until at least November, 1967, at a more rapid pace than can long be maintained without appreciable inflation. (p. 12)

On the other hand, when he was explaining what would happen if policy did seek to target too low a level of unemployment, the discussion was entirely hypothetical. What he said—with some longish parts omitted, and italics added—was,

> Let us assume the monetary authority *tries* … *suppose* that it takes 3 per cent as the target … *Suppose* also that we start out at a time when prices *have been* stable and when unemployment is higher than 3 per cent … Accordingly, the authority *increases* the rate of monetary growth … Income and spending *will* start to rise … the rise in income *will* take the form of an increase in output … Producers *will* tend to react to the initial expansion in aggregate demand by increasing output … But it describes only the initial effects … Employees *will* start to reckon on rising prices … In order to keep unemployment at its target level of 3 per cent, the monetary authority *would* have to raise monetary growth still more … (pp. 9–10)

Noting the italic words, it is impossible to read this as a description of what had been happening in the years up to 1967 in the United States. It is clearly and unambiguously a theoretical discussion of what would happen if the policymaker tried—as it would later be put—to 'exploit the Phillips curve'. Again, in Friedman (1968a), there is no suggestion of error about the Phillips curve leading to poor policy.

One could add also the point that Friedman said that the adjustment of expectations—'anticipations' actually—would take decades. It seems an extraordinarily long time for wage bargainers to take to understand there was inflation going on. That, just by itself might be said to call into question whether Friedman made a prediction about the shifting Phillips curve that turned out to be confirmed in the 1970s. But in Forder (2018e) I argued that he may have had something else in mind altogether—not a more or less rational process of understanding policy, but rather a process of adaptation of institutions and habit-based behaviour. If that is right, it too takes his argument away from the role it is supposed to have in the story of the Phillips curve.

On the question of the originality of Friedman (1968a), the widespread belief is that it announced the idea that expectations would adjust to reality so that the Phillips curve would shift. Many others had said that earlier, and in Forder (2010b) I pointed to several authors who had noted various isolated statements of it. Darity and Goldsmith (1995) and Boianovsky (2005) could be added for noting the appearance of the argument in Champernowne (1936). The importance of the matter though is not that Friedman had the odd precursor here and there, but that the idea was commonplace long before 1968. It was perfectly routine, an entirely ordinary thought. That is of course what common sense would lead one to expect, since the idea that the economics of 1968 was so primitive in this regard is absurd. In advancing the idea that if inflation continued, wage-setters would come to expect it, Friedman (1968a) offered nothing of any originality. Beyond that, though, whatever mistakes it might be alleged others made, Friedman himself cannot have believed the argument new at that time since, as I showed in Forder (2018d), he had made it himself several times before.

It is true, of course, that it is the paper that introduced the terminology of the 'natural rate of unemployment'—or more or less introduced it, after its appearance in Friedman (1966c, p. 60). That in itself is nothing more than a piece of terminology. Once it is accepted that fully anticipated inflation does not affect unemployment, and that an ongoing steady inflation will in due course be fully anticipated, we have a 'vertical Phillips curve', or a 'natural rate of unemployment'. Since those things were accepted, the terminology, though original to Friedman, adds no insight. It has been argued by de Vroey (2016) that Friedman's account gave a special explanation of the mechanism generating the vertical Phillips curve. But even if that were correct, it would be very much de Vroey's discovery, long after Friedman wrote. It could not be a defence of the view that Friedman's paper had a devastating impact in the 1970s. The supposed-importance of Friedman's paper turns on what it said about the Phillips curve and expectations, and on those things, it said nothing original.

Nor is there anything else of any significant, substantive originality in the paper. His history of attitudes to monetary policy was repeated again and again; his description of actual Federal Reserve policy, likewise, had appeared in a number of places, though he updated it to 1968. What he said about Brazil, and changing expectations there appeared before in Friedman (1966c), and before that in Friedman (1963/1968), although in that case without the vocabulary of 'the Phillips curve'.

Beyond that, though, there is a striking point in that the paper is not just unoriginal, but also poorly written and full of errors. I considered a good selection of these in Forder (2018b). Some of them are just spelling mistakes, or minor factual ones, though others are muddles in his argument. Even those, though, can easily enough be corrected. So they do not mean that the argument of the paper fails; but they do mean it is a notably careless presentation—rather obviously, it is something Friedman thought unimportant. All in all, it is a fantastically over-rated paper, perhaps even more so than Friedman (1953b).

To all this can be added the point that Friedman himself seems—at first—not to have thought much of the paper. Certainly, if he thought he was making an important argument, he might have taken more care in writing it up. Perhaps even more notable is his failure to refer to it—or to its importance anyway—in the following years. As I pointed out in Forder (2018d), until others started to say that the paper had been important in overturning the consensus, Friedman mentioned it only rarely himself, and never gave it any emphasis.

Friedman (1968b, p. 15) offers one example where he cited the paper, but not for its discussion of the Phillips curve. Then when it was reprinted in Friedman (1969c), it appeared as Chapter 5 of 13, without even any note in an introduction to indicate that its author thought it introduced a new idea, crushing to the established consensus. That was more attention than it had when he debated the relative power of monetary and fiscal policy with Walter Heller, who had been Chairman of the Council of Economic Advisers when the Kennedy tax cut was proposed. Friedman (1969e, p. 47), said that he believed too much was expected of fine-tuning, but did not mention any plan to reduce unemployment with inflation, and made no mention of Friedman

(1968a). Surely, had he actually thought it important, that would be evident from Friedman (1973b) and Lawrence and Norman (1973)—substantial interviews in *Challenge* and *Playboy*. In each of these, he discussed the cause and control of inflation. If, five years before, he had destroyed the foundations of the policymaking consensus, he would have found a way of working it into the conversation. And even before that, when he wrote Friedman (1970e)—his own account of 'The counter-revolution in monetary theory'—the supposedly revolutionary character of the Presidential Address was no part of the story he wanted to tell.

Not only did he give very little attention to Friedman (1968a) in the following years, but he gave even less to the Phillips curve. One point follows from his slightly earlier discussion of the Phillips curve, expectations, and the natural rate of unemployment in Friedman (1966c). That, as noted above, was his second round of comments in a debate. The interesting point is that his first, primary contribution to that debate—Friedman (1966b)—was reprinted in Friedman (1968d), but the all-important—allegedly all-important—Friedman (1966c) was omitted. Perhaps most notable, though, is the absence of the Phillips curve from his discussions of indexation. It would seem to be ideally designed to illustrate his point about how clauses to revise nominal contracts in the light of inflation could affect the basis of bargaining and reduce the sacrifice ratio. It would be possible to list many places he did not mention it, and the *Challenge* and *Playboy* interviews would again be on that list. Another notable example would be the debate in Shonfield et al. (1974). There, Friedman he emphasized the revenue-raising objectives of government as the main source of inflation, and although there was much debate over cost-push inflation, he made no mention at all of the Phillips curve. Indeed, between 1968 and 1975 his only substantial discussion of it—perhaps his only discussion of any kind was in Friedman (1970f)—one of his papers on the monetary framework discussed above (p. 247) and thence in Friedman (1974a).[1] There, Friedman said

[1]Friedman is reported as saying he thought the curve vertical in verbal discussion in Friedman (1971a, p. 70). His remark does not seem either quite pertinent or quite consistent with his comments in Friedman (1970f) which was already published at the time of the conference. Perhaps the record is inaccurate. Still, there is no sign of Friedman indicating that the idea had been original in Friedman (1968a).

that Keynesian theory had supplied the 'missing equation' by presuming the price level was exogenous. He continued by saying, 'More recently, the developments symbolized by the "Phillips curve" reflect attempts to bring the determination of prices back into the body of economic analysis, to establish a link between real magnitudes and the rate at which price change from their initial historically determined level' (1974, p. 32). And that was all he said—there was nothing about the Phillips curve being seen as offering a menu of choice; nothing about policy being inflationary. What he did say, though, as far as it went, was quite sensible. As discussed in Forder (2013), Keynesian analysis of the 1950 and 1960s often did regard wage change at less than full employment as more or less unrelated to unemployment. During the 1960s the analysis of wage change gave more and more attention to the idea that unemployment might be one of its crucial determinants. By no means was all of that work inspired by, or much like, Phillips'. But to say that it was 'symbolized by the Phillips curve' captures the point very nicely.

All that raises an apparent problem as to how it is that such a poor paper as Friedman (1968a) came to be so influential, but the answer to that it is that it did not. It came to be highly cited for sure, and to be reported as having been influential. But the case that it actually had influence, rather than merely later being said to have done so is another matter. In the first place, there are signs that it was not nearly so widely read as might be supposed. For a paper by a controversialist such as Friedman, making an argument which was, supposedly, so crucial to the understanding of macroeconomics, it is remarkable how little was ever made of its very evident and plentiful weaknesses. The kind of drafting errors discussed in Forder (2018b) are not fatal to its argument, such as it is, but they do expose it to criticism, and make it vulnerable to ridicule. Yet except for Kaldor (1970), who pointed to what may be no more than a mistake about a date, no one seems to have taken the opportunity. The natural explanation is that those who read the paper, thought it not worth the effort to comment on it, and perhaps that there were not many who even read it very closely.

In addition to that sort of inference, as I pointed out in Forder (2014, pp. 88–89), discussion of the paper is strikingly absent from various commentaries and compilations where it would certainly have been mentioned if it had been seen as containing an important argument. And the further investigation of the reception of Friedman (1968a) undertaken in Forder and Sømme (2019) confirms that initially it was not seen as saying anything new, and it was only later, as it was recast in particular roles it never really had that it started to be highly regarded. In due course, its fame came just to be part of the Phillips curve myth.

So much for the idea that Friedman (1968a) was an important paper. So much for the idea that it was even thought important by audience or author.

4 1975 and After

A different sub-chapter—a blatantly different one—opened in 1975 when Friedman's attitude to the Phillips curve and to Friedman (1968a) both changed rather suddenly. There is a hint of the idea that a supposed tradeoff influenced policy in Friedman (1975e)—a conference discussion of developments in monetary policy—but it was only in Friedman (1975a) that it was really taken up. There, what he said was quite different from anything he had suggested before. He described Phillips' analysis as 'utterly fallacious' (p. 15) for being conducted in nominal terms, with no quarter given for the point that he had been analysing a period with no sustained inflation. Rather, it was the 'general intellectual climate' (pp. 16–17) which led Phillips to think in terms of nominal rather than real wages, and this was apparently part of a 'Keynesian confusion' (p. 16) between the nominal and the real. Here, giving it this kind of emphasis for the first time, Friedman said, 'In my Presidential Address to the American Economic Association seven years ago, I argued that the long-run Phillips curve was vertical' (p. 23). Concerning the expectations argument, credit was given to Phelps, though it was Phelps (1970) that he cited, in preference to Phelps (1967) or the original version of the 1970 paper—Phelps (1968). Apart from the aspersion that Phillips was remarkably lacking in intellectual fortitude, the picture painted is clearly intended to describe a rather extraordinary degree of Keynesian foolishness (or ignorance). And in addition to the claims made, there is also the point that the anonymity the Keynesians had so often been given by means of the terminology of 'the income-expenditure theory' was removed. And it was here, that the mistake, like Phillips' own, was very much centred on the idea of the Phillips curve. And Friedman (1968a), for the first time, was presented as having diagnosed the problem and identified the solution.

Friedman (1975a) happens to have been quickly followed by Friedman (1976h) and Friedman (1977a), with all three having marked similarities. The first of those was a second edition of Friedman (1962b), his book on price theory, with the most noticeable change being the addition of a whole chapter on the Phillips curve. The second was his Nobel lecture. Both painted a picture of the Phillips curve having been foolishly interpreted as suggesting a long-run tradeoff between inflation and unemployment until Friedman and Phelps introduced the expectations argument.

The inclusion of the extra chapter in Friedman (1976h) is interesting for more than one reason. First of all, being a lone topic on macroeconomics, it does not fit the book at all. In the Preface, Friedman noted its inclusion (p. vii), and explained that it was the text of a lecture given in London in 1974 and that its relevance to price theory should be clear. It might be said that expectations are important in price theory, but the issues could perfectly well be illustrated with an example closer to the core of the book. Presumably, Friedman was anxious to advertise his latest presentation of the Phillips curve. There is a little more to it since, whilst that chapter preserved the text of Friedman (1975a), it also had an extra page on the possibility of a 'positively sloping Phillips curve' (and a couple more on cyclical unemployment). In the 'Questions and Answers' printed at the end of Friedman (1975a), Mark Brady had asked (p. 31) about the possibility that inflation could produce 'a misallocation of resources and malinvestment', and consequently a positively sloped Phillips curve. Answering that question, Friedman said the 'crucial' (p. 32) point was whether the inflation was open or repressed. Considering the former, the only possibility for an effect on employment that he saw arose from agents economizing on money balances, and this affecting labour market behaviour. On the other hand, in Friedman (1976h), Brady was not mentioned, and it was reported as a fact that in recent experience there had been a tendency for both inflation and unemployment to be higher. The issue about economizing on money balances was not mentioned, and Friedman said that the explanation of the positively sloping curve was that inflation was not open, and that extra interventions prompted by the inflation raised the natural rate of unemployment. Even if the text of Friedman (1975a) had been revised for publication, the discussion of the positively sloping Phillips curve can hardly—contrary to what the Preface said—have been part of the 1974 lecture, since then Brady's question would have made no sense. The appearance, then, is that Friedman adopted Brady's idea and described himself as having presented it at the IEA.

Friedman (1977a) softened some of what was said about Phillips—rather than anything being utterly fallacious, the problem arose from a 'hypothesis associated with the name of A. W. Phillips' (p. 454), and Phelps (1967) was cited—though this time so was Friedman (1966b)—a mistake, actually, as it should have been Friedman (1966c)—and a *Newsweek* article—Friedman (1966d). These were presented as supporting the claim that Friedman had been sceptical of the idea of a stable Phillips curve 'from the outset' (p. 455). (Actually, though the *Newsweek* piece discussed expectations in relation to inflation and unemployment, the Phillips curve was not mentioned.) It is an oddity that Friedman thought works of these dates substantiated that claim, but it is true that the Phillips curve—using that label—only featured prominently in American policy discussions after about 1970. Perhaps, in all this muddle, Friedman somehow thought that was where the story began. That would be another blow against the idea that he was describing the actual policy in 1968, and add another little complication to sorting out the history. But, clearly enough, what Friedman said about these things can hardly be treated as reliable.

Still, despite softening his claims about Phillips, in Friedman (1977a), the general sense that policy had been set on a foolish basis owing either to a failure to appreciate the difference between nominal and real variables, or that expectations would adjust to reality, is still clear. In this case, Friedman took other little steps. One was to invoke Pierre S du Pont, protesting about the danger of inflation to the French National Assembly in September 1790, and commenting on the harm done by people with good intentions but poor reasoning. And secondly, Friedman specifically presented his story as showing the scientific credentials of economics—the story of the Phillips curve was one of progressive understanding, at first flawed, but developing towards a correct account. It is a pity that such a satisfactory story should be so flawed in its history.

The possibility of a positive relationship of inflation and unemployment also featured in Friedman (1977a), though in this third discussion, the question of government-created distortions was only a minor part of his explanation, with emphasis being placed on the loss of efficiency of the price system, and the confusing of relative price signals, which Friedman said Hayek (1945) 'emphasized so brilliantly', and in connection with which he cited Lucas (1973, 1975), and Harberger (1976), though none of them is quite relevant. It seems very surprising that Friedman did not give that answer when first asked the question, or failing that, when he thought about the matter more in preparing Friedman (1976h)—it suggests a remoteness from the work of Hayek and Lucas, along with, perhaps, a fixation with the dangers of price control.

In what is perhaps a particularly interesting aspect of the whole matter, though the point has already been noted, Friedman's story about the Quantity Theory also changed. In the first of his encyclopedia entries on it—Friedman (1968c), the Phillips curve made no appearance. Indeed, why should it? It is nothing to do with the Quantity Theory. But then in the second—Friedman (1987a), it very much did appear, and the fact that it was vertical was presented as somehow being a central idea of the Quantity Theory. Really, it still had nothing to do with it. But there it was. By that time, though, as well as the textbooks considered by Forder (2015), Mayer (1975) had made the monetarists' rejection of the Phillips curve one of the characteristic propositions dividing them the Keynesians and momentum was well and truly behind the creation of a myth.

None of this is to Friedman's credit. On the Phillips curve, he changed his tune, and changed it to put himself and his supposed insights at the centre of the story. It is not just the story either, but the tone and temper of what he said that changed. For a long time, he said nothing about it. Then the long footnote in Friedman (1968a), and the whole discussion of Friedman (1974a) are on one side of chasm, and on that side, there is no hint that a great error was ever made in understanding the long-run effects of inflationary policy, nor of policy being in any way set on the basis of an idea of an exploitable tradeoff. In 1975 the tone is very different, and a succession of works is then found on the other side of that chasm. There is a sudden appearance in the story of Friedman's revelatory insight of 1968, and that revelation takes all its significance from the idea of these foolish mistakes being made. And then there is also such a marked change in Friedman's attitude to the advertising of his work. Whether by means of the sudden attention given to the importance of the Presidential Address, or the reprints of versions of the Nobel Lecture, and the insults it contains, the contrast with the neglect of that Address earlier in the story is very marked.

Another point, a less immediately apparent one, perhaps, concerns the question of how Friedman's later story about the Phillips curve—the story of 1975 and after—relates to what he had said about indexation, and how it could help to control inflation, in 1973 and 1974. Both ideas arise from consideration of the question of why policymakers allow inflation to occur (or persist). In making the case for indexation, Friedman found the answer in the fiscal objectives of government. But in debate at the IEA, Peter Jay presented him with a serious challenge arising from a cost-push story about ever worsening outcomes of inflation and unemployment. Apparently having no answer, in 1975—again, just by coincidence, at the IEA—Friedman then found the motives leading to inflation in a different story—a story about the foolish mistakes of policymakers. Not just the abandonment of the old story, but parts of the new one may owe something to Jay. Like Friedman (1968a), he had previously seen policymakers shifting abruptly between expansion and contraction. Jay added, in addition, the point that, in the British case particularly, each cycle produced a worse result. Friedman (1975a) abandoned his and Jay's image of policymakers as simply lurching from port to starboard and back, but used a version of a Phillips curve story to theorize Jay's ongoing deterioration in outcomes.

It is easy to imagine that the second story, firmly rooted in sound money politics, received the better initial reception. There may be an aspect of the matter which is somewhat specific to Britain, in that cost-push inflation was much more feared, including by those who were otherwise sympathetic to Friedman's positions. And perhaps the superficial impression of policymakers moving round the Phillips curve was a little like a plausible story. But in any case, it seems the reception of Friedman (1975a) was good enough for Friedman to think it wise to develop the presentation to become his Nobel Lecture. So it looks as if a good part of the explanation of his simultaneous abandonment of his novel idea about indexation, and adoption of one about the Phillips curve might be just a matter of how they were received in Britain, at the IEA.

17

Monetary Trends in the United States and the United Kingdom

Friedman and Schwartz (1982) was Friedman's last work of any real significance. It was the long-delayed companion volume to Friedman and Schwartz (1963a, 1970), and the book that was fiercely attacked by Hendry and Ericsson (1983) and then in the British press. It had taken 16 years since the completion of the draft submitted to the NBER reading committee to incorporate the analysis of the UK they had suggested, six of them during Friedman's retirement.

It was more narrowly focussed on statistics than the earlier volumes, but otherwise, shared plenty with them. It is a huge book, pursuing a small number of main lines of enquiry whilst taking up numerous issues that arise along the way. It is intensively footnoted, though that being so much a characteristic of these three works, but not of many of Friedman's others, is surely the work of Schwartz. Though the descriptions of what they did with the data were careful, the statistical technique was, like Friedman and Meiselman (1963) rather simple. 'Not even Durbin–Watson statistics are given and no adjustments are made for serial correlation'—as Mayer (1982, p. 1529) put it. It also substantially shared with *A Monetary History* the methodological approach of adopting a point of view and seeking to construe the evidence in the

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_17

light of it. It is a Quantity Theorist's study of monetary trends and the door is never really open to the possibility of the theory's being wrong, even if that means that sometimes, something must be described as an unresolved puzzle, or the like (pp. 337, 428). And they never really take seriously the possibility that answers of a quite different kind from their own might be better ones.

Although they summarized at the beginning of the book their fifty-three 'Principal empirical findings', the main themes concerned the stability of the demand for money; the effect of monetary changes on nominal income and its division into price and quantity effects; and the matter of the relationship between inflation and interest rates. Strangely making no mention of the foretaste of the work in Schwartz (1975), which did report a Durbin–Watson statistic although only to say that no adjustment was made for the autocorrelation it revealed (p. 148), they said that the demand for money was stable, and argued further that (with suitable adjustments to the data) they could show that the same factors described money demand in both the United States and the United Kingdom. On interest rates, they found general support for the idea that they adjusted to expected inflation and thought the period of adjustment was something like six to nine years, saying that this was shorter than others had suggested.

A key aspect of the work was the approach of 'phase averaging' This meant that the average value of a series over a particular cyclical upswing or downswing gave one data point for a variable, and 'rates of change' came from the trends of these cycle phase averages. The intention was to remove cyclical aspects of the data so as to study the longer term trends. The data they used was also transformed in various ways so as to try to remove the effect of such things as price controls and financial development. The phase averaging technique in particular was what made the book definitely about 'trends' and as Laidler (1982) said, made the subsequent analysis much simpler than it would otherwise have been. He also said that it was unlikely to appeal to the 'econometric purist' (p. 295)—as indeed was going to be shown to be true.

He, nevertheless, was very much impressed by the book, seeing it very much it in relation to the earlier works of the same authors and Cagan (1965), and saying 'I find it difficult seriously to fault this book'

(p. 294), though even he saw limitations of the analysis as well as the possibility that the fact that the results were based on phase averages could lead to misappreciation of what was being said. One such case to which he drew attention was their finding of the non-existence, or possibly positive slope of the 'Phillips curve'. As he said, what their results showed was that there was no secular relationship between inflation and output. But with phase averaging they were in no position to present findings about its importance during a cycle. The phase averaging, he suggested, might also have something to do with the finding that in the United Kingdom there was no relationship between changes in the money supply and in nominal income. Laidler drew attention to the importance of changes in the exchange rate as perhaps driving real income and the quantity of money in opposite directions, and suggested that such incidents could obscure the relationship that prevailed in periods of exchange rate stability.

The phase averaging and adjustments of the data led others to much more hostile overall responses. It was not just that the econometric purists, like Hendry and Ericsson who took exception. Artis (1984) expressed his concern at 'ill-judged' (p. 205) transformations of the British data, and questioned the phase averaging approach applied to British data where the identification of the cycles itself was less secure than in the American case. He wondered about the value of the attempt, as well as the quality of the analysis in trying to show there was a single demand for money equation applying to both countries; and noted that phase averaging created a question as to how fully economic processes were worked out within a cycle, and regretted that the authors had not sought to specify relationships allowing for the description of cyclical dynamics.

A different kind of limitation of the book—more like an aspect of its character than of the execution of the argument—is that the authors do give the impression of fighting old battles and doing it with old weapons. Most apparent in this vein is the effort put into the question of the stability of the demand for money. Tobin (1965, p. 481) had said that Friedman and Schwartz (1963a) had 'put to rout the neo-Keynesian, if he exists, who regards monetary events as mere epiphenomena, postscripts added as afterthoughts to the nonmonetary factors that completely determine income, employment, and even prices'. But that rout, apparently, had to be achieved all over again, as the authors of Friedman and Schwartz (1982) began with the same old story of the simple Quantity Theory, its different interpretations, a Keynesian challenge and a reformulation—much of it lifted from Friedman (1968c)—and then a discussion of the division of monetary effects into price and quantity changes, lifted from Friedman (1974a), with some extra discussion, mainly of the formation of expectations (or 'anticipations') added.

Had they produced their book in 1966, it would have been seen as presenting more lines of argument complementary to those of Friedman and Schwartz (1963a). So much later, though, it must have seemed quite different. A case for the stability of velocity had been made—powerfully made—in the earlier book. It was not likely that anyone unpersuaded then and by the work that had followed, was going to be persuaded by an analysis so full of data manipulations as this. And as to finding the same equation describing behaviour in both countries, quite apart from wondering what the point of trying to prove that might be, it also led to some of the easiest to accept criticisms—Goodhart (1982, p. 1544) noted that it was thoroughly unconvincing to have a dummy for rising velocity in World War I which was the same in both countries, despite the very different length and depth of engagement in the War. (He could have added that the possibility of Britain losing was also there, whereas that could hardly have happened to the United States). Similarly, their assumption that velocity changed 2.5% per year in America because of increasing financial sophistication is part of their demonstration of the stability of the demand for money, but as Hall (1982) said, they might need another dummy variable for deregulation, or as he might have said, for a changing technology of banking. Really, it is questionable whether such variables lead to explanations at all and at some point the necessity of using them surely means that the appropriate conclusion is that velocity is not stable.

The fighting of old battles is probably visible in the discussion of the Phillips curve as well. They were not old battles of 1963 of course, but only of 1975, pretending to be older, but one wonders which economists of 1982 it is imagined would have been interested to learn that in phase average data, there was no Phillips trade off. But Friedman is not the only one who might spend too long on issues that had been resolved, so I had better say no more.

Even Friedman's fights over intellectual history have an aspect of fighting old battles. Patinkin (1948) was cited (as usual) but otherwise there is no mention of him except where he is being said to be wrong about doctrinal history, including at one point, in a footnote that runs well over a page (pp. 45–46). It was unnecessary for the reader, and an irrelevance in the work, but for some reason it was there—perhaps because Schwartz, rather than Friedman wanted to take on Patinkin on the history? Similarly, Temin (1976) was criticized at length for having supposedly 'succumbed to the Keynesian assumption that the price level is an institutional datum' (p. 33 n20) and for his 'implicit identification of nominal and real magnitudes (pp. 50–51 n45). Temin (1983, p. 735 n19), it might be added, rebutted them on this point, and it is an oddity that though Schwartz (1981) made the same sort of point, and Temin (1981) had responded, her paper, but not the response were cited. Meanwhile, no work of Warburton was mentioned, nor Angell, nor Hicks, and Mints appeared only because he edited a book of readings. Keynes, on the other hand, was mentioned all the time—a notable change from earlier work in which the authors had seemed rather coy about naming him. But he appeared mainly so that his errors could be exposed.

Their discussion of interest rates is fresher, and that too is probably the part of the book where fine scholarship shows to best advantage. Still, it is hard to escape the feeling that just showing Keynes to have been wrong was an important part of the motivation. The 'Wicksell–Keynes' view, as the authors described it (e.g. p. 563) was that changes in the interest rate were determined by changes in the productivity of capital. That, they said, was not supported by the data. Even here, the same thing had been said by Cagan (1965, pp. 252–255), as Friedman and Schwartz noted, so there was an element of rehash about it. It is though worth noting the long period of adjustment they suggested—even if it was shorter than that found by others—and their point that expectations had been more responsive to inflation in later periods, presumably because the effects of inflation became more present to the minds of ordinary economic agents as time went on.

As to the old weapons with which to fight these battles, they made the exogeneity of the supply of money more or less an axiom. ‘We shall’, they said, ‘for the most part take it for granted’ (p. 35) that the money supply is exogenous. If anything, they took it further than they had in Friedman and Schwartz (1963a) and that obviously opened one route for a wholesale rejection of their findings. Moore (1983, p. 120) condemned the book on these grounds. Congdon (1983) was by no means as committed as Moore to endogeneity, but he did argue that Friedman and Schwartz’ conclusions for the United Kingdom could not be upheld because they failed to give attention to the different behaviour of the authorities, and an alternative story for the UK would have interest rates, set by central banks, being the exogenous variable. ‘Has Friedman got it wrong?’ he asked, concluding in the affirmative.

Such criticisms, arising from the institutional or historical understanding of the British case really highlight the costs of the delay in publication, but in any case the British audience seems to have been much more sceptical about the work than the American. Frazer (1983)—to take an extreme case, perhaps—gave the impression of not being able to see any limitations at all in the work, but Brown (1983)—the other assessment, along with Hendry and Ericsson (1983), written for the Bank of England—found little in it of value. He considered Freidman and Schwartz’ principal claims about the United Kingdom in the light of the annual data, and his own and other qualitative assessments of causes and effects and does not seem to have seen any reason that Friedman and Schwartz offered a serious challenge to existing policymaker views.

All in all, then, much merit as it has, this book never had the opportunity to make the impact of Friedman and Schwartz (1963a). It was too long delayed. Economics had moved on; the idea of a stable demand for money function was nothing surprising in 1982; the old tools of Friedman and Schwartz looked very out of date and simply could not stand the scrutiny of advanced econometrics. The kind of free-wheeling data handling that had shown such cleverness in 1963 was far behind what would be regarded as sophisticated in 1982. If anything, Friedman’s ingenuity in making things come out the way he wished could then be even more of a source of suspicion than it would have been otherwise. Perhaps there was a battle to be had there over whether to prefer 'simple' econometrics, but this book did not engage that battle and despite a brief expression of sympathy for the idea from Goodhart (1982), Friedman and Schwartz were poorly placed to win it. Indeed, even the definition of money—being an empirical matter—might have been inappropriate so long after the authors' investigation of that matter. But such things were not within the scope of this book. Perhaps, as Mayer (1982) seems to have thought, they would have been better exploring 'cycles' rather than 'trends'. But for sure, as Artis (1984, p. 207) said, one thing this book was not going to do was draw new converts to monetarism. Returns really do diminish and this book illustrates the point because nearly 20 years after *A Monetary History*, there was too little to be gained by a book so inviting of scepticism as this one that was really setting out to show, again, that 'money matters', or I suppose, this time, 'money still matters'.

18

Part III Conclusion

Less varied than his earlier work, surely Friedman's work on money was just as important. Friedman and Schwartz (1963a) was not perfect, it is not that there was nothing to argue about, but it is an outstanding book, and the early reception of it surely shows all those things. It does not argue all the things later stories suggest, and it is not even pointed in the direction of arguing some of them. But it is a great work of scholarship and it does do what the authors were setting out to do—to make a powerful case that when the data is organized around the Quantity Theory, a great deal falls into place.

Apart from appreciating the project encapsulated by Friedman and Schwartz (1963a, b, 1970), with Cagan (1965), and I suppose Friedman and Schwartz (1982), also part of it, there are other points to note. An important one is that apart from simply making his case in these works and others, like Friedman (1964d), Friedman tried three times to find a way to advance shared understanding by specifically contrasting the Quantity Theory and the income-expenditure theory. For all the doubts introduced about whether his case was successful, it was nevertheless Friedman's efforts to bring theory and evidence together that started the debates in which these doubts appeared. His empirical

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_18

orientation is not to be doubted, and the fact that some of the responses were theoretical ones, seeking mainly to show that his evidence was not decisive, does not weaken that point. Another is that much of his work in the 1970s addressed the question of why it was, in practical terms, that policymakers allowed inflation to persist. There are clever arguments there, although he never really seemed to want to bring them together and offer his final conclusions on the matter. It was as if he was happy to present each one as 'the' explanation when it came along, and ignore the issue of fitting them together.

On the other hand, others' irritation at Friedman is understandable as well. He was a slippery debater, and tried to defend some propositions he should have never taken up, or abandoned quickly, and admitted that is what he was doing. It is difficult to believe that his way of writing history to his own benefit really went unnoticed by those who had lived through the period, and contributed to the developments, even if nothing was said about it. The 'oral tradition', although perhaps innocent, could well look like another try at the same thing. None of that can have made him look like a reputable scholar, and hardly have encouraged anyone else to give him his due when he was making unwelcome good points. When it was, as it usually was, possible to find a way to resist his arguments, it cannot be surprising that his opponents took the opportunity. His idea about the Phillips curve is quite a clever one too—of course, that *might* have been the explanation of rising inflation. Evidently a lot of people found it very plausible. More than that, though, it can also be seen as the last move in both of these on going issues. Having failed to convince the profession of the superiority of the Quantity Theory over the income-expenditure theory; *and* having had to retreat from indexation, so that the idea that inflation provided revenue lost its publicity-gaining value, the Phillips curve story addressed both matters. Here, by pretence, the Keynesians could be shown to be wrong; and here was a story about how to control inflation—by accepting the neutrality of money and the natural rate of unemployment, both of which could be found in Friedman's earlier work. It only worked, though, by reconstruing both the history of the Keynesian era and Friedman's earlier work.

On the wider points of methodology there is more to note. In his monetary work, just as in his earlier work, his methodological presumptions are fairly clear. The emphasis on the confrontation of theory with data is there again. But it is notable that overt 'as if' theorizing is not there. In all Friedman's work of the period, it hardly appears, popping up when the question of the Federal Reserve's motives for its behaviour are in question. It is just like the earlier uses, in other words, in that it serves to describe the processes, internal to an agent, by which decisions are taken. But there is no need for 'as if' theorizing otherwise, since it is clearly presumed that the assumptions made are very realistic. Otherwise, there is every sign that no such reasoning would be appropriate, and indeed Friedman and Schwartz (1963b, p. 59) actually declared the need to specify the transmission mechanism if their theory is to be accepted.

Part IV

Popular Writing

19

Part IV Introduction

For all the insight and excellence in Friedman's purely academic work, it makes for only a part of his influence on twentieth-century affairs since as well as that, there was his more direct engagement with policy debate, through popular writing, especially in his *Newsweek* column, and in addition he became noted for arguing a close association between political and economic freedom. That position appeared most importantly in Friedman (1962a)—*Capitalism and Freedom.* Both that and a second book of somewhat similar spirit—Friedman and Friedman (1980)—*Free to Choose*—were popular writings, but also had a close relationship to Friedman's other work because nearly all the arguments, and all of the best ones, were presented with and framed as discussion of issues of economic policy. Later than that, Friedman or the Friedmans carried on writing popular work, but none was nearly so successful, nor deserved to be. Friedman and Friedman (1984) was probably a bit too much of an attempt to capitalize on Friedman and Friedman (1980). Friedman (1992c) was a collection of popular writing on money. Versions of several chapters had been previously published in academic outlets but the book was written to be partly educational, as well as entertaining. That is plainest in the newly written first chapter—a piece naively relying too

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_19

much on Furness (1909) to tell an apparently astounding story of 'stone money' of Yap (or Uap). There was Friedman and Friedman (1998a), of course, but *Capitalism and Freedom*, and *Free to Choose* are much the most important of the books, and together with the *Newsweek* articles, and other journalism, are an important part of the basis of Friedman's reputation.

20

Capitalism and Freedom

Friedman (1962a) is a short book, described as originating in the Wabash lectures Friedman gave in 1956, and ostensibly staking out the claim that there is an intimate relationship between the capitalist system and political freedom. It is a mixed book, too, because Friedman's treatment of 'freedom' from what was apparently intended as a philosophical point of view, is facile. When he moved on to considering ways in which freedom—the free market in particular—might deliver desirable outcomes, the book changed completely and very well earns its reputation.

1 The Matter of 'Freedom'

The first chapter of Friedman (1962a) concerns the matter of 'The relation between economic freedom and political freedom'. Although the version of his ideas on this subject in *Capitalism and Freedom* must be the best-known one, Friedman actually wrote much more about it, and it is quite an education to find out what he said in his various versions of the story.

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_20

The matter first came up in Director et al. (1950) which was a symposium on freedom at Chicago called '*A Positive Program for Conservatives*'. Friedman's role was to discuss things that could not be achieved by the market system. Before Friedman spoke, Wallis (1950) said that although many expressed support for free enterprise, they often ended up accepting market interventions. That was disappointing because, he said, just as Friedman (1953b) did, slightly later, that the social objectives of supporters and opponents of the free market were usually the same, but they differed over matters of technical analysis. So he set about explaining how the free market was supposed to work, and described it as a mechanism for the coordination of the economic activities of millions of different people and pointed out that it depended crucially on prices. It had two notable advantages. One was that it made it, as Wallis said,

> possible for vast multitudes – who may not even know of each other's existence – to work together effectively, to co-operate in the one task of economic activity. In fact, it makes it possible for them – even knowing of each other' existence and disapproving of each other's existence – to work together on this particular aspect of their life, and let other things alone. (p. 5)

The second was that it provided for maximum individual freedom and minimal coercion. That was because the consequences of individuals' actions fell on themselves, and because there was no need for anything such as uniformity of religious view, patriotism, or the like. He then emphasized that this did not mean that everyone could do what they wanted, because the market imposed limits on what could be bought or sold by any particular person, and emphasized the information-providing capability of prices, and the importance of competition.

Director (1950) then commented on how unhappy he was to be called a conservative since he was a 'traditional liberal' but went on, in fact to expand on the point at the end of Wallis' contribution to the effect that competition was essential to the system and so he supported intervention to restrict the formation of cartels. Then, noting that some production had to be by monopoly, he said that resulted in an unsatisfactory choice between private and public provision; that the free enterprise system could not provide itself with a monetary system, and government had to do that, and that Friedman would discuss that and the question of inequality. He went on, then, to discuss various existing market interventions expressing varying degrees of disapproval as he did so.

Friedman (1950b), having the role of discussing what the price mechanism could not do, listed the provision of public works and public buildings, along with a monetary framework, and the relief of poverty. The problem, he said, was to achieve these goals in ways that interfered with the free market as little as possible. On the monetary question he said that it was ironic that it was often said that the free-market system had a tendency to produce instability, when the Federal Reserve System had been so much responsible for the Depression. He said the monetary system needed to be reformed, and he set out his ideas along the lines of Friedman (1948a), including the need for a 'a definite rule' rather than the 'present uncertainty of monetary action on the part of authorities', and the proposal for fiscal policy to operate through automatic stabilizers (p. 13).

On the matter of inequality, his view was that the need for action was widely recognized. He said 'Some way must be found to help those people who draw blanks in the lottery of life' (p. 12) and 'No matter how perfect the market system is, there will always be some people who are just simply unable to get from it enough to support what society regards as a minimum level of living' (p. 13). He commented that various policies were meant to address this, but did so in ways which damaged the operation of the price system—listing rent control, tariffs, farm price support, minimum wages, and saying there were many more examples. Notably he thought that a guarantee of a minimum income was entirely warranted, but contrasted this with the guarantee of a minimum price for anyone's product. He also said that the serious political problems arising from redistribution would be better handled if the matter was decentralized, although he did not elaborate on that.

Friedman (1951/2013)—'Neoliberalism and its prospects'—was another short piece written as a contribution to the Norwegian business magazine *Farmand*. He began by citing A. V. Dicey's 'magnificent book', *Lectures on the relation between Law and public opinion in England during* *the nineteenth century*, Dicey (1905), saying that Dicey had argued that legislation lags twenty years behind a general change in opinion. Friedman asserted on his own account that most legislation of his time still arose from the earlier intellectual movement towards collectivism which Dicey had seen emerging. However, he continued that he thought the trend of intellectual opinion was changing, citing recent negative experiences of socialism and saying that since there was an opportunity to establish lines of thinking that would guide future legislators, it was important to be clear what view 'liberalism' offered.

He then gave an account of the origin of erroneous socialist thinking, saying that it arose from an underestimation of the difficulties of coordinating economic activity without the price system and the belief that there was much broader agreement on detailed objectives than there really was. These things led to the belief that 'one could achieve widespread agreement on a "plan" couched in precise terms and hence avoid those conflicts of interest that could be resolved only by coercion' (p. 2). Friedman did not say so, but those remarks were presumably not general lessons, but were intended to describe the situation in Norway, (in which case his picture is broadly consistent with that of Evan Lange and Pharo [1991]). Friedman's immediately following remark came out of the blue, however. It was,

> The means collectivists seek to employ are fundamentally inconsistent with the ends they seek to attain. A state with power to do good by the same token is in a position to do harm; and there is much reason to believe that the power will sooner or later get into the hands of those who will use it for evil purposes'. (pp. 2–3)

He also opined, with no evident basis that, 'The collectivist belief in the ability of direct action by the state to remedy all evils is itself, however, an understandable reaction to a basic error in nineteenth-century individualist philosophy' (p. 3). That error was the failure to see that individuals could combine to 'usurp power and effectively limit the freedom of other individuals' (p. 3). That seems rather a peculiar claim since nineteenth-century liberals were not so naïve as that. One wonders what Friedman thought of the idea of the 'tyranny of the majority', from Mill

(1859). But in any case, the nineteenth-century liberals surely saw a danger in trade unionism. As Friedman's piece developed it became clear that it was industrial combinations that Friedman had in mind, and his concern, like that of Director (1950) was to point to the value specifically of the competitive system. That system, he said, 'would seek to use competition amongst producers to protect consumers from exploitation, competition amongst employers to protect workers and owner of property, and competition amongst consumers to protect the enterprises themselves' (p. 3). The role of the state, then, was very much like that of the three authors of *A Positive Program for Conservatives* taken together: the maintenance of law and order, the provision of certain public works, ensuring the freedom to establish enterprises, and the provision of monetary stability. And he then added the function of 'relieving misery and distress' (p. 3) before concluding with a comment that a crucial aspect of all these was that they could be achieved by rule-governed policy, and a hope that the intellectual trend would indeed move in a neoliberal direction.

Friedman (1955d) and Harris (1955) were a pair of articles on 'liberalism' in Collier's Year Book, which were published as follow-ups to Kirk (1954) on 'conservatism', and were intended to represent two versions of liberalism. Whereas Kirk had—as seems natural for a yearbook—discussed current world events from a conservative point of view, both the liberals described the historic development of a line of thinking. For Harris, it started with Adam Smith, and passed along through the likes of Ricardo and Malthus, to Keynes and Roosevelt, thereby arriving at the mid-twentieth-century, left-wing, 'liberal' attitude in politics.

Friedman described the development of seventeenth- and eighteenth-century liberalism as emphasizing the importance of individuals' control over their own destiny, the political reaction against authoritarianism and support for competition and free trade. This he contrasted with contemporary American 'liberalism', which he said was more centralizing in its politics and 'distrusts the market in all its manifestations and favours widespread government intervention in, and control over, economic activity', and he cited Schumpeter (1954, p. 394) for the observation that 'as a supreme, if unintended, compliment, the enemies of the system of private enterprise have thought it wise to appropriate its label' (p. 390). Friedman said that political and economic liberalism derived from the same philosophy, but here he noted that they were not always associated—Russia and Japan being examples of countries that had developed as liberal economies without liberal politics, and Britain in the twentieth century as a country which had, until the last few years, drifted towards collectivism whilst maintaining most of the elements of liberal politics. He said that nineteenth-century liberals such as James Mill had argued that democratization would promote economic liberalism, and later, Simons, Mises, and Hayek had thought of economic liberalism as a way of facilitating the achievement of political freedom, without guaranteeing it.

