Cowles and the Tradition of Macroeconomics

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Introduction

My task is to survey the main currents of macroeconomic thought — including monetary and financial economics — as they have been pursued within the Cowles Commission and Cowles Foundation. It sounds straightforward enough, a bit daunting maybe, but straightforward. An ambiguity arises, however, as soon as one starts to make the necessary distinctions. What is it that distinguishes macroeconomics from other parts of the subject? Is Marschak's "The Rationale of the Demand for Money and Money Illusion" or Tobin's "Liquidity Preference as Behavior towards Risk" macroeconomics or is it not? It is always possible to adopt Justice Stewart's line about pornography: I may not be able to define it, but I know it when I see it. In fact, I think that approach would work well with macroeconomics too. Nevertheless, this is one definitional question whose trail leads to interesting matters of substance, so I shall follow it a bit further.

Perhaps the simplest definition would be the literal one: macroeconomics is aggregative economics, theoretical and applied work in which the primitive entities are sums and index numbers. There is plenty of that sort of thing in the Cowles Commission canon; Lawrence Klein's first attempts to build and estimate complete models of the economy are part of macroeconomics if anything is part of macroeconomics. But that definition starts to fail as soon as we apply it to macroeconomic theory. By the literal definition, Oscar Lange's *Price Flexibility and Employment* is not a work of macroeconomics. Its fundamental objects are the excess demand functions for *n* commodities. It is true that these are already aggregated over families and firms; but absolutely nothing in the book would be changed if every excess demand were rewritten as the sum of an arbitrary number of individual agents' net demands for goods and services. There is no doubt in my mind, however, that Lange's book should be classified as a contribution to macroeconomics; and it is clear that he intended it that way.

It is no accident that this question of definition becomes acute precisely when one turns to theory. Everyone will understand what I mean when I say that, in a sense, there is no such thing as macroeconomic theory. The basic structure of economic theory rests on a strictly microeconomic foundation. The most dyed-in-the-wool macroeconomist, when asked to justify some assumption or line of argument, will almost certainly respond by showing how it might arise from the choices of individuals and the interactions of individuals in the market. But then it is not at all clear what particular role macroeconomic theory can have or why it needs to be pursued at all. Casual thinkers will have no trouble soothing their consciences with thoughts about simplicity or transparency or ‘‘as if,” but a group like the Cowles Commission/Foundation with its explicit devotion to rigor must find coexistence discomforting.

Now let me try it from the other side. I once heard Nicholas Kaldor say — in conversation, I believe — that macroeconomics is the part of the subject in which everything you learned in school is wrong. That way lies paradox, of course, but Kaldor had a useful point in mind. Macroeconomics is in practice usually the study of system-wide pathology: the business cycle, unemployment, inflation. The practicing macroeconomist often must deal with situations that do not-more strongly, cannot — arise in the economics learned in school. To a rigorous economist this can only mean that the assumptions underlying the traditional theory do not always apply, and therefore neither does the theory one learned in school. For discussing the economy as a whole, the only theory we learned in school is Walrasian general equilibrium theory. Common observation then leads to the realization that the assumptions of Walrasian theory and the accompanying Arrow-Debreu equilibrium concept are not very suitable tools for understanding many important macroeconomic events.

A natural reaction for a rigorous economist would be to go back to the microeconomic foundations and try to reconstruct system-wide theory in a way that could give a reasonable account of those everyday pathologies. There have been some attempts in that direction, associated with the names Drèze, Bénassy, Malinvaud, and others. As it happens, little or none of this work has been associated with the Cowles Foundation although the work of Don Patinkin provided an opportunity that was not grasped. (There is an alternative reaction. It is possible to invent artful dodges that allow one to cling to the Walrasian model and still "in principle" accommodate the gross macroeconomic facts, or some of them, though not very well, and not without straining our credulity. If the Cowles Commission had stayed in Chicago, I might be obliged to discuss that line of thought.)

I could define macroeconomics as non-Walrasian economics. But I am not prepared to leave it at that, and there is warrant for my reluctance in the history of the subject and in the history of the Cowles Foundation itself. It must be inevitable that an academic discipline whose subject matter includes the major pathologies of the business cycle will be asked to study particular business cycles, the current business cycle for example, and to say something useful about them, or it. (Something useful is not necessarily the same thing as something prescriptive, but it is not a large step from one to the other.) Macroeconomics has been a practical subject from its beginning. Perhaps it would be enough to define macroeconomics as applied — or at least applicable — non-Walrasian economics. But applied system-wide economics is in practice aggregative economics, with few exceptions.

I am back where I started. Macroeconomics is aggregative economics, especially aggregative general "equilibrium" economics. (I put "equilibrium" in quotes because I do not mean to limit the word to price- mediated market clearing.) In explaining the logic and asserting the plausibility of a relation between aggregates, we will almost always want to show how it could arise from the interaction of individual agents. Microeconomic arguments designed to do this are indeed part of the "microfoundations of macroeconomics." Those foundations will often have to be non-Walrasian if they are to make sense out of observation. Whether in any particular instance it is better to seek Walrasian or non-Walrasian foundations is part of the "macrofoundations of microeconomics."

I shall discuss the place of Cowles in the development of macroeconomics in chronological fashion. First, in the next section, I describe developments in the Chicago years of the Cowles Commission. Then, I discuss the New Haven years of the Cowles Foundation. I conclude with some general remarks that were stimulated by my review of Cowles's contributions to macroeconomics.

