Discussion by Leonid Hurwicz

1. Opening Observations

The author of the first paper, Martin Shubik, just whispered to me that, to some extent, he was proceeding in his comments on the assumption that his discussant (myself) may have been the only person who had read his paper. Possibly that was one of the more realistic assumptions made here today.

I think I should start out with the obvious warning that I am very much an outsider in this group. I have never focused on problems of monetary theory, and my comments should be viewed in that light. I will probably be more general than others have been.

Listening to the discussions about the metaphysical nature of fiat money, I was reminded of an essay from the late thirties by D. H. Robertson which was published, I think, in one of the English bank periodicals. It was written in the form of a dialogue between Socrates and an economist. Socrates was trying to understand fiat money — what it means and why it works. And he apparently had read the inscription on the British pound note. It said that the bearer of this note would be paid on demand one pound sterling. And Socrates asked, "What is this thing that one would get in exchange for a pound note?" And the economist said, "Well, it is another piece of paper just like this one." Doesn't this capture the paradox of fiat money we are all trying to explain with the help of elaborate mathematics?

Another part of the story is, I think, also relevant. Socrates and the economist had gotten to the end of their conversation where Socrates, by his usual method of questioning, had demonstrated that the economist did not have the first notion what money is or whether it makes any sense. Nevertheless, at the very end Socrates asked, "Well, how does this system work?" And the economist's parting reply was, "Very well, thank you." That may have been overly optimistic, but it seems to capture the flavor of a good part of our subject.

Another thing that struck me while listening yesterday to the debate concerning fiat money is how much more emotion people put into it compared with discussions I am more familiar with, such as those of general equilibrium. My naive interpretation of this contrast was that this is what happens as soon as the word *money* is mentioned. Somebody else pointed out that it shows either that money is not neutral or that people are not neutral about money. But what I really think is that it shows something more serious, namely, that there is a

very close connection between one's abstract philosophical model and one's policy recommendation about something very immediate and concrete, for instance, whether next week the Federal Reserve should be buying or selling bonds. The closeness of this connection disturbs me. For while I feel there has been tremendous scientific progress in this field—as shown by the contributions presented here—I also see an enormous distance between the analytical conclusions one can draw from what I regard as toy models and any direct policy conclusions. Of course, somebody might ask whether there is available any better rationale for Federal Reserve policy decisions than these very abstract models. But here I am very fortunate in not being a monetary economist: I do not have to answer that question.

2. Two Questions to Ask About Money

Although I will be primarily focusing on Shubik's paper, for my own understanding I will have to put it in a perspective by relating it to others. There have been basically two kinds of papers presented at this conference: those which ask why there is such a thing as money (fiat money in particular) and those which ask, given that there is an institution of money, how the system works.

The Shubik paper is obviously in the second category, and he introduces money by fiat, his own fiat in this case. He starts with a definition which says that money is a good that can be exchanged against any other commodity (there could be one or two or almost any number except $n-1$, if I remember correctly), and from there on he has in this paper (as well as in some other papers) certain rules, which are in the nature of rules of the game. For instance, in the simplest kind of economies, you have to pay "cash on the barrelhead." So there is an extra constraint, one in addition to the usual budget constraint that we have in ordinary equilibrium models. Of course, it is very natural to ask why it is that Shubik has this kind of a restriction in this model. The explanation that Shubik gives is that if you are going to be rigorous about it, you have to be precise about defining the rules of the game. In other words, it should be possible to get a bunch of undergraduates together and give them those rules, and they should then be able to play this game. It should not run off the board, so to speak.

Now it is true that rules must be precise. But that does not imply that the rules have to be as highly determinate as in this paper. Let me give just one very trivial example. In Shubik's paper there are very precise conditions as to who makes the first move, who makes the second move, and so forth. In some respects, that is essential for this model because the author has to have the fiat money created on the first day of creation so that the rest of the game can be played. But there are aspects of the game that could be left up to the players and not made into rules, for instance, which player will put down the first card. Then, in order to complete the model, you might introduce some other elements, perhaps random moves determining who goes first. What I am saying is that the degree of determinism in the rules is not a methodological necessity, but rather a matter of analytical convenience, a device that makes it easier to work things out.

The question is whether a theory so constructed is sufficiently robust. What if it should turn out that having specified one of those details, such as the sequence of moves, you have injected something into the solution that otherwise would not have come in? It is customary for all of us who construct such models both to make simplifying assumptions, because that makes our job

easier, and also to assure the audience that it does not really matter, that things would work out in roughly the same way without those assumptions. I think we are all often guilty of that. But in this particular case, being a consumer, I am entitled to be skeptical of these assurances — only mildly skeptical, though, because many of these assumptions probably are harmless. Still, it shows how much more work there is to be done.