He then presented examples saying they could clarify the relation between the two, 'though they cannot of course demonstrate it' (p. 361). One of these concerned the question of organizing political dissent in capitalist and state controlled economies. In the latter, it would be hard to acquire funds, and state-run enterprises might in any case not sell dissenters the materials. In a capitalist country, the profit motive would resolve that problem. Secondly, he noted that those who had resigned government jobs in the United States as a result of being accused of being communists had been able to find employment elsewhere, but if the government were the only employer, that would not be possible, and the exercise of freedom would thereby be constrained. He said that some authors—meaning Hayek (1944), obviously—had foreseen then-current tendencies towards collectivism as leading to serfdom, whereas what had actually happened was that those tendencies had been checked by opposition to encroachment of civil liberties and reported that there was one particularly striking example in the British experience with the direction of labour through the 'Control of Engagement Order'. He said,

> Socialist economic thinking in the postwar period called for compulsory allocation of labor to achieve 'social priorities'; though some compulsory powers were provided by law, they were never widely used; the powers themselves were permitted to lapse; and the whole character of attempted economic policy changed because compulsory allocation of labor so clearly interfered with widely and deeply cherished civil rights. (p. 361)

He turned to what he called a more detailed account of the content of economic liberalism and the role of the state, saying that the particularities would differ according to context, but the principles could be stated. These depended on the conception of people and the world and the liberal picture saw the freedom of individuals or families as the ultimate value and people as being more concerned with their own interests than those of others. These made voluntary exchange and the freedom to establish enterprises essential to the principled organization of cooperation. So, beyond such matters as the enforcement of contracts and keeping markets free, government had only three roles—handling natural monopoly; dealing with what he called 'neighbourhood effects'—meaning something like externalities, and protecting children and other 'irresponsible individuals' (p. 362). He elaborated on the first two in conventional ways and admitted that the third posed problems which to some extent needed pragmatic solutions.

At the end of the paper he turned to giving examples. One collection concerned straightforward regulation of economic activity through tariffs, exchange controls and the like. Another, concerning medical treatment, is noteworthy for showing his commitment to the position he was arguing. He said that there was a public role in dealing with contagious diseases and the like, but in the case of the treatment of individuals, there was no role. The market could perform that function. He considered the challenge that medical bills are unpredictable and said that the market could also provide insurance, and if people did not pay the premium, 'that is their free choice', and 'To the argument that people don't get as much medical service as is 'good' for them, the liberal will reply that each man should judge for himself' (p. 363). Individuals are not necessarily the best judge of their interests, said Friedman, but they should be allowed to make their own mistakes.

A second example perhaps shows both that commitment to following the logic of the argument, and an unusual idea. He considered slum housing saying that it imposed costs on society in terms of policing and fire protection. Since that was an externality it should be met with a tax. The usual idea that the poor should have subsidized housing, in contrast, could only arise from paternalism since if it were from concern for inequality, they would be given money. This led him to conclude that an objection to the liberal society that some felt was that it gives people what they want rather than what others think is good for them. And that rather rhetorical move was followed by a long quotation from Adam Smith also on the subject of the role of government in the system of natural liberty.

Then, in 1956, came the Volker Fund lectures at Wabash College—five of them, to judge by the collected papers. The first two were on the 'basic principles of liberalism' and the role of government in a liberal society. The third was on 'The Keynesian Revolution and economic liberalism'. That was interesting for its claim that Keynesianism created a climate favourable to 'collectivism' (p. 1), apparently because Keynes pointed to what he believed was a flaw in the price mechanism. That does not really make Friedman's case since Keynes (1936, Chapter 24 part III) also believed he had pointed to a remedy which he celebrated precisely because it required only limited extension of the powers of the state. But Friedman quickly passed on to argue that the Pigou effect meant that the flaw does not exist and thence to the question of the effectiveness of fiscal policy. On that he presented quite a long discussion of ideas and work that seem to be what became Friedman and Meiselman (1963). If that is so it is interesting because that piece of research must have been underway long before it appeared. The remaining two were then on the question of how to organize monetary policy (following the general line later put in Friedman [1959a]), and questions in the distribution of income and alleviation of poverty.

The lecture on the idea of liberalism—Friedman (1956d)—began with the point that the 'liberalism' that developed from the seventeenth to nineteenth century was quite different from the mid-twentieth-century kind in that it 'emphasized the individual as the basic unit in society, and freedom as the central goal in the relations amongst individuals, so that, 'In politics, it represented a reaction against authoritarian political regimes' (p. 1). Deciding to use the word in that, earlier, sense he said that economic and political liberalism were closely related and 'I know of no example in history of a country that has been politically free that has not also had economic liberalism in the sense that the major part of its economic activities were organized through a free and private market' (p. 2). There were he noted, cases of countries with economic but not political freedom, though the least politically free also usually had very little economic freedom. Of this, he said 'The reason seems clear' (p. 2) and was that capitalist activity provides sources of power independent of the political authorities.

He continued by drawing attention to the complexity of this relationship, saying that 'In early nineteenth-century England, the philosophical radicals and their allies regarded political reform as primarily a means of achieving economic liberalism'. They opposed the landed interest because laissez faire would promote economic development, 'and the wide distribution of its fruits'. For this reason, they promoted giving 'power to the people' who it was presumed would vote for their interests in the form of laissez faire. On the other hand, at the end of the nineteenth century, people such as Dicey, Mises, Hayek, and Simons saw economic freedom as a means towards political freedom. And of his own time, he said that whereas he had feared that increasing state intervention would lead to the suppression of political liberties, in fact there had been a slowing down of the growth of intervention, and he said that Britain again provided the best example, and repeated the substance of the story about Control of Engagement and the claim that the end of that mechanism had begun a reduction in state control.

Friedman went on to say that the threat to freedom came from the power to coerce and 'The preservation of freedom requires the elimination of such power to the fullest possible extent and the dispersal and distribution of whatever power cannot be eliminated' (p. 3), and to argue that economic strength could be dispersed and power thereby eliminated, whereas it was harder to do that with political power. That point really rested on his assertion but he said its force could 'be demonstrated best by example' (p. 4). The example was that of a hypothetical question as to how political support for capitalism might be organized in a socialist society. And then he again worked through his idea about the difficulty of organizing a political campaign in a socialist country. He said in this connection that one difficulty would be raising small sums of money from a large number of individuals and that, 'Radical movements in capitalist societies have never been financed this way' (p. 4), but by a rich benefactor. He contemplated the government providing funds but pointed to the difficulty it would have in deciding who should receive them, since if such funds are available, no doubt many would apply. Then he said that if a paper were to be printed, the capitalists would have to persuade the government printer to produce it.

In the course of this discussion he also noted that the patronage of the rich made possible by inequality is one defence of freedom. It is necessary for the advocates of a cause to persuade only one person to support it in order for it to be funded. He also said that allowing people to seek work where they like makes it possible for them to dissent from government views, and that an implication was that minority groups had much to gain from capitalism. In connection with that, he expressed puzzlement that members of those groups often felt, on the other hand, that the system was to blame for discrimination against them—that was a theme later pursued in Friedman (1984a), in relation to the question of why it was that Jews tended—in Friedman's view—to be averse to capitalism.

From there, the lecture moved to assert that the fulfilment of economic potential required the coordination of actions, and that this could be achieved only voluntarily or by compulsion. Since both parties to a voluntary exchange benefit from it, that system can bring coordination without compulsion. Exchange then makes possible the division of labour. And finally he said that the success of the system was so great that opposition to it grew from that success. He asserted that the system gave people what they wanted rather than what others thought they ought to want so that the real objection to the market was often actually an objection to freedom.

Obviously, there are various intellectual weaknesses in this lecture. There are historical questions Friedman slid over, such as whether the fact that the eighteenth- and nineteenth-century English liberals opposed the landed interest is quite the same thing as seeking the wide distribution of the fruits of economic growth; or whether what they supported is properly regarded as giving 'power to the people'. It is difficult to know exactly who Friedman had in mind, but on the other hand, easy to see liberalism as seeking the advancement of the capitalist and mercantile classes, rather than the industrial workers. The nonexistence of radical movements unsupported by wealthy philanthropists raises the question of what Friedman would count as a radical movement, (or as 'wealthy', for that matter) but there are the Friendly Societies, early trade unionism, and the Independent Labour Party to consider, for example. He surely picked up his information about the philosophical radicals from Dicey (1914), which he also cited in other lectures. That interpretation is only one. Brebner (1948) thought Dicey had philosophical radicalism all wrong, and William Thomas (1979) very much emphasized the diversity of the movement. Jones (2008), though, thought economic policy 'curiously peripheral' to their activities in Parliament.

The recurrence of discussion of the Control of Engagement Order is another peculiar aspect. As a mechanism of economic management, far from being the important matter Friedman made out, it barely warrants a footnote in the history of the immediate postwar period (cf. the brief discussion by Wilson (1952, pp. 237–238)). However, the Order was carried into effect, so Friedman's conditional remarks about such a contingency are ill-informed. But on the other hand it only applied to some (albeit many) workers, and so could not have given authorities 'complete control over the jobs men might take'. And certainly it was unpopular, including in the Parliamentary Labour Party, but the idea that its withdrawal, in 1950, 'ushered in' a shift of policy seems to have no basis at all. By that time a major reduction in control had already been implemented through Harold Wilson's 'bonfires of controls' of 1948–1949 in which, as Irving (2014) makes clear, an appreciation of their ineffectiveness was very much part of the motivation. Then, after the Conservative Party returned to government in 1951, more controls were removed, rationing ended, and there was some denationalization. None of that was anything to do with the withdrawal of the Control of Engagement Order. On the contrary, as Cairncross (1991, pp. 38–40) said, in official circles, the postwar controls were never seen as anything other than temporary. Hence there was no turning point in thinking and Friedman was talking through his hat.

One might indeed wonder how Friedman came to know about the arrangements at all, but the explanation must be that Rolfe (1954), who had been his student, and thanked him for help with the paper, wrote an account of the British manpower policy and described the Order at just about the time Friedman was preparing his Wabash lectures. A bigger surprise, perhaps is that Friedman missed some much better arguments that were available to him. One was that the Order arose in the context of other restrictions on the free market, including the fixed exchange rate, which made increasing exports essential. Increasing exports necessitated increasing the production of coal, and that made it necessary to move labour to the mines. Had Friedman argued that it was planning and control in other areas that led policymakers to additional interventions, he would have had a good case. He could also have argued, with merit, that trade union control of wages impeded their adjustment and created circumstances where direction of labour seemed necessary. And there was another point, argued by Roberts (1949) that he could well have made. That was that although few orders were issued, there was an unknown number of people who took jobs they did not want because of the possibility of receiving an order. Here, Roberts needed no instruction from Friedman, even thirty years before, having married and changed her name, she became Prime Minister.

Any of those limitations might be brushed aside on the basis that—perhaps—the lecture was quickly written, and clearly not a central part of Friedman's work, so that it is not too surprising if here and there it is a bit careless or ill-considered. And indeed, that could make it seem that my emphasis on the limitations of the lecture is eccentric. Indeed, such an excuse might, perhaps, seem appropriate for a lecture he thought unimportant, but there is more to it than that. First, the Wabash lectures were, according to Friedman and Friedman (1998a, p. 622 n5), some 'to which young academics were invited to hear lectures by leading free-market intellectuals'. So they were not (or not just) lectures to students, but apparently intended to change the minds of, or provide ammunition for, their teachers. And there is no objection to that, one might say, but the question is what then to say about Friedman's slapdash approach. The audience might have expected their leading intellectuals to produce lectures of sounder content than his.

But there is more to it even than that because much of the same material, with the same limitations, appeared in Friedman (1958g), which was his contribution to Morley (1958)—a collection based on a conference in September 1956. Much of the drafting was just the same, although it was 'England' rather than Britain that provided the most striking example. Adam Smith was then introduced and quoted as saying the 'division of labour is limited by the extent of the market'. Friedman interpreted that as his seeing the importance of the coordination of different people's activities. The same account was given of Control of Engagement which, 'despite great misgivings, the Labor Party found it necessary to impose' (p. 170). Then the same story told about organizing dissent in a socialist country, and the same point made about the interests of minorities.

After that, though, much the same thing appeared as Chapter 1 of Friedman (1962a)—*Capitalism and Freedom*, where he said, 'freedom of the individual, or perhaps the family' is the liberal's 'ultimate goal in judging social arrangements' (p. 12). That is a longer piece, and he added examples of specific freedoms that were or recently had been denied to the British and Americans, as a result, for example of foreign exchange controls, trade restrictions, or license requirements. He also observed that people living in fairly free societies were apt to forget how unusual they had been, saying that in the Western world of the nineteenth and early twentieth centuries the emergence of political freedom had clearly come along with the development of capitalism, and likewise did it 'in the golden age of Greece and in the early days of the Roman era' (p. 10). The same idea about the Philosophical Radicals was there, and a slightly more specific claim about them was made: 'They believed that the masses were being hampered by the restrictions that were being imposed upon them, and that if political reform gave the bulk of the people the vote, they would do what was good for them, which was to vote for laissez faire' (p. 10). Friedman commented that this was indeed what had happened. In the discussion of the 'control of engagements' order, the spelling of 'Labour' was corrected, but the basic discussion was the same. He introduced the thought that 'the liberal conceives of men as imperfect beings' (p. 12). His idea about the difficulty of organizing debate in a socialist system was there again, taking three pages. He then gave further examples supporting his case—the refusal of the BBC to allow Churchill to 'talk over the British radio' between 1933 and the outbreak of war because, as Friedman said, 'the BBC was a government monopoly and his position was too "controversial"' (p. 19). Friedman also added a discussion of the ways in which screenwriters who had been 'blacklisted' by Hollywood as a result of being suspected of communism, and argued that it was a desirable aspect of the system that they were able to find employment, including in Hollywood, as a result of its being costly to film-makers to reject their work.

So, most of the weaknesses of the earlier versions to which I have drawn attention were retained in *Capitalism and Freedom*, and some others crept in. To say that the 'masses' voted for laissez faire in Britain is very peculiar. It is difficult to know even what he meant, but according to Neil Johnston (2013) the first election in which even a majority of over 21-year-old men were registered to vote was in the twentieth century and so well past the age of laissez faire, in so far as there ever was one. The question of Churchill's broadcasts was rather more complex than Friedman seems to have appreciated or wanted to relate. He did not say so, but his facts and the point that Churchill would have been more likely to be allowed to speak if there were competition in broadcasting were probably lifted from Coase (1950, pp. 166–167)—a book Friedman said he admired.[1] Coase also pointed out, however, that BBC policy of the time was to achieve political balance and they did that by inviting the political parties to nominate people to appear. It was because Churchill was out of favour with his own party that he was not nominated. Friedman did not mention that. The claim that Churchill was not allowed to speak is also, strictly, incorrect, to judge by Robinson (2012, pp. 78–79) who discussed his broadcasts in the 1930s.

So we have, in other words, a succession of statements of the same sort of position, spread over more than a decade where, but for some additional examples, it is hard to see that Friedman's view developed at all. And that is certainly not to be explained by the sophistication of the view with which he started. The account of the Philosophical Radicals is facile, the introduction of Control of Engagement makes a mountain out of almost nothing, and there is in general no depth to the discussion at all. Ideas such as that 'the masses' voted for laissez faire are laughable.

[1] In a 1951 letter to G. L. Bach in the Friedman Collection at the Hoover Institution, Box 21, file 1.

In so far as it has any show of learning, it seems to have come from Dicey, Rolfe, and Coase. It is almost as if he is writing of some fictional land called 'England' where the facts of its history really are what the story teller says them to be. I suppose the material had enough to make it recognizable—'liberalism', 'the war', 'Churchill', 'the BBC'—and what was said about them all was thereby made to seem plausible. The extraordinary thing is that Friedman apparently thought this was a satisfactory way to go on.

Excepting the admiring noises of Friedman's supporters, the line of thinking has not attracted much specific comment, but Macpherson (1968) subjected it to critical analysis. He was no sympathizer with Friedman, but treated *Capitalism and Freedom* as the leading text making the extreme case for free-market liberalism and noted a collection of logical and factual flaws in it. Amongst them were the point that, contrary to Friedman, freedom of exchange is not freedom except when there is also freedom to refrain from engaging in any transaction; the historical evidence presented by Friedman could just as well show that political freedom was necessary to the emergence of the free market as the other way round. He also took issue with Friedman's purported demonstration that a socialist state could not secure freedom, noting that if it had the will to do so, there would be no difficulty. Friedman's argument assumed it lacked the will, and showed that in that case, it would have power to prevent the exercise of freedom. On that point he further noted that the government monopoly of employment was of little significance since a government which wished to suppress opposition would have plenty of ways of doing so other than making dissenters unemployed. The fact that existing socialist states had nearly all been established in developing countries, had encountered hostility from the west, and had become socialist through violent means all meant that they tended to be hostile to dissent. But Macpherson suggested that socialism achieved democratically in a developed country could be different; and finally commented on the incorrect interpretation of Marx that Friedman had advanced—'This nonsense is unworthy of Professor Friedman's talents' (p. 106), he said. If anything, Macpherson might be said to have been taking Friedman too seriously, but indeed, the argument in *Capitalism and Freedom* fell far short of what Friedman claimed for it, and there is really no sign that he was at all aware of it. Friedman received a more sympathetic treatment from Barry (1977, 1978), though he was filling in gaps in Friedman's argument for him.

A different sort of weakness, more in the structure of the argument than its components, comes in Friedman's idea about the difficulty of financing a political campaign in a socialist country. It surely is true that if all economic activity were strictly state controlled, it might be difficult to organize the production of any materials the state did not actually want to see produced, capitalist propaganda sheets amongst them. But except in so far as Friedman's objective is merely to show that such a fully socialized economy would have this characteristic, it is difficult to see what the argument achieves. It certainly does not show that a country with fixed exchange rates, or a tariff, or professional licensing, or even with coal rationing or a small amount of direction of labour is one in which political dissent is hard to voice. In so far as Friedman's goal was to argue generally against nearly all specific market interventions, he has an argument that each of them infringes freedom, and is for that reason undesirable. But the argument that complete governmental control would infringe freedom does not make that point. Perhaps, it might be said, he meant to say, along the lines of Hayek (1944), that any state interference would tend to lead to that extreme, and since the extreme is undesirable, any control must be. I suggested an argument like that it was available concerning the Control of Engagement Order, but Friedman did not make it. And indeed, in the version in Friedman (1962a) he clearly accepted some none-too-clearly specified interventions are required.

Nor is *Capitalism and Freedom* the end of it, because in 1978 *Commentary* magazine published a symposium with the title 'Capitalism, Socialism, and Democracy', on the idea that there is an 'inescapable connection' between capitalism and democracy. That was Arrow et al. (1978). Comments were published from 26 intellectuals of various dispositions and, as was no doubt intended, they expressed a variety of responses. Some thought there certainly was such a connection, some thought not, some disparaged the question. Students of each one's view would probably not be surprised by their responses, but most wrote with intelligence and several with panache. Friedman's contribution, Friedman (1978b), slightly longer than average, stands out, though, for the fact that it consisted of a string of quotations coming to about 1700 words from Chapter 1 of Friedman (1962a) and a few more introducing and presenting a quotation from Adam Smith expressing what Friedman took to be the same sort of view, and ended with Friedman's salutation 'Welcome aboard' (p. 41). It avoided the clear-cut errors of the earlier versions simply by not reproducing them, but about a third of it was concerned with the discussion of maintaining free discussion in a socialist economy. He seems to have been the only contributor who had nothing to say beyond what he had said before, and transparently the only one who thought that reproducing what he had said before made an appropriate contribution.

Then in the preface to the second edition of *Capitalism and Freedom*—Friedman (1982c)—he complained that when first published, the book's 'views were so far out of the mainstream that it was not reviewed by any major national publication', and said 'It is inconceivable that such a publication by an economist of comparable professional standing but favourable to the welfare state or socialism or communism would have received a similar silent treatment' (p. 5). The thought that anyone might have looked at the first chapter and thought it not worth reviewing was clearly not in mind, but more remarkably, he then went on to comment that even when the *Commentary* symposium was produced, only nine of the 25 other contributors were sympathetic to his book's central message. It is as if he thought the point was to address the arguments of his book whereas even the symposium's title is rather more suggestive of Schumpeter (1943) who, of course, had thought socialism and democracy compatible (Chapter XXIII). Did Friedman really expect a group of intellectuals who had been selected specifically to debate an issue all to agree with him? Did he really think that something so intellectually poor as Chapter 1 of his book was going to be what united them? And then parts of his book were published again in Walker (1988), with a discussion. That was another instance of Friedman reproducing bits of his book, whilst other authors produced new material. And the whole book was published a third time, as Friedman (2002), unchanged but for a new preface in which he celebrated the widening acceptance of free-market approaches and their success, and commented that the example of Hong Kong, before its return to China, had convinced him that 'economic and civil freedom' without political freedom was possible (p. ix).

In Friedman (1962/2017) he returned to Dicey (1914)—a 'great book'—as he called it (p. 54), and followed up with what he admitted to be more superficial history, but apparently thought that was a reasonable way of addressing his question of whether a free society is stable. The same attitude to the facts beyond his immediate knowledge shows in Friedman (1976b)—the one that said there was only a 50–50 chance of democracy surviving five years in Britain (p. 70, above). He began by saying that free societies were very rare, and,

> There was a small example in the 5th century, B.C., on the Peloponnesian peninsula, in Athens; but that was only a partly free society. It was a society that was free for the citizens of Athens, but not for the slaves who also inhabited the city. There is a brief spurt of freedom during the Renaissance in the Middle Ages. (p. 8)

He then said that the most extended period of freedom was in West Europe and the United States in the eighteenth, nineteenth, and early twentieth centuries (without introducing any qualification about slavery), and that was the only instance he wanted to discuss. Why, writing in 1976 he would have limited it to the 'early' twentieth century must be one question—was the idea really that freedom was gone by then? The Athens he was thinking about is no more in the Peloponnese than in Georgia—it is in Attica. One can only guess what point he imagined he had about the 'Renaissance in the Middle Ages', or what he would have to say about the Spanish Inquisition, or the Index Librorum Prohibitorum. The Renaissance was a period of creativity and learning, but to say it was of 'freedom' on any meaning approximating Friedman's is something else entirely. It is difficult to avoid the thought that all he had in mind was that the system allowed the likes of the Medici family to make an enormous amount of money.

The interesting point, again though, is not that Friedman made these mistakes, but that he again seems indifferent to the facts. The first publication of the piece was in the intellectual and literary magazine *Encounter* and it is a surprise of a kind that such mistakes survived the editorial and production process, but Friedman's insouciance about displaying his own ignorance is the remarkable thing. And all this surely must be put beside the extraordinary claim in Friedman (1962a, p. 3), that,

> The great advances of civilization, whether in architecture or painting, in science or literature, in industry or agriculture, have never come from centralized government.

after which he immediately noted that Columbus had been financed by a monarch, which would be pertinent, had geography been on his list, and named 14 other people he said had not worked to government directives. Indeed, as Popper said, it is easy to find confirmatory instances for many hypotheses. But did Friedman really think that private enterprise irrigated Mesopotamia and built the pyramids? What would he have to say about the Great Wall of China, the Sistine Chapel, or the King James Bible? Versailles? Perhaps Friedman did not think the Atom Bomb a great advance in civilization, but when it came along, a little later, would he perhaps have been willing to treat the moon landing as refuting his hypothesis?

It probably should be noted too that of all Friedman's discussions of freedom, the most satisfactory is one with a co-author. It is Friedman and Friedman (1988). It expands on the idea from Dicey that legislative changes are produced by changes in intellectual currents, and as the legislative tide reaches its flood, Dicey's countercurrents start to reverse the intellectual flow. It is no path-breaking work of scholarship, and it is a bit narrow in its conception of the issues, but it is an intelligent piece of writing, well informed on its sources, cohesive in presentation and pursuing a line of argument through its stages; it is properly referenced, and avoids blatant historical absurdity, whilst marshalling aspects of an outline of history to the cause of making the authors' case.

For the most part, though, beyond observing an association between controlled economies and repressive states, Friedman offers very little, and all in all, as much is nonsense or fiction as is insightful. Though he tries to theorize the association between capitalism and freedom, he never did so with any commitment to discovering the facts. In so far as he has a source for any of his ideas, it was Dicey, and one book by Dicey at that; for history—or geography—he seems to have written down whatever schoolboy recollection or half-appreciated tidbit happened to come to him. In the entire collection of Leeson and Palm (2017)—'Friedman on Freedom'—presumably selected as being, in some sense, the best of his writing on this topic, Friedman and Friedman (1988) stands out as one of two or at most three which are worth reading for their content, rather than merely for the insight they offer on Friedman's thinking, his mind, even more his limitations, but perhaps most of all, the fact that he seems not to have cared.

2 The Economics of Capitalism and Freedom

But if *Capitalism and Freedom*, Chapter 1, its precursors and derivatives, offers nothing more than a line or two of insight, and those not of any weight, the rest of the book is a different matter. Its second chapter concerned the proper roles of government in the economy. These were, as before, the maintenance of the rule of law and a monetary system; the handling of natural monopoly, which he called 'technical monopoly', and 'neighbourhood effects'; and acting to protect 'madmen or children' (p. 33).

Paternalism was not referred to again, the discussion of money was deferred to a later chapter, but monopoly and neighbourhood effects were addressed in more detail. Of the former, he explained the difficulty and said that the options were to tolerate private monopoly, to publicly regulate, or create a public monopoly, and suggested that although no general rule could be made, in a dynamic economy, the private monopoly might be best as it would be most responsive to change. Here, he argued that there was a good example arising from the operation of the Interstate Commerce Commission. He said it had been set up to regulate railway monopoly. But when competitors in the form of road and air travel had emerged, it started to protect railways against them and Friedman said that if it had never existed, transportation would have become highly competitive, which it had not. The general attitude of tolerance of monopoly, expecting it to be hard to sustain in the long term, is interesting since van Horn (2009) points out that it was a change from Friedman's earlier hostility to it, and certainly marks a break from the views of Simons (1948b)—or even Director, Friedman, and Wallis (1950)—and that line of Chicago thinking.

'Neighborhood effects' were defined as those arising when individuals 'have effects on other individuals for which it is not feasible to charge or recompose them' (p. 30), and he exemplified this by saying 'The man who pollutes a stream is in effect forcing others to exchange good water for bad' (p. 30). That is suggestive that 'neighbourhood effects' are externalities, but Friedman seems to have had a wider class of cases in mind since he then introduced the question of paying for roads. He said that for minor roads, it would be prohibitively expensive to collect tolls, but that taxing fuel was a sensible way of approximating a charge for road usage. Since the revenue could not be apportioned to particular roads, they could not be privately provided. On the other hand, for major roads, tolls were possible, so they should be privately provided. It is an interesting argument, and it fits the definition of 'neighborhood effect' in so far as charging is impracticable, though there is no externality causing the difficultly.

Still thinking about the practicality of charging, he then moved to consider National Parks, saying that most people regarded their provision as obviously a proper role of government. City parks, he said, might be publicly provided because it was too difficult to identify their beneficiaries, whereas toll gates at Yellowstone were perfectly practicable, and indeed existed, though charges were not nearly high enough to meet the costs of provision. He said,

> If the public wants this kind of activity enough to pay for it, private enterprise will have every incentive to provide such parks … I cannot myself conjure up any neighborhood effects or important monopoly effects that would justify governmental activity in this area. (p. 31)

Closing the National Parks was one of many provocative proposals in the book, but it is surely notable that Friedman's analysis is in terms of whether private provision would ever be possible, not whether the outcome would be efficient. There might indeed be no price at which enough people visited the Park to pay its total cost. But as Hotelling (1938b) argued, the requirement of efficiency is that consumers pay the marginal cost of their usage, not the total cost divided by the number of users. That creates another potential role for government in subsidizing the operation of such services. Friedman's failure to address this is presumably a mistake, but certainly leaves his analysis incomplete. Perhaps he would have said that the demands of freedom prohibit such governmental activities, but he would then have been admitting that his proposals would not achieve efficiency as it is normally understood in economics.

Two chapters then dealt with domestic and international monetary matters. In the first, Friedman's views along the lines of Friedman (1960a) were presented, and in the second, those from Friedman (1951d, 1953d). He then turned to fiscal policy. Considering macroeconomic arguments, he said that large fiscal expenditures had been argued to be justified by depression, then by the threat of secular stagnation, then by the necessity of managing the business cycle. The first two were no longer serious issues, but had left a legacy of high expenditure, and the third had a tendency to expand the budget because in practical terms expenditures were increased more in recession than they were reduced in booms. He suggested that if, instead of varying expenditures, the practice was to vary taxes, outcomes would be very different—it would be easy to cut taxes in recessions, and harder to raise them in booms, thereby creating political pressures for smaller, rather than larger, government. It seems he must have had it in mind that government finances would stay within range of being in balance, or he would have been led to fear ever-growing debt. He questioned the size of the Keynesian multiplier on theoretical grounds, saying that the simple calculations presumed that, for example, changes in government expenditure were not at all offset by changes in private expenditure. This would be true only if the private sector were indifferent as to whether they held money or bonds, or their spending was unaffected by the interest rate, neither of which was something any reasonable economist believed. Friedman noted that the 'extreme assumption, implicit in a rigid quantity theory of money' (p. 83) led to the conclusion that the offset would be complete. Having done that, he pointed to the work of Friedman and Meiselman

(1963) as showing that the actual outcome was closer to the Quantity Theory extreme than the Keynesian one.

He considered the role of government in education in a chapter which was very much like Friedman (1955e). He said that at higher levels of education, the matter of neighbourhood effects becomes insignificant, citing Becker (1960) and Theodore Schultz (1961), but that there was underinvestment, and attributed that to imperfection of the capital market making it hard to finance education. Neither of the sources gives terribly strong support to that picture, but it had also been a conclusion of Friedman and Kuznets (1945). In any case—also repeating an idea from that book (p. 90 n1)—Friedman considered whether it really made a case for state involvement. He suggested instead that it would be possible for individuals to sell a portion of their future income and thereby finance their education in the manner of an issue of 'equity', rather than 'debt'. So long as the costs and benefits were correctly calculated and so long as the scheme was fully enforced, and self-financing, individuals would then choose what they regarded as the optimal amount of education. Friedman noted there were various practical difficulties, and said that it would be better for the private sector to develop the scheme, but clearly did contemplate state involvement in creating the possibility (p. 105). Here, then, he did recognize an efficiency issue and the idea of 'equity finance' for education was hailed as a great and prescient contribution by Barr (2016).

Friedman's chapter, though, is surely best remembered for its presentation of the 'voucher' scheme for basic schooling. Noting that there is a public good aspect to basic education, he accepted the case for state finance, and drew attention to the point that this does not require direct state provision. The provision could be privatized and parents given 'vouchers' for a sum of money to pay for education, with the option then for them to add to that amount, whilst the schools could be profit or non-profit institutions needing only to be approved by the government as providing the kind of education which was properly publicly financed. The advantages were mainly in terms of enhancing choice over schools, and Friedman specifically drew attention to the difficult position under the existing system of those residents of poor neighbourhoods who had particular educational ambitions for their children, but lacked the means to take them outside the state system. They had no option but to accept the local school.

The next chapter was about discrimination generally and is not the most distinguished. Considering racial discrimination, he noted, following Becker (1957) that those who discriminate impose economic costs on themselves, and that, as Becker had also suggested, preferring to look at a beautiful person rather than an ugly one is not normally called discrimination, but a preference for living near people of one race rather than another is. From this, Friedman concluded 'It is hard to see that discrimination can have any meaning other than a 'taste' of others that one does not share' (p. 110). Invoking the idea of freedom, he argued that it was improper for the state to restrict decisions people took because of matters of taste and drew an analogy between the question of discrimination and free speech, saying that the case for the latter was aversion to the view that 'momentary majorities' (p. 114) would decide what it was acceptable to say, and equivalently, momentary majorities should not determine which characteristics are relevant to employment.

Not for the first time, a fissure is apparent between Friedman's precisely aimed arguments in economics, and his ham-fistedness as soon as he is outside that realm. In Becker's treatment the 'taste for discrimination' was specifically defined as whatever leads a person to 'act as if he were willing to pay something either directly or indirectly … to be associated with some persons instead of others' (p. 14) and that definition was invoked to avoid the 'philosophical issues' (p. 13) arising from trying to distinguish discrimination from taste-based behaviour. He was arguing 'as if', giving a concept an operational definition prior to using it to understand the data, and thereby sticking to the positive rather than normative aspects of the issue. Friedman should easily have recognized the device. When he expressed a difficulty in seeing what the difference between taste-based behaviour and discrimination was, he did not thereby remove the problem. And since the difficulty he was having was in drawing a normative distinction, his approach is also no way of keeping a sharp distinction between positive and normative enquiries, which Friedman sometimes held to be so important. On the contrary, it is precisely a way of blending the two, pretending one to be the other.

Supposing that Friedman was not going to follow Collard (1972, p. 789) in regarding Becker's approach as inadequate to the problem, he could still have found reason for intervention had he so wished. For example, he had accepted—of course—that an individual's freedom to harm others is properly restricted. But it should be easy to see that violent harms are not the only kind. Consequently, once the propriety of restricting harm is admitted, there is going to be a grey area as to exactly what is permitted and what is not. In particular, he might, for example, have considered a society where discrimination is reasonably widespread, yet deprecated. Then, one person's discriminatory actions tend to create a social environment increasing other's willingness to act in the same way. There is then harm done to someone who was not involved in the original transaction. That case meets Friedman's definition of a neighbourhood effect and so justifies action. But he either did not want to, or did not think to, pursue such lines of argument.

Chapter 8—rather a mish-mash—returned to the matter of monopoly, which had been considered in Chapter 2, and the 'social responsibility' of business and labour. On the former, this time Friedman argued on the basis of work by Stigler (1949) and Nutter (1951), that it was much less prevalent than often supposed, and that much of the monopoly power that did exist was in one way or another created or supported by government. He did, later and elsewhere—Friedman (1973a)—admit the possibility of harmful monopoly, and even of government-sponsored monopsony to countervail it! He said it was also true of unions that they had much less monopoly power than was often supposed. But drawing on Friedman (1951a) he said that they did raise wages in some sectors, although ones that would have been highly-paid anyway, and thereby lowered them in others. And he also said they sometimes promoted effective monopolization of a product market. Pointing to the behaviour of the mining unions in the 1930s, he argued they were able to stop production whenever stocks were high enough to threaten a fall in the price of coal, with the benefits of the higher prices being shared between firms and workers. Naturally, he said that government support of monopoly should end and that unions should be subject to anti-trust laws, but did not indicate how they would then function.

In addition to those points, he said that corporate tax should be abolished and shareholders taxed on undistributed profits. That was only rather loosely connected to the issue of monopoly, but the idea was that since reinvested profits were not taxable, firms were encouraged to make below-par investments rather than distributing the profits. From there, he drifted further into making the point that graduated income tax systems encouraged tax avoidance and that such avoidance should be prevented and high tax rates reduced (pp. 132–133).

Then, and briefly, he discussed the 'social responsibility' of business and unions. He said that business had one social responsibility: 'to use its resources and engage in activities designed to increase its profits so long as it stays within the rules of the game' (p. 133). Unions, similarly, had a responsibility to further the interests of their members. On the theme of the social responsibility of business, he quickly asked a string of rhetorical questions as to how businessmen are supposed to know what their social responsibility is, whether those individuals could decide what the social interest is, whether they could decide to take that responsibility on themselves, and whether it was tolerable that public functions of taxation and expenditure be exercised by people who happened to be in control of business.

In response to his own question he simply asserted 'If businessmen are civil servants rather than the employees of their stockholders then in a democracy they will, sooner or later, be chosen by the public techniques of election and appointment' (p. 134). But then immediately, and not quite consistently, he said that long before that happened their decision-taking power would have been taken away from them and 'A dramatic illustration was the cancellation of a steel price increase by U.S. Steel in April 1962 through the medium of a public display of anger by President Kennedy and threats of reprisals on levels ranging from anti-trust suits to examination of the tax reports of steel executives' (p. 134). Friedman's conclusion was that the incident showed how much power there was in Washington and that the powers required for a police state already existed.

Apart from having a peculiar idea of a police state, his account of events was incomplete. The price increase announced by U.S. Steel was matched within a day by several other producers, so an anti-trust investigation might well have been appropriate, and Friedman might have welcomed that, especially considering his remarks earlier in the chapter. Other producers, however, did not raise prices and those that did reversed their decision within a few days. On that, Friedman might have argued that the presence of fringe of non-colluding firms that made the price increase impossible. He also did not say any more about why he felt business executives would find their decision-taking power had been taken away from them, except that the comment about a police state implies an answer. In a journalistic account of the incident, Hoopes (1963) reported the allegation that tax audits were to be made, and the denial of it. It would be interesting to know where Friedman learned it to be a fact, since this was not noted by Sheahan (1967) or Barber (1975) and in his *Playboy* interview he retreated somewhat, saying that the threat was 'implicit'.

What he did was go on immediately to say that the case exemplified the attitude that business and labour had a social responsibility to set prices and wages to keep inflation down. That, of course, was a response to the idea of cost-push inflation, to which Friedman was so much opposed. He did not acknowledge any thoughtful basis for the idea, but simply said than an attempt to control inflation by such means would lead to shortages which would have to be resolved somehow, and then,

> Price controls, whether legal or voluntary, if effectively enforced would eventually lead to the destruction of the free-enterprise system and its replacement by a centrally controlled system. (p. 135)

and commented that they would still not stop inflation, as that depends on the money supply.

Then in less than a page of Friedman (1962a) he turned to broader issues of corporate social responsibility. He denied that corporations should give donations to universities, rejected the idea of allowing corporate charitable donations to be tax-deductible, and said,

> A major complaint made frequently against modern business is that it involves the separation of ownership and control – that the corporation has become a social institution that is a law unto itself, with irresponsible executives who do not serve the interests of their stockholder. (p. 135)

This, he said, was not true but that the policy of allowing a tax deduction for corporate charitable gifts was a step towards changing that. Like cost-push inflation, the concern over the separation of ownership and control, brought to the fore by Berle and Means (1932) was a real one, not to be subjected to an unreasoned dismissal by Friedman in five words. But as is apparent, his whole discussion of the matter was intemperate and poorly reasoned.

He returned to the issue of corporate social responsibility in Friedman (1965c, 1970i). The second of those became his best-known contribution on the matter and a subject of great debate. Its presentation was somewhat calmer that the earlier version, although again with a focus on the question of price control and barely a mention of any other aspect. His premise was that a business executive works for the shareholders and has a responsibility to secure their interests. He noted that some actions which were well justified on profit-seeking grounds were sometimes described in terms of social responsibility, and said that he had no objection to that. Naturally he was much more interested in the point that if 'social responsibility' really meant anything, it must be that the business is to act contrary to the shareholders' interest. Since profits were being reduced, this meant that the managers were in effect imposing taxes on the owners and deciding how to spend the revenue. He then raised the same sort of issues as previously about how the executive was to know how to serve the social interest that was the object, and in this referred exclusively to the case of controlling inflation. Switching attention to the case of unions, he pointed to the difficulties that would be faced by union leaders if they tried to act to control inflation, rather than in the interests of their members.

Then, he said that the idea of social responsibility threatened the free society, and expanding on this gave the example of the short-sightedness of businessmen who gave support to the idea. He said they were contributing to the view that profit is wicked and that when that view was adopted 'the iron fist of Government bureaucrats' would control business, so that 'Here, as with price and wage controls, businessmen seem to me to reveal a suicidal impulse'. At the end of the piece he said that the idea of social responsibility, 'does not differ in philosophy from the most explicitly collectivist doctrine. It differs only by professing to believe that collectivist ends can be attained without collectivist means' and repeated the claim from Friedman (1962a) that it was 'fundamentally subversive'.

On the general matter of 'social responsibility', one important family of issues, exemplified in Aune (2007, p. 212) for example, would be that customers might welcome knowing that they are buying, for example, from a firm which does not employ sweat-shop labour. Friedman's answer to that, of course, would be that if the indicated policy is going to attract customers and thereby maximize profit, it is nothing to do with corporate social responsibility. The difficulty is that once we reach that point, it is apparent that it is going to be very hard to know for sure whether such things are in fact profit maximizing, and a patchwork of grey areas appears. It might be, for example, that consumers welcome the effort to behave in ways they regard as socially responsible, without seeing any specific standard as essential. Or it might be that certain 'socially responsible' behaviour by firms gives them an opportunity for effective advertising. Such considerations suggest that Friedman's hyperbolic conclusions about the 'suicidal' tendency of businessmen and the 'fundamentally subversive' threat of the idea are out of place.

Here, though, there is a little more to Friedman's position than might sometimes be made apparent. No one is really likely to feel the free society is undermined by corporate provision of community facilities, whether they are strictly justified by the profit-enhancing attraction of labour to the area or not. The big concern in the discussion of corporate social responsibility in Friedman (1962a) to judge by his splenetic treatment of it, was Kennedy's intervention over steel prices. Indeed, the fact that the book was published in the same year as the steel controversy suggests the possibility that this is what led to the inclusion of the discussion. Similarly, in Friedman (1965c) and Friedman (1970i), the real concern seems to have been when 'responsibility' moved into areas with a profound relation to the market system—most particularly and overtly when decisions were to be taken in the cause of controlling inflation. *That* was the acceptance of a kind of responsibility well beyond the normal goals of the firm, and one that invited a change in societal attitudes or policymaker expectations. It promoted the idea of corporations acting as agents of the state, in pursuing whatever were its current goals. Seeing the danger there, I think, makes sense of his idea that it was fundamentally subversive, even if that is still an extreme view. But the crucial matter—and the one to which Kennedy contributed—is not the managers 'taxing' the shareholders, but the abridgement of the mechanism of wage and price setting. Friedman, though, did not make the point at all clear.

