

THE LONG AND UNFINISHED ROAD TO FRIEDMAN AND MEISELMAN'S "THE RELATIVE STABILITY OF MONETARY VELOCITY AND THE INVESTMENT MULTIPLIER"

BY
GEORGE S. TAVLAS

Milton Friedman and David Meiselman's 1963 article, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958," was one of the most influential studies to come out of the Keynesian-monetarist debates of the 1960s and 1970s. The gestation of the article, however, is shrouded with considerable inaccuracy and ambiguity. I use archival materials to provide a more accurate chronological ordering of the gestation of the article than has hitherto been available. I show that the gestation was subject to considerable delays. I provide reasons that explain why a long-promised follow-up paper was never completed and why a book sequel to Friedman's 1956 Studies in the Quantity Theory of Money, planned as a co-edited work shortly after the appearance of the Friedman and Meiselman 1963 article, was not published until 1970 and was edited by Meiselman alone.

I. INTRODUCTION

Milton Friedman and David Meiselman's 1963 article, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1897–1958," was one of the most influential studies to come out of the Keynesian-monetarist debates

George S. Tavlas: Bank of Greece, and the Hoover Institution, Stanford University. I have benefitted from valuable comments by Pedro Duarte, the editor of this journal, and by Michael Belongia, Harris Dellas, Stephen G. Hall, Robert Hetzel, Peter Ireland, David Laidler, Ed Nelson, and two referees. Correspondence may be addressed to George Tavlas, Bank of Greece, 21 E Venizelos Ave, Athens, 10250, Greece, Tel. no. +30 210 320 2370; Fax. no. +30 210 320 2432; Email address: gtavlas@bankofgreece.gr. I thank Maria Monopoli for superb research assistance. I am grateful to David H. Sun and the staff of the Hoover Institution Library & Archives for their assistance.

ISSN 1053-8372 print; ISSN 1469-9656 online/24/02000201-224 © The Author(s), 2023. Published by Cambridge University Press on behalf of History of Economics Society.
doi:10.1017/S105383722300024X

of the 1960s and 1970s. Friedman and Meiselman's finding, based on simple regressions, that the money stock was highly correlated with consumption and in a stable way, whereas the variable representing autonomous expenditure was not, generated, as Edward Nelson (2020, vol. 2, p. 91) put it, "a sensation."¹ Phillip Cagan (1992, p. 720) noted that the Friedman and Meiselman results were viewed at the time of their publication as "dramatic" but "were not accepted by Keynesians, who argued that the Keynesian theory was not adequately represented by [simple regressions] and that econometric models of the entire economy, based on Keynesian theory, were superior to small-scale models." Numerous papers were published in which counter-results and affirmations were presented. The September 1965 issue of the *American Economic Review* included critical appraisals by Albert Ando and Franco Modigliani (1965) and Michael DePrano and Thomas Mayer (1965), along with a response by Friedman and Meiselman (1965).² The Federal Reserve Bank of St. Louis entered the debate with a paper by Leonall Andersen and Jerry Jordan (1968), which expanded the Friedman and Meiselman single-equation approach, adding dynamics and making the baseline specification multivariate rather than bivariate.³ Although the empirical methodology employed by Friedman and Meiselman would not, with the passage of time, stand up to developments in econometric estimation, Niels Thygesen was able to argue as late as 1977 that "in retrospect ... this [the Friedman–Meiselman study] is the single most influential study among Friedman's many publications" (Thygesen 1977, p. 75).⁴

The gestation of the Friedman and Meiselman paper, however, is shrouded in considerable ambiguity. Friedman and Meiselman (1963, p. 165) wrote that "work reported on this paper is the end product of a series of studies in the [University of Chicago's Department of Economics] Workshop in Money and Banking first started nearly a decade ago." The paper was published by the Commission on Money and Credit

¹ Meigs (1972, p. 19, quoted from Nelson 2020, vol. 2, p. 91) expressed the view that the Friedman and Meiselman article was "one of the most devastating critiques of the conventional Keynesian faith." More recently, Desai (2015, p. 156) stated that the paper "had dented, if not shaken, a pillar of the Keynesian edifice," while Chao (2019, p. 502) called the paper a "seminal ... work on monetarism." Earlier, Chao (2003, p. 77) had characterized the Friedman–Meiselman study as "ground-breaking."

² Edge (1967) provided a survey of the initial stages of the debate between Friedman and Meiselman and their critics.

³ The Andersen and Jordan specification became known as the "St. Louis equation." Silber (1971) expressed the view that the empirical findings in the debate often conformed to the political persuasions of the researchers. Silber's paper was aptly titled "The St. Louis Equation: 'Democratic' and 'Republican' Versions and Other Experiments." Bias (2014) provided a chronological ordering of many of the papers published in the debate, mainly from 1963 to the mid-1980s.

⁴ Thygesen's 1977 paper, "The Scientific Contributions of Milton Friedman," was published in the *Scandinavian Journal of Economics* in conjunction with Friedman's having been awarded the Nobel Prize in Economics in 1976. Nelson (2020, vol. 2, p. 91) correctly pointed out that citations of the Friedman–Meiselman paper in the recent literature have become infrequent. Exceptions include Walsh (2017, p. 12), who called the paper "[o]ne of the earliest times-series econometric attempts to estimate the impact of money"; McCallum (2016, pp. 56–57), who described the Friedman–Meiselman results as "striking" and added that "the Friedman–Meiselman study was highly influential in generating a reconsideration by the profession of the importance of monetary policy, with the outcome assigning a greatly enhanced role to the latter"; and Sims, who, in his Nobel Prize lecture (Sims 2012), gave the paper a prominent place in his description of the evolution of the profession's understanding of monetary policy. See also Auerbach (2016, pp. 418–419) and Lucas (2016, p. 15). Credit for the Thygesen (1977) and Sims (2012) sources cited in the present paper belongs to Nelson (2020, vol. 2, ch. 12).

(CMC) in a book titled *Stabilization Policies* (Commission on Money and Credit, 1963). The book consisted of seven independent research studies, each of which had been finalized by 1962. A foreword to the book by Bertrand Fox and Eli Shapiro, the director of research and the deputy director of research, respectively, at the CMC, was written in August 1962. The foreword makes clear that each of the studies appearing in the book had been finalized by that date. Friedman and Meiselman's remark that work on the paper had commenced almost a decade earlier would mark the start of the project around 1953 or 1954.⁵ However, in a 1995 interview of Meiselman conducted by Robert Hetzel, Meiselman stated that the project began in 1955. Meiselman recounted that he and Friedman "had clear results by 1958 but delayed publication until 1963 because of the time involved in checking the calculations" and because he (Meiselman) "had to estimate the regressions by hand" (Hetzel 1995, p. 13).⁶ Thygesen (1977, p. 74) offered a different endpoint for the project. He wrote that the paper "was finalized in 1960, almost three years prior to its publication."

In light of the fact that the Friedman and Meiselman paper caused a "sensation," how did that paper come about? Can a more accurate timeline for the gestation of the paper than the above account be constructed? In what follows, I provide a narrative of the origins of the Friedman–Meiselman project. I also provide a different—and, I believe, a more accurate—chronological ordering of the gestation of the Friedman and Meiselman paper. I use archival materials to show the following. (1) Clear results for the estimation period 1897 through 1953 had been produced by June 1956, at which time the results appeared as a solo Friedman presentation. (2) Between June 1956 and February 1960 *nothing* was done on re-estimation—at least with respect to re-estimation of results that were deemed to be presentable—although work on updating the data likely had commenced. (3) In a Chicago workshop version of the paper, presented in October 1959, Meiselman's name appeared as a co-author with Friedman. That paper's *narrative* was almost identical to that in Friedman's solo presentation of June 1956. The results *were* identical. (4) Preliminary results from the extended—1897 to 1958—sample period became available only in April/May 1960. (5) Between late summer 1960 and early summer 1961, another version of the paper was presented in a Chicago workshop; the likely date for the presentation was the fall of 1960. Thus, the major delay in completing the paper occurred from June 1956 to mid-1960. What caused that delay? It was not due entirely to what Meiselman called "the time involved in checking the calculations" and estimating the regressions. I provide evidence that suggests that two factors in the delay were the following. First, Meiselman apparently had a tendency to get sidetracked on his projects—at least, that was Friedman's view—and was also engaged in writing his PhD dissertation (completed in 1961). Second, Friedman had committed to work on multiple projects, leaving insufficient time for his work with Meiselman.

