

IS-LM: an explanation

The *IS-LM* diagram, which is widely, but not universally, accepted as a convenient synopsis of Keynesian theory, is a thing for which I cannot deny that I have some responsibility. It first saw the light in a paper of my own, "Mr. Keynes and the Classics" (1937), but it was actually written for a meeting of the Econometric Society in Oxford in September 1936, just eight months after the publication of *The General Theory* (Keynes, 1936). (There I used different lettering but here I keep that which has become conventional.) And this is not my only connection with it; I also made use of it in some chapters (11-12) of my book *The Trade Cycle* (1950), and again in a paper which appears as "The Classics Again" in my *Critical Essays* (1967).¹ I have, however, not concealed that, as time has gone on, I have myself become dissatisfied with it. I said, in my contribution to the Festschrift for Georgescu-Roegen, that "that diagram is now much less popular with me than I think it still is with many other people" (1976, p. 140-1). In the reconstruction of Keynesian theory which I published at about the same time (1974), it is not to be found. But I have not explained the reasons for this change of opinion, or of attitude. I shall try, in this article, to do so.

The author is a Nobel Laureate in Economics and Professor Emeritus, Oxford University. This article was originally written for presentation to the Marshall Society in Cambridge, England, in November 1979 and repeated at a symposium at the European University Institute, Florence, Italy, in May 1980. It has undergone considerable changes between, and since, these presentations as a result of the discussions that took place on those occasions, and for other reasons.

¹ The date of this latter paper is really 1958, when it appeared, in an earlier version, in the *Economic Journal* as a review of Patinkin. I still believe that the use I made of *IS-LM* in that paper is perfectly legitimate. I am much less sure about the version in *The Trade Cycle* (1950).

I

I must begin with the old story. "Mr. Keynes and the Classics" was actually the fourth of the relevant papers which I wrote during those years. The third was the review of *The General Theory* that I wrote for the *Economic Journal*, a first impression which had to be written under pressure of time, almost at once on first reading of the book. But there were two others that I had written before I saw *The General Theory*. One is well known, my "Suggestion for Simplifying the Theory of Money" (1935a), which was written before the end of 1934. The other, much less well known, is even more relevant. "Wages and Interest: the Dynamic Problem"² was a first sketch of what was to become the "dynamic" model of *Value and Capital* (1939). It is important here, because it shows (I think quite conclusively) that that model was already in my mind before I wrote even the first of my papers on Keynes.

I recognized immediately, as soon as I read *The General Theory*, that my model and Keynes' had some things in common. Both of us fixed our attention on the behavior of an economy *during a period*—a period that had a past, which nothing that was done during the period could alter, and a future, which during the period was unknown. Expectations of the future would nevertheless affect what happened during the period. Neither of us made any assumption about "rational expectations"; expectations, in our models, were strictly exogenous.³ (Keynes made much more fuss over that than I did, but there is the same implication in my model also.) Subject to these *data*—the given equipment carried over from the past, the production possibilities within the period, the preference schedules, and the given expectations—the actual performance of the economy within the period was supposed to

² See Hicks (1935b). The paper is reprinted in the supplement which was added to the second edition of my *Theory of Wages* (1963).

³ It is true that when I came to "Mr. Keynes and the Classics" I did propose to make investment depend on current output (just as Kaldor was to do, three years later, in his "Model of the Trade Cycle" [1940]). But I have never regarded this as an essential part of the *IS-LM* construction. I have fully accepted, in later work, that a capital stock adjustment principle, or some equivalent, is a better expression of what one had in mind. But whatever view one takes about this, it is still the case that it has never been intended, in any of the versions for which I am responsible, that investment changes should be entirely explicable by changes in output, of whatever sort. Even in my *Trade Cycle* book (1950), there was autonomous investment. There was always a residual element, depending on expectations, and many other things, which could vary independently.

be determined, or determinable. It would be determined as an equilibrium performance, with respect to these data.

There was all this in common between my model and Keynes'; it was enough to make me recognize, as soon as I saw *The General Theory*, that his model was a relation of mine and, as such, one which I could warmly welcome. There were, however, two differences, on which (as we shall see) much depends.

