MIT and Money

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> *A Study of Industrial Fluctuation* (1915) . . . was written in good part when [Dennis] Robertson was but twenty-two years of age and in the third year of his economic study! Such precocity is hard to match. It reminds one again of the incredible ability of talented youth to master in a season all that the past has established, and then to push the flag forward another furlong.
> —Paul A. Samuelson, "D. H. Robertson (1890–1963)" (1963)

Money at MIT was always fundamentally about monetary policy and so could not really get started until after the 1951 Treasury-Fed Accord allowed the Federal Reserve System to shift away from its wartime policy of pegging Treasury rates to support the government bond market (Hetzel and Leach 2001). And even then, the Keynesian priors of the founders of the MIT economics department for a long time downgraded the importance of monetary policy relative to fiscal policy, and hence also of monetary theory relative to the theory of public finance.

Neither of these background facts particularly differentiated MIT from many other institutions in the immediate postwar period, with the crucial

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exception of the University of Chicago, which early on positioned itself as the center of anti-Keynesian reaction, organized under the banner of Milton Friedman's "monetarism." This exception, however, proved vitally important for the development of MIT. Indeed, one thing that would eventually differentiate MIT from other institutions on the subject of money was its deliberate decision to position itself as the center of antimonetarist reaction. Money at MIT was always about monetary policy, but it was also always about countering monetarism.

Thus, at Chicago, even before the 1951 accord made monetary policy possible, Friedman (1946) had already mounted a sustained attack on Oskar Lange's crypto-Keynesian *Price Flexibility and Employment* (1944) and had already put forth his own alternative in "A Monetary and Fiscal Framework for Economic Stability" (Friedman 1948), which can be read as a reformulation of Henry Simons's 1934 manifesto "A Positive Program for Laissez Faire." Subsequently, the students in Friedman's famous Workshop on Money and Banking produced *Studies in the Quantity Theory of Money* (Friedman 1956), and Friedman's essay for that collection, "The Quantity Theory of Money: A Restatement," was the analytical foundation for his subsequent manifesto, *A Program for Monetary Stability* (1959). The empirical foundation of the monetarist edifice, *A Monetary History of the United States* (Friedman and Schwartz 1963), followed soon after. In the field of money, Chicago got a very big jump on MIT.

The central figure in MIT's response to the Chicago initiative was clearly Franco Modigliani, who joined MIT in 1962 after visiting for a year in 1960. But the choice of Modigliani as MIT's champion was very much a consequence of the economic department's prior conception of how one ought to think about money as a matter of pure economic theory, which is to say that it was very much a consequence of Paul Samuelson's views on the matter. I start therefore with Samuelson.

Don Patinkin (1983, 163–65), writing retrospectively for Samuelson's Festschrift, traced the evolution of Samuelson's treatment of monetary policy, and of the quantity equation, through all editions of his best-selling textbook, *Economics* (for the backstory of *Economics*, see Giraud, this volume). In general he tells a story of Samuelson's increasing appreciation of both, starting with monetary policy, which comes into its own in the fourth and fifth editions (1958, 1961), and continuing with the monetarist practice of using the quantity equation as the preferred framework for thinking about macroeconomic issues, which gets respectful notice in the eighth edition (1970) and after. The evolution of Samuelson's textbook

treatment follows the profession, of course, but it also reflects the evolution of Samuelson's own personal views. Samuelson started professional life, it is important to appreciate, with a nonmonetary view of the cycle as driven by the interaction of real investment and consumption in the multiplier-accelerator model (Samuelson 1939). But he concluded, as we shall see, with the view that monetary disequilibrium is of the essence for departures from long-run equilibrium.

Throughout that evolution, one constant was Samuelson's practice of viewing monetary matters through the lens of neoclassical price theory; for him, it was all a matter of money demand and supply. Money demand was just a special case of the general theory of consumer demand, and money supply was a decision variable of the monetary authority. This approach to monetary theory he shared with most of the profession, notably including Patinkin, so much so that it has often been hard for historians of this period even to recognize the shared perspective as a specific approach, rather than just logic or science. My coinage of the term "Monetary Walrasianism" (Mehrling 1998, 2002a, 2010), and my insistence on tracing the roots of the approach to the early work of Jacob Marschak (1934, 1938) as well as John R. Hicks's more familiar "Suggestion for Simplifying the Theory of Money" (1935), has drawn critical attention to this surprising unanimity. Although Samuelson left the formal development of this approach largely to others (most importantly James Tobin and Franco Modigliani), he quite definitely endorsed it. Even more, for Samuelson, Monetary Walrasianism provided the crucial link between value theory and macroeconomic modeling, a link that could be depended on to persist (so he hoped) even as the postwar world evolved toward more flexible prices, in globally integrated and developed private capital markets.