Friedman (1962a) then moved to the matter of the licensing of professions, objecting to that as an infringement of freedom of contract. Raising a comparison with the Medieval guilds and even the caste system, he cited Gellhorn (1956) for the large number of occupations that were subject to licensing, including, at the more absurd end, dealers in scrap tobacco, 'yacht salesmen, tree surgeons and well diggers' (p. 139). Then, Friedman distinguished registration, certification, and licensure as possible governmental activities. The first involves a requirement that those practising some activity register themselves as doing so; the second appends to the register information as to their qualifications; the third requires certain qualifications as a condition of allowing practice. Obviously various justifications might be given for each, but Friedman's main interest was in the last since that is the one where the requirement restricts individuals' rights of contract. In discussing it, he chose mainly to focus on the medical profession, arguing that people should be allowed to practise medicine without a requirement of a government license.

It is obviously a provocative position and Friedman said he was taking on the most difficult case. He was again following a line of thinking from Friedman and Kuznets (1945)—indeed it was at least very closely related to the one that caused Reinold Noyes such concern—but it makes for one of the best-put arguments of the book. He said the American Medical Association operated as a trade union, and like any union its interests would be damaged by the existence of a fringe of non-union members who were allowed to practise. The licensure rules, however, made it free of such competition. He also described what he thought was a web of further control over training and hospitals that depended on licensing, and then moved to ask whether it was even effective in maintaining the quality of medical care—the ostensible justification of the rules.

He argued that the restriction of the number of physicians meant that some care—in effect medical care, but described as osteopathy, for example—must be being given by people other than doctors and the true quality measure should incorporate their care (he also made the point that some people were going without care altogether). Entry restrictions also reduced innovation since research and experimentation was usually limited to the licensed. And he even suggested that quality was harmed by the difficulty of bringing a malpractice suit in circumstances where expert evidence could in practical terms only come from other licensed physicians who tended to be reluctant to give it.

Finally, he considered the question of how the system would operate if there were no licensure. Here, his point was that thinking simply in terms of what patients might do if, as it were, the licensing rules were abolished and nothing else changed was inappropriate. The proper question was that of what else would change in that circumstance. He suggested that medical partnerships might form, and they would have a powerful interest in maintaining their own reputations and would then operate in the manner of department stores, giving their customers reassurance as to the quality of the product they were selling.

Friedman's conclusion was a strong one. He said,

> I am myself persuaded that licensure has reduced both the quantity and the quality of medical practice; that it has reduced the opportunities available to people who would like to be physicians, forcing them to pursue occupations they regard as less attractive; that it has forced the public to pay more for less satisfactory medical service, and that it has retarded technological development both in medicine itself and in the organization of medical practice. I conclude that licensure should be eliminated as a requirement for the practice of medicine. (p. 158)

He had indeed made arguments suggestive of that conclusion, and had one or two indicative pieces of evidence, though he was nowhere near demonstrating his case. But then nor did he say he was. He said he was himself persuaded. Even that might be intended to be provocative, and no doubt also rhetorically persuasive. But taken at face value, the contrast with the bald claims about discrimination, or even more of the previous chapter or his other works on corporate social responsibility, for example, is stark. On medical licensure, avowedly taking on a challenging case, he presented a string of clever arguments, each one making a worthwhile point, and insisted on no conclusion at all, but merely reported his own response. It is a fine piece of entertaining writing, intelligent commentary, and a well-judged provocative argument, rolled into one.

The final three topics of the book were then on the related issues of the distribution of income, social welfare, and the alleviation of poverty, and all three drew to some degree on Friedman (1956e). Together they combine the strengths and weaknesses of the book very clearly—there are Friedman's clumsy attempts at general, philosophical reasoning, combined with his sharp eye for possible unconsidered effects of governmental actions, and a lively scepticism about their benefits. An illustration of the first comes from his remarks on the relation of inherited and acquired wealth. He said that it was widely held that it is essential to distinguish them, but 'This distinction is untenable' (p. 164). He raised the question of the ethical difference between someone who becomes rich through inheriting a valuable talent from their parents from someone who inherits financial wealth. Indeed, there is a question there. But what Friedman has shown is that there is no clear-cut distinction. He has not shown that there is no basis for treating the two—or certain types of the two—differently. He has pointed to a problem, a difficulty, and declared it to destroy a distinction. That is not even to engage with the problem, much less to bring a philosophical resolution to it.

On the other hand, his observation that some career choices seem to be more-or-less conscious gambles by people who have safe options available is suggestive of a 'taste for uncertainty' (p. 163) and the question of whether the state should overrule that is answered by his well-aimed question as to what attitude one should take to a group of people who start with equal endowments and choose to gamble until they are unequal. That, of course, was a line of thinking suggested by Friedman (1953f), though there he was less pointed about it. That line also led him to suggest that the progressivity of taxes actually observed is a consequence of voters having a good idea of their economic position when they vote. He speculated that if the only votes were on tax schedules for future generations, there would be support for much less progressivity—it was one of the many precursors of the 'veil of ignorance' of Rawls (1971), though Friedman came to something like the opposite conclusion from that author as to how it would affect choices about distribution.

Continuing on the theme of inequality, he commented that high taxes make pre-tax distributions of income more unequal by discouraging entry into high-paying activities; and that they promote tax evasion, making effective tax rates lower than they seem, and their effect capricious. High taxes on income inhibit the accumulation of wealth and that makes it harder for people to become wealthy, but also disincentivizes risk-taking by those who are wealthy. He suggested that a single tax rate levied on a wide base would raise more revenue than the actual graduated taxes. The assertion that minimum wages cause unemployment was ordinary enough. Friedman, though, also contrasted the idea that such regulations were supported by people of goodwill, with the thought that it was northern firms, threatened by southern competition, which supported them. He also criticized various specific programmes of his time for their inefficiency in achieving stated goals, capricious distributive effects, and illiberalism.

And finally he came to the proposal of a 'negative income tax'—another idea that was to become a memorable proposal of the book. Some threshold would be established and individuals (or families, if the tax system is so organized) with an income below that level would receive a payment of a certain percentage of the difference between their income and the threshold amount. A person with no income would therefore be guaranteed a certain amount, and if they then earned more, they would lose a portion, but only a portion, of the payment they were receiving. Advantages of the scheme are that it is well-targeted at poverty, it provides money, rather than benefits in kind, and does so without a complete removal of incentives to earn. And according to Friedman's quick calculations, it would be much less expensive than existing programmes. He said, notably, that it had a disadvantage in that since it made the redistribution so transparent there was a political risk that the figures would be changed to benefit a large number of people. Friedman said 'I see no solution to this problem except to rely on the self-restraint and good will of the electorate' (p. 194). It is a very limp response from someone usually so keen to insist on taking the incentives operating on people so seriously. Had he been looking to criticize the idea, rather than in advocating it, it is hard to imagine that same point would not have been presented as damning it.

When he returned to the proposal in Friedman (1968e) he gave it a slightly more detailed treatment, added as an advantage that it would reduce bureaucracy and thereby the possibility of using the bureaucracy for 'political patronage' (p. 213) would be eliminated, and dealt with some superficial disadvantages before considering the same question of the danger that the threshold below which income tax was negative would tend to rise. This time he said, reasonably enough, that the relevant question was whether the negative income tax was more susceptible to this danger than existing programmes. He noted that in *Capitalism and Freedom* he had treated this as a disadvantage and said that he had changed his mind. His revised view was that because the scheme was so tied into general income taxation, it would be obvious that increasing the threshold would make it more expensive. Furthermore, so he argued, since it did not require a large bureaucracy to implement the scheme, there would equally not be a bureaucracy with an interest in expanding it. Certainly the first of those is so much in character with the arguments of the earlier book that it is a wonder he did not think of it in 1962! The book then ended with a reprise of government failures, and the hope that the intellectual tide was turning.

3 Reactions to *Capitalism and Freedom*

Despite the praise that has since been heaped on it; despite, indeed, the very large number of copies sold, it is very much a mixed book. Certainly it was, as Bowman (1963, p. 1474) said, 'evangelical and uncompromising'. No doubt as a result of that, some readers did not appreciate it at all—Keyserling (1963) was one. He dismissed the book as not offering economic analysis, but as deducing its conclusions from the proposition that freedom is the most important value. Kilgour (1964, p. 504) was another. All he saw was that Friedman wanted to turn the clock back by abolishing various activities of government and commented 'The only thing is that it won't work, never has worked, never will work. It is absurd today to write books like this, unless they be written in novel form'. No hint there of Friedman's concern with finding effective responses to poverty, or improving educational opportunities, or choice in medical care, or of eliminating over-regulation of interstate transport, or even improving monetary policy. And Baran (1963) was equally hostile, though in a different league in understanding the issues. He appreciated Friedman's arguments very well, as arguments of a basically conservative disposition, presuming that the existing economic order would remain. So Friedman's proposals were 'radical' only in very limited ways that presumption allowed. He argued that Friedman failed to recognize the deep failings of the capitalist system in providing opportunities, and being dominated by monopoly and cited Simons (1934) against Friedman saying that if his policies were implemented, government would be larger than it was.

Others saw much more in the book. *The Economist* (16 February 1963), called the book 'A tract for the times', and whilst noting it took some extreme positions, and was naïve about the remedies for poverty, also saw great merit, saying of the discussion of medical care, that Friedman's argument was 'devastating'. It also had a good hearing from Lerner (1963, p. 459) who judged the book very well, saying that in spite of its 'extravagances', 'I find myself in enthusiastic agreement some 90% of the time. For the book powerfully demonstrates an impressive number of ways in which both freedom and welfare could be increased by a fuller utilization of the price mechanism', and praised particularly his analysis of the American Medical Association's 'self-virtuous conspiracy against the American public', his courage in arguing for flexible exchange rates, and his analysis of monetary issues, despite, as Lerner put it, 'his inability to credit governmental authority with the power to learn to avoid even 'inexcusable' misjudgements'. Of Friedman's 'antigovernment complex' though, he also said that it was to be deplored not for sometimes leading to poor proposals, but for its 'inhibitory effect on potential readers of an important book' (p. 460). That, very probably, was too true.

Similarly, Boulding (1963, p. 120) saw the book as first and foremost a ‘plea for the moral value of exchange and the institutional framework of the market as an organizer of social life’, although he too thought it likely not to reach those who most needed its message, and noted limitations in over-emphasizing the uniqueness of exchange in social interaction. Hicks (1963), likewise, saw much merit in the economic arguments, particularly commending those on education and even saying of the question of medical licensing that Friedman made a case ‘from which much is to be learned’ (p. 320).

Breul (1963) perhaps wrote the best review. He declared himself as having started the book ready to rebut its antique arguments made by an economist known to associate himself with the economic thought of the nineteenth century, but had found something unexpected. Friedman did not assert that the market would eventually eliminate poverty, but called for state action to do that; and every time he pointed to the failure of intervention to achieve its goals, he had an alternative policy to propose that would achieve them better. Existing arrangements being, as Breul clearly thought, a shambles, he hoped that Friedman’s book would start a new Poor Law debate with, of course, better outcomes than that of the 1830s.

Indeed, up to a point Friedman’s plans do minimize state intervention, but more importantly, they use market mechanisms, rather than those of control, to achieve much the same objectives as the proponents of control. It is just the sort of manifesto hinted at by Friedman’s various remarks about differences of opinion being over scientific matters rather than objectives. The book is very much a *positive* plan for economic freedom.

By the same token, his moral ‘liberalism’, even in so far as he could articulate it, is mostly irrelevant. Scepticism of the effectiveness of government action is quite a different thing from the view that for the government even to attempt to intervene in private economic relationships is improper. In any case, despite the fuss made about it, the latter idea had only a minimal role in Friedman’s arguments. In the first place, most of his arguments simply present his ideas as good ideas; proposals that would make things better for people. He had an attitude of mind which made it natural to see the ways in which more or less rational people, more or less looking out for themselves, will be led to behave in a way which will deliver very satisfactory outcomes. And his book is a great advertisement for this attitude of mind as providing a productive way of assessing social programmes. But that is an attitude of mind, not a philosophical principle.

All this clearly shows that Friedman thought 'freedom' important, but even in terms of articulating his reasoning, he had some difficulty. *Time* was told that he thought it the 'supreme good': 'His basic philosophy is simple and unoriginal: personal freedom is the supreme good—in economic, political, and social relations. What is unusual is his consistency in applying this principle to any and all problems', it had reported (see p. 26, above). In *Capitalism and Freedom* itself it was said that freedom of the individual or the family was the 'ultimate goal in judging social arrangements' (p. 18) for the liberal.

But that is obviously not true of Friedman. There are points in *Capitalism and Freedom* where the overruling of personal freedom is accepted, as of course there must be. In fact, in Friedman (1988a) he was not only clear, but a little irritated about it, correcting Richard Cooper by saying,

> No, no, I'm afraid you've misstated my position. You stated it correctly earlier that I would be willing to trade off some economic prosperity for freedom of capital movement. Now you've tried to put me into a position of saying that I would trade off any degree of reduction in prosperity … I don't want to make a lexicographic ordering; I simply want to say that I put a very high value on human freedom, but there conceivably can be trade-offs. (p. 108)

So it is one value amongst many.

More important though, Friedman never really accepts that there is much of a tradeoff to be made. Even in his discussion with Cooper, he did not actually propose making any compromise of freedom. Actually, his position is almost always that freedom—or the free market, generally—delivers the best available outcome. It is where there might be doubt that he avoids the issue by an appeal to freedom. But even that happens rarely. Since he does not allow himself to be confronted by a tradeoff and so does not take seriously the challenge of some competing value, there is no occasion to test the question of how important freedom is. In this, *Capitalism and Freedom*, might be compared with Wallich (1960)—a contemporary book, written by an economist of something of the same disposition as Friedman. His title was *The Cost of Freedom*. Wallich doubted that the Soviet system was necessarily less effective than the American, particularly in generating growth, but clearly said that freedom was more valuable than the extra economic benefits that might be available. He might have been wrong, and Friedman right, but Wallich is the one who exhibits a *preference* for freedom, whilst what Friedman does is declare that preference, and then find that it brings the other benefits he seeks. Friedman really has nothing in the way of philosophy at all.

21

Newsweek and Journalism

Newsweek provided a fabulous outlet for Friedman's views. He wrote just over 300 pieces at the rate of about one every three weeks from 1966 to 1984, initially taking turns with Samuelson and Henry Wallich, then with Samuelson alone, then with Lester Thurow. There was one last piece—Friedman (1986b)—in which he self-effacingly celebrated the vindication of his 1974 prediction of a fall in the price of oil, and said that it was much easier to foresee the direction of change than the timing! He wrote another few dozen pieces for the *Wall Street Journal*, and a few dozen more for the *San Francisco Chronicle*, and *The New York Times*, nearly all of them after 1984. They were much less frequent than the *Newsweek* columns, but many were of a similar length and character and so probably in his mind were something of a substitute.[1]

[1]The information in this paragraph about where, when, and how often Friedman wrote relies on the Hoover Institution Collected Works of Friedman, as do most of the specific citations and quotations following. In 1979 *The Chronicle* also ran a series of excerpts from lectures he had given in the style of the later chapters of *Capitalism and Freedom*. There is a scattering of other articles as well of course—the database lists 464 pieces in newspapers (not counting letters to editors).

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_21

Many of these, mostly from *Newsweek*, were republished, sometimes more than once in Friedman (1972c, 1975f, 1983). In all cases, the articles were arranged thematically with short introductions to the theme, in the first two cases written by Friedman, and in the third by his admirer William R. Allen, who had persuaded him to allow the publication of the volume. The first book also included more general comments by Friedman extracted from a round-table discussion, but the latter two began rather better with Friedman's interview in *Playboy*, which is probably Friedman's best account of his views and outlook up to that date.

All these pieces are short and most of them arise either from current events or some particular experience Friedman had had. They are all opinionated and naturally enough they push Friedman's views. So there are recurring themes, and frequent appearances for the importance of the quantity of money, the incentives of regulatory agencies leading them to action contrary to the interests of consumers, the failure of government programmes, the undesirability of pegging exchange rates, and the power of lobbyists for special interests in promoting restriction of the market. The articles do little to really respond to other points of view, but they are too short to do that. What many of them—the best of them—do achieve is a highly effective presentation of the reasons to think as Friedman does. Some provide little lessons in economic principles; some are gems of Friedmanesque reasoning on what the true effects of a policy will be; some have imaginative, market-based ideas for overcoming problems; and particularly in the 1970s, a good number make a running commentary on unfolding events, especially in relation to monetary policy. As says his reputation, he showed no reluctance to criticize the powerful—oil companies for fabricating reasons they should be subsidized in Friedman (1967d); the Pope for presuming economic development should be planned in Friedman (1967e), and of course, the Federal Reserve, again and again. A sufficiently determined opponent of Friedman's views would no doubt find them irritating and declare them facile, but one would really have to be very determined indeed not to see them at least as capturing an aspect of a problem, or raising a serious concern about why some or other government action was not working out as planned.

They are uniformly critical, direct, sometimes even witty, and always right on the point that Friedman wanted to make, often ending with a ringing final line. Most of the very best come when Friedman has a clever idea and a clever and clear explanation of it for his readers. An outstanding case was Friedman (1967f) in which he protested about legislatively enforced car safety standards, which no doubt arose from Nader (1965). He said the new requirements raised the cost of car production, and could be thought of as a tax on cars, with the revenue spent on car safety. He said the effect could then be seen as a delegation of authority to tax to the body responsible for safety-regulation; there had been no comparison of what improvements in safety could be achieved with that much money in other ways; the setting of standards would come to be overly influenced, or controlled, by the car makers; small car makers might be driven out of business, and one result would be the loss of their innovation; the compliance cost would fall more heavily on foreign producers, since they sell only a fraction of their production in the United States, so there would be an effect of sheltering US producers from competition; and whilst safety might initially be improved, reduced competition would reduce innovation, including safety innovation; and the higher price of cars would increase the average age of those on the road, presumably damaging safety. Not a bad deregulationist case in 750 words!

A few were more like a commentary on political developments, with an opinion attached—such as when he welcomed the first budget of the Thatcher government in Friedman (1979b), or his commentary on the developments in the campaign for a balanced budget amendment in Friedman (1982d). One of his little lessons came in Friedman (1967g) where he deprecated non-tariff means of restricting international trade, pointing out that such things as quotas took the pressures of competition off the foreign producers, to the detriment of consumers, and deprived the government of revenue. Free trade was best, but tariffs were to be preferred to other kinds of restriction since at least the government took the money. Or, Friedman (1969f) was a discussion of the facts underlying a reported increase in unemployment from 3.5 to 4%, which he said some saw as a portent of collapse. He pointed out that most unemployment was for only a brief period, and described various sorts of frictional unemployment. He noted that the average duration of unemployment had been five and a half weeks and the increase in unemployment, large though it seemed, would be accommodated by that duration rising to six and a half weeks. That surely made out his conclusion that though not desirable, it was not a catastrophe. 'We badly need less hysteria and dogmatism and more perspective, proportion and balance in judging these matters', he said.

There is variability and sometimes Friedman was a little more dogmatic and a little less argumentative than when at his best. It is as if he sometimes felt that his views were the things of interest, rather than his ideas. So, in Friedman (1973j), on speculation in the currency markets, he said there had been private and official intervention, and the latter was really speculation by governments. The former, he said, was socially useful in forcing appropriate adjustment of exchange rates, whereas the official intervention was harmful in that it sought to 'postpone the recognition of reality'. He said that governments were likely to be losers since they speculated with others' money, and that the US had lost $30m in recent months. But as to an argument beyond that, there was really none. He did nothing to explain what was wrong with the feeling that official stabilization of unruly markets was to be welcomed, and in so far as he made an argument, it leaned heavily on his presumption that a cynical attitude guides one to truth. Sure enough, those disposed to agree with Friedman will readily share that attitude and it is not hard to fill in the argument, but it was another ideal subject for a good lesson. On this occasion, though, he did not deliver it. (He made a much better job of the same theme in Friedman [1978c]).

In the 1970s the centre of gravity of the subject matter of his columns also shifted. The increase in inflation naturally led to him writing more about monetary policy. But the fact that in 1971 Nixon adopted price control measures, and then tried to maintain them even in the face of the oil shock also led Friedman to frequent discussion of that policy. These columns provide a different sort of picture, because one can see some changes but, for the most part, what is most visible is the continuity of his views over the period. Indeed, he does not always quite stand by what he had said in one column—a prediction, for example—when writing another, but that is a venial sin in a newspaper man.

So, by the time of the Nixon price controls, Friedman had a long record of opposition to such things, notably in Friedman and Stigler (1946), his dissent in Despres et al. (1950), and Friedman (1966b). But in the 1970s the issue moved to centre-stage in American policy. In Friedman (1970k) he made one of his clever arguments, saying that price rises can appear to businessmen to arise from cost increases because when demand increases, those at the beginning of the supply chain must bid more for resources, but all downstream firms then perceive an increase in the price they must pay for their inputs, which leads them to the view that they are raising prices because costs have increased. The truth though was that it was an increase in demand that initiated the process and so he said, 'Inflation is always and everywhere a monetary phenomenon'. Then in Friedman (1971g), responding to Nixon's wage and price freeze of August 1971, he said any apparently beneficial effect would be only cosmetic since the effect of price rises would appear in reduced discounts, or instead of wages, overtime and perks could change. On the other hand, the appearance that something useful was being done would take the pressure off government spending restraint, and that could have a real effect in increasing inflation. And his conclusion was that sooner or later, suppressed inflation would emerge into the open.

Having made those points, in Friedman (1971h)—in *The New York Times*—he said that wage and price controls were 'deeply and inherently immoral'. In the first place they threatened the foundations of free society by substituting discretionary authority for voluntary market interactions. In the second they undermined individual morality by encouraging people to spy on each other, creating widespread incentives to evade the controls and prohibiting activities which were in what he called 'the public interest', meaning no doubt, the joint interest of the parties involved. He was objecting, not just to the 'freeze' of August 1971, but even more to 'Phase II' in which the administratively appointed 'Price Commission' and 'Pay Board' would determine allowable increases, with instructions to do so in such a way as to control inflation. Here, his primary concern was with this discretionary power. He accepted there had already been compromises, but said that Nixon's policy was a 'massive' further step. He noted further that although the appeal was to 'patriotism' to control inflation, that could not determine which prices should rise and which should not, and the vacuum could only be filled by arbitrary judgement. The Presidential 'request' that dividends not be raised created a further concern in that since he had no relevant powers, the informal enforcement devices could only be nefarious. The following day, in Friedman (1971i) he expanded on the thought that the effect of controls was to encourage people to report each other, and to prohibit behaviour—in the form of aspects of private contracting—which had never been thought in any way wrong, and the idea that that would be destructive of social cohesion. In the circumstances of there being such doubt as to whether the controls would even achieve their stated objective, the moral case, he concluded, should be considered.

Friedman (1971j) noted that after the introduction of price control, though Nixon's fiscal recommendations had been sensible, Congress reduced taxes more, and spending less, than Nixon recommended. The danger, Friedman thought, was an inflationary explosion when price control was removed, and he feared a fiscal stimulus, also causing inflation, saying, 'The only hope of preventing this dismal outcome rests with the Federal Reserve System'. That was not a great hope, he indicated, as its understanding of policymaking was confused. As it turned out, in Friedman (1972e) he was celebrating the rapid fall in inflation before the freeze, treating its timing as showing the freeze had been irrelevant in controlling prices, while saying it was damaging in various other ways. Later, in Friedman (1978d) he varied that again, saying the freeze had suppressed inflation, although of course still saying an inflation explosion followed.

Friedman (1973g) commented that controls had had very little effect on inflation and said it fell in 1970 and 1971 because monetary growth had been reduced in 1968 and 1969. Of the future, he said,

> Inflation threatens to speed up in 1973 and 1974 because the rate of monetary growth has speeded up sharply in recent months… if the Federal Reserve cuts monetary growth sharply and holds it there, inflation will continue to taper off with or without controls.

In Friedman (1973n) Nixon was described as having 'encouraged' a highly expansionary fiscal policy in August 1971. The Fed was criticized for moving to an expansionary monetary policy in early 1972, but the price rise in 1973 was said to show 'This "experiment" in price control has ended as have all others—in an inflationary explosion'. He added a footnote saying that to confirm his remarks were not hindsight, one could look to Friedman (1971j). Indeed, they were not hindsight, but they were not quite consistent with Friedman (1973g).

Then in Friedman (1974l), he quoted Friedman (1971g) saying that sooner or later the freeze would end with the emergence of suppressed inflation and said, 'Precisely that has occurred'. Perhaps not 'precisely', since part of the story in 1971 had been that the freeze would bring a temporary improvement in the figures, but that had not happened, and in the intervening articles, Friedman had made future inflation depend on monetary policy, not the delayed effects of a post-freeze explosion. Still, in the 1974 article, he used the presumed artificiality of the current price rise to say that the high reported rate of inflation would subside when that effect worked through. Friedman (1977g) was another savage attack on price control. On the undesirability of control, he was entirely consistent. Then, when the possibility of rationing of fuel emerged, he criticized that for its inefficiency in Friedman (1973k) and for its inequity in Friedman (1973i). In the latter he made a characteristic and compelling argument. Part of it was to consider a proposal to distribute ration coupons to every family entitling them to purchase a certain amount at a low price, and allowing the market to set prices for larger amounts. He said that disregarding administrative costs, etc., it was equivalent to sending every household a cheque for the implied amount and financing it by a tax on oil companies. Thus described it appeared to him that there was no case in equity for such an approach and he rhetorically asked how it could be that there was such a case if the mechanism were disguised with ration coupons. And when discussion of shortages started to become more frequent, Friedman (1973o, 1974m), naturally enough explained them in terms of regulated prices. Again though, he was completely consistent in his aversion to controls. Friedman (1973p) concluded 'If the U.S. ever succumbs to collectivism, to government control over every facet of our lives, it will not be because the socialists win any arguments. It will be through the indirect route of wage and price controls.'

On macroeconomic and monetary policy, Friedman (1972d, p. ix), the preface to the first of the collections of *Newsweek* articles, observed that whilst many of his columns were on normative questions, those on macroeconomics were on matters of positive economics. Obviously with the fact that he was sharing space with the much more fiscalist Samuelson in mind, he commented that 'The most important single question of this kind is the role of monetary vs fiscal policy in affecting the course of events'. The relation of monetary and fiscal policy did occasionally come up, but thrashing out any disagreement about it was certainly not a priority in the columns themselves. His view that monetary policy is the more important really only comes through from the fact that he wrote a great deal about it, and so much less about fiscal policy.

The articles on monetary policy, taken as a group, very much tell an unfolding story throughout the period. Naturally enough, it was a Quantity Theorist's story, with the money supply always the crucial explanatory variable. More than that, though, there was a recurring theme of Friedman finding that changes in the direction of policy came too late, and when they came, went too far. Policy thereby introduced instability. The variability of lags between monetary changes and their effects was often asserted and used in explaining events, and on many occasions he said that a rule for steady money growth would be preferable to the policy actually followed. And the errors of the Federal Reserve were frequently attributed to their paying too much attention to interest rates, and too little to the quantity of money. In all these ways, he was thoroughly consistent and indeed if anything rather repetitious, with the variety between articles arising principally from the occurrence of further occasions when, in Friedman's view, the same mistakes were made again.

So, for example, in Friedman (1966e) he predicted 'inflationary recession' as a result of the adjusting of expectations of inflation and said there was going to seem to be a dilemma as to whether to tackle inflation or unemployment, but the right policy was to do neither. Instead, there should be steady money growth with taxes and spending set to achieve budget balance at high employment. Then he described 'erratic' policy, explained by a failure to allow for lags and a focus on interest rates in Friedman (1967h) and using the same title—'Current monetary policy'—in Friedman (1967i) he made much the same argument, though this time fearing recession rather than inflation, and calling for steady money growth. Friedman (1968f) recognized that monetary growth had been stable and welcomed that fact, whilst doubting it would continue. In Friedman (1969g) and Friedman (1969h) he described two changes of direction which he said had been too violent, both times calling for steady monetary growth. Then in Friedman (1969g) the substance of his criticism of the Federal Reserve focussed on the matter of lags, saying that on average there were six months between changes in monetary growth and changes in income and prices, but that it could vary between three and nine or more, and this idea was used to explain fluctuations in industrial production and consumer prices since 1958.

Friedman (1970b) welcomed the appointment of Burns as Chairman and repeated the view that policy had been too tight so that there would be a severe recession. He described policy as often confused by a focus on 'credit' and hence interest rates, rather than 'money' and said that Burns understood the correct position, and that 'Inflation is always and everywhere a monetary phenomenon'! But then Friedman (1970j) said that the shift to expansionary policy in December 1968 explained the inflation then occurring, and that the Federal Reserve had in fact not given up its concern with interest rates. In Friedman (1971k) he reported that the quantity of money had 'exploded' and said that this happened because of policy overreaction and the focus on interest rates. And again he said 'erratic changes in monetary growth' were harmful.

Friedman (1972b), a piece that looks as if it may have been written with Friedman (1972a)—his underappreciated lecture to the American Philosophical Society—in mind, expanded on the case for a monetary rule. Amongst others, one reason was that experience showed that discretionary policy was destabilizing. Another was that research had established that over 'any considerable period of years' the money supply was related to income and prices; but that 'the same relation is much looser from month to month, quarter to quarter, or even year to year', and that lags were variable. Together they made a case for steady monetary growth, and abstinence from attempts at fine-tuning.

Then in Friedman (1972f) he said that there had been undesirable variability in monetary growth; in Friedman (1973d) he said 'it takes several years for monetary growth to exert its full influence on prices' and that despite the appearance that the Federal Reserve was paying more attention to the quantity of money, monetary growth had been 'both higher and more variable' since Burns took over than it was before. And Friedman (1973m) again called for steady money growth, warning that it was not a panacea.

In this respect, Burns proved a disappointment to Friedman, not just over his support for price control, but over the variability of policy as well. In Friedman (1971k) he said money supply had grown far too fast at the beginning of the year, and raised the question of whether the Federal Reserve was able to control it. He said it was, but it failed to do so because it thought in terms of operating its policy through interest rates rather than bank reserves. A little later, Friedman (1972f) praised the Fed for adopting a money supply target and pursuing it via reserve growth rather than an assessment of credit conditions. He feared that the Fed would not realize that money growth needed to slow from then on to stabilize inflation. But in Friedman (1975g) he said that the failure to hit money supply targets was due to continuing to operate policy through the federal funds rate. In doing this, it was fallible and 'any error tends to cumulate and be self-reinforcing', though he explained neither that, nor the companion claim that although targeting reserves would still not be precise, errors would tend to cancel each other. He also, of course, frequently emphasized that the lag between monetary causes and their effects was variable. As he said in Friedman (1973n), when criticizing the Federal Reserve for allowing money to grow too fast, 'Over short periods, many factors other than monetary growth affect the rate of inflation… over long periods, money growth is dominant'. Friedman (1974n) once again described the confusion resulting from an inappropriate focus on interest rates rather than the money supply.

The specific point that the Federal Reserve was repeating past errors was forcefully made in Friedman (1975h). 'As has occurred repeatedly during its 60-year history, when it shifted it did so too late and too far', he said. He also said that the attempt to control interest rates rather than the money supply was the source of the problem, and observed that his studies had shown that the Federal Reserve invariably claimed credit when things went well, and blamed bad outcomes on forces outside its control. Similarly, Friedman (1975g) said, 'Erratic swings in monetary growth are not a new phenomenon', giving the same explanation, and saying these swings were damaging. Friedman (1976i) said that contrary to what the Federal Reserve claimed, swings in monetary growth lasting six or eight months destabilized income and that they occurred because of the Federal Reserve's procedures.

Friedman (1978e) described the recurrence of 'inflationary recession' arising from policy lags. He said that expansion brought a fall in unemployment followed 'much later' by inflation, and hence a reversal of policy leading first to a recession whilst inflation initially continued. Here, he presented data based on a two-year lag between money and prices, showing the consistency of the relationship. He called for a gradual reduction of the rate of money growth until stabilized at 4% p.a., and said that the failures of the Federal Reserve were due to political pressures and its obsolete procedures. Friedman (1979c) again complained of the Federal Reserve's 'propensity to swing from one extreme to the other'.

Friedman (1980d) commented on the Federal Reserve's announcement fifteen months earlier—in October 1979—of a new approach aimed at controlling the monetary aggregates, and announced that it had failed, and that monetary growth remained volatile. Then, as in Friedman (1981), operating procedures were blamed, and in this later column he also noted that 'swings have clearly become shorter in duration and wider in amplitude since the announced changes', this time also blaming the resistance of commercial banks and bureaucratic inertia for the poor procedures. In Friedman (1982e) the 'unprecedentedly erratic behaviour' of the economy was blamed on 'unprecedented volatility in monetary growth', and he quoted three recent examples of cyclical reversals in output coming three months after reversals in monetary growth. The tendency for swings in policy was, in Friedman's view, certainly undiminished.

Friedman (1982f) was about the definition of 'monetarism' and Friedman said it was a new name for an old idea. Of that idea he then said, 'The keystone of the quantity theory is the distinction between the nominal quantity of money … and the real quantity of money'. The two-year lag between monetary changes and their effect on inflation was discussed and he said that although in principle active monetary policy could be stabilizing, experience showed that it was not, so he advocated a steady rate of money growth.

The issue of the consistency of all this with Friedman's academic work is sometimes raised, as it was by Solow (1984, p. 135) who, somewhat to the same effect as Tobin (1976), criticized him for advancing a much simpler and more thoroughgoing—and so, in Solow's view, less reasonable—version of monetarism in *Newsweek* than he did in his academic work. The analysis of Nelson (2004) points to at least a broad consistency, and in any case, if there is mild inconsistency—which there is, here and there—it should be seen in the context of Friedman writing such short pieces. Corners are cut, because not everything can be hedged in a single page.

A different sort of point, though, and a more subtle one, concerns the positioning of the *Newsweek* analyses relative to his academic ones. One notable point is that these discussions of monetary policy from, say, 1966 to 1982—a little more than a decade and a half—make no mention at all of the Phillips curve. In other respects, the points he made follow his academic work. But on the question of the central problem with monetary policy, whether before or after 1975, what he consistently emphasized in *Newsweek* was that policy was erratic. An erratic policy is not one that is targeting some point of a shifting Phillips curve. One might strain to interpret what he said in that way, but there are two reasons the attempt must fail. First, he clearly described policy as vacillating between expansion and contraction, driven at each turn by inflation or unemployment. It is plain that they are two evils, and policy was being said to be inconsistent. The inconsistency was explained by Friedman in terms of a misunderstanding of the lags. There is a clear possibility of an alternative account which makes policy coherent, even if based on error. That coherence would come from the understanding that there are two evils, but there is a tradeoff between them. That is not the explanation Friedman chose. Secondly, in Friedman's explanation there was another aspect to policy failure—policy was erratic because the operating procedures were flawed. He said it again and again. That pointed at a policymaker mistake, but not one arising from the Phillips curve. The idea that policy was overly inflationary because of an erroneous idea about a tradeoff between inflation and unemployment, though ridiculous on other grounds, would not be out of character with the timbre of Friedman's arguments. It is the kind of mistake he might have said policymakers made. But by saying nothing that gives any hint of such a mistake, he made it perfectly plain that he did not think that. For the same kind of reason, nor could it be plainer that in his view policy failure had nothing to do with a failure to understand the relevance of inflation expectations. Again, it would have been the easiest thing in the world for him to introduce that idea, and entirely in keeping with the general disposition of his columns. But he made no approach to it at all and surely therefore, he did not believe it.

All in all, Friedman's *Newsweek* columns show a master at work. In short pieces—literally hundreds of them—he made his points with clarity and conviction. They are, of course, not quite perfect. Corners are certainly cut, and there are some moves that he would probably not want to defend even as short cuts if anyone really wanted to argue with him. But then they are pieces of a few hundred words. It is easy to see from these alone why his popular writing is so highly regarded. But they also provide a large database of what was on his mind at various times as he observed the American economy and American policymaking. Those insights too convey to the later reader much about his attitude toward contemporary developments.

22

Free to Choose

Friedman and Friedman (1980)—*Free to Choose*—is sometimes thought of as a sequel to *Capitalism and Freedom*. Certainly it has features in common. The basic form and intent of the argument is the same: The government does too much and its actions, well-meaning as they are, do more harm than good and quite often exacerbate the specific problems they seek to address. Many of the topics were the same as well—international trade, monetary management, a little on fiscal management, the failure of the school system and the case for education vouchers, the negative income tax, and a complaint about professional licensing, with special attention on medical care—though there, the argument was not so emphatically put, and the conclusions were somewhat more cautious. One or two new things were added or much expanded—there was much more on consumer and worker protection and a chapter on the control of inflation that really had not featured at all in the earlier book.

In the reactions to this book, there was much more overt hostility than there had been to Friedman (1962a). Sure enough, the lucidity of the book was widely praised but responses to its content were not nearly so sympathetic. Valone (1982) described Friedman's thinking about the market and freedom and, invoking Singer (1978) said, rather

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_22

mildly, compared to others, 'All of this, however, is an oversimplification' (p. 109), later also accusing the Friedman of 'a misunderstanding of the common good and a misunderstanding of democracy' (p. 110). Basu (1982) objected to Freidman's strategy, saying that in the issues he considered there were arguments for and against state intervention and the role of the academic was to present the best of the arguments and argue a case, 'But Friedman, having first decided which side he is on, produces some shoddy arguments in support of the opposing viewpoint and quickly demolishes them' (p. 1780). In the specifics of making out his case, Basu was far from fair to Friedman, and would have done well to take his own advice, but his attitude is clear. And Bradfield (1982) put it more succinctly yet, saying '*Free to Choose* often twists arguments and simplifies history' (p. 266). 'A second fundamental flaw', said Bradfield, was that Friedman's position was that 'despite corporate power to manipulate governments, workers, and consumers will be better off if government's role in the economy is reduced'. One feels Friedman might have said 'because of …', but there it is. Desai (1980) thought the discussion of inflation the best of the book and said 'the rest is a mixture of argument and assertion by selective evidence. There is a persistent tendency to present as incontrovertible scientific truths statements which are the authors' political beliefs' (p. 505), and that they present 'the most naïve version of American history as a march, hand in hand, of capitalism and freedom' (p. 506). Only Yankovic (1981) took a much different view, noting as others did not, the importance of the book's subtitle: 'A personal statement'. He saw it as a collection of powerful and important arguments, not as an attempt to offer a final resolution. Those are, to judge by JStor, all the reviews in academic journals. There were more in the popular press, but a good part of the tone there was much the same. To cite one, Heilbroner (1980) said 'Some of the Friedmans' specific proposals appear to me to be shrewd and worth consideration, but not on the basis of the winnowed evidence or shabby arguments they have advanced on their behalf. *Free to Choose* is to serious economic and political debate what fundamentalist preaching is to Bible scholarship'. Even *The Economist* (8 March 1980) seems to have been less impressed than it was by the earlier book, saying it was robust, but drawing attention mainly to the lack of substantiation of so many points it made. And Arrow (1980), writing in *New Republic*, agreed with some of the specific conclusions of the book, but made much of the limitations of its argumentation.

The pointed hostility of these responses is notable—there are no Abba Lerners, ready to take issue with Friedman, but seeing the point in his argument, as there were in response to *Capitalism and Freedom*. But for Arrow, not writing in an academic journal, there are no Abba Lerners in another way as well, since as a group the professional standing of these reviewers hardly matches that of those of the earlier book. *Capitalism and Freedom* was greeted by reasoned, respectful commentary, in addition to Lerner, by Hicks and Boulding, and all three engaged with the work. For *Free to Choose*, there was Desai, but most reviews had only criticism for it, and there was also the attack on Friedman and Reaganomics by Rayack (1987) apparently inspired by it.

True enough, many of the arguments were much less fresh than they had been in 1962—education vouchers and the negative income tax in particular were nothing like as novel as they had been. But the reviewers were not hostile because they were bored with the ideas, or had devised new arguments against them. On the contrary, some of them were very short on arguments themselves.

More notable than that though, is the point that in certain ways the later book is better than the earlier. For one thing, there is none of the absurd attempt to philosophize about freedom; and though Dicey still had one or two mentions, there was practically none of the phony learning of the earlier book in its stories of historical developments based around just one or two ideas from him. Nothing was made of any attempt by the Labour government to control where people worked. It is a much longer book too, and that is because the arguments are much more fully made and more detailed. And that fact also highlights the much better cohesion of the overall argument through the book.

Rather than trying to philosophize, the book was off to a much better start deploying Leonard Read's (1958) story of the family tree of the pencil, emphasizing how many different activities go into making a simple product, and that that is done without conscious coordination. Each chapter was then full of ideas, and real examples of the kinds of dangers the Friedmans saw in state intervention. Sure enough, it was one-sided—there is no serious attempt to put the other case, and the book might persuade the unwary reader too quickly. But then it is a 'personal statement'. Except perhaps where *Capitalism and Freedom* was at its strongest—on monetary questions—*Free to Choose* is much more impressive, and throughout the case that is put is a case that has all the appearance of needing an answer.

So, one might consider Chapter 7 on consumer protection which although some of the ideas were presented in Friedman (1978f), went furthest beyond Friedman (1962a). The authors began with a short history of the creation of government agencies and their development after the New Deal. They said there had been a great explosion in anti-growth sentiment, and cited Edward Teller saying that building the first nuclear power station took eighteen months, whereas when he wrote it took 12 years. The implication was that it was the regulatory environment that made the difference. Friedman and Friedman suggested that the least satisfactory products then available were the state-supplied ones, such as the postal service, schooling, and rail transport. The most satisfactory ones were high-tech products, privately produced. The authors contrasted this view with the attitude of Nader (1965). That book, questioning the safety of the Chevrolet, Corvair, they said, had been one factor leading to the creation of National Highway Traffic Safety Administration, but when, ten years later, it investigated the Corvair, it found it as safe as similar vehicles.

They discussed the Interstate Commerce Commission which had been considered in Friedman (1962a) and moved to consider the Food and Drug Administration, which had not. The meat packing industry had welcomed regulation as it also brought certification which they expected to help in export markets. Pharmacists were concerned about the sale of useless drugs so that their self-interest and public spirit coincided. Little resulted until a new drug killed just over 100 people whereupon legislation required all new drugs to be approved as safe. They said with, admittedly, nothing to support it, 'A cozy symbiotic relation developed between the pharmaceutical industry and the FDA' (p. 244) until the Thalidomide disaster in 1961.[1] That led to legislation requiring not only that a drug be proven to be safe, but also that it be proven to

[1] Thalidomide was a sedative given to pregnant women in some European countries which turned out to cause severe birth defects.

be effective. Naturally enough the Friedmans said that the protection of the public from unsafe drugs was desirable, but they pointed out that the introduction of new, effective ones is also desirable. They said that rate of introduction of new drugs had fallen, while development time, and hence cost had risen. Citing Wardell and Lasagna (1975) they said costs had risen 50-fold, and development time had quadrupled since the 1950s and 1960s (p. 246). So, noting that companies that produced dangerous drugs also suffered in the market place—the makers of Thalidomide had paid large amounts in damages—the question was whether the incremental safety brought by the legislation was sufficient to outweigh the losses caused by the delayed licensing of useful drugs. Citing Peltzman (1974) they said that the evidence was it did more harm than good. They also suggested there is an asymmetry in reactions to risk, since the institutional consequences of permitting a drug that turns out to be dangerous are much more serious than those of delaying the licensing of a useful one. That difficulty, they said, could only be aggravated by the presidential award made to the individual who prevented the approval of Thalidomide in the United States.