The more obvious difference was that mine was a flexprice model, a perfect competition model, in which all prices were flexible, while in Keynes' the level of money wages (at least) was exogenously determined. So Keynes' was a model that was consistent with unemployment, while mine, in his terms, was a full employment model. I shall have much to say about this difference, but I may as well note, at the start, that I do not think it matters much. I did not think, even in 1936, that it mattered much. *IS-LM* was in fact a translation of Keynes' nonflexprice model into my terms. It seemed to me already that that could be done; but how it is done requires explanation.

The other difference is more fundamental; it concerns the length of the *period*. Keynes' (he said) was a "short-period," a term with connotations derived from Marshall; we shall not go far wrong if we think of it as a year. Mine was an "ultra-short-period"; I called it a week. Much more can happen in a year than in a week; Keynes has to allow for quite a lot of things to happen. I wanted to avoid so much happening, so that my (flexprice) markets could reflect propensities (and expectations) as they are at a moment. So it was that I made my markets open only on a Monday; what actually happened during the ensuing week was not to affect them. This was a very artificial device, not (I would think now) much to be recommended. But the point of it was to exclude the things which might happen, and must disturb the markets, during a period of finite length; and this, as we shall see, is a very real trouble in Keynes.

In the rest of this article, I shall take these two issues separately, beginning with the fixprice-flexprice question, which is the easier.

II

It will readily be understood, in the light of what I have been saying, that the idea of the *IS-LM* diagram came to me as a result of the work I had been doing on three-way exchange, conceived in a

Walrasian manner. I had already found a way of representing three-way exchange on a two-dimensional diagram (to appear in due course in chapter 5 of *Value and Capital*). As it appears there, it is a piece of statics; but it was essential to my approach (as already appears in "Wages and Interest: the Dynamic Problem") that static analysis of this sort could be carried over to "dynamics" by redefinition of terms. So it was natural for me to think that a similar device could be used for the Keynes theory.

Keynes had three elements in his theory: the marginal efficiency of capital, the consumption function, and liquidity preference. The market for goods, the market for bonds, and the market for money: could they not be regarded in my manner as a model of three-way exchange? In my three-way exchange I had two independent price parameters: the price of *A* in terms of *C* and the price of *B* in terms of *C* (for the price of *A* in terms of *B* followed from them). These two parameters were determined by the equilibrium of two markets, the market for *A* and the market for *B*. If these two markets were in equilibrium, the third must be also.

Keynes also appeared to have two parameters—his *Y* (income *in terms of wage units*) and *r*, the rate of interest. He made investment depend on *r* and saving on *Y*; so for each value of *r* there should be a value of *Y* which would keep saving equal to investment—excess demand on the market for goods then being zero. This gave a relation between *r* and *Y* which I expressed as the *IS* curve. The demand for money depended on *Y* (transactions balances) and on *r* (liquidity preference). So for any given supply of money (*in terms of wage units*) there should be a relation between *r* and *Y* which would keep the money "market" in equilibrium. One did not have to bother about the market for "loanable funds," since it appeared, on the Walras analogy, that if these two "markets" were in equilibrium, the third must be also. So I concluded that the intersection of *IS* and *LM* determined the equilibrium of the system as a whole.

Now this was really, at that stage, no more than a conjecture, for I had not properly shown that the Walras analogy would fit. In Walras, all markets are cleared; but in *IS-LM* (following Keynes) the labor market is not cleared; there is excess supply of labor. Does this, by itself, upset the Walras model? I think that by now it is generally accepted that it does not. It will nevertheless be useful, for what follows, to check the matter over in detail.

In strictness, we now need four markets, since labor and goods

will have to be distinguished. But before giving them those names, let us look at the matter in terms of a general Walrasian four-goods model.

We then say that commodities A , B , C , and X are being traded, with X as standard (*numéraire*). Prices p_a , p_b , p_c are reckoned in terms of the standard; $p_x = 1$. Demands and supplies on the ABC markets are functions of the three prices. The three equations $S_a = D_a$ and so on are sufficient to determine the three prices. Further, since

$$S_x = p_a D_a + p_b D_b + p_c D_c, D_x = p_a S_a + p_b S_b + p_c S_c,$$

when the supply and demand equations are satisfied for ABC , that for X follows automatically.

