

# A Tale of Two Cities

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Recently socialist economists, for the first time applying to their problem tools of modern economic theory, found that a free market for consumer goods, and even for labor, is probably the safest guarantee for the best allocation of national resources to various productions (and to leisure), provided that the amount and use of national savings are determined by central bodies and that industries which naturally incline to a monopolistic or semi-monopolistic organization are entrusted to public managers.  
—Jacob Marschak, “Peace Economics” (1940)

It was quite a surprise, albeit a very pleasant one, to learn that my book *Fischer Black and the Revolutionary Idea of Finance* (2005) had been chosen to receive the ESHET award. I speak not from false humility, but rather from a realistic recognition that this book, along with my previous one, *The Money Interest and the Public Interest: American Monetary*

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*Thought, 1920–1970* (1997), represents an attempt to trace a peculiarly American line of intellectual development, a line with roots in American pragmatism and institutionalism. In all of this work I have been using the story of the development of American monetary and financial thought as a narrative thread through the quite incredible ups and downs of the twentieth century, a story of war and peace as well as boom and depression.

Within that larger story, the drama of money and finance is a tale of shifting boundaries, between public and private, between state and market, even between the Treasury and the Federal Reserve. I have tried to tell this story by following the effects of these shifting boundaries on the development of monetary and financial thought inside the academy. I use the word *effects* on purpose. I do not mean to deny the “performativity” of economic theory in the sense of MacKenzie 2006; economists, especially American economists, have always been engineers, looking to put their ideas into practice in the world. Rather I mean to direct attention to the fact that while the engineering impulse in economics is constant, the problems that direct the attention of that impulse are specific to each moment in time and arise from historical developments leading up to that time.

The biographical form that I have used in my work has thus been meant not so much to celebrate the way that the ideas of disembodied genius have changed the world but rather, and quite the reverse, to trace how economic thought develops by engagement with the concrete problems faced by each generation, an engagement that is both facilitated and obstructed by intellectual inheritance from past generations. I focus attention on the development of individual minds, even though my larger goal is to understand the development of economic thought more generally, because the task of understanding an individual is more delimited, and hence conceivably possible. But given that larger goal, the choice of which individual mind to enter is clearly crucial.

Reviewers of my first book sometimes objected to my pretense to be telling a story about American monetary thought, since I focused the story around Allyn Young, Alvin Hansen, and Edward Shaw. They asked, “What about Milton Friedman, James Tobin, Franco Modigliani?” Readers of my second book similarly sometimes objected to my pretense to be telling a story about American financial thought, since the book focuses on Fischer Black. “What about Harry Markowitz, Bill Sharpe, Merton Miller, Franco Modigliani, Robert Merton, Myron Scholes, and Paul Samuelson?” A convenient answer to this kind of question is that, on principle, I confine my attention to people who are safely buried. I used this convenient answer often! But the more honest answer is that I have felt the need to get some

distance from the people whom the *internal* processes of the academic discipline have already anointed, in order to see more clearly the effect of *external* influences on the development of thought.

I'm going to do that again today. I will be telling a story about the development of macroeconomics, but my central figure will not be Keynes, or Wicksell, or even Fisher or Kalecki. My story is new, and so necessarily only a sketch. My goal today is to tell a story that looks at a period you already know but through a different lens. My story thus complements the standard story, such as that of Roger Backhouse and David Laidler (2004), who use as their lens the role of Keynes's *General Theory* in facilitating transition from the diversity of interwar macroeconomics to the hegemony of the IS-LM model in the postwar period.

### A Tale of Two Equations

From an American perspective, the story of the development of macroeconomics is less about Keynes and more about the rise and fall of what I have previously called "monetary Walrasianism" (Mehrling 1997). The central texts of monetary Walrasianism include, among others, Franco Modigliani's "Liquidity Preference and the Theory of Interest and Money" (1944), Don Patinkin's *Money, Interest, and Prices* (1956), and James Tobin's "Liquidity Preference as Behavior towards Risk" (1958). I have previously suggested that a key foundational figure for the monetary Walrasian project was Jacob Marschak, insofar as his 1938 "Money and the Theory of Assets" set the agenda (Mehrling 2002 n. 12). Today I want to expand on that suggestion, both backward and forward in time.

