



# FISHER'S LAST STAND ON THE QUANTITY THEORY: THE ROLE OF MONEY IN THE RECOVERY

BY

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## I. INTRODUCTION

In his recent article, Robert Dimand details for the profession the existence of four obscure papers by Irving Fisher (1935, 1936, 1937, 1940). He also brings to our attention two brief *Econometrica* contributions (Fisher 1946, 1947), neither of which is listed in the American Economic Association's *Index of Economic Journals*, as he points out. For this, and for his careful discussion of the content of those papers, we are indeed in his debt.

Two other purposes are served. One is an extended commentary on Fisher's dealings with various investigations and investigators regarding velocity. The major part of the article is concerned with that. As to it, the principal thrust is the adjustments necessary for velocity to be approximately constant, that is, to establish that "the velocity of money varies little, except as explained below" (Fisher 1946, p. 179). To use his apt metaphor, "the velocity of circulation ... [is not] simply a cushion for changes of money" (Fisher 1940, p. 56). The discussion shows the continuity between his early work in *The Purchasing Power of Money* and research concerns in the last decade of his life.

Secondly he also questions, nay denies, principally by way of Fisher (1936) the conclusion in Steindl (1997). In that paper Fisher's analysis of the events of 1929–33 was examined. The focus was in terms of whether he saw the economy's deterioration as due in large part to policy-influenced changes in the money supply; that is, did he use a quantity theoretic framework? The conclusion was, "Fisher was no longer a practicing quantity theorist" (Steindl 1997, p. 259).<sup>1</sup>

## II. SUMMARY OF PREVIOUS ANALYSIS

It is this latter issue with which this note is concerned. My 1997 inquiry deals with whether Fisher saw movements of output and prices in the thirties as due

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<sup>1</sup> Hence the sarcasm, indeed contemptuousness, associated with his turn of Isaac Stern's apocryphal quip as to the best way to Carnegie Hall: "If he had practiced any more, he would surely had gotten to Carnegie Hall!" (Dimand 2000, p. 344)

to the behavior of policy-induced, i.e., “exogenous,” changes in the quantity of money. The jumping off point is the observation that “it was that era which was the [quantity] theory’s trial by fire, its time in the desert” (Steindl 1997, p. 242). In particular, what emphasis did he give to the causes of movements in his favorite monetary aggregate  $M'$ , net demand deposits?<sup>2</sup>

To that end, his work on the debt-deflation (1932, 1933a) became the focus. Central to that was an inquiry into his money supply mechanism, which was implicit because “questions of the money supply mechanism simply were not of interest to him” then and in his earlier work (Steindl 1997, p. 249). In the course of the investigation, it became clear that Fisher had abandoned a money supply mechanism that could be regarded as having deposits driven by an exogenous element.<sup>3</sup> In his work prior to the Depression, Fisher “held to the view that the supply of money—both currency and deposit currency—was essentially determined ‘exogenously’ ... With *Booms and Depression*, this changed” (Steindl 1997, pp. 247–48). Now, currency was no longer determined by the demand for it. Rather the Federal Reserve fixed the amount.

Of more consequence, banks no longer determined the composition of their earning assets. They were essentially passive to the debt-deflation-induced repayment of loans and the consequent run-up in their excess reserves. They did not acquire other earning assets as debtors repaid their previous borrowings. In the “stampede of liquidation [arising from] a general state of *over*-indebtedness, [the] new borrowings will by no means suffice to restore the balance, and there must follow a net shrinkage of deposits” (Fisher 1932, p. 15; Steindl 1997, p. 248). Consequently, the stock of deposits, his check-book money, fell and with the decline in deposits, economic activity also banked down. In other words, Fisher now subscribed to a real-bills doctrine view of banking! Accordingly, the fall in prices was not linked to policy actions inducing a decline in deposits but to “the desire to reduce over-indebtedness, that is, as due to increases in the demand for money” (Steindl 1997, p. 250). That was the basis of his declining “efficiency of money” analyses, with its associated (sixty-one percent) fall in deposit velocity (Fisher 1932, p. 96).

The resulting excess demand for money could have been offset with expansionary monetary actions, as would indeed be the obvious policy preference of those with a quantity theory orientation. But this was neither Fisher’s preferred solution nor his principal policy conclusion.

The increase in the demand for money was of sufficient consequence that he interpreted it in terms of a liquidity trap, *viz*:

[With falling prices] people *can* hoard what they *do* have; so that ... a mere new supply of money, to replace what has been liquidated or hoarded, might fail to raise the price level by failing to get into circulation ... For a prompt boost of the price level, therefore, a mere increase in  $M$  might prove

<sup>2</sup> As a case in point, see the extensive quote relying on its importance in his dealings with Eugene Meyer, the chairman of the Federal Reserve board (Dimand 2000, p. 334).