The Chicago Years

In the Chicago phase, there was no identifiable "Cowles Commission approach" to macroeconomic theory. In contrast, in the sphere of the econometrics of simultaneous economic relations there was a "movement" of which the Cowles Commission was the headquarters. I can remember when simultaneous-equations methods were referred to in conversation as "Cowles Commission methods," though it goes without saying that there were roots elsewhere. In the sphere of microeconomic theory, to take another important example, Cowles was one epicenter of research on activity analysis, primarily through the work and influence of Tjalling Koopmans.

So far as macroeconomic theory is concerned, however, the situation was different. There was no identifiable in-house view to speak of. If particular ideas were associated with the Cowles Commission it was more nearly the accidental result of the fact that this or that scholar was domiciled at Cowles at this or that particular time and less the manifestation of a local school of thought. One can discern intellectual lineages, of course. Don Patinkin's first appointment as a research assistant at Cowles came in 1946, a year after Oscar Lange had departed to become Poland's first ambassador to the United Nations. I presume that Patinkin, in his student days, heard Lange's lectures at the University. (Evsey Domar assures me that Lange was a supernaturally clear lecturer. Any reader of Lange's papers will find it easy to believe.) That is enough to account for the relation of some of Patinkin's early work to Lange's. It still does not amount to an incipient school of thought.

There is one partial exception to the individualistic view. The Cowles Commission could hardly keep from becoming the first center in the United States for the building of complete macroeconomic models. Given Koopmans's earlier association with Tinbergen, the ongoing work on estimation of simultaneous systems, and, finally, the arrival of Klein in 1944, the brand-new author of a Ph.D. thesis on the Keynesian Revolution, the rest was all but inevitable. Today one would hardly think of the building and estimation of complete econometric models as the ideology of a school. But one only needs to look back at Koopmans's (1947) review of Burns and Mitchell, and his ensuing (1949) methodological exchange with Rutledge Vining, to realize that raw nerves were being touched.

To students of my generation, it was clear that Koopmans was carrying a flag, consciously and conscientiously trying to establish the econometrically estimated complete model as a scientifically superior alternative to older methods of business-cycle research. The "older methods" had two parts. The first was a kind of analytical-verbal-statistical storytelling, not unattractive or unenlightening in the hands of a master, but pretty clearly self-limiting as a step on the way to a reproducible "science." The second strand was the mechanical, more or less natural-historical, numerical processing of a vast array of time series, as exemplified in Burns and Mitchell's *Measuring Business Cycles*, a review of which was the occasion for Koopmans's manifesto.

This methodological debate, which only occasionally broke into print, was widely seen as a clash between two institutions: the Cowles Commission and the National Bureau of Economic Research, but of course it was much more than that. The victory of the model-building school was complete; on this score there is by now no perceptible difference between the Cowles Foundation and the National Bureau. I do not count Robert Lucas's revival of the Burns–Mitchell interest in the natural history of covarying time series as a blow in this old battle, any more than Darwin's interest in the variations within and between species was antipathetic to the ideas of population genetics. The methods proposed by Lucas and Sargent for dealing with time series are nothing if not probability-based model building. The approach via richly parameterized vector autoregressions sponsored by Christopher Sims is more nearly another round in the methodological spiral. It is, like the Burns–Mitchell procedure, explicitly a theoretical. But it is too soon to know what will come of it.

Lange and Patinkin

I return to macroeconomics in the Chicago days, and I start with Lange. Price Flexibility and Employment was an important book for my generation of graduate students. It did not count as much as Value and Capital or The Foundations of Economic Analysis even then; and it has not worn nearly as well. But at the time it seemed more important than its later eclipse would suggest. It held out the promise of using the Hicksian version of general equilibrium theory and the dynamics of Samuelson to say things of absolutely central importance to macroeconomics.

Begin with an economy in full equilibrium and imagine one market suddenly perturbed into excess supply: a former hermit joins the labor force, or the weather changes in a way that increases the yield of apples. This is a monetary economy, so that the nominal wage or the money price of apples falls. That will tend to restore equilibrium in the labor or apple market; but other markets will be disequilibrated and so other prices will change. Suppose all money prices are flexible and respond to excess supply or demand in the natural way. Will the economy return to full equilibrium, having successfully absorbed the initial excess supply of labor or apples? That was Lange's question, and you can see why I classify it as macroeconomics. Keynes had insisted that the answer was No.

We will all recognize this as a question about the stability of equilibrium, and so does Lange: that is the title of his mathematical appendix, which was issued separately as a Cowles Commission Paper. In the text of the book, however, he chooses to make his argument verbally. (Milton Friedman called Lange's method "verbal mathematics" in a review that I will discuss later. It made for a forbiddingly repetitive text that was very hard reading.) Here is the heart of Lange's argument. Suppose the equilibrium is unique. "Let the prices of all factors and products change in the same proportion. There will be no substitution of one factor for another or of one product for another, nor will there be any change in output because product prices change in exactly the same proportion as factor prices. Only such changes in the demand or supply of any good are possible as are due to a desire to substitute goods for money or vice versa." If there is what Lange calls "a neutral monetary effect" — if a proportional change in all nominal prices causes a proportional change in the excess demand for money in the same direction — then all-around deflation will not eliminate the original excess supply of labor or apples. If the monetary effect is positive, if all-around deflation reduces the excess demand for money more than proportionally, thus generating a shift of excess demand to goods, price flexibility will be able eventually to restore full employment. If the monetary effect should be negative, then deflation only widens the gap and price flexibility is destabilizing. Notice that the monetary effect depends both on the demand for money and its supply. A system that holds the nominal quantity of money constant in a deflation is strongly stabilizing.