My story is a tale of two equations. The first one is the quantity equation that Irving Fisher revived in 1911:

$$MV = \sum pQ. \quad (1)$$

In this equation  $M$  is the quantity of money,  $V$  the velocity of money,  $p$  the price of various goods sold, and  $Q$  the quantity of various goods sold. The left-hand side captures the turnover of money balances, while the right-hand side captures the exchange of goods. In 1911, Fisher was concerned mainly with how  $M$ ,  $V$ , and  $Q$  interact to determine the price level and its fluctuation over time. The quantity theory of money, for example, uses the quantity equation to talk about the relationship between  $M$  and the price level. But the same framework was used subsequently, by Fisher and others, to analyze the determination of  $Q$  and its fluctuation over time. Just so, in my book on the development of American monetary economics,

the quantity equation provides the framework for discussion by just about everyone.

The second equation is the Euler equation that lies at the heart of both modern finance and modern macroeconomics:

$$U'(C_{it}) = E_t[\delta U'(C_{it+1})R_{jt+1}]. \quad (2)$$

In this equation  $C_{it}$  is the consumption of consumer  $i$  at time  $t$ ,  $U$  is a function that translates consumption into utility terms,  $\delta$  is the subjective discount rate, and  $R_{jt+1}$  is the gross return on asset  $j$  in the period between  $t$  and  $t + 1$ . For finance, this equation is about how asset prices depend on time and risk preferences, the equation is called the “consumption CAPM,” and the asset in question is typically equity or long-term bonds (Breedon 1979; Cochrane 2001). But the same equation can be used to talk about the intertemporal fluctuation of income, and as such is at the core of both real business cycle theory and its New Keynesian variants (Woodford 2003; Sargent 2008). In this application, the asset is typically capital, or a rate of interest.

I submit to you that a very large intellectual revolution is involved in moving from the first equation to the second. The most obvious change is a shift from money to finance, and from the quantity of money (or aggregate demand) to the rate of interest as the relevant policy instrument. This already is a huge *substantive* change, from the city of money to the city of finance, from the public Board of Governors in Washington to the private securities exchange in New York. In what follows, however, I will be focusing attention on three far-reaching *methodological* changes involved in the shift from the first equation to the second.

1. Risk. The second equation incorporates risk explicitly insofar as the realization of the gross asset return is stochastic; hence, the expectations operator  $E[.]$ .
2. Equilibrium. The second equation is a first-order condition characterizing individual optimization taking price as given, but also possibly characterizing economy-wide equilibrium in an endowment economy taking price as the equilibrating factor.
3. Time. The second equation incorporates time explicitly insofar as the risk that is important for individual choice has to do with the future time period  $t + 1$  when asset returns will be realized.

In all three respects, we have clearly come a long way from Fisher 1911. The distance we have traveled has, however, been obscured by the tendency of the profession to read something like equation 2 back into Fisher, which is to say the tendency to read modern economics as a natural exten-

sion of Fisher's pioneering work. James Tobin (1985) more than anyone else is responsible for this reading, but there have been plenty of historians of thought writing in support of the story of continuity from Irving Fisher. For my purposes, it is more important to emphasize the elements of discontinuity.

I have elaborated this argument in more detail elsewhere (Mehrling 2001), so a single quotation will suffice to make the point here. Here is Fisher, writing in 1930:

While it is possible to calculate mathematically risks of a certain type like those in games of chance or in property and life insurance where the chances are capable of accurate measurement, most economic risks are not so readily measured. To attempt to formulate mathematically in any useful, complete manner the laws determining the rate of interest under the sway of chance would be like attempting to express completely the laws which determine the path of a projectile when affected by random gusts of wind. Such formulas would need to be either too general or too empirical to be of much value. (316)

This passage reveals an attitude toward the “dark forces of time and ignorance”—the phrase is from Keynes in the *General Theory*—not unlike that of Keynes himself. Like Keynes in his *Treatise on Probability* (1921), and also like Frank Knight in his *Risk, Uncertainty, and Profit* (1921), Fisher thought that the mathematics of risk was *not* an appropriate analytical framework for problems of intertemporal choice, much less intertemporal general equilibrium. Fisher thus explicitly *rejected* the line of analytical development that leads to equation 2. But if Fisher didn't do it, then how did we get to equation 2? My central argument will be that monetary Walrasianism served as the bridge that carried us from equation 1 to equation 2. And the key figure in understanding monetary Walrasianism, I suggest, is Jacob Marschak.<sup>1</sup>

### Jacob Marschak

Marschak himself started with Irving Fisher and the equation of exchange, in his PhD thesis “Die Verkehrsgleichung” (1924).<sup>2</sup> According to his own

1. For the supporting arguments on risk and time, I am indebted to the work of Elton McGoun (2007) and Christian Walter (2006), who got me started on the right track.

