NOTES AND MEMORANDA

On a Method of Statistical Business-Cycle Research. A Reply

1. In the Economic Journal of September 1939, p. 558, Mr. Keynes discusses the method of statistical business-cycle research used in Vol. I of the League of Nations publication of which I am the author.[1] Mr. Keynes has serious objections and numerous questions. Although part of these objections and questions have been answered in the second volume, which has appeared recently, there remain a number of points which I think it is worth while to discuss separately. I shall follow Mr. Keynes's argument exactly in the order in which he gave it.

2. To begin with, Mr. Keynes formulates a number of *conditions* which, in his mind, must be fulfilled in order that the method of multiple correlation analysis may be applied. With the formulation he gives on p. 560—viz. that "the most he may be able to show is that, if they (*i.e.*, certain given factors) are *veræ causæ*, either the factors are not independent or the correlations involved are not linear, or there are other relevant respects in which the economic environment is not homogeneous over a period of time—I find myself only partly in agreement. *I think something more can be shown*, viz. that *in so far as one agrees*

(a) that the explanatory variables chosen explicitly are the relevant ones;

(b) that the non-relevant explanatory variables may be treated as random residuals, not systematically correlated with the other explanatory variables; and

(c) that the mathematical form of the relation is given,

certain details on the probability distributions of their "influences" can be given (in such cases in which the "bunch-map" does not "explode"). These details are the central (most probable) values and the standard deviations of the regression coefficients, measuring the "influences." In plain terms: these influences can be measured, allowing for certain margins of uncertainty.

[1] *Statistical Testing of Business Cycle Theories. A Method and its Application to Investment Activity.* Geneva, 1939.

Although I am adding three further conditions, I do not think these mean too serious a restriction.

This may best be demonstrated by the answers to Mr. Keynes's further questions.

3. Mr. Keynes goes on to ask: "Am I right in thinking that *the method . . . essentially depends on the economist having furnished . . . a complete list?*" I think this is right—indeed, it is my condition (*a*) above—but, as has been stated in § 2 of my first volume, it does not matter, if non-relevant factors have been forgotten, and therefore the restriction seems to me far less serious than Mr. Keynes assumes.

What factors are relevant and what are not will not always be clear beforehand. It must then be tried out (cf. § 4 below).

As to condition (*b*), this may be tested afterwards—*e.g.*, by calculating the serial correlation for the residuals and the bunch maps. This has been done on pp. 80–90 for some of the more important cases (cf. also § 6 below).

The implications of condition (*c*) will be considered below (§ 9).

4. "*The method is one neither of discovery nor of criticism,*" Mr. Keynes continues. I do not understand this, since I see the following possibilities of discovery or of criticism. As to discovery, it sometimes happens that the course of the curves itself suggests that some factor not mentioned in most economic textbooks must be of great importance. As an example I may point to the case of the "explanation of consumers' outlay for the U.S. after the war." It appeared that capital gains had a considerable influence on consumption, and it would have been difficult to learn this from one of the usual textbooks on economics.

As another example one could take the "explanation" of share prices in the United States between 1919 and 1932, where it is very clear that fluctuations in dividends and interest rates alone cannot explain share-price fluctuations. One has to add the rate of increase in share prices some months ago in order to get a satisfactory fit. This "discovery" is partly due to the method of multiple correlation, since it would be difficult to find out otherwise whether the fit is good after inclusion of the new factor.

As to the possibility of criticism, it seems to me that the value found for one or more of the regression coefficients may imply a criticism on one or more of the theories that have been used. Many theories *e.g.* hold that there is a considerable influence of the rate of interest on the demand for money or on investment activity, and our results for the U.S., which are given in Vol. II, suggest that this influence is small, or at least has been small in that country during that period.

