quarter of 1932 to the third quarter of 1937 it is 2·1 per cent., and from the fourth quarter of 1937 to the third quarter of 1938 it is 6·2 per cent., with a maximum deviation in this last period of $8\frac{1}{4}$ per cent. in the second quarter of 1938. From 1929 to 1933 the *Economist* quarterly curve lags some 10 per cent. behind the annual curve, with a deviation in the middle of 1931 of 16 per cent. The series of jumps in which the quarterly curve rises from the very bumpy bottom of 1932 is hardly likely to represent any real profit experience.

To the objection that the graphical quarterly index gives "assumed" and not "reported" profits, the answer is that until all companies end their financial years at the same time and publish quarterly profits, any index must be calculated; and it is suggested that the method described, which seeks to establish the current profits of all companies in each quarter, gives the most accurate results within the limits of the available data. In other words, the curve of quarterly earnings is, as nearly as may be established, a true picture of the changing profitability of the aggregate of the companies considered.

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London.

OFFICIAL PAPERS

The League of Nations

Professor Tinbergen's Method

A Method and its Application to Investment Activity. By J. Tinbergen. (Statistical Testing of Business-Cycle Theories I.) (Geneva: League of Nations, 1939. Pp. 169. 3*s.* 6*d.*)

In the preface to this volume Mr. Loveday explains that it is to be regarded as the first instalment of the second stage of the investigation of the League of Nations' inquiry into the Business Cycle, of which Prof. von Haberler's *Prosperity and Depression* was the first stage. The ultimate object is to apply statistical tests to the alternative theories of the Business Cycle catalogued by Prof. von Haberler. But this instalment is limited to an explanation of the statistical method which it is proposed to employ, followed by three examples. In the first chapter Prof. Tinbergen deals with some of the logical issues involved; in the second chapter he explains in general terms the method of multiple correlation analysis; and in the next three chapters he applies this method to three selected examples—namely, Fluctuations in Investment, Residential Building, and Investment in Railway Rolling-stock.

The second chapter, which gives in brief compass a most lucid account of the statistical method to be employed, is very good indeed. But the first chapter, which should deal with the difficult logical problems involved in applying to economic data methods which have been worked out in connection with material of a very different character, is grieviously disappointing. So far as it goes, it is helpful; but it occupies only four pages, and it leaves unanswered many questions which the economist is bound to ask before he can feel comfortable as to the conditions which the economic material has to satisfy, if the proposed method is to be properly applicable. Since Mr. Loveday invites criticisms and suggestions as to future procedure, I would urge that the next instalment should be primarily devoted to the logical problem, explaining fully and carefully the conditions which the economic material must satisfy if the application of this method to it is to be fruitful.

Prof. Tinbergen is obviously anxious not to claim too much. If only he is allowed to carry on, he is quite ready and happy at the end of it to go a long way towards admitting, with an engaging modesty, that the results probably have no value. The worst of him is that he is much more interested in getting on with the job than in spending time in deciding whether the job is worth getting on with. He so clearly prefers the mazes of arithmetic to the mazes of logic, that I must ask him to forgive the criticisms of one whose tastes in statistical theory have been, beginning many years ago, the other way round. Let me catalogue the questions to which I should like to get an answer in the next instalment.

(1) Prof. Tinbergen begins with a very important and necessary admission. " The part which the statistician can play in this process of analysis," he explains on p. 12, " must not be misunderstood. The theories which he submits to examination are handed over to him by the economist, and with the economist the responsibility for them must remain; for no statistical test can prove a theory to be correct." Can the statistical test, nevertheless, prove a theory to be *incorrect*? Here also Prof. Tinbergen qualifies his claims, but he does go so far as to say: " It can, indeed, prove that theory to be incorrect, or at least incomplete, by showing that it does not cover a particular set

of facts." But is not this going too far? At best, only those theories can be shown to be incorrect which, in the view of the economist who advances them, accept as applicable the various conditions which will be set forth below.