There is just this one identical relation between the four equations. We could use it to eliminate the X equation, as just shown, or to eliminate any one of the other equations, while retaining the X equation. Thus the system of three prices for ABC can be regarded as determined by equations for ABC , or by equations for BCX , CAX , or ABX .

Thus far Walras. But now suppose that one of the commodities is sold on a fixprice market, where the price is fixed in terms of the standard, but where the equation of supply and demand does not have to hold. The actual amount sold will be equal to the demand or to the supply, whichever is the lower. So let p_a be fixed, with the equation $D_a = S_a$ removed. The remaining (variable) prices can still be determined from the equations $S_b = D_b$, $S_c = D_c$, for the p_a which appears as a parameter in these equations is now a constant. If it turns out that at these prices $S_a > D_a$, it is only D_a that can actually be traded. When calculating S_x and D_x , we must use this *actual* D_a for both D_a and S_a . With that substitution, we have $S_x = D_x$, as before.

And it is still possible, using this construction, to let the equation for the standard, $S_x = D_x$, replace one of the equations otherwise used, as could be done in the all-round flexprice case. For with D_a substituted for S_a , $p_a(S_a - D_a) = 0$ is an identity. The only terms in $S_x - D_x$ that survive, on application of this identity, are those which relate to the flexprice commodities B and C . The subsystem of BCX will then work in the regular Walrasian manner. We can determine p_b and p_c from any pair of the three equations that are left.

In this way, the Walrasian analogy gets over its first hurdle; but

there is another, close behind it, which may be considered more serious. We have so far been making demands and supplies depend only on prices; and for the pure case of multiple exchange with flexible prices, that may probably be accepted. But as soon as a fixprice market is introduced, it ceases to be acceptable. It must be supposed that the demands and supplies for B and C will be affected by what happens in the market for A . That can no longer be represented by the price, so it must be represented by the quantity sold. Assuming, as before, that there is excess supply in the A market, this is D_a . So demands and supplies for B and C will be functions of p_b , p_c , and D_a . The BCX subsystem would then *not* be complete in itself; but the whole system, with D_a included as a parameter, would still work in the way that has been described.

We would then have three variables to be determined, p_b , p_c , and D_a —and four equations. They are the demand-supply equations for BCX (the X equation being constructed with the *actual* D_a , as before); and there is also the demand equation for D_a , which makes D_a a function of p_b and p_c . As before, any one of the BCX equations can be eliminated. The system is determined, whichever equation we choose to eliminate.

The model is still very formal; but now it is the same kind of model as the *IS-LM* model. We could represent that as a three-way (ABX) model, in which there is just one price (p_b , which becomes the rate of interest) that is determined on a flexprice market, and one quantity (Y) which plays the part of D_a . I have deliberately taken a case which in the same formal terms is slightly more complicated, since I have admitted two flexprice markets, for B and for C . It may indeed be useful to show that there is, in principle, no difficulty in introducing a second flexprice market—or, for that matter, introducing several. It could be useful, even for macroeconomic purposes, to introduce a second flexprice market—for instance, a market for foreign exchange.

But that is not the reason I have introduced the extra market. The important use of a four-way model, in this connection, is that it enables us to consider the market for goods and the market for labor separately. And when we take them separately, quite interesting things happen.

One could construct a model in which only the market for labor was a fixprice market, and not only the rate of interest but also the price (or price level) of finished products was flexible. That would fit very exactly into the scheme which has just been out-

lined, with demand-supply equations determining D_a (employment) and the two flexible prices p_b, p_c . It is possible that Keynes himself sometimes thought in terms of that sort of model (see, for example, Keynes, 1936, ch. 21); but it cannot be this which *IS-LM* is supposed to represent. For Y is taken to be an index not only of employment, but also of output, so the prices of products also are supposed to be fixed in terms of the standard; and it is hard to see how that can be justified unless the prices of products are derived from the wage of labor by some markup rule. But if that is so, we have not one, but two, fixprice markets.

Say that A and B are fixprice markets, while C is flexprice. As long as we follow the Walrasian practice of working entirely in terms of price parameters, there is no trouble. p_a and p_b are then fixed, so that all demands and supplies are functions of the single variable p_c . p_c is determined on the market for C (or, equivalently, on the market for X) as before. And the actual amounts of A and B that are traded are D_a or S_a, D_b or S_b —whichever, at the equilibrium p_c , turns out to be the lower.