This will remind the modern reader instantly of the Pigou effect. Time flies; in an inserted footnote, Lange refers to “The Classical Stationary State” as having appeared after his own text was written. I think one has to regard Lange as a co-discoverer of the Pigou effect, and in a slightly more general context. It is interesting that Lange thought of his analysis as weakening the “classical” claim that a market economy is self-regulating, because the case of a neutral or perverse monetary effect opens the possibility that an initial burst of excess supply will not be eliminated automatically by price flexibility. Pigou, of course, thought of himself as strengthening the classical case and refuting the Keynesian contention that a (quasi-)equilibrium could exist short of full employment. The difference is easily isolated. Lange’s monetary effect relates to the excess demand for money. Pigou was thinking of the relevant nominal money supply as constant. Deflation would presumably decrease the nominal demand for money and thus surely generate a stabilizing positive monetary effect. (The possibility of contrary speculative effects with *falling* prices was analyzed later by Patinkin and Schelling.)

There is a more profound difficulty with Lange’s argument. He never really asks why money should be held in the first place. The (excess) demand for money appears only as the negative of the nominal excess demand for goods, by Walras’s Law. Suppose that Lange had adopted the expedient of entering the real holding of money as an argument in agents’ utility functions. Then he would have had to entertain the possibility that money is generally a better substitute or closer complement for some goods than for others. It would be false to state that “(t)here will be no substitution of one factor for another or of one product for another” after a uniform deflation of all prices; demand would shift in favor of goods complementary to real cash balances. To save the argument, Lange would have needed a separability or independence assumption between money and goods, which has no real appeal and would have, in any case, narrowed the domain of validity of his results. Apparently he never thought along these lines; even in the mathematical appendix, he begins with demand functions for goods that depend only on the relative prices of goods. Of course he was not alone among general-equilibrium theorists at that time in not even trying to tell a convincing story about a monetary economy.

I mentioned earlier that Price Flexibility and Employment captured the imagination of economists of my generation because it seemed to offer the combination of general-equilibrium rigor and macroeconomic relevance. The effect wore off quickly, however, perhaps largely as a result of Milton Friedman’s adverse review. Friedman’s attack on the book was methodological, not substantive; it was not a monetarist salvo directed at incipient Keynesianism. He argued that the abstract taxonomic approach adopted by Lange was of little or no help in interpreting the real world. In effect, it would be just as hard to find out whether the monetary effect was positive or negative in any particular instance as to find out whether Walrasian equilibrium was stable or unstable under flexible prices. Educated guesses might be more fruitfully directed at the stability question itself. I think Friedman had a point. He probably failed to give abstract theorizing its full value in narrowing the range of acceptable possibilities and suggesting ways in which “common sense” might go wrong. But he was probably right that Lange’s approach would in the end butter no parsnips.

The true heir to Lange was Don Patinkin, who moved the argument decisively in the aggregative direction. I cannot follow that trail to its culmination in *Money, Interest and Prices* because by then we are well away from the Cowles Commission. But Patinkin's first steps were clearly taken before he left Chicago for Jerusalem, and they are part of our story. His 1948 and 1949 papers on price flexibility are directly centered on Keynes and Pigou. He concedes that the Pigou effect almost certainly saves the formal validity of orthodox economics. I say "almost" because Patinkin points out that in a general deflation the price level can fall but still be bounded away from zero, and saving can diminish but still be bounded away from zero. He also sees the empirical significance of Kalecki's point that inside money need offer no leverage for the Pigou effect. Lange's reasoning was strictly local. But that is hair-splitting.

Patinkin's main point in these papers is that the key issue is the speed with which self-correction occurs. Prolonged deflation is not "underemployment equilibrium," but it has the same practical implications. And the deflation required for Pigou's rescue operation could indeed last a long time. Patinkin emphasizes the important observation that widespread extrapolation of deflation could lead to the postponement of expenditure so that the level of output could get worse before it gets better. (Lange had also remarked on the unfavorable effects of elastic expectations.) I do not know if Patinkin was the first important writer in the Keynesian tradition to stake out its claim to be the economics of slow, even if ultimately stabilizing, disequilibrium motion. He was certainly one of those who planted that way of looking at the matter in American macroeconomics.

There is one more thing to be said before I leave this set of ideas. In another 1949 paper, "Involuntary Unemployment and the Keynesian Supply Function" — which appeared as a Cowles Commission Paper — Patinkin firmly adopted the view that involuntarily unemployed workers were "off their supply functions. He contemplated the possibility that other markets might fail to clear and even suggested what we now call the "short-side principle." Had the timing been a bit different, the formal development of general-equilibrium theory with quantity constraints might have taken root at the Cowles Commission. It did not.

Klein's Economic Fluctuations in the United States

The other major landmark of the Chicago years in macroeconomics was Klein's *Economic Fluctuations in the United States*, which was published as a monograph in 1950. This book established Klein as Tinbergen's successor in the enterprise of building complete macroeconomic models and estimating them econometrically. From it has stemmed the whole industry that has met the market test in the United States and has now been reimported into many countries of Europe. (1 presume, though a foreigner sees only the tip of the iceberg, that the Tinbergen tradition never died out in the Dutch Central Bureau of Statistics. Perhaps for that very reason, Dutch model building evolved along its own path and diverged from the rather straightforward eclectic-Keynesian route upon which Klein set it. In any case, it is Klein's example that became the ancestor of most of the working models of today.)