<sup>3</sup> This came as a distinct surprise. In my earlier treatment of his Depression views, I concluded that he in fact had a money supply mechanism in which the Federal Reserve could increase the money stock (Steindl 1995, pp. 99–105). My later, closer investigation convinced me I was mistaken.

insufficient, unless supplemented by some influence exercised directly on the moods of people to accelerate  $V$ —that is, to convert the public from hoarding (Fisher 1932, p. 140).

And, true to form, Fisher had a ready solution: the *deus ex machina* for increasing velocity was the Silvio Gesell inspired stamp scrip—*Schwundgeld*. He was so enamored of this that in the press of the Depression anxieties he took time to write a book, *Stamp Scrip* (Fisher 1933b), extolling its benefits.<sup>4</sup> One of its principal advantages was that like money it can be spent but “it is *unlike* money, because IT CAN NOT BE HOARDED” (Fisher 1933c, p. 8).

### III. NEW EVIDENCE

Does this sound like a quantity theorist? Where is the evidence that the Depression could be understood as resulting from monetary contraction and the recovery from expansion of the money stock, even using Fisher's preferred  $M'$  net demand deposits? The data, monthly industrial production, wholesale prices, and net demand deposits, as well as correlation techniques were readily available to him.<sup>5</sup> He had used them in earlier work. It is on the basis of this analysis that I concluded that he “was no longer a practicing quantity theorist” (Steindl 1997, p. 259).

On these matters, Dimand says nothing. Fisher's adoption of a real-bills money and deposit mechanism is not cause for comment. Neither his liquidity trap coupled with his associated proposal for stamp scrip nor his failure to argue for monetary expansion as a cure for the Depression, is reason for Dimand to question whether Fisher maintained his quantity theory eyes. Instead, Dimand moves directly to a discussion of Fisher's last monetary writings, the bulk of which are concerned with investigations showing that velocity was essentially constant. In fact, it is basically only in the Cowles Commission lecture (Fisher 1936) that he relates movements in money to the economy.

This brings us first and foremost to that paper because that lecture is the one upon which Dimand principally relies in disputing my analysis. After a perfunctory acknowledgment that the cause of the Depression is a complex matter, Fisher states, and this Dimand quotes in full, “one cause towers above all others, the collapse of our deposit currency. The depression was a money famine—a famine, not of pocket-book money but of check-book money, the money, or so-called money, recorded on the stubs of our check-books, our deposits subject to check” (Dimand 2000, p. 332). To emphasize the extent of the fall in deposits, Fisher points to a thirty-five percent decline in demand deposits between 1929 and 1933—“That is, our chief circulating medium had shrunk by \$8 billion dollars” to \$15 billions (Fisher 1936 p. 104; Dimand 2000, p. 332–33).

This certainly can be understood in terms of his debt-deflation hypothesis and its correlative real-bills focus, given that he neither assigns any role to the Federal Reserve for engineering the deposit decline nor does he amend his

<sup>4</sup> Though I referred to *Stamp Scrip* in my analysis of his proposal for boosting velocity (Steindl 1997, p. 251), I failed to include it in the References.

<sup>5</sup> See for example, the scatter diagram in figure 2 in Steindl (1997, p. 256).

earlier real-bills oriented analysis. It follows directly and indeed is fully compatible with his “over-indebtedness causing debt-deflation” thesis (Fisher 1932, 1933a). There is nothing in it that has the Depression resulting from an “exogenous” contraction of “check-book money,” induced, for instance, by central bank actions. It is the flight from over-indebtedness that leads to the downward spiraling of deposits due to banks’ passivity in acquiring other assets. The contraction of deposits is not laid at the feet of the monetary authorities. Hand and hand with his debt-deflation view is the real-bills deposit mechanism he adopted.

Further, the liquidity trap is still prominent in his Cowles Commission Conference lecture. How else does one interpret his statements that “moreover in 1929 all money circulated faster than in 1933 when people were hoarding” and total demand deposits of “15 billions of check book money *left largely idle?*” (Fisher 1936, pp. 104–105, my italics)<sup>6</sup> Why the hoarding? Why is it that still in 1936, the entire stock of 1933’s check book money was largely idle balances, if not for a liquidity trap notion carried over from his debt-deflation analysis?