At any rate, Prof. Tinbergen agrees that the main purpose of his method is to discover, in cases where the economist has correctly analysed beforehand the qualitative character of the causal relations, with what strength each of them operates. If we already know what the causes are, then (provided all the other conditions given below are satisfied) Prof. Tinbergen, given the statistical facts, claims to be able to attribute to the causes their proper quantitative importance. If (anticipating the conditions which follow) we know beforehand that business cycles depend partly on the present rate of interest and partly on the birth-rate twenty years ago, and that these are independent factors in linear correlation with the result, he can discover their relative importance. As regards disproving such a theory, he cannot show that they are not *veræ causæ*, and the most he may be able to show is that, if they 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 (perhaps because non-statistical factors are relevant).

Am I right in thinking that the method of multiple correlation analysis essentially depends on the economist having furnished, not merely a list of the significant causes, which is correct so far as it goes, but a *complete* list? For example, suppose three factors are taken into account, it is not enough that these should be in fact *veræ causæ*; there must be no other significant factor. If there is a further factor, not taken account of, then the method is not able to discover the relative quantitative importance of the first three. If so, this means that the method is only applicable where the economist is able to provide beforehand a correct and indubitably complete analysis of the significant factors. The method is one neither of discovery nor of criticism. It is a means of giving quantitative precision to what, in qualitative terms, we know already as the result of a complete theoretical analysis—provided always that it is a case where the other considerations to be given below are satisfied.

(2) The next condition is that all the significant factors are measurable (and, presumably it should be added, that we have adequate statistical knowledge of their measure). Prof. Tinbergen states this condition with emphasis, but he does so in

terms which do not satisfy me without further explanation. He writes (p. 11) :—

> " The inquiry is, by its nature, restricted to the examination of measurable phenomena. Non-measurable phenomena may, of course, at times exercise an important influence on the course of events ; and the results of the present analysis must be supplemented by such information about the extent of that influence as can be obtained from other sources."

He suggests here that the method can be usefully applied if *some* of the factors are measurable, the results obtained from examining these factors being " supplemented " by other information. But how can this be done ? He does not tell us. His method of calculating the relative importance of these measurable factors essentially depends on the assumption that between them they are comprehensive. He gives them such regression coefficients that they completely explain the phenomenon under examination. How can they be " supplemented " by other information ?

If it is necessary that *all* the significant factors should be measurable, this is very important. For it withdraws from the operation of the method all those economic problems where political, social and psychological factors, including such things as government policy, the progress of invention and the state of expectation, may be significant. In particular, it is inapplicable to the problem of the Business Cycle.

(3) Must we push our preliminary analysis to the point at which we are confident that the different factors are substantially independent of one another ? This is not discussed. Yet I think it is important. For, if we are using factors which are not wholly independent, we lay ourselves open to the extraordinarily difficult and deceptive complications of " spurious " correlation.

Moreover, Prof. Tinbergen is concerned with " sequence analysis " ; he is dealing with non-simultaneous events and time-lags. What happens if the phenomenon under investigation itself reacts on the factors by which we are explaining it ? For example, when he investigates the fluctuations of investment, Prof. Tinbergen makes them depend on the fluctuations of profit. But what happens if the fluctuations of profit partly depend (as, indeed, they clearly do) on the fluctuations of investment ? Prof. Tinbergen mentions the difficulty in a general way in a footnote to p. 17, where he says, without further discussion, that " one has to be careful." But is he ? What precautions does he take? On p. 39, in a passage which I do not fully understand, he suggests that the difficulty can be overcome by some adjustment of time-lag. It is not so easy for the reader to see his way through a logical problem of this kind, that it can be safely left without a clear and comprehensive discussion.

In practice Prof. Tinbergen seems to be entirely indifferent whether or not his basic factors are independent of one another. For example, when he examines the fluctuations of investment, his basic factors are: (1) profits earned, (2) the price of capital goods, (3) interest rates, (4) profit margins, (5) index of production of consumers' goods, (6) rate of increase in the general price-level. I infer that he considers independence of no importance. But my mind goes back to the days when Mr. Yule sprang a mine under the contraptions of optimistic statisticians by his discovery of spurious correlation. In plain terms, it is evident that if what is really the same factor is appearing in several places under various disguises, a free choice of regression coefficients can lead to strange results. It becomes like those puzzles for children where you write down your age, multiply, add this and that, subtract something else, and eventually end up with the number of the Beast in Revelation.