Just so, James Tobin (1983, 197), following after Patinkin in the 1983 Festschrift, states firmly: "Paul Samuelson's greatest contribution to macroeconomics was the neoclassical synthesis, of which he was the principal architect. This *Weltanschauung* reconciled the classical and Keynesian strands of his thinking and that of many of his contemporaries." "Classical" in this context means Léon Walras and neoclassical price theory, as in Samuelson 1947. "Keynesian" means Alvin Hansen and the aggregative multiplier-accelerator model, as in Samuelson 1939. Classical is about long-run equilibrium, while Keynesian is about short-run disequilibrium caused by "adjustment costs and lags, market imperfections, and discrepancies of information and expectations" (Tobin 1983, 198). Wearing his classical hat, Samuelson was concerned about efficient allocation;

wearing his Keynesian hat, he was concerned about aggregate income stabilization.

But, notwithstanding Tobin, Frank Hahn (1983, 51) writes in the same Festschrift that "Samuelson, like all his contemporaries, and indeed most of his successors, never seems to have found it necessary to provide a link between his general equilibrium (value) theory and his macroeconomic modeling." Fair enough, perhaps, from the viewpoint of pure theory, since "the pure Arrow-Debreu model predicts that there is no money and that there is no Stock Exchange" (32; see also Hahn 1965). Samuelson himself, however, seems to have felt that a more informal link was sufficient for his purposes, and it is that informal link that also apparently satisfied Tobin and others.

The most complete account of that link that Samuelson himself ([1967] 1972, 557) ever offered can be found in his purely verbal account "Money, Interest Rates, and Economic Activity: Their Interrelationship in a Market Economy," which emphasizes "dynamic modifications of the crude classical system," in particular "dynamic assumptions . . . about the creation of money growth as a result of mines, banks, or governments." (The "market imperfections, and discrepancies of information and expectations" emphasized by Tobin are, in Samuelson's own account, not so fundamental to the neoclassical synthesis, though clearly important for specific policy applications.) As a consequence, the money rate of interest can and does deviate from the rate of profit, with consequences for investment spending and hence real activity. More generally, Samuelson insists, "once we introduce systematically into a post-Keynesian system treatment of stocks of assets, monetary and real, and take into account fluctuating levels of real unemployment, there remain no inconsistencies between the classical system and the Keynesian system. The synthesis of common content emerges with an eclectic position on the interplay of real and monetary factors in determining the structure and levels of interest rates" (559).

This emphasis on stocks of assets, and the consequent disequilibrium dynamics of adjustment, is the core of Samuelson's mature vision of the neoclassical synthesis and also the connection to finance in his thinking, because introducing asset stocks means also introducing asset prices. For Samuelson in 1967, monetary disequilibrium inevitably involves asset price disequilibrium in the form of deviation of the level of market prices from their fundamental values, hence inefficiency. As he said years later, "Modern markets show considerable *micro* efficiency [but also] considerable *macro* inefficiency, in the sense of long waves in the time series of

aggregate indexes of security prices below and above various definitions of fundamental values" (quoted in Jung and Shiller 2005, 221). In his mature formulation it is these disequilibrium asset price fluctuations that drive the investment fluctuations that kick-start the multiplier-accelerator mechanism that produces aggregate instability.

Paul A. Samuelson

Paul Samuelson (1968, 3) began his career as a mathematical economist, which at that time meant being a "specialist in value theory," and it is this starting point that shaped his conception of monetary economics. Although his 1941 Harvard dissertation, "Foundations of Analytical Economics," had little to say about money, Samuelson (1968, 4) claimed retrospectively to have had "a correct vision of the proper version" of neoclassical monetary theory as early as 1937, by which date he had already drafted the bulk of the dissertation. (Note that such dating of the dissertation would put Samuelson, born in 1915, in the running to match the precocity of Robertson.) The essentials of that 1937 vision he published ten years later in a few pages added to the revised version of his dissertation, *Foundations of Economic Analysis* (Samuelson 1947, 117–24). Given the importance of Samuelson in shaping the MIT economics department, and the importance of these few pages for understanding Samuelson's own conception of money, our investigation must begin here.