They turned to consumer protection more generally, pointing to instances where supposed safety regulations had done harm, and thence to the environment and the activities of the Environmental Protection Agency, which had been established in 1970. That, as their earlier reference to the early environmentalist manifesto of Carson (1962) made clear, was a particular concern. They had no difficulty in accepting the existence of externalities, but pointed to the tendency to try to control them by regulation rather than charges. Citing Myrick Freeman and Haveman (1972) they said that the reason for avoiding charges was that governments preferred to avoid making the existence of tradeoffs explicit and therefore preferred the clarity of prohibitory regulation. They dealt similarly with the matter of energy and particularly the oil crisis. Taking the opportunity to complain about Nixon's introduction of price control in 1971, they argued that regulating the price of oil after 1973 was sure to cause a shortage, and they said the shortage would disappear immediately if prices were deregulated. Furthermore, the general propensity to intervention reduced the incentive on the private sector to innovate and thereby address the problem in an effective way.

They then considered the effectiveness of the market in protecting consumer interests. Revisiting some issues previously discussed, they pointed to ways, such as reputational ones, by which private suppliers are disciplined by the market. Not only the producers, but intermediaries, such as department stores, had reputations of their own, but also expertise in assessing product quality, and that protected the ultimate consumer. Private testing agencies also existed; advertising, they said, perhaps responding to Galbraith (1958) without naming him, did not truly distort consumer preferences. That was not quite consistent with their complaining about the government advertising Treasury bonds that returned less than inflation—something which had been one of Friedman's themes over a long period, of course. It was monopoly, rather than poor products, that they thought was the great danger to consumers. That could be addressed by freer international trade.

At various points in the chapter they suggested general lessons. One was that the origin of regulation is in political coalitions that form for the best of reasons, but might have incompatible objectives (p. 240). Every 'act of intervention establishes positions of power' (p. 232), they said. The power having been created, the interested parties get to work to take control of it, and their success creates problems which become the reasons for broadening the powers. Then, the powers are used according to the incentives on the regulator, not the intentions of the original proposers of the legislation. As Friedman (1973l) put it in a *Newsweek* article, hoping regulators would do otherwise was as futile as hoping a cat would bark.

All of that could be controversial, though some of it was to become widely accepted. None of it could reasonably be said to have been fully and conclusively argued. But it is a polemical book. These arguments were far more satisfactory than those of *Capitalism and Freedom* (except where those leaned on Friedman's other studies of monetary problems); they were more cogent in themselves, and made numerous points that need answers; evidence was presented—'one-sided' though it may have been, it was much more credible than the vague assertions of *Capitalism and Freedom.* On these issues, then, the book offered a far richer picture than those of the earlier book.

There is still the matter of the advocacy of monetarism, which the book contained, to be considered. But the hostile reaction the book received was certainly not focussed on that. And considering the better reception of the earlier book, it can hardly be explained by its argumentative weaknesses. On the contrary, argumentation is much better deployed than in the earlier book. There may be an aspect of jealousy to it—Friedman had become a much more famous figure by the time of the second book than he had been at the time of the first. And perhaps his seeming to be so successful with arguments that were much less than fully persuasive was more annoying in that context. But surely it must also be that perceptions of Friedman more broadly had changed. Certainly, he was seen as the brains behind the book—very noticeably many of the reviews of *Free to Choose* treated it as if he were its only author, although it clearly had two. Between 1962 and 1980 he had, I suppose, become much more of a menace to conventional opinion, and that may have provoked hostility. But that for makes no kind of reason not to respond to his arguments. Beyond that, though, for many on the left, he had become a hate-figure as well, after Chile and the controversy over his Nobel Prize. There seems to be a clear indication of a change in attitudes to Friedman in the comparison of these two books, the second of which is in fact much the better one.

23
Other Causes

Radical as many of Friedman's ideas where they are all of a kind and in certain ways, the package was limited. Not all of them are radical in being new. The general intent of some of them—the various manifestations of sound money views, most notably—would be better described as 'traditional' than 'innovative'. They are all radical, though, in being conceived as going to the root of the problem. They are not all by any means presented as being complete solutions, but Friedman's project is always to try to see the true nature of the problem and design a response to that. Again and again those solutions turn on creating market or market-like processes, or on relying on market processes that are already there. This is one obvious respect in which they are all of a kind. But those so far considered are also all of a kind in being consistent with a social conservatism. Some of the proposals shock, but it is as if they are designed to shock the intellect, not to create moral shock.

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_23

1 Missing Issues?

It would have been interesting to know to which conclusion Friedman's liberalism would have led him in the abortion debate; or the legalization of homosexual acts. Neither of those has the aspect of being economic activity that brings Friedman's ideas to life, but they are issues of freedom, and in any case, he could have commented on the power of the market in relation to the supply of pornography. He did not comment on the fact that prostitution was illegal in the United States and many other countries, though it is easy enough to construct a Friedmanesque case for legalization—there is a question of freedom, and the state not judging individuals' interests for them; there is the practicality of preventing it; the dangers of violence which are exacerbated by putting the activity outside the domain of normal law enforcement; and if human trafficking were made the issue, it could be argued that allowing legal sex work would give industry lobbyists every reason to co-operate with law-enforcement in stopping it. But Friedman seems not to have addressed this issue. He had very little to say even about prohibition, though since it ended the year Friedman was 21 he must have had some thoughts on it—there was nothing in Friedman (1962a) and just the briefest of mentions in Friedman and Friedman (1980). Perhaps Rose would not have liked some of these things discussed, or one could wonder whether, for all his self-proclaimed radicalism, Friedman was just too prudish to involve himself with these issues.

2 Foreign Aid

Then there are issues that do appear in Friedman's writings, but at least as far as *Capitalism and Freedom* and *Free to Choose* are concerned, do not get the kind of attention that might be expected. One considered especially in Friedman (1958d) and, responding to Wolf (1961), in Friedman (1961a) would be the question of foreign aid. It was often justified as necessary to limit Soviet influence over developing countries. But Friedman made the case that it was always in practice aid given to governments. A portion of it was then wasted, but materially all of it was deployed by government. It was therefore inevitable that recipient governments would grow and come to exercise more control over the economy and the country. In this way, said Friedman, socialism was promoted, not contained. It is another of those clever arguments seeming to deliver the opposite conclusion to the conventional one and it is something of a surprise not to find it developed in either of the books.

3 The Privatization of Money

Another issue was the question of the competitive supply of money. It is not quite so clearly a 'popular' rather than an academic topic. But Friedman did discuss what he took to be the necessity of government provision of money in his popular writings as well as such places as Friedman (1948a) where it was axiomatic that, 'Government must provide a monetary framework for a competitive order since the competitive order cannot provide one for itself' (p. 246). In Friedman (1960a) his position was both more thoughtful and more equivocal. He noted that a commodity money system, which might exist without a government, though in practice usually did not, is expensive to operate since resources must be devoted to the creation of money. On the other hand, a privately produced fiduciary money backed by a commodity was liable to be over-issued, that this would lead to the failure of the issuer, and that was a particularly serious matter as it would be likely to damage individuals other than the issuer and holder of the money, so that damage could be widespread. The danger of such failure could be avoided by creating a purely fiduciary money. But of that, he said that the separate private issuers each have an incentive to increase their issue, and there was nothing to stop that issuance until the value of the money fell so far as to make more production uneconomic—in that case, it was a commodity money where the commodity was, more or less, just the paper. Consequently, he said that there needed to be an external limit placed on issuance, and since competition did not provide that limit, the production of money was a variety of technical monopoly and so there was no 'presumption', as he put it (p. 7) in favour of private rather than state provision.

That, along with the particular importance of the matter and the necessity of such things as avoiding fraud was sufficient for Friedman to conclude that government involvement was justified. But the infeasibility of competitive production of money was just the point challenged by Benjamin Klein (1974) and Hayek (1976a). Klein in particular noted specifically in response to Friedman that the fear of money being produced until its value fell to the marginal cost of production depended on its being 'competitively' produced in the sense that different producers' monies were indistinguishable. So long as that was not the case, as he and Hayek both said, producers have an incentive to maintain the value of their money. So, one might say, the monopoly position of governments gives them the opportunity to debase their currencies since citizens have no practical alternative to their use; and pure competition leads to marginal cost pricing. But the monopolistic competition these authors envisaged was a distinct case, not well described by either of the other models. Obviously, Friedman might have been led to reflect on his view that the model had nothing to offer! The argument for competition was further buttressed by White's (1984) discussion of the actual operation of free banking in nineteenth-century Scotland.

Friedman and Schwartz (1986b) considered the matter again, noting White's research and accepting that the system worked well. They doubted its relevance, though, because Scotland was a small and stable country so that the bank owners were well known. Perhaps more importantly, in the view of Friedman and Schwartz, the shareholders bore unlimited liability. Furthermore, although the banks issued currency, it was backed by gold or Bank of England notes and hence was not a pure fiduciary issue. They also noted that there was nothing preventing contracting parties agreeing payment in whatever form they liked, so that in that sense, competing currencies perfectly well could emerge. The answer to that might be that there is a difficulty of entering the market, and a better prospect was the one suggested by Hayek (1976b, Chapter 1) that the European governments could bind themselves to allowing the circulation of each other's currencies in each country. Friedman and Schwartz (1986b) did not address that idea, though it was of a kind that might have been expected to appeal to Friedman. He might also have been expected to make the point that when European Monetary Union did arrive, it was through the creation of a state-controlled, monopoly money. It could have been through encouraging the circulation of multiple currencies throughout the area.

4 Drugs

Like the question of free banking, that of the legalization of drugs was not mentioned in Friedman and Friedman (1980). He had mentioned it in Friedman (1972g). That was a *Newsweek* article which drew attention the relation between the issue and prohibition and made clear that Friedman thought Nixon's just-announced 'war on drugs' would be no more successful. Friedman questioned the right of others to prevent people taking drugs and said that issue did not need to be resolved since in the case of drugs, the prohibitory legislation was making matters worse for addicts and the rest of society. If they were legalized, the incentive of dealers to induce addiction would disappear, and crime committed by those who became addicted would be less, since it would not be so necessary to finance the addiction. As to everybody else, the harm done by addiction, he said, was almost wholly from the fact that drugs are illegal—there was the crime of the addicts, and the bribery of officials by the dealers. And finally, there was no practical means of enforcing prohibition in any case.

Friedman and Friedman (1984, Chapter 7) addressed the issue again, adding a few pages to the *Newsweek* article but using much of its text. And he took up again later, in Friedman (1989b)—a letter in the *Wall Street Journal* which generated some controversy, and made a much fuller argument in Friedman (1991a) and, in partnership with libertarian psychiatrist Thomas Szasz, in Friedman (1991b), as well as writing other newspaper pieces. In Friedman (1991a) he produced data on the murder rate suggesting that it had increased steeply at the time of prohibition and in 1972, and had declined when prohibition was ended. That was presumably because of the crime associated with the contraband. He also said that there were enormous numbers of blacks in prison—about four times as many per head of population as in apartheid South Africa, and pointed to the harm done to inner cities where the drug dealers became role models. In Friedman (1991b, p. 66) he argued along the same lines, also suggesting that crack cocaine would never have been invented, except for the way in which the prohibition on cocaine affected the economics of its supply.

Thornton (2016) described this as if the matter was one of Friedman's great causes, calling Friedman (1972g) 'a shot that was heard round the world', and saying 'Friedman continued to make the case against the war on drugs throughout his career, never wavering in what seemed at times to be a hopeless effort'. He was already 60 when the *Newsweek* article was published, and he had already fallen out with Nixon over price controls, and was therefore not, contrary to Thornton, his adviser. Then he wrote nothing more for over a decade, and after that, only a little more, mostly letters, and all of it when he was well into retirement. Considering how suitable an issue it would seem to be, that is remarkably little. The question seems almost custom-made to be answered in the kind of ways he was answering questions about state intervention in Friedman (1962a). It raises the question of freedom; intervention is certainly less than fully effective, and there is a good argument that it does more harm than good, with adverse side-effects of the attempt at prohibition being numerous, and Friedman being expert at finding them. He might not have thought it as significant a matter in 1962, but the issue's non-appearance in three hundred and eighty pages of *Free to Choose* does show that it was not at the top of his list of priorities. Thornton speculates that the explanation of its omission was that the Friedmans were frightened of being portrayed as trying to persuade young people that drugs are safe. More likely they, like those they expected to be their admirers, were just a bit too conventional—socially conventional—to go in that direction.

5 Conscription

And then there was military conscription—'the draft'. Friedman (1962a, pp. 35–36) included a list of governmental activities which 'so far as I can see' could not be justified on his liberal principles and compulsory military service in peacetime was one of them. He took the view that the appropriate mechanism was to pay the amount required to hire willing recruits. He said, 'Present arrangements are inequitable and arbitrary, seriously interfere with the freedom of young men to shape their lives, and probably are even more costly than the market alternative.' However, he said that universal military training to provide a reserve for wartime was a different matter and might be justified. Clearly, the implication of these brief remarks was that conscription in wartime could be acceptable.

He subsequently became very much involved in the argument about ending the draft. He may have changed his position slightly so as to extend his opposition to conscription to small wars, such as that in Vietnam, as of 1966 or 1967, at least, but certainly he opposed conscription in *Newsweek* in Friedman (1966f). His principal contribution on the topic was then at a conference in Chicago—Friedman (1967j, pp. 202–203), and a very similar discussion in Friedman (1967a). The whole conference, published as Tax (1967) contains contributions by a very impressive array of participants from the military, politics, and various academic disciplines and seems to have been influential.

Friedman made the point that there was an infringement of freedom in the arrangements and presented what was for him a typically clever array of arguments on the matter. A volunteer force would be more effective; freedom would be enhanced for those who would have been drafted, but chose not to serve, but also for those who did serve, since the existence of the draft led to other infringements of freedom, such as travel restrictions on those who might be drafted. Ending the draft would end the social discrimination that arose from exempting those attending college and rejecting those failing educational tests; it would allow better planning by those otherwise subject to conscription; it would stop people marrying or going to college to avoid the draft. He considered and dismissed the ideas that the services would become racially unbalanced, or that a professional army would be more of a political danger than a conscripted one. But the most noted argument was one on the costs of alternative approaches. He considered the work of Oi (1967) who found the cost to be manageable, but more importantly, argued that the cost being paid under the draft arrangements was incorrectly measured. As he put it, everyone serving for less pay than would induce them to volunteer, was paying an implicit tax. If that cost were included, an all-volunteer arrangement would almost certainly lower the cost since those who served would be those who were willing to do so for the lowest income.

It is a good argument, of course, but not quite complete since there is another aspect that Friedman might have considered, pointing mildly in a different direction. Marginal taxes are distortionary. The true cost of public expenditure is the cost of the goods or services purchased, plus the further cost of the distortions brought by the tax which finances it. For small expenditures, those distortions are small, and in any case, usually they are unavoidable. But when the draft 'taxes' those conscripted, as Friedman put it, that tax is a 'lump sum' rather than a marginal tax, and so is non-distortionary. When the military is large enough that could bring a substantial benefit. It is an interesting question how large it would have to be for that to be a consideration, but Friedman did not raise the point. He came close in Friedman (1967j, pp. 202–203) when he said that the case against conscription was weaker during a major war since to achieve very high voluntary enlistment would be expensive and consequently taxes very high, so that the implicit tax on the conscripted might be 'less bad than the alternative'. But that does no more than gesture at the point, if it even does that.

Friedman carried on the argument after the conference, as noted by Singleton (2016), in various press outlets, the *New Individualist Review*, and similar publications. He also revisited the issue after the abolition of the draft, in a debate with Congressman Pete McCloskey in Anderson (1982). By then he had also been a member of The Gates Commission and hence an author of Gates et al. (1979). It is sometimes suggested that the Commission was concerned with making a recommendation as to whether to end conscription. In fact, Nixon was already committed to that, and it was appointed to advice on how to go about it. Nevertheless, it was composed of those initially of a variety of views on the desirability of conscription and they ended up feeling it should be abolished. To all appearances, Friedman's arguments were amongst the most persuasive.

6 The Popularization of Monetarism

'Money matters'; 'inflation is always and everywhere a monetary phenomenon'; 'inflation is produced by government and government alone'—they are all very Friedman, but they do not all mean quite the same thing. In his most serious academic work on money, the focus of Friedman's attention had really not been on the causes of inflation. That is very much true of Friedman and Schwartz (1963a) which is really not a study of the causes of inflation. The book is about the money stock in the United States. It is very much broader than a study of one particular policy problem. The work on differentiating the Quantity Theory from the income-expenditure theory, up to Friedman (1974a) never had the understanding of inflation at its heart; his stories about the development of monetary thought, likewise, were not about that; and his restatement of the Quantity Theory gave no prominence to the explanation of inflation, and nor did his restatements of his restatement until sometime later. When he first said that inflation was always and everywhere a monetary phenomenon, it was not actually an inference taken from the Quantity Theory at all—he was making a point about cost-push inflation and variations on that idea. He could have said 'inflation is always and everywhere an aggregate demand phenomenon', and in the context in which he was speaking, that would have done just as well. That way of putting it was not, for Friedman, the natural language to use, but the point is that the sentiment being described when he first used the expression was not an essentially monetarist one at all.

Sure enough, Friedman and Schwartz found long swings of price change associated with changes in the quantity of money—a handful of such long swings in the ninety-odd years they studied. There may be causation there, but it is nothing like a finding of close determination. On the contrary, when it came to the relationship between money and activity over shorter periods, they said, as quoted above (pp. 234–235, above).

> Mutual interaction, but with money rather clearly the senior partner in longer-run movements and in major cyclical movements, and more nearly an equal partner with money income and prices in shorter-run and milder movements – this is the generalization suggested by our evidence.
>
> Friedman and Schwartz (1963a, p. 695)

And of course Friedman did say—occasionally, though in what should have been uncontroversial remarks—that it was possible that such things as trade union actions could raise wages. They could, 'in fact push up wages sufficiently to create unemployment' as he put it in Friedman (1963/1968, p. 29), also accepting that this might lead to inflation. Even though he may have once or twice tried to deny it, the same is obviously true of the price of oil as well, even if, on Friedman's account, the effect could be at most only temporary. Indeed, Friedman and Friedman (1998a, pp. 253–254) said so themselves, even if in a mildly contradictory statement, saying that governments like to blame business, unions, Arab sheikhs and bad weather for inflation but,

> All these can produce high prices for individual items; they cannot produce rising prices for goods in general. They can cause temporary ups or downs in the rate of inflation. But they cannot produce continuing inflation…

Friedman (1972b) had it perfectly when he said that over 'any considerable period of years' the money supply was related to income and prices; but that 'the same relation is much looser from month to month, quarter to quarter, or even year to year'.

I suggested above (p. 285) that Friedman's thinking on indexation should have led him to accept the possible benefits of wage control during disinflation. But the point might run further—in crisis circumstances all manner of policy might be appropriate, even though, of course it might be accepted that long-term stabilization would require a moderate rate of growth of the money supply.

Rather than contemplate any such line of thinking though, as his views gained acceptance amongst policymakers, Friedman put more emphasis on specifically the growth of the money supply. So, for example, in *Newsweek*, Friedman often predicted inflation year to year on the basis of what had happened to monetary growth. He held on to the variability of lags and occasionally made reference to temporary effects of price control or of its ending, but really did not do that consistently. Business, unions, Arab sheiks and bad weather could, on his own account, bring temporary effects. Those effects clearly might matter when there is an urgent need to control inflation. That is all the more true when the relevance of inflation expectations is recognized. For this reason, and because of the long and variable lags, setting a monetary target *this* year might or might not produce much benefit in the near future, and consequently, some other measures, in addition or instead, might work better.

In *Free to Choose* the question of inflation control was addressed with a collection of charts showing steeply rising prices and money supply in various countries, and a lot of assertions about how it could only be increases in the quantity of money that caused inflation over long periods. One clue that it was the growth of the money supply causing the increase in prices, said the authors (p. 256), was that the graphs were drawn so that the line showing money was for six months earlier than that showing prices. But the variables were steeply trending, and nothing was demonstrated. Indeed, the point precisely recalls the issue over which Friedman, all those years before, had dismissed the work of James Angell on the monetary causes of inflation (p. 114, above).

In Feldberg et al. (1976) on his trip to South Africa he took the same view, at the same sort of time, with the same sort of diagrams. And in an Australian trip, described in Friedman (1975d), that was true again, though there was an extra twist. Having said in Friedman (1975d, p. 10),

> inflation is not a capitalist phenomenon, not a socialist phenomenon, not a trade union phenomenon. It is a printing press phenomenon. The immediate proximate source of inflation everywhere, under all circumstances, is a more rapid increase in the quantity of money than in output.

he then said, a few days later of his diagram of the money supply and the price level in Australia,

> For a period between 1970 and 1973 the price index ran ahead of the quantity of money and I do not exactly know what the explanation is. I trust that the economists here will be able to enlighten me on what was going on there. Perhaps there was something wrong with the numbers I used. But for our purposes the important thing is the general concordance between the two series and the fact that in 1973-74 period when the

> money supply started to expand very rapidly, prices also started to expand very rapidly. (p. 56)

And in Friedman (1976j, p. 12), and similarly elsewhere, it was 'The direct cause of inflation is too much money relative to output. There is no other route through which inflation can be produced. There is no inflation in history which has not been preceded by a rapid increase in the quantity of money per unit of output.' No, there were cases where it was so produced, and cases where the data was wrong.

The Phillips curve, interestingly, remained more or less absent from Friedman's purely popular discussions of the matter. It appeared in his presentation at the IEA, and in his Nobel Lecture and in his 1987 encyclopedia entry on the Quantity Theory, but is absent from *Free to Choose* *Newsweek* and mostly so from other newspaper discussions—though it appeared, for the benefit of a British audience, again, when the Nobel Lecture was reprinted by the IEA as Friedman (1977c) with a slight change in the title from 'Inflation and unemployment', to 'Inflation and unemployment: The new dimension of politics'. It is rather mysterious that he thought it appropriate for the IEA, and the Nobel Lecture, and encyclopedia entries, and carried on with it all his life, even into the posthumous Friedman (2010), but for some reason he did not think it appropriate for so much other popular writing.

Whatever the reason for that, the effect was to leave Friedman relying on the long-term relationship between money and prices when Friedman himself said that over short periods it could not be relied on. Graphs of lines for money and prices, both pointing straight from the bottom left to the top right of a page may impress the unwary, but Friedman should have expected other economists to think less of him if he conducted himself in that kind of way. Monetarism was not even quite an appropriate topic for *Free to Choose*, which has so many strong points in making Friedman's case to leave people free to choose. But in discussing inflation, he seems to have become very much concerned with pointing to the 'simple truths' supposedly, though not actually, flowing from his research. Somehow, the important things seem to have become insisting on the point that money growth determines inflation, denouncing the foolishness of the governments of the time who he said denied it, and of course, to see himself in the newspapers over the control of inflation just as he did over the coming failure of British democracy.

7 Conclusion

Friedman was then, perhaps not quite the radical sometimes imagined. There were matters that would seem to find a home in his style of argument, and could well have stirred up controversy, which he did not touch. Equally, in the case of conscription and drugs he clearly did feel strongly, and there is really no reason to doubt that he saw himself trying to change things in respect of these issues. Of a number of the other issues in *Capitalism and Freedom* and *Free to Choose*, one might feel that whilst the arguments are sincere, they are not expected to bring fruit. Obviously he was happy to attract controversy over all these sorts of things, as nearly everything else about his life indeed suggests. Perhaps it was not controversy, though, but notice that he wanted, and he did himself no good by the way he went with his popular argument for the monetarist case late in the 1970s and the 1980s. He probably damaged the cause as well, since the discerning observer could see there was nothing much to some of those arguments, and they made easy targets. It is rather a pity that it was as his views on inflation started to gain acceptance that he started to put them so poorly.

24
Part IV Conclusion

Friedman was, as has been said so many times, a brilliant popularizer of his ideas. This shows so clearly in a large number of his *Newsweek* articles, the presentation of the free-market ideas of Friedman (1962a) and Friedman and Friedman (1980), and in the cases of drugs and conscription, that are his great contributions. As regards the case for market mechanisms, the second of the books is a better book by far than the first. Though it has a poor chapter on inflation, *Capitalism and Freedom* is badly marred by Friedman's foolish attempts to grapple with 'liberalism'. Yet it is the first that is so much admired. It was so much admired, indeed, that when it came to producing a festschrift for Friedman, in the form of Selden (1975), the theme of was not monetary theory, not monetary history, not even consumption, but that of capitalism and freedom.

The difference between the reception of the two books is not going to be explained by the arguments about education vouchers or deregulation having turned sour, or by public housing having started performing its intended role, and his arguments thereby being proven wrong. Rather, it seems a much better conjecture that the way he conducted himself over the public argument about Chile, as well as those before *Free to Choose*, concerning inflation and monetary policy, contains

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_24

much of the answer—he was behaving like a charlatan as Robert Neild recalled it. The same sentiment is very evident in Kaldor (1982), where there is a scathing attack on Friedman in the introduction (and a more scathing one in Kaldor (1985)). The problem was that 'Inflation is always and everywhere a monetary phenomenon' did not mean quite what, by 1975, he wished it meant. It was a catch-phrase for him, but its meaning needed to be adapted to suit his later purpose. Friedman and Schwartz (1963a), for which he earned and deserved so much praise, had not quite been about the issue that was pertinent to the troubles of the time. But Friedman behaved as if he and he alone had answers, and as if all that time, he had been battling over the matters that in the 1970s and 1980s were the focus of public concern. But it was not quite true.

As to *Capitalism and Freedom*, though, it is really only dressed up as a book about freedom. It is a book about persuading by many little examples, that the market offers a fine mechanism for achieving more or less agreed ends. I suppose that it is so highly regarded because in 1962 these arguments needed to be made, and there is a heroism about Friedman's making them—blatantly insisting on the brilliance of the market, when everyone else seems to have some kind of doubt, when there was plenty of room for doubt, indeed, as to whether the capitalist system would survive the Communist challenge. And Friedman was not just unrelenting but much more importantly, unabashed, not in the defence of the market system, but crusading for its advance.

25

Conclusion: The Legacies of Milton Friedman

If Friedman has taken to growing turnips in 1958, he would have been a most notable economist. *A Theory of the Consumption Function* would secure that. But there is also his evidently substantial share in a detailed and innovative—too innovative for some tastes—study of professional incomes, the Friedman–Savage utility function and other contributions on utility theory giving him an important position in the development of consumer theory. *A monetary and fiscal framework for economic stability* might alone give him a claim to being one of the clearest and deep-thinking of the advocates of rules of that time. But that needs to be seen with his analysis of the destabilizing potential of imperfectly-timed policy—something notable enough to be referenced in his Nobel citation. And there is *The case for flexible exchange rates*, which was to prove a prescient essay. Not only that, but it provides the outline for most of the arguments over fixed and flexible exchange rates for at least two decades, and some hints at other questions that were to feature international economics over an even longer period. In addition to those, there is a host of imaginative and clever works on such a variety of matters as the thought of Alfred Marshall and Wesley Mitchell; the power of trade unions; the determination of the distri-

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4_25

bution of income according to individual preferences and the social organization consequently required to optimize incentives in the light of it; contributions on the variety of possible monetary standards and their pros and cons. All that leaves out some notable contributions in statistics, and two extraordinary, long book reviews which describe a manifesto for the conduct of research in economics. It is a striking characteristic of all this work that the empirical agenda advocated in those reviews is implemented again and again in his own work. When Friedman addressed a problem in economics he did it with a view either to describing the actual world or devising ways of finding out how best to describe it.

On strict chronology, there was also *The Quantity Theory of Money—A Restatement* in that period, but there is no need to be fussy about that since Friedman was nowhere near the end of his work. A second phase of his career can be seen as beginning with that essay. It made a theoretical pitch which had in fact already come to inform much of Freidman's work but was to be central to his academic publications and all the disputes they aroused up to 1974, and some of them after that date. Just for its scholarly content, *A Monetary History of the United States* stands far above almost any postwar work in economics. Friedman only shares credit for that, of course, but then he has a share of the credit for one of its nearer rivals in the shape of *Monetary Statistics of the United States*, too, and according to taste, *Monetary Trends in the United States and the United Kingdom* might be added.

Friedman's work tended to be controversial—it goes hand in hand with being innovative—but controversy over his work on money and monetary policy does seem to have developed a harder edge than that over his earlier work. His work on the consumption function generated controversy, but it was fascinated controversy—controversy from those who wanted to engage with his ideas and certainly to test them, but also to carry forward a project of understanding consumption in terms like his. There was a different atmosphere in the argument over monetary policy. Admiration of the scholarship of *A Monetary History* could be there, and undimmed, but it is not to that book that one looks for the source of dispute. Rather it is to Friedman and Meiselman, and to a lesser extent *A Theoretical Framework for Monetary Analysis*. These, not *A Monetary*

History were the works by which Friedman sought to establish that his way of looking at things was to be preferred to the alternatives. In this, just the same empirical approach as in the earlier period is evident. In relation to the empirical issues he was raising, Friedman consistently sought means of achieving empirical resolution. His success in setting that agenda, and bringing the argument to the issues he raised is most notable. On the other hand, it is also notable that on those specifics, he seems to have persuaded few of those who ever doubted his views.

The contrast between his ability to set the agenda and his inability to persuade those he challenged might be explained in a variety of ways. Perhaps it was just that he was, after all, the one challenging orthodoxy. Perhaps he should carry a burden of proof. It might also be that just as so many were willing to argue endlessly about *The Methodology of Positive Economics*, so it may be that presented with a new way of thinking about how to compare theories, they were pulled towards assessing that method as well as using it to assess the theories. In that case, the approach of Friedman and Meiselman was never going to be accepted as amounting to a run-off between two theories, but rather, in effect, as a contest between a method and a theory, against a theory. Inevitably, doubts about the method could be found, and they become doubts about the confidence to be placed in Friedman's conclusions. Or one aspect might be that by the time of Friedman and Meiselman he had a record of raising controversy and might easily be identified as a maverick, and hence as someone whose views were not to be accepted until thoroughly tested. Perhaps the cleverness, and sometimes the slipperiness, of his earlier arguments also suggested a warning that one should be on the lookout.

All that is a little speculative and more than a little sociological, but whether right or wrong in substance, in that spirit one can hardly deny to Friedman some sympathy if he found the experience frustrating. He was the one bringing to the debate theory which could be confronted with data. That was his modernized Quantity Theory. He had confronted it with data. In some confrontations—like the long views of *A Monetary History*—theory seemed to perform very well. In others, such as his analysis of business cycles or the demand for money, he might have admitted that the matter was less clear, but he would have been perfectly entitled to the view that nothing transpired which definitely required the rejection of the theory. This not being enough, he offered his opponents a head-to-head test between his theory and theirs, but could not persuade them. Meanwhile, amongst his opponents, though they addressed the specific tests he offered, it surely seemed to him that there was much less inclination to bring a wide range of data to bear in seeking an overall assessment of the matter. In all this, quite possibly, Tobin's *Post hoc ergo propter hoc* contribution was the most frustrating of all. A clever analyst could show that it was possible for money and activity to move in either sequence in either theory, and to treat that as defeating Friedman. For all its cleverness, it was taxonomic reasoning, and from Friedman's point of view, nothing more.

His popular engagement offers another whole dimension to his work. From an early stage he had made many media appearances, but the Volker Fund lectures, and most importantly *Capitalism and Freedom*, and his *Newsweek* articles, as well as his advising of Goldwater, Nixon, and Reagan in the 1960s and early 1970s made him into a pre-eminent public intellectual, at least in the United States. The positions he took in *Capitalism and Freedom* and *Newsweek* are arguably as remarkable as his academic work. They are not as deep, but they do show his cleverness in seeing the hidden shape—or possible hidden shapes, at least—in economic relationships, and describing them in systematic terms. The consistency and ingenuity with which he pointed to the otherwise unnoticed benefits of the price mechanism should command great admiration. Probably there is more than that to be said since although he seems to have stopped short of extending his ideas to where they would have been socially shocking, there was also a fearlessness about his position. It is not so much the fearlessness of taking on powerful lobbies, but simply that of putting himself in what he had reason to expect to be a very small minority. To be persuaded—and to declare it—of the anti-social behaviour of the American Medical Association takes more than just ingenuity and cleverness.

In a third phase of his career, whilst such engagement with policy and policymaking continued, his more purely academic work was of much less note. There was, in this period, *Monetary Trends*, but by 1982 when it appeared, it was well outside its temporal context, and whatever merits may be found in it, its reception was poor. Apart from that, after *A Theoretical Framework for Monetary Analysis*, the scattering of academic work he produced was much less weighty than it had been earlier in his life.

There was, though, in this period, a perverse twist of history in that Friedman was seen to win the argument against the Keynesians. That came when he switched to demeaning Keynesians and policymakers in equal measure for the supposed centrality of the Phillips curve in their thinking. There was no sign of that in earlier rounds of the debate he had orchestrated. But when he advanced a position in terms of the Phillips curve, the alternative Keynesian 'language' of earlier debates was gone, and replaced by outright foolishness. And the Quantity Theory, around which everything had previously revolved, featured, if at all, only by the pretence that its important feature was the making of a distinction between real and nominal variables. It was here—most of all through his Nobel Lecture—but only here, that he won the widespread accolades of victor in the great 'Keynesians versus monetarists' debate. But winning it there, of course, was winning it on false pretences.

Whilst serious academic work took a back seat, with his foreign trips—notably those to the United Kingdom, Chile, South Africa, and Australia, and rather later, China—and the commentary on policy in those places that he made; and through *Free to Choose*, and its television version he must also have become much better known outside the United States. No doubt also because of the award of the Nobel Prize in 1976, and the adoption of policy apparently motivated by his ideas in many countries he became much more of a global figure than he had been up to then. It is at this time, too, just as his policy ideas started to gain some traction, that he also came to be so vilified, first over his involvement in Chile, and then more generally. His views as well as the places and ways in which he expressed them attracted much more adverse criticism. Some of that, presumably because of its personal character, and because his motivations were questioned, he seemed not to like at all, despite the taste he had previously shown for controversy.

Before considering the question of the legacy of his work, there are two observations to make. The first concerns the narrowness of his capabilities. The foolishness of some of the things he said is palpable. That is true of so much on methodology or causation, as well as things like that remark about Verwoerd's theory of apartheid. Some, no doubt, feel I have been very hard on him for silly mistakes he made about such matters as Athens being in the Peloponnese, and state action never having led to any great science or architecture, and the like. But there is a pattern there, of which those things are a part. His account of the philosophy of liberalism was very weak, though he kept repeating it. His idea that he had a story to tell about the development of liberal thinking in nineteenth-century Britain was absurd. He should have realised it, because its only authentic source was apparently Dicey and however often he repeated his story, he never seemed to have felt an inclination to learn any more. That ever-recurring reference to that author was presumably supposed to impress the readers of the moment, but it can hardly do so when one reads more of what Friedman says, and finds there is only Dicey he knows. It is just faux learning, and wretchedly so when one notices that the references that suggest his wide learning so often seem to be specific sources he picked up from his colleagues, and not really his learning at all. Dicey was mentioned by Samuelson (1951b) in a book to which Friedman contributed; Stigler (1954), his friend, discussed J. N. Keynes and presumably put Friedman on to him; and a letter from Schwartz reveals that she suggested the du Pont quotation that popped into his Nobel lecture to him.[1]

Outside his narrow domain, then, he had very little to offer, and so little, indeed, that he seems to have been hardly aware of it. And the same sort of thing is probably more or less true of his stories about the development of monetary thought as well. In that case, perhaps the stories were just propaganda, told for the purpose of putting his work at their centre. But his reticence about saying it was his work that he was talking about might suggest otherwise, and in any case, it could just as well be that he did not know much more. Certainly, there is the same appearance that he had no inclination to learn any more as he repeated the story. And there is the methodology essay which, as an account of what he was thinking or what he was doing, is simply a failure. And it

[1] Hoover Archive, Milton Friedman Collection, Box 109 File 16.

too shows neither learning in the subject, nor the slightest concern at the lack of it.

The implication is clear enough. It is that talented though he was in interpreting economic data, and finding patterns in it, and suggesting explanations of behaviour, he was entirely lacking in intellectual breadth. His scientific economics was brilliant and his popularizing of economic ideas outstanding. But when he sought to deploy forms of analysis that were not centrally concerned with the analysis and systemization of data, or the effect of various incentives on agents being studied, he was all at sea, and often looked foolish. In history, methodology, appreciating or articulating any difficulty about causation, or such things as sensitivity to the political environment in other people's countries it is the same story. Actually it is the same story in autobiography too—there is no sign that he had any idea how to go about it. The one outstanding exception to that was when he said that he and Stigler 'lived, breathed, and slept economics'. He was certainly right about himself.

Another observation concerns his methodology and in particular, its relation to the Popperianism with which he is often associated. There is nothing more to say about the methodology essay itself, but on some of his other remarks, and on the question of how he went about his own work there is, and taking his economics as a whole a clear picture emerges.

His criticism of 'taxonomic' theorizing was well aimed, although it might be doubted that there is never a role for such an approach. And Friedman's idea that his own empirically-based work offered something that such reasoning could not is quite right. He described that difference in terms of 'falsifiability' but that—because of the association of the word with Popperian approaches—is a little misleading. Friedman's contrast was between taxonomic theory and theory to be tested. The former was theory specifically designed to cover all cases. Observations might be fitted into it, but could not test it. On the other hand, there were empirical approaches which suggest the world is a certain way and which may therefore turn out to be incorrect if the world is in fact another way.

Popper's distinction was a different one. Consider, for example, this remark on how the acceptance of a theory affects some minds. Acceptance of a theory, he said, had,

> the effect of an intellectual conversion or revelation, opening our eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still "un-analysed" and crying aloud for treatment… A Marxist could not open a newspaper without finding history; not only in the news, but also in its presentation – which revealed the class bias of the paper – and especially of course in what the paper did *not* say.
>
> Popper (1963, pp. 34–35)

That was not the complaint Friedman made against Lange or Lerner. They were not said to see confirmations of their theory in the data. And on the other hand, the complaint by Popper is not that Marxism was *designed* as a taxonomic system. Surely the Marxist would say, for example, that if the capitalist classes, of their free will, ceased exploiting labour, the theory would be falsified. It is not, in the taxonomic way, that there was another branch of Marxism that covered worlds like that. Similarly, Friedman would say that if velocity proved unstable, that would falsify his theory. But Popper's complaint was about the invulnerability of theory in the face of apparent refutation.

Making the necessary changes to the last few lines of the quotation, to remove the references specifically to Marxism, it is hard not to see this as a description of Friedman's view—and that of so many of his followers—of the Permanent Income Hypothesis, the stability of velocity, the failure of price control, or the futility of discretionary policy. Indeed, the point has been decisively confirmed by Diesing (1985), who, in an admirably forensic study, showed that in Friedman's hands, some evidence fits the theory; some evidence, adjusted, fits it; sometimes ad hoc changes can be brought to secure a fitting, but without leading to any revision of the substance of the theory; sometimes the data is presumed incorrect; and sometimes something is left unexplained and declared to be a puzzle.

It is not that falsification cannot be imagined. But with that last resort, the resort of the unexplained puzzle, available or, one suspects, in extremis, even without it, when a skilled analyst is in control, nothing is going to be admitted to be a falsification. And indeed, in one of his discussions of 'why economists disagree' Friedman also revealed an attitude very much like Popper's description of the advocates of one of these theories. His (and Rose's) bafflement at the disagreement of others with his theories makes the point very clearly.

Another point worth noting, though rather more subtle is that Friedman did not go to great lengths to expand the range of potential falsification of his theory. For example, he never took up the matter of improving understanding of the lags in monetary policy. From 1963 at the latest—that is, from Friedman and Schwartz (1963b), this was the presenting scientific problem to which Friedman had no good answer. It was also a central one in shaping his policy proposals, since his 'long and variable lags' were at the heart of his case for rules. I suppose one view would be that the economic system is such that the links between money and nominal income cannot be understood in detail and the idea of 'long and variable lags' merely stands for that view. But that is to give up on science, and indeed, Minsky (1963) and Warburton (1963) had suggested approaches to addressing the issue. The Popperian, thinking in terms of the logic of advancing the subject, would surely think the lags were the things to be studied and understood. It would be by developing a more detailed and more accurate account of the determination of the lags that one would both understand macroeconomics better, and produce the additional organized data that might provide a true challenge for the Quantity Theory. It is the devising of tests such as those, rather than Friedman's approach, for example, of reporting the correlation between money and prices in additional countries, that would be the Popperian approach. Rather than telling his readers that he knew of no cases where an inflation was not accompanied by an increase in the money supply, he would have been telling them that in addition to there being no falsifying instances, the details of the relationship were coming to light. Or perhaps, for example, it would turn out that the means by which the money supply was increased was a determinant of the length of the lag. Or perhaps, he would have had to tell them that enquiry into the details led to the falsification of the theory. Either way, *that* is scientific progress in the manner of Popper.

In this kind of way, again, Friedman was no Popperian. Similarly, having satisfied himself as to a general relationship between money and nominal income in other countries, he could have enquired into the causes of the Depression too, on the same basis as he analysed that in the American case. But these sorts of tests do not seem much to have interested him.

The question of his influence must also be considered, though it is a difficult matter, since 'influence', one might reflect, is a tricky word. One mistake made astonishingly often in the case of Friedman is to attribute to him originality in proposing ideas which were in fact routine at the time he stated them. There is the response to Kuznets' consumption data in terms of the variability of income; the idea that economic models work by assuming decision-taking processes operate only 'as if' according to detailed calculations; the ideas that expectations adjust to reality and that long-run equilibrium is determined by relative rather than absolute prices; and sometimes it would even be possible to form the impression that no one had thought that inflation might be a monetary phenomenon before Friedman came up with the idea. And it is not, to be clear, that someone, somewhere had once had these ideas before Friedman, but that they were commonplace in the intellectual environment in which he worked. He has no claim to originality in any of these points.

