

DISCUSSION OF THE MEYER-RASCHE AND TAYLOR PAPERS

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For us at this conference, the 1970s constitute ten years of additional data and some theoretical developments that suggest new ways of interpreting those and earlier data. The two papers presented this morning -- in part, because of the assignments given the authors -- contain very different views about the lessons of the 1970s. I will come to still a third view and, as it happens, one that does not represent a compromise between them.

As I understand it, Meyer-Rasche accepted the task of summarizing lessons from the data of the 1970s, while Taylor accepted the task of summarizing lessons from the theoretical developments of the 1970s. That division of labor did not turn out well; it encouraged Meyer-Rasche to proceed as if one could learn lessons from data without invoking theory.

On the basis of the preliminary draft of the Meyer-Rasche paper made available to me and on the basis of their oral remarks this morning, I am left somewhat in the dark about the point of view of the Meyer-Rasche paper. I know what they did, but I'm not sure what their message is.

Based on what they did, one might infer that for Meyer-Rasche, the 1970s represent no more than ten years of additional data. They

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use those data and earlier data in the same way that most economists ten years ago used the data available to them. In particular, both their so-called structural models and their reduced-form models consist of regression equations that in form are the same as those most economists used in the 1960s. Moreover, Meyer-Rasche extrapolate from those regression equations for the effects of different policies in the same way that many economists in the 1960s extrapolated from their estimates. That is why I say that for Meyer-Rasche, the 1970s seem to represent no more than ten years of additional data.

Even at the level of pure empiricism, a different lesson can be drawn. The Meyer-Rasche extrapolation procedure applied in the late 1960s did badly predicting the 1970s. Why, then, believe that those same procedures applied now will do well predicting the 1980s?

Happily, though, we do not have to decide on the basis of pure empiricism. The theoretical developments of the 1970s -- many of which are described in Taylor's paper -- provide convincing arguments why we should not take seriously as "multipliers" the correlation coefficients or the functions of them presented in the Meyer-Rasche paper.

Meyer-Rasche are aware of the criticism of the multiplier interpretation of their estimates. In effect, they acknowledge the criticism and say that they are unwilling to defend such an interpretation. That, though, is what leaves me confused about their message. Nor does it help to suggest, as Meyer seemed to in his oral remarks, that their estimates of Phillips curve trade-offs provide upper bounds on the unfavorableness of this trade-off. Logically, such a claim also requires a supporting argument. Moreover, upper bounds can be interesting, or not interesting. All of GNP is an upper bound on the output

loss that accompanies a one percent cut in the inflation rate, but it is not an interesting upper bound. Meyer-Rasche must convince us that their estimates are interesting upper bounds if, in fact, they are upper bounds at all. Such convincing must take the form of a theoretical argument that says why it is legitimate to extrapolate in particular ways from particular correlations.

In the 1960s, many economists thought that their policy extrapolations from the kinds of models used by Meyer-Rasche were legitimized by existing theory. The theoretical developments of the 1970s have convinced many of us that that is not so. Although Taylor's paper describes some of those developments, his paper stops short of describing in full generality why we were led astray badly by the kind of theorizing that was used. Since that kind of theorizing still persists, it is worthwhile summarizing in a general way what is wrong with it.

Whether we are talking about most textbooks in macroeconomics or most macroeconometric models, the models from which policy implications are drawn consist of a set of relationships -- a consumption function, an investment function, a money demand function, and so on. Let us label these  $M_1, M_2, M_3, \dots, M_N$  (M for model). The style of macroeconomics textbooks is to present the complete model and its policy implications and also to present separate chapters -- one on consumption, one on investment, one on money demand, and so on -- that are meant to justify one by one the relationships of the complete model, the  $M_1$ . When builders of macroeconometric models try to justify their models, they also proceed in this way. In order to get at what is wrong with this kind of theorizing, we must describe the logical relationship

between these justifying chapters and the macroeconomic or macroeconomic model consisting of  $M_1, M_2, \dots, M_N$ .