If he had the correct vision in 1937, why did he not include it in his dissertation? Thirty years later, Samuelson (1968, 4) offered an explanation for why he waited: "Frankly, I was repelled by the abstract level at which Oskar Lange, Hicks, and others carried on their discussion of Say's Law, staying at the level of equation counting and homogeneity reckoning, without entering into the concrete character of the models." Pushed by Don Patinkin to elaborate, he added that he had chosen not to involve himself in the "then-popular game of reconciling 'loanable funds' and 'liquidity preference' theories of interest determination," a debate that built on the analytical framework of Hicks's 1939 *Value and Capital* and culminated in Lange's *Price Flexibility and Employment* (1944). Specifically, writes Samuelson (1972, 288), "I never felt that indifference curve analysis of the triad, money, goods, and bonds, had the fruitfulness of such analysis of tea, salt, and apples."

And yet, in the 1947 sketch, it was exactly indifference curve analysis that Samuelson (1968, 4) did offer, and it was exactly this 1947 analysis

that he retrospectively characterized as the "nub of the matter." Simply put, in 1947 Samuelson approached monetary economics as nothing more than an extension of consumer demand theory. His starting point was the observation that "money yields a real service" in facilitating transactions, in which respect it is much like other commodities that enter the neoclassical utility function. Money is different only because its real service depends on its value, and that means we have to include prices as well as quantities as arguments in the neoclassical utility function. That done, however, all we have to do is to maximize utility subject to a budget constraint and out drops the demand for money (I is income, r is an exogenous rate of interest, and p_m is the price of money):

$$M = M(p_1, \ldots., p_n, p_m, I, r). \text{ (Samuelson 1947, 121)}$$

Repelled in 1937 but ready to commit to print in 1947, what changed Samuelson's mind? As he says, not *Value and Capital* (Hicks 1939), notwithstanding his respectful nod in the preface to his 1941 thesis to "Professor Hicks' long-awaited treatise," whose "similarity in point of view has been reassuring." Instead, we perhaps find the clue in Samuelson's (1947, 121) citation of Jacob Marschak's 1943 "Money Illusion and Demand Analysis," an empirical paper that concludes with these words: "A reduction of money incomes is likely to have a somewhat stronger effect on the real expenditure on meat than would a proportionate rise in the price of meat, accompanied by a proportionate rise in the price of other foods and/or in other living costs" (48). In other words, the simple dichotomy between nominal and real that is such a characteristic feature of classical monetary theory seems not to be a feature of the real world. Samuelson took the lesson that the classical equation of exchange, $MV = PY$, presents an overly simplified view of the role of money, and that modern neoclassical methods might be able to add something.

Marschak's empirical finding seems to have emboldened Samuelson not only to publish but also (unlike Hicks and Lange) to leave Keynes entirely aside and to proceed instead solely from Walras. Thus in 1947 Samuelson blithely abstracts from dynamical considerations and expectations, risk and uncertainty, and hence also liquidity preference and speculative demand. He considers the Hicks-Keynes notion of a possible monetary theory of interest, but only to reject it in favor of the marginal productivity of capital theory of interest. And he also considers the Hicks-Keynes notion of liquidity preference as the origin of the tendency for the term structure of interest rates to be positively sloped (122–24), but rejects

that too on the grounds that the supposed empirical regularity is a mere historical anomaly and hence nothing that needs to be explained. In 1947 Samuelson's monetary theory is self-consciously neoclassical, which is to say pre-Keynesian. He merely sets himself the task of translating the best of neoclassical monetary thought into mathematics.

In doing so Samuelson was following, perhaps unconsciously, in the footsteps of Dennis Robertson, whose *Money* (1922) Samuelson (1963, 519) remembers as his "earliest introduction" to the subject. In the preface to that book, Robertson explicitly states his intention to "treat the theory of money as a special case of the general theory of value," and that of course is exactly what Samuelson was doing in 1947. Like Robertson, Samuelson embraced the foundational insight of the quantity *theory* of money that money is that special thing which, if its quantity were doubled along with all prices, would leave real quantities unchanged for the simple reason that such doubling would leave unchanged the real service that money can provide. Unlike Robertson, Samuelson rejected the quantity *equation* as a useful analytical starting point for understanding the consequences of actual changes in the money supply. In 1947 he offered an alternative analytical starting point based in neoclassical price theory that abstracted from dynamics and uncertainty because he thought he did not need them. In his mind, he was establishing the frictionless surface result; adding in the frictions was for someone else, a problem of engineering, not of science.