5. A further point raised by Mr. Keynes is how it will be possible *to supplement the results of multiple correlation analysis by other information*, especially by information of a non-statistical nature. Mr. Keynes is of the opinion that, since the regression equation completely explains the course of the phenomenon under explanation, there will be no room left for other information. I think the point is in the fact that the explanation obtained by correlation analysis is not complete. It has been very clearly stated, I think, that there always remain residuals which are unexplained. It may happen, and in fact has happened several times, that some of these residuals can be explained by additional information. It may be *e.g.* that there is a negative residual in any given year because there was a strike. Such was the case for residential building in Stockholm in 1933, and, in fact, the largest negative residual in the graph on p. 101 is in 1933.

There may also be an exceptional residual due to a tax being changed in some given year. Or it may be known that in that given year there was a panic. One may also take account of additional information by making some correction beforehand. Such was the case in 1926 in Great Britain; this is why estimated pig-iron consumption was corrected before the correlation calculation was made (cf. p. 158, note (3)). I think all these cases are examples of opportunities for supplementary information to play its part in the explanation of economic phenomena.

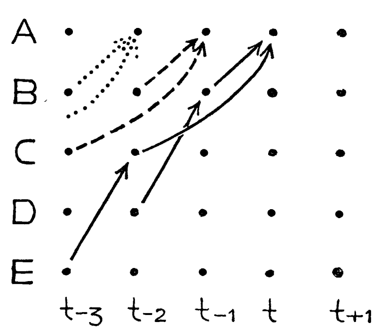
6. There seems to be some misunderstanding in Mr. Keynes's discussion as to the question whether the explanatory variables should be *independent* of each other.

There is a statistical and there is an economic meaning of the word independent. So far as the *statistical* meaning is concerned, independency can only mean that they are uncorrelated. This is clearly not necessary. The only conditions required by the multiple correlation analysis in this respect are that the residuals —*i.e.*, the neglected influences—are not systematically correlated with any of the explanatory variables, and that the correlations between any two or any greater number of explanatory variables are not so large that the "bunch map" "explodes." This is a less stringent condition than independency in the above sense.[1]

Economic dependency or independency has to be understood

[1] In many cases the condition on the bunch map is already more stringent than is necessary, as I pointed out in § 6 of Vol. I, when discussing Dr. Koopmans's method. Beams for less important variables may be disregarded.

in another way. Here it seems useful to make a distinction between first causes, second causes, etc. These notions may be illustrated by the diagram (graph 1) where each dot represents a certain phenomenon during a certain time unit. Dots on one (horizontal) row represent the same phenomenon at consecutive time units. Dots in one (vertical) column represent various phenomena at the same moment. If now a change *e.g.* in phenomenon A can only be caused by one in B one time unit before or by one in C two time units before, we indicate this by the arrows linking up B_{t-1} with A_t and C_{t-2} with A_t. This couple of causal connections—existing for every time unit t—represents the body of direct causal connections to which A is subject.



GRAPH 1.—Symbolic representation of logical structure of dynamic economics (sequence analysis).

Changes in B at moment $t - 1$ and C at moment $t - 2$ may be called the first causes of any change in A at moment t.

But, in turn, changes in B may be due to changes in D one time unit before. Such a change in D at moment $t - 2$ would therefore be a " second " cause of a change in A at moment t. The same would be true for a change in E at moment $t - 3$, which itself is a first cause of a change in C at moment $t - 2$.

Now, the aim of our " explanations " is to explain the fluctuations in any variable by their first causes. Second causes should be included in the explanation of each of those variables the fluctuations of which are first causes, and so on. In that way all causal connections forming together the logical structure of our model find their place. It would be wrong to include in an " explanation " a first cause together with one of the second causes that explains that first cause. A concrete example may illustrate this. Let demand for cotton cloth depend on its price, and let this price depend on the price of raw cotton. To explain the demand for cotton cloth by both its price and that of raw cotton would be nonsense. There first cause and second cause would have been taken together.

If a first cause is said to be dependent on a second cause, then in this sense the explanatory variables should not be dependent.

