Memories of Friedman and Patinkin

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During the period between the two world wars the University of Chicago produced an extraordinary group of monetary economists. For these notes, I will concentrate on two of them: Milton Friedman and Don Patinkin. I knew them both well, and both were important to my own economic growth. Both of them are remembered more for their books than for their journal articles, but the *Journal of Political Economy* published them both, including the interesting exchanges that I will discuss here.

Both Friedman and Patinkin did graduate work at Chicago, Friedman in the 1930s and Patinkin in the 1940s. Patinkin got his Chicago PhD in 1947, working under Oscar Lange. Friedman got his degree from Columbia in 1940, supervised by Simon Kuznets. The two did not overlap at Chicago, but both of them recalled classes on monetary economics with Henry Simons and Lloyd Mints and, less directly, Jacob Viner and Frank Knight.

In 1956 Friedman published *Studies in the Quantity Theory of Money—a Restatement*, a book consisting of a long introduction by Friedman himself, followed by the dissertations of four of his students: Phillip Cagan, John Klein, Eugene Lerner, and Richard Selden. These four dissertations—the first fruit of the Chicago Money and Banking Workshop—are stunning examples of economics at its best. I will come back to them, but first I want to review Friedman's introduction, which was focused almost entirely on clarifying and reviving a version of the quantity theory of money.

Friedman began with the concern that it "is clear that the general approach (the quantity theory) fell into disrepute after the crash of 1929 and the subsequent Great Depression and only recently has been slowly re-emerging into professional respectability" (3). One source of this disrepute was "the proponents of the new income-expenditure approach" who described versions of the quantity theory that were "an atrophied and rigid charicature." Friedman argued that Chicago economists—mainly Simons, Mints, and Knight—had formulated a more sophisticated and useful version.[1]

This was his first attack on the economics of Keynes. Later attacks came in his 1970 *JPE* paper "A Theoretical Framework for Monetary Analysis"

[1] This Chicago version of the quantity theory has been discussed in much more detail by many authors. See in particular Laidler (2010), Nelson (2017), and Tavlas (2017).

and again in a 1971 extension, also in the *JPE*, "A Monetary Theory of Nominal Income."

At this point Don Patinkin, who had followed all three of these statements of Friedman's, lost patience. In 1972 the *JPE* published Patinkin's "Friedman on the Quantity Theory and Keynesian Economics." Here is his abstract:

> The article is based on textual evidence from the quantity-theory and Keynesian literature. It shows, first, that the conceptual framework of a portfolio demand for money that Friedman denotes as the "quantity theory" is actually that of Keynesian economics. Conversely, Friedman detracts from the true quantity theory by stating that its formal short-run analysis assumes real output constant, while only prices change. Friedman also incorrectly characterizes Keynesian economics in terms of absolute price rigidity. He does this by overlooking the systematic analysis by Keynes and the Keynesians of the role of downward wage flexibility during unemployment, and of the "inflationary gap" during full employment. Otherwise Friedman's interpretation of Keynes is the standard textbook one of an economy in a "liquidity-trap" unemployment equilibrium. The author restates his alternative interpretation of Keynesian economics in terms of unemployment *dis*equilibrium. (1972, 883)

Patinkin went on to develop these assertions in detail. Later, in the text, he added that "it is obviously no criticism of Friedman—nor does it derogate from his stature as a monetary economist—to say that his analytical framework is Keynesian. All that is being criticized is Friedman's persistent refusal to recognize this is so" (886).

In fact this 1972 *JPE* issue contained, in addition to Patinkin, reactions to Friedman's 1971 paper from Karl Brunner and Allan Meltzer, James Tobin, and Paul Davidson. These were followed by an 85-page counterattack from Friedman. (The editors at the time were Robert Gordon and Harry Johnson.)

Friedman's 1970 and 1971 papers did not mention Simons, Mints, or Knight, nor did they refer explicitly to a Chicago tradition. This time Keynes was discussed at some length. But Friedman continued to refer to "the Keynesian challenge to the quantity theory" and to cast the two as incompatible contestants. A common reaction from Patinkin, Brunner and Meltzer, and Tobin was to argue that Keynesian ideas and the quantity theory can and should be parts of a single model. Reading it now one expects some kind of unification, and there are times when this seems about to happen. But Friedman would not have it. The long 1971 debate began with confusion and ended there.

It is surprising to me that Friedman did not connect "the proponents of the new income-expenditure approach" to the national income account time series that Simon Kuznets had created in the late 1920s (and that the US government has maintained ever since). These data opened up an exciting new world for economists who wanted measurements on the state of the economy as a whole. Kuznets's data included "real" series only: national money supply data were still in the future. The model-building pioneers of quantitative macroeconomics—Jan Tinbergen, Lawrence Klein, and soon many others—worked with Kuznets's data because they were the only good data they had. I think this was all there was to the "new income-expenditure approach."

If so, then what was needed to restore the quantity theory was to construct time series on the money supply at the same level of accuracy as Kuznets's data on real series. And this is exactly what Friedman's students did in the substantive chapters of *Studies in the Quantity Theory of Money*. All four dissertations constructed aggregate, economywide time series on some well-defined measure of a money supply and measures of nominal prices. Cagan's study of postwar hyperinflations provides monthly time series—suitable for his high-frequency data—carefully documented, for prices, measures of cash balances, and real per capita incomes for seven countries. There is an explicit theoretical model—set out and calibrated. Comparisons of theory and time-series data are shown graphically and assessed econometrically.

Cagan's dissertation was the crown jewel of *Studies in the Quantity Theory of Money*, a breathtaking achievement that is still drawn on. The other three students also produced new monetary time series, shedding light on interesting situations. John Klein analyzed Germany from 1932 to 1944. Eugene Lerner studied the Southern Confederacy of 1861–65 (see also his 1954 *JPE* paper). Richard Selden's "Monetary Velocity in the United States" covered 1839–1951. In his introduction, Friedman also provided a valuable discussion of the way these very different situations all served as natural experiments. What Friedman and his students had achieved in part, then, was to do for aggregate monetary theory what Kuznets had done for the aggregate real income and product accounts. They created a synthesized "money" consisting of many component assets that can be measured and add up to a whole, just as Kuznets had done with "consumption" and other real aggregates.

The typology of M0, M1, M2, and so forth was not available in 1956. The concept M1 was initiated by Homer Jones at the St. Louis Fed in 1960. Many people were involved in developing it further. Friedman, along with Allan Meltzer, Karl Brunner, and others, were supporters and users.[2] Pa-

[2] I thank William Barnett and Stephen Williamson for information on the role of the St. Louis Fed. It is a fascinating story just touched on here.

tinkin was right, I think, to insist that monetary theory can fit quite nicely with Keynesian ideas. Friedman and his students showed how to do it with actual time-series evidence.

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