But now suppose that, as before, we change the parameters, making demands and supplies functions of D_a and D_b (assuming that there is excess supply in both markets), not of p_c only. One would at first say that at a (provisionally given) p_c , D_a would be a function of D_b and D_b of D_a ; and there need be nothing circular about that. There are just these two “curves” in the $(D_a D_b)$ plane (like supply and demand curves); at their intersection, the equilibrium is determined.

It must be this which, in the *IS-LM* model, is supposed to happen. We are now to take A to be the labor market, C the market for loanable funds (as before), and B the market for finished products (consumption goods and investment goods not being, so far, distinguished). p_a is the fixed money wage; p_b , the fixed price level of the finished products; p_c , the rate of interest, the only price that is left to be determined on a flexprice market.

How, then, do we identify the “curves”? One, which makes D_b (effective demand for products) a function of D_a (employment) is easy to find in Keynes. D_b depends on D_a , since the consumption component of D_b increases when employment increases (the consumption function), while the investment component depends on the rate of interest, provisionally given. There is no trouble about that. But what of the other “curve”—the dependence of D_a on D_b , of employment on effective demand? Keynes took it for

granted that they must go together, but the matter needs looking into. For it is here that there is a danger of going seriously wrong by neglecting time.

III

It is not true, of course, that time has been wholly neglected. As I said at the beginning, all the prices and quantities that have figured in the analysis must belong to a period; the past (before the period) and the future (beyond the period) have always been playing their regular parts. What has been neglected is the flow of time within the period. It is here that the length of the period is important.

In my own version ("Wages and Interest: the Dynamic Problem" or *Value and Capital*), the period ("week") was kept very short, so that little could happen within it. The actual outputs of products and (probably also) the actual input of labor would be largely predetermined. What could vary, considerably, would be prices. So for the study of price formation on flexprice markets, the "week" had something to be said for it.⁴ But that was not what Keynes was interested in; so he had to have a longer period.

It is not unreasonable to suppose that the prices which are established in flexprice markets, during a "week" (or even at a point of time) do reflect the expectations of traders, their liquidity positions, and so on. That is to say (it is equivalent to saying), we may fairly reckon that these markets, with respect to these data, are in equilibrium. And one could go on, as we have in fact been seeing, even while maintaining the "week" interpretation, to admit that there are some markets which are fixprice markets, in which demands and supplies do not have to be equal. Then it is only to the markets which are flexprice markets that the equilibrium rule applies. Now it would be quite hard to say, in terms of such a model, that effective demand would determine employment. It is so tempting to say that there can be no output without labor input, so that an increase in demand must increase employment (as Keynes effectively did). But the question is not one of the relation between input and output, in general; it is a question of the rela-

⁴ No more than something. I have myself become pretty critical of the *Value and Capital* temporary equilibrium method when applied to flow markets. (I do not question its validity for the analysis of markets in stocks.) See chapter 6 of my *Capital and Growth* (1965).

tion between current demand and current input, both in the current period. It is at once shown, on the "week" interpretation, that current output is largely predetermined; while, if the price of output is fixed, current demand may be greater or less than current output (stocks being decumulated or accumulated). How, then, is current input to be determined? We can only make it determinate, as a function of current demand, if we can bring ourselves to introduce some *rule*, according to which the extent of excess demand (or supply) in the current period will affect the employment that is offered, again in the current period. If we have such a rule, we can complete the circle, and show, in the current period, effective demand and employment simultaneously determined.

It is quite a question whether we would be justified, in general, in imposing such a rule.⁵ For the effect on current input of excess demand or supply in the product market is surely a matter of the way in which the excess is interpreted by decision makers. An excess which is expected to be quite temporary may have no effect on input; it is not only the current excess but the expectation of its future which determines action. It may be useful, on occasion, to suspend these doubts, and so to make models in which current input depends on excess demands (or supplies) in the product markets according to some rule. But one can hardly get a plausible rule while confining attention to what happens within a single period. So it would seem that the proper place for such a proceeding is in sequential models, composed of a succession of periods, in each of which the relevant parameters have to be determined; there is then room for linkages between the periods, and so for lags. I have myself made some attempts at the construction of such models.⁶ I think they have their uses, but they are not much like *IS-LM*.