Now, there is a point which is related to the two-part structure of this field (the two parts being, first, explaining why there is money and, second, explaining how things work, given that this money is subject to certain rules). If you are going to make certain assumptions in the first part of your work about the nature of the economy which has generated the phenomenon of money, then presumably you should somehow maintain those assumptions in the second part of your work, so that you are sure that the explanation of how money works once it exists is not inconsistent with the explanation of what permitted the institution of money to develop in the first place. Here again, we have received verbal assurance from various people in this group that the assumptions made are not going to cause any trouble. But when I look at the actual models, I do not see that there is a rigorous check on this. If we are to have a division of labor between the two parts of the field, such checks are indispensable. Ultimately, I think, it is essential to have a unified theory that embraces both parts.

3. Explaining Why Money Exists

Just one more remark about explaining how the institution of money, or fiat money, has come into existence or why it exists. There are two possible ways of thinking about that type of explanation. One is to study the history of money, going back at least to ancient times in Babylon and Asia Minor and to the goldsmiths of London and perhaps to a few places in between, and ask, Why did money come into use in these particular times and places? Taking into account the historical background, why did fiat money (as distinct from commodity money) come into existence? Thus, one could construct an *evolutionary* theory of money. Alternatively, we have an approach that does not pretend to be a potential explanation of this historical evolution. Instead, it is an *endogenous* theory of money which is somewhat static rather than evolutionary in character, and it implies a positive equilibrium level of money. What makes a theory endogenous is that you do not introduce deus ex machina rules which force people to use money, but rather you introduce elements into the situation which would make them want to use money.

Here what has struck me, and I would hope that we shall go somewhat beyond it, is the inclination toward a kind of philosophical monism, a tendency to develop a certain amount of religion that only one aspect is important while other aspects or functions are not. In this respect, I am rather in a camp with Jim Tobin and others. I am impressed by the traditional explanations involving about three or so factors of equal a priori plausibility. At the present state of knowledge, I would not know how to assess their relative importance. I could very well imagine that, for classroom purposes, we would construct different models, each of which involve only one of those aspects: store-of-value, transaction facilitation, or some other function. Each of these models by itself would perhaps be enough to explain why something like what we call money has come into existence. But then, of course, before we come to policymaking, and even earlier than that, when we come to some kind of systematic empirical

testing, we would have to allow for all of these different functions of money in order to see how to relate the various features of the model to reality.

I also believe (and this, too, may be related to some things that Tobin said) that there are phenomena which we observe in the context of money which logically do not at all have to go together with money. One example of this is the credit that Shubik refers to. He starts out introducing some sort of economy in which there is just commodity money, like gold or salt. And then he says, Well, but there really is not enough of this commodity money. The reason there is not enough is that in this model there is the (Shubik) rule which says that you must pay in that commodity or you cannot buy things. You are not permitted, in effect, to barter your initial endowments. As Shubik tells the story, when you enter the trading room you have to surrender to the referee, or somebody like that, all of your initial commodity endowments other than the money commodity. In real life you might be able to barter, but his rule does not permit it. Well, I do not know whether to think that there really was this kind of phase in the history of the development of these institutions, which I doubt, or that this difficulty shows the inadequacy of this kind of model. But then Shubik says, Well, if this kind of problem develops, if there is not enough of the commodity money, credit will enter. Okay, that is one way to remedy the deficiency of this particular model. But when you think about the problem of credit as such, you can very well think of credit in a barter multiperiod economy. If people have to wait until after harvest before they can have something to eat, it is very likely they will be lent resources by somebody else. In the work of Böhm-Bawerk, and undoubtedly earlier authors, you will find many such examples. So I do not see a priori that there is a very close relationship between the phenomenon of credit and the phenomenon of money. If you are in a monetary economy, then, of course, there will be a monetary aspect, an important monetary aspect, to credit. And I think there are other examples of putting more of a burden on the monetary aspects of things than perhaps would be justified from a more general point of view.

Now let me give an example of a model that probably has much in common with Lucas'. But I must admit that I only got to see a small part of Lucas' paper, and very recently, so I apologize for any overlap. In any case, my story is not an attempt at originality. Rather, it illustrates how one can go about constructing endogenous theories without bringing in the issue of overlapping generations.