Anyone who opens *Economic Fluctuations* today is in for a surprise. Contemporary macroeconomics is loaded with agitated discussion of its microeconomic foundations, or its lack of them. This characterization surely applies to theoretical controversy and also, though to a lesser extent, to discussion of econometric implementation. Most participants think of this as a recent emphasis.

But in fact almost the whole first half of Klein's 1950 book is devoted to the microeconomic formulation of the (intertemporal!) optimizing behavior of households and firms and a formal aggregation of the results into a macromodel. The analysis is not very subtle or deep by contemporary standards. After a third of a century, that is hardly surprising. But the analysis is ummistakably there.

What is more, the intention to base operational macroeconometric models on a rigorous microeconomic foundation probably originated with Klein. Tinbergen's 1937 booklet and his 1939 volumes for the League of Nations have only the tersest "common sense" or a priori justification for the choice of equation forms and particular explanatory variables. He makes no attempt to base the final model on the aggregation of systematic microrelationships. Of course, the notion of doing so is not obscure or difficult to conceive and, to tell the truth, I half suspect that the current obsession with microfoundations has been pushed too far. One would not want aggregative relations to be palpably inconsistent with the notion of a substratum of individual agents behaving in some reasonable way. Excessive formality, however, exacts its own cost: usually the adoption of simplistic micromodels for agents and markets, for no better reason than that they are conventional in other uses and can easily be aggregated. Nevertheless, *Economic Fluctuations* does not ignore the question of microfoundations.

A few years earlier, in fact, in *Econometrica* for 1946, Klein had published a paper called "Macroeconomics and the Theory of Rational Behavior." In it he asked how a well-specified exact microeconomic model might logically entail exact relations among aggregates. Here an aggregate is just a (real-valued) function of many microvariables; and part of the problem of aggregation is to find the appropriate specification of aggregates within a particular micromodel and to give them a meaningful interpretation. For instance, Klein proposed that if a micromodel involves marginal productivity relations among the inputs and outputs of individual firms and their prices, then output aggregates (i.e., functions of individual outputs), input aggregates, and price indexes ought to obey analogous equations. Klein referred to the earlier (1938) work of Francis Dresch, a student of Griffith Evans at Berkeley, who had worked out some nice properties of Divisia indexes in general-equilibrium models, but Klein found it unsatisfactory because the Divisia indexes lack the analogue property that a macrovariable should be a function only of "corresponding" microvariables.

There followed a brief flurry of articles on the problem of exact aggregation, by Kenneth May, Shou Shan Pu, Klein again, and finally Andre Nataf. It was all over in two years. The other authors tended to reformulate the question in a more formal and, I think, more natural way: when does a micromodel imply the existence of an interesting macromodel, i.e., one involving a smaller number of functions of the microvariables? This formulation focuses attention on the conditions for functional dependence rather than on Klein's analogue property. Interpreted strictly as Klein proposed it, his desired property is almost impossible to achieve. It requires, as Nataf showed, additive separability in the microvariables. It is fair to say that this literature petered out more or less fruitlessly; Klein made essentially no reference to it in *Economic Fluctuations*.

Three separate models are actually estimated in Klein's book, using annual data from the 1920S and 1930s and omitting the war years after 1941. The models are chosen to illustrate economic and econometric ideas. The first contains three linear behavior equations. A consumption function has separate marginal propensities to consume from wage income and profits (estimated as 0.80 and 0.25, by the way); an investment equation has current and lagged profits and the lagged stock of capital on the right-hand side; given the particular consumption function, the model also needs an equation that predicts the wage bill from current and lagged output. Then there are three identities, one for each side of the national accounts and one identifying investment with the change in the capital stock. Since the model has lags, it has dynamics. Indeed, it has an almost undamped oscillatory mode, which is perhaps not surprising for that sample period.

Model II is just a stalking-horse and barely that. It consists of one structural equation, a consumption function linear in current and lagged disposable income and the lagged money stock, all in real per capita terms. All other expenditure is treated as exogenous. The single equation is inverted to illustrate the use of the reduced-form method for estimating the structural parameters of a just-identified model, and also to test the dependence of consumer expenditure on real cash balances. The latter just fails of statistical significance; but since one of the "exogenous" variables in the reduced form is gross investment plus net exports plus government expenditures including transfers less the sum of tax receipts, corporate saving, and inventory profits, the results were presumably not meant to be taken dead seriously.

Model III is the star turn. It is billed as a "large structural model" with — brace yourself — twelve endogenous variables. Great oaks.… In broad outline, the model is about what one would expect from its time and context. It is primarily demand oriented, though the supply side enters through the labor-demand and investment equations. There is an experimental attempt to include a production function, but it does not turn out well.

The model has a number of interesting features. As if to confirm my earlier comments on the near-irrelevance of formal aggregation of microtheory for macroeconomic models, Klein makes real consumption expenditure a linear function of real disposable income and describes it as "the demand equation for consumer goods as developed from the theory of household behavior." To tell the truth, I do not blame him for that.