The fallacious procedure of including both a first and a second cause in one "explanation" should be carefully distinguished from another procedure which I think is absolutely legitimate. Suppose—as is the case in Schultz's well-known analysis of the sugar market [1]—that the price in year t is (by the demand relation) determined by production in year t, whereas production in year t is determined by price in year $t - 1$. Speaking loosely and somewhat inaccurately, we may say that there are two relations between prices and production. It is more accurate, however, to say that there are two relations, of which one is between prices and production without lag and one between prices and production next year.

The lag is not essential. It may be that there is no lag, but that prices depend on production and incomes through the demand relation, and production depends on prices and costs through the supply relation. There are, then, two relations between prices and production, the one with incomes as a third variable, the other with costs as a third variable. An example is to be found in Vol. II, p. 70.

This is also the situation, I think, in the case of investments and profits. Investments depend on profits and, say, interest rates through the investment plans of the entrepreneurs; and profits depend on investments and consumption and costs through the relation determining—almost by definition—profits. I do not think there is a difficulty of principle in this situation. For the statistical testing there may be a difficulty of accuracy. Provided, however, the economist can guarantee us what variables enter into each of these relations and reliable statistics exist for all these variables, the statistician is able to estimate the degree of uncertainty in the results in the ordinary way.

This question has also been discussed at length on p. 60 of Vol. I, and there it has been pointed out that the two relations may exist at the same time and may be tested statistically under certain conditions. It would seem that Mr. Keynes has not read these pages in connection with his question (3).

7. Next Mr. Keynes discusses the question, also raised in

[1] Henry Schultz, *The Theory and Measurement of Demand*, Chicago, 1938, p. 175.

Vol. I, whether *the influence of profit rates and interest rates on investment is such that it is only the difference between these two variables which affects the volume of investment.* The only thing I could do, in order to answer this question, was to see whether, in those cases where I had at my disposal figures on profit *rates*, the coefficients found for the profit rate and the interest rate were or were not the same but with the opposite sign. The theory that it is the difference between the two variables which affects investment activity would require this equality with negative sign of the two coefficients. In fact, it appeared (see p. 66) that the coefficients found in the two cases concerned were of about equal order of magnitude and showed opposite signs. In the other cases, where profit figures did not represent profit rates, it was impossible to make this test. So the result does not seem to be bad.

In the other cases—where only total profits were known, not profit rates—as soon as we know the average amount of capital C on which these total profits were earned, it would be possible to make the same type of calculations. This may be done by calculating profit rates year by year, and introducing this new series, instead of total profits, in the correlation calculation. In such cases where total capital only shows slow movements we may say that, as an approximation, the new series will be proportional to the old one :

$$\text{profit rates} = \frac{1}{C} \text{ total profits,}$$

where C is approximately constant and equal to total capital.

This being so, it follows that the regression coefficient which will be found for profit rates will be approximately C times that for total profits—provided that no serious intercorrelations with other explanatory series are present. If the theory is correct that the difference between profit rate and interest rate is relevant, this must show itself now in the equality (but sign) of that C times as large coefficient for profit rates and the coefficient for interest rates.

Mr. Keynes finds " devastating inconsistencies " in the fact that *the regression coefficients found for profits, in the " explanation " of investment activity, are widely different* in the various countries. The explanation I " gaily " give—as he says—does not seem to be clear to him. I shall give it at some more length. It is in the fact that profit figures available for the various countries are not comparable. Mr. Keynes sums up what they are : sometimes profit rates, sometimes absolute amounts, etc. Only if I had had available the same type of profit figures for all countries could I have made comparisons. This I did for such series as iron prices and interest rates, which are, in fact, comparable for the various countries. To make comparisons between the regression coefficients for profits is not permissible, and this is the reason why I did not make any.