Here enters Friedman. From Samuelson's point of view, most of the claims of Friedman's monetarism must have seemed to be more about engineering than about science, as for example Friedman's claim about the superiority of monetary policy over fiscal policy, and his claim about the superiority of a constant money growth rule over a more activist countercyclical interest rate policy. After all, on the scientific fundamentals of the theory of money, there was no real difference between Samuelson and Friedman. Instead, the big scientific difference at stake seemed to be about the optimality of laissez-faire, so it was here that Samuelson (1958) chose to deploy his efforts, starting with his celebrated overlapping generations model in "An Exact Consumption-Loan Model of Interest with or without the Social Contrivance of Money." The paper was never intended to provide theoretical foundations for monetary theory, and certainly not to replace the foundations Samuelson had laid in 1947. (The paper does not even cite that earlier work but instead takes off from Samuelson's "Pure Theory of Public Expenditure" [1954].) Patinkin (1983, 160–62) quite correctly reviews negatively the cottage industry that missed Samuelson's

point, and so attempted to build monetary theory on these purported alternative foundations.

In the 1958 paper, by contrast to the 1947 one, money does not enter directly into the utility function or serve as a medium of exchange. Rather, money is a store of wealth in which the working generation accumulates its saving for retirement. In the model, money is the only store of wealth (by assumption), and without it there is no way for the competitive market to achieve the social optimum. Under the special assumptions that Samuelson makes, the social optimum is achieved when the rate of interest on consumption-loans is exactly equal to the rate of population growth. Thus, in the special case when the population is stationary, money that bears zero nominal interest can help achieve the optimum. But it can help also in the case when population is growing or shrinking so long as prices fall or rise at a rate sufficient to set the real rate of interest equal to the rate of population growth. Contrary to monetarist presumption, price stability is not necessarily a feature of the social optimum.

Observe that this 1958 argument implicitly accepts, presumably just for the sake of argument, the simplistic quantity equation that Samuelson rejects in 1947 and that Friedman resuscitates in his 1956 "Restatement." But, to repeat, that is because Samuelson in 1958 was not mainly trying to make a contribution to the theory of money. Rather, his central point was that competitive markets cannot always be depended on to achieve the social optimum. The social contrivance of money is one possible solution, but there are others. A social security system, for example, that taxes the current working generation and transfers the proceeds to the current retired generation could also achieve the social optimum. More generally, the challenge facing economists is to devise "social collusions," "self-imposed fiats," and other "social contrivances" to achieve the social optimum. Fiat money, used as a store of value, is only one such possible social contrivance.

So much for the scientific claims of monetarism, but what about the engineering claims? Robertson had followed up *Money* with a manual for central bankers, *Banking Policy and the Price Level* (1926), that Samuelson (1963, 518) found "almost unreadable." Clearly someone else would have to be recruited to engage the engineering claims of monetarism. What about Franco Modigliani? Because MIT was basically an engineering school, it was important for the economics department to have someone representing the engineering side of economics (for a deeper exploration of the importance of this engineering dimension, see Halsmayer, this

volume). Even more, because the policy success of the Keynesian agenda in the outside world was so important for elevating the scientific status of Keynesian economics, it was important to have someone serving as the public face of MIT's views on money for the next generation. Modigliani would fill both the inside and the outside roles.

Modigliani and Monetarism

On the scientific fundamentals, regarding both the theory of money and the nonoptimality of laissez-faire, Modigliani agreed entirely with Samuelson. Himself a student of Marschak at the New School in New York, Modigliani (2001, 19) was a Monetary Walrasian born and bred. Even more, his 1944 paper "Liquidity Preference and the Theory of Interest and Money," part of his PhD dissertation, advanced a specifically monetary interpretation of Keynes that would become the analytical frame through which he viewed macroeconomics for the rest of his life (cf. Modigliani 2001, 20–44). Following Hicks (1937) more than Keynes himself, Modigliani always viewed the problem of economic stabilization largely as a matter of adjusting the supply of money to the quantity that would be demanded at full employment. Thus, like Friedman and the monetarists, he emphasized monetary policy over fiscal policy, but, unlike Friedman and the monetarists, he also emphasized active intervention over a constant money growth rule. "In other words, unemployment is caused by a lack of money. And since money is determined by the central bank, this is caused by an improper monetary policy" (Modigliani 2001, 39).