The remainder of this paper is structured as follows. To set the stage, [section II](#) provides an overview of the results in Friedman and Meiselman (1963). [Section III](#) reconstructs the chronology of the 1963 paper's gestation. [Section IV](#) provides reasons for what was a lack of progress on the paper from mid-1956 to mid-1960, and on both a follow-up paper to their 1963 article and a book sequel to the 1956 volume, *Studies in the*

⁵ Nelson (2020, vol. 2, p. 90) put the initiation of the project "around 1954."

⁶ Similarly, Chao (2019, p. 502) stated, "Friedman and Meiselman ... obtained empirical results in 1958."

Quantity Theory of Money, edited by Friedman (Friedman 1956b). The follow-up paper was never completed and the book sequel was not published until 1970 and was edited by Meiselman alone and not, as had been initially intended, by both Friedman and Meiselman. Section V discusses the legacy of the Friedman–Meiselman study. Section VI concludes.

II. THE 1963 STUDY

In their 1963 study, Friedman and Meiselman presented regression results for what they called “two alternative hypotheses” of income determination: (1) the Keynesian income-expenditure model, in which nominal income is determined by autonomous expenditures; and (2) the quantity theory model, in which nominal income is determined by the stock of money. The basic modeling strategy was to estimate and compare the following two equations (1963, p. 170):

$$Y = \alpha_0 + \alpha_1 M \quad (1)$$

$$Y = \beta_0 + \beta_1 A \quad (2)$$

The first equation expresses the level of nominal income, Y , as a linear function of the stock of money, M . The coefficient, α_1 , is the income velocity. The second equation expresses income as a linear function of nominal autonomous expenditures, A . The coefficient, β_1 , is the multiplier (1963, p. 170). The authors used personal consumption in place of nominal income as the dependent variable in most regressions because of potential statistical problems created by correlations between aggregate income and autonomous expenditures.⁷ The main issue studied was the relative in-sample forecast accuracy of consumption produced by the two hypotheses as measured by the unadjusted R^2 . Autonomous expenditures were defined as net private domestic investment plus the net government deficit on income and product account plus the net foreign balance. The primary measure of the money stock was currency outside banks plus adjusted demand deposits plus time deposits issued by commercial banks (i.e., M2).

The authors estimated simple regressions in which autonomous expenditures and money were, alternately, the independent variable, as in equations (1) and (2). As specified in levels, equation (1) violates homogeneity of degree one in prices so long as the constant term, α_0 , is not equal to zero. According to long-run money neutrality, a doubling of the money supply should lead to a doubling of nominal income (and nominal consumption). However, in equation (1), a doubling of the money supply cannot lead to a doubling of nominal income because of the constant term in the equation. To account for price homogeneity, equation (2) should have been specified in logs; if homogeneity held, α_1 should have been found to equal to unity.⁸

⁷ Specifically, the problem with using Y as the dependent variable is that A is a component of Y , which would amount to regressing a variable on part of itself. The authors presented several sets of results in which nominal income was regressed on the money stock. See their tables II-2 (part d), II-4 (part d), II-5, II-6 (part d), and II-7 (part d). Reflecting their concern with endogeneity, Friedman and Meiselman did not present comparable results for equations in which autonomous expenditures were the independent variable.

⁸ A referee pointed out that a constant term was included presumably to allow the R -squared to be interpreted normally in the regressions: with the constant term suppressed, R -squared would no longer have a standard

Friedman and Meiselman also estimated multiple regressions in which both autonomous expenditures and money were used as explanatory variables in the same equation. The variables (both levels and changes in the levels) were mainly expressed in nominal terms. To account for price changes, separate regressions were estimated in which a price index was added to equations.⁹ As expressed in the paper's title, the estimation period was 1897 to 1958. The authors estimated ordinary least squares (OLS) regressions for the models using annual data over the entire sample period and (sometimes overlapping) subperiods. They estimated regressions on quarterly data for the periods 1945:Q3 to 1958:Q4 and 1946:Q1 to 1958:Q4. They estimated equations using various lags of the explanatory variables. In total, they presented results—by my count—for 215 equations; numerous correlation coefficients between variables were also reported. The paper was 104 pages in length (including appendices); the main text (excluding and figures) was forty-three (single-spaced) pages in length.

The results indicated that money influenced consumption but that, apart from the 1930s, autonomous expenditures did not do so. The R^2 for the regression of consumption on money for the entire sample period (annual data) was .97; for the regression of consumption on autonomous expenditures, it was .57. The R^2 for the regression of consumption on money for the quarterly period 1945:Q3 to 1958:Q4 was .97; for the same period, the R^2 for the regression of consumption on autonomous expenditures was .26. In thirteen equations in which consumption was regressed on both money and autonomous expenditures over various periods using annual data, the coefficient on autonomous expenditures was negative in each case; in those regressions, the coefficient on money was positive and significant, ranging between .653 (1929 to 1939) to 2.110 (1948 to 1957). Excluding the 1929 to 1939 subperiod, the coefficient on M ranged from 1.029 to 2.110, which the authors interpreted to be stable.¹⁰

Friedman and Meiselman (1963, p. 166) summarized their findings as follows:

The results are strikingly one-sided. Except for the early years of the Great Depression, money ... is more closely related to consumption than is autonomous expenditures.... This is so both for nominal values, which is to say, when no adjustment is made for price change, and for 'real' values, which is to say, when the variables are adjusted for price change. It is true both for absolute values and for year-to-year or quarter-to-quarter changes. Such correction as there is between autonomous expenditures and consumption is in the main a disguised reflection of the common effect of money on both. So far as these data go, the widespread belief that the investment multiplier is stabler than the

interpretation. Friedman and Meiselman, however, did not explicitly make this point to justify the inclusion of the constant term.

⁹ The authors (1963, p. 178) argued that deflating all variables by the same price index to obtain real values would introduce common errors of measurement and, thus, spurious correlation into the regressions. Nevertheless, Friedman and Meiselman presented correlation coefficients between variables expressed in real terms. In a simple regression (that is, a regression with a single regressor), the square of the correlation coefficient is equal to the equation's R^2 .

¹⁰ Friedman and Meiselman did not provide a formal analysis of the stability of the coefficients. Their conclusion that the coefficients on M were stable was an informal assessment. In 1960, Gregory Chow published what became a famous article that contained a formal test of whether the coefficients in two different linear regressions on different data sets are equal. See Chow (1960). Chow was a participant in the Chicago workshops in the mid-1950s.

monetary velocity is an invalid generalization from the experience of three or four years [i.e., the years of the Great Depression]. It holds for neither later nor earlier years.

The authors concluded that the simple version of the income-expenditure model was “almost completely useless as a description of stable empirical relationships” (1963, p. 187).

Friedman and Meiselman indicated that the 1963 paper would not be the final product of their collaboration. They wrote: “similar analyses have also been made or are in the process of being made for other countries. The final version of this study will include the data from a wide variety of different countries” (1963, p. 170). As we shall see, results for a small group of other countries were available in 1956. As we shall also see, the follow-up “final version” of the Friedman–Meiselman project never materialized.¹¹

III. THE ROAD TO 1963

Friedman, 1956

In June 1956, Friedman presented a series of five lectures at Wabash College in Indiana. The lectures focused mainly on the advantages of a free market economy and the conduct of monetary policy. The lectures were transcribed and distributed.¹² Along with several other works by Friedman, they served as the basis for Friedman’s 1962 book, *Capitalism and Freedom*.