More innovatively, the model contains separate demand equations for inventories (in stock form) of consumer goods and producer goods. Klein then proposes that the year-to-year change in the price indexes for consumer and producer goods might be driven by the residuals in these equations. Excess inventories would push the price of output down, and unusually small inventories would push it up. But then he remarks that it might be just as reasonable, maybe more so, to make the residuals in the inventory equations drive changes in output instead. In the econometric work, he aggregates consumer and producer goods and opts — more or less — for output adjustment. I say "more or less" because the equation for annual changes in output also contains the annual change in the GNP deflator. (That is not so easy to interpret. Modern inventory-theoretic models would suggest feeding stock levels back into both output-setting and price-setting equations.) When the model is estimated, it turns out that it is price change that is most closely associated with the catastrophic fall of aggregate output in the 1930s and the early stages of the recovery, but hardly anyone would draw the causal arrow from prices to output. In the rental-housing market, by contrast, it is indeed the vacancy rate, an excess-stock indicator that drives rents.

I shall mention just one more sidelight on this pioneer essay. After estimating Model III and discussing the outcome, Klein performs a few experiments, mostly to illustrate how the model might be extended, for instance, by endogenizing more variables. Among these experiments is one in which the change in the annual wage rate (i.e., wage bill per person employed) is expressed as a linear function of the volume of unemployment, lagged unemployment, the lagged wage, and a time trend. Is this the first econometric Phillips curve? The statistical properties of the equation look pretty good but Klein does not discuss it at all, and so does not comment on the orders of magnitude or on the role of the lagged wage.

It is striking — to a reader at this date — that Klein attempts no forecasting exercise with any of his models. (He had earlier written an article on the famous failure of the wartime forecasts of the postwar transition. To youthful readers I should perhaps say that they were generally much too pessimistic about employment prospects after the war.) The only entry in the index under "forecasting" refers to a passage at the very beginning where it is pointed out that an econometrician interested only in forecasting needs only an estimate of the reduced form of a model. Klein was quite obviously aiming at structural estimation. But, from the perspective of measuring how far our field has come, it is too bad he stopped there. There has been a staggering increase in the complexity, sophistication, and size of econometric models of the whole economy since this pioneering work, and Klein is still in the forefront. It would be interesting to know how much improvement in forecasting accuracy has accompanied this development. It goes without saying that today's bigger models can provide coordinated forecasts of many more variables.

In 1951, Carl Christ returned to Klein's Model III. He reports, first, that Andrew Marshall had already tested the model for stability of structure by extrapolating it to 1946 and 1947 and comparing the forecast residuals with the in-sample standard errors. Five of the behavioral equations failed this test: the demand functions for M_1 and M_2, the consumption function, the wage-bill equation, and the output-adjustment equation already mentioned. Christ, a sensible fellow even then, drops M_1 and M_2, because they are needed nowhere else in the model, tries some alternative consumption functions, reinserts a production function, which in turn requires a labor-demand function, and goes back to a wage-adjustment equation (in fact, a Phillips curve). His revision of Klein's Model III looks sensible, but it does no better than a naive model in extrapolation to 1948.

There was a formal discussion. Friedman concluded that the whole econometric model-building enterprise had been shown to be worthless and congratulated the Cowles Commission on its self-immolation. Klein demonstrated the truth of the French doggerel:

> Cet animal est très méchant.
> Quand on l'attaque, il se défend.

Carl Christ behaved like a perfect gentleman.

Final Notes on the Chicago Years

There is one last important product of the Chicago phase that deserves to be mentioned, although it came to completion only after the move to Yale. In 1952, Harry Markowitz published his article "Portfolio Selection" — another acorn — in the *Journal of Finance*. In it he proposed to compare portfolios, i.e., bundles of “elementary” securities, by their means and variances. Most of the article was given over to characterizing efficient portfolios, i.e., those undominated in the obvious sense by any other portfolio, and suggesting how efficient portfolios might be found in practice. But Markowitz was quite explicit that an investor whose utility can be expressed as a quasi-concave function of the mean and variance of his or her portfolio could find a best portfolio among the efficient ones. This classic work was later extended in a Cowles Foundation monograph. It is the ancestor of the capital-asset pricing model and indeed of most of the modern theory of portfolio choice.

It must be evident from the story so far that macroeconomics was not the chief glory of the Cowles Commission in its Chicago days. There emerged, as I mentioned early on, no Cowles Commission tradition in macroeconomics distinct from what was going on elsewhere. The important developments I have sketched — the Lange-Patinkin line, Klein’s first attempts, and finally Markowitz — were personal achievements, not in the sense that the authors were isolated, because they give warm acknowledgment to advice and stimulus from others in the Cowles community, but in the sense that they were not additions to an ongoing structure identified with the group.

Even a nonparticipant can guess why that was. The intellectually dominant senior figures at the Cowles Commission were Tjalling Koopmans and Jacob Marschak. Koopmans had no real interest in macroeconomics. It was and is too loose for his exact and fastidious mind. He must, of course, have been a tremendous help to Klein on econometric matters and to Markowitz on linear and quadratic programming. And, as noted earlier, he was a towering standard-bearer for the rigorous approach to empirical inference in economics and to economic science generally. But Koopmans was just not tuned to the compromises that have to be made in the macroeconomic enterprise.