Having said that, he then moved to discuss the rebound in deposits from the 1933 low. He pointedly noted that the increase was “not [brought about] in the usual way by business men borrowing of [*sic*] banks but by the Government selling its bonds to the banks” (Fisher 1936, p. 105; Dimand 2000, p. 333).<sup>7</sup> Indeed, from the beginning of the recovery through March 1936, the most recent month for which he would have had data for his July 10 presentation, bank reserves rose 121 percent and bank holdings of U. S. government securities increased 109 percent. The rise in bank reserves was not, however, due to Federal Reserve policy. Federal Reserve Credit actually declined, being one percent *lower*. The rise in reserves was principally attributable to a 153 percent increase in the gold stock—a phenomenon that did not go unnoticed among economists.<sup>8</sup>

Among other things, it was this seemingly anomalous situation in which deposits increased because of bank purchases of governments rather than because of business lending, “the usual way,” that was further ammunition in his campaign for 100 percent reserves: “it is the smallness of our reserve requirements which makes possible the contraction and expansion of the deposit currency” (Fisher 1936, p. 106). Coupled with the growing volume of excess reserves, which had risen 600 percent from the beginning of the recovery, this led him to take a positive view of the soon-to-be implemented doubling of reserve requirements, at the same time expressing skepticism that a doubling “may not be enough” (Fisher 1936, p. 106).

There was not a modicum of concern that an increase in reserve requirements might have a deflationary effect, a typical Fisherian posture of his many

<sup>6</sup> Only the first of these quotes is reproduced in Dimand (2000, p. 333).

<sup>7</sup> As this discussion makes clear, I was in error when I wrote that there was no evidence that Fisher ever looked at money or deposit data during the recovery (Steindl 1997, p. 258).

<sup>8</sup> Representative opinions are in Steindl (1998, pp. 836–37) and include Lehmann’s observation that “the American economy would probably have suffered if gold had been refused entry” as well as Simmons’s analysis that the Treasury effectively controlled bank reserve positions by altering its deposit account at the Federal Reserve.

crusades: witness the extended discussion concerning Clark Warburton, particularly as it relates to Prohibition (Dimand 2000, pp. 339–42).<sup>9</sup> Perhaps Fisher was caught up in the righteousness of his crusade so that he did not think through the money supply consequences of his position. Perhaps this reflected his lifelong disinterest in money supply mechanisms. More likely, this was another variation on his real-bills orientation from *Booms and Depressions*. This time it was based on banks being unable to decide the composition of their assets because “the usual way” of lending, loans to business, was not in his view operational due to a “shortage of borrowers.”

As in the foregoing analysis, there is little to indicate that Fisher had abandoned his earlier Depression views and had now adopted the approach of a practicing quantity theorist. One of the most interesting things is the role of the introduction of discretionary changes in reserve requirements by virtue of the Banking Act of 1935 and his enthusiasm for increasing them beyond the newly legislated maximum. The money supply consequences of doing so were ignored, suggesting his continued adherence to a real-bills money mechanism. The article is not a refutation of his previous analysis.

Thereafter, Fisher (1940, 1946, 1947) did not return to any formal analysis of the relationship between the behavior of the money supply and the economy, at least according to the discussion in Dimand (2000, pp. 336–44). Rather, the bulk of his remaining monetary work had him and his coterie of colleagues concentrating their energies on establishing the approximate constancy of velocity, even to the point of arguing that “the drop [in velocity] between 1929 and 1933 ... was probably more apparent than real,” a position that contrasted with his 1936 Cowles Commission lecture (Dimand 2000, p. 337).

#### IV. CONCLUSIONS

The question prompting this, as well as the earlier, investigation was whether Fisher in the 1930s was a practicing quantity theorist. Earlier I answered in the negative. Professor Dimand's survey of Fisher's late monetary work concluded that he continued to employ the quantity theory.

He and I clearly disagree. Perhaps this is due to what each views as the appropriate, relevant notion of “a practicing quantity theorist.” If by a practicing quantity theorist one means undertaking research seeking to establish the constancy of velocity, and that was the main focus of his monetary activities, to retain continuity with the work in *The Purchasing Power of Money*, that is one view.

The criterion by which I concluded that Fisher was not a practicing quantity theorist relates to using the theory to analyze and understand the behavior of the economy in the 1930s because “it was that era which was the theory's trial by fire, its time in the desert” (Steindl 1997, p. 242). According to that view, movements of the money stock are the result of policy actions, appropriate or

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<sup>9</sup> Dimand is a bit cautious when he states that Warburton's employer, the FDIC, “discouraged research not directly related to his job” (2000, p. 339). In fact, Warburton firmly believed that “my job would be at stake” (Steindl 1995, p. 159, n. 1) if he persisted in his monetary research activities.

not. The movements in the money stock then affect the economy. Fisher's later work, particularly that presented in his Cowles Commission lecture (1936) with its real-bills orientation, liquidity trap, and embracing of substantial reserve requirement increases, indicates that he had not reverted to using the quantity theory as a vehicle to understand the economy. It does not alter the earlier judgement that he was not a practicing quantity theorist.

On the question of the appropriate notion of a practicing quantity theorist, then, there can and will be disagreement. It may be best therefore to let each economist decide whether or not Fisher was one.

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