In 1944 Modigliani's emphasis on monetary intervention placed him in diametric opposition to the more radical Keynesians, such as Abba Lerner (1943), whose "functional finance" urged a policy of deficit spending to achieve full employment, without regard for the consequence in mounting government debt (Modigliani 2001, 43–44). In Modigliani's view, by contrast, fiscal policy came into its own only when monetary policy lost its traction, which is to say under conditions of a liquidity trap, such as during the Great Depression. In normal times, monetary policy had plenty of traction simply because of the downward stickiness of prices and wages; changes in the nominal money supply produced changes also in the real money supply, hence in the rate of interest, real output, and employment. To be sure, once full employment was reached, continued monetary expansion would have no real effect, since prices and wages would rise to neutralize the effect of any increase in the nominal money supply. But

below full employment, the real effect was significant. Monetary policy *could* stabilize output and employment, and therefore monetary policy *should* stabilize output and employment.

Having adopted this intellectual framework, Modigliani organized his subsequent work to elaborate the underlying mechanisms. Interestingly, it was not money demand and supply that initially occupied him but the links with real expenditure, and it was these investigations that would be recognized in the 1985 Nobel award (Kouri 1986). Thus the life cycle hypothesis arose from an investigation of the determinants of consumption spending and saving, while the Modigliani-Miller theorem arose from an investigation of the determinants of business investment spending. On money, it was Tobin (1956, 1958) at Yale who initially took the lead for the Keynesians, not Modigliani. Only after Modigliani (1963) joined MIT did he turn his attention seriously to the monetary transmission mechanism in "The Monetary Mechanism and Its Interaction with Real Phenomena" and also to direct confrontation with Friedman and monetarism (Ando and Modigliani 1965, 1969; Modigliani 1964, 1977, [1977] 1986).

Not only MIT but eventually the entire Federal Reserve System had its hopes riding on Modigliani when, in 1964, he took on supervisory responsibility for development of what was then known as the FMP (Fed-MIT-Penn) model, the first large-scale econometric model of the United States (Modigliani 1971a, 1975). In subsequent years, a generation of graduate students found employment and dissertation topics, and so launched their careers, by working on that model (see, e.g., Modigliani and Sutch 1966, 1969; Modigliani, Rasche, and Cooper 1970). (For the larger story of how graduate teaching spread the MIT gospel, see Svorenčík, this volume.) The way the model worked, monetary policy set the short-term rate of interest, which then got translated into a long-term rate by a term structure equation (preferred habitat version) and a risky rate through an equity pricing equation. These capital market rates then entered the expenditure equations in the "real" sector of the model. Modigliani (1975) provides a simplified graphical representation, which is reproduced below as figure 1.

As a forecasting model, the initial FMP was a disappointment; financial innovation and institutional change meant that the estimated equations were not stable. As Pentti Kouri (1986, 318) states, "The criticism of Lucas [1976] and the rational expectations school applies as much to the MIT model as to all other large-scale models." But as a concrete crystallization of postwar central bank ambition, it carried the day. The importance of

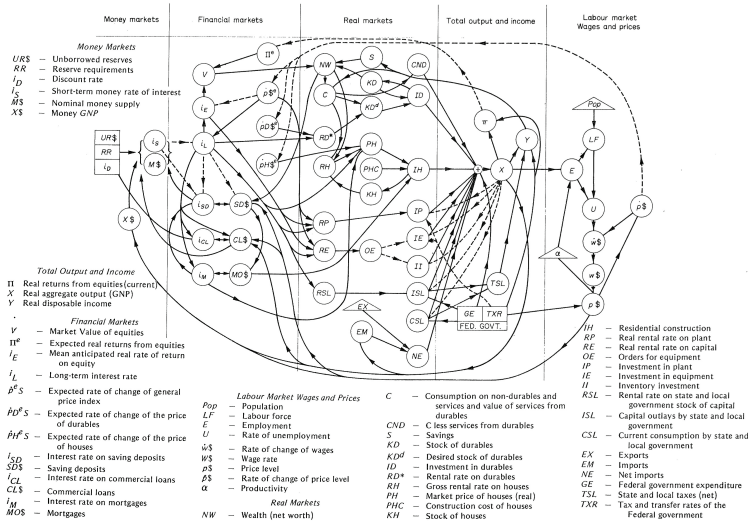

Figure 1 Modigliani's (1975) graphical representation of the FMP (Fed-MIT-Penn) econometric model

central banks as contributors to macroeconomic stabilization became the new orthodoxy, and it was Modigliani (1971b) who got that ball rolling. In the event, the criticism of Tobin and William Brainard (1968) proved decisive, and the alternative framework suggested by Tobin (1969) provided the starting point for the next generation of econometric modeling (Fromm and Klein 1975).