The mention of the above example suggests another point worth enlarging on for the sake of illustration. It will be observed that Prof. Tinbergen includes profits earned and the rate of interest as amongst the factors influencing investment. But, as Prof. Tinbergen himself points out (p. 66), some economists would argue that it is the *difference* between these two factors which matters, rather than their absolute amounts. How does that affect matters? Moreover, they would mean the difference between profits measured as a percentage on current *cost* of capital-goods and the rate of interest. Now, Prof. Tinbergen does not seem to care in what unit he measures profit. For the pre-war United States it is the share price index, for the pre-war United Kingdom non-labour income, for pre-war Germany dividends earned as a percentage of capital, for the post-war United States the net income of corporations, and for the post-war United Kingdom net profits earned as a percentage of capital. Thus it is sometimes a rate and sometimes an absolute quantity; and when in the final outcome he multiplies this hotch-potch, sometimes by a large coefficient and sometimes by a small one, and then subtracts from it the rate of interest multiplied (usually) by a small coefficient, I do not know whether there is room here for the theory that investment may be governed by the difference between the rate of profit [1] on cost and the rate of interest on loans, or whether we have merely reached the number of the Beast. Prof. Tinbergen is by no means unaware of what a difference the way he measures profit can make. He gaily points out (p. 57) as a matter of some interest, but not of any concern, that the series which he takes to represent profits in Germany leads to a regression coefficient for that factor twice as great as the series he takes for the United States, and the series he takes for Great Britain to a coefficient nearly four times as great. (This is an extraordinary example of the candid way in which, if only he is allowed to get on with all this arithmetic unhindered, he is ready to admit at the end of it what must seem to the reader to be devastating inconsistencies.) 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.

(4) Prof. Tinbergen explains (p. 25) that, generally speaking, he assumes that the correlations under investigation are *linear* :—

> "As a rule, curvilinear relations are considered in the following studies only in so far as strong evidence exists. A rough way of introducing the most important features of curvilinear relations is to use changing coefficients—for instance, one system of coefficients for the description of situations not far above normal and another for the description of extremely high levels. This amounts to approximating a curve by means of two straight lines. Another way of introducing curvilinear relations is to take squares of variates, or still other functions, among the 'explanatory series.'"

I have not discovered any example of curvilinear correlation in this book, and he does not tell us what kind of evidence would lead him to introduce it. If, as he suggests above, he were in such cases to use the method of changing his linear coefficients from time to time, it would certainly seem that quite easy manipulation on these lines would make it possible to fit any explanation to any facts. Am I right in thinking that the *uniqueness* of his results depends on his knowing beforehand that the correlation curve must be a particular kind of function, whether linear or some other kind ?

Apart from this, one would have liked to be told emphatically what is involved in the assumption of linearity. It means that

[1] I should have liked to have said "the *expected* rate of profit." But there is no room for expectations, so far as I can discover, in the theory of investment with which the economists have supplied Prof. Tinbergen.

the quantitative effect of any causal factor on the phenomenon under investigation is directly proportional to the factor's own magnitude. In a parenthesis, on page 26, to which the reader is not likely to attach much importance, Prof. Tinbergen does, indeed, mention this in passing. But it is a very drastic and usually improbable postulate to suppose that all economic forces are of this character, producing independent changes in the phenomenon under investigation which are directly proportional to the changes in themselves; indeed, it is ridiculous. Yet this is what Prof. Tinbergen is throughout assuming. For instance, in his example of fluctuations in investment, the assumption of linearity means that if the increase in profits is twice greater in one year than in another, then its influence on the quantity of investment will also be exactly twice as great; and similarly, that the effect on investment of a change in the rate of interest will always be directly proportional to the amount of that change. And if such an unlikely assumption is to be made, one must clearly be very careful in choosing one's method of measurement and also one's base; [1] especially when one bears it in mind that Prof. Tinbergen's measures are nearly always *indirect*. That is to say, they are not direct measures of the factor itself, but are index numbers of some associated phenomenon. Thus even if the factors themselves produce a directly proportional effect, this is not likely to be true of the indirect indices which are employed.

