



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

J. M. Keynes

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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).

(ii) In § 5 Professor Tinbergen finds room for outside explanations in the "residual." It follows that, in certain cases, the *larger* the residual, the more accurate the analysis will be. The more important the outside explanations are, the larger the residual ought to be. But does he not, in general, judge the accuracy of his analysis by the *smallness* of his residual?

(iii) In § 7 is it for the economist to tell him beforehand whether or not it is the difference between profit rates and interest rates that matters? Or is it for him to tell the economist afterwards? In § 3 Professor Tinbergen agrees that it is for the economist to tell him. Here he seems to reverse the rôle. I notice throughout some uncertainty as to who, the economist or the statistician, is in the saddle and who the patient ass.

(iv) In § 8 I understand well enough that his method can deal by time-lags with expectations of the type that the future will resemble the very recent past. How does he deal with expectations of change?

(v) In § 9 there is an important misunderstanding. I did not say that linear relations are ridiculous. What I said was (p. 564) that "it is a very drastic and usually improbable postulate to suppose that all economic forces are of this character . . . indeed it is ridiculous." Professor Tinbergen's footnote on the influence of changes in the rate of interest illustrates the point. If the influence of changes is linear, it follows that the influence of the absolute rate is not linear.

(vi) I am afraid it may be true that if I moved in statistical circles (§ 12) I should find trend terms a terribly convenient "catch-all." I do not like the ring of this passage. A trend term, as described, seems to get very near to being a method for correcting imperfect results and obscuring the fact that the explanation given is in fact a wrong one. I should like to hear a great deal more of the precise part played, both theoretically and practically, by "trend terms" and "residuals."

(vii) I do not follow the exclusion of supply factors in § 12. For it is the actual amount of investment that we are studying.

Professor Tinbergen appeals to me several times to cook (or, should it be, eat?) more pudding myself before declaring it indigestible. I would ask in return for an experiment on his part. It will be remembered that the seventy translators of the Septuagint were shut up in seventy separate rooms with the Hebrew text and brought out with them, when they emerged, seventy identical translations. Would the same miracle be vouchsafed if seventy multiple correlators were shut up with the same statistical

material? And anyhow, I suppose, if each had a different economist perched on his *a priori*, that would make a difference to the outcome.

No one could be more frank, more painstaking, more free from subjective bias or *parti pris* than Professor Tinbergen. There is no one, therefore, so far as human qualities go, whom it would be safer to trust with black magic. That there is anyone I would trust with it at the present stage or that this brand of statistical alchemy is ripe to become a branch of science, I am not yet persuaded. But Newton, Boyle and Locke all played with alchemy. So let him continue. J. M. KEYNES

ECONOMIC WELFARE: A COMMENT

IN his notes on "Economic Welfare," Professor L. G. Melville attempts to show that the economist need not *assume* "some sort of postulate of equality" when he formulates policies for the redistribution of income aimed at increasing economic welfare.¹ Professor Melville would prove the existence of equal capacity to enjoy expenditure among "various groups of people, some of whom are richer and some poorer."

Professor Melville reasons that within any two groups, randomly selected from the total population, the same number of people of any particular capacity to enjoy expenditure would be found. He maintains that this equality of capacity would hold for two different income-groups because, "for all practical purposes, groups selected by differences in income are selected at random as far as this characteristic is concerned."²

But this is not acceptable. The criterion of classification is one of the determinants of the variable, and random groupings are not to be made in this way. If an analogy can be pardoned, the procedure is as unfruitful as would be the attempt to secure random groups of men's weights by selecting the groups according to differences of height. One's income may condition one's capacity to enjoy expenditure. If groups are formed according to differences in income, there is none of the randomness necessary to place within each group the same number of persons with any particular capacity to enjoy expenditure.³

¹ ECONOMIC JOURNAL, September 1939, pp. 552-3.

² *Loc. cit.*, p. 553.

³ Professor Melville might seek to answer by repeating, "It is true that as an individual grows richer, experience and education may improve his innate capacity for enjoyment, but observation and introspection allow us to assume that this improvement is not sufficient to prevent the marginal utility of money from falling" (p. 553). Surely, however, this cannot raise the equality of capacity for satisfaction to the rank of the scientifically demonstrable.