2. Arrow 1979 provides a biographical sketch, but see also Hagemann 2006, 2007. Marschak was born in 1898 in Kiev, Ukraine, earned his PhD at the University of Heidelberg in 1922, and spent the 1920s at the Kiel Institute for World Economics. His first major publication (1923)

testimony (Marschak 1974, 3:3), there is a straight line of intellectual development from that early paper to the mature statement of monetary Walrasianism in his 1950 paper “The Rationale of the Demand for Money and of ‘Money Illusion.’” Thus a story of continuity from equation 1 to equation 2 works for Marschak even if not for Fisher. But that is getting ahead of the story. To begin, it is important to emphasize that Marschak’s initial development of the quantity framework involved integrating the theory of money not with the theory of value—that would come later—but rather with the theory of capital.<sup>3</sup>

The high-water mark of that early work is achieved in Marschak’s book *Kapitalbildung (Capital Accumulation)*, which contains the results of an extensive research project with Walther Lederer at the Kiel Institute for World Economics (Marschak and Lederer 1936; see Goldschmidt 1938). The publication of the book was delayed because of political developments in Germany (see Hagemann 2007), but we can see the essential analytical framework in a paper Marschak published under the intriguing title “Econometric Parameters in a Stationary Society with Monetary Circulation” (1934a). This publication coincided with Marschak’s departure from Germany for a position at Oxford University and hence can be considered a kind of inaugural address for his new English-speaking colleagues.

In this paper Marschak is interested in how the vertical organization of industry, by subsuming much of the exchange of intermediate goods within the boundaries of the firm, increases the efficiency of monetary circulation. The whole point of the exercise is to derive an analytical framework that can be taken to the data, in this case the German data for the late 1920s. Marschak is not looking to test any particular economic theory, but rather to use economic theory to suggest mathematical equations that he can use to characterize empirical regularities in the data. That is what Marschak thought the econometric project was all about.

In conjunction with his move to Oxford, Marschak attended the meetings of the Econometric Society, held in Leiden in September–October 1933, and he wrote up the proceedings for publication. His account of his

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engaged von Mises on the socialist calculation debate. In 1933 he left for Oxford University; from 1935 to 1939 he served as director of the Oxford Institute of Statistics, from 1939 to 1942 he was at the New School for Social Research, and from 1943 to 1948 he was director of the Cowles Commission for Research in Economics. McGuire and Radner 1972 is a Festschrift for Marschak, and Marschak 1974 contains his collected works.

3. This intellectual project he shared with Arthur Marget (1938), among others.

own paper, “Theoretical Problems Suggested by Roosevelt’s Policy,” provides key evidence of his intellectual framework as of that date. The important equations are as follows (Marschak 1934b, 196):

$$q_i = \sigma_i \left( \frac{P_i}{W} \right) = \delta_i (p_1, p_2 \cdots w \cdot e),$$

$$\sum p_i q_i = w \cdot e = MV,$$

$$P = \lambda (p_1 \cdots p_n, q_1 \cdots q_n).$$

As in his earliest work, the centerpiece is the equation of exchange, now written right to left. He has added, however, the idea that the quantities  $q$  in the equation are the result of a supply  $\sigma$  and demand  $\delta$  relationship for each good  $i$ , and he is clearly prepared to use the standard apparatus of production functions and utility functions to help organize thinking about the form of those supply and demand functions.<sup>4</sup> Thus, in effect, he has a quantity equation in center place, with a Walrasian system appended.

At the 1933 meeting, Marschak’s main concern was to use that analytical apparatus to point out the possible inconsistency of various elements of Roosevelt’s economic plan for the United States. Roosevelt was proposing policies to affect certain individual prices, wages, quantities, purchasing power, and the price level. The point of Marschak’s analytical framework is to show that there is a *logical* relationship between all these elements that has apparently not been recognized by the policy makers themselves. This kind of contribution to policy analysis is also what Marschak thought the econometric project was all about.