Marschak had much more interest in the subject. He taught it; he obviously cared deeply about unemployment; and some of his papers sound as if they might have been stimulated by the concerns of practical macroeconomists. Nevertheless, Marschak, for all his breadth of interest and sureness of taste, was not really “into” macroeconomics either, in yesterday’s cliché. His class-room lectures on the subject, to judge by the semipublished version, were not inspired. Articles like “The Role of Liquidity under Complete and Incomplete Information” and “The Rationale of the Demand for Money and of ‘Money Illusion’” are aimed at the foundations of monetary theory and therefore of macroeconomics, but I think they do not get from the trees to the forest. On the other hand, those senior people at the University who were fundamentally involved in macroeconomics — Milton Friedman, Lloyd Mints, Henry Simons — were out of sympathy with the whole Cowles Commission enterprise of mathematical rigor and generality.

The New Haven Years

With the move to Yale, the picture changed decisively. Macroeconomics at the Cowles Foundation acquired a characteristic direction and spirit; and the source was pretty clearly James Tobin. I do not mean to suggest that the work done under the Cowles imprint was henceforth monolithic in theme or method. There was plenty of variety, as my narrative will show, but the Tobin research program was certainly the most prominent feature in the landscape, and it drew others along by example.

Tobin and the Linkage between the Real Economy and the Financial System

The transition did not take long. The first of the Cowles Foundation Papers is number 99, dated 1956. Tobin's classic article on "The Interest Elasticity of the Demand for Cash" is number 106, in 1956, and the even more famous "Liquidity Preference as Behavior towards Risk" is number 118, dated 1958. I do not need to rehearse their contents. They are, after all, in the direct line of work for which Tobin was awarded the Nobel Prize in 1981, and they have been anthologized and discussed many times, most recently in the surveys by Douglas Purvis and Johan Myhrman in the first issue of *The Scandinavian Journal of Economics for 1982*. My point here is a more systematic one. The long-term research program I characterized as identified with Tobin was directed toward describing and understanding the links between the real economy and the financial system. Monetary theory proper is part, but only a part, of this broader undertaking.

This was certainly a concern inherited from Keynes. One of the shocking things in the *General Theory* was its insistence that money is not a veil, nor is the financial system generally, not even in the medium run. And one of the complaints sometimes made today about the course that Keynesian economics has taken since the *General Theory* is that it has neglected the macroeconomic role of financial institutions and financial decisions. Whether or not this is a fair comment in general, it can hardly be an accurate statement about the work of Tobin, or about the macroeconomics done at the Cowles Foundation under his inspiration. (For completeness, I should add that a belief in the real importance of the operations of the financial system did not originate with Keynes. Wicksell, Robertson, and Raw trey, each in his own way, had it too. Tobin's starting point, however, was Keynes.)

The papers of 1956 and 1958 are about the liquidity-preference theory. The question that needed to be answered was: Why is any wealth held in the form of non-interest-bearing money? If for transactions purposes, which is easy to accept, then it is a useful assist for liquidity-preference theory to show that the transactions demand for money will have a non-zero interest elasticity. The second paper goes further and generates a portfolio demand for money. (An important influence here, a carryover from Chicago, was Markowitz's theory of portfolio selection.) Neither of these papers, however, has much to do with financial institutions directly. The project turns to the analysis of financial intermediation more generally with the well-known article on "Financial Intermediaries and the Effectiveness of Monetary Controls" by Tobin and William Brainard, followed two years later by another article with a similar title by Brainard alone, and again in 1968 by "Pitfalls in Financial Model Building" by the two jointly. During the same period there appeared a number of papers by Richard Porter, Donald Rester, James Pierce, and still other Yale students, which fill out the picture by providing more detailed analyses of bank behavior and related topics. Many of these were later collected in Cowles Foundation Monographs, and I will come back to them.

The Tobin project takes a decisive step with the "pitfalls" paper. Here there is at least a sketch of a complete model in which the real economy (the demand side anyway) is explicitly linked with a differentiated financial system. Several assets are introduced (currency/bank reserves, Treasury bills, bank loans, demand deposits, time deposits, and equities) so that the question of the substitutability among assets arises by necessity. This, of course, is the hallmark of the Tobin research project. In this paper, the emphasis is not so much on the model itself as on the "pitfalls" of the common failure to observe financial identities explicitly in model building, in particular that it forces all shifts in the demand for any single asset to be offset wholly by the residual asset. This article has been much discussed in the past fifteen years; for recent examples see Smith (1975) and Purvis (1978) and the literature they cite.

"Pitfalls" does contain some simulation runs of the model with guessed values of the parameters. These serve to demonstrate that the covariation and even the relative timing of endogenous variables can differ according to the source of the exogenous shock that sets the model in motion. The discussion has a now-familiar ring: "One of the basic theoretical propositions motivating the model is that the market valuation of equities, relative to the replacement cost of the physical assets they represent, is the major determinant of new investment." This valuation ratio, you will be surprised to hear, is called "*p*." This appears to be its first appearance; when the notation clicked off one more letter in the alphabet, I do not know.

A typical conclusion from the simulations runs like this: "Changes in excess reserves are ... an unreliable guide to the thrust of the financial system, as measured by p. When monetary policy is expansionary, excess reserves go up along with *p*. When ... nonmonetary events are raising both *p* and the demands on the banking system, net free reserves fall." There are also cases in which the volume of demand deposits moves in the same direction as *p* and cases of the reverse correlation. Another set of simulations shows that the cyclical timing of peaks and troughs can be an unreliable indicator of causality: one endogenous variable can lead another when the system is driven by shocks to a particular exogenous variable and lag it when the source of disturbance is elsewhere. This particular pitfall is pretty deep, so deep that much of the profession still falls into it.