Suppose that you want to explain why, in a certain economy, a commodity money developed. And let us say that this commodity is salt. (According to Webster, the word *salary* comes from the word *salt*, so it served to some extent as commodity money.) First you postulate, for instance, that somehow the society has increased either in size or economic complexity, so the problem of the so-called double coincidence has become more severe. (I do not know why it is called a *double* coincidence; I think *coincidence* is good enough.) The probability of coincidence of wants has diminished, and that creates a need for some alternative device. If I want to sell something, and I cannot get for it what I really want, then in a model with uncertainty, since I have to take something else, I would rather take something that is not the idiosyncratic desire of just one single person in this huge society, but rather something that almost everybody uses from time to time and is likely to take off my hands later. Let us say salt turns out to be this kind of thing. In a stochastic model with time (just the transaction time, not generational time), we might find that as the economy

grows the probability that people will accept salt in exchange for whatever they are selling will increase. Then it will also turn out that perhaps you would find it desirable to hold inventories of salt a little higher than would be needed just for tomorrow's soft-boiled eggs. And so you would develop a liquidity theory.

Is there anything very strange about this model? I think it is very much in the spirit of what Hicks wrote in his paper on money in the 1930s, and it seems a perfectly reasonable theory of a pure exchange economy with only an uncertainty of encounters and without overlapping generations. Of course, I have only told it in words, but it is very easy to set it to mathematical modeling music.

I mention this example only to show that, for me at least, there is no philosophical necessity for a stress on the intergenerational aspect of money. Of course, that does not mean that the intergenerational aspect is unimportant. But the example does show that the fiat phenomenon (the fact that salt will somehow acquire a value in the market that is higher than if its only use were for soft-boiled eggs) can already be present in an economy without an intergenerational structure. Admittedly, my example is not one of pure fiat money, but only of commodity money with a fiat aspect.

To get pure fiat money, it is often assumed that nothing else is durable, so that in a multigeneration world money is the only transmissible store of value. I, however, do not see a compelling necessity for making money the unique intergenerational bridge. The fact is that we do have durable goods of one sort or another, and again, there is no reason to go in a monistic direction. I think one can say that there are these other stores of value, whether land or capital or whatever, but that for certain purposes they are not adequate—for example, because of the lack of liquidity properties. Therefore, fiat money is needed in this situation as well.

4. Explaining How Money Works

Now let me go back to the more technical issues of constructing these models as games. I have already commented that one has to question very seriously the imposition of rules that are artificial and might be distorting. I think that to the greatest extent possible one should impose only those rules that are absolutely required by the logic of the problem. But I am very sympathetic with Shubik's effort to make sure that these rules are explicit enough so that one can test whether in fact the game is feasible. And here I would like to make a brief digression to some work dealing with the feasibility of game-theoretic models, work that has been going on recently which may have some lessons for our subject.

Various game-theoretic models have been constructed for pure exchange models, with the objective of setting up rules of the game so that the solution would turn out to be the competitive (Walrasian) solution. Those of us who have gone through the usual microeconomics education would think that this had been accomplished a long time ago. Isn't that what the whole story of the Walrasian auctioneer is about? Well, some people (especially Frank Hahn) have insisted that you usually do not have an auctioneer. Okay, so there is a problem: can you set up a game-theoretic market model without having an auctioneer, just with actual flesh and blood agents, and still yielding the competitive outcome?

A few years ago, David Schmeidler, a few other people, and I started constructing such models. When you try to do that, it is natural to use as motive

power to push the players toward equilibrium something in the nature of excess demand. The problem is, what does it mean to have excess demand in a formal game model? Well, one way that this works out is that you are really permitting interim infeasible allocations. That obviously is very dangerous. What if there is a police raid, and the game is interrupted in the middle, before equilibrium has been reached? How are they going to pay off? So it is highly desirable to have a model in which the mathematics guarantees feasibility at every step, not only at equilibrium.

Schmeidler and others have proceeded to take care of the problem essentially by devising a balanced game. By *balanced* I simply mean that things add up properly: the sum of net trades is zero—not only at equilibrium but also away from equilibrium. But balance is only one of the two feasibility requirements. The other is what we call *individual feasibility,* which means that, for example, you rule out negative holdings because (in models without credit) negative holdings lack a sensible physical interpretation.

So the question arose whether it is possible to devise a game model which would be individually feasible as well as balanced and still yield a Walrasian outcome. Let me stress here that we had, in this work, one less constraint than some other model builders because we were not claiming to be constructing a positive theory model, only a normative one. In other words, we were not asking whether our model mirrors a real market, but rather an easier question: can we just set up rules of the game which would produce competitive market results?