These early papers allow us to characterize Marschak’s intellectual formation independent of the later work for which he is best known. In his intellectual formation, he was a kind of proto-econometrician, pragmatically using both economic theory and statistics to characterize regularities in the data. But the purpose of these studies was always to provide the basis for economic management, both microeconomic and macroeconomic. In microeconomics, the big problem was monopoly and the solution was public ownership and management, but not any further intervention into market processes. In macroeconomics, the big problem was instability, and the solution was monetary management. These are the problems to which the “econometric movement”—as Kenneth Arrow (1979, 502) calls

4. A key step toward this Walrasian formulation is apparently Marschak’s 1931 habilitation thesis on the elasticity of demand.

it—was supposed to be providing the solution, and we must understand Marschak’s work in that context.<sup>5</sup>

From this standpoint, we can understand Marschak’s “Money and the Theory of Assets” (1938) not as a new departure but rather as continuous with his previous thought. Whereas previously he had a quantity equation framework with a Walrasian system *appended*, from 1938 on he would have a Walrasian system with a money demand equation *incorporated*.<sup>6</sup> Marschak assumes that utility depends on the moments  $x$ ,  $y$ , and  $z$  of the distribution of consumption yields generated by various assets, and proceeds to derive a first-order condition for the consumer optimum. If the price of asset  $a$  is  $p$ , and the price of asset  $b$  is  $q$ , then we have the following (p. 316):

$$\frac{p}{q} = \frac{U_x \frac{dx}{da} + U_y \frac{dy}{da} + \dots}{U_x \frac{dx}{db} + U_y \frac{dy}{db} + \dots}, \text{ (marginal productivity equations).}$$

Here we see the first pier of the bridge between equation 1 and equation 2.

The immediate impetus for Marschak’s formal approach to the problem seems to have been Hicks’s famous “Suggestion for Simplifying the Theory of Money” (1935), which proposed to open the field of money to the apparatus of supply and demand. Equally important, however, was the earlier work of Hicks that urged fellow economists to overcome their fear of using the mathematical apparatus of probability and risk. As early as 1931, Hicks had urged rejection of Frank Knight’s philosophical objections to employing the concept of risk, mainly on pragmatic grounds. Marschak would have encountered Hicks’s views at the 1933 meeting of the Econometric Society, if not before, as Hicks presented a paper titled “The Application of Mathematical Methods to the Theory of Risk.” Following Hicks, what Marschak did in 1938 was, in a single stroke, to incorporate both risk and (portfolio) equilibrium into the quantity equation framework, but not yet time.

5. Arrow’s impression that Marschak’s contribution was largely synthetic tends to miss Marschak’s preexisting capacious analytical framework within which it was possible to find room for narrower contributions of many different kinds. Similarly, his impression that Marschak changed his economics to fit the changing environments within which he worked tends to miss the continuity of his thought.

6. The early history of money demand theory, a missing chapter in Mirowski and Hands 2006, remains to be written. Those interested in tracing Marschak’s own development on this point will want to consider, in addition to the endpoints Marschak 1938 and Marschak 1950, Makower and Marschak 1938 and the intermediate steps (Marschak 1943, 1949).



It is important to emphasize that Marschak apparently saw this emergent monetary Walrasian construct as completely compatible with the work of Keynes. For himself, he still preferred to think and talk in terms of money and purchasing power, rather than aggregate demand, but more for the sake of analytical clarity than anything else (see for example Marschak 1942b). Knut Wicksell, after all, had shown how an excess of investment over saving could be understood as the same thing as an excess supply of money (Marschak 1941b). In this sense there is a straight line from Marschak's original (1934a, 1934b) quantity-theoretic econometric project to the neo-Walrasian econometric project mooted in Marschak 1942a and Marschak and Andrews 1944, the papers that are usually treated as Marschak's contribution to the Cowles econometric modeling effort.<sup>7</sup>

It is important also to emphasize that Marschak apparently saw this monetary Walrasian construct as completely compatible with the work of the American institutionalists. Marschak's "Methods in Economics" (1941a), which reviews a discussion of the work of the institutionalist Frederick C. Mills, is clearly a precursor to Tjalling Koopmans's famous "Measurement without Theory" (1947), but with much more sympathy for the institutionalists. (See also Marschak [1951] 1974, his "Comment on Mitchell.") Like the institutionalists, Marschak's project was to a large extent about "social engineering," about understanding the world in order to make it better. For this goal, he insisted, it is vital to avoid the "ravages of methodology" (1941a, 441) and to use pragmatically whatever tools are available to work toward the goal.