He might have a claim to having been the one who led to their acceptance. Rather than merely understanding the ideas, perhaps it was by Friedman that economists were led to believe them. This would be more true in some cases than others. Again, influence is a tricky matter. It is hard to know how to confirm it for certain, but surely his work was central in persuading very many economists that the extent of cost-push inflation had been exaggerated and that demand control was necessary to inflation control. In this case, it would be the power and quality of his research, and the consistency of his putting the case that brought his influence. Something like that could be true of the arguments he put in relation to consumption as well. One could say that he was 'influential' in persuading economists to go further than they otherwise would have in the direction of making unrealistic assumptions. But it would not be the power, quality, or consistency of his argument on that point, but

I suppose just the happenstance of one of his essays finding its way onto more reading lists than it should have, and this being the lesson that students understood they were expected to draw. It is a kind of influence, but not the same kind. On the question of the understanding of the effect of ongoing inflation on wage bargaining, though, it is difficult to see that anyone was in enough doubt about it for him to have influenced thinking in those areas—unless, by his controversial stance, he might have caused the expectations argument to be called into question by some, as I suggested in Forder (2010b).

Influence of a different sort would make it a vaguer, more general matter of the extent to which waters of a certain colour from the Friedmanite tributary flowed into wider stream of economic thinking. Again, his monetary analysis and work on consumption would usually be said to offer the strongest flows, but most of an assessment must be judgemental. To my eye, the Friedmanite pigmentation of theory fifty years later in the area of consumption was the stronger of the two. It is not uniquely Friedmanite, of course, so his influence is shared. Still, the modern starting point in considering consumption is, I would think, that households have a reasonably good idea of their financial position over a number of years, and plan their expenditure as best they can, in the light of that picture. In detailed analysis, there may be any number of qualifications, and some of them will be quite foreign to *A Theory of the Consumption Function*. But the influence of Friedman's thinking is established by the starting point, not dissolved by the fact that it is only a starting point. In monetary matters, though, it is really not true that the Quantity Theory is so much the starting point.

On money, a different point arises. That is that it would be a pity, and an intellectual delinquency to suppose that the value of *Monetary Statistics* and *A Monetary History* arise merely from their influence on economic analysis. Great works of scholarship do not necessarily have much in the way of that kind of *influence*, but they are still great works of scholarship. Would that there were more economists who would set themselves to study the facts in such detail as Friedman and Schwartz did. Certainly, that book led to much more work, by Friedman, by those who responded to him, and by others. Beyond that, the matter is less clear. Despite the casual way that its enormous influence is often declared, there is a question as to exactly where it lies. The question would stand more investigation, but there does seem to be doubt as to whether the proposition that 'money matters' was ever as emphatically denied as is sometimes suggested, especially by Friedman. Such things as the idea, sometimes voiced, that it was because of *A Monetary History* that central banks responded in the way they did to the crisis of 2007–2008 seems even less well-founded. They responded to prevent bank failures. But it is surely not being claimed that 'the Keynesians', or anyone else with influence on policy thought that widespread bank failure was unimportant. The claim of Friedman and Schwartz was much more specifically, that bank failure was damaging because of its effect on the quantity of money. That thought is hardly the one influencing post-crisis policy. And if it was debt deflation that the policymakers were worried about, that comes from Fisher (1933) and is something to which Friedman, or Friedman and Schwartz gave less attention than might have been expected.

On his more general influence on monetary policy, attention should also be given to the point that Friedman did more than argue for certain theoretical conclusions, but advocated some specific policy responses as well. These are not in the character of developments that might be added to his basic theory, as in the case of consumption. They are independent proposals in themselves. Over the long view, he certainly did not win on all the specifics he thought important. And equally, it is hard to see that there has been some particular theoretical development which would have changed his view.

One might consider four specific battles he fought: over floating exchange rates; rules and discretion; the appropriate operating target for policy; and central bank independence. Floating exchange rates have largely been implemented, the outstanding exception being much of the European Union, though there, the creation of the euro makes for a properly fixed system, and although he doubted the wisdom of the particular proposal it is perhaps not of a kind to which he objected. On the other hand, this is hard area to argue that what happened, happened because of him—indeed, he denied that it did, and was plainly right in this. The question of rules and discretion is a little more difficult. Many policymakers declare themselves to operate by 'rules', but as Rivot (2015) has argued, what they support would not be ‘rules’ in Friedman’s sense. And where the ‘rule’ is in fact an inflation target, it fails the test set for example in Friedman (1968a) of being a variable directly under the control of the policymaker. And in so far as policy is rules-based, the stated rationale tends to owe more to the rational expectations theorists than to Friedman, and it might be recalled that Sargent and Wallace (1975, p. 242) gave an impression of finding Friedman’s reasons for a rule ‘foolish’. Really, to make all that show the influence of Friedman specifically would be quite a job. On the question of the operating target, Friedman clearly and insistently advocated the quantity of money, and did so specifically in comparing it with interest rates. In this area, his position has been rejected almost completely. And then there is central bank independence. One can question what is meant by ‘independence’ and argue that some central banks would not meet an objective definition, but if it is Friedman’s influence we are looking for, the appearance of the thing might be as significant as the substance. On this, it could hardly be clearer that Friedman was on the losing side. For all that is said about his personal impact in forming the way policy is made, on the details of the matter, his views seem to have had very little weight.

In the light of these sorts of points, though, it is all the more important to insist that ‘influence’ is not everything. The standing and admiration deserved by a scholar is not measured in that kind of way. *A Monetary History* is no less a great book because of facts like these. But many of the Friedman fan-club seem to suppose it, and his following work, had effects that they really did not.

Other aspects of legacy arise from more diffuse sources. One is that he left something of a legacy of Manichean dispute. Although there are certainly earlier traces, the idea that macroeconomics revolved around a contest of ‘Keynesians versus monetarists’ probably came to prominence in the middle 1970s. It would be an interesting enquiry to see where and how it began, but Friedman’s virulent attacks on ‘Keynesianism’ in that decade must be part of the picture of its development. It was visible much later both in the naming of ‘New Keynesian’ economics—plainly intended to re-establish propositions presumptively rebutted—and in the frequent emphasis put on new discoveries of Keynesian truths by their authors. It is unfortunate in itself, but the impression that

Friedmanism should be seen as having been over a long period a rejection of Keynesianism also gives a poor impression of Friedman's work. For most of his career, his attitude to Keynesianism was much more subtle and somewhat more sympathetic than the picture organized around the Phillips curve suggests.

There is, however a different picture one might adopt. It is to be recalled that Friedman was not new to economics in 1970, but having published his first paper in 1935 was certainly someone who learned his first economics in the world before the *General Theory*. So he had another way of looking at things—presumably a way substantially derived from Mitchell and Burns and their predecessors, and certainly, as we know from the signs of his later change of view, not a narrowly Quantity Theoretic one. He was not, therefore, faced with an existing Keynesian orthodoxy to accept or reject, but rather with the question of whether the analysis of the *General Theory* appeared to add useful tools. His earliest comments, from Friedman (1941b) and Friedman (1943a) point to limitations in the Keynesian approach and say that he thought it added little. But that does not exactly make him an anti-Keynesian. In Friedman (1948a) he advocated stabilization policy, even if rules-based, and in Despres et al. (1950) found nothing in fiscal or monetary policy about which to dissent from the other committee members—and it is not that they were notably anti-Keynesian themselves. There is nothing there to make him anti-Keynesian either.

In Friedman (1948a) he also noted the Pigou effect, but did not present it specifically as showing Keynes to be wrong. That, he did do in Friedman (1956c). It appeared rather mysteriously in that role in Friedman (1957a) and thereafter Friedman seemed to develop something of an obsession with it. It is rather peculiar since it is difficult to see it was of practical significance, and furthermore, it is difficult to see that there was really any evidence supporting it. On this matter, that issue did not seem to concern him and from the mid-1950s he consistently presented the Pigou effect as showing that Keynes was wrong. Even that, though, is not a firm rejection of the view that Keynesian policy offers a practical approach and certainly made no case that Keynesian analysis was deeply foolish.

In the following years, he saw more and more importance in the Quantity Theory, but when led to reformulate it, did so in terms he was in due course happy to admit had a Keynesian influence, and in the same period wrote a book about that most Keynesian of concepts, the consumption function. As I have argued, it was a much less anti-Keynesian book than is sometimes implied. Friedman and Becker (1957) and Friedman and Meiselman (1963) take him a step further, but it is not a great step—there are two approaches, and the question is that of which one yields more useful forecasts. It is not a matter of fundamental analysis, and there is nothing prohibiting a view that it is a close decision. Friedman (1968a) was about rules, not the Phillips curve, and although he certainly objected to fine-tuning, there is nothing specifically anti-Keynesian in that (and it was fine-tuning by monetary policy that he discussed, while elsewhere his position was Keynesians had little use for that tool at all). And then there was Friedman (1974a) which, like Friedman and Meiselman (1963) is certainly an attempt to choose between two theories, and certainly one that favours, in Friedman's mind, the Quantity Theory. The 1974 version perhaps shows more overt hostility to Keynesianism, but again, there is not much in the way of an allegation that Keynesians were fundamentally confused, or that their framework definitely could not provide useful answers. In particular his discussion of the Phillips curve presented it entirely as a progressive development. In any case, as Rivot (2012) argued, there are much more substantial commonalities of view between Friedman and Keynes than the later understanding would permit. So to see Friedman, as he sometimes seems to be seen, as something like the anti-Keynes is much less than a perfect representation.

True enough, in those years, anti-Keynesians of any sort, certainly those of Friedman's standing may have seemed to be in short supply, so he might still stand out. And there is the matter of his objections to interference with markets, which, though not quite 'Keynesianism' was a feature of policymaking. Friedman (1986a) in due course made the association between the two in Friedman's mind clear—although that too is from a later date. But in terms of damning Keynesian macroeconomics, it is really Friedman (1975a) and his allegation that the Phillips curve was at the heart of it that marks the beginning. It was only when, as inflation rose, he latched onto the Phillips curve as somehow proving he had been right all along, that the really black-and-white aspect of his anti-Keynesianism came into his presentations.

Before then, Friedman had never been a Keynesian, but he had really not been nearly so much of an anti-Keynesian as is sometimes suggested either. It is a perception of the later period that there are only those two positions and if he was not one, he must have been the other. And it is a further perception of later times, that he was, from an early stage, set upon a principal task of undermining all the various aspects of Keynesian thinking.

By something of the same token, the relationship between Friedman and Lucas might not be seen as it is sometimes is. The kind of picture from Tobin (1981) of Lucas offering a second version of monetarism seems to place Friedman in the role of being his precursor. As Hoover (1984, 1988) saw it, Friedman and Lucas were related by having equivalent accounts of long-run equilibrium and in virtue of Lucas taking his lead from Friedman. Like de Vroey (2016), he saw the crucial difference between them as methodological and presented it as the opposition between Friedman's 'Marshallian', and Lucas' 'Walrasian' approaches, though de Vroey also noted that this put Friedman on the same side of that divide as Keynes.

On the crucial matter of expectations formation, Friedman never committed himself to a general approach that described more than that in due course expectations would catch up with reality. It is a paradigmatic piece of common sense more than anything, and that is surely how it was intended. The insistence that the only appropriate way to model the matter is by means of rational expectations, as de Vroey explains Lucas' approach, is very far indeed from that. And even the point that Friedman and Lucas share a common starting point, or alternatively that Lucas took his lead from Friedman in rejecting the Phillips curve is clearly one that needs to be reformulated in the light of the facts. Since Friedman was not the originator of the idea that continuous inflation would change expectations, and since it is in any case not such a problematic one that Lucas can be said to have needed any help appreciating it for himself, there seems nothing very profound that would link them at all. Friedman should be seen as applying sense to the matter—as he did to the matter of consumption. But it is common sense, not high theory. The Lucasian departure is one of a quite different kind. So to regard Friedman as in some way the precursor of Lucas is to take entirely the wrong view of his idea of the relation of theory and experience. It is, as de Vroey said, the work of Lucas that marks the great departure from previous thinking.

Something more needs to be said about the natural rate of unemployment. Certainly, Friedman created some terminology, and that saturates the later discussion of macroeconomics. It is also clearly the case that during the 1970s and 1980s a new view emerged which put the natural rate, or some similar idea, at the centre of analysis. It is natural to link the two. But once it is recognized that there is nothing of much factual substance in the story about Friedman debunking a prior wisdom cast in term of the Phillips curve, it is none too clear what that link might be, or what should otherwise be said. It would be another project to investigate that question, but what is apparent is that the influence of Friedman in these developments must be very much less than is often supposed.

There is another area, though, where Friedman may have left a greater legacy than is often noticed. That concerns his support for and advocacy of the price mechanism. It is a notable aspect of his life and work that on this, he was not only unwavering, but ferociously determined as well. It was the value of the price mechanism that motivated Friedman and Stigler (1946), Friedman's dissent from Despres et al. (1950), and Friedman (1953d). It was at the heart of his opposition to incomes policy, and his break with Nixon. At that time it was the basis for his declaration that controls are 'deeply and inherently immoral' (p. 365 above). He kept up that kind of treatment through the 1970s. As I noted, when he supported indexation, he should have seen that there was a plausible, nearly parallel case for wage control, but he does not seem to have been willing to contemplate it. He was entirely unswerving in his defence of the price mechanism. I even wonder whether his fascination with the Pigou effect finds some of its motivation here as well. The point was that Keynes had alleged a flaw in the *price* system. Perhaps that was the idea to which Friedman took such exception.

In this, three things about his position can be distinguished. One is simply the importance and effectiveness of the price mechanism in directing resources. But another point that Friedman saw rather more clearly than others, is that attempts to regulate prices often have the effect, not of overruling the mechanism, but of displacing it. The restriction of foreign exchange creates the black market; wage control generates employment perks. And in a third aspect, again and probably more so, a perception of Friedman, there is much more to the moral worth of the price system than is often seen.

On that question, there is the point that it facilitates people reaching their own decisions about what to supply or consume, and in the end, what kind of life to lead. Another point, and a recurring clever theme of *Capitalism and Freedom*, for example, is the possibility of utilizing the price mechanism in ways not previously considered, and doing it with moral purpose. In this, education vouchers were the outstanding example. There, redistribution and the price mechanism worked together. The design of the negative income tax had the same aspect of providing redistribution in combination with close attention to the incentives created. And actually, his whole story about 'freedom' did not really come to much more than allowing the price mechanism to work—whether the matter was professional licensure, drugs, provision of national parks, discrimination, or whatever. For Friedman, there was not much else to it, but for Friedman, that was a great deal.

Up to a point, in this area, it seems reasonable to say that he has had a great influence. To economists, these sorts of observations about the price mechanism seem very ordinary. Indeed the same line of thinking is taken much further in works such as Levitt and Dubner (2005). The authors of that book, though, acknowledge the inspiration of Friedman and indeed it is, in its focus on prices and incentives, a thoroughly Friedmanite book. Its inspiration lies, as it were, in seeing the cost of conscription as including an implied tax on the conscripted, and so many other arguments like that presented by Friedman.

Even in economics, and even in more moderate forms than those of Levitt and Dubner, ordinary as it seems, this kind of emphasis on the price mechanism represents quite a marked change. In policymaking one thing that clearly changed was that incomes policy was abandoned. There was surely a Friedmanite contribution to that. But in analysis and understanding too, things have changed. The most notable case may be that of the theory of wage bargaining. As discussed in Forder (2013), in the 1950s it really did have an aspect of denying the relevance of the power of price signals. And there is a fairly closely related matter in what I argued about the importance of *A Theory of the Consumption Function.* Friedman saw simplicity where others saw complexity. But they saw so much complexity that one might just as well say that Friedman saw order where they saw chaos. The order he saw, though was the order of purposive behaviour guided by prices. The insight was not exactly revolutionary, but Friedman's consistency in applying it, and insisting on it, were. And so it became with all of economics: that a piece of analysis without some form of price mechanism at work could be considered economics at all is almost unthinkable, but that was not true in the 1950s. Applied broadly, it is the line of thinking which makes it more precise to define economics as the analysis of rational behaviour, rather than, say, the study of the relation between scarce means and unlimited wants.

It must also be noted, though, that there is a further Friedmanite legacy in that this kind of economics sometimes seems to be tainted with Friedman's name. As it happens, he said himself, in Friedman (1967) that left-wing intellectuals so despised Barry Goldwater that they refused even to discuss the merits of his proposals. Some of that curse of Goldwater applies to Friedman's ideas as well. It is not often, for example, that the advocates of education vouchers or negative income tax seem to think their case will get a better hearing if they say that Friedman supported it. In that respect, his divisiveness is one of the things that limits his influence.

That divisiveness probably has many sources. His slipperiness, his association with contractionary policy in the 1970s and the 1980s; perhaps the impression of indifference created by the language of the 'natural rate of unemployment', and Chile, of course. And probably it finds some of its source in the uncompromising manner with which he put his case. But then, it is that manner; and his determination and consistent commitment to his case that makes him the outstanding advocate of the price mechanism. It is those things that make it Friedman's case, far more than that of others who had much the same view. His constant reiteration of variations on the point—in *Newsweek* and elsewhere—is part of it. And his willingness to push the arguments absolutely as far as they would go, and sometimes further is part of it as well. *Capitalism and Freedom*, I have said, was a clever book, but far from an erudite one, and there, as in more than a few other places, Friedman made some foolish remarks. But that fearlessness—the fearlessness that has as a by-product that the professor makes a highbrow clown of himself—was part of the man as well. It is the lack of caution that makes *Capitalism and Freedom* the book it is. And its being the book it is that makes it so important in conveying the power and the inevitability of prices and thereby transforming so much economic analysis.

References

Abrams, B. A. (2006). How Richard Nixon Pressured Arthur Burns: Evidence from the Nixon Tapes. *Journal of Economic Perspectives, 20*(4), 177–188.

Acheson, A. L. K., & Chant, J. F. (1973). Bureaucratic Theory and the Choice of Central Bank Goals: The Case of the Bank of Canada. *Journal of Money, Credit and Banking, 5*(2), 637–655.

Ackley, G. (1951). Review of Duesenberry, *Income, Saving and the Theory of Consumer Behavior*. *Review of Economics and Statistics, 33*(3), 255–257.

Akerlof, G. A., Arrow, K. J., Buchanan, J. M., Coase, R. H., Cohen, L. R., Friedman, M., et al. (2002). *Brief of Economists Amici Curiae in Support of Petitioners Eric Eldred et al.* In the Supreme Court of the United States.

Alchian, A. A. (1953). The Meaning of Utility Measurement. *American Economic Review, 43*(1), 26–50.

de Alessi, L. (1965). Economic Theory as a Language. *Quarterly Journal of Economics, 79,* 472–477.

Alford, R. F. G. (1956). Marshall's Demand Curve. *Economica, 23*(89), 23–48.

Aliber, R. Z. (1962). Speculation in the Flexible Exchanges: The European Experience: 1919–1926. *Yale Economic Essays, 2,* 171–245.

Aliber, R. Z. (1970). Speculation in the Flexible Exchange Revisited. *Kyklos, 23*(2), 303–314.

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4

Allais, M. (1953). Le comportement de l'homme rationnel devant le risque: Critique des postulats et axiomes de l'ecole Americaine. *Econometrica, 21*(4), 503–546.

Allen, C. L. (1954). Review of Friedman, *Essays on Positive Economics*. *Southern Economic Journal, 20*(4), 394–396.

Andersen, L. C., & Carlson, K. M. (1970). A Monetarist Model for Economic Stabilization. *Federal Reserve Bank of St Louis Economic Review, 52,* 7–25.

Andersen, L. C., & Jordan, J. L. (1968, November). Monetary and Fiscal Actions: A Test of Their Relative Importance in Economic Stabilization. *Federal Reserve Bank of St Louis Review,* 50, 11–24.

Anderson, R. L. (1946). Review of Friedman and Kuznets, *Income from Independent Professional Practice*. *Journal of the American Statistical Association, 41*(235), 398–410.

Anderson, M. (Ed.). (1982). *Registration and the Draft*. Stanford: Hoover Institution.

Anderson, T. W., & Friedman, M. (1960). A Limitation of the Optimum Property of the Sequential Probability Ratio Test. In I. Olkin, S. G. Ghurye, W. Hoeffding, W. G. Madow, & H. B. Mann (Eds.), *Contributions to Probability and Statistics: Essays in Honor of Harold Hotelling* (pp. 57–69). Stanford, CA: Stanford University Press.

Ando, A. K., & Modigliani, F. (1963). The 'Life Cycle' Hypothesis of Saving: Aggregate Implications and Tests. *American Economic Review, 53*(1), 55–84.

Ando, A. K., & Modigliani, F. (1965). The Relative Stability of Monetary Velocity and the Investment Multiplier. *American Economic Review, 55*(4), 693–728.

Angell, J. W. (1936). *The Behavior of Money: Exploratory Studies*. New York: McGraw-Hill.

Angell, J. W. (1941). *Investment and Business Cycles*. New York: McGraw-Hill.

Angell, J. W. (1957). *Journal of the American Statistical Association, 52*(280), 599–602.

Angell, J. W. (1960). Appropriate Monetary Policies and Operations in the United States Today. *The Review of Economics and Statistics, 42*(3), 247–252.

Archibald, G. C. (1959). On the State of Economic Science. *British Journal for the Philosophy of Science, 10*(37), 58–69.

Archibald, G. C. (1961). Chamberlin Versus Chicago. *The Review of Economic Studies, 29*(1), 2–28.

Archibald, G. C. (1963). Reply to Chicago. *Review of Economic Studies, 30*(1), 68–71.

Arrow, K. J. (1980, March 22). Review of Milton and Rose Friedman, *Free to Choose*. Harcourt Brace, New Republic, pp. 25–27.

Arrow, K. J., Barrett, W., Berger, P., Buckley, W. F., Jr., Draper, T., Frankel, C., et al. (1978). Capitalism, Socialism, and Democracy: A Symposium. *Commentary, 65*(4), 29–71.

Arrow, K. J., & Debreu, G. (1954). Existence of an Equilibrium for a Competitive Economy. *Econometrica, 22,* 265–290.

Artis, M. J. (1984). Review of Milton Friedman and Anna J. Schwartz, *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices and Interest Rates, 1867–1975*. *Economica, 51*(202), 205–207.

Asimakopulos, A. (1959). Review of Milton Friedman, *A Theory of the Consumption Function*. *Canadian Journal of Economics and Political Science, 25*(1), 77–79.

Attanasio, O. P., & Weber, G. (2010). Consumption and Saving: Models of Intertemporal Allocation and Their Implications for Public Policy. *Journal of Economic Literature, 48*(3), 693–751.

Aune, J. A. (2007). How to Read Milton Friedman: Corporate Social Responsibility. In S. May, G. Cheney, & J. Roper (Eds.), *The Debate Over Corporate Social Responsibility* (pp. 207–218). Oxford: Oxford University Press.

Bach, G. L. (1947). Monetary-Fiscal Policy, Debt Policy, and the Price Level. *American Economic Review, 37*(2), 228–242.

Backhouse, R. E., & Boianovsky, M. (2016). Secular Stagnation: The History of a Macroeconomic Heresy. *European Journal of the History of Economic Thought, 23*(6), 946–970.

Backhouse, R. E., & Forder, J. (2013). Rationalizing Incomes Policy in Britain, 1948–1979. *History of Economic Thought and Policy, 1,* 17–35.

Bailey, M. J. (1956). The Welfare Cost of Inflationary Finance. *Journal of Political Economy, 64*(2), 93–110.

Bailey, M. J., Olson, M., & Wonnacott, P. (1980). The Marginal Utility of Income Does Not Increase: Borrowing, Lending, and Freidman-Savage Gambles. *American Economic Review, 70*(3), 372–379.

Bangs, R. B. (1947). Review of Friedman and Stigler, *Roofs or Ceilings?* *American Economic Review, 37*(3), 482–483.

Baran, P. A. (1963). Review of Milton Friedman, *Capitalism and Freedom*. *Journal of Political Economy, 71*(6), 591–594.

Barber, C. L. (1966). The Quantity Theory and the Income Expenditure Theory in an Open Economy, 1926–1958. *Canadian Journal of Economics, 32*(3), 375–377.

Barber, W. J. (1975). The Kennedy Years: Purposeful Pedagogy. In C. D. Goodwin (Ed.), *Exhortation and Control* (pp. 135–193). Washington, DC: Brookings Institution.

Barger, H., & Landsberg, H. H. (1944). *American Agriculture, 1899–1939.* New York: NBER.

Barna, T. (1947). Review of Friedman and Kuznets, *Income from Independent Professional Practice*. *Economica, 14*(53), 64–68.

Barr, N. (2016). Milton Friedman and the Finance of Higher Education. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Barrett, C. R., & Walters, A. A. (1966). The Stability of Keynesian and Monetary Multipliers in the United Kingdom. *Review of Economics and Statistics, 48*(4), 395–405.

Barry, N. P. (1977, December 2). Milton Friedman's Political Ideas. *The Cambridge Review.*

Barry, N. P. (1978, February 3). Milton Friedman's Political Ideas. *The Cambridge Review.*

Basu, K. (1982). Review of Milton and Rose Friedman, *Free to Choose*. *Economic and Political Weekly, 17*(44), 1780–1781.

Baumol, W. J. (1951). The Neumann-Morgenstern Utility Index—An Ordinalist View. *Journal of Political Economy, 59*(1), 61–66.

Beams, N. (2006). Milton Friedman 1912–2006: 'Free Market' Architect of Social Reaction. *World Socialist Web Site.* https://www.wsws.org/en/articles/2006/11/frie-n21.html. Accessed 10 February 2019.

Becker, G. S. (1957). *The Economics of Discrimination.* Chicago: Chicago University Press.

Becker, G. S. (1960). Underinvestment in College Education? *American Economic Review, L*(2), 356–364.

Becker, G. (2017). *Much Ado About Nothing? Re-assessing Friedman's 'Restatement' of the Quantity Theory*. M.Phil Thesis, Oxford University.

Beggs, M. (2016). Review of Macroeconomics and the Phillips Curve Myth. *History of Economics Review, 64*(1), 79–84.

Benishay, H. (1962). Free Reserves Up or Level? *Journal of Political Economy, 70*(4), 403.

Berle, A., & Means, G. C. (1932). *The Modern Corporation and Private Property.* New York: Macmillan.

Bias, P. V. (2014). A Chronological Survey of the Friedman-Meiselman/Andersen-Jordan Single Equation Debate. *Research in Business and Economics Journal, 10,* 1–20.

Bird, R. C. (1963). Consumption, Savings and Windfall Gains: Comment. *The American Economic Review, 53*(3), 443–444.
Bird, R. C., & Bodkin, R. G. (1966). The National Service Life-Insurance Dividend of 1950 and Consumption: A Further Test of the 'Strict' Permanent Income Hypothesis. *Journal of Political Economy, 73*(5), 499–515.
Blaug, M. (1975). Kuhn Versus Lakatos, or Paradigms Versus Research Programmes in the History of Economics. *History of Political Economy, 7*(4), 399–433.
Blodgett, R. H. (1935). *Cyclical Fluctuations in Commodity Stocks.* Philadelphia: University of Pennsylvania Press.
Bloomberg, L. N. (1947). Rent Control and the Housing Shortage: A Commentary on 'Roofs or Ceilings?' by Friedman and Stigler. *Journal of Land and Public Utility Economics, 23*(2), 214–218.
Bodkin, R. G. (1959). Windfall Income and Consumption. *The American Economic Review, 49*(4), 602–614.
Bodkin, R. G. (1960). Windfall Income and Consumption. In I. Friend & R. Jones (Eds.), *Consumption and Saving*. Washington, DC: University of Pennsylvania.
Bodkin, R. G. (1963). Windfall Income and Consumption: Comment. *The American Economic Review, 53*(3), 445–447.
Boianovsky, M. (2005). Some Cambridge Reactions to the General Theory: David Champernowne and Joan Robinson on Full Employment. *Cambridge Journal of Economics, 29*(1), 73–98.
Boland, L. A. (1979). A Critique of Friedman's Critics. *Journal of Economic Literature, 17*(2), 503–522.
Boland, L. A. (2010). Review of Maki, *The Methodology of Positive Economics*. *Economics and Philosophy, 26*(3), 376–382.
Boland, L. A. (2016). Reading and Misreading Friedman's 1953 Methodology Essay. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.
Bordo, M. D., & Rockoff, H. (2013). Not Just the Great Contraction: Friedman and Schwartz's 'A Monetary History of the United States 1867–1960'. *American Economic Review, 103*(3), 61–65.
Bordo, M. D., & Schwartz, A. J. (1979). Clark Warburton: Pioneer Monetarist. *Journal of Monetary Economics, 5,* 43–65.

Boulding, K. E. (1948). Price Control in a Subsequent Deflation. *Review of Economics and Statistics, 30*(1), 15–17.

Boulding, K. E. (1954). Review of Friedman, *Essays on Positive Economics*. *Political Science Quarterly, 69*(1), 132–133.

Boulding, K. E. (1963). Review of Milton Friedman, *Capitalism and Freedom*. *The Journal of Business, 36*(1), 120–121.

Boumans, M. (2016). Friedman and the Cowles Commission. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Bowley, A. L. (1939). Review of Henry Schultz, *The Theory and Measurement of Demand*. Chicago: University of Chicago Press. *Economica, 6*(22), 213–216.

Bowman, W. S. (1963). Review of Milton Friedman, *Capitalism and Freedom*. *Yale Law Journal, 72*(7), 1469–1474.

Bradfield, M. (1982). Review of Milton and Rose Friedman, *Free to Choose: A Personal Statement*. *Canadian Public Policy, 8*(2), 265–266.

Brady, D. S. (1956). Family Saving, 1888 to 1950. In R. W. Goldsmith, D. S. Brady, & H. Mendershausen (Eds.), *A Study of Saving in the United States* (pp. 139–273). Princeton, NJ: Princeton University Press.

Brady, D. S., & Friedman, R. D. (1947). *Savings and the Income Distribution* (pp. 247–265). New York: NBER.

Brainard, W. C., & Tobin, J. (1968). Pitfalls in Financial Model Building. *American Economic Review, 58*(2), 99–122.

Brebner, J. B. (1948). Laissez Faire and State Intervention in Nineteenth-Century Britain. *Journal of Economic History, 8*(51), 59–72.

Brechling, F. P. R., & Lipsey, R. G. (1963). Trade Credit and Monetary Policy. *Economic Journal, 73*(292), 618–641.

Breit, W. (1999). Review of Milton and Rose Friedman, *Two Lucky People*. *The Independent Review, 3*(3), 453–457.

Breul, F. R. (1963). 'Capitalism and Freedom': An Essay Review. *Social Service Review, 37*(2), 201–207.

Bridgman, P. W. (1927). *The Logic of Modern Physics*. New York: Macmillan.

Brimmer, A. F. (1962). Price Determination in the United States Treasury Bill Market. *Review of Economics and Statistics, XLIV*(2), 178–183.

Bronfenbrenner, M. (1958). Review of Goldsmith, and others, *A Study of Saving in the United States*. *Annals of the American Academy of Political and Social Science, 318*, 183–185.

Bronfenbrenner, M. (1961). Statistical Tests of Rival Monetary Rules. *Journal of Political Economy, 69*(1), 1–14.

Bronfenbrenner, M., & Mayer, T. (1960). Liquidity Functions in the American Economy. *Econometrica, 28*(4), 810–834.

Brooks, D. (1998). *Econ-Icons*. https://www.nytimes.com/1998/05/31/books/econ-icons.html. Accessed 10 February 2019.

Broster, E. J. (1937). A Simple Method of Deriving Demand Curves. *Journal of the Royal Statistical Society, 100*(4), 625–641.

Brown, A. J. (1955). *The Great Inflation*. London: Royal Institute of International Affairs and Oxford University Press.

Brown, A. J. (1983). Friedman and Schwartz on the United Kingdom. In R. C. O. Matthews (Ed.), *Bank of England Panel of Academic Consultants*. London: Bank of England.

Brown, A. J. (1985). *World Inflation Since 1950*. Cambridge: NIESR and Cambridge University Press.

Brunner, K. (1970). The 'Monetarist Revolution' in Monetary Theory. *Weltwirtschaftliches Archiv, 105,* 1–30.

Brunner, K., & Meltzer, A. H. (1968). What Did We Learn from the Monetary Experience of the United States in the Great Depression. *Canadian Journal of Economics, 1*(2), 334–348.

Brunner, K., & Meltzer, A. H. (1974). Friedman's Monetary Framework. In R. J. Gordon (Ed.), *Milton Friedman's Monetary Framework* (pp. 63–76). Chicago: Chicago University Press.

Burns, A. F. (1949). Annual Report of the NBER. Wesley Mitchell and the National Bureau. In NBER (Ed.), *Wesley Mitchell and the National Bureau* (pp. 3–55). New York: NBER.

Burns, A. F. (1957). *Prosperity Without Inflation*. New York: Fordham University Press.

Burns, A. F. (1970/1978a). Inflation: The Fundamental Challenge to Stabilization Policies. [Remarks before the Seventeenth Annual Monetary Conference of the American Bankers Association, Hot Springs, Virginia, 18 May, 1970. Reprinted in A Burns (Ed.), Reflections of an Economic Policy Maker (pp. 91–102)]. Washington, DC: American Enterprise Institute for Public Policy Research.

Burns, A. F. (1970/1978b). *The Basis for Lasting Prosperity*. [Address at Pepperdine College, 7 December, 1970. Reprinted in A Burns (Ed.), Reflections of an Economic Policy Maker (pp. 103–115)]. Washington, DC: American Enterprise Institute for Public Policy Research.

Butkiewicz, J. L., & Abrams, B. A. (2012). The Political Business Cycle: New Evidence from the Nixon Tapes. *Journal of Money Credit and Banking, 44*(2–3), 385–399.

Butler, E. (1985a). *Milton Friedman*. Aldershot: Gower.

Butler, E. (1985b). *Milton Friedman: A Concise Guide to the Ideas and Influence of the Free Market Economist*. London: IEA.

Cagan, P. (1956). The Monetary Dynamics of Hyperinflation. In M. Friedman (Ed.), *Studies in the Quantity Theory of Money* (pp. 25–117). Chicago: University of Chicago.

Cagan, P. (1965). *Determinants and Effect of Changes in the Money Stock, 1875–1960*. New York: NBER.

Cairncross, A. (1991). Reconversion, 1945–51. In N. Crafts & N. W. C. Woodward (Eds.), *The British Economy Since 1945* (pp. 25–51). Oxford: Oxford University Press.

Caldwell, B. J. (1980). A Critique of Friedman's Methodological Instrumentalism. *Southern Economic Journal, 47,* 366–374.

Caldwell, B. J. (2001). Hayek: Right for the Wrong Reasons. *Journal of the History of Economic Thought, 23,* 141–151.

Calkins, R. D. (1953). Economic Research in Relation to Public Policy. *American Economic Review, 43*(2), 429–441.

Campbell, J. (1993). *Edward Heath*. London: Cape.

Cargill, T. F. (1979). Clark Warburton and the Development of Monetarism Since the Great Depression. *History of Political Economy, 11*(3), 425–448.

Carson, R. (1962). *Silent Spring*. Boston: Houghton Mifflin.

Cesarano, F. (2006). The Origins of the Theory of Optimum Currency Areas. *History of Political Economy, 38*(4), 711–731.

Chamberlin, E. H. (1933). *The Theory of Monopolistic Competition*. Cambridge, MA: Harvard University Press.

Champernowne, D. G. (1936, June). Unemployment, Basic and Monetary: The Classical Analysis and the Keynesian. *Review of Economic Studies, 3*(3), 201–216.

Champernowne, D. G. (1958). Review of Friedman, *A Theory of the Consumption Function*. *Journal of the Royal Statistical Society, 121*(1), 124–126.

Chandler, L. V. (1958). *Benjamin Strong, Central Banker*. Washington, DC: Brookings.

Chant, J. F., & Acheson, A. L. K. (1972). The Choice of Monetary Instruments and the Theory of Bureaucracy. *Public Choice, 12,* 13–33.

Chant, J. F., & Acheson, A. L. K. (1973). Mythology and Central Banking. *Kyklos, 26*(2), 362–379.

Cherrier, B. (2011). The Lucky Consistency of Milton Friedman's Science and Politics, 1933–1963. In R. van Horn, P. Mirowski, & T. A. Stapleford (Eds.), *Building Chicago Economics* (pp. 335–367). Cambridge: Cambridge University Press.

Christ, C. F. (1976). A Modest Proposal. In J. L. Stein (Ed.), *Monetarism* (p. 337). Amsterdam: North Holland.
Christ, C. F. (1994). The Cowles Commission's Contributions to Econometrics as Chicago, 1939–1955. *Journal of Economic Literature, 32*(1), 30–59.
Church Committee. (1975). *Covert Action in Chile, 1963–1973.* Washington, DC: Government Printing Office.
Clark, L. (Ed.). (1958). *Consumer Behavior: Research on Consumer Reactions*. New York: Harper and Brothers.
Clayton, G., Gilbert, J. C., & Sedgwick, R. (Eds.). (1971). *Monetary Theory and Monetary Policy in the 1970s*. Oxford: Oxford University Press.
Clower, R. W. (1964). Monetary History and Positive Economics. *Journal of Economic History, 24*(3), 364–380.
Coase, R. H. (1950). *British Broadcasting: A Study in Monopoly*. London: London School of Economics and Political Science.
Cockett, R. (1994). *Thinking the Unthinkable*. London: HarperCollins.
Collard, D. (1972). Review of *The Economics of Discrimination* by G. S. Becker. *Economic Journal, 82*(326), 788–790.
Collier, I. (2017). *NBER. Mitchell to Burns About Friedman. 1945*. http://www.irwincollier.com/category/nber/. Accessed 10 February 2019.
Committee for Economic Development. (1947). *Taxes and the Budget: A Program for Prosperity in a Free Economy*. New York: Committee for Economic Development.
Condon, E. U. (1949). *The Journal of Business of the University of Chicago, 22*(2), 134–135.
Congdon, T. (1983, July). Has Freidman Got It Wrong? *The Banker*, pp. 117–125.
Cootner, P. H. (1966). Review of Milton Friedman and Anna J. Schwartz, *A Monetary History of the United States, 1867–1960. History and Theory, 5*(1), 100–108.
Courchene, T., & Shapiro, H. T. (1964). The Demand for Money: A Note from the Time Series. *Journal of Political Economy, 72*(5), 498–503.
Cronon, E. D., & Jenkins, J. W. (1994). *The University of Wisconsin: A History* (Vol. 4). Madison, WI: University of Wisconsin Press.
Cross, R. B. (2001). The Friedman Memoirs. *Journal of Economic Studies, 28*(1), 55–64.
Crum, W. L., Fennelly, J. F., & Steltzer, H. (1942). *Fiscal Planning for Total War*. New York: NBER.

Culbertson, J. M. (1960). Friedman on the Lag in Effect of Monetary Policy. *Journal of Political Economy, 68*(6), 617–621.

Culbertson, J. M. (1961). The Lag in Effect of Monetary Policy: Reply. *Journal of Political Economy, 69*(5), 467–477.

Culbertson, J. M. (1964). United States Monetary History: Its Implications for Monetary Theory. *National Banking Review, 1,* 359–379.

Currie, L. (1934a). The Failure of Monetary Policy to Prevent the Depression of 1929–32. *Journal of Political Economy, 42*(2), 145–177.

Currie, L. (1934b). *The Supply and Control of Money in the United States*. Cambridge: Harvard University Press.

Dale, E., & Meloy, C. (1962). Hamilton MacFarland Barksdale and the DuPont Contributions to Systematic Management. *Business History Review, 36*(2), 127–152.

Darity, W. J., & Goldsmith, A. (1995). Mr Keynes, the New Keynesians, and the Concept of Full Employment. In P. Wells (Ed.), *Post-Keynesian Economic Theory* (pp. 73–93). Boston: Kluwer.

Davidson, P. (1974). A Keynesian View of Friedman's Theoretical Framework for Monetary Analysis. In R. J. Gordon (Ed.), *Milton Friedman's Monetary Framework* (pp. 90–110). Chicago: Chicago University Press.

Davidson, P. (1978). Why Money Matters: Lessons from a Half-Century of Monetary Theory. *Journal of Post Keynesian Economics, 1*(1), 46–70.

Day, A. C. L. (1955). The Taxonomic Approach to the Study of Economic Policies. *American Economic Review, 45*(1), 64–78.

Dean, J. (1941). *The Relation of Cost to Output for a Leather Belt Shop*. New York: NBER.

Debreu, G. (1959). *Theory of Value: An Axiomatic Analysis of Economic Equilibrium*. New Haven: Yale University Press.

Deighton, L. (1978). *SS-GB.* London: Cape.

Dellas, H., & Tavlas, G. S. (2009). An Optimum-Currency-Area Odyssey. *Journal of International Money and Finance, 28*(2009), 1117–1137.

Dennis, K. (1986). Boland on Friedman: A Rebuttal. *Journal of Economic Issues, 20*(3), 633–660.

DePrano, M. (1968). Money or Autonomous Expenditures? Tests of Alternative Hypotheses. *Business Economics, 3*(2), 35–41.

DePrano, M., & Mayer, T. (1965). Tests of the Relative Importance of Autonomous Expenditures and Money. *American Economic Review, 55*(4), 720–752.

Desai, M. (1980). Review of Milton and Rose Friedman, *Free to Choose: A Personal Statement*. *International Affairs, 56*(3), 505–506.

Despres, E., Hart, A. G., Friedman, M., Samuelson, P. A., & Wallace, D. H. (1950). The Problem of Economic Instability: A Committee Report. *American Economic Review*, pp. 505–538.

Dewey, E. R., & Dakin, E. F. (1947). *Cycles: The Science of Prediciton*. New York: H. Holt and Company.

Dicey, A. V. (1905). *Lectures on the Relation Between Law and Public Opinion in England During the Nineteenth Century* (1st ed.). London: Macmillan.

Dicey, A. V. (1914). *Lectures on the Relation Between Law and Public Opinion in England During the Nineteenth Century* (2nd ed.). London: Macmillan.