If one is to make sense of the *IS-LM* model, while paying proper attention to time, one must, I think, insist on two things: (1) that the period in question is a relatively long period, a "year" rather

⁵ My mind goes back to a conversation I had, a few years ago, with a distinguished economist, who might at an earlier date have been reckoned to be a Keynesian. I was saying to him that I had come to regard J. S. Mill as the most undervalued economist of the nineteenth century. He said, "Yes, I think I understand. *Demand for commodities is not demand for labour*. It is true, after all."

⁶ In particular, in *Capital and Growth* (1965, chs. 7-10).

than a "week": and (2) that, because the behavior of the economy over that "year"⁷ is to be *determined* by propensities, and such-like data, it must be assumed to be, in an appropriate sense, *in equilibrium*. This clearly must not imply that it is an all-round flexprice system; the exogenously fixed money wage, and (as we have seen) the exogenously fixed prices of products must still be retained. But it is not only the market for funds, but also the product market, which must be assumed to be in equilibrium.

Though the prices of products are fixed, it is not necessary to suppose that there is disequilibrium in the product market. Even at the fixed price and fixed wage, when these are maintained over the relatively long period, it will pay producers to adjust supply to demand, as far as they can. For a loss is incurred in producing output that cannot be sold, and a profit is forgone when output that could profitably be sold is not produced. There are problems of adjustment, of which sequential analysis can take account; but there may be purposes for which it is legitimate to leave them to one side. We should then assume that the product markets, during the "year," are in equilibrium and remain in equilibrium. And since it is to be continuing equilibrium, maintained throughout the "year," this must mean that plans (so far as they relate to the proceedings of the year) are being carried through without being disturbed.

It is not, I think, inconsistent to suppose that the product markets are in equilibrium, while the labor market is not in equilibrium. For although there are some possibilities for adjusting supply to demand in the case of unemployment on the labor market (even while prices and wages remain unchanged), as by withdrawal of elderly labor from the market, or by departure of migrants, they are surely less than the corresponding possibilities in the market for products. A model which permits excess supply in the labor market, but no product market disequilibrium, is not inconsistent.

Once we allow ourselves to assume that product markets remain in equilibrium, things become easier. For once we assume that production plans, during the period, are carried through consistently, we have the relation between current input, during the period, and current output, during the period (which has been made equal to effective demand within the period) for which we have been looking. There are some difficulties about production processes which

⁷The *year* must clearly be long enough for the firm to be "free to revise its decisions as to how much employment to offer" (Keynes, 1936, p. 47, n. 1).

were begun before the commencement of the period, and others which will not be completed at the end of the period, but these, perhaps, may be overlooked. We can then proceed to the two "curves" in the $(D_a D_b)$ plane, by which employment and effective demand are simultaneously determined.

The goal is reached, but at a considerable price. For how, after all, can this equilibrium assumption be justified? I do not think it can be justified for all purposes, maybe not for the most important purposes; but I have come to think that there is one purpose for which it may sometimes be justified. I have described this purpose in chapter 6 of my book *Causality in Economics* (1979); an abstract of the argument of that chapter may be given here.

We are to confine attention to the problem of explaining the past, a less exacting application than prediction of what will happen or prescription of what should happen, but surely one that comes first. If we are unable to explain the past, what right have we to attempt to predict the future? I find that concentration on explanation of the past is quite illuminating.

We have, then, facts before us; we know or can find out what, in terms of the things in which we are interested, did actually happen in some past year (say, the year 1975). In order to explain what happened, we must confront these facts with what we think would have happened if something (some alleged cause) had been different. About that, since it did not happen, we can have no factual information; we can only deduce it with the aid of a theory, or model. And since the theory is to tell us what would have happened, the variables in the model must be determined. And that would seem to mean that the model, in some sense, must be in equilibrium.