One phrase of Mr. Keynes's in this connection is obscure to me : " He insists that his factors must be measurable, but about the units in which he measures them he remains singularly care-free, in spite of the fact that in the end he is going to add them all up." I suppose the misunderstanding is in the tail : I do not add up the " factors " (in my terminology the " explanatory variables "), but I add up their " influence," which is the product of the variable and its regression coefficient (cf. p. 22); and this product is independent of the units in which the explanatory variable is expressed. (Not, however, of the units in which the "variable to be explained" (the dependent variable) is measured.[1])

8. Speaking on *expectations*, Mr. Keynes says (p. 563, note) : " But there is no room for expectations so far as I can discover in the theory of investment with which the economist has supplied Prof. Tinbergen." May I, in this respect, draw Mr. Keynes's attention to pp. 34, 35 and 36, where expectations have been discussed in various ways ?

In general, I may add the statement that expectations are, in my opinion, products of the human mind which are based on past experience, even though they relate to future moments. The simplest type of expectation at moment t on the value some variable x will have at moment $t + 1$ is that it is assumed equal to the last-known value of that variable, say $x\ (t - 1)$. This type is more frequent, it seems to me, than is often thought. I assumed it to be valid for profit expectations. I do not deny that external events may also influence them. I only think that these external events will be, as a rule, of an unsystematic character, and may thus be part of the unexplained residuals.

Another, somewhat more complicated, type of expectation may be built upon the last-known rate of increase in x, say $x(t - 1) - x(t - 2)$, which is applied to the last-known value of x, viz. $x(t - 1)$, yielding for the expectation of $x(t + 1)$ the value

[1] If more complicated forms of functions are used, the influences will not even be added up, but this goes beyond our discussion here.

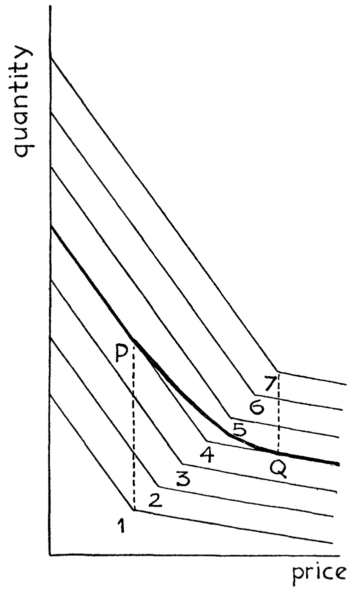
$x(t-1) + 2\{x(t-1) - x(t-2)\}$. The factor 2 is due to the distance of two time units between $t-1$ and $t+1$.

9. One question of great concern to Mr. Keynes is that of the *linearity of the relations assumed.* To begin with, Mr. Keynes says that he has not discovered any example of curvilinear correlation and that I have not told him what kind of evidence would have led me to introduce it. This Mr. Keynes will find in Chapter I, where, when introducing the "partial scatter diagrams," I pointed to their use for discovering whether any correlation is or is not linear. This idea has been applied in the graphs III 9–11 on pp. 81–83. In the first of these graphs a slight curvilinearity has been found for the influence of profits on investment activity, but the deviations from the linear relationships did not seem sufficiently important to render it necessary to recalculate the correlation with *e.g.* quadratic functions. For the rest I may perhaps point to Vol. II, where some more interesting cases of curvilinear relations are discussed.

The assertion that, in connection with curvilinear correlation, "it would certainly seem that quite easy manipulation on these lines would make it possible to fit any explanation to any facts"—an assertion often heard from non-statistical critics—gives a very inadequate picture of the situation. One must not forget that curvilinearity is by no means identical with manipulation *at will* of the coefficients. For each value of the explanatory variable whose influence is assumed to be curvilinear only one coefficient is possible, and reasons of continuity require that these coefficients should not fluctuate too much. My experience is that the possibilities of improving correlations by curvilinearity are very restricted. Especially on this point I want to recommend to any non-convinced reader : try it yourself!