Samuelson Redux

Until 1965 the MIT economics department was two departments in one, the original industrial relations department that had been established as a service department for the MIT engineers, and a newly emerging scientific economics department that had been growing slowly around the personage of Paul Samuelson. In 1965 the industrial relations faculty moved into the Sloan School of Business, while a new crop of the best and brightest junior faculty that could be found was added to the economics department: Duncan Foley from Yale, Miguel Sidrauski from Chicago, and Michael Piore from Harvard (Piore 2013, 21; Cherrier, this volume).

The plan was for MIT to be the place where the scientific debate of the next generation would take place, get resolved, and then be transmitted to the world through successive graduating classes of newly minted PhDs. During the Kennedy administration, when MIT's Bob Solow joined Yale's James Tobin on the Council of Economic Advisers, the ascendance of Keynesian policy activism was established, and the MIT department emerged as a proud center of scientific support for that policy consensus. But neither policy nor science stands still, and so neither could MIT. Debate would therefore proceed, albeit under the watchful eye of the old heads—in 1965 Samuelson turned fifty—so ensuring that the hard-won Keynesian wisdom of the past would be bolstered, not overturned.

Unfortunately for the plan, all was not well with the scientific foundations of Monetary Walrasianism, notwithstanding the tacit agreement by both monetarists and Keynesians to continue building on those foundations. The difficulty was something called the Hahn problem, pointed out by Frank Hahn in a short paper "On Some Problems of Proving the Existence of Equilibrium in a Monetary Economy" (1965). Hahn's target was Don Patinkin's magnum opus *Money, Interest, and Prices* ([1956] 1965), not Samuelson's *Foundations* sketch, but the force of the criticism applied to both equally. Simply put, adding money balances to the utility function does not provide the foundations for monetary theory that one might think it does, because there is no guarantee that equilibrium exists (unless other auxiliary assumptions are made as well), and even when it does, there will always also exist an equilibrium in which the price of money is zero, so that zero real balances are held.

In plain terms, money is inessential in Walrasian equilibrium. The problem is not, as is often asserted, that Walrasian equilibrium is a model of a barter economy without money. Quite the contrary, it is more accurate to say that Walrasian equilibrium is about a world in which the monetary mechanism is so efficient that the velocity of money is infinite, so that a vanishingly small amount of money is able to do all the work that is needed to facilitate transactions. Obviously that is not the world we live in, so one response to Hahn was simply to abandon the search for a foundational model and attend instead to practical models; that is essentially what Modigliani was doing. But the question of analytical foundations remained.

Samuelson's paper "What Classical and Neoclassical Monetary Theory Really Was" (1968) can be read as his attempt to sketch a possible road forward that would, by embracing dynamical considerations, provide a more adequate response to Hahn. It was only a sketch, but it was a tremen-

dously influential sketch for the generation of students, and junior faculty, who were passing through MIT. Footnote 4 of the paper in effect passes the torch to the younger generation: "My colleague, Professor Miguel Sidrauski, has independently arrived at such a dynamic formulation" (7). Samuelson is sketching an outline of the direction he hopes and expects the young tyros to follow.

The 1968 paper came to be known colloquially as Samuelson's "jackass" paper on account of its opening rhetorical gambit about a farmer in search of a lost animal who asks himself, "If I were a jackass, where would I have gone?" Samuelson identifies his own younger self as the jackass lost in classical monetary theory, while his current self looks back and reflects on where he had gone astray and how he had managed to find his way to the correct neoclassical monetary theory. Speaking mathematically, his mistake had been simply to append the quantity equation to a system of real supply and demand equations, rather than to integrate money more deeply into the analysis by taking explicit account of its real services in one of the real demand equations.

It was in reading Keynes, so Samuelson reflects, that he managed to find his way. No doubt. But the reference to Marschak (1943) hints at a further influence, perhaps a mediating influence. Indeed, the intellectual journey that Samuelson recalls for his young self is very much the intellectual journey that Marschak himself had followed, just a few years earlier, and in print.

In *Econometrica* Marschak (1934, 196) published the following system of equations:

$$q_i = \sigma_i\left(\frac{p_i}{w}\right) = \delta_i\left(p_1, p_2 \ldots w \cdot e\right),$$
$$\sum p_i q_i = w \cdot e = MV,$$
$$P = \lambda\left(p_1 \ldots p_n, q_1 \ldots q_n\right).$$

The first of these is more or less exactly the set of equations that Samuelson (1968, 3–4) reconstructs as the mistaken version of the classical system, that is, a set of supply σ and demand δ equations for each good *i* (that Samuelson labels *A′*), with a quantity equation appended to determine the absolute level of prices (that Samuelson labels *B′*).