Diesing, P. (1985). Hypothesis Testing and Data Interpretation: The Case of Milton Friedman. *Research in the History of Economic Thought and Methodology, 3,* 61–89.

Director, A. (1950). Contribution of Aaron Director. In *A Positive Program for Conservatives: A Symposium* (pp. 8–11). Chicago, W. Allen Wallis Papers, Box 29, University of Rochester, River Campus Libraries.

Director, A., Friedman, M., & Wallis, W. A. (1950). *A Positive Program for Conservatives: A Symposium.* Chicago, W. Allen Wallis Papers, Box 29, University of Rochester, River Campus Libraries.

Dornbusch, R., & Fischer, S. (1978). *Macroeconomics* (1st International Student edn.). Tokyo: McGraw Hill.

Dow, L. A. (1961). Institutionalism and Contemporary Price Theory. *American Journal of Economics and Sociology, 20*(2), 181–193.

Downs, A. (1957). *An Economic Theory of Democracy*. New York: HarperCollins.

Duck, N. W. (1988). Money, Output and Prices: An Empirical Study Using Long-Term Cross Country Data. *European Economic Review, 32*(8), 1603–1619.

Duesenberry, J. S. (1948). Income–Consumption Relations and Their Implications. In L. A. Metzler (Ed.), *Income, Employment and Public Policy: Essays in Honour of Alvin H Hansen* (pp. 54–81). New York: W. W. Norton.

Duesenberry, J. S. (1949). *Incomes, Saving and the Theory of Consumer Behaviour*. Harvard, MA: Harvard University Press.

Ebenstein, L. (2007). *Milton Friedman*. Basingstoke: Palgrave.

Edge, S. K. (1967). The Relative Stability of Monetary Velocity and the Investment Multiplier. In *Australian Economic Papers, 6*(9), 192–207.

Edgeworth, F. Y. (1894). Demand Curves. In *Palgrave's Dictionary of Political Economy.* London: Macmillan.

Edie, L. (1931). *The Banks and Prosperity*. New York: Harper.

Eisenhart, C., Hastay, M. W., & Wallis, W. A. (1947). *Selected Techniques of Statistical Analysis*. New York: McGraw-Hill.

Eisner, R. (1958). The Permanent Income Hypothesis: Comment. *American Economic Review, 48*(5), 972–990.

Ellsberg, D. (1961). Risk, Ambiguity, and the Savage Axioms. *Quarterly Journal of Economics, 75*(4), 643–669.

Emmett, R. B. (2011). Sharpening the Tools in the Workshop: The Workshop System and the Chicago School's Success. In R. van Horn, P. Mirowski, & T. A. Stapleford (Eds.), *Building Chicago Economics* (pp. 93–115). Cambridge: Cambridge University Press.

Emminger, O. (1977). *The DM in the Conflict Between Internal and External Equilibrium 1948–75 Princeton Essays in International Finance No. 122 Princeton University*. Princeton: Princeton University Press.

Epstein, R. (1987). *A History of Econometrics*. Amsterdam: North Holland.

Erickson, S. (Ed.). (2001). *A Conversation with Harris and Seldon*. London: IEA.

Ericsson, N. R., Hendry, D. F., & Hood, S. B. (2016). Milton Friedman as Empirical Modeler. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Evans, R. A. (1984). The Aggregate Consumption Function. In F. van der Ploeg (Ed.), *Mathematical Methods in Economics* (pp. 95–120). Chichester: Wiley.

Fabricant, S. (1942). *Employment in Manufacturing, 1899–1939*. New York: NBER.

Farrell, M. J. (1959). The New Theories of the Consumption Function. *Economic Journal, 69*(276), 678–696.

Feldberg, M., Jowell, K., & Mulholland, S. (1976). *Milton Friedman in South Africa*. Cape Town and Johannesburg: University of Cape Town and The Sunday Times (of South Africa).

Fellner, W. J. (1942). *A Treatise on War Inflation—Present Policies and Future Tendencies in the United States*. Berkeley and Los Angeles: University of California Press.

Fellner, W. J., Gordon, R. J., Friedman, M., & Walker, C. (1974). Indexing and Inflation. *Journal of Monetary Economics,12*(4), 519–541.

Fels, R. (1977). Review of Jerome L. Stein, *Monetarism*. *Journal of Political Economy, 15*(2), 549–551.

Ferber, R. (1953). *A Study of Aggregate Consumption Functions*. New York: NBER.

Finn, D. R. (1979). Objectivity in Economics: On the Choice of a Scientific method. *Review of Social Economy, 37*(1), 37–61.

Fisher, I. (1933). The Debt-Deflation Theory of Great Depressions. *Econometrica, 1*(4), 337–357.

Fisher, M. R. (1956). Explorations in Savings Behaviour. *Bulletin of the Oxford University Institute of Statistics, XVIII*, 201–277.

Forder, J. (2010a). Friedman's Nobel Lecture and the Phillips Curve Myth. *Journal of the History of Economic Thought, 32*(3), 329–348.

Forder, J. (2010b). The Historical Place of the 'Friedman-Phelps' Expectations Critique. *European Journal of the History of Economic Thought, 17*(3), 493–511.

Forder, J. (2013). Macroeconomics and the L-Shaped Aggregate Supply Curve. In G. C. Harcourt & P. Kriesler (Eds.), *Oxford Handbook of Post-Keynesian Economics* (pp. 245–264). Oxford: Oxford University Press.

Forder, J. (2014). *Macroeconomics and the Phillips Curve Myth*. Oxford: Oxford University Press.

Forder, J. (2015). Textbooks on the Phillips Curve. *History of Political Economy, 47*(2), 207–240.

Forder, J. (2016). *Milton Friedman's Lack of Influence on British Economic Policy*. Mimeo, Balliol College Oxford.

Forder, J. (2017). Harry Johnson on the Phillips Curve. *Journal of the History of Economic Thought, 39*(4), 503–522.

Forder, J. (2018a). A Response to David Laidler's Review of *Macroeconomics and the Phillips Curve Myth*. https://tinyurl.com/JF18respDEWL. Accessed 10 February 2019.

Forder, J. (2018b). Two Lectures by Friedman: One Famous, One Good. *History of Economic Ideas, 26*(3, supplement), 51–67.

Forder, J. (2018c). *Three Accounts of the Monetarist Revolution: Johnson, Friedman, and Brunner.* Mimeo, Balliol College Oxford.

Forder, J. (2018d). What Was the Message of Friedman's Presidential Address to the American Economic Association? *Cambridge Journal of Economics, 42*(2), 523–541.

Forder, J. (2018e). Why is Labour Market Adjustment so Slow in Friedman's Presidential Address? *Review of Keynesian Economics, 6*(4), 461–472.

Forder, J. (2019). *Whatever Happened to Cost-Push Inflation?* Mimeo, Balliol College Oxford.

Forder, J., & Monnery, H. (2019). Why Did Milton Friedman Win the Nobel Prize? *Econ Journal Watch, 16*(1).

Forder, J., & Sømme, K. (2019). Explaining the Fame of Friedman's Presidential Address. *Cambridge Journal of Economics*.

Foster, W. T., & Catchings, W. (1929). Is the Reserve Board Keeping Faith? *Atlantic Monthly, 144,* 93–102.

Frazer, W. J. (1983). Monetary Trends in the U.S. and the U.K. *Southern Economic Journal, 49*(3), 833–846.

Frazer, W. J. (1988a). *Power and Ideas: Milton and the Big U-Turn* (Vol. 1). Gainsville, FL: Gulf/Atlantis.

Frazer, W. J. (1988b). *Power and Ideas: Milton and the Big U-Turn* (Vol. 2). Gainsville, FL: Gulf/Atlantis.

Frazer, W. J., Jr., & Boland, L. A. (1983). An Essay on the Foundations of Friedman's Methodology. *American Economic Review, 73*(1), 129–144.

Freed, D., & Landis, F. (1980). *Death in Washington*. Westport, CN: Lawrence Hill.

Freedman, C. F., Harcourt, G. C., Kriesler, P., & Nevile, J. W. (2016). How Friedman Became the Anti-Keynes. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Freedman, C. F. (2006). Not for Love nor Money: Milton Friedman's Counterrevolution. *History of Economics Review, 42,* 87–119.

Freeman, A. M., & Haveman, R. H. (1972). Clean Rhetoric and Dirty Water. *The Public Interest, 28,* 51–65.

Freeman, H. A., Friedman, M., Mosteller, F., & Wallis, W. A. (1949). *Sampling Inspection.* New York: McGraw-Hill.

Friedman, I. S. (1973). *Inflation. A World-Wide Disaster*. Boston: Houghton Mifflin.

Friedman, M. (1935a). Pigou's Method for Measuring Elasticities of Demand from Budgetary Data. *Quarterly Journal of Economics, 50*(1), 151–163.

Friedman, M. (1935b). Review of Kuznets, *Seasonal Variations in Industry and Trade*. *Journal of Political Economy, 43*(6), 830–832.

Friedman, M. (1936a). Review of Blodgett, *Cyclical Fluctuations in Commodity Stocks*. *Journal of Political Economy, 44*(6), 842–843.

Friedman, M. (1936b). Marginal Utility of Money and Elasticities of Demand. *Quarterly Journal of Economics, 50*(3), 532–533.

Friedman, M. (1936c). Further Notes on Elasticity of Substitutions: 1. Note on Dr. Machlup's Article. *Review of Economic Studies, 3*(2), 147–149.

Friedman, M. (1937). The Use of Ranks to Avoid the Assumption of Normality Implicit in the Analysis of Variance. *Journal of the American Statistical Association, 32*(200), 675–701.

Friedman, M. (1938). Mr. Broster on Demand Curves. *Journal of the Royal Statistical Society, 101*(2), 450–454.

Friedman, M. (1939a). A Correction: The Use of Ranks to Avoid the Assumption of Normality Implicit in the Analysis of Variance. *Journal of the American Statistical Association, 34*(205), 109.

Friedman, M. (1939b). Review of Leven and Robertson Wright, *The Income Structure of the United States*. *Journal of the American Statistical Association, 34*(205), 224–225.

Friedman, M. (1940a). Review of Tinbergen, *Business Cycles in the United States, 1919–1932, Vol II*. *American Economic Review, 30*(3), 657–660.

Friedman, M. (1940b). A Comparison of Alternative Tests of Significance for the Problem of *m* Ranking. *The Annals of Mathematical Statistics, 11*(1), 86–92.

Friedman, M. (1941a). Review of Triffin, *Monopolistic Competition and General Equilibrium Theory*. *Journal of Farm Economics, 23*(1), 389–391.

Friedman, M. (1941b). Review of Kreps, *Measurement of the Social Performance of Business*, Washington, DC, Government Printing Office. *American Economic Review, 31*(4), 850–851.

Friedman, M. (1942a). Discussion of the Inflationary Gap. *American Economic Review, 32*(2), 314–320.

Friedman, M. (1942b, May 7). Statement by Milton Friedman Before the Ways and Means Commitee of the House of Representatives. In U.S. Congress, *Committee on Ways and Means, Data on Proposed Revenue Bill of 1942 (revised)* (pp. 171–175). House of Representatives, 77th Congress, 2nd Session, 24 April–27 June 1942.

Friedman, M. (1942/1953). Discussion of the Inflationary Gap. In M. Friedman (Ed.), *Essays in Positive Economics*. Chicago: Chicago University Press.

Friedman, M. (1943a). The Spendings Tax as a Wartime Fiscal Measure. *American Economic Review, 33*(1), 50–62.

Friedman, M. (1943b). Methods of Predicting the Onset of Inflation. In C. Shoup (Ed.), *Taxing to Prevent Inflation* (pp. 111–153). New York: Columbia University Press.

Friedman, M. (1944). Review of Altman, *Saving, Investment and National Income*. *Review of Economics and Statistics, 26*(2), 101–102.

Friedman, M. (1946). Lange on Price Flexibility and Employment: A Methodological Criticism. *American Economic Review, 36*(4), 613–631.

Friedman, M. (1947a). Utilization of Limited Experimental Facilities When the Cost of Each Measurement Depends on Its Magnitude. In C. Eisenhart,

M. W. Hastay, & W. A. Wallis (Eds.), *Selected Techniques of Statistical Analysis* (pp. 319–328). New York: McGraw-Hill.

Friedman, M. (1947b). Planning an Experiment for Estimating the Mean and Standard Deviation of a Normal Distribution from Observations on the Cumulative Distribution. In C. Eisenhart, M. W. Hastay, & W. A. Wallis (Eds.), *Techniques of Statistical Analysis* (pp. 339–352). New York: McGraw-Hill.

Friedman, M. (1947c). Lerner on the Economics of Control. *Journal of Political Economy, LV*(5), 405–416.

Friedman, M. (1948a). A Monetary and Fiscal Framework for Economic Stability. *American Economic Review, 38*(3), 245–264.

Friedman, M. (1948b). Review of Dewey, E. & E. Dakin, *Cycles: The Science of Prediction*. New York: H Holt and Co. *Journal of the American Statistical Association, 43*(241), 139–145.

Friedman, M. (1949a). Professor Friedman's Proposal: Rejoinder. *The American Economic Review, 39*(5), 949–955.

Friedman, M. (1949b). The Marshallian Demand Curve. *Journal of Political Economy LVII*(December), 463–495.

Friedman, M. (1950). Wesley C. Mitchell as an Economic Theorist. *Journal of Political Economy, 58*(6), 465–493.

Friedman, M. (1951a). Some Comments on the Significance of Labor Unions for Economic Policy. In D. M. Wright (Ed.), *The Impact of the Union* (pp. 204–234). New York: Harcourt, Brace and Company.

Friedman, M. (1951b). *Should Taxes Be Bigger and How Can They Be Better*. University of Chicago Round Table, 30 September. Hoover Institution online collection of the works of Milton Friedman. Accessed 10 February 2019.

Friedman, M. (1951c). *Can the Control of Money Stop Today's Inflation?* University of Chicago Round Table, 11 February. Hoover Institution online collection of the works of Milton Friedman. Accessed 10 February 2019.

Friedman, M. (1951d). Commodity-Reserve Currency. *Journal of Political Economy LIX*(3), 203–232.

Friedman, M. (1951e). Some Comments on the Significance of Labor Unions for Economic Policy—Contributions to General Discussion. In D. M. Wright (Ed.), *The Impact of the Union* (pp. 235–239). New York: Harcourt, Brace and Company.

Friedman, M. (1951f). Comment on Carl Christ, 'A Test of an Econometric Model for the United States, 1921–1947.' In G. Haberler (Ed.), *Conference on Business Cycles* (pp. 107–114). Washington, DC: NBER.

Friedman, M. (1951g). The Controversy Over Monetary Policy: Comments on Monetary Policy. *The Review of Economics and Statistics, 33*(3), 186–191.
Friedman, M. (1951/1953, July–December). Les effets d'une politique de plein emploi sur la stabilité sur la stabilité économique: Analyse formelle. *Economie Appliquee, IV*, 441–456. Reprinted as 'The effects of a full employment policy on economic stability: A formal analysis' in M. Friedman (Ed.), *Essays in Positive Economics* (pp. 117–132). Chicago: University of Chicago.
Friedman, M. (1951/2013, February 17). Neoliberalism and Its Prospects. *Farmand,* pp. 89–93.
Friedman, M. (1952a, February). The 'Welfare' Effects of an Income Tax and an Excise Tax. *Journal of Political Economy, LX*(1), 25–33.
Friedman, M. (1952b). Price, Income and Monetary Changes in Three Wartime Periods. *American Economic Review, 42*(2), 612–625.
Friedman, M. (1952c). Replies to Questions and Other Material for the Use of the Subcommittee on General Credit Control and Debt Management. In U. S. Congress, *Joint Committee on the Economic Report, Monetary Policy and the Management of the Public Debt, Their Role in Achieving Price Stability and High-Level Employment, Part 2*. Washington, DC: Government Printing Office.
Friedman, M. (1952d). Comment. In B. F. Haley (Ed.), *A Survey of Contemporary Economics, Volume II* (pp. 455–457). Chicago: Richard D. Irwin.
Friedman, M. (1953a). The Effects of Full Employment Policy on Economic Stability. In M. Friedman (Ed.), *Essays in Positive Economics* (pp. 117–132). Chicago: University of Chicago.
Friedman, M. (1953b). The Methodology of Positive Economics. In M. Friedman (Ed.), *Essays in Positive Economics* (pp. 3–43). Chicago: Chicago University Press.
Friedman, M. (1953c). *Essays in Positive Economics*. Chicago: Chicago University Press.
Friedman, M. (1953d). The Case for Flexible Exchange Rates. In M. Friedman (Ed.), *Essays in Positive Economics* (pp. 157–203). Chicago: University of Chicago.
Friedman, M. (1953e). Economic Advice and Political Limitations: Rejoinder. *Review of Economics and Statistics, 35*(3), 252.
Friedman, M. (1953f). Choice, Chance, and the Personal Distribution of Income. *Journal of Political Economy, 61*(4), 277–290.

Friedman, M. (1953g). Discussion. *American Economic Review, 43*(2), 445–448.

Friedman, M. (1954). The Marshallian Demand Curve: A Reply. *Journal of Political Economy, 62*(3), 261–266.

Friedman, M. (1954/1968). Why the American Economy Is Depression Proof. *Nationalekonomiska föreningens förhandlingar, 3*, 58–77. Reprinted in M. Friedman (Ed.), *Dollars and Deficits* (pp. 72–96). Englewood Cliffs, NJ: Prentice Hall.

Friedman, M. (1955a). What All Is Utility? *Economic Journal, 65*(259), 405–409.

Friedman, M. (1955b). Marshall and Friedman on Union Strength: Comment. *Review of Economics and Statistics, 37*(4), 401.

Friedman, M. (1955c). Walras and His Economic System. *American Economic Review, 45*(5), 900–909.

Friedman, M. (1955d). Liberalism, Old Style. In *Collier's Year Book*. New York: P F Collier & Son.

Friedman, M. (1955e). The Role of Government in Education. In R. A. Solo (Ed.), *Economics and the Public Interest* (pp. 123–144). New Brunswick, NJ: Rutgers University Press.

Friedman, M. (1956a). The Quantity Theory of Money—A Restatement. In M. Friedman (Ed.), *Studies in the Quantity Theory of Money* (pp. 3–21). Chicago: University of Chicago.

Friedman, M. (Ed.). (1956b). *Studies in the Quantity Theory of Money*. Chicago: Chicago University Press.

Friedman, M. (1956c, June). *The Keynesian Revolution and Economic Liberalism*. Lecture at Wabash College, Hoover Institution online collection of the works of Milton Friedman. Accessed 10 February 2019.

Friedman, M. (1956d). *The Basic Principles of Liberalism*. Lecture at Wabash College, Hoover Institution online collection of the works of Milton Friedman. Accessed 10 February 2019.

Friedman, M. (1956e). *The Distribution of Income and the Welfare Activities of Government*. Lecture at Wabash College, Hoover Institution online collection of the works of Milton Friedman. Accessed 10 February 2019.

Friedman, M. (1957a). *A Theory of the Consumption Function*. Princeton: Princeton University Press.

Friedman, M. (1957b). Savings and the Balance Sheet. *Oxford Bulletin of Economics and Statistics, 19*(1), 125–136.

Friedman, M. (1958a). The Supply of Money and Changes in Prices and Output. In *The Relationship of Prices to Economic Stability and Growth.*

Compendium of Papers Submitted by Panelists Appearing Before the JEC 85th Congress, 2d Sess, March 31 1958 (pp. 241–256). Washington, DC: US Government Printing Office.
Friedman, M. (1958b). Reply. In L. H. Clark (Ed.), *Consumer Behavior: Research on Consumer Reactions*. New York: Harper Brothers.
Friedman, M. (1958c). Permanent Income Hypothesis: Comment. *American Economic Review, 48*(5), 990–991.
Friedman, M. (1958d). Foreign Economic Aid: Means and Objectives. *Yale Review, 47*(4), 500–516.
Friedman, M. (1958e). The Friedman-Becker Illusion: Supplementary Comment. *Journal of Political Economy, 66*(6), 547–549.
Friedman, M. (1958f). *Inflation Lecture to the Mont Pèlerin Society*. Princeton, NJ, 3 to 8 September 1958. Hoover Institution Milton Friedman Archive, Box 44 Folder 12.
Friedman, M. (1958g). Capitalism and Freedom. In F. Morley (Ed.), *Essays on Individuality* (pp. 168–183). Philadelphia: University of Pennsylvania Press.
Friedman, M. (1959a). Comment on Morton. In *Proceedings of the Eleventh Annual Meeting of the Industrial Relations Research Association* (pp. 184–195).
Friedman, M. (1959b). The Demand for Money: Some Theoretical and Empirical Results. *Journal of Political Economy, 67*(4), 327–351.
Friedman, M. (1959c). The Demand for Money: Some Theoretical and Empirical Results. *American Economic Review, 49*(2), 525–527.
Friedman, M. (1959d). The Demand for Money: Some Theoretical and Empirical Results. *National Bureau of Economic Research Working Paper Series* 68.
Friedman, M. (1959e). Statement and Testimony. In U. S. Congress, *Joint Economic Committee, Employment, Growth and Price Levels* (pp. 605–648). Washington, DC: Government Printing Office.
Friedman, M. (1960a). *A Program for Monetary Stability*. New York: Fordham University Press.
Friedman, M. (1960b). In Defense of Destabilizing Speculation. In R. W. Pfouts (Ed.), *Essays in Economics and Econometrics*. Chapel Hill, NC: University of North Carolina Press.
Friedman, M. (1960c). Comments on Bodkin 'Windfall Income and Consumption'. In I. Friend & R. Jones (Eds.), *Proceedings of the Conference on Consumption and Savings* (pp. 191–206). Philadelphia: University of Pennsylvania.

Friedman, M. (1961a). Economic Aid Reconsidered: A Reply. *Yale Review, 50*(4), 519–540.
Friedman, M. (1961b). The Lag in Effect of Monetary Policy. *Journal of Political Economy, 69*(5), 447–466.
Friedman, M. (1961c). Vault Cash and Free Reserves. *Journal of Political Economy, 69*(2), 181–182.
Friedman, M. (1961d). Review of Thomas Wilson, *Inflation*. *American Economic Review, 51*(5), 1051–1055.
Friedman, M. (1961e). Real and Pseudo Gold Standards. *Journal of Law & Economics, 4*, 66–79.
Friedman, M. (1961f). The Demand for Money. *Proceedings of the American Philosophical Society, 105*(3), 259–264.
Friedman, M. (1962a). *Capitalism and Freedom*. Chicago: University of Chicago Press.
Friedman, M. (1962b). *Price Theory: A Provisional Text*. Chicago: Aldine.
Friedman, M. (1962c). Should There Be an Independent Monetary Authority? In L. B. Yeager (Ed.), *In Search of a Monetary Constitution* (pp. 219–243). Cambridge, MA: Harvard University Press.
Friedman, M. (1962/2017). Is a Free Society Stable? *New Individualist Review, 2*(Summer), 3–10.
Friedman, M. (1963a, September). The Present State of Monetary Theory. *Economic Studies Quarterly, 14,* 1–15.
Friedman, M. (1963b). More on Archibald Versus Chicago. *Review of Economic Studies, 30*(1), 65–67.
Friedman, M. (1963c). Price Determination in the United States Treasury Bill Market: A Comment. *Review of Economic Statistics, 45*(3), 318–320.
Friedman, M. (1963d). Windfalls, the 'Horizon' and Related Concepts in the Permanent Income Hypothesis. In C. F. Christ (Ed.), *Measurement in Economics* (pp. 3–28). Stanford: Stanford University Press.
Friedman, M. (1963e). Notes on Nissan Liviatan's Paper. In C. F. Christ (Ed.), *Measurement in Economics* (pp. 59–66). Stanford: Stanford University Press.
Friedman, M. (1963/1968). *Inflation: Causes and Consequences.* Bombay: Asia Publishing House. Reprinted without introduction and conclusion from the hosts in Milton Friedman 1968 (Ed.), *Dollars and Deficits* (pp. 17–60). Englewood Cliffs, NJ: Prentice Hall.
Friedman, M. (1964a). Note on Lag in Effect of Monetary Policy. *The American Economic Review, 54*(5), 759–761.
Friedman, M. (1964b). Comment on 'Collusion in the Auction Market for Treasury Bills'. *Journal of Political Economy, 72*(5), 513–514.

Friedman, M. (1964c). Using the Free Market to Resolve the Balance of Payments Problem. *Financial Analysts Journal, 20*(2), 21–25.

Friedman, M. (1964d). Reports on Selected Bureau Programs. In *NBER Annual Report*. Washignton, DC: NBER.

Friedman, M. (1964/1969). Post-War Trends in Monetary Theory and Policy. *National Banking Review, 2*(1), 1–9. Reprinted in M. Friedman (Ed.), *The Optimum Quantity of Money and Other Essays*. London: Macmillan.

Friedman, M. (1965a). Foreward. In *Determinants and Effect of Changes in the Money Stock, 1875–1960* (pp. xxiii–xxvii). New York: NBER.

Friedman, M. (1965b). A Program for Monetary Stability. In M. D. Ketchum & L. T. Kendall (Eds.), *Readings in Financial Institutions* (pp. 189–209). Boston: Houghton Mifflin.

Friedman, M. (1965c, August 24). Social Responsibility: A Subversive Doctrine. *National Review,* pp. 721–723.

Friedman, M. (1966a). Interest Rates and the Demand for Money. *Journal of Law & Economics, 9*, 71–85.

Friedman, M. (1966b). What Price Guideposts? In G. P. Schultz & R. Z. Aliber (Eds.), *Guidelines, Informal Controls, and the Market Place* (pp. 17–40). Chicago: Chicago University Press.

Friedman, M. (1966c). Comments. In G. P. Schultz and R. Z. Aliber (Eds.), *Guidelines, Informal Controls, and the Market Place* (pp. 55–61). Chicago: Chicago University Press.

Friedman, M. (1966d, October 17). An Inflationary Recession. *Newsweek*, p. 92.

Friedman, M. (1966e, October 17). Inflationary Recession. *Newsweek*, p. 92.

Friedman, M. (1966f, December 19). A Volunteer Army. *Newsweek*, p. 100.

Friedman, M. (1966/1969). Free-Market Determination of Exchange Rates. In L. H. Officer & T. D. Willett (Eds.), *The International Monetary System, Problems and Proposals*. Englewood Cliffs, NJ: Prentice-Hall.

Friedman, M. (1967a, May 14). The Case for Abolishing the Draft—And Substituting for It an All-Volunteer Army. *New York Times Magazine*, pp. 114–119.

Friedman, M. (1967b). The Monetary Theory and Policy of Henry Simons. *Journal of Law and Economics, 10,* 1–13.

Friedman, M. (1967c). Must We Choose Between Inflation and Unemployment. *Stanford Graduate School of Business Bulletin, 35*, 10–13, 40, 42.

Friedman, M. (1967d, June 26). Oil and the Middle East. *Newsweek*, p. 63.

Friedman, M. (1967e, April 24). Papal Economics. *Newsweek*, p. 68.
Friedman, M. (1967f, June 5). Auto Safety Standards. *Newsweek*, p. 80.
Friedman, M. (1967g, July 17). The Kennedy Round. *Newsweek*, p. 80.
Friedman, M. (1967h, January 9). Current Monetary Policy. *Newsweek*, p. 59.
Friedman, M. (1967i, October 30). Current Monetary Policy. *Newsweek*, p. 80.
Friedman, M. (1967j). Why Not a Volunteer Army? In S. Tax (Ed.), *The Draft* (pp. 200–207). Chicago: University of Chicago.
Friedman, M. (1967k). Value Judgments in Economics. In S. Hook (Ed.), *Human Values and Economic Policy* (pp. 85–93). New York: New York University Press.
Friedman, M. (1967/1968). L'economie politique des accords monétaires internationaux. In E. M. Claassen (Ed.), *Les Fondements philosophiques des systèmes économiques*. Paris: Payot. Reprinted as 'The political economy of international monetary arrangements' in M Friedman (Ed.), *Dollars and Deficits*. Englewood Cliffs, NJ: Prentice Hall.
Friedman, M. (1968a). The Role of Monetary Policy. *American Economic Review, LVIII*(1), 1–17.
Friedman, M. (1968b). Why Economists Disagree. In M. Friedman (Ed.), *Dollars and Deficits* (pp. 1–21). Englewood Cliffs, NJ: Prentice Hall.
Friedman, M. (1968c). Money: Quantity Theory. In D. Sills (Ed.), *International Encyclopaedia of the Social Sciences.* New York: Macmillan.
Friedman, M. (1968d). *Dollars and Deficits.* Englewood Cliffs, NJ: Prentice Hall.
Friedman, M. (1968e). *The Case for the Negative Income Tax* (M. Laird, Ed., pp. 202–220). New York: Anchor Books, Doubleday.
Friedman, M. (1968f, June 3). Monetary Policy. *Newsweek*, p. 85.
Friedman, M. (1969a). The Optimum Quantity of Money. In M. Friedman (Ed.), *The Optimum Quantity of Money and Other Essays* (pp. 1–50). London: Macmillan.
Friedman, M. (1969b). Round Table on Exchange Rate Policy. *American Economic Review, 59*(2), 364–366.
Friedman, M. (1969c). *The Optimum Quantity of Money and Other Essays*. London: Macmillan.
Friedman, M. (1969d, March 3). No Taxation Without Representation. *Newsweek*, p. 76.
Friedman, M. (1969e). Has Fiscal Policy Been Oversold? In M. Friedman & W. Heller (Eds.), *Monetary vs Fiscal Policy* (pp. 43–62). New York: W. W. Norton.

Friedman, M. (1969f, October 20). Unemployment Figures. *Newsweek*, p. 101.
Friedman, M. (1969g, August 18). Monetary Overkill. *Newsweek*, p. 75.
Friedman, M. (1969h, January 20). The Inflationary Fed. *Newsweek*, p. 78.
Friedman, M. (1970a, June 15). Burns and Guidelines. *Newsweek.*
Friedman, M. (1970b, February 2). A New Chairman at the Fed. *Newsweek*, p. 68.
Friedman, M. (1970c). Comment on Tobin. *Quarterly Journal of Economics, 84*(2), 318–327.
Friedman, M. (1970d). The New Monetarism: Comment. *Lloyd's Bank Review, 98*(October), 52–53.
Friedman, M. (1970e). *The Counter-Revolution in Monetary Theory*. London: IEA.
Friedman, M. (1970f). A Theoretical Framework for Monetary Analysis. *Journal of Political Economy, 78*(2), 193–238.
Friedman, M. (1970g, September 17). The Counter-Revolution of Monetary Theory. *Financial Times*, p. 12.
Friedman, M. (1970h). Discussion of Kindleberger, The Case for Fixed Exchange Rates, 1969. In F. E. Morris (Ed.), *The International Adjustment Mechanism.* Boston, MA: Federal Reserve Bank of Boston.
Friedman, M. (1970i, September 13). The Social Responsibility of Business is to Increase Its Profits. *New York Times Magazine*.
Friedman, M. (1970j, July 6). Monetary Overheating. *Newsweek*, p. 75.
Friedman, M. (1970k, September 28). Inflation and Wages. *Newsweek*, p. 77.
Friedman, M. (1971a). Contribution to General Discussion. In G. Clayton, J. C. Gilbert, & R. Sedgwick (Eds.), *Monetary Theory and Monetary Policy in the 1970s* (pp. 68–71). London: Oxford University Press.
Friedman, M. (1971b). *A Theoretical Framework for Monetary Analysis*. New York: NBER.
Friedman, M. (1971c, March–April). A Monetary Theory of Nominal Income. *Journal of Political Economy, 79*(2), 323–337.
Friedman, M. (1971d). A Monetary Theory of Nominal Income. In G. Clayton, J. C. Gilbert, & R. Sedgwick (Eds.), *Monetary Theory and Monetary Policy in the 1970s*. London: Oxford University Press.
Friedman, M. (1971e). Summary of the General Discussion. In G. Clayton, J. C. Gilbert, & R. Sedgwick (Eds.), *Monetary Theory and Monetary Policy in the 1970s*. Oxford: Oxford University Press.
Friedman, M. (1971f). Government Revenue from Inflation. *The Journal of Political Economy, 79*(4), 846–856.

Friedman, M. (1971g, August 30). Why the Freeze Is a Mistake. *Newsweek*, p. 22.
Friedman, M. (1971h, October 28). Morality and Controls I. *New York Times*.
Friedman, M. (1971i, October 29). Morality and Controls II. *New York Times*.
Friedman, M. (1971j, October 18). Will the Kettle Explode. *Newsweek*, p. 30.
Friedman, M. (1971k, May 3). Money Explodes. *Newsweek*, p. 81.
Friedman, M. (1972a). Monetary Policy. *Proceedings of the American Philosophical Society, 116*(3), 183–196.
Friedman, M. (1972b, February 7). The Case for a Monetary Rule. *Newsweek*, p. 67.
Friedman, M. (1972c). *A Economist's Protest.* Glen Ridge, NJ: Thomas Horton and Company.
Friedman, M. (1972d). Preface. In M. Friedman (Ed.), *An Economist's Protest*. Glen Ridge, NJ: Thomas Horton and Company.
Friedman, M. (1972e, May 22). Controls: An Exercise in Futility. *Newsweek*, p. 86.
Friedman, M. (1972f, October 16). The Fed on the Spot. *Newsweek,* p. 98.
Friedman, M. (1972g, May 1). Prohibition and Drugs. *Newsweek*, p. 104.
Friedman, M. (1973a). *Foreword to The Economics of The Israeli Diamond Industry by Michael Szenberg*. New York: Basic Books.
Friedman, M. (1973b). Facing Inflation. *Challenge, 16*(5), 29–37.
Friedman, M. (1973c). Contemporary Economic Problems. *Economic Notes of Banca Monte dei Paschi di Siena, 2,* 5–18.
Friedman, M. (1973d, August 27). The Inflationary Fed. *Newsweek*, p. 74.
Friedman, M. (1973e, October 8). Living with Inflation. *Newsweek*, p. 90.
Friedman, M. (1973f). *Money and Economic Development*. New York: Praeger.
Friedman, M. (1973g, January 29). Perspective on Controls. *Newsweek*, p. 58.
Friedman, M. (1973h, October 29). More on Living with Inflation. *Newsweek*, p. 96.
Friedman, M. (1973i, December 10). The Inequity of Gas Rationing. *Newsweek*, p. 113.
Friedman, M. (1973j, April 23). Speculation and Speculation. *Newsweek*, p. 96.
Friedman, M. (1973k, November 19). Why Some Prices Should Rise. *Newsweek*, p. 130.
Friedman, M. (1973l, February 19). Barking Cats. *Newsweek*, p. 70.
Friedman, M. (1973m, September 17). The Hard Truth. *Newsweek*, p. 74.
Friedman, M. (1973n, May 14). 'Steady as You Go' revisited. *Newsweek*, p. 100.
Friedman, M. (1973o, December 31). Why Now? *Newsweek*, p. 29.

Friedman, M. (1973p, June 25). Monumental Folly. *Newsweek*, pp. 64–65.
Friedman, M. (1974a). A Theoretical Framework for Monetary Analysis. In R. J. Gordon (Ed.), *Milton Friedman's Monetary Framework* (pp. 1–62). Chicago: Chicago University Press.
Friedman, M. (1974b, September 1). Letter to the Editor. *New York Times*.
Friedman, M. (1974c). Reply to Ulmer. *Challenge, 17*(1), 63–64.
Friedman, M. (1974d). *Monetary Correction*. London: IEA.
Friedman, M. (1974e). A Bias in Current Measures of Economic Growth. *Journal of Political Economy, 82*(2), 431–432.
Friedman, M. (1974f). Comments on Critics. In R. J. Gordon (Ed.), *Milton Friedman's Monetary Framework* (pp. 132–177). Chicago: Chicago University Press.
Friedman, M. (1974g). Monetary Policy in Developing Countries. In P. A. David & M. W. Reder (Eds.), *Nations and Households in Economic Growth: Essays in Honor of Moses Abramovitz*. London: Academic Press.
Friedman, M. (1974h, May, 13). Inflation-Proofing the Income Tax. *Newsweek*, p. 120.
Friedman, M. (1974i, July). Using Escalators to Help Fight Inflation. *Fortune*, pp. 94–97, 174, 176.
Friedman, M. (1974j). Inflation, Taxation, Indexation. In L. Robbins (Ed.), *Inflation: Causes, Consequences, Cures* (pp. 71–81). London: IEA.
Friedman, M. (1974k). Inflation, Taxation, Indexation: Response to Discussion. In L. Robbins (Ed.), *Inflation: Causes, Consequences, Cures* (pp. 81–82, 84–87). London: IEA.
Friedman, M. (1974l, June 24). Perspective on Inflation. *Newsweek*, p. 73.
Friedman, M. (1974m, March 4). FEO and the Gas Lines. *Newsweek*, p. 71.
Friedman, M. (1974n, September 23). Is Money Too Tight? *Newsweek*, p. 82.
Friedman, M. (1975a). *Unemployment Versus Inflation?* London: IEA.
Friedman, M. (1975b, June 2). Congress and the Federal Reserve. *Newsweek*, p. 62.
Friedman, M. (1975c). Gold, Money and the Law: Comments. In H. G. Manne & R. L. Miller (Eds.), *Gold, Money and the Law* (pp. 71–82). Chicago: Aldine Publishing Company.
Friedman, M. (1975d). *Milton Friedman in Australia*. Sydney: Constable and Bain.
Friedman, M. (1975e). Discussion. *American Economic Review, 65*(2), 176–179.
Friedman, M. (Ed.). (1975f). *There Is No Such Thing as a Free Lunch*. La Salle, IL: Open Court.

Friedman, M. (1975g, December 8). How to Hit the Money Target. *Newsweek*, p. 85.

Friedman, M. (1975h, March 10). What Is the Federal Reserve Doing? *Newsweek*, p. 63.

Friedman, M. (1976a). Comments on Tobin and Buiter. In J. L. Stein (Ed.), *Monetarism* (pp. 310–317). Amsterdam: North Holland.

Friedman, M. (1976b, November). The Line We Dare Not Cross. *Encounter*, pp. 8–14.

Friedman, M. (1976c). Autobiographical Essay. In A. Lindbeck (Ed.), *Nobel Lectures, Economics 1969–1980*. Singapore: World Scientific Publishing Co.

Friedman, M. (1976d). Homer Jones—A Personal Reminiscence. *Journal of Monetary Economics, 2*(4), 433–436.

Friedman, M. (1976e). Preface. In F. Machlup (Ed.), *Essays on Hayek* (pp. xxi–xiv). New York: New York University Press.

Friedman, M. (1976f). Blacks, Whites and Egalitarianism. In M. Feldberg, K. Jowell, & S. Mulholland (Eds.), *Milton Friedman in South Africa* (pp. 48–49). Cape Town and Johannesburg: Graduate School of Business of the University of Cape Town and The Sunday Times.

Friedman, M. (1976g, May 3). Rhodesia. *Newsweek*, p. 77.

Friedman, M. (1976h). *Price Theory* (2nd ed.). Chicago: Aldine Publishing Company.

Friedman, M. (1976i, June 14). Are These Monetary Swings Necessary? *Newsweek*, p. 80.

Friedman, M. (1976j). Inflation: Is It an Incurable Disease? In M. Feldberg, K. Jowell, & S. Mulholland (Eds.), *Milton Friedman in South Africa*. Cape Town and Johannesburg: Graduate School of Business of the University of Cape Town and The Sunday Times.

Friedman, M. (1977a). Nobel Lecture: Inflation and Unemployment. *Journal of Political Economy, 85,* 451–472.

Friedman, M. (1977b, March 21). How to Ration Water. *Newsweek*, p. 73.

Friedman, M. (1977c). *Inflation and Unemployment: The New Dimension of Politics*. London: IEA.

Friedman, M. (1977d). The Conventional Wisdom of J K Galbraith. In A. Seldon (Ed.), *From Galbraith to Economic Freedom* (pp. 12–42). London: IEA.

Friedman, M. (1977e). *From Galbraith to Economic Freedom*. London: IEA.

Friedman, M. (1977f, October 3). Why Inflation Persists. *Newsweek*, p. 84.

Friedman, M. (1977g, May 2). A Monstrosity. *Newsweek,* p. 20.

Friedman, M. (1978a). *Has the Tide Turned?* Glasgow: Strathclyde Business School.

Friedman, M. (1978b). Capitalism, Socialism, and Democracy: A Symposium. Contribution of Milton Friedman. *Commentary, 65*(4), 39–41.

Friedman, M. (1978c, January 30). Back to the Gaming Table. *Newsweek,* p. 65.

Friedman, M. (1978d, November 20). History Repeats. *Newsweek*, p. 94.

Friedman, M. (1978e, April 24). Inflationary Recession. *Newsweek*, p. 81.

Friedman, M. (1978f). *The Economics of Freedom*. Cleveland: Standard Oil Company of Ohio.

Friedman, M. (1979a, December 17). Friedman on Indexation. *International Business Week,* pp. 7, 10.

Friedman, M. (1979b, July 9). Hooray for Margaret Thatcher. *Newsweek*, p. 56.

Friedman, M. (1979c, February 19). The Fed: At It Again. *Newsweek*, p. 65.

Friedman, M. (1980a). Prices of Money and Goods Across Frontiers. *The World Economy, 2*(4), 497–511.

Friedman, M. (1980b). Memorandum to the Treasury and Civil Service Committee. In *Treasury and Civil Service Commitee, Session 1979–80. Memoranda on Monetary Policy* (pp. 55–61). London: HMSO.

Friedman, M. (1980c, March 3). Monetarism: A Reply to the Critics. *The Times*, p. 19.

Friedman, M. (1980d, December 1). The Fed Fails—Again. *Newsweek*, p. 78.

Friedman, M. (1981, December 21). Monetary Instability. *Newsweek*, p. 71.

Friedman, M. (1982a). Monetary Policy: Theory and Practice. *Journal of Money, Credit and Banking, 14*(1), 98–118.

Friedman, M. (1982b). Monetary Policy: Theory and Practice: Reply. *Journal of Money, Credit and Banking, 14*(3), 404–406.

Friedman, M. (1982c). *Capitalism and Freedom* (2nd ed.). Chicago: Chicago University Press.

Friedman, M. (1982d, September 13). The Uses of Hypocrisy. *Newsweek*, p. 75.

Friedman, M. (1982e, February 15). The yo-yo Economy. *Newsweek*, p. 72.