Is there any ground for the suspicion that the assumption of linearity rules out cyclical factors? And what is the position of (*e.g.*) the Acceleration Principle, according to which the propensity to save is a function of the *absolute* level of activity and the inducement to invest is a function of the *changing* level of activity? Prof. Tinbergen explains fluctuations in investment mainly by fluctuations in profits; so that if profits fluctuate cyclically, investment will also. But he does not attempt to explain fluctuations in profits. Suppose that linear correlation is also assumed in the case of all the factors on which profits depend, and so on down to the final analysis? Is it possible that

[1] For example, let us suppose that the rate of interest having been 3 per cent. in the base year rises to 4 per cent. and then to 5 per cent. Is the quantitative effect of 5 per cent. *five-fourths* of the effect of 4 per cent, *i.e.* proportional to the excess above 0? Or is it *double*, the difference between 5 and 3 being double the difference between 4 and 3, *i.e.* proportional to the excess above 3? I rather think that Prof. Tinbergen means the latter. But in this case, if he had happened to pitch on a base year when the rate was $3\frac{1}{2}$ per cent., the effect of a rise from 4 to 5 per cent. would be *treble* instead of double, *i.e.* proportional to the excess above $3\frac{1}{2}$. The reader needs some guidance on such a matter.

there could be a cyclical fluctuation in a system, all the ultimate independent determinants of which had fixed regression coefficients and were in linear correlation with their consequences, except in the case where one of the ultimate determinants is itself a periodic function of time (*e.g.* sun-spots)? Where and how does the element of *reversal* come in? I ask this question without pretending to answer it. But I should like to know the answer. For if it is in the negative, Prof. Tinbergen is engaged on the task of explaining business cycles by a method one of the working postulates of which is that cycles can only be explained by other cycles.

(5) The treatment of time-lags and trends deserves much fuller discussion if the reader is to understand clearly what it involves. To the best of my understanding, Prof. Tinbergen is not presented with his time-lags, as he is with his qualitative analysis, by his economist friends, but invents them for himself. This he seems to do by some sort of trial-and-error method. That is to say, he fidgets about until he finds a time-lag which does not fit in too badly with the theory he is testing and with the general presuppositions of his method. No example is given of the process of determining time-lags which appear, when they come, ready-made (cf. p. 48). But there is another passage (p. 39) where Prof. Tinbergen seems to agree that time-lags must be given *a priori*.

The introduction of a trend factor is even more tricky and even less discussed. This element is not obtained by reference to secular changes in the scale of the economy as a whole, but is strictly related to the factors under discussion. In the case of fluctuations in investment, " trends," Prof. Tinbergen explains (p. 47), " have been calculated as nine-year moving averages for pre-war periods—which are long enough to allow of the first and last four years being omitted—and as rectilinear trends for post-war periods—which are too short to allow of omitting eight years." This seems rather arbitrary. But, apart from that, should not the trends of the basic factors be allowed to be reflected in a trend of the resulting phenomenon? Why is correction necessary? I have probably misunderstood the argument, since this is not the sort of mistake to which Prof. Tinbergen is liable.

Although there may be many factors with different trends, there is only one trend line, and I have not understood the process by which this single trend is evolved. The use of rectilinear trend (in post-war years) means, apparently, that a straight line is drawn between the first year of the series and the last. The result is, of course, that it makes a huge difference at what date you stop. In the case of the United States (p. 56) the series runs from 1919 to 1933, which, as a result of the abnormal circumstances of the first and last years, involves the paradox that the United States was in a severe downward trend throughout the whole period, including the period ending in 1929, amounting in all to 20 per cent.; whereas if Prof. Tinbergen had stopped in 1