In his "Cross Section of Business Cycle Discussion" (1945) Marschak was even more pedagogically explicit, urging young folk "not to despise God's gift of equations," which can be used to integrate knowledge coming from economic theory and knowledge coming from statistical measurement, in the manner of Tinbergen 1939.<sup>8</sup> Keynes famously urged his audience to dream of a possible future world when economists would be about as important as dentists. Marschak held out such a humble role as a possible life choice for his own students in the here and now. As it happened, many found such a role attractive, and the rise of monetary Walrasianism followed the career trajectory of those students.

7. It follows that Marschak 1934a and 1934b should be viewed as part of the prehistory of econometrics that the standard story (Morgan 1990) treats as beginning with Trygve Haavelmo.

8. Marschak and Lange [1940] 1995 provides a defense of Tinbergen that Keynes blocked from publication in the *Economic Journal*.

## The Second Generation

The main themes of Marschak's monetary Walrasianism were thus all in place by 1945, if not earlier. What remained to do was to shore up the analytical foundations of the project on the one hand, and to develop the policy applications on the other. A key step showing the way forward on the first front was von Neumann and Morgenstern's work to show how rational choice under risk could be understood to imply the existence of an implicit von Neumann–Morgenstern utility construct (Marschak 1946). Marschak himself took the opportunity to reformulate his 1938 paper as Marschak 1950, which therefore qualifies as a second pier in the bridge between our two equations. But he was content to leave the rest of the work to the broader econometric movement. For this purpose, no one was more important than the young Kenneth Arrow.

As early as 1941, Arrow had written his master's thesis on stochastic processes under Abraham Wald and Harold Hotelling at Columbia University. His comfort with the mathematics of probability subsequently proved valuable for the war effort, most notably in his paper "On the Use of Winds in Flight Planning" (1949), which was only published after the war.<sup>9</sup> In this paper, Arrow solved exactly the kind of problem that Irving Fisher in 1930 had held out as obviously insoluble; Fisher's projectile becomes Arrow's airplane. If Fisher was wrong about that problem, then maybe the problem of intertemporal choice under uncertainty was not so obviously insoluble either. We can understand Arrow's subsequent work as an attempt to explore just such a possibility, first with the microeconomics of choice under risk (1951), then with the general equilibrium problem (1953). Here we have the third pier in our bridge, because here we finally have an explicit treatment of time.

Hicks had already introduced in *Value and Capital* (1939) the idea of extending the well-known static model of general equilibrium to incorporate both risk and time, by the simple stratagem of treating commodities at different times and in different states of the world each as a distinct commodity with its own distinct price. In 1953 Arrow showed that a complete set of state-contingent *securities* markets can play exactly the same role as a complete set of commodity futures markets, in the sense of implementing the same allocation.

Arrow 1953 can thus be read as a more formal foundational version of Marschak 1950. Money itself is behind the scenes as a security that pays

9. Here is another example of the spillover of the "protocols of war" into economics (Klein, forthcoming).

in every state. Like the earlier papers of Marschak, Arrow's intertemporal general equilibrium model was supposed to be the analytical foundation on which more policy-oriented work could be built. The Fed-MIT-Penn econometric model (Ando and Modigliani 1969) can thus be seen as the ultimate realization of Marschak's vision, as can also Tobin's "General Equilibrium Approach to Monetary Theory" (1969). The high-water mark of monetary Walrasianism was also the high-water mark of American Keynesianism.

This sketch of the rise of monetary Walrasianism begs the question why it ultimately fell. What happened? An article by Robert Lucas, "An Equilibrium Model of the Business Cycle" (1975), will serve to set the scene. Lucas takes as his text a Tobin-style monetary growth model, which he finds inadequate in ways that have become well known: "On the one hand, it is easy to postulate agents and market institutions which ignore or foolishly waste information: the result is a theory which seriously understates agents' abilities to vary their decision rules with changes in the environment (such as, for example, the theory underlying the major econometric forecasting models [to wit, the FMP model])" (1138). For our purposes the important bit is the sentence that follows: "It is equally easy to postulate 'efficient' securities markets which rapidly transmit *all* information to all traders: the result is a static general equilibrium model."