Friedman, M. (1982f, July 12). Defining Monetarism. *Newsweek*, p. 64.

Friedman, M. (1983). *Bright Promises, Dismal Performance: An Economist's Protest* (W. R. Allen, Ed.). New York: Harcourt Brace Jovanovich.

Friedman, M. (1984a, June). Capitalism and the Jews. *Encounter*, pp. 74–79.

Friedman, M. (1984b). Financial Futures Markets and Tabular Standards. *Journal of Political Economy, 92*(1), 165–167.
Friedman, M. (1985a). Monetary Policy in a Fiat World. *Bank of Japan Monetary and Economic Studies, 3,* 11–18.
Friedman, M. (1985b). *Letter to J Daniel Hammond.* Published as an Appendix to J. D. Hammond, 'An Interview with Milton Friedman on Methodology', Research in the History of Economic Thought and Methodology, 1992, vol. 10, pp. 91–118.
Friedman, M. (1985c). The Case for Overhauling the Federal Reserve. *Challenge, 28*(3), 4–12.
Friedman, M. (1985/2005). My Evolution as an Economist. In W. Breit & B. T. Hirsch (Eds.), *Lives of the Laureates* (4th ed.). Boston: MIT Press.
Friedman, M. (1986a). Keynes's Political Legacy. In *Keynes's General Theory: Fifty Years on* (pp. 45–55). London: IEA.
Friedman, M. (1986b, March 10). Right at Last, an Expert's Dream. *Newsweek*, p. 8.
Friedman, M. (1987a). Quantity Theory of Money. In J. Eatwell, M. Milgate, & P. Newman (Eds.), *The New Palgrave—A Dictionary of Economics*. London: Macmillan.
Friedman, M. (1987b). Arthur F. Burns. In Board of Governors of the Federal Reserve System (Ed.), *In Memoriam: Arthur F Burns 1904–1987*. Washington, DC: Board of Governors of the Federal Reserve System.
Friedman, M. (1987c, February). Letter to the Editor: Milton Friedman Replies. *Encounter,* p. 78.
Friedman, M. (1987d). Review of Thomas Sargent, *Rational Expectations and Inflation*. *Journal of Political Economy, 95*(1), 218–221.
Friedman, M. (1988a). Discussion: Objectives, Options, and Obstacles. In R. Dinshaw (Eds.), *The Unstable Dollar. Domestic and International Issues* (pp. 103–137). Hamburg: Verlag Weltarchiv GMBH.
Friedman, M. (1988b). A Proposal for Resolving the U.S. Balance of Payments Problem. Confidential Memorandum to President-Elect Richard Nixon. In L. Melamed (Ed.), *The Merits of Flexible Exchange Rates: An Anthology* (pp. 429–438). Fairfax, VA: George Mason University Press.
Friedman, M. (1988c). Robbins Memorial Lecture: Exchange Rates in a Fiat Money World. In R. Hinshaw (Ed.), *The Unstable Dollar. Domestic and International Issues* (pp. 185–203). Hamburg: Verlag Weltarchiv GMBH.
Friedman, M. (1988d). Introduction. In L. Melamed (Ed.), *The Merits of Flexible Exchange Rates: An Anthology* (pp. xix–xxv). Fairfax, VA: George Mason University Press.

Friedman, M. (1989a, June 22). China's Inflation Time Bomb. *San Francisco Chronicle*.

Friedman, M. (1989b, September 7). An Open Letter to Bill Bennett. *Wall Street Journal*, p. A14.

Friedman, M. (1989c). The Case for Floating Exchange Rates. *Financial Times*, p. 21.

Friedman, M. (1990). *Milton Friedman in China*. Hong Kong: Chinese University of Hong Kong.

Friedman, M. (1991a). The War We Are Losing. In M. B. Krauss & E. P. Lazear (Eds.), *Searching for Alternatives: Drug-Control Policy in the United States*. Stanford, CA: Hoover Institution.

Friedman, M. (1991b). On Drugs and Liberty. *America's Drug Forum*, No. 223.

Friedman, M. (1992a, April 3). Interview: The Evolution of the Chicago School of Economics. *Maroon*.

Friedman, M. (1992b). Do Old Fallacies Ever Die? *Journal of Economic Literature, 30*(4), 2129–2132.

Friedman, M. (1992c). *Money Mischief.* San Diego: Harcourt Brace and Company.

Friedman, M. (1995). Monetary System for a Free Society. In K. D. Hoover & S. M. Sheffrin (Eds.), *Monetarism and the Methodology of Economics*. Cheltenham: Elgar.

Friedman, M. (1996). Review of Peter Groenewegen, *A Soaring Eagle: Alfred Marshall 1842–1924*. *Journal of Economic Literature, 34*(4), 1989–1991.

Friedman, M. (1997, November 19). Why Europe Can't Afford the Euro. *The Times*, p. 22.

Friedman, M. (Ed.). (2000/2012). *Commanding Heights Interview.* Reprinted in, Lanny Ebenstein 2012 (Ed.), *The Indispensable Milton Friedman* (pp. 233–253). Washington, DC: Regnery Publishing, Inc.

Friedman, M. (2001). The IEA's Influence in Our Times. In C. Robinson (Ed.), *A Conversation with Harris and Seldon* (pp. 70–72). London: IEA.

Friedman, M. (2002). *Capitalism and Freedom*. Chicago: Chicago University Press.

Friedman, M. (2003). Preface. In R. Leeson (Ed.), *Keynes, Chicago and Friedman*. London: Pickering & Chatto.

Friedman, M. (2005). *Addendum to Biographical Essay*. http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/friedman-bio.html.

Friedman, M. (2009). Final Word. In U. Maki (Ed.), *The Methodology of Positive Economics. Reflections on the Milton Friedman Legacy* (p. 355). Cambridge: Cambridge University Press.

Friedman, M. (2010). Trade-Offs in Monetary Policy. In R. Leeson (Ed.), *David Laidler's Contributions to Economics* (pp. 114–118). Basingstoke: Palgrave.

Friedman, M., & Becker, G. S. (1957). A Statistical Illusion in Judging Keynesian Models. *Journal of Political Economy, 65*(1), 64–75.

Friedman, M., & Becker, G. S. (1958a). Reply to Kuh and Johnston. *Review of Economics and Statistics, 40*(3), 298.

Friedman, M., & Becker, G. S. (1958b). The Friedman-Becker Illusion: Reply. *Journal of Political Economy, 66*(6), 545–547.

Friedman, M., & Friedman, R. D. (1980). *Free to Choose*. London: Penguin.

Friedman, M., & Friedman, R. D. (1984). *Tyranny of the Status Quo*. London: Secker & Warburg.

Friedman, M., & Friedman, R. D. (1988). The Tide in the Affairs of Men. In A. Anderson & D. L. Bark (Eds.), *Thinking About America*. Stanford: Hoover Institution.

Friedman, M., & Friedman, R. D. (1998a). *Two Lucky People*. Chicago: University of Chicago Press.

Friedman, M., & Friedman, R. D. (1998b). Two lucky people. *The Commonwealth*, pp. 10–12.

Friedman, M., & Kuznets, S. (1939). Income from Independent Professional Practice. *NBER Bulletin* (72–73), 1–31.

Friedman, M., & Kuznets, S. (1945). *Income from Independent Professional Practice*. New York: NBER.

Friedman, M., & Meiselman, D. I. (1963). The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958. In *Stabilization Policies* (pp. 165–268). Englewood Cliffs, NJ: Commission on Money and Credit/Prentice Hall.

Friedman, M., & Meiselman, D. I. (1964). Keynes and the Quantity Theory: A Comment on the Friedman-Meiselman CMC Paper. Reply to Donald Hester. *Review of Economics and Statistics, 46*(4), 369–376.

Friedman, M., & Meiselman, D. I. (1965). Reply to Ando and Modigliani and to DePrano and Mayer. *American Economic Review, 4*(55), 753–785.

Friedman, M., & Roosa, R. V. (1967). *The Balance of Payments: Free Versus Fixed Exchange Rates*. Washington, DC: American Enterprise Institute for Public Policy Research.

Friedman, M., & Savage, L. J. (1947). Planning Experiments Seeking Maxima. In C. Eisenhart, M. W. Hastay, & W. A. Wallis (Eds.), *Selected Techniques of Statistical Analysis* (pp. 363–372). New York: McGraw-Hill Book.

Friedman, M., & Savage, L. J. (1948). The Utility Analysis of Choices Involving Risk. *Journal of Political Economy, LVI*(4), 279–304.
Friedman, M., & Savage, L. J. (1952). The Expected Utility Hypothesis and the Measurability of Utility. *Journal of Political Economy, LX*(6), 463–474.
Friedman, M., & Savage, L. J. (1953). The Utility Analysis of Choices Involving Risk. In K. E. Boulding & G. J. Stigler (Eds.), *Readings in Price Theory* (pp. 57–96). London: George Allen and Unwin.
Friedman, M., & Schwartz, A. J. (1963a). *A Monetary History of the United States 1867–1960*. Princeton: Princeton University Press.
Friedman, M., & Schwartz, A. J. (1963b). Money and Business Cycles. *Review of Economics and Statistics, 45*(1), 32–64.
Friedman, M., & Schwartz, A. J. (1965). *The Great Contraction*. Princeton, NJ: Princeton University Press.
Friedman, M., & Schwartz, A. J. (1970). *Monetary Statistics of the United States: Estimates, Sources, Methods*. New York: National Bureau of Economic Research.
Friedman, M., & Schwartz, A. J. (1982). *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975*. Chicago: Chicago University Press.
Friedman, M., & Schwartz, A. J. (1986a). The Failure of the Bank of United States: A Reappraisal. A Reply. *Explorations in Economic History, 23*(2), 199–204.
Friedman, M., & Schwartz, A. J. (1986b). Has Government Any Role in Money? *Journal of Monetary Economics, 17*(1), 37–62.
Friedman, M., & Schwartz, A. J. (1991). Alternative Approaches to Analyzing Economic Data. *American Economic Review, 81*(1), 39–49.
Friedman, M., & Stigler, G. J. (1946). *Roofs or Ceilings?* Irvington-on-Hudson: Foundation for Economic Education.
Friedman, R. D. (1976). Milton Friedman: Husband and Colleague (2). The Beginning of a Career. *The Oriental Economist, 44*(6), 18–22.
Friedman, R. D. (1977). Milton Friedman: Husband and Colleague (10). The Nobel Award. *The Oriental Economist, 45*(2), 24–28.
Friend, I. (1958). Further Comments on 'A Theory of the Consumption Function'. In L. Clark (Ed.), *Consumer Behaviour: Research on Consumer Reactions* (pp. 456–458). New York: Harper and Brothers.
Friend, I., & Kravis, I. B. (1957). Consumption Patterns and Permanent Income. *The American Economic Review, 47*(2), 536–555.

Friend, I., & Kravis, I. B. (1957). Entrepreneurial Income, Saving and Investment. *The American Economic Review, 47*(3), 270–301.
Frost, G. (2002). *Anthony Fisher*. London: Profile Books.
Furness, W. H. (1909). *The Island of Stone Money: Uap of the Carolines.* Philadelphia: J.B. Lippincott.
Galbraith, J. K. (1954). *The Great Crash of 1929*. Boston: Houghton Mifflin.
Galbraith, J. K. (1958). *The Affluent Society*. Boston: Houghton Mifflin.
Galbraith, J. K. (1981). *A Life in Our Times*. London: Andre Deutsch.
Gates, T., Curtis, T., Dent, F., Friedman, M., Greenwalt, C., Greenspan, A., et al. (1979). *Report of the President's Commission on an All-Volunteer Armed Force*. Washington, DC: United States Government Printing Office.
Gellhorn, W. (1956). *Individual Freedom and Governmental Restraints*. Baton Rouge: Louisiana State University Press.
Georgescu-Roegen, N. (1936). Marginal Utility of Money and Elasticities of Demand. *Quarterly Journal of Economics, 50*(3), 533–539.
Giersch, H. (Ed.). (1974). *Essays on Inflation and Indexation*. Washington, DC: American Enterprise Institute for Public Policy Research.
Gilboy, E. W. (1938). The Propensity to Consume. *Quarterly Journal of Economics, 53*(1), 120–140.
Goldenweiser, E. A. (1945). Postwar Problems and Policies. *Federal Reserve Bulletin, 31*(2), 112–121.
Goldenweiser, E. A. (1951). *American Monetary Policy*. New York: McGraw Hill.
Goodhart, C. A. E. (1964). Review of Milton Friedman and Anna J. Schwartz, *A Monetary History of the United States, 1867–1960*. *Economica, 31*(123), 314–318.
Goodhart, C. A. E. (1982). Monetary Trends in the United States and the United Kingdom: A British Review. *Journal of Economic Literature, 20*(4), 1540–1551.
Goodman, J. F. B. (1975). Indexation of Wages and Salaries. In B. Carsberg, E. V. Morgan, & M. Parkin (Eds.), *Indexation and Inflation*. London: Financial Times.
Gordon, R. J. (Ed.). (1974). *Milton Friedman's Monetary Framework*. Chicago: University of Chicago Press.
Gordon, R. J. (1974b). Introduction. In R. J. Gordon (Ed.), *Milton Friedman's Monetary Framework* (pp. ix–xii). Chicago: Chicago University Press.
Graham, B. (1937). *Storage and Stability*. New York: McGraw Hill.
Graham, B. (1944). *World Commodities and World Currency*. New York: McGraw Hill.

Graham, F. D. (1942). *Social Goals and Economic Institutions*. Princeton: Princeton University Press.
Gramley, L. E., & Chase, S. B. (1965). Time Deposits in Monetary Analysis. *Federal Reserve Bulletin, 51*(October), 1380–1406.
Gravelle, H., & Rees, R. (1992). *Microeconomics* (2nd ed.). London: Longman.
Groenewegen, P. (1995). *A Soaring Eagle: Alfred Marshall 1842–1924*. Aldershot: Edward Elgar.
Gurley, J. G. (1960). Liquidity and Financial Institutions in the Postwar Period. In U. S. Congress, *Employment, Growth, and Price Levels: Joint Economic Committee* (Study Papers 14–23). Washington, DC: Government Printing Office.
Gurley, J. G., & Shaw, E. S. (1960). *Money in a Theory of Finance.* Washington, DC: Brookings Institution.
Haavelmo, T. (1943). The Statistical Implications of a System of Simultaneous Equations. *Econometrica, 11*(1), 1–12.
Haberler, G. (1948). Causes and Cures of Inflation. *Review of Economics and Statistics, 30*(1), 10–14.
Haberler, G. (1954). *Currency Convertibility*. Washignton, DC: American Enterprise Association.
Hagen, E. E. (1955). The Consumption Function: A Review Article. *The Review of Economics and Statistics, 37*(1), 48–54.
Hahn, F. H., & Neild, R. (1980a, February 25). Monetarism: Why Mrs. Thatcher Should Beware. *The Times*, p. 19.
Hahn, F. H., & Neild, R. (1980b, March 13). Monetarism: No Basis For Theory That Economy is Self Regulating. *The Times*, p. 22.
Hall, R. E. (1982). Monetary Trends in the United States and the United Kingdom: A Review from the Perspective of New Developments in Monetary Economics. *Journal of Economic Literature, 20*(4), 1552–1556.
Hamilton, E. J., Rees, A., & Johnson, H. G. (1962). *Landmarks in Political Economy*. Chicago: Chicago University Press.
Hammond, J. D. (1986). Monetarist and Antimonetarist Causality. *Research in the History of Economic Thought and Methodology, 4,* 109–126.
Hammond, J. D. (1992). An Interview with Milton Friedman. In *Research in the History of Economic Thought and Methodology, 10*, 91–118.
Hammond, J. D. (1996). *Theory and Measurement: Causality Issues in Milton Friedman's Monetary Economics*. Cambridge: Cambridge University Press.
Hammond, J. D. (Ed.). (1999). *The Legacy of Milton Friedman as Teacher*. Cheltenham: Edward Elgar.

Hammond, J. D. (2003). Remembering Economics. *Journal of the History of Economic Thought, 25*(2), 133–143.
Hammond, J. D. (2009). Early Drafts of Friedman's Methodology Essay. In U. Mäki (Ed.), *The Methodology of Positive Economics. Reflections on the Milton Friedman Legacy* (pp. 68–89). Cambridge: Cambridge University Press.
Hammond, J. D., & Hammond, C. (2006). *Making Chicago Price Theory*. Chicago: University of Chicago Press.
Hansen, A. H. (1932). *Economic Stabilization in an Unbalanced World*. New York: Harcourt, Brace and Company.
Hansen, A. H. (1941). *Fiscal Policy and Business Cycles*. New York: W. W. Norton.
Hansen, A. H. (1947). The General Theory. In S. E. Harris (Ed.), *The New Economics: Keynes' Influence on Theory and Public Policy* (pp. 133–145). New York: Alfred A. Knopf.
Hansen, A. H. (1949). *Monetary Theory and Fiscal Policy*. New York: McGraw Hill.
Hansen, A. H. (1957). *The American Economy*. New York: McGraw Hill.
Harberger, A. C. (1976). Inflation. In R. M. Hutchins & M. J. Adler (Ed.), *The Great Ideas Today*. Chicago: Encyclopaedia Britannica.
Hardy, C. O. (1946). Prospects of Inflation in the Transition Period. In E. A. Goldenweiser (Ed.), *Postwar Economic Studies No. 4*. Washington, DC: Board of Governors of the Federal Reserve System.
Harford, T. (2017). *Fifty Things That Made the Modern Economy*. London: Little Brown.
Harris, S. E. (1948). Symposium: Ten Economists on Inflation. *Review of Economics and Statistics, 30*(1), 1–29.
Harris, S. E. (1951). Introductory Remarks. *The Review of Economics and Statistics, 33*(3), 179–184.
Harris, S. E. (1951). Summary and Comments. *The Review of Economics and Statistics, 33*(3), 198–200.
Harris, S. E. (1955). Liberalism. In *Collier's Year Book*. New York: P. F. Collier & Son.
Harrod, R. F. (1946). Review of Lange, *Price Flexibility and Employment*. *The Economic Journal, 56*(221), 102–107.
Harrod, R. F. (1964). Review of Friedman and Schwartz. *The University of Chicago Law Review, 32*(1), 188–196.
Harrod, R. F. (1971). Discussion of Friedman, 'A Monetary Theory of Nominal Income'. In G. Clayton, J. C. Gilbert, & R. Sedgwick (Eds.),

Monetary Theory and Monetary Policy in the 1970s (pp. 58–63). London: Oxford University Press.

Hart, A. G. (1946). The Problem of Full Employment: Facts, Issues and Policies. *American Economic Review, 36*(2), 280–290.

Hayek, F. A. (1944). *The Road to Serfdom*. London: Routledge.

Hayek, F. A. (1945, September). The Use of Knowledge in Society. *American Economic Review, 35,* 519–530.

Hayek, F. A. (1976a). *Choice in Currency.* London: IEA.

Hayek, F. A. (1976b). *Denationalisation of Money*. London: IEA.

Heilbroner, R. L. (1980, April 17). The Road to Serfdom. *New York Review of Books.*

Helm, D. (1984). Predictions and Causes: A Comparison of Friedman and Hicks on Method. *Oxford Economic Papers, 36*(4), 118–134.

Henderson, L., & Nelson, D. M. (1941). Prices, Profits, and Government. *Harvard Business Review, 19*(4), 397.

Hendry, D. F. (1980). Econometrics—Alchemy or Science? *Economica, 47*(188), 387–406.

Hendry, D. F., & Ericsson, N. R. (1983). Assertion Without Empirical Basis: An Econometric Appraisal of *Monetary Trends in … the United Kingdom* by Milton Friedman and Anna J. Schwartz. In R. C. O. Matthews (Ed.), *Bank of England Panel of Academic Consultants: Panel Paper 22* (pp. 45–101). London: Bank of England.

Hendry, D. F., & Ericsson, N. R. (1991). An Econometric Analysis of UK Money Demand in Monetary Trends in the United States and the United Kingdom by Milton Friedman and Anna J. Schwartz. *American Economic Review, 81*(1), 8–38.

Hepple, A. (1967). *Verwoerd*. Baltimore: Penguin.

Hester, D. D. (1964). Keynes and the Quantity Theory: A Comment on the Friedman–Meiselman CMC Paper. *Review of Economics and Statistics, 46*(4), 364–368.

Hester, D. D. (1964). Keynes and the Quantity Theory: A Comment on the Friedman–Meiselman CMC Paper: Rejoinder. *Review of Economics and Statistics, 46*(4), 376–377.

Hetzel, R. L. (2016). Arthur Burns and Milton Friedman. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Hicks, J. R. (1956). *A Revision of Demand Theory*. Oxford: Oxford University Press.

Hicks, J. R. (1963). Review of Milton Friedman, *Capitalism and Freedom*. *Economica, 30*(119), 319–320.

Hirsch, A., & de Marchi, N. (1990). *Milton Friedman: Economics in Theory and Practice*. Ann Arbor: University of Michigan Press.

Hoffman, C. (1957). Review of Friedman, *A Theory of the Consumption Function*. *Annals of the American Academy of Political and Social Science, 314,* 198–199.

Holden, A. (1980, February 17). The Free Market Man. *The Observer*, pp. 33, 35.

Hoopes, R. (1963). *The Steel Crisis*. New York: John Day.

Hoover, K. D. (1984). Two Types of Monetarism. *Journal of Economic Literature, 22*(1), 58–76.

Hoover, K. D. (1988). *The New Classical Macroeconomics*. Oxford: Blackwell.

Hoover, K. D. (2009). Milton Friedman's Stance: The Methodology of Causal Realism. In U. Maki (Ed.), *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (pp. 303–320). Cambridge: Cambridge University Press.

Hoover, K. D. (2015). A Review of James Forder's, *Macroeconomics and the Phillips Curve Myth*. *Balliol College Annual Record* (pp. 56–59), SSRN: 2630589.

Hopkin, B. (1974). *The Control of Demand*. Cardiff: University College, Cardiff.

Hotelling, H. (1938a). Review of Schultz, *The Theory and Measurement of Demand*. Chicago: University of Chicago Press. *Journal of the American Statistical Association, 33*(204), 744–747.

Hotelling, H. (1938b). The General Welfare in Relation to Problems of Taxation and of Railway and Utility Rates. *Econometrica, 6*(3), 242–269.

Hotelling, H., Bartky, W., Deming, W. E., Friedman, M., & Hoel, P. (1948). A Report of the Institute of Mathematical Statistics Committee on the Teaching of Statistics. *Annals of Mathematical Statistics, 19*(1), 95–115.

Houthakker, H. S. (1958a). The Permanent Income Hypothesis. *American Economic Review, 48*(3), 396–404.

Houthakker, H. S. (1958b). The Permanent Income Hypothesis: Reply. *American Economic Review, 48*(5), 991–993.

Howson, S. (2011). *Lionel Robbins*. Cambridge: Cambridge University Press.

Howson, S. (2016). Friedman and Robbins. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Hume, D. (1739). *Treatise of Human Nature*. London: Noon.

Humphrey, T. M. (1971). Role of Non-Chicago Economists in the Evolution of the Quantity Theory in America 1930–1950. *Southern Economic Journal, 38*(1), 12–18.

Humphrey, T. M. (1998). Historical Origins of the Cost-Push Fallacy. *Federal Reserve Bank of Richmond Economic Quarterly, 84*(3), 53–74.

Hutchison, T. W. (1938). *The Significance and Basic Postulates of Economic Theory*. London: Macmillan.

Hutchison, T. W. (1954). Review of Friedman, *Essays on Positive Economics*. *The Economic Journal, 64*(256), 796–799.

Hynes, J. A. (1998). The Emergence of the Neoclassical Consumption Function: The Formative Years, 1940–1952. *Journal of the History of Economic Thought, 20*(1), 25–49.

Irving, H. (2014). The Birth of a Politician: Harold Wilson and the Bonfires of Controls, 1948–9. *Twentieth Century British History, 25*(1), 87–107.

Jay, P. (1973, December 5). The Good Old Days of Stop-Go Economics. *The Times*, p. 18.

Jay, P. (1974a, July 1). How Inflation Threatens British Democracy with Its Last Chance Before Extinction. *The Times*, p. 14.

Jay, P. (1974b). Do Trade Unions Matter? In L. Robbins (Ed.), *Inflation: Causes, Consequences, Cures* (pp. 27–34). London: IEA.

Jay, P. (1976). *A General Hypothesis of Inflation, Employment, and Politics*. London: IEA.

Jay, P. (1980, May 1). Who Really are the British Monetarists? *The Listener, 103*(2660), 561–564.

Jenkins, P. (1976a, November 3). Bully Boys. *The Guardian*, p. 12.

Jenkins, P. (1976b, December 9). Farewell State? *The Guardian*, p. 14.

Jensen, E. R. (1985). Friedman's Chi-Sqaure Test. In S. Kotz & N. Johnson (Eds.), *Encyclopedia of Statistical Sciences* (pp. 247–250). New York: Wiley.

Jevons, M. (1978). *Murder at the Margin*. Princeton: Princeton University Press.

Jevons, M. (1985). *The Fatal Equilibrium.* Cambridge: MIT Press.

Johnson, H. G. (1951). The Taxonomic Approach to Economic Policy. *Economic Journal, 61*(244), 812–831.

Johnson, H. G. (1962). Monetary Theory and Policy. *American Economic Review, 52*(3), 335–384.

Johnson, H. G. (1965). A Quantity Theorist's Monetary History of the United States. *Economic Journal, 75*(298), 388–396.

Johnson, H. G. (1968). The Economic Approach to Social Questions. *Economica, 35*(137), 1–21.

Johnson, H. G. (1969). The Case for Flexible Exchange Rates, 1969. In H. G. Johnson & J. E. Nash (Eds.), *The UK and Floating Exchange Rates*. London: IEA.

Johnson, H. G. (1970). Recent Developments in Monetary Theory—A Commentary. In D. R. Croome & H. G. Johnson (Eds.), *Money in Britain 1959–1969* (pp. 83–114). Oxford: Oxford University Press.

Johnson, H. G. (1971a). The Keynesian Revolution and the Monetarist Counter-Revolution. *American Economic Review, 61*(2), 1–14.

Johnson, H. G. (1971b). *Macroeconomics and Monetary Theory*. London: Gray-Mills.

Johnson, H. G. (1971c). Is There an Optimal Money Supply—I. In M. Intriligator (Ed.), *Fronteirs of Quantitative Economics*. Amsterdam: Noth-Holland.

Johnson, H. G. (1972). *Inflation and the Monetarist Controversy. Professor Dr F De Vries Lectures*. Amsterdam: North Holland.

Johnson, H. G. (1976, October 23). The Nobel Milton. *The Economist*, p. 95.

Johnson, K., & Johnson, M. (2009). Seminars by Hicks and Koopmans, University of Chicago, October 1946. *Research in the History of Economic Thought and Methodology, 27C*, 201–212.

Johnston, J. (1958a). Review of Friedman, *A Theory of the Consumption Function*. *Review of Economics and Statistics, 40*(4), 431–435.

Johnston, J. (1958b). A Statistical Illusion in Judging Keynesian Models: Comment. *Review of Economics and Statistics, 40*(3), 296–298.

Johnston, N. (2013). *The History of the Parliamentary Franchise* (House of Commons Research Paper 13/14). London.

Jones, H. S. (2008). Philosophic Radicals (Act. 1830–1841). In *Oxford Dictionary of National Biography*. Oxford: Oxford University Press.

Jordan, J. L. (1986, October). The Andersen–Jordan Approach After Nearly 20 Years. *Federal Reserve Bank of St. Louis Review, 68*(8), 5–8.

Kaldor, N. (1942). Review of Triffin, *Monopolistic Competition and General Equilibrium Theory*. *Economica, 9*(36), 409–412.

Kaldor, N. (1970, July). The New Monetarism. *Lloyds Bank Review*, pp. 1–17.

Kaldor, N. (1982). *The Scourge of Monetarism* (1st ed.). Oxford: Oxford University Press.

Kaldor, N. (1985). *The Scourge of Monetarism* (2nd ed.). Oxford: Oxford University Press.

Kareken, J., & Solow, R. M. (1963). Lags in Monetary Policy. In *Stabilization Policies* (pp. 1–96). Englewood Cliffs, NJ: Commission on Money and Credit/Prentice Hall.

Katona, G. (1949). Effect of Income Changes on the Rate of Saving. *Review of Economic Studies, 31*(2), 95–103.

Katona, G. (1951). *Psychological Analysis of Economic Behavior*. New York: McGraw-Hill.

Katona, G. (1968). On the Function of Behavioral Theory and Behavioral Research in Economics. *American Economic Review, 58*(1), 146–149.

Kaufman, G. (2013). *Dearing, Ronald Ernest [Ron], Baron Dearing (1930–2009)*. Oxford: Oxford Dictionary of National Biography.

Kemmerer, D. L. (1960). Review of Lester Chandler, *Benjamin Strong, Central Banker*. *Economic History Review, 12*(3), 501–502.

Kemmerer, D. L. (1964). Review of Milton Friedman and Anna J. Schwartz, *A Monetary History of the United States, 1867–1960*. *American Historical Review, 70*(1), 195–197.

Keynes, J. M. (1923). *Tract on Monetary Reform*. London: Macmillan.

Keynes, J. M. (1925). Alfred Marshall, 1842–1924. In A. C. Pigou (Ed.), *Memorials of Alfred Marshall*. London: Macmillan.

Keynes, J. M. (1936). *The General Theory of Employment, Interest and Money*. London: Macmillan.

Keynes, J. N. (1891). *The Scope and Method of Political Economy*. London: Macmillan.

Keyserling, L. H. (1963). Review of Milton Friedman, *Capitalism and Freedom*. *The Annals of the American Academy of Political and Social Science, 350,* 195–196.

Kilgour, D. G. (1964). Review of Milton Friedman, *Capitalism and Freedom*. *The University of Toronto Law Journal, 15*(2), 504–504.

Kindleberger, C. P. (1970). The Case for Fixed Exchange Rates, 1969. In F. E. Morris (Ed.), *The International Adjustment Mechanism*. Boston, MA: Federal Reserve Bank of Boston.

Kirk, R. (1954). Conservatism. In *Collier's Year Book.* New York: P. F. Collier & Son.

Klappholz, K., & Agassi, J. (1959). Methodological Prescription in Economics. *Economica, 26*(101), 60–74.

Klein, B. (1974). The Competitive Supply of Money. *Journal of Money, Credit and Banking, 6*(4), 423–453.

Klein, J. J. (1956). German Money and Price Rises, 1932–44. In M. Friedman (Ed.), *Studies in the Quantity Theory of Money* (pp. 121–159). Chicago: University of Chicago.

Klein, L. R. (1958). The Friedman–Becker Illusion. *Journal of Political Economy, 66*(6), 539–545.

Klein, N. (2007). *The Shock Doctrine*. London: Penguin.

Knight, F. H. (1922). Ethics and the Economic Interpretation. *Quarterly Journal of Economics, 36*(3), 454–481.

Knight, F. H. (1923a). Some Books on Fundamentals. *Journal of Political Economy, 31*(3), 242–359.

Knight, F. H. (1923b). Review of O. Fred Boucke, *A Critique of Economics: Doctrinal and Methodological*, New York: Macmillan 1922. *American Economic Review, 13*(2), 286–288.

Knight, F. H. (1925). Economic Psychology and the Value Problem. *Quarterly Journal of Economics, 39*(3), 372–409.

Knight, F. H. (1935). *The Ethics of Competition*. New York: Augustus M. Kelley.

Knight, F. H. (1941). Review of Triffin, *Monopolistic Competition and General Equilibrium Theory*. *American Journal of Sociology, 46*(6), 914.

Knight, F. H. (1944). Realism and Relevance in the Theory of Demand. *Journal of Political Economy, 52*(4), 289–318.

Knight, F. H. (1946). Comment on Mr. Bishop's Article. *Journal of Political Economy, 54*(2), 170–176.

Koopmans, T. C. (1947). Measurement Without Theory. *Review of Economics and Statistics, 29*(3), 161–172.

Koopmans, T. C. (1957). *Three Essays on the State of Economic Science*. London: McGraw Hill.

Kreinin, M. E. (1961). Windfall Income and Consumption—Additional Evidence. *American Economic Review, 51*(3), 388–390.

Kreinin, M. E. (1963). Windfall Income and Consumption: A Further Comment. *The American Economic Review, 53*(3), 448.

Kuh, E. (1958). A Note on Prediction from Keynesian Models. *Review of Economics and Statistics, 40*(3), 294–295.

Kuznets, S. (1933). *Seasonal Variations in Industry and Trade*. Chicago: University of Chicago Press.

Kuznets, S. (1941). Capital Formation, 1879–1938. In W. C. Mitchell, H. Hoover, & J. M. Clark (Eds.), *Studies in Economics and Industrial Relations* (pp. 53–78). Philadelphia: University of Pennsylvania Press.

Kuznets, S. (1942). *Uses of National Income in Peace and War*. New York: NBER.

Kuznets, S. (1946). *National Product Since 1869*. New York: NBER.

Kuznets, S. (1952). Proportion of Capital Formation to National Product. *American Economic Review, 42*(2), 507–526.

Laidler, D. E. W. (1966). The Rate of Interest and the Demand for Money—Some Empirical Evidence. *Journal of Political Economy, 74*(6), 543–554.

Laidler, D. E. W. (1969). *The Demand for Money*. New York: HarperCollins.

Laidler, D. E. W. (1971). The Influence of Money on Economic Activity—A Survey of Some Current Problems. In G. Clayton, J. C. Gilbert, & R. Sedgwick (Eds.), *Monetary Theory and Monetary Policy in the 1970s* (pp. 73–135). London: Oxford University Press.

Laidler, D. E. W. (1973). Review of Patinkin, *Studies in Monetary Economics*. *Economic Journal, 83*(329), 262–264.

Laidler, D. E. W. (1982). Friedman and Schwartz on Monetary Trends: A Review Article. *Journal of International Money and Finance, 1*(1), 293–305.

Laidler, D. E. W. (1991). *The Golden Age of the Quantity Theory*. Princeton: Princeton University Press.

Laidler, D. E. W. (1993). Hawtrey, Harvard and the Origins of the Chicago Tradition. *Journal of Political Economy, 101*(6), 1068–1103.

Laidler, D. E. W. (2015). Review of James Forder, *Macroeconomics and the Phillips Curve Myth*. Oxford University Press, 2014. https://tinyurl.com/DEWLJF2015.

Laidler, D. E. W. (2017). Review of Milton Friedman, *Contributions to Economics and Public Policy*, edited by Robert A. Cord and J. Daniel Hammond. *History of Political Economy, 49*(4), 730–739.

Lampman, R. J. (1993). *Economists at Wisconsin, 1892–1992*. Madison: University of Wisconsin Press.

Lange, E., & Pharo, H. (1991). Planning and Economic Policy in Norway, 1945–1960. *Scandinavian Journal of History, 16*(3), 215–228.

Lange, O. (1945). *Price Flexibility and Employment*. Bloomington, IN: Principia Press.

Larmer, B. (1998, December 28). Undying Memory. *Newsweek*, p. 31.

Lasky, V. (1975). *Turning Defeat into Victory: The Soviet Offensive Against Chile*. New York: American-Chilean Council.

Latané, H. A. (1960). Income Velocity and Interest Rates—A Pragmatic Approach. *Review of Economics and Statistics, 42*(4), 445–449.

Lawrence, M., & Norman, G. (1973, February). An Interview with Milton Friedman. *Playboy*, pp. 51–56, 58–60, 62, 64, 66, 68, 74.

Lazarus, I. (1946). Review of Friedman and Kuznets, *Income from Independent Professional Practice*. *Columbia Law Review, 46*(4), 680–682.

Leeson, R. (2000). Patinkin, Johnson and 'The Shadow of Friedman'. *History of Political Economy, 32*(4), 733–763.

Leeson, R. (Ed.). (2003). *Keynes, Chicago and Friedman*. London: Pickering & Chatto.

Leeson, R., & Palm, C. G. (Eds.). (2017). *Milton Friedman on Freedom*. Stanford, CA: Hoover Institution Press.

Lerner, A. P. (1943). Functional Finance and the Federal Debt. *Social Research, 10*(1), 38–51.

Lerner, A. P. (1944). *The Economics of Control*. New York: Macmillan.

Lerner, A. P. (1962). *Journal of the American Statistical Association, 57*(297), 211–220.

Lerner, A. P. (1963). Review of Milton Friedman, *Capitalism and Freedom*. *The American Economic Review, 53*(3), 458–460.

Lerner, E. (1956). Inflation in the Confederacy. In M. Friedman (Ed.), *Studies in the Quantity Theory of Money* (pp. 163–175). Chicago: University of Chicago.

Lester, R. A. (1946). Shortcomings of Marginal Analysis for Wage-Employment Problems. *The American Economic Review, 36*(1), 63–82.

Letelier, O. (1976, August 28). Economic Freedom's Awful Toll. *The Nation.*

Leven, M., & Wright, K. R. (1938). *The Income Structure of the United States*. Washington, DC: Brookings.

Levin, F. J., & Meulendyke, A.-M. (1982). Monetary Policy: Theory and Practice: Comment. *Journal of Money Credit and Banking, 14*(3), 399–403.

Levitt, S. D., & Dubner, S. J. (2005). *Freakonomics: A Rogue Economist Explores the Hidden Side of Everything*. New York: William Morrow.

Levrero, E. S. (2018). An Initial 'Keynesian Illness'? Friedman on Taxation and the Inflationary Gap. *Cambridge Journal of Economics, 42*(5), 1219–1237.

Levy, D. M., & Peart, S. J. (2017). *Escape from Democracy: The Role of Experts and the Public in Economic Policy.* Cambridge: Cambridge University Press.

Lewis, M. K. (1967). Friedman–Meiselman and Autonomous Expenditures. *American Economic Review, 57*(3), 541–548.

Lewis, P. (1974, August 18). Challenging the Olympian Fed. *New York Times*, p. 121.

Lindley, D. V. (1980). L. J. Savage—His Work in Probability and Statistics. *The Annals of Statistics, 8*(1), 1–24.

Lipsey, R. G. (1963). *An Introduction to Positive Economics*. London: Weidenfeld & Nicholson.

Liviatan, N. (1963). Tests of the Permanent-Income Hypothesis Based on a Reinterview Savings Survey. In C. F. Christ (Ed.), *Measurement in Economics* (pp. 29–58). Stanford: Stanford University Press.

London, S. (2003, June 7). Lunch with the FT: An interview with Milton Friedman. *Financial Times Magazine*, pp. 12–13.

Lothian, J. R. (1985). Equilibrium Relationship Between Money and Other Economic Variables. *American Economic Review, 75*(4), 828–835.

Lothian, J. R., & Tavlas, G. S. (2018). How Friedman and Schwartz Became Monetarists. *Journal of Money, Credit and Banking, 50*(4), 757–787.

Lucas, R. E. (1973). Some International Evidence on Output-Inflation Tradeoffs. *American Economic Review, 63*(3), 326–334.

Lucas, R. E. (1975). An Equilibrium Model of the Business Cycle. *Journal of Political Economy, 83*(6), 1113–1144.

Lucia, J. L. (1985). The Failure of the Bank of United States: A Reappraisal. *Explorations in Economic History, 22*(4), 402–416.

Lusardi, A., & Mitchell, O. S. (2014). The Economic Importance of Financial Literacy: Theory and Evidence. *Journal of Economic Literature, 52*(1), 5–44.

Lydall, H. F. (1958). Review of Milton Friedman, *A Theory of the Consumption Function*. *Kyklos, 11*(4), 563–564.

MacDougall, D. (1987). *Don and Mandarin*. London: John Murray.

Macesich, G. (1964). The Quantity Theory and the Income Expenditure Theory in an Open Economy: Canada 1926–1958. *Canadian Journal of Economics and Political Science, 30*(3), 368–390.

Macesich, G. (1966). Empirical Testing and the Income Expenditure Theory. *Canadian Journal of Economics and Political Science, 32*(3), 377–379.

Machlup, F. (1946). Marginal Analysis and Empirical Research. *The American Economic Review, 36*(4), 519–554.

Machlup, F. (1948). Misconceptions about the Current Inflation. *Review of Economics and Statistics, 30*(1), 17–22.

Machlup, F. (1955). The Problem of Verification in Economics. *Southern Economic Journal, 22*(1), 1–21.

Machlup, F. (1960). Another View of Cost-Push and Demand-Pull Inflation. *Review of Economics and Statistics, 42*(2), 125–139.

Mack, R. P. (1948). The Direction of Change in Income and the Consumption Function. *Review of Economics and Statistics, 30*(4), 239–258.

Mack, R. P. (1952). Economics of Consumption. In H. S. Ellis (Ed.), *A Survey of Contemporary Economics*. Homewood, IL: Richard D. Irwin.

Macpherson, C. B. (1968). Elegant Tombstones: A Note on Friedman's Freedom. *Canadian Journal of Political Science, 1*(1), 95–106.

Mäki, U. (1986). Rhetoric at the Expense of Coherence. *Research in the History of Economic Thought and Methodology, 4,* 127–143.

Mäki, U. (Ed.). (2009a). *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy*. Cambridge: Cambridge University Press.

Mäki, U. (2009b). Preface. In U. Mäki (Ed.), *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (pp. xvii–xviii). Cambridge: Cambridge University Press.

Mäki, U. (2009c). Reading the Methodological Essay in Twentieth-Century Economics: Map of Multiple Perspectives. In U. Mäki (Ed.), *The Methodology of Positive Economics. Reflections on the Milton Friedman Legacy* (pp. 47–67). Cambridge: Cambridge University Press.

Mariyani-Squire, E. (2018). Milton Friedman's Causal Relist Stance? *Oxford Economic Papers, 70*(1), 719–740.

Markowitz, H. (1952). The Utility of Wealth. *Journal of Political Economy, 60*(2), 151–158.

Marshall, A. (1885). *The Present Position of Economics*. London: Macmillan.

Marshall, A. (1887/1926). Evidence Before the Gold and Silver Commission. In *Official Papers of Alfred Marshall.* London: Macmillan.

Marshall, A. (1890). *Principles of Economics* (1st ed.). London: Macmillan.

Marshall, A. (1895). *Principles of Economics* (3rd ed.). London: Macmillan.

Marshall, A. (1920). *Principles of Economics* (8th ed.). London: Macmillan.

Marshall, A. (1923). *Money, Credit and Commerce*. London: Macmillan.