Then, in 1938, Marschak published "Money and the Theory of Assets," which puts money in the utility function along with other assets, and proposes the following first-order conditions (*x* and *y* are moments of the statistical distribution of asset a and asset b, and *p* and *q* are their market prices):

$$\frac{p}{q} = \frac{U_x \frac{dx}{da} + U_y \frac{dy}{da} + \ldots}{U_x \frac{dx}{db} + U_y \frac{dy}{db} + \ldots}.$$

This is more or less exactly the condition that Samuelson (1968, 9, equation AIII) sets forth as the mathematical essence of the correct version of the neoclassical system.

In 1968 Samuelson makes clear that for him, at the most foundational level, the neoclassical synthesis is not about price stickiness (notwithstanding Modigliani) but more fundamentally about integrating dynamical considerations. By contrast to 1947, in 1968 Samuelson adds capital, labor, and land to the simple Walrasian exchange schema, and focuses attention on the steady state, which he thinks of as the end result of dynamic convergence. In that steady state the marginal productivity of capital determines the rate of interest (no longer exogenous), and the convenience yield on money is equalized to that rate of interest. As in 1947, there is no equation of exchange in the model, but nonetheless the model is homogeneous in the sense that doubling money and all prices (including asset prices) leaves real allocations unchanged. Notwithstanding this homogeneity, he concludes, "Correct neoclassical theory does not lead to the narrow anti-Keynesian view of those Chicago economists who allege that velocity of circulation is not a function of interest rates" (Samuelson 1968, 11).

A new claim in 1968, disarmingly introduced as nothing more than a digression, is that the steady-state equilibrium with money is suboptimal, since money is "a free good from society's point of view," and hence the social optimum is achieved only when money demand is satiated:

$$\partial U/\partial M = 0 \text{ instead of } r \times (\text{positive constant}) > 0$$

Says Samuelson (1968, 10; see also 1969): "Evidently we have here an instance of a lack of optimality of laissez-faire; there is a kind of fictitious internal diseconomy from holding more cash balances, as things look to the individual. Yet if all were made to hold larger cash balances, which they turned over more slowly, the resulting lowering of absolute price would end up making everybody better off." This new claim would almost immediately be countered by Milton Friedman in his famous essay "The Optimum Quantity of Money" (1969), with its audacious proposal for achieving the social optimum simply by engineering a steady deflation at the rate of interest. So far as I have been able to determine, Samuelson

never responded to Friedman's sally, leaving that task to others such as Hahn (1971), although he did respond to other critics (Samuelson 1969).

As for the larger challenge, Foley and Sidrauski (1971) soon produced *Monetary and Fiscal Policy in a Growing Economy* as a "revision and reconstruction of macroeconomic theory." This work, more than anything else, represents the high point of the MIT approach to monetary theory, integrating statics and dynamics, goods markets and asset markets, fiscal policy and monetary policy. But for all its many virtues, it simply sidestepped the looming foundational issues, leaving them for later. "Indeed, we still lack in 1972 a really adequate theoretical structure that encompasses the foundations of a money economy. On to the drawing boards!" (Samuelson 1972, 292).

But the necessary foundational undershoring never happened. Instead the 1970s saw not only the rise of Friedman's monetarism but also the beginnings of the new classical revolution, rational expectations, and ultimately real business cycle theory (Lucas 1972; Sargent 1976; Sargent and Wallace 1975).

Losing Control

Always in the history of monetary economics there is debate between those who start from state-issued fiat money and those who start from privately issued bank credit money. In the United States, the former starting point has typically dominated academic circles, while the latter starting point has typically dominated practical banking, including central banking, starting with the debate over the founding of the Federal Reserve (Mehrling 2002b). At MIT, both Samuelson and Modigliani belonged to the first camp (as also did Friedman at Chicago), which meant conceptualizing money as a kind of paper gold, an asset that is no one's liability, issued by the state through its proxy the central bank. Since there is no cost to producing it, the basic policy question is how the state should best manipulate the quantity it produces to maximize social benefit. Samuelson characteristically asked that question in the context of a steady-state intertemporal equilibrium, while Modigliani asked it in the context of a short-run unemployment equilibrium, but for both men the unit of analysis was the nation-state, and for both men the social optimum referenced the citizens of that nation-state.