Mr. Keynes seems to be very much opposed to linear relations. He even calls them ridiculous. I think there are strong reasons that reduce their degree of ridiculousness. (1) First it is a well-known mathematical proposition that *almost any function may be approximated, for not too long intervals, by linear functions.* The exceptional functions for which this proposition does not hold need not interest us here at all. Most economists would not even be aware of their existence. (2) The second reason why I think linear relations are not so ridiculous, is that *observation simply teaches us that they occur.* In Vol. II (on p. 12), I give some examples to which I may refer. (3) Apart from these two reasons, *is it not natural to begin any attempt at analysing the economic mechanism by making the simplest assumption compatible with general theory?*

I think this is so common an approach in almost every inductive piece of research work that I cannot follow Mr. Keynes's fear for this instrument of analysis; *the more so, since the partial scatter diagrams prevent us from diverging too much from reality.* (4) In addition to all this, I think there is one theoretical reason why *for great masses of individuals the joint reaction may be much more linear than any individual reaction would be.* If we imagine the individual demand curve to be highly curvilinear, and if we imagine a great number of such demand curves to be added up, then the very fact that the place of maximum curviture will be



GRAPH 2.—Demand curves for 7 individuals (thin curves), assumed to have points of high curvature at different prices and the curve of total demand (divided by 7) (heavy curve) showing a much smoother course between P and Q.

different for most of these individual curves will already lead to a joint curve which is much more linear than any of the individual curves (cf. graph 2).[1]

10. An important further question raised by Mr. Keynes can be given the following form. Investment activity, he says,

[1] In a footnote Mr. Keynes asks whether the quantitative effect of a 5 per cent. interest rate is *five-fourths* of the effect of a 4 per cent. rate, or *double*; the difference between 5 and 3, the base year value, being double the difference between 4 and 3. The answer is that one should not ask what the effect of a 5 per cent. rate is, but what the effect of *a change from* 5 *to* 3 is. This, in fact, is double the effect of a change from 5 to 4, if linear formulae are used, and equals the effect of a change from 4 to 2. Thus stated, the influence of any change is independent of the choice of the base year.

has been explained by profit rates and other factors. *But profit rates themselves have not been explained.* I think Mr. Keynes is right in requiring that profits should also be explained, and I have only to add that precisely this remark has been made in the last chapter of Vol. I, where the problem for Vol. II was given, and that Vol. II is wholly devoted to giving a complete explanation of all relevant phenomena, included in the model of society used for the explanation of the business-cycle phenomena. The important question *how cycles can be explained by a simultaneous system of linear relations* has been dealt with also in the introduction to Vol. II, and I may refer to that, hoping that Mr. Keynes will re-formulate his criticism after having had the opportunity of reading Vol. II.[1]

For the reader of the present discussion I may, however, give one very simple example of how a couple of linear relations may lead to a cyclic movement—viz., *the case of the cobweb problem with straight supply and demand curves.* In numerous theoretical papers I gave other examples, and so did other authors (FRISCH, ROOS, KALECKI, LUNDBERG, CHAIT, and others).

11. Again, Mr. Keynes has great difficulties in finding out how I have *determined the lags* involved in some of the relations. I think this is not so mysterious as Mr. Keynes seems to think it, and especially that there is no contradiction between the way in which lags and the way in which the regression coefficients have been determined. In principle both *have been determined so as to make the correlation the highest possible and by only admitting such values as seemed to have economic sense.*

In both cases *a priori* values, if they could be fixed, have been preferred to "free" values—*i.e.*, values determined by correlation analysis. Where, however, no *a priori* values could be indicated, the method of maximum correlation has been employed. I think this is a logical treatment.

In the case of the "explanation" of general investment activity for post-war United States, a lag of half a year was judged to be a good *a priori* estimate. The lag in question equals the sum of the following time intervals:

(*a*) the interval elapsing between the making of profits and the knowledge that they have been made;

(*b*) the psychological lag between the moment of this

[1] I am coming back to this question in an article to be published in the *Review of Economic Studies* of February 1940. I shall in particular enter into the question "how reversal comes in."

knowledge and the moment of reaction in the form of new investment orders; and

(c) the technical lag between the ordering and delivery of capital goods.