Applying these notions to the *IS-LM* construction, it is only the point of intersection of the curves which makes any claim to representing what actually happened (in our "1975"). Other points on either of the curves—say, the *IS* curve—surely do not represent, make no claim to represent, what actually happened. They are theoretical constructions, which are supposed to indicate what *would have happened* if the rate of interest had been different. It does not seem farfetched to suppose that these positions are equilibrium positions, representing the equilibrium which corresponds to a different rate of interest. If we cannot take them to be equilibrium positions, we cannot say much about them. But, as the diagram is drawn, the *IS* curve passes through the point of inter-

section; so the point of intersection appears to be a point on the curve; thus it also is an equilibrium position. That, surely, is quite hard to take. We know that in 1975 the system was not in equilibrium. There were plans which failed to be carried through as intended; there were surprises. We have to suppose that, for the purpose of the analysis on which we are engaged, these things do not matter. It is sufficient to treat the economy, as it actually was in the year in question, as if it were in equilibrium. Or, what is perhaps equivalent, it is permissible to regard the departures from equilibrium, which we admit to have existed, as being random. There are plenty of instances in applied economics, not only in the application of *IS-LM* analysis, where we are accustomed to permitting ourselves this way out. But it is dangerous. Though there may well have been some periods of history, some "years," for which it is quite acceptable, it is just at the turning points, at the most interesting "years," where it is hardest to accept it.

What I have been saying applies, most directly, to the *IS* curve; what of the other?

In elementary presentations of the *IS-LM* model, the *LM* curve is supposed to be drawn up on the assumption of a given stock of money (the extension to a stock of money given in terms of wage units comes in only when the level of money wages is allowed to vary, so I shall leave it to one side). It is, however, unnecessary to raise those puzzling questions of the definition of money, which in these monetarist days have become so pressing. For I may allow myself to point out that it was already observed in "Mr. Keynes and the Classics" that we do not need to suppose that the curve is drawn up on the assumption of a given stock of money. It is sufficient to suppose that there is (as I said)

a given monetary system—that up to a point, but only up to a point, monetary authorities will prefer to create new money rather than allow interest rates to rise. Such a generalised (*LM*) curve will then slope upwards only gradually—the elasticity of the curve depending on the elasticity of the monetary system (in the ordinary monetary sense). (p. 157)⁸

That is good as far as it goes, but it does not go far enough. For here, again, there is a question of time reference; and it is a very tricky question. The relation which is expressed in the *IS* curve is a flow relation, which (as we have seen) must refer to a period, such as the year we have been discussing. But the relation expressed in

⁸ In the reprint of this paper in my *Critical Essays* (1967), the passage appears on p. 140.

the *LM* curve is, or should be, a stock relation, a balance-sheet relation (as Keynes so rightly insisted). It must therefore refer to a point of time, not to a period. How are the two to be fitted together?

It might appear, at first sight, that we must proceed by converting the stock relation into a relation which is to hold for the period—treating it, in some way, as an average of balance-sheet relations over the period. But this has to be rejected, not merely because it is clumsy, but because it does not get to the point. It has been shown that, if we adopt the equilibrium interpretation, on the *IS* side, the economy must be treated *as if* it were in equilibrium over the period; that means, on the *IS* side, that the economy must remain in flow equilibrium, with demands and supplies for the flows of outputs remaining in balance. It would be logical to maintain that on the *LM* side the economy must be treated similarly. There must be a *maintenance* of stock equilibrium.

I have examined the relation between stock equilibrium and flow equilibrium in chapter 8 of my *Capital and Growth* (1965), where I have shown that the maintenance of stock equilibrium over the period implies the maintenance of flow equilibrium over the period; so it is a sufficient condition for the maintenance of equilibrium over time, in the fullest sense. A key passage is the following:

Equilibrium over time requires the maintenance of stock equilibrium; this should be interpreted as meaning that there is stock equilibrium, not only at the beginning and end of the period, but throughout its course. Thus when we regard a “long” period as a sequence of “short” periods, the “long” period can only be in equilibrium over time if every “short” period within it is in equilibrium over time. Expectations must be kept self-consistent; so there can be no revision of expectations at the junction between one “short” period and its successor. The system is in stock equilibrium at each of these junctions; and is in stock equilibrium with respect to these consistent expectations. That can only be possible if expectations—with respect to demands that accrue within the “long” period—are *right*. Equilibrium over time thus implies consistency between expectations and realisations within the period. It is only expectations of the further future that are arbitrary (exogenous) as they must be. (pp. 92-93)⁹

That is the formal concept of full equilibrium over time; I do

⁹ I have made a few minor alterations in wording to make it possible to extract the passage quoted from the rest of the chapter.

not see how it is to be avoided. But for the purpose of generating an *LM* curve, which is to represent liquidity preference, it will not do without amendment. For there is no sense in liquidity, unless expectations are uncertain. But how is an uncertain expectation to be realized? When the moment arrives to which the expectation refers, what replaces it is fact, fact which is not uncertain.