From an international standpoint, however, state-issued fiat money is a promise to pay international reserves, much the same as bank money is

a promise to pay national currency, which is to say that it is a promise to pay gold under a gold standard, sterling under a sterling standard, or dollars under a dollar standard. Under the fixed exchange rate system established at Bretton Woods, the promise to pay was more or less explicitly the obligation of each national central bank to settle its official balance of payments in dollars, at the agreed rate of exchange. Nondollar currencies were thus clearly forms of credit. For our purposes, this institutional arrangement is important because it opened the door to the alternative tradition of monetary analysis that starts from bank credit.

At MIT, the keeper of this alternative tradition was Charles Kindleberger. Recruited to MIT in 1948, and only five years older than Samuelson, he was a generation older intellectually with a lifetime's worth of wartime adventure behind him (Kindleberger 1991; see also Meardon, this volume). An unreconstructed student of the indigenous American institutionalist tradition of monetary economics (see Mehrling 1997), Kindleberger had an international perspective informed by his experience with prewar monetary instability, which experience he had explored in his Columbia dissertation, "International Short-Term Capital Movements" (1937). For him, dollars were not a fiat currency issued by the state to its citizens but rather the short-term liability of the United States to the rest of the world. As world currency markets recovered in the aftermath of war, the United States was operating like a bank, providing liquidity to the world by borrowing short and lending long (Kindleberger, Despres, and Salant 1966).

A second source of dissent from the dominant academic view came from the emerging field of finance, which arose in concert with the postwar recovery of private capital markets domestically. Financial assets are forms of credit, promises to pay money at some point in the future. But they also have a current market price at which they are shiftable into current money. And some assets, for example, short-term money market assets, have prices that are sufficiently stable that the assets serve as good substitutes for bank deposits. From this point of view, not only bank liabilities but also all high-quality, short-term debts are forms of money, even if not formally means of payment. As private credit markets recovered, this institutional fact opened a second door to the alternative tradition of monetary analysis.

At MIT, the keeper of this second alternative tradition was Fischer Black (Mehrling 2005). Recruited to MIT from the University of Chicago in 1975 on the strength of his contributions to the pricing of financial derivatives (as

Black and Scholes 1973), Black always viewed macroeconomics as the biggest unsolved problem of finance. Lacking any formal education in finance or economics, he was introduced to the capital asset pricing model by his friend and colleague Jack Treynor, and used that model for the rest of his life as the intellectual framework for understanding the world around him. For him, dollars were not fiat currency issued by the nation-state but rather the short-term liabilities of financial intermediaries who stand between investors who want to hold less risk than the risky market portfolio (which they achieve by holding some of their wealth in bank deposits) and investors who want to hold more risk than the risky market portfolio (which they achieve by borrowing from the banking system to leverage their positions). This vision of a possible future world Black (1970) published as "Banking and Interest Rates in a World without Money," and then spent his life working to make that possibility an actuality.

Strange bedfellows, Charles Kindleberger and Fischer Black, but each one in his own way sensed a crucial dimension of the gathering forces that would sweep away the rigidities of the Bretton Woods apparatus put in place in the closing days of World War II, and replace it with a new world of financial globalization under a dollar standard that would rival the era of financial globalization under the sterling standard of the late nineteenth century. The intellectual edifice built by Samuelson and Modigliani was about a world passing away, even while Samuelson and Modigliani remained at the helm and continued to defend that edifice. Within MIT, Kindleberger adapted by refashioning himself as an economic historian before retiring in 1976 to write his best seller *Manias, Panics, and Crashes* (1978). Black bided his time until the financial revolution was sufficiently far advanced to have room for him, whereupon he left MIT for Goldman Sachs in 1984.

Kindleberger and Black both saw the future because they both understood a dimension of money that was simply left out of the standard models, both Keynesian and monetarist. They saw a common future, but they saw it from opposite sides. For Black, the important thing was the way that financial innovation was breaking down existing outdated institutions of national supervision, regulation, and management. For Kindleberger, the important thing was the way that globalization was challenging economists to develop new institutions for supranational supervision, regulation, and management. Both were right. The collapse of Bretton Woods was a vindication of Black, and the subsequent poor economic performance of the flexible exchange system was a vindication of Kindleberger.

Meanwhile, the task of incorporating the Kindleberger-Black perspective on money into the standard corpus of economic theory remains to be done. Echoing Samuelson, it could be said that we still lack a really adequate theoretical structure that encompasses the foundations of a money economy. Monetary Walrasianism, in both its Keynesian and its monetarist variants, has had its run. Perhaps it is now time to try something else. "On to the drawing boards!"

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