For post-war United States (a) would seem to be negligible, (b) would seem to be short—some months, *e.g.*—and (c) equally some months. A total of half a year seems reasonable.

For the pre-war European cases (a) and (b) will probably have higher values; so will (c), but since here pig-iron production was taken as a measure of investment activity, it is not fully involved in the observed lag. For the pre-war cases the lag has not therefore been chosen on *a priori* grounds.

12. About the *method by which trends are eliminated* Mr. Keynes does not seem to be well informed. In supposing that a trend is drawn by connecting the first and the last year of a series, he is evidently wrong.

A glance at pp. 133 and 134 or at any elementary text-book on these matters could have helped him.

Moreover, Mr. Keynes thinks it rather arbitrary to use nine-year moving averages as trends in pre-war periods and straight lines in post-war years. I am sorry I have not explained this more fully; in statistical circles I think this is hardly any more a matter of dispute. For short periods there is not much difference between a straight trend and a moving average. For long periods there is, and then the moving average is decidedly better: it follows more closely the development of the curves, *e.g.* the long waves. The advantage of straight-line trends is that no observations are lost at the extremes. This is why they have been preferred for the (short) post-war period.

" But, apart from that "—Mr. Keynes continues—" should not the trends of the basic factors (in my terminology: the explanatory variables, J.T.) be allowed to be reflected in a trend of the resulting phenomenon? Why is correction necessary? " The answer is: since there are often a number of explanatory variables that show very smooth and slow changes; for them a trend term is a catch-all. They cause a trend-difference between the observed series and the series calculated from the fluctuating explanatory variables only. The trend term included in the " explanations " does not, therefore, represent the trend line of the variable to be explained, but only the *trend difference* between that variable and the combination of the explanatory variables. And *this difference is far from being so sensitive to changes in period* as the trend of each variable separately, since a change in period will in most cases change the trend in the dependent variable in about the same extent as the trend in the agglomerate of explanatory variables. Therefore the procedure followed here is not, as Mr. Keynes thinks, disastrous.

13. There is one further question of a technical nature which may be considered next. Mr. Keynes asks why correlations have not been made for parts of the periods considered, *i.e.* why the period has not been broken up into sub-periods. But this is precisely what has been done on pp. 70 and 71, 74 and 75 of Vol. I. So I think I need not defend myself against this reproach.

14. The final question I wanted to answer here is the very important one, raised on p. 566, where Mr. Keynes says: "*How far are these curves and equations meant to be no more than a piece of historical curve fitting and description, and how far do they make inductive claims with reference to the future as well as the past?* I have not noticed any passage in which Prof. Tinbergen himself makes any inductive claims whatever. He appears to be solely concerned with statistical description. Yet the ultimate purpose which Mr. Loveday outlines in the preface is surely an inductive one. If the method cannot prove or disprove a qualitative theory and if it cannot give a qualitative guide to the future, is it worth while?"

I am sorry again if I have not been clear enough in this respect, but the intention is the following. *If there is no reason to suppose that the laws that have governed the reactions of individuals and firms in the past will have changed in the near future*, it seems possible to reach conclusions for the near future by measuring as exactly as possible those same reactions in the past.

Of course this is only true if no structural changes take place. *But even if they take place it will, in many cases, be possible to "localise" their influence—i.e.*, to indicate which of the elementary or direct causal relations they affect. All other relations may be assumed to remain unaffected, and the change in the relations affected may even, perhaps, be estimated. As an example, suppose that a tariff is introduced. This will affect the supply function of certain imported commodities. It will, however, not affect the demand function; nor the supply function for, say, money or the relation determining income from prices, production, etc. *Of course it will change the variables involved in all these functions, but not the functions themselves.* The change brought about in the only function affected—viz. the supply function for the commodities concerned—may even be estimated.