Let me, then, state a problem currently being studied by Maskin, Postlewaite, and me. It is to set up rules of a game which (at all times, not merely at equilibrium) are balanced and individually feasible and produce competitive market results at equilibrium. It turns out from an example constructed by Postlewaite, and rather to our surprise, that if you insist on both feasibility conditions, it is not possible in general to set up a game which would give ordinary Walrasian outcomes. (It is possible if you have the kind of technical assumptions that rule out boundary solutions. You could do it, for example, if you have Cobb-Douglas utility functions, but not in general, when boundary solutions are possible.)

I point this out because it shows that there are serious difficulties in fitting together the ideas that would be admissible in the context of a genuine game model and the ideas that we bring in from what people refer to as general equilibrium theory but what is really the Walrasian static theory (which is, of course, a much narrower concept). And for this reason, although I may not like every one of the devices Shubik has employed, I very much applaud his insistence on checking on the feasibility aspect.[1] What I am saying is this: if you believe that the game model is the appropriate one, and if you insist on the formal feasibility aspect of it, even aside from whether the model is realistic, you must expect some very serious problems in modeling competitive markets. Whether this means that we should expect the economy to be other than as the usual competitive Walrasian model tells us it is, or whether it means that our modeling techniques are faulty is still an open question.

One aspect of solutions of noncooperative games may deserve some additional attention. We know from the elementary two-person zero-sum examples in von Neumann and Morgenstern that there may exist no pure (that is,

[1]In fact, it was a paper by Shapley and Shubik dealing with commodity money, where they did not get optimality although they had the feasibility very much under control, that stimulated me to go into this particular line of research.

nonrandomized) strategy Nash equilibrium. Consider, then, a two-person game in which both players' (unique) equilibrium strategies are mixed (randomized). Here the probability distribution (the mix) of each player's strategies is fixed (at the optimal mix), but the actual (pure) strategies used will vary from one play to another. By the definition of *Nash equilibrium*, any alternative mix would not raise (and might lower) the mathematical expectation of utility for a given agent given the (probabilistic) behavior of the other.

Let us think of one player as the monetary authority and the other as the public. Assuming that the game has a unique Nash equilibrium, one may define the problem of finding the optimal policy for the authority as simply that of finding the strategy mix maximizing its expected utility when the public plays its Nash strategy mix.

Thus, if the public's behavior is consistent with the Nash hypothesis, the authority can assure itself of attaining the value of the game (its expected utility at the Nash equilibrium) by playing its appropriate mixed strategy.[2] The point is that the public cannot defeat this strategy even if the authority's policy mix is revealed in advance. I shall not try to judge whether this has a bearing on the recent debates concerning the ineffectiveness of policies in an environment of rational expectations. (See Hurwicz 1951.[3])

Returning to controversial issues in game modeling, let me say that I am also very sympathetic with Shubik's approach to noncooperative games. In fact, I have just used the same approach, namely, that of Nash equilibrium. But we know that this concept of equilibrium has been subjected to many criticisms. It would be, I believe, a little dangerous to identify the general notion of a noncooperative game equilibrium with a particular notion that Nash proposed. Just to mention one example (and again I am not talking about empirical merit but philosophically), an alternative notion of a noncooperative game is where everybody is maximizing the minimum possible payoff (see Hurwicz 1953).[4] Perhaps the reality is that we want neither Nash nor "maximin" equilibria, but something else. We should not get too committed to any one of these game concepts at present.

What makes me think of that in particular is another aspect of Shubik's work. He notes that he is confining himself to relatively simple strategies. In his model, essentially what happens is that first everybody makes a bid on how much money they would like to get from the central bank on this first day of creation, and then they tell the trading posts how many dollars they would spend on that commodity. Ultimately they will be told how many units they will get at what price. Well, the particular aspect of simplification here is the absence of *sequential* strategies; they are all *one-move* strategies. Yet when we talk about games (say, chess), especially in extensive form, we think of a strategy as something more than a move, namely, a rule prescribing what move to make given the information concerning the preceding moves. (In fact, the term *strategy* was introduced to mean something more than a move.) In Shubik's model, however, the move and the strategy are essentially the same

[2]Interestingly enough, the Treasury Department recently decided to follow such a mixed strategy by not announcing in advance the timing of future gold sales. A Treasury official indicated that one objective was to deter speculation. (See *Minneapolis Star*, October 17, 1979, p. 3.)