Some of the theoretical and empirical background for the 1968 paper is to be found in three Cowles Foundation collective monographs published together in 1967, although individual chapters go back to the early years of the decade. Monograph 19 reprints "Liquidity Preference as Behavior

Towards Risk" and continues with a number of studies, mostly theoretical but occasionally empirical, of *Risk Aversion and Portfolio Choice*. The authors include Donald Rester (joint editor of the three volumes, with Tobin), Susan Lepper (mostly about the role of loss offsets available in the tax code as a way of influencing the assumption of risk in individuals' portfolios), E. S. Phelps, and Richard Rosett.

Monograph 20, entitled *Studies of Portfolio Behavior* shifts attention to financial intermediaries and other institutional holders of assets. Alan Reston studies the way nonfinancial corporations govern and adjust their holdings of cash and securities in response to changes in transactions, interest rates, and other balance-sheet items. Rester looks at the supply of loans by commercial banks, and James Pierce studies portfolio management by commercial banks. Roy Wehtle does a very detailed analysis of the demand for securities for investment purposes by four life insurance companies. Re concludes, by the way, that these companies' portfolios are managed aggressively and well, thereby probably contributing to the efficiency of capital markets in general. These four papers are primarily empirical in character; all but Pierce's were Yale Ph.D. theses.

Monograph 21 is mostly about monetary policy in a differentiated financial system. It includes three more Ph.D. theses: Richard Porter's theory of the way commercial banks divide their assets between loans and investments, William Brainard's "Financial Intermediaries and a Theory of Monetary Control" mentioned earlier, and Peter Sloane's statistical analysis of the determinants of yield differentials among bonds of different quality. The volume also contains the well-known Tobin-Brainard article on the way monetary policy effects are propagated by asset substitutions through the chain of financial intermediaries, Tobin's paper, "Commercial Banks as Creators of Money," and Arthur Okun's study, for the Commission on Money and Credit, of the quantitative effect of monetary policy and Treasury debt management on short- and long-term interest rates. Because the bill rate is more sensitive than the government bond rate to monetary policy, the excess of the bond rate over the bill rate naturally falls as credit tightens. Okun concludes, nevertheless, that moderate changes in the relative supply of bills and bonds will have little effect on the yield differential. Re attributes this mixture of results to inelastic expectations of the future long rate.

Many of these empirical papers — and some of the theoretical ones too — are no longer of great interest. The craftsmanlike Ph.D. thesis is rarely riveting reading even when it is still warm, as most of us know from experience. I have given a bit of detail here for rather different reasons. One is to give credit where credit is due, if I may make a joke. The other, and more important, reason is to drive home the point that the move to Yale gave the Cowles Foundation what the Cowles Commission had lacked at Chicago: a substantive macroeconomic point of view of its own. I do not mean a political-economic "line"; I mean the eminently scientific view that in a world with a complex set of portfolio preferences, financial institutions, and paper assets (some with fixed and some with market-determined yields), monetary theory and monetary policy are not well represented by a model in which an undifferentiated "*M*" is exogenously varied by means of helicopter drops, and idealized helicopter drops at that. Instead, money supplies actually change in the course of transactions between the Treasury and the public, or between banks and the non-bank public, in which at least one other asset besides money must change hands.

In such a world, the effects of "monetary policy" will likely depend on the particular transactions that take place and the wealth and portfolio changes that they bring about. A more important conclusion is that, in such a world, with consumers having finite lifetimes and finite horizons, and intertemporal markets less than perfectly transparent, financial policies will have real effects in as long a run as actually matters. This point of view was given definitive and influential expression by Tobin in his 1969 "A General Equilibrium Approach to Monetary Theory," which is not listed as a Cowles Foundation Paper and, more recently, in his Nobel Lecture, which is. But it needs to be said that others at the Cowles Foundation, especially Brainard, Hester, and Willem Buiter, as well as those mentioned earlier, made important contributions of their own.

In such a climate, many flowers bloomed. It will not do to single out isolated examples here and there. But I must make an exception for Brainard's "Uncertainty and the Effectiveness of Policy," which has been on my list of Things I Wish I Had Written since 1967. I was once able to explain to the Board of Directors of the Federal Reserve Bank of Boston that Brainard had proved that if you don't know what you're doing, for heaven's sake do it gently. There are a few more sustained research efforts that I will mention, however, if only to convey some picture of the intellectual environment.

Macroeconometrics, Wage and Price Behavior, and Growth Theory

After Klein's departure for Michigan and Pennsylvania, the business of constructing macroeconomic models more or less disappeared from Cowles Foundation publications. What had been a handicraft pursuit in 1950 was growing into a heavy industry and outgrowing the academic setting. My casual impression is that nowadays the Wharton and Michigan models have little or no integrated relation to the nearby economics departments. The only throwback to the frontier mentality was Ray Fair at Princeton, who continued the experiment of forecasting systematically with a small model uncontaminated by add factors, gossip, and extraneous hints from the great world. When he moved to Yale in the late 1970s, macroeconometrics returned to Cowles. Fair's research effort has retained its maverick quality. He has specialized in the construction of relatively small models, by contemporary standards, emphasizing a basis in economic theory that is explicit and visible. Fair's models are less like the traditional black box and more like those plexiglas-sided ant farms children get for Christmas. They lend themselves to policy experiments and to such exercises as seeing how far rational expectations in the securities markets will take you in a model whose other markets are modeled with plausible assumptions, or trying to isolate the sources of exogenous shocks. Perhaps these modest constructions are no substitute for the complete model, but I would be sorry to see them go.