Each justifying chapter consists of a set of assumptions. Let us label these sets of assumptions  $S_1, S_2, \dots, S_N$  (S for story), where for each  $i$ ,  $S_i$  is said to justify  $M_i$ . The most extravagant claim made about the relationship between  $S_i$  and  $M_i$  is the following: For each  $i$ ,  $S_i$  implies  $M_i$ . In particular, it is never claimed that the converse is also true. In other words, in general,  $S_i$  and  $M_i$  are not equivalent and more is implied by  $S_i$  than just  $M_i$ . This nonequivalence has two consequences.

First, it implies that consistency among the  $M_i$  does not imply consistency among the  $S_i$ . If the  $S_i$  are mutually inconsistent, then it cannot be claimed that there is an underlying theory of the  $M_i$ . Note, in this regard, that consistency among the  $S_i$  is never checked and, as I illustrate below, that inconsistency is easy to demonstrate for most macroeconomic models.

Second, if the  $S_i$  are mutually consistent, nonequivalence between  $S_i$  and  $M_i$  implies that we are missing many of the implications of the underlying theory by limiting attention to the  $M_i$ . Thus, for example, the  $S_i$  often contain at least hints of a welfare analysis of inflation. As is well known, the typical  $M_i$  provide no such analysis.

I will now briefly defend the nonequivalence claim and, at the same time, argue that inconsistencies are present in standard macro models. And, since this is St. Louis, I will begin by focusing on money demand.

The usual way to defend the money demand functions of most macroeconomic models is to appeal to a transaction cost model of the Baumol

(1952), Tobin (1956), or Miller-Orr (1966) variety. Those models explain money demand in the presence of default-free, higher-yielding securities -- Treasury bills, say -- by transactions costs, for example, trips to the bank. But the models imply more than a money demand function. They imply that if the ratio of the public's means of payments to its holdings of interest-bearing assets changes as a result, say, of open-market operations, then there is a change in the amount of resources used up in transactions. But such a change contradicts the usual resource-supply assumptions of most macro models. Those make no allowance for an altered amount of resources being used up in transactions. For this and other reasons, the implications for open-market operations of the theory of interest in the inventory models are very different from those of most macro models, particularly monetarist models (see Bryant and Wallace 1979).

It is also standard to assume that the money demand function that one derives for a closed economy holds with only minor modifications for an open economy in a world in which each of several countries issues its own money. It is this view that lies behind the attachment to (the viability of) laissez-faire floating exchange rates. But such a claim is supported neither by an acceptable theory (see Wallace 1979), nor by recent experience. That experience suggests that the demand for a particular money in a world of many monies may be very different from the demand for a single money in a closed economy.

In the 1970s, of course, inconsistencies regarding expectation formation have received the most attention. Expectation formation is important because macroeconomics is concerned primarily with aspects of behavior that depend upon views about the future -- asset acquisition

versus current consumption, the composition of assets, or nominal wage determination in those contracts that Taylor discusses at length in his paper. It has been argued convincingly that the  $M_i$  of most macroeconomic models contain, either implicitly or explicitly, forecasting schemes that are good schemes in some environments and not in others. (See, for example, Lucas 1976.) Moreover, careful examination of the  $S_i$  reveals that the particular forecasting schemes imbedded in the  $M_i$  were chosen because they were good schemes in particular environments. The inconsistency arises because the environment implied by all the  $M_i$  -- including various specifications for policy -- may not correspond at all to that assumed in the various  $S_i$ . This kind of inconsistency is avoided by using a perfect foresight (rational expectations) equilibrium concept. By using that concept, the economist avoids imposing on the individuals whose behavior is being modeled any fixed way of extrapolating from the past, and ensures that he or she is not attributing to them views about the future that make no sense for the environment they are in.