I have suggested, in my most recent book (1979), a way of cutting the knot, but I do not have much faith in it.

We must evidently refrain from supposing that the expectations as they were before April (some date in the middle of the "year") of what is to happen after April, were precise expectations, single-valued expectations; for in a model with single-valued expectations, there can be no question of liquidity. And we must also refrain from the conventional representation of uncertain expectations in terms of mean and variance, since that makes them different in kind from the experiences which are to replace them. There is, however, a third alternative. Suppose we make them expectations that the values that are expected, of the variables affecting decisions, will fall within a particular range. This leaves room for liquidity, since there are no certain expectations of what is going to happen; but it also makes it possible for there to be an equilibrium, in the sense that what happens falls within the expected range. A state of equilibrium is a state in which there are no surprises. What happens (during the period) falls sufficiently within the range of what is expected for no revision of expectations to be necessary (p. 85).

As far as I can see, that is the only concept of equilibrium over time¹⁰ which leaves room for liquidity.

IV

I accordingly conclude that the only way in which *IS-LM* analysis usefully survives—as anything more than a classroom gadget, to be superseded, later on, by something better—is in application to a particular kind of causal analysis, where the use of equilibrium methods, even a drastic use of equilibrium methods, is not inappropriate. I have deliberately interpreted the equilibrium concept, to be used in such analysis, in a very stringent manner (some would say a pedantic manner) not because I want to tell the applied economist, who uses such methods, that he is in fact committing himself to anything which must appear to him to be so

¹⁰ I should here make an acknowledgement to G. L. S. Shackle, who in much of his work has been feeling in this direction.

ridiculous, but because I want to ask him to try to assure himself that the divergences between reality and the theoretical model, which he is using to explain it, are no more than divergences which he is entitled to overlook. I am quite prepared to believe that there are cases where he is entitled to overlook them. But the issue is one which needs to be faced in each case.

When one turns to questions of policy, looking toward the future instead of the past, the use of equilibrium methods is still more suspect. For one cannot prescribe policy without considering at least the possibility that policy may be changed. There can be no change of policy if everything is to go on as expected—if the economy is to remain in what (however approximately) may be regarded as its *existing* equilibrium. It may be hoped that, after the change in policy, the economy will somehow, at some time in the future, settle into what may be regarded, in the same sense, as a new equilibrium; but there must necessarily be a stage before that equilibrium is reached. There must always be a problem of traverse. For the study of a traverse, one has to have recourse to sequential methods of one kind or another.¹¹

¹¹ I have paid no attention, in this article, to another weakness of *IS-LM* analysis, of which I am fully aware; for it is a weakness which it shares with *The General Theory* itself. It is well known that in later developments of Keynesian theory, the long-term rate of interest (which does figure, excessively, in Keynes' own presentation and is presumably represented by the r of the diagram) has been taken down a peg from the position it appeared to occupy in Keynes. We now know that it is not enough to think of the rate of interest as the single link between the financial and industrial sectors of the economy; for that really implies that a borrower can borrow as much as he likes at the rate of interest charged, no attention being paid to the security offered. As soon as one attends to questions of security, and to the financial intermediation that arises out of them, it becomes apparent that the dichotomy between the two curves of the *IS-LM* diagram must not be pressed too hard.

The modern "post Keynesian" view of interest takes its origin from R. F. Kahn (1953). But I have done a good deal of work on it myself, in chapter 23 of *Capital and Growth* (1965), in lecture 3 of "The Two Triads" (1967), in the second chapter of *The Crisis in Keynesian Economics* (1974) and in the section on Keynes in *Economic Perspectives* (1977, pp. 77 ff).

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