Here Lucas seems to have in mind a model like that of Arrow 1953, which, notwithstanding the time and state subscripts on the commodities, he interprets as hopelessly tied to a static and certain world. Prices in the model are established once and for all at the start of time, and time involves nothing more than selecting which contingent branch to follow at each fork. The problems of risk and time have thus not in fact been addressed by the intertemporal general equilibrium model, as Arrow himself would agree. "The existence theorem for general intertemporal equilibrium can be taken as a proof that perfect foresight is at least a consistent theory" (Arrow 1978, 159). The analytical foundations of monetary Walrasianism turned out not to be so secure as had been thought.

Lucas's reference to "'efficient' securities markets," which conflates a literature in finance (Fama 1965, 1970) with the literature on intertemporal general equilibrium in economics, inadvertently points the way to the future. Already proving foundational for the emerging new field of modern finance, the idea of efficient markets would become the foundation also for a new direction in macroeconomics. One of the ironies of history is that it was Paul Samuelson—a Keynesian but never a monetary Walrasian—who got the ball rolling to clarify the idea of efficient markets by connecting it

to a particular kind of stochastic process known as a martingale (Samuelson 1965, 1973, 1976, 1984).

Samuelson has often told the story about how his thinking on these matters was sparked by a postcard from Jimmie Savage at the University of Chicago that drew his attention to the early work of Louis Bachelier.<sup>10</sup> Bachelier had assumed that security prices follow a Brownian motion and had derived various formulae from that assumption. Samuelson's contribution was to show that efficiency meant that security prices, suitably adjusted, would follow a martingale, which is to say that expected future price, suitably discounted, is equal to the present price. The key to Samuelson's proof was to tease out the implications of the idea that the expected value of a pure speculation must be zero.

In all of these papers, Samuelson worked with an exogenously fixed discount rate, and he was only ever thinking about asset prices, not at all about macroeconomic fluctuations. In a more general economic model, however, the discount rate should be endogenous and should fluctuate with the economy as a whole. That is the model of Lucas's "Asset Prices in an Exchange Economy" (1978), in which he derives the Euler equation that has come to orient modern macroeconomics.<sup>11</sup> He writes it like this (p. 1434):

$$U'(\sum_j y_j) p_i(y) = \beta \int U'(\sum_j y'_j) (y'_i + p_i(y')) dF(y', y).$$

Lucas himself did not go so far as to treat this equation as the centerpiece of a real business cycle theory—he confined himself to an exchange economy—but his students did. This is the sense in which modern macroeconomics comes from the theory of efficient markets.

Indeed, in a sense the fundamental problems of finance and macroeconomics are very much the same: both seek mechanisms for controlling the dark forces of time and ignorance, and for that purpose both adopt

10. Two decades earlier, Arrow (1941) had included a discussion of Bachelier in his master's thesis: "Bachelier used probabilities continuously changing in time to treat the theory of speculation on exchanges. Both simple operations and options are considered. The mathematical expectation of the speculation is taken to be 0." Thus, Arrow was studying martingales uptown at Columbia University at exactly the same time that he was attending Marschak's seminar downtown at the New School for Social Research.

11. See also LeRoy 1973. Both Lucas and LeRoy conclude from their analysis that there is no reason to suppose that the martingale property will necessarily be a feature of efficient asset market prices. In other words, Samuelson's results were a special limiting case. The martingale property was subsequently rehabilitated in finance by shifting attention to risk-neutral pricing under a "martingale equivalent" probability measure (Harrison and Kreps 1979).

quite similar conventions of modeling the dark forces as if they followed a well-defined stochastic process. The problem of both is therefore conceptualized as a problem of risk control. Finance is concerned with individual control of portfolio risk; macroeconomics is concerned with social control of aggregate risk. Foundations having been established, subsequent developments in both finance and macroeconomics have been driven by the fact that, empirically speaking, the Euler equation around which both fields organize themselves is simply an embarrassment (Lettau and Ludvigson 2009).

In finance, the practical response to empirical failure was to write the Euler equation as

$$1 = E_t[M_{it+1}R_{jt+1}], \quad (3)$$

where  $M$  is a stochastic discount factor treated as a free variable that must fit the cross-section pattern of asset returns.<sup>12</sup> It follows from 3 that

$$0 = E_t[M_{it+1}(R_{jt+1} - R_{jt+1})].$$

John Cochrane (2001) emphasizes that such a form is especially useful for practitioners whose interest focuses on relative asset prices. In this formulation, we simply take the time and risk premia implied by the data on some asset prices and use them to calculate the price of some other assets. We do not ask where the premia come from, although we might well develop more or less elaborate statistical models to help us characterize them.