Now having said that perfect foresight is an equilibrium concept, it should be evident that it is misleading to discuss its merits or its implications in terms of a particular policy conclusion like "policy (whatever that means) does not matter." The perfect foresight equilibrium concept has been around for a long time. It would be surprising, indeed, if that concept alone implied a result like "policy doesn't matter." In general, of course, by themselves equilibrium concepts imply very little. The importance of the perfect foresight equilibrium concept has nothing to do with the validity of some vague conclusion

like "policy does not matter." Why, then, all the attention to "policy doesn't matter" in this morning's papers?

In 1975, there appeared a paper by Tom Sargent and me in which a result of that sort was obtained. We took a particular  $M_1, M_2, \dots, M_N$ , one that we argued resembled in many respects standard macro models, and replaced a fixed forecasting scheme, one of the  $M_i$ , by perfect foresight. We argued that the replacement made a great difference for the implications of the model. In particular, under perfect foresight and certain other assumptions, all policies in a certain class gave rise to the same equilibrium values for real variables. This result did not follow under the fixed forecasting scheme. Our message was, therefore, that the kind of forecasting scheme imposed matters greatly. Such a message, though, is very different from one that says that the perfect foresight version should be taken seriously as a model of this or any other economy. From the discussion above -- and from remarks in our 1975 paper -- it should be evident that the imposition of a perfect foresight equilibrium concept does not by itself turn a hodgepodge of indefensible relationships into a coherent model.

The Sargent-Wallace "policy-doesn't-matter" result is to be contrasted with a neutrality result obtained by Lucas (1972). The Lucas result was obtained from a model that is coherent in the sense that its conclusions are derived from a mutually consistent (and defensible) set of assumptions, a single  $S$ . The Lucas neutrality result, however, applies only to alternative deficits consisting of money transfers that individuals know they will receive in proportion to their holdings of money. This is neither monetary policy in the sense of open market operations -- there is, in fact, only one asset in the Lucas

model -- nor is it the kind of fiscal policy that any country ever follows. The Lucas model is important because it is the first coherent model that implies anything like Phillips curve correlations. The model implies that it is not legitimate to extrapolate from these correlations for the effects of different policies.

What is new about the 1970s and what offers bright prospects for the 1980s is not so much the view I have set out about the illogical structure of standard macroeconomics. That view can, I think, be found in Leontief (1947) and Koopmans (1947) and, I might add, in the attitude of many nonmacroeconomists toward macroeconomics. What is new and exciting about the 1970s is the progress we have made in devising defensible assumptions that can explain a wide range of macroeconomic phenomena. Lucas (1972) is an outstanding example. In the work on search and matching models (see, in particular, Mortensen 1979), we see the beginnings of a theory of unemployed resources. And, perhaps, in new work on money (see, for example, Kareken and Wallace 1979), there are ideas about how to confront long-standing problems in monetary theory. Although I think we are making rapid progress, the profession is very far from having reached a consensus.

First, not everyone, by any means, agrees that we must completely abandon the style of macroeconomic theorizing and modeling that I have described above. For many, to do that is to abandon macroeconomics. This is right if macroeconomics is defined by a style of modeling. But if, instead, macroeconomics is defined by the phenomena it seeks to explain and by the policies it seeks to analyze, then this is not a call for abandoning macroeconomics. It is a call for abandoning a fallacious style of reasoning that has evidently gotten us nowhere. Second, even



among those who agree that we must, as it were, start over in macroeconomics and monetary theory, there is little agreement about how to proceed. For example, in my very brief listing of promising developments, I did not include disequilibrium theory. In my view, disequilibrium theory is not very promising, but many economists disagree.

Given the lack of consensus on theory, it would be surprising if there were consensus on policy. And there is not. Academics, of course, thrive on controversy, which very naturally accompanies the development of substantially new theories in a field. Policymakers, in contrast, seek consensus. Since the economics profession is far from having reached consensus on macroeconomic policy, I do not envy the task of policymakers in the 1980s. The absence of professional consensus leaves policymakers in the position of having to make up their own minds.

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