Mas-Colell, A., Whinston, M. D., & Green, J. (2005). *Microeconomic Theory.* Oxford: Oxford University Press.

Mason, W. E. (1976). The Empirical Definition of Money: A Critique. *Economic Enquiry, 14,* 525–538.

Matthews, R. C. O. (1983). Monetary Trends in the United Kingdom. Introductory Note. In *Bank of England Panel of Academic Consultants: Panel Paper 22* (pp. 3–7). London: Bank of England.

Matthews, R. C. O. (2000). Review of Middleton, *Charlatans or Saviours? Economists and the British Economy from Marshall to Meade*. *Economic Journal, 110*(461), F188–F189.

Mayer, T. (1975). The Structure of Monetarism (II). *Kredit und Kapital, 8,* 293–313.

Mayer, T. (1982). Monetary Trends in the United States and the United Kingdom: A Review Article. *Journal of Economic Literature, 20*(4), 1528–1539.

Mayer, T. (1993). Friedman's Methodology of Positive Economics: A Soft Reading. *Economic Inquiry, 31*(2), 213–223.
McCloskey, D. (1983). The Rhetoric of Economics. *Journal of Economic Literature, 21*(2), 481–517.
McIntosh, R. (2006). *Challenge to Democracy*. London: Politico's.
Meade, J. E. (1951). *The Theory of International Economic Policy, Vol 1: The Balance of Payments*. London: Oxford University Press.
Meade, J. E. (1955). The Case for Flexible Exchange Rates. *Three Banks Review, 27*, 3–27.
Meigs, A. J. (1962). *Free Reserves and the Money Supply*. Chicago: University of Chicago Press.
Meiselman, D. I. (Ed.). (1970). *Varieties of Monetary Experience*. Chicago: Chicago University Press.
Melitz, J. (1965). Friedman and Machlup on the Significance of Testing Economic Assumptions. *Journal of Political Economy, 73*(1), 37–60.
Melitz, J., & Martin, G. (1971). Financial Intermediaries, Money Definition, and Monetary Control: Comment. *Journal of Money, Credit and Banking, 3*(3), 693–701.
Meltzer, A. H. (1963). The Demand for Money: The Evidence from the Time Series. *Journal of Political Economy, 71*(3), 219–246.
Meltzer, A. H. (1964). A Little More from the Time Series. *Journal of Political Economy, 72*(5), 504–508.
Meltzer, A. H. (1965). Monetary Theory and Monetary History. *Swiss Journal of Economics and Statistics, 101*(4), 404–422.
Meltzer, A. H. (2003). *A History of the Federal Reserve, Volume 1: 1913–1951.* Chicago: University of Chicago Press.
Middlemas, K. (2010). Margaret Thatcher. In V. Bogdanor (Ed.), *From New Jerusalem to New Labour: British Prime Ministers from Attlee to Blair* (pp. 144–165). Basingstoke: Palgrave Macmillan.
Miller, R. F. (1963). Review of Milton Friedman, *Price Theory: A Provisional Text*. *American Economic Review, 53*(3), 466–469.
Mill, J. S. (1859). *On Liberty*. London.
Mills, F. C. (1948). Living Costs, Prices, and Productivity. *Review of Economics and Statistics, 30*(1), 6–8.
Mincer, J. (1958). Investment in Human Capital and Personal Income Distribution. *Journal of Political Economy, 66*(4), 281–302.
Minsky, H. P. (1963). Comment on Friedman's and Schwartz' Money and Business Cycles. *Review of Economics and Statistics, 45*(1, Part 2), 64–72.

Mints, L. W. (1950). *Monetary Policy for a Competitive Society.* New York: McGraw-Hill.

Mishan, E. J. (1986, November). Fact, Faith, & Myth. *Encounter*, pp. 65–67, 70–77.

Mitch, D. (2016). A Year of Transition: Faculty Recruiting at Chicago in 1946. *Journal of Political Economy, 124*(6), 1714–1734.

Mitchell, W. C. (1913). *Business Cycles*. Berkeley: University of California Press.

Modigliani, F. (1949). Fluctuations in the Saving-Income Ratio: A Problem in Economic Forecasting. In *Studies in Income and Wealth* (pp. 369–444). New York: NBER.

Modigliani, F. (1964). Some Empirical Tests of Monetary Management and of Rules Versus Discretion. *Journal of Political Economy, 72*(3), 211–245.

Modigliani, F. (1977). The Monetarist Controversy or, Should We Forsake Stabilization Policies. *American Economic Review, 67*(2), 1–19.

Modigliani, F., & Ando, A. K. (1960). The 'Permanent Income' and the 'Life Cycle' Hypothesis of Saving Behavior: Comparison and Tests. In I. Friend & R. Jones (Eds.), *Proceedings of the Conference on Consumption and Savings* (pp. 49–174). Philadelphia: University of Pennsylvania Press.

Modigliani, F., & Ando, A. K. (1976). Impacts of Fiscal Actions on Aggregate Income and the Monetarist Controversy: Theory and Evidence. In J. L. Stein (Ed.), *Monetarism*. Amsterdam: North Holland.

Modigliani, F., & Brumberg, R. (1954/1979). Utility Analysis and the Consumption Function: An Attempt at Integration. In A. Abel (Ed.), *The Collected Papers of Franco Modigliani* (Vol. 2, pp. 128–197). Cambridge, MA: MIT Press.

Modigliani, F., & Brumberg, R. (1955). Utility Analysis and the Consumption Function: An Interpretation of Cross-Section Data. In K. K. Kurihara (Ed.), *Post-Keynesian Economics* (pp. 388–436). London: George Allen & Unwin.

Moggridge, D. E. (2008). *Harry Johnson: A Life in Economics*. Cambridge: Cambridge University Press.

Montes, L. (2015). *Friedman's Two Visits to Chile in Context*. http://jepson.richmond.edu/conferences/summer-institute/papers2015/LMontesSIPaper.pdf.

Moore, B. J. (1983). A Monument to Monetarism. *Journal of Post Keynesian Economics, 6*(1), 118–121.

Moore, B. J. (1988). *Horizontalists and Verticalists*. Boston: MIT Press.

Morley, F. (1958). *Essays on Individuality*. Philadelphia: University of Pennsylvania Press.

Morrison, G. (1966). *Liquidity Preference of Commercial Banks*. Chicago: Chicago University Press.

Morton, W. A. (1950). Trade Unionism, Full Employment, and Inflation. *American Economic Review, 40*(1), 13–39.

Morton, W. A. (1959). Wage-Push Inflation. In *Proceedings of the Eleventh Annual Meeting of the Industrial Relations Research Association* (pp. 183–195).

Mosteller, F. (1981). Memorial Service Tribute. In *The Writings of Leonard Jimmie Savage: A Memorial Selection* (pp. 25–28). Washington, DC: American Statistical Association and the Association of Mathematical Statistics.

Mundell, R. A. (1961). A Theory of Optimum Currency Areas. *American Economic Review, 51*(4), 657–665.

Murchison, W. (Ed.). (1978). *Tax Limitation, Inflation & the Role of Government*. Dallas: The Fisher Institute.

Musgrave, R. A., & Miller, M. H. (1948). Built-In Flexibility. *American Economic Review, 38*(1), 122–128.

Nader, R. (1965). *Unsafe at Any Speed*. New York: Grossman.

NBER. (1948). *Annual Report of the NBER: The Cumulation of Economic Knowledge*. New York: NBER.

NBER Committee on Price Determination. (1943). *Cost Behavior and Price Policy*. New York: NBER.

Neff, P. (1949). Professor Friedman's Proposal: A Comment. *The American Economic Review, 39*(5), 946–949.

Neild, R. (2014). The 1981 Statement by 364 Economists. In D. Needham & A. Hotson (Eds.), *Expansionary Fiscal Contraction* (pp. 1–9). Cambridge: Cambridge University Press.

Nelson, E. (2004). News–Magazine Monetarism. In P. Minford (Ed.), *Money Matters* (pp. 123–147). Cheltenham: Edward Elgar.

Nelson, E. (2009). *Milton Friedman and UK Economic Policy: 1938–1979* (Federal Reserve Bank of St. Louis Working Paper).

Nelson, E. (2012). Review of Ruger, Milton Friedman, and of Ebenstein, Milton Friedman. *Journal of Economic Literature, 50*(4), 1106–1109.

Nelson, E. (2016). Milton Friedman and the Federal Reserve Chairs in the 1970s. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy.* Oxford: Oxford University Press.

Nelson, E. (book draft). *Milton Friedman and Economic Debate in the United States, 1932–1972*. https://sites.google.com/site/edwardnelsonresearch/.

Nerlove, M. (1958). Review of Friedman, *A Theory of the Consumption Function*. *Journal of Farm Economics, 40*(1), 163–164.

Nichols, J. P. (1964). Review of Friedman and Schwartz, *A Monetary History of the United States, 1867–1960*. *Journal of American History, 51*(1), 101–103.

Nobay, A. R., & Johnson, H. G. (1977). Monetarism—Historic-Theoretic Perspective. *Journal of Economic Literature, 15*(2), 470–485.

Nordhaus, W. D. (1975). The Political Business Cycle. *Review of Economic Studies, 42*(2), 169–190.

Noyes, C. R. (1936). *The Institution of Property*. New York: Longmans, Green and Co.

Noyes, C. R. (1945). Director's Comment. In M. Friedman & S. Kuznets (Eds.), *Income from Independent Professional Practice* (pp. 405–410). New York: NBER.

Noyes, C. R. (1948). *Economic Man in Relation to His Natural Environment*. New York: Columbia University Press.

Nurkse, R. (1944). *International Currency Experience.* Geneva: League of Nations.

Nutter, G. W. (1951). *The Extent of Enterprise Monopoly in the United States, 1899–1939*. Chicago: Chicago University Press.

Oi, W. Y. (1967). The Costs and Implications of an All-Volunteer Force. In S. Tax (Ed.), *The Draft* (pp. 221–264). Chicago: University of Chicago.

Okun, A. M. (1963). Comment on Friedman's and Schwartz' Money and Business Cycles. *Review of Economics and Statistics, 45*(1, Part 2), 72–77.

Okun, A. M. (1970). *Political Economy of Prosperity*. Washington, DC: Brookings.

Oliver, H. M., Jr. (1953). Economoic Advice and Political Limitations. *Review of Economics and Statistics, 35*(3), 251–252.

Oliver, H. M., Jr. (1954). Review of Friedman, *Essays on Positive Economics*. *Ethics, 65*(1), 71–72.

Oppenheimer, P. M. (1970). Muddling Through: The Economy, 1951–1964. In V. Bogdanor & R. Skidelsky (Eds.), *The Age of Affluence 1951–1964*. London: Macmillan.

Oppenheimer, P. M. (1974, September 26). Review of Friedman, *Monetary Correction*. *The Listener*, p. 410.

Orcutt, G. H. (1958). Further Comments on, 'A Theory of the Consumption Function'. In L. Clark (Ed.), *Consumer Behavior: Research on Consumer Reactions* (pp. 459–462). New York: Harper and Brothers.

Page, A. N. (1968). *Utility Theory: A Book of Readings*. New York: Wiley.

Parkin, M. (1979). Review of Jerome L. Stein, *Monetarism*. *Journal of Political Economy, 87*(2), 432–436.
Parkin, M. (1986). Review of Patinkin, *Essays on and in the Chicago Tradition*. *Journal of Money, Credit and Banking, 18*(1), 104–116.
Parsons, W. (1989). *The Power of the Financial Press*. Aldershot: Edward Elgar.
Patinkin, D. (1948). Price Flexibility and Full Employment. *American Economic Review, 38*(4), 543–564.
Patinkin, D. (1956). *Money, Interest and Prices*. New York: Harper & Row.
Patinkin, D. (1956/1965). *Money, Interest and Prices* (2nd ed.). New York: Harper & Row.
Patinkin, D. (1965). An Indirect-Utility Approach to the Theory of Money, Assets, and Savings. In F. H. Hahn & F. P. R. Brechling (Eds.), *The Theory of Interest Rates* (pp. 52–79). London: Macmillan.
Patinkin, D. (1969). The Chicago Tradition: The Quantity Theory, and Friedman. *Journal of Money Credit and Banking, 1*(1), 46–70.
Patinkin, D. (1972). Keynesian Monetary Theory and the Cambridge School. In H. G. Johnson & A. R. Nobay (Eds.), *Issues in Monetary Economics* (pp. 3–30). Oxford: Oxford University Press.
Patinkin, D. (1973). Frank Knight as Teacher. *American Economic Review, 63*(5), 787–810.
Patinkin, D. (1974). Friedman on the Quantity Theory and Keynesian Economics. In R. J. Gordon (Ed.), *Milton Friedman's Monetary Framework* (pp. 111–131). Chicago: Chicago University Press.
Patinkin, D. (1979). Keynes and Chicago. *Journal of Law and Economics, 22*(2), 213–232.
Patinkin, D. (1981a). Introduction: Reminiscences of Chicago 1941–47. In *Essays in and on the Chicago Tradition.* Durham, NC: Duke University Press.
Patinkin, D. (1981b). *Essays on and in the Chicago Tradition*. Durham, NC: Duke University Press.
Patinkin, D. (1981c). Some Observations on the Inflationary Process. In M. J. Flanders & A. Razin (Eds.), *Developments in an Inflationary World*. New York: Academic Press.
Patinkin, D. (1986). A Review Essay: A Reply. *Journal of Money, Credit and Banking, 18*(1), 116–121.
Peltzman, S. (1974). *Regulation of Pharmaceutical Innovation*. Washington, DC: American Enterprise Institute for Public Policy Research.
Perlman, M. (1976). Jews and Contributions to Economics: A Bicentennial Review. *Judaism, 25*(3), 301–311.

Pesek, B. P., & Saving, T. R. (1967). *Money, Wealth and Economic Theory*. New York: Macmillan.
Pettengill, R. B. (1958). Review of Friedman, *A Theory of the Consumption Function*. *Southern Economic Journal, 24*(4), 491–492.
Phelps, E. S. (1967). Phillips Curves, Expectations of Inflation and Optimal Unemployment Over Time. *Economica, 34*(135), 254–281.
Phelps, E. S. (1968). Money Wage Dynamics and Labour Market Equilibrium. *Journal of Political Economy, 76*(4), 678–711.
Phelps, E. S. (1970). Money Wage Dynamics and the Labour Market Equilibrium. In E. S. Phelps (Ed.), *Microeconomic Foundations of Employment and Inflation Theory* (pp. 124–166). London: Macmillan.
Phillips, A. W. H. (1958). The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957. *Economica, 25*(100), 283–299.
Picker, R. C. (2011). The Razors-and-Blades Myth(s). *University of Chicago Law Review, 78*(1), 225–255.
Pigou, A. C. (1910). A Method of Determining Numerical Value of Elasticities of Demand. *Economic Journal, 20*(80), 636–640.
Pigou, A. C. (1936). Marginal Utility of Money and Elasticities of Demand. *Quarterly Journal of Economics, 50*(3), 532.
Pigou, A. C. (1943). The Classical Stationary State. *Economic Journal, 53*(212), 343–351.
Pigou, A. C. (1947). Economic Progress in a Stable Environment. *Economica, 14*(55), 180–188.
Pliatzky, L. (1982). *Getting and Spending*. Oxford: Basil Blackwell.
Poole, W., & Kornblith, E. (1973). The Friedman–Meiselman CMC Paper: New Evidence on an Old Controversy. *American Economic Review, 53*(5), 908–917.
Popper, K. R. (1934). *Logik der Forschung.* Vienna: Springer.
Popper, K. R. (1945). The Poverty of Historicism, III. *Economica, 12*(46), 69–89.
Popper, K. R. (1957). *The Poverty of Historicism*. London: Routledge.
Popper, K. R. (1959). *The Logic of Scientific Discovery*. London: Hutchinson & Co.
Popper, K. R. (1963). *Conjectures and Refutations*. London: Routledge & Kegan Paul.
Pringle, R. (2002). An Interview with Milton Friedman. *Central Banking, 1*, 15–23.

Rawls, J. (1971). *A Theory of Justice*. Cambridge, MA: Harvard University Press.

Rayack, E. (1987). *Not so Free to Choose*. New York: Praeger.

Read, L. E. (1958). I, Pencil: My Family Tree as Told to Leonard E. Read. *The Freeman, 8*(12), 32–37.

Reder, M. W. (1982). Chicago Economics: Permanence and Change. *Journal of Economic Literature, 20*(1), 1–38.

Rees, A. (1951). Postwar Wage Determination in the Basic Steel Industry. *American Economic Review, 41*(3), 389–404.

Reid, M. G. (1952). Effect of Income Concept upon Expenditure Curves of Farm Families. In *Studies in Income and Wealth* (Vol. XV). New York: National Bureau of Economic Research.

Reid, M. G. (1962). Consumption, Savings and Windfall Gains. *American Economic Review, 52*(4), 728–737.

Reid, M. G. (1963). Consumption, Savings and Windfall Gains: Reply. *American Economic Review, 53*(3), 444–445.

Rieber, M. (1964a). Collusion in the Auction Market for Treasury Bills. *Journal of Political Economy, 72*(5), 509–512.

Rieber, M. (1964b). Collusion in the Auction Market for Treasury Bills: Rejoinder. *Journal of Political Economy, 72*(5), 515.

Ritter, L. S. (1960). Review of Milton Friedman, *A Program for Monetary Stability*. *The American Economic Review, 50*(4), 765–768.

Ritter, L. S. (1963). The Role of Money in Keynesian Theory. In D. Carson (Ed.), *Banking and Monetary Studies* (pp. 134–150). Homewood, IL: Richard D. Irwin.

Rivot, S. (2012). The Great Divide? Keynes and Friedman on Employment Policy. *Cahiers d'eonomie politique, 62,* 223–251.

Rivot, S. (2015). Rule-Based Frameworks in Historical Perspective: Keynes' and Friedman's Monetary Policies *Versus* Contemporary Policy-Rules. *European Journal of the History of Economic Thought, 22*(4), 601–633.

Robbins, L. (1932/1984). *The Nature and Significance of Economic Science.* London: Macmillan.

Robbins, L. (Ed.). (1974). *Inflation: Causes, Consequences, Cures*. London: IEA.

Roberts, M. (1949, September 9). Letter to the Editor. *Dartford Chronicle.*

Robertson, D. H. (1954). Utility and All What? *Economic Journal, 64*(256), 665–678.

Robinson, N. (2012). *Live from Downing Street*. London: Bantam.

Rockoff, H. (2015). *Henry Simons and the Quantity Theory of Money*. Becker-Friedman Institute Working Paper, September 2015.
Rolfe, S. E. (1954). Manpower Allocation Under British Planning, 1945–49. *American Economic Review, 44*(3), 354–368.
Rose, S. (1974). The Agony of the Fed. *Fortune, 90*(1), 90–93, 180–190.
Rossett, C. (1984). Looking Back on Chile. *National Review, 36*(10), 25–53.
Rosten, L. (1966, November 15). An Infuriating Man. *Look Magazine*, p. 14.
Rottenberg, S. (1956). On Choice in Labor Markets. *International Labor Review, 9*(2), 183–199.
Rotwein, E. (1959). On 'The Methodology of Positive Economics'. *Quarterly Journal of Economics, 73*(4), 554–575.
Ruger, W. (2013). *Milton Friedman*. New York: Bloomsbury.
Ruggles, R. (1952). Methodological Developments. In B. F. Haley (Ed.), *A Survey of Contemporary Economics* (Vol. II, pp. 408–455). Homewood, IL: Richard D. Irwin.
Salant, W. S. (1942). The Inflationary Gap: Meaning and Significance for Policy Making. *American Economic Review, 32*(1), 308–314.
Samuelson, P. A. (1943). Full Employment After the War. In S. E. Harris (Ed.), *Postwar Economic Problems*. New York: McGraw Hill.
Samuelson, P. A. (1947/1965). *Foundations of Economic Analysis.* Harvard: Harvard University Press.
Samuelson, P. A. (1948). *Economics* (1st ed.). New York: McGraw-Hill.
Samuelson, P. A. (1951a). *Economics: An Introductory Analysis* (2nd ed.). New York: McGraw-Hill.
Samuelson, P. A. (1951b). Economic Theory and Wages. In D. M. Wright (Ed.), *The Impact of the Union* (pp. 312–343). New York: Harcourt Brace and Company.
Samuelson, P. A. (1955). *Economics* (3rd ed.). New York: McGraw Hill.
Samuelson, P. A. (1958). *Economics* (4th ed.). New York: McGraw Hill.
Samuelson, P. A. (1963). Problems of Methodology: Discussion. *American Economic Review, 53*(2), 231–236.
Samuelson, P. A. (1964). A. P. Lerner at Sixty. *Review of Economic Studies, 31*(3), 169–178.
Samuelson, P. A., & Solow, R. M. (1960). Analytical Aspects of Anti-inflation Policy. *American Economic Review, 50*(2), 177–194.
Samuelson, R. J. (1998, June 15). The Age of Friedman. *Newsweek*, p. 44.
Sandbrook, D. (2013). *Seasons in the Sun: The Battle for Britain, 1974–1979*. London: Penguin.

Sargent, J. R. (1952). Britain and the Sterling Area. In G. D. N. Worswick & P. H. Ady (Eds.), *The British Economy, 1945–1950*. Oxford: Clarendon Press.

Sargent, T. J. (1986). *Rational Expectations and Inflation*. New York: Harper and Row.

Sargent, T. J., & Wallace, N. (1975). 'Rational' Expectations, the Optimal Monetary Instrument, and the Optimal Money Supply Rule. *Journal of Political Economy, 83*(2), 241–254.

Savage, L. J. (1950). The Theory of Statistical Decision. *Journal of the American Statistical Association, 46*(253), 55–67.

Savage, L. J. (1954). *Foundations of Statistics*. New York: Wiley.

Savage, L. J. (1976). On Rereading R.A. Fisher. *Annals of Statistics, 4*(3), 441–500.

Scharnhorst, G. (1980). *Horatio Alger, Jr*. Boston: Twayne Publishers.

Schrock, N. W., & Smith, W. J. (1979). Keynes, the Keynesians, and Friedman: Three Views of Money in the Trap. *Journal of Post Keynesian Economics, 2*(1), 135–138.

Schuettinger, R. (1978). Wage-Price Control: The First 5000 Years. In C. G. F. Simkin (Ed.), *Wage-Price Controls: Myth and Reality*. Turramurra: The Centre for Independent Studies.

Schultz, H. (1933). A Comparison of Elasticities of Demand Obtained by Different Methods. *Econometrica, 1*(3), 274–308.

Schultz, H. (1938). *The Theory and Measurement of Demand*. Chicago: University of Chicago Press.

Schultz, T. W. (1961). Investment in Human Capital. *American Economic Review, 51*(1), 1–17.

Schultze, C. L. (1958). Review of Friedman, *A Theory of the Consumption Function*. *Science, 127*(3292), 243.

Schultze, C. L. (1959). Recent Inflation in the United States. In *Study of Employment, Growth and Price Levels, Joint Economic Committee, US Congress Sept 1959* (pp. 1–137). Washington, DC: Government Printing Office.

Schumpeter, J. A. (1943). *Capitalism, Socialism and Democracy*. London: George Allen & Unwin.

Schumpeter, J. A. (1954). *History of Economic Analysis*. New York: Oxford University Press.

Schwartz, A. J. (1975). Monetary Trends in the United States and the United Kingdom, 1878–1970: Selected Findings. *Journal of Economic History, 35*(1), 138–159.

Schwartz, A. J. (1981). Understanding 1929–1933. In K. Brunner (Ed.), *The Great Depression Revisited* (pp. 5–48). Boston: Martinus Nijhoff.

Schwartz, A. J. (1998, September 1). Review of Milton and Rose Friedman, *Two Lucky People, The Region*.

Scitovsky, T. (2008). Lerner, Abba Ptachya. In S. N. Durlauf & L. E. Blume (Eds.), *New Palgrave Dictionary of Economics*. Basingstoke: Palgrave Macmillan.

Selden, R. T. (1956). Monetary Velocity in the United States. In M. Friedman (Ed.), *Studies in the Quantity Theory of Money* (pp. 179–257). Chicago: University of Chicago.

Selden, R. T. (1959). Cost-Push Versus Demand-Pull Inflation. *Journal of Political Economy, 67*(1), 1–20.

Selden, R. T. (1962). Stable Monetary Growth. In L. B. Yeager (Ed.), In *Search of a Monetary Constitution* (pp. 322–356). Cambridge, MA: Harvard University Press.

Selden, R. T. (1975). *Capitalism and Freedom: Problems and Prospects.* Charlottesville, VA: University Press of Virginia.

Shapiro, E. (1978). *Macroeconomic Analysis* (4th ed.). New York: Harcourt Brace Jovanovich.

Shaw, E. S. (1950). *Money, Income and Monetary Policy*. Chicago: University of Chicago Press.

Shaw, E. S. (1958). Money Supply and Stable Economic Growth. In N. H. Jacoby (Ed.), *United States Monetary Policy* (pp. 49–72). New York: The American Assembly.

Sheahan, J. (1967). *The Wage-Price Guideposts*. Washington, DC: Brookings Institution.

Shonfield, A. A., Friedman, M., Neild, R., Worswick, G. D. N., & Oppenheimer, P. M. (1974). *Controversy: Inflation is Caused by Government and No One Else?* BBC 2, Broadcast 23 September. Transcript at the Hoover Institution Archieve, Milton Friedman Collection, Box 55, file 13.

Shoup, C., M. Friedman, & R. Mack (1943). *Taxing to Prevent Inflation: Techniques for Estimating Revenue Requirements.* New York: Columbia University Press.

Simon, H. A. (1963). Discussion. *American Economic Review, 53*(2), 229–231.

Simons, H. C. (1933/1994). Banking and Currency Reform. In *Research in the History of Economic Thought and Methodology* (Archival Supplement Volume 4). Greenwich, CT: JAI Press.

Simons, H. C. (1934). *A Positive Program for Laissez-Faire: Some Proposals for a Liberal Economic Policy.* Chicago: University of Chicago Press.

Simons, H. C. (1935). Review of Currie, *The Supply and Control of Money in the United States*. *Journal of Political Economy, 43*(4), 555–558.

Simons, H. C. (1936). Rules Versus Authorities in Monetary Policy. *Journal of Political Economy, 44*(1), 1–30.

Simons, H. C. (1948a). Introduction: A Political Credo. In H. Simons (Ed.), *Economic Policy for a Free Society* (pp. 1–39). Chicago: University of Chicago Press.

Simons, H. C. (1948b). *Economic Policy for a Free Society*. Chicago: University of Chicago.

Sinclair, D. (1976, September 13). Inflation: 'The Tax That Never Has to Be Passed by Parliament'. *The Times*, p. 7.

Singer, P. (1978). Rights and the Market. In J. Arthur & W. H. Shaw (Eds.), *Justice and Economic Distribution*. Englewood Cliffs, NJ: Prentice Hall.

Singleton, J. D. (2016). Slaves or Mercenaries? Milton Friedman and the Institution of the All-Volunteer Military. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Slichter, S. H. (1948). The Problem of Inflation. *Review of Economics and Statistics, 30*(1), 1–29.

Slobodian, Q. (2018). *Globalists*. Cambridge, MA: Harvard University Press.

Smithies, A. (1945). Forecasting Postwar Demand: I. *Econometrica, 13*(1), 1–14.

Snowdon, B., & Vane, H. R. (1999). Conversation with Milton Friedman. In B. Snowdon & H. R. Vane (Eds.), *Conversations with Leading Economists* (pp. 124–144). Cheltenham: Edward Elgar.

Snyder, C. (1924). New Measures in the Equation of Exchange. *American Economic Review, 14*(4), 699–7113.

Snyder, C. (1935). The Problem of Monetary and Economic Stability. *Quarterly Journal of Economics, 49*(2), 173–205.

Sohmen, E. (1961). *Flexible Exchange Rates*. Chicago: Chicago University Press.

Solomon, D. (1947). Review of Friedman and Kuznets, *Income from Independent Professional Practice*. *American Journal of Sociology, 55*(2), 216.

Solow, R. M. (1966a). The Case Against Guideposts. In G. P. Schultz & R. Z. Aliber (Eds.), *Guidelines, Informal Controls, and the Market Place* (pp. 41–54). Chicago: Chicago University Press.

Solow, R. M. (1966b). Comments. In G. P. Schultz & R. Z. Aliber (Eds.), *Guidelines, Informal Controls, and the Market Place* (pp. 62–65). Chicago: Chicago University Press.

Solow, R. M. (1984). Conversation Between Arjo Klamer and Robert Solow. In A. Klamer (Ed.), *The New Classical Macroeconomics: Conversations with New Classical Economists and Their Opponents* (pp. 127–148). Brighton: Wheatsheaf Books.

Stein, J. L. (1970). Review of Milton Friedman, *The Optimum Quantity of Money*. *Journal of Money, Credit and Banking, 2*(4), 397–419.

Stein, J. L. (1976a). Introduction: The Monetarist Critique of the New Economics. In J. L. Stein (Ed.), *Monetarism*. Amsterdam: North Holland.

Stein, J. L. (Ed.). (1976b). *Monetarism.* Amsterdam: North Holland.

Stein, J. L., & Tower, E. (1967). The Short-Run Stability of the Foreign Exchange Market. *Review of Economics and Statistics, 49*(2), 173–185.

Steindl, F. G. (2004). Friedman and Money in the 1930s. *History of Political Economy, 36*(3), 521–531.

Steindl, F. G. (1990). The 'Oral Tradition' at Chicago in the 1930s. *Journal of Political Economy, 98*(2), 430–432.

Stigler, G. J. (1945). Review of A. P. Lerner, *The Economics of Control*. *Political Science Quarterly, 60*(1), 113–115.

Stigler, G. J. (1949). *Five Lectures on Economic Problems*. New York: Longmans Green.

Stigler, G. J. (1950). The Development of Utility Theory II. *Journal of Political Economy, 58*(5), 373–396.

Stigler, G. J. (1954). The Early History of Empirical Studies of Consumer Behavior. *Journal of Political Economy, 62*(2), 95–113.

Stigler, G. J. (1988). *Memoirs of an Unregulated Economist.* Chicago: University of Chicago Press.

Tavlas, G. S. (1998). Was the Monetarist Tradition Invented? *Journal of Economic Perspectives, 12*(4), 211–222.

Tavlas, G. S. (2011). Two Who Called the Great Depression: An Initial Formulation of the Monetary-Origins View. *Journal of Money, Credit and Banking, 43*(2/3), 565–574.

Tavlas, G. S. (2019, forthcoming). The Intellectual Origins of the Monetarist Counter-Revolution Reconsidered: How Clark Warburton Influenced Milton Freidman's Monetary Thinking. *Oxford Economic Papers.*

Tax, S. (Ed.). (1967). *The Draft*. Chicago: University of Chicago.

Teixeira, P. N. (2007). Dr Smith and the Moderns. *Adam Smith Review, 3,* 139–157.

Temin, P. (1976). *Did Monetary Forces Cause the Great Depression?*. New York: W. W. Norton.

Temin, P. (1977). Money, Money Everywhere: A Retrospective Review. *Reviews in American History, 5*(2), 151–159.

Temin, P. (1981). Notes on the Causes of the Great Depression. In K. Brunner (Ed.), *The Great Depression Revisited* (pp. 108–124). Boston: Martinus Nijhoff.

Temin, P. (1983). Monetary Trends and Other Phenomena. *The Journal of Economic History, 43*(3), 729–739.

Temin, P. (1989). *Lessons from the Great Depression*. Cambridge, MA: MIT Press.

Thomas, J. J. (1989). The Early Econometric History of the Consumption Function. *Oxford Economic Papers, 41*(1), 131–149.

Thomas, L. B. (1973). Speculation in the Flexible Exchange Revisited—Another View. *Kyklos, 26*(1), 143–150.

Thomas, W. (1979). *The Philosophical Radicals: Nine Studies in Theory and Practice*. Oxford: Oxford University Press.

Thompson, H. S. (1972). *Fear and Loathing in Las Vegas*. London: Harper Perennial.

Thornton, M. (2016). Milton Friedman, Drug Legalization, and Public Policy. In R. A. Cord & D. Hammond (Eds.), *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.

Timlin, M. F. (1946). Review of Lange, *Price Flexibility and Employment*. *The Canadian Journal of Economics and Political Science/Revue canadienne d'Economique et de Science politique, 12*(2), 204–213.

Tinbergen, J. (1951). Reformulation of Current Business Cycle Theories as Refutable Hypotheses. In G. Haberler (Ed.), *Conference on Business Cycles* (pp. 131–141). Washington, DC: NBER.

Tobin, J. (1951). Relative Income, Absolute Income, and Savings. In *Money, Trade, and Economic Growth: Essays in Honour of John Henry Williams* (pp. 135–156). New York: Macmillan.

Tobin, J. (1956). The Interest-Elasticity of Transactions Demand for Cash. *Review of Economics and Statistics, 38*(3), 241–247.

Tobin, J. (1958). Discussion of Milton Friedman's, 'A Theory of the Consumption Function'. In L. Clark (Ed.), *Consumer Behaviour: Research on Consumer Reactions* (pp. 447–454). New York: Harper and Brothers.

Tobin, J. (1960). Towards Improving the Efficiency of the Monetary Mechanism. *Review of Economics and Statistics, 42*(3), 276–279.

Tobin, J. (1961). Money, Capital and Other Stores of Value. *American Economic Review, 51*(2), 26–37.

Tobin, J. (1963). Commercial Banks as Creators of Money. In D. Carson (Ed.), *Banking and Monetary Studies*. Homewood, IL: Richard D. Irwin.

Tobin, J. (1965). The Monetary Interpretation of History. *American Economic Review, 55*(3), 464–485.

Tobin, J. (1970a). Money and Income: Post Hoc Ergo Propter Hoc. *Quarterly Journal of Economics, 84*(2), 301–317.

Tobin, J. (1970b). Post Hoc Ergo Propter Hoc: Rejoinder. *Quarterly Journal of Economics, 84*(2), 301–317.

Tobin, J. (1974). Friedman's Theoretical Framework. In R. J. Gordon (Ed.), *Milton Friedman's Monetary Framework* (pp. 77–89). Chicago: Chicago University Press.

Tobin, J. (1976). Reply. In J. L. Stein (Ed.), *Monetarism* (pp. 332–336). Amsterdam: North Holland.

Tobin, J. (1981). The Monetarist Counter-Revolution Today—An Appraisal. *Economic Journal, 91*(361), 29–42.

Tobin, J. (1995). The Natural Rate as New Classical Macroeconomics. In R. B. Cross (Ed.), *The Natural Rate of Unemployment* (pp. 32–42). Cambridge: Cambridge University Press.

Tobin, J., & Buiter, W. (1976). Long-Run Effects of Fiscal and Monetary Policy on Aggregate Demand. In J. L. Stein (Ed.), *Monetarism*. Amsterdam: North Holland.

Tobin, J., & Ross, L. (1971). Living with Inflation. *New York Review of Books, 16*(8), 23–26.

Tobin, J., & Watts, H. W. (1960). Consumer Expenditures and the Capital Account. In I. Friend & R. Jones (Eds.), *Proceedings of the Conference on Consumption and Savings*. Philadelphia: University of Pennsylvania.

Toma, M. (1982). Inflationary Bias of the Federal Reserve System: A Bureaucratic Approach. *Journal of Monetary Economics, 10*(2), 163–190.

Triffin, R. (1940). *Monopolistic Competition and General Equilibrium Theory*. Cambridge: Harvard University Press.

Tsiang, S. C. (1969). A Critical Note on the Optimum Supply of Money. *Journal of Money Credit and Banking, 1,* 266–280.

Ulman, L. (1955). Marshall and Friedman on Union Strength. *Review of Economics and Statistics, 37*(4), 384–401.

Ulmer, M. (1974). Letter to the Editor: Counting Up GNP. *Challenge, 17*(1), 63–64.

Vaihinger, H. (1911/1924). *Die philosophie des als ob*. Leipzig: Reuther and Reichard.

Valone, J. J. (1982). Review of Milton and Rose Friedman, *Free to Choose: A Personal Statement*. *Business and Professional Ethics Journal, 1*(3), 107–112.
van Horn, R. (2009). Reinventing Monopoly and the Role of Corporations. In P. Mirowski & D. Plehwe (Eds.), *The Road from Mont Pèlerin* (pp. 204–237). Cambridge, MA: Harvard University Press.
Veblen, T. (1898). Why Is Economics Not an Evolutionary Science? *Quarterly Journal of Economics, 12*(4), 373–397.
Veblen, T. (1908). Fisher's Capital and Income. *Political Science Quarterly, 23*(1), 112–128.
Verwoerd, H. (1966a). The South African Government's Policy of Apartheid, December 5, 1950. In *Verwoerd Speaks.* Johannesburg: APB Publishers.
Verwoerd, H. (1966b). The Policy of Apartheid, September 3, 1948. In *Verwoerd Speaks: Speeches 1948–1966* (pp. 1–19). Johannesburg: APB Publishers.
Vickrey, W. S. (1947). Resource Distribution Patterns and the Classification of Families. *Studies in Income and Wealth, X,* 260–329.
von Neumann, J., & Morgenstern, O. (1944). *The Theory of Games and Economic Behavior.* Oxford: Oxford University Press.
de Vroey, M. (2009a). A Marshall-Walras Divide? A Critical Review of the Prevailing Viewpoints. *History of Political Economy, 41*(4), 709–736.
de Vroey, M. (2009b). On the Right Side for the Wrong Reason: Friedman on the Marshall-Walras Divide. In U. Maki (Ed.), *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (pp. 321–346). Cambridge: Cambridge University Press.
de Vroey, M. (2016). *A History of Macroeconomics from Keynes to Lucas and Beyond*. Cambridge: Cambridge University Press.
Wald, A. (1945). Sequential Tests of Statistical Hypotheses. *Annals of Mathematical Statistics, 16*(2), 117–186.
Wald, A. (1947). *Sequential Analysis*. New York: Wiley.
Walker, M. A. (1988). *Freedom, Democracy, and Economic Welfare: Proceedings of an International Symposium.* Vancouver: Fraser Institute.
Wallich, H. C. (1960). *The Cost of Freedom*. New York: Harper Brothers.
Wallis, W. A. (1942). How to Ration Consumer Goods and Control their Prices. *American Economic Review, 32*(3, Part 1), 501–512.
Wallis, W. A. (1950). Contribution of W. A. Wallis. *A Positive Program for Conservatives: A Symposium.* Chicago: University of Chicago. W. Allen Wallis Papers, Box 29, University of Rochester, River Campus Libraries.

Wallis, W. A. (1980). The Statistical Research Group, 1942–1945. *Journal of the American Statistical Association, 75*(370), 320–330.

Wallis, W. A., & Friedman, M. (1942). The Empirical Derivation of Indifference Functions. In O. Lange, F. MacIntryre, & T. O. Yntema (Eds.), *Studies in Mathematical Economics and Econometrics*. Chicago: Chicago University Press.

Walras, L. (1874/1954). *Elements of Pure Economics* (W. Jaffé, Trans.). London: Allen & Unwin.

Walters, A. A. (1987). Milton Friedman. In J. Eatwell, M. Milgate, & P. Newman (Eds.), *New Palgrave Dictionary of Economics.* London: Macmillan.

Warburton, C. (1943). Measuring the Inflationary Gap. *American Economic Review, 33*(2), 365–369.

Warburton, C. (1952). How Much Variation in the Quantity of Money Is Needed? *Southern Economic Journal, 18*(4), 495–509.

Warburton, C. (1953). Rules and Implements for Monetary Policy. *Journal of Finance, 8*(1), 1–21.

Warburton, C. (1963). Comment on Friedman's and Schwartz' Money and Business Cycles. *Review of Economics and Statistics, 45*(1 part 2), 77–78.

Wardell, W. M., & Lasagna, L. (Eds.). (1975). *Regulation and Drug Development*. Washington, DC: American Enterprise Institute for Public Policy Research.

Warren, R. B. (1942). *Financing the War.* Philadelphia: Tax Institute.

Weston, J. F. (1955). Toward Theories of Financial Policy. *Journal of Finance, 10*(2), 130–143.

White, L. H. (1984). *Free Banking in Britain: Theory, Experience, and Debate, 1800–1845*. Cambridge: Cambridge University Press.

Whittlesey, C. R. (1948). *Principles and Practices of Money and Banking*. New York: Macmillan.

Wicker, E. R. (1965). Federal Reserve Monetary Policy, 1922–33: A Reinterpretation. *Journal of Political Economy, 73*(4), 325–343.

Wicker, E. R. (1990). Review of William Frazer, *Power and Ideas: Milton Friedman and the Big U-Turn*. *Journal of American History, 76*(4), 1332–1333.

Williams, F. M., & Zimmerman, C. C. (1935). *Studies of Family Living in the United States and Other Countries.* Department of Agriculture, Miscellaneous Publication 223. Washington, DC: Government Printing Office.

Wilson, T. (1952). Manpower. In G. D. N. Worswick & P. H. Ady (Eds.), *The British Economy, 1945–1950*. Oxford: Clarendon Press.

Wilson, T. (1961). *Inflation*. Cambridge, MA: Harvard University Press.

Wilson, T. (1971). Discussion of Friedman, 'A Monetary Theory of Nominal Income'. In G. Clayton, J. C. Gilbert, & R. Sedgwick (Eds.), *Monetary Theory and Monetary Policy in the 1970s* (pp. 64–68). London: Oxford University Press.

Wilson, W. J. (1947). *The Beginnings of OPA*. Washington, DC: Office of Temporary Controls.

Wolf, C. (1961). Economic Aid Reconsidered. *Yale Review, 50*(4), 518–532.

Wolfe, A. B. (1936). The Theory of Optimum Population. *Annals of the American Academy of Political and Social Science, 188,* 243–249.

Wolfe, H. D. (1949). Review of H. A. Freeman, M. Friedman, F. Mosteller, and W. A. Wallis, *Sampling Inspection*. *Journal of Marketing, 13*(4), 576–577.

Wright, D. M. (Ed.). (1951). *The Impact of the Union.* New York: Harcourt, Brace and Company.

Yankovic, D. J. (1981). Review of Milton and Rose Friedman, *Free to Choose: A Personal Statement*. *Journal of Economic Literature, 19*(2), 568–570.

Author Index

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4

U

V

W

Y

Z

Subject Index

J. Forder, *Milton Friedman*, Great Thinkers in Economics,
https://doi.org/10.1057/978-1-137-38784-4