In a lecture titled “The Keynesian Revolution and Economic Liberalism,” Friedman compared the Keynesian income-expenditure theory to the quantity theory of money. The lecture consisted of two parts: the first part, which was five and a half pages in length (single-spaced), discussed the analytic structures of the Keynesian theory and the quantity theory. The second part, which was also five and a half pages in length, presented empirical evidence for the two theories. In discussing the empirical evidence, Friedman stated:

In a research group at the University of Chicago called the Workshop in Money and Banking, we have been making a fairly intensive comparison of the two theories for the United States from 1896 to 1953 [not from 1896 to 1958 as in Friedman and Meiselman (1963)] and for a number of foreign countries. Our procedure is limited to the two equations cited earlier [that is, the Keynesian income expenditure model under which income is determined by autonomous expenditure and the quantity theory under which income is determined by the money stock]. The aim is to see which equation fits historical experience during a particular series of years the better. (1956a, p. 7)

¹¹ Friedman and Meiselman (1964, 1965) collaborated in their replies to criticisms of their work by Hester (1964), and, as mentioned, Ando and Modigliani (1965) and DePrano and Mayer (1965). But those collaborations were not the promised “final version” of their project that was intended to include results from other countries.

¹² It is unclear whether the lectures were intended for wide distribution or that their transmission and editing were undertaken primarily with the aim of distribution. Friedman (1962, preface) wrote that his wife, Rose Director Friedman, had “translated the lectures into something more closely approaching written English,” indicating that he wanted transcriptions made for his own use. I am grateful to a referee for these observations. For a discussion of the contents of the lectures, see Tavlas (2023a, ch. 8).

In the above quotation, Friedman misstated the estimation period; it was, in fact, 1897 to 1953, and not 1896 to 1953. In a footnote, Friedman singled out ten individuals, one of whom was Meiselman, as having “participated with me in these studies” (1956a, p. 16n4).¹³ The names were not listed in alphabetical order. Meiselman was listed third, after Gary Becker and Phillip Cagan. However, Gregory Chow, who, as mentioned (in footnote 8), had attended the Chicago workshops in the mid-1950s, wrote the following: “Regressions were performed by Meiselman” (Chow 2016, p. 30). In all likelihood, Friedman did not initially intend to produce a publication from the results. Thus, he did not think it was important to single out Meiselman's work on estimation.

The connections between Friedman's 1956a presentation and the 1963 Friedman–Meiselman paper are clear-cut. In his 1956 lecture, Friedman provided the same empirical set-up as in the 1963 paper: equations (1) and (2) above were presented along with empirical results in which personal consumption was regressed on autonomous expenditures and the money stock in separate specifications. He explained that he used consumption instead of nominal income because of the correlation between aggregate income and autonomous expenditures. He defined autonomous expenditures as net domestic private investment plus the net government deficit plus the net foreign balance; he defined the money supply as currency in the hands of the public plus adjusted demand deposits plus time deposits in commercial banks (M2). The variables were expressed in nominal terms. To account for price changes, Friedman presented separate regressions in which a price index was added as a regressor. Equations were estimated for each model on annual data over the entire sample period and for seven subperiods, except for the subperiod 1946 to 1953, for which quarterly data were used. Equations were estimated using both levels of the variables and changes in the levels. As discussed in the previous section, each of these procedures and definitions was used in the 1963 study.

Friedman attached two tables to the presentation.¹⁴ Table 1 consisted of three parts. The first part presented the coefficient of determination (unadjusted R^2) and estimated coefficients for twenty-six regressions using nominal values of the variables. Regression results were reported using annual data for the entire sample period and for seven sometimes overlapping subperiods. Results were also reported for the quarterly period 1946:Q1 to 1953:Q4. The second part presented the corresponding results for twenty-six specifications in which the price level was included as a separate variable. The third part presented correlations between the variables expressed in real terms. Table 2 (not included in the present paper) presented results for Germany, Chile, and India. In his discussion, Friedman (1956a, p. 10) stated that results had also been obtained for Czechoslovakia and France, although results for the latter two countries were not presented.

What did the results show? First, for the period as a whole, the R^2 between consumption and money was .948; for the relationship between consumption and autonomous expenditures, the R^2 was .448. Second, results for various subperiods were similar. The R^2 for the relationship between consumption and money was stable among the periods; it ranged between .890 and .987. The relationship between consumption and

¹³ The others were Gary Becker, Phillip Cagan, Raymond Zelder, John Klein, Divurri Ramana, Boris Pesek, John Deaver, Roy Elliot, and Robert Snyder.

¹⁴ Nelson (2020, vol. 2, p. 373n132) previously pointed out that Friedman attached tables to his Wabash College lecture. Nelson, however, did not discuss the contents of the tables.

Table 1. Friedman (1956a) and Friedman–Meiselman (1963): Selected Regression Results

Part A: Annual Data					
Specification	$C = \alpha_0 + \alpha_1 M$		$C = \beta_0 + \beta_1 A$		Period
	α_1	R^2	β_1	R^2	
Friedman (1956a)	1.52	.986	1.70	.248	1897–1908
Friedman and Meiselman (1963)	1.64	.992	2.56	.587	1897–1908
Friedman (1956a)	1.88	.826	1.56	.856	1921–1933
Friedman and Meiselman (1963)	1.66	.804	1.38	.711	1921–1933
Friedman (1956a)	1.65	.848	2.30	.783	1929–1939
Friedman and Meiselman (1963)	1.53	.832	2.50	.878	1929–1939
Friedman (1956a)	1.19	.890	0.74	.043	1938–1953
Friedman and Meiselman (1963)	1.26	.918	1.86	.157	1938–1953
Friedman (1956a)	1.10	.948	2.77	.448	1897–1953
Friedman and Meiselman (1963)	1.32	.970	5.16	.571	1897–1958
Part B: Quarterly Data					
Specification	$C = \alpha_0 + \alpha_1 M$		$C = \beta_0 + \beta_1 A$		Period
	α_1	R^2	β_1	R^2	
Friedman (1956a)	2.63	.903	3.87	.356	1946:Q1–1953:Q4
Friedman and Meiselman (1963)	2.42	.970	3.70	.261	1946:Q3–1958:Q4
Part C: Annual Data					
Specification*	$C = \gamma_0 + \gamma_1 A + \gamma_2 M$			Period	
	γ_1	γ_2	R^2		
Friedman and Meiselman (1963)	–.425	1.38	.986	1897–1958	

Sources: Friedman (1956a) and Friedman and Meiselman (1963).

*Friedman (1956a) did not provide results for this specification.

autonomous expenditures ranged from .043 to .960, with the latter being by far the highest value obtained. Using language that closely resembled the description (given above) in his 1963 paper with Meiselman, Friedman called the results “striking, and surprisingly clear cut” in favor of the quantity theory (1956a, p. 9).