Another example of recent Cowles Foundation macroeconomics is one aspect of the variegated work of William Nordhaus. In 1972 and 1973, he wrote two papers — one in collaboration with Wynne Godley — on the behavior of the prices of manufactured goods during business cycles, and a third on wage behavior. It is the work on prices that concerns me here. On the whole it concludes that prices respond primarily to costs. They do not exhibit the degree of countercyclical variability that would be implied by the standard supply-demand market-clearing paradigm. Nordhaus and Godley thus confirm a long line of Anglo-American research going back to Hall and Hitch in the 1930S and Robert Neild and Edwin Kuh in the 1950s. The price equations of the standard large econometric models seem to fit into the same mold. It is either one of the scandals of currently fashionable macroeconomics or a sign of its divine inspiration, depending on your point of view, that so much of it rests squarely on the assumption of price flexibility and market clearing, which appears to be rejected by empirical study, whether econometric or institutional.

In this connection, it is striking that the list of Cowles Foundation Papers includes almost none belonging to the rational-expectations-with-market-clearing school that has captivated so many young macroeconomists who are away from the bracing air of the New England shoreline. I am of two minds about this. There is, of course, something refreshing and admirable about being unfashionable and right. But there is also a danger that graduate students and young faculty acquire an unearned certitude. You can learn a lot by watching so great and pure a matador as Jim Tobin, but you probably do not learn a lot about the bull. Tobin's controversies with Milton Friedman, however, do teach the reader a lot about Friedman's ideas too.

Finally, the late 1950s and the 1960s were the heyday of growth theory, whether regarded as Walrasian equilibrium in motion or as macroeconomics with a supply side. Tobin's "A Dynamic Aggregative Model" preceded the arrival of the Cowles Foundation in New Haven; but he and others on the Cowles staff explored several distinctive lines of research afterwards. Eyewitnesses testify that growth theory bulked even larger as a discussion and seminar topic than the list of published papers suggests.

Tobin's contribution was primarily in the "money-and-growth" tradition, but that label understates its scope. It is true that the superneutrality of money was one of the persistent topics of that literature; more important in the long run, however, was his characteristic emphasis on the need for a serious analysis of asset preferences in any model of economic growth. I should also mention his paper on "Economic Growth as an Object of Policy" because I have elsewhere cited it as part of the mental furniture of the Council of Economic Advisers in John Kennedy's administration.

The main function of growth theory, from Harrod and (more particularly) Domar on, was to provide a supply side for long-run aggregative theory. At Cowles, this aspect led to both normative and positive research. Several papers by Koopmans analyzed the Ramsey optimal-growth problem with that combination of clarity *and* attention to detail that is so hard to achieve. Birds of passage like David Cass and Emanuel Drandakis also made important contributions to normative growth theory, centering on efficient paths and consumption turnpikes. Drandakis did original work on the descriptive two-sector model as well, especially in disentangling sufficient conditions for convergence to the steady state, some originating in the technology and some in the assumptions about saving. Ned Phelps and Ben Massell explored "embodiment" and "vintage" notions and helped to clarify the theory of the competitive return to investment in those circumstances. The Nordhaus-Tobin contribution to the National Bureau's 50th anniversary colloquium ("Is Growth Obsolete?", 1972) contains a little bit of everything.

Concluding Remarks

I want to end with some general remarks about methodology whose only excuse is that they seem to have been evoked by my reading of Cowles Commission/foundation contributions to the literature of macroeconomics.

Our self-conscious notion of our own method is very formal. A model is confronted by the facts. A hypothesis is embedded in a formal model before it is tested; indeed, otherwise it cannot be tested. I think this methodology is inherited from our picture of hard science. The classical heroic example is general relativity. Einstein's theory is conceived inside his head and is born as a mathematical model. The model predicts that light will be bent by a gravitational field. Nature offers an experimental opportunity. The measurements are made. The theory is confirmed. Thunderous applause.

I continue to believe that there is some validity in that greatly idealized picture of science; and it may have some relevance for economics, even for macroeconomics. That there is another paradigm of research was driven home to me a year or two ago when I read a series of extended lectures by half a dozen eminent biologists, each describing the process by which he had achieved a major scientific advance. They were all, as I remember, concerned with the physiology of the nervous system, perhaps all with vision. (I lent the book to a friend, and he to another, and I have not yet been able to track down the reference.) In this paradigm, the characteristics of the heroic scientist are experimental dexterity, an instinct for just the right angle of vision, keenness of observation, and a flash of insight, almost empathy: so *that's* the way it must work (muscles contract, or nerve cells communicate, or rods and cones see color). It seems to me more and more likely that for a lot of macroeconomics this is a better analogy than the idealized “hard-science” view. Since we are dealing with a vast and complicated system rather than with an organism or a tiny piece of an organism, and since we cannot do laboratory dissections, the latter analogy will never be completely absent, but neither is it the whole story.

When I reflect on the best of the Cowles contributions to macroeconomics, like Tobin-Brainard *q* or Okun’s brief article on potential GNP, it seems to me that they resemble the second of these patterns more than they do the first. I have no doubt that it will be found, after detached study, that the successful coexistence of large coordinated research enterprises, like the one presided over by Lawrence Klein, and federations of small-scale handicraft operations, like the Cowles Foundation, is a rational, maybe even efficient, response to differences in technology, tastes, and, of course, the presence of transaction costs.

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