[3]Author names and years refer to the works listed at the end of this book.

[4]Recently such "maximin" equilibria have been studied by William Thomson of the University of Minnesota and Eric Maskin at M.I.T.

thing: the game is almost a one-move game.

It would be natural to think of more complex behavior. The strategy could very well be that I will do such and such on the first move, and then, depending on what the other players do, I will go to the left or to the right, and so on. Going further in that direction, you get into the theory of "supergames" which have the unpleasant feature that there is an overabundance of possible equilibria. But what is the conclusion from this unpleasantness? Should we ignore the fact that people do think a couple of moves ahead? Or should we conclude that we are not using the right concept of equilibrium? I think such questions should loom rather large on our agenda.

Shubik indicated in a comment that he is basically a believer in infinite horizons, but that in fact he works with finite ones. That is true of many people because it is mathematically convenient. To compensate for the horizon truncation, Shubik uses a certain device, the surrender value of capital goods at the end of the creation. (In his world there is not only a first day of creation but also a day of judgement, when everything gets cashed in.) He feels that this device yields solutions in a finite-horizon world which correspond to stationary solutions for infinite horizons. Whether this is the case is very worth pursuing, and I would hope that it works out that way; but I think that at the moment this is just a heuristic conjecture. Furthermore, it is not out of the question that one could approach this problem of infinite horizons directly (without truncation), as has been done in other fields of allocation theory through infinite-dimensional linear spaces. Perhaps somehow these two kinds of approaches could be brought together.

Let me go back for a moment to an aspect of evolutionary theories on which I think I have quite a bit in common with the suggestions or hints that Tobin gave us yesterday. I am quite impressed by some of the research (by Schelling and Ullman-Margalit) on how social conventions develop. An example is an attempt to explain why, in a given country, people always drive on one side of the road. If you imagine that people choose randomly which side of the road to drive on, you can see why there might be some evolutionary tensions developing. Ullman-Margalit examined the Nash equilibria in a game of this kind, and the basic research question there is (from a short-run static point of view) whether it might turn out that perhaps the right conventions will not develop. In that case, there might be sufficient mortality, so that kind of society would not have a very high probability of survival as compared with others. Thus, if you added to the game-theoretic models a Darwinian evolutionary component, you could imagine a theory of how social conventions come into existence. I think it would be very worthwhile to look at money phenomena, fiat money in particular, from that point of view. We would then have an analytical theory helpful in explaining historical processes and not merely the final asymptotic outcome. And so, just as Shubik was referring to mathematical institutional economics, this would be mathematical historical economics. (You can see that there is quite a bit of imperialism on the part of mathematical economists.)

5. Normative Implications

I have one other comment, related to the normative versus positive aspects. In general, the presentations that we have had here have been in the spirit of positive science, in the sense that people try to explain existing monetary institutions and phenomena. They try to explain the essential features of

monetary institutions as they exist. It is true there is a normative aspect, for instance, in Wallace's model, having to do with appropriate open market policies. But as far as the institutions are concerned, they are treated in a positive rather than normative way.

I happen to be interested in the design of economic institutions. Therefore, I would regard as a real test of these theories, not just to check whether they fit all the wiggles in, say, the historical interest rate curves, but to see to what extent they could, coming from the analytical side, result in the invention of some new institutional structures which might turn out to be of value to people who really run the system. It is my impression that most economic institutional innovations have been due not to an economist but to a lawyer, a practical banker, or perhaps a politician. I would like to reverse this to some extent. To illustrate: imagine us economists around 1880 setting up a model with enough free parameters for institutional arrangements so that when we solve it for the *institutional optimum* it turns out that we have invented the Federal Reserve System. That is what I mean by an analytical approach to the design. Of course, we are still very far away from being able to do this. But it would be interesting to see to what extent the models we have seen have in them enough leeway so that one could examine the comparative performance of alternative institutional frameworks.

6. A Final Note

Finally, let me say that the game framework has very considerable generality, because it permits one to study virtually all situations in which everybody is trying to optimize in some way. Thus the game-theoretic approach is much more general than the particular Nash or von Neumann and Morgenstern models we so often use. But even generalized game models are too specialized because they tend to ignore aspects of the economic process where informational decentralization is particularly important. An analysis of such informational issues would by far exceed the appropriate scope for my discussion. Let me, therefore, only note that Lucas' concluding comment concerning the parameters θ in his model (where agents observe their own θ but not anyone else's) is an example of dispersion of information, with significant implications for the efficiency of resource allocation.