To provide an indication of the similarity between the 1956a Friedman results and the Friedman and Meiselman 1963 results, Table 1 of the present paper reproduces findings from selected estimation periods for equations (1) and (2) from both the 1956a and 1963 works. Part A of the table reports the coefficients on money and autonomous expenditures and the R^2 of the regressions for various common subperiods and for the entire estimation period (1897 to 1953 for the 1956a study; 1897 to 1958 for the 1963 study) using annual data. Part B of the table reproduces results for equations (1) and (2)

estimated on quarterly data (1946:Q1 to 1953:Q4 for the 1956a study; 1946:Q3 to 1958:Q4 for the 1963 study). Part C of the table reproduces the results from the 1963 study for a multiple regression with both money and autonomous expenditures used as regressors over the full estimation period. The basic message from the table is that the results changed very little in the case of specifications common to the 1956a and the 1963 studies. Of course, the 1963 study provided results for many more regressions than the 1956a study—215 regressions in the 1963 study versus 85 regressions (of which 57 regressions pertained to data for the United States) in the 1956a study. Moreover, the narrative in the 1963 study was richer and deeper than in the earlier study.¹⁵

Developments: October 1959 to December 1961

October 1959. For a period of about three and a half years following Friedman's June 1956a presentation at Wabash College, there was little progress on the quantity theory versus income-expenditure theory project. On October 27, 1959, however, a draft, titled "Judging the Predictive Abilities of the Quantity and Income-Expenditure Theories," was presented at the Chicago Workshop in Money and Banking. The paper appeared under the joint authorship of Friedman and Meiselman.¹⁶ It included the following introductory footnote: "Sections of the paper dealing with the statistical results and their interpretation were written primarily by Milton Friedman. Sections of the paper describing the construction of the models were written primarily by David Meiselman" (Friedman and Meiselman 1959, p. 1n1).

The text of the paper was ten (double-spaced) pages in length. The paper included an additional eleven pages of tables and data appendices. More than half of the ten pages of text were an exact duplication of parts of Friedman's 1956a Wabash lecture.¹⁷

As with the Wabash lecture, the 1959 paper included two tables: Table 1 consisted of three parts. The first part presented results of twenty-six regressions comparing the performances of the quantity theory and the income-expenditure theories; the second part added a price index to the twenty-six regressions; the third part reported regressions in which the variables were entered in real terms. Table 2 reported results for foreign countries. Each of the three parts of Table 1 was identical to that presented in Table 1 of the Wabash lecture. Table 2 of the 1959 paper was slightly different from the corresponding 1956a version. In addition to reporting results for Germany, Chile, and India—those regression results were the same as in the 1956a paper—the 1959 paper also included regression results for France.¹⁸

Meiselman had begun working on the project as a research assistant in the mid-1950s. In an interview conducted with Ed Nelson on April 30, 2013, Meiselman stated that he had been offered a job by Friedman around 1954 to work on the project for twenty hours a week (Nelson 2020, vol. 2, pp. 90–91). As mentioned, however, Friedman's acknowledgments in his 1956 presentation, in which he listed Meiselman's name as one among ten names, suggests that Friedman may not have intended to produce a paper based on the

¹⁵ As discussed below, the 1963 study included a description of the monetary transmission mechanism. The 1956 study did not do so.

¹⁶ This manuscript was previously cited by Nelson (2020, vol. 2, p. 91).

¹⁷ Approximately six and a half pages of the 1959 paper reproduced parts of the Wabash lecture.

¹⁸ Results for eight regressions using French data over four subperiods during 1920 to 1954 were reported.

results, in which case he did not view it as important to single out Meiselman. It may also have reflected the fact that, at that stage, the project was a multi-country study in which individual students had an input because they provided data on particular countries on which they had been working in their dissertations. As Friedman's challenge to the Keynesian orthodoxy gathered momentum in the second half of the 1950s, Friedman may have viewed the results generated in that project as increasingly relevant in that challenge. Therefore, it is likely that he wanted to produce a paper containing updated results while giving credit to Meiselman as a co-author.

If this was Friedman's intention, there were several problems of implementation. First, in the late 1950s, Meiselman apparently was preoccupied with writing his PhD dissertation. Second, Meiselman may have had a tendency to get sidetracked on his work. Third, Friedman had taken on multiple projects, such that his ability to focus on the project with Meiselman was, at least in part, compromised. I return to these issues below.

December 1959. On December 1, 1959, Friedman wrote to Eli Shapiro, the deputy director of research at the Commission on Money and Credit. The purpose of the letter was to solicit financial support for the Friedman–Meiselman project. Friedman wrote: "I am writing to ask whether the Commission on Money and Credit might be interested in financing the completion of a research project that we began some years ago in the Workshop in Money and Banking at the University of Chicago but have so far not had the resources to carry through."¹⁹ In his letter, Friedman explained the substance of the project, and he attached the October 1959 workshop paper to the letter. In seeking financial support, Friedman described the remaining work needed to complete the project: (1) incorporation of revised data for the existing (1897 to 1953) sample period; (2) extension of the sample period; and (3) extension of the empirical approach to additional countries, including Canada, Sweden, and Great Britain (Friedman, 1959b).

In his letter, Friedman stated that the goal would be to produce "a book that could become a second volume in the publications of the Workshop in Money and Banking following after *Studies in the Quantity Theory of Money*." Friedman wrote that he would work on the project with a "Research Associate (currently David Meiselman)."

The Commission approved funding for the project but the amount provided was below the sum that had been sought by Friedman. Friedman asked for \$19,000 per year for two years. The Commission agreed to \$12,000 for only a single year. In a February 10, 1960, letter to University of Chicago Chancellor Lawrence Kimpton, in which the Commission agreed to fund the project, Bertrand Fox, the Commission's research director, referred to the follow-up project. Fox wrote: "It is also understood that publication [of the Friedman–Meiselman study] will not preclude separate publication by the authors as a part of the broader study which is in progress" (Fox, 1960).

Friedman and Meiselman, February 1960. Friedman delivered a lecture on the research project at Harvard University on February 17, 1960. What survives from that lecture are three handwritten pages of text and a single handwritten table, both by Friedman. The text is titled, "Talk at Harvard, Feb. 17, 1960: The Comparative Stability of Velocity & the Multiplier." The text describes the income-expenditure and quantity theory frameworks. The text includes—in Friedman's handwriting—equations (1) and

¹⁹ Milton Friedman's correspondence referred to in this paper is contained in the Milton Friedman Papers at the Hoover Institution Library & Archives, Stanford University.

(2) above, plus extensions of those equations that include the price level as an additional variable. The table reports the R^2 for forty-six regressions for the United States and the R^2 for forty-two regressions for Germany, Chile, India, and France—a combined total of eighty-eight regressions. The table does not provide the estimated coefficients on the independent variables. The estimation period for the U.S. data is given as 1897 to 1953, indicating that re-estimation on the basis of the extended 1897 to 1958 sample period had not yet been made. Although the table does not provide estimates of the coefficients, the results are identical to those given in Friedman's 1956a Wabash presentation and in the October 1959 workshop paper. This circumstance is evident from the reported values of R^2 . For all of the regressions, the R^2 are identical to those reported for the corresponding regressions in June 1956 and October 1959.

I conclude that, from June 1956 to February 1960, the results from the monetary velocity-investment study that were presented in 1956 had not changed in the subsequent several years. Work appeared to have been underway, however, in revising and updating the data.

April/May 1960. Friedman and Meiselman presented new regressions at another Chicago workshop on May 6, 1960. Four tables and a two-page (typed) text exist from the presentation. All of the empirical results pertain to extended, quarterly sample periods: 1946:Q1 to 1958:Q4 and 1951:Q1 to 1958:Q4. The regressions used mainly personal disposable income as the dependent variable; that variable was regressed on various lags of the levels and first-differences of several measures of money. Judging from the results in the tables, the Friedman–Meiselman project was work in progress. The two-page text confirmed that work on the project was ongoing; it stated that Friedman and Meiselman were experimenting with alternate definitions of variables, and that “the quantity theory calculations are moving through the calculation sequence” (Friedman and Meiselman 1960, p. 2).

Late 1960 and 1961. Information relating to the remaining steps in the gestation of the Friedman and Meiselman paper includes the following.