In macroeconomics, the practical response to empirical failure was essentially the reverse, to abandon asset pricing consequences in order to focus on macroeconomic variables. To achieve this, the asset in the Euler equation has generally been treated as simply an interest rate, and the Euler equation has been interpreted as the IS curve in a larger macroeconomic model. Because the gross return in the Euler equation is a real return, while the interest rates we see in the real world are nominal, there is implicitly an expectation of price inflation involved (the AS curve), as well as an expectation of nominal interest rates (the Taylor rule, which replaces the LM curve of the Tobin-era models).

Michael Woodford (2001, 232) writes the IS curve version of the Euler equation as

$$y_t = E_t y_{t+1} - \sigma(i_t - E_t \pi_{t+1}) + g_t.$$

12. Under the martingale equivalent measure, equation 3 can be written as  $1 = E_t^M[R_{jt+1}]$  or  $P_t = E_t^M[P_{t+1}]$ .

In this equation,  $y$  is log GDP,  $i$  is the nominal interest rate, and  $\pi$  is the rate of inflation, so the difference in parentheses is the prospective real rate of interest, and  $g$  is an exogenous disturbance (introduced in part to mop up the empirical failure of the Euler equation). We are encouraged to think about this equation as a linearized version of the intertemporal Euler equation, where the parameter  $\sigma$  captures the intertemporal rate of substitution. Woodford emphasizes that such a form is especially useful for practitioners whose interest focuses on monetary policy, which is understood here to be about setting the optimal parameters of the rule driving the nominal rate of interest.

### Conclusion

The life and work of Jacob Marschak make clear that the econometric movement was, at least in part and at its inception, about building the analytical capacity to implement a kind of market socialism. For that purpose the economy was viewed as a kind of multidimensional stochastic process whose fluctuations had proven to be unacceptably violent. Economic policy intervention was obviously required, but effective intervention would require much more detailed knowledge of the underlying stochastic process. Therefore, philosophical objections to the use of the mathematics of risk for economic problems had to be put aside. A probabilistic risk model, although likely a poor approximation of reality, could not be a worse approximation than a certainty model, and anyway rational thought about pressing economic problems could only be helped by bringing out into the open the implicit assumptions underlying trained intuition. The goal was simply to use any and all resources to defeat the dark forces of time and ignorance.

Of course, the government is not the only one trying to defeat the dark forces, as Lucas (1976) pointed out in his famous critique. Households and firms too can be presumed to be steering toward their own chosen targets in the face of their own stochastic challenges, and it is not obvious a priori that governmental intervention offers anything more than an additional stochastic challenge for individual agents to overcome. Indeed, in this respect, the whole development of modern finance can be understood as offering an image of one sector of the economy where the interaction of individual portfolio allocation decisions arguably achieves an efficient result without governmental intervention of any kind. This strong

result clearly rests on the maintained hypothesis that the dark forces can be adequately modeled as a well-behaved stochastic process, which is certainly questionable (Arrow 1981).

In *both* cases, both the project of the econometric movement and the project of modern finance, the ultimate objective was to develop practical methods of risk control. For that purpose, again in *both* cases, it seemed acceptable to abstract from the more intractable features of the problem in order to make some progress. In effect, finance and macroeconomics have both adopted specific conventions for treating the problem of the dark forces. There is nothing in principle wrong with that, nor indeed anything particularly new.<sup>13</sup>

What is more worrisome is the fact that finance and macroeconomics have adopted *different* conventions, and conventions moreover that are deeply inconsistent with one another, and this despite their mutual embrace of the very same Euler equation. When we remember that these inconsistent conventions are now deeply embedded in the institutional structures underlying our dual economy of risk control, we see the possibility that economic events can develop in ways that make it impossible for both conventions simultaneously to adapt smoothly to a changing underlying risk environment.

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13. See Keynes 1936, chap. 12: “In practice we have tacitly agreed, as a rule, to fall back on what is, in truth a *convention*. The essence of this convention . . . lies in assuming that the existing state of affairs will continue indefinitely. . . . We know from extensive experience that this is most unlikely. . . . Nevertheless the above conventional method of calculation will be compatible with a considerable measure of continuity and stability in our affairs, *so long as we can rely on the maintenance of the convention.*”

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