In many cases only small changes in structure will occur in the near future. What, in both circumstances, is the purpose of the establishment of our set of relations ? It is, above all, to calculate how the system would move if certain of these relations were changed. Suppose that Government changes its attitude and invests more in dull times, less in boom periods. This amounts to a change in the investment relation—*i.e.*, in the relation telling how investment activity depends on its determining factors. *With this new investment relation instead of the old one, and all other relations unchanged, what will the characteristic movements of the economy be ?* This is the type of question we are able to answer with the help of our schemes. In Vol. II a number of examples are given. It is found *e.g.* that a change in the opportunities for stock exchange speculation would presumably make the movements much more stable. It is also found that a stabilisation of consumption outlay, and in a smaller degree one of investment outlay, would have the same effects. For such questions it does not matter so much whether or not, in the absence of any new policy, small structural changes would have occurred. We are less interested in " forecasting " than in the outcome of " variation problems."

15. Indicating more elementary cases where, also in Mr. Keynes's opinion, the method will be fruitful, he discusses the case of net investments in railway rolling stock, and observes that one should not take " rate of increase in traffic " and " profits " as separate factors, but, instead of the latter, " that part of profits which is not due to the rate of increase in traffic." In addition, he wants to include as explanatory factors (1) the age of the existing rolling stock, (2) the capacity of the existing shops to produce more rolling stock and (3) the state of confidence as to the maintenance of traffic and as to the effect of competition with other forms of transport.

Including, instead of profits, the " part of profits which is not due to the rate of increase in traffic " is not necessary, as long as one is interested only in the joint effect of the rate of increase in traffic and " that part of profits, etc.," as compared with the influence of interest rates and of iron prices. This was the primary object of my study. One might try to go further by decomposing profits and including its " independent " part (in the above sense) as an explanatory series; but this could as well be left to a separate " explanation " of profits, as one of the first causes, by its own " causes " which may be considered as " second causes " to investment (cf. § 6).

The inclusion of (1) above would mean the inclusion of a trend series, as was stated on p. 40 of Vol. I, where the "echo effect" was discussed, of which Mr. Keynes is evidently thinking here. It would not contribute very much to the explanation of the *cyclic* fluctuations. Factor (2) does not belong, in my view, to the factors to be included in a *demand* equation; in my opinion it is a *supply* factor, which will, for the part it plays indirectly in the determination of demand, be reflected in prices. Factor (3) must be measured, in so far as systematic causes are at work, by the rate of increase in traffic, already included, and by some other variables which, especially for pre-war periods (to which this part of the study relates), will show almost entirely a trend development. I am here thinking of such figures as the increase in motor traffic or shipping or the growth of population, industrialisation, etc. *Summa summarum* it seems to me that my way of estimating the influence of the rate of interest on railway investments in rolling stock would not be influenced very much by the supposed omissions. Perhaps here also, however, the best answer might be an invitation to try it out.

16. In conclusion, I want to apologise for not having been clear enough in some of my arguments when writing Vol. I; I hope that this paper fills some gaps. As to the real controversies—apart from a number of evident misunderstandings of Mr. Keynes's on mathematical questions—I must admit that in my view the method under discussion promises—and actually yields—much more than Mr. Keynes thinks. Since the proof of the pudding is in the eating, I hope Mr. Keynes and other critics will give more attention to the economic premises, and especially that competing "explanations" of actual series representing some economic phenomena will be given, in order that the "public" may choose!

J. TINBERGEN.

Rotterdam School of Economics.

COMMENT

PROFESSOR TINBERGEN'S very valuable reply does not require any extensive comment from me. The arguments on both sides are fairly before the reader. But I may add footnotes on one or two points :—

(i) In § 4 Professor Tinbergen's example is not well chosen. He will find the explanation of capital gains in U.S.A. as an influence on consumption set forth quite explicitly in my "General Theory of Employment," p. 319 (also more generally p. 93).