- Between late summer 1960 and early summer 1961, a version of the paper was presented at another Chicago workshop.²⁰ Harry Johnson, who attended the workshop, made the important point that the paper lacked an account of the monetary transmission mechanism; without such an account, Johnson did not think that the paper's findings would be persuasive.
- In the May 1961 issue of the *American Economic Review's* Papers and Proceedings, workshop participant Boris Pesek referred to the Friedman and Meiselman study as: “A preliminary report to the Commission on Money and Credit, 1960” (Pesek 1961a, p. 91n3). Pesek's use of the word “preliminary” suggests that he understood that the version to which he had access was being revised.
- In the summer of 1961, Harry Johnson received the Friedman and Meiselman study, along with other CMC studies, from the CMC.²¹ Johnson was writing a survey paper, “Monetary Theory and Policy,” which was published in the June 1962 issue of the

²⁰ This information was provided to me in personal correspondence from David Laidler, who attended the workshop in question. David thought that the workshop might have taken place in the fall of 1960 but added that he could not be sure.

²¹ Again, this information was provided in personal correspondence from David Laidler. David was employed by Johnson to read and abstract the CMC papers for Johnson in the summer of 1961. David

American Economic Review. Johnson (1962, p. 68) referred to the Friedman and Meiselman study as “forthcoming.” He called it a “major study.”²²

- In a letter dated December 1, 1961, addressed to Bertrand Fox, the director of research at the Commission, Friedman and Meiselman co-signed a letter that stated: “Unfortunately, we have been delayed in completing the revisions of our paper, ‘The Relative Stability of Money Velocity and the Investment Multiplier in the U.S., 1897–1958,’ for publication in the series of studies sponsored by the Commission. We shall, however, try to have the revised manuscript in your hands by December 15 and trust that this short delay will not inconvenience you or the members of the staff” (Friedman and Meiselman, 1961).
- The published version of the Friedman and Meiselman paper included a (five-page) section that described the monetary transmission mechanism in detail. In light of Johnson’s (1962) characterization of the paper as a “major study,” and in view of his earlier workshop remarks that the paper would not be believable without a discussion of monetary transmission, what Johnson received in the summer of 1961 most likely was the final version as far as empirical results and main arguments are concerned.

When, then, was the version that contained the main body of text, including the section on the monetary transmission mechanism, and the finalized empirical results, completed? From the above discussion, it is clear that the paper was not completed in April/May 1960 since the brief text available from the May 6, 1960, workshop stated that “the quantity theory calculations are moving through the calculation sequence.” We have seen that another workshop presentation of the Friedman and Meiselman project took place between the late summer of 1960 and the early summer of 1961. At that presentation, Johnson argued that the paper’s results would not be convincing without a discussion of an account of the mechanisms through which money affects the economy. That presentation likely occurred in the fall of 1960, but it might have been later. We also have seen that, in a May 1961 paper, Pesek referred to the Friedman and Meiselman report as “a preliminary report,” suggesting that Pesek had not had access to the finalized study.²³ And, in his 1962 survey paper, Johnson, who had thought that the late 1960/early 1961 workshop version was not convincing, called the finalized paper “a major study.” Johnson had received that version in the early summer of 1961. Based on the foregoing factors, I conjecture that the “finalized” version—that is, the version with the crucial section on monetary transmission—was finalized in early 1961, although this estimate contains a wide confidence interval.²⁴

IV. WHY THE DELAY?

The above discussion has documented the following. First, initial results for what became the 1963 Friedman–Meiselman study had been produced in June 1956 under

believes that Johnson likely had read the revised version of the paper before the bundle of CMC papers arrived in the summer of 1961. Johnson might have received the revised version directly from Friedman.

²² In his 1962 survey, Johnson also referred to the forthcoming CMC paper, “Lags in Fiscal and Monetary Policy,” co-authored by Brown, Solow, Ando, and Kareken (1963).

²³ However, the fact that the draft was designated as “preliminary” does not exclude the possibility that the draft might have been the finalized version.

²⁴ This is also the view of David Laidler.

Friedman's name alone. In his 1956a presentation, Friedman acknowledged the contributions of ten participants in the Chicago workshop, one of whom was Meiselman, who had been hired by Friedman to do empirical work for the study. The estimation period for the regressions with U.S. annual data was 1897 to 1953. The estimation period for the regression with U.S. quarterly data was 1946:Q1 to 1953:Q4. Friedman also presented empirical results for Chile, Germany, and India. In light of the advanced stage of the regression results and the data requirements needed to perform the regressions, work on the project likely commenced in 1953 or in 1954.²⁵ Second, very little—if any—work on the project took place from June 1956 to late 1959. At some point during that period, Meiselman had become listed as an equal collaborator with Friedman on the project, suggesting that the project had been restarted with the aim of producing a publication. A Chicago workshop presentation by Friedman and Meiselman, using the June 1956 results, had been made in October 1959. Friedman had presented identical results at Harvard in February 1960. Third, new, preliminary regression results, with an extended estimation period, had been presented at still-another Chicago workshop in May 1960. The documentation associated with the latter presentation stated that work on the project was ongoing. The project was completed in December 1961.

Reasons for the Delay

What caused the four-year break (from June 1956 to May 1960) in moving forward on the project? In his December 1959 letter to Shapiro, Friedman attributed the delay to a lack of “resources to carry through” the project. Whatever the merit of that attribution, it likely was not the only reason.

Work overload. Meiselman began graduate studies at Chicago in the late 1940s.²⁶ He completed his PhD at that institution in 1961. His thesis, titled *The Term Structure of Interest Rates*, was written under Friedman's supervision. The thesis integrated evidence from cash markets and future markets into a unified theory that showed the way interest rates behave over time through the term structure. Meiselman's thesis, which won the Ford Foundation Prize for the best dissertation in economics in 1961, became a classic in the area of term-structure theory.²⁷ It was published as a book in 1962.²⁸

What is relevant for the present study is that Meiselman had been asked to update and expand the regressions for the project with Friedman during a period, 1959 to 1961, in which he was completing his PhD dissertation. The fact that the dissertation became a classic is an indication that it absorbed a large amount of time and effort. There is also the

²⁵ This conjecture is consistent with Nelson's (2020, vol. 2, p. 90) view that the project commenced “around 1954.”

²⁶ Meiselman's biographical entry in the 1974 American Economic Association Directory of Members states that he completed a Bachelor of Arts degree at Boston University in 1947 and a Master of Arts at Chicago in 1951.

²⁷ According to the expectations hypothesis of the term structure of interest rates, the graphical representation of which is the yield curve, the long-term interest rate is determined by current and future expected short-term rates in a way in which the expected final value of wealth from investing in a sequence of short-term bonds equals the final value of wealth from investing in long-term bonds. In a review of the term-structure literature, Kessel (1965, p. 12) wrote: “Meiselman ... was able to make the expectations hypothesis operational even when the market could not anticipate future rates of interest correctly. He showed that expectations, whether or not they are correct, nevertheless affect the term structure of interest rates.”

²⁸ See Meiselman (1962).

matter that the regression estimations had to be done by hand, which itself was time-consuming. In addition, Meiselman's CV lists him as an assistant professor at Chicago during the years 1958 to 1962, indicating that he had teaching responsibilities while working on both his dissertation and the project with Friedman.

Follow-through on assignments. In 1958 and 1959, Meiselman began applying for positions outside of the University of Chicago, using Friedman as a reference.²⁹ Friedman wrote several recommendation letters. The letters indicated that Friedman thought highly of Meiselman's intellectual capacity but also thought that Meiselman had a tendency to spread himself too thin. For example, in a March 1958 letter to Ernest T. Baughman of the Chicago Fed, Friedman wrote that Meiselman "tends to rush off in a number of different directions" (Friedman, 1958a). In a June 1958 letter to Homer Jones of the St. Louis Fed, Friedman wrote that Meiselman "has "something of a tendency to go off tangents" (Friedman, 1958b).³⁰ In an April 1959 letter to Henry H. Villard, an economist at the City College of New York, Friedman said: "My one doubt with respect to Meiselman is that something of a tendency on his part to spread himself too thin in too many directions."³¹

Friedman's workload. During the years in which he worked on the project with Meiselman, Friedman completed three highly influential books: *A Program for Monetary Stability* (Friedman, 1960), *Capitalism and Freedom* (Friedman, 1962), and *A Monetary History of the United States, 1867–1960*, co-authored with Anna Schwartz (Friedman and Schwartz, 1963).³² In addition to those books, in the late 1950s and the early 1960s, Friedman published numerous articles and commentaries in academic journals and other publication outlets. Friedman's multiple projects may have left him with insufficient time for what was supposed to be joint work with Meiselman.

Follow-Up to Studies

As documented, in his December 1959 letter to Eli Shapiro, in which Friedman sought funding for his project with Meiselman, Friedman wrote that the goal was to produce "a second volume of the publication following after *Studies in the Quantity Theory of Money*." The *Studies* volume, published in 1956b, consisted of four essays by former Friedman students based on their PhD dissertations, plus an introductory essay by Friedman: the now-classic "The Quantity Theory of Money: A Restatement."³³ The follow-up volume to *Studies* was to be co-edited by Friedman and Meiselman and was to include an updated version of the 1963 Friedman and Meiselman 1963 project. Like the

²⁹ Meiselman stated: "I was pretty much told that at least for the foreseeable future, they didn't really envisage that I would probably get any tenure there [at Chicago]" (Hetzel 1995, p. 1).

³⁰ Jones had been Friedman's undergraduate teacher at Rutgers University in the late 1920s.

³¹ Specific information on dates of the three above letters is listed in the References to this paper. These letters are contained in the Milton Friedman Papers at the Hoover Institution Library & Archives at Stanford University.

³² In describing the publication of the latter two books in 1962 and 1963, respectively, Nelson (2020, vol. 2, p. 2) wrote that "an avalanche of Friedman's writings reached print—over one thousand pages in total." The book, *A Program for Monetary Stability*, was based on lectures Friedman delivered at Fordham University in 1959.

³³ See Friedman (1956c). The authors of the other essays were Phillip Cagan, John Klein, Eugene Lerner, and Richard Selden.

1956b book, *Studies*, the follow-up volume would be comprised of essays written by Chicago workshop participants. Based on archival materials, I have identified four workshop participants who had agreed to contribute papers to the follow-up volume: John Deaver, Boris Pesek, Oktay Yenel, and George Macesich.³⁴

At some point in the early 1960s, Friedman had put Meiselman in charge of following through with each of those who had agreed to contribute a paper for the volume. This circumstance is evident in a letter from Pesek to Friedman, dated September 6, 1961. Pesek wrote: "David [Meiselman] just wrote me about the second volume of *Studies* which you plan to publish. I shall try to write a rough [sic] draft as soon as possible for his comments" (Pesek, 1961b).

More than two years later, in late 1963, Macesich wrote to Friedman, asking about the status of the volume. On November 27, 1963, Friedman, in turn, wrote to Meiselman, who, at the time, was visiting Johns Hopkins University.³⁵ Friedman wrote: "A letter from George Macesich reminds me of something I meant to talk to you about.... He writes to ask 'what the status is of that book of essays on the quantity of theory which you and Dave were going to edit.' I write to ask the same question, what has happened?" (Letter from Milton Friedman to David Meiselman, November 27, 1963).

Meiselman responded with a letter dated December 11, 1963. The response made it clear that Meiselman had not followed through in contacting those who had agreed to contribute to the new volume. Meiselman wrote that he had been preoccupied with the completion of the 1963 paper for the Commission, which "I had been working on these many years," and had performed "a set of additional calculations on the same series that appeared in the CMC [i.e., the Commission] paper." As to the follow-up volume, Meiselman wrote: "The volume has more or less been in limbo for the past year, and I would like very much to get back to work on it" (Letter from David Meiselman to Milton Friedman, December 11, 1963).

In a letter dated, January 23, 1964, Friedman wrote the following to Meiselman.

I am rather distressed by your reply on the main point of the papers for the [new] volume.... The reason I am distressed is because it is clear from your letter that everything is in a chaotic state except only for the manuscript by George Macesich. We have, I believe, a strong obligation to him and to other people whom we asked to participate in this to get a manuscript [that is, the follow-up to the 1963 Friedman–Meiselman paper] out in the very near future (Friedman, 1964).

Friedman continued: "I think we really must either fish or cut bait." He expressed the view that the first order of business was to complete his updated paper with Meiselman "by the end of this summer, or else we must write letters to all of the people whom we have been in correspondence with and tell them that we are very sorry but we shall be

³⁴ Deaver, Macesich, and Pesek completed their PhD theses under Friedman's supervision in the late 1950s. Deaver's dissertation was titled "The Chilean Inflation and the Demand for Money." Macesich's dissertation was titled "Monetary Disturbances in the United States 1834–45." Pesek's thesis was titled "Monetary Policy of Czechoslovakia, 1945–53." As mentioned above, Pesek was among the ten workshop participants whom Friedman mentioned as having contributed to the results that he (Friedman) presented at his 1956a Wabash College presentation. Yenel earned his PhD at Istanbul University in 1957. He was a Post-Doctoral Fellow at Chicago during the 1959–60 academic year. Friedman and Meiselman expected him to produce a study on the Turkish economy for their planned volume.

³⁵ Meiselman's bio lists his position as: Visiting Faculty, Johns Hopkins, 1963–1964.

unable to go through with this project and we shall have to apologize to them for having imposed extra work on them” (Friedman, 1964).³⁶

The follow-up to the 1963 Friedman–Meiselman study was never completed. Moreover, Friedman “cut bait,” and bowed out of the project that aimed to produce a successor to the 1956b volume, *Studies in the Quantity Theory of Money*. As mentioned, in both Friedman’s 1959 letter to the Commission’s Eli Shapiro and in the Friedman–Meiselman study itself (1965, p. 170), Friedman wrote that he intended to produce a follow-up volume to *Studies* that would extend empirical applications to countries other than the United States. However, Friedman’s aspiration to directly contribute to international applications of the quantity theory waned after 1963.³⁷ Meiselman, however, continued to work on the successor to *Studies*. The result was the 1970 book *Varieties of Monetary Experience*, which Meiselman edited alone. The book consisted of six empirical papers by former workshop participants. Meiselman did not contribute a paper to the volume—which would have been the long-intended follow-up to the 1963 Friedman and Meiselman study—but he did write a four-page introduction in which he stated: “The six studies in the present volume ... continue the tradition of *Studies in the Quantity Theory of Money* both in subject matter and in general research strategy. All are empirical analyses of the role of monetary behavior in shaping economic events” (Meiselman 1970, p. 2). After briefly describing the contents of the six studies, Meiselman concluded his introduction as follows:

Finally, for his generous help, all of the authors owe a special debt to Milton Friedman, who founded the Workshop in Money and Banking at the nadir of the quantity theory and whose scholarship and zealous devotion to positive economics sparked the revival of concern for monetary phenomena and the development of the modern quantity theory. (Meiselman 1970, p. 6)

Why Did Friedman Pull Out?

Four of the six essays in *Varieties* studied the determinants of the demand for money in country-specific historical episodes: Colin Campbell for South Korea (from 1953 to 1961) and Brazil (from 1948 to 1965), John Deaver for Chile (from 1878 to 1955), Adolfo Cesar Diz for Argentina (from 1935 to 1962), and George Macesich for Canada (from 1926 to 1958). The remaining two essays were by Michael Keran, who formulated a simple model, in which income depends on money, to study the role of monetary policy in the business cycle in postwar Japan; and Morris Perlman, who provided an international cross-section study of the demand for money (drawing on data from forty-seven countries). Deaver and Macesich were part of the initial group that had been identified by

³⁶ The January 23, 1964, letter from Friedman to Meiselman was the last available written correspondence between the two economists to discuss their proposed follow-up study.

³⁷ During the 1962–63 academic year, Milton and Rose Friedman took a year-long trip around the globe (to twenty-one countries). One of the purposes of the trip was to gather material so that Friedman’s ongoing research on the role of money in economic fluctuations in the United States could be extended to other countries (Friedman and Friedman 1998, p. 279). By the end of the trip, Friedman had moved on from the idea of working on such a study. Friedman wrote the following about the trip: “Although I never wrote the book that I intended to on the basis of the material I gathered in the countries I studied ... I did stimulate a good idea of work on monetary issues by other scholars” (Friedman and Friedman 1998, p. 332).

Friedman and Meiselman as likely contributors to their planned, co-edited sequel to the 1956b book, *Studies in the Quantity Theory of Money*.

Thus, although Meiselman apparently did not produce the estimations needed to provide the basis for a follow-up paper to the 1963 Friedman and Meiselman Commission study, he did deliver a sequel to the 1956b *Studies*. However, contrary to the original plans, Friedman did not contribute to the 1970 book *Varieties of Monetary Experience*, either as an author of an essay or as a co-editor.

Why did Friedman decide not to be involved in the *Varieties* projects? There are at least two reasons. First, Friedman had become heavily involved in other projects during the second half of the 1960s, including the much-delayed follow-up to his ongoing project with Anna Schwartz, published as the book *Monetary Statistics of the United States: Estimates, Sources, Methods* (Friedman and Schwartz, 1970); the editing of books composed of his essays —*Dollars and Deficits* (Friedman, 1968) and *The Optimum Quantity of Money, and Other Essays* (Friedman, 1969); and the presidency of the American Economic Association (in 1967).³⁸ Second, and related to the first point, by the later 1960s, Friedman had made a broader-based decision to leave it to others to conduct research on international case studies (other than with regard to the United Kingdom).³⁹

Why did Meiselman fail to deliver a solo-authored sequel to his 1963 paper with Friedman? The answer likely has to do with his multiple job changes after he left Chicago in 1962. His CV lists the following work history from 1962 to 1966: Economist, U.S. Treasury, 1962–63; Senior Economist, U.S. House of Representatives, 1963; Senior Economist, Inter-American Development Bank Fiscal Mission to Peru, 1964; Senior Economist, Office of the Comptroller of the Currency, 1964–66; Senior Consultant, International Bank for Reconstruction and Development, 1966. From 1966 to 1971, Meiselman held a full professor position at Macalester College. There were also visiting teaching positions: Johns Hopkins University, 1963–64, and the University of Minnesota, 1966–68(?). In 1971, Meiselman became professor of economics at the Virginia Polytechnic Institute (now Virginia Tech), a position he held until his retirement in 1997. Information that I received from several former Friedman PhD students suggest that Friedman expected his students to fend for themselves after they left Chicago. Meiselman's post-Chicago career seems to be a case in point.

Reviews of Varieties

In the event, the reviewers of *Varieties* noticed that the essays in the book were dated. Reviewing the book for the *Journal of Money, Credit and Banking*, David Laidler (1972,

³⁸ For a discussion of Friedman's professional activities in the second half of the 1960s, see Nelson (2020, vol. 2, chs. 12–14). Nelson (2020, vol. 2, p. 111) pointed out that, by early 1967, the NBER steering committee for the Friedman and Schwartz project recommended that the United Kingdom be added to the project so that it was broken “up into two separate books: one covering US monetary statistics and one covering monetary trends (and, it was hoped, also cycles) in the United Kingdom and the United States. The extra work involved, and Friedman's many other commitments, meant that neither book saw publication in the 1960s. Indeed, the book on monetary trends [i.e., Friedman and Schwartz 1982] did not even reach print during the 1970s.”

³⁹ As noted above in 34, Friedman's aspiration to contribute to international applications of the quantity theory waned after his 1962–63 global trip.

p. 121) noted that its research agenda was “very much the one that Milton Friedman laid down in the mid-1950s and thereafter pursued, along with a number of associates.” Laidler (1972, p. 122) then cut to the heart of the matter. He wrote:

Had these studies been published in, shall we say, 1966, they could have been welcomed as unquestionably constituting a major addition to the literature. If we judge this book as an attempt to extend our knowledge of what were perceived as the central problems of monetary economics in the early 1960s, using the techniques available in the early 1960s, then this is of the very first quality.

But, Laidler continued, “as a 1970 publication, it cannot be rated so highly.” Laidler went on to criticize the essays for their omissions in taking account of developments in such areas as the impacts of the capital mobility on the effectiveness of monetary policy and the effects of stock adjustment versus expectations lags on the demand for money. Laidler, who had been a graduate student at Chicago in the early 1960s, wrote as someone who had inside knowledge about the book’s gestation:

The reason for these deficiencies is straightforward. These essays have been subject to a long delay in publication. Some of the work reported here was well underway when this reviewer was a graduate student at Chicago in 1960–63, and there is abundant textual evidence that virtually all the substantive work for these studies was complete by 1966. Every serious reservation that I have about this volume stems from this delay. (Laidler 1972, pp. 122–123)

In concluding his review, Laidler (1972, p. 123) stated: “The Chicago Money Workshop has a well deserved reputation for freshness and originality; in this case the goods are somewhat stale from having been stored too long and that is a pity.”

Other reviewers provided similar appraisals, praising the Chicago workshop but criticizing the Meiselman-edited book for having been out of date. Reviewing the book for the *Journal of Finance*, Herschel Grossman (1971, p. 1012) suggested that the editor had been lethargic in his duties: “The editor’s introduction is brief and uncritical.” As for the essays, Grossman (1971, p. 1012) asserted:

Each of the studies reinforces the impressive record of the Workshop as a steady producer of sound economic research. I think that it is safe, and relevant, however, to say that individually, with the possible exception of Morris Perlman’s piece, none of the studies evidences sufficient novelty of method or results to meet the current refereeing standards of the leading professional journals. The essential problem with this volume is that.... Each of the studies ... employs economic theory and econometric methods which were developed in the Workshop during the fifties and early sixties.

A similar assessment was provided by Douglas Fisher (1972, p. 90) in a review for the *Journal of Economic Literature*: “The studies are somewhat dated by now.... The lag in the production of this volume is unfortunate for the contributors whose efforts, often as good as anything in print at the time their work was finished, must now be judged primarily useful to specialists on the various countries studied.”⁴⁰

⁴⁰ Fisher completed his PhD at Chicago in 1965.

V. THE AFTERMATH

The Friedman–Meiselman paper continued to be the occasional focus of empirical studies in the decades following its publication, although citations to the paper progressively decreased. In a 1973 article, “The Friedman–Meiselman CMC Paper: New Evidence on an Old Controversy,” William Poole and Elinda Kornblith examined the post-sample performances of the Friedman–Meiselman equations and those of the critics of Friedman–Meiselman.⁴¹ Poole and Kornblith (1973, p. 915) found that “neither the simple Keynesian theory nor the simple quantity theory models provide an adequate understanding of business cycle fluctuations ... over the 1959–70 period.” In a 1992 paper, “Monetary vs Fiscal Policy: New Evidence on an Old Debate,” Peter Kretzmer aimed to correct for methodological problems associated with the 1960s’ papers—for example, Kretzmer used VAR estimation to more accurately account for feedback effects.⁴² Kretzmer’s (1992, p. 25) results suggested that “while monetary policy has become less effective over the years, it is still more effective than fiscal policy.”

Two issues that emerge from the long-running debate spurred by the Friedman–Meiselman project are the following. First, given that the gestation of the Friedman–Meiselman paper overlapped to some extent with Friedman’s classic 1957 study, *A Theory of the Consumption Function*, the latter of which was also part of his criticism of Keynesian economics on empirical grounds, was there any interconnection between the two research projects? Second, what did the advances in econometric methods made during the odyssey of the Friedman and Meiselman project add to the economics profession’s knowledge about the relative effects of money and autonomous expenditures on income?

The 1957 study was to a large extent an investigation of how to best model a structural consumption relationship; it was concerned with structural modeling. In distinguishing between measured income and permanent income, Friedman considered his formulation of the permanent income hypothesis of consumption behavior to be inconsistent with models in which there was a mechanical multiplier relation founded on current consumption–current income linkages.⁴³ Thus, the study can be regarded as casting doubt about estimates of the multiplier—at least as formulated in the early Keynesian theories. In contrast, the Friedman–Meiselman study was more concerned with what the income-expenditure theory and the quantity theory imply for reduced form, bivariate behavior of key aggregates. This interpretation of the two studies is consistent with a remark made in the Friedman–Meiselman study. Thus, after noting that there seemed to be “no consensus whether consumer expenditures on adding to the stock of durable goods are better regarded as autonomous or induced,” Friedman and Meiselman stated: “Perhaps the

⁴¹ Poole and Kornblith (1973) evaluated the post-sample performances of the studies by Hester (1964), Ando and Modigliani (1965), and DePrano and Mayer (1965). The extended sample period covered the years 1959 to 1970.

⁴² Using quarterly data, Kretzmer estimated VARs for two subsamples: 1950:Q2 to 1979:Q4 and 1962:Q2 to 1991:Q4.

⁴³ Friedman (1957) showed that a simple model in which current consumption was regressed on current income underestimated the slope of the consumption function and overestimated the intercept if the pertinent variables were permanent consumption and permanent income.

bulk of opinion would be that they are better regarded as induced” (1963, p. 247). At that point they provided the following footnote:

Note that this is the implication for the present problem of the discussion in Milton Friedman, *A Theory of the Consumption Function*. It is there argued that current services of consumer durables are related to permanent income, that additions or subtractions to durable goods are related to transitory income. However, for present purposes we are interested in the relation to measured income. (1963, p. 247n1)

In sum, the permanent income hypothesis presented reasons why the multiplier formula needed to be modified (in a manner that tended to make it smaller compared with the prevailing literature), whereas the Friedman–Meiselman paper did not attempt to glean the underlying multiplier formula but instead tried to estimate the aggregate multiplier value in the data; because the spending multiplier varied across time, while velocity was fairly stable over time, the aggregate multiplier came out unfavorably.

The Legacy

During the early 1960s, the economics profession downplayed the role of monetary forces in the economy. Fiscal policy was considered the primary way to affect economic activity. Against this backdrop, the Friedman and Meiselman study had an enormous impact on econometric model building. Keynesian macroeconomic models, which, at the time, confined the effects of money to simple cost-of-borrowing monetary channels with modest effects on economic activity, were expanded to include additional monetary channels.⁴⁴ These included wealth and credit availability channels. Additionally, simple cost-of-borrowing channels focusing on total investment spending were disaggregated into subcomponents, including separate equations for business investment, housing, and consumer durables. In this connection, a report on the Federal Reserve MIT Keynesian macroeconomic model by Frank de Leeuw and Edward Gramlich (1969) is indicative of the effect exerted by the Friedman–Meiselman study on macroeconomic models. After referring to the influence of the Friedman–Meiselman study, de Leeuw and Gramlich wrote (1969, p. 265): “Many econometric models include only one channel.” The authors continued: “The Federal Reserve–MIT econometric model project was motivated by a desire to examine [the existence of other channels]” (1969, p. 266). What impact did the inclusion of additional monetary policy channels and the disaggregation of the cost-of-borrowing channel have on the effectiveness of monetary policy in the Federal Reserve–MIT model? Here is the way that de Leeuw and Gramlich put it:

Although the fiscal multipliers ... roughly agree with those of other econometric models, the money multipliers are much larger.... [T]he monetary multipliers have about the same ultimate effect as these obtained by the Staff of the St. Louis Federal Reserve Bank recently in a regression of GNP on monetary and fiscal variables, though the timing patterns and the effect of fiscal policy computed by the two studies are radically different. Our monetary multipliers are smaller than what a simple quantity theory of money would imply. (1969, p. 289)

⁴⁴ For a discussion of the influence of Keynesian macroeconomic models on policy-makers during the 1970s, see Tavlas (2023b).

Similarly, Poole and Kornblith (1973, p. 908) stated that the controversy generated by the Friedman–Meiselman study “continues to be ... enormously valuable. Many economists have been stimulated to work on the questions disputed by the monetarist and Keynesian combatants, and it is probably correct to argue that two models of the U.S. economy, the St. Louis Federal Reserve and the FRB-MIT-PENN models—are a direct outgrowth of the controversy.” As well, Christopher Sims’s work on VAR models can be viewed as an outgrowth of his entry into the Friedman–Meiselman controversy in which Sims (1972) pointed out that the assumption that the money stock was exogenous, in the sense of being uncorrelated with disturbance terms in the monetarist regressions, was testable.⁴⁵

In summary, the monetarist counter-revolution was critical in restoring the roles of money and monetary policy to academic and policy thinking in the 1960s and 1970s. The Friedman–Meiselman study—despite weaknesses in its econometric application—played a prominent role in that counter-revolution while, ultimately, also giving rise to more realistic macroeconometric model construction and improved methods of econometric estimation.

VI. CONCLUDING REMARKS

The Chicago Workshop in Money and Banking started in 1951, but apparently it was not “regularized” until 1953 (Nelson 2020, vol. 1, p. 398). During the initial years of the workshop, Friedman came up with the idea of comparing the empirical performances of the Keynesian income-expenditure theory and the quantity theory. In a paper, “Price, Income, and Monetary Changes in Three Wartime Periods,” which Friedman presented at the December 1951 meetings of the American Economic Association and published in the May 1952 issue of the *American Economic Review*, he stated:

But I take it that the major issue has been about the theories, not as alternative languages but as empirical hypotheses. In this sense they are different and comparative: the quantity theory asserts in essence that the velocity of circulation of money is the empirical variable that behaves in a consistent fashion: the income-expenditure theory, that the propensity to consume, or the consumption function, is the empirical variable that behaves in a stable or consistent fashion. ([1952] 1969, p. 166)

The “major issue” concerning the behavior of the two empirical hypotheses became a project undertaken in the workshop sometime around 1953 or 1954.⁴⁶

Friedman did not initially intend to produce a published paper from the results presented in his Wabash lectures. Once he did so, he brought in Meiselman to be an equal collaborator. Meiselman’s role was to update the data and estimate additional empirical specifications. In selecting Meiselman to (1) co-author a paper with him for the

⁴⁵ This point was made by Sims (2012, p. 1191). Sims’s VAR framework allowed the avoidance of the assumption of exogeneity in a relationship by treating all variables as potentially endogenous.

⁴⁶ Clark Warburton (1944) was the first economist to compare the empirical performances of a quantity-theory-based analysis with that of a Keynesian-type income-flow analysis. For a discussion of Warburton’s views, and of his influence on Friedman’s monetary thinking, see Lothian and Tavlas (2018) and Tavlas (2019).

volume published by the Commission on Money and Credit, (2) co-author an intended follow-up to the Commission study, and (3) co-edit a sequel to the 1956 book *Studies in the Quantity Theory of Money*, Friedman chose the person who had been the primary worker on the 1956 regression results. Meiselman's name first appeared under co-authorship with Friedman on an October 1959 draft.

During the late 1950s, and throughout the 1960s, Friedman undertook multiple projects as he spearheaded the monetarist counter-revolution against the Keynesian orthodoxy. Friedman's ability to handle multiple projects may have been a reason why he was, in my view, unfair with Meiselman in his (Friedman's) recommendation letters for Meiselman, who, in the late 1950s and early 1960s, was working on a PhD dissertation, teaching (in his capacity as assistant professor), conducting a job search, and estimating regressions with a hand calculator. Together, however, these two economists produced one of the most influential studies of the 1960s in the area of macroeconomics, but they were unable to come through with the long-promised follow-up to their 1963 paper, while the sequel to the 1956 volume, *Studies*, was published with a very long delay and without Friedman's participation. This paper has provided some reasons.

COMPETING INTERESTS

The author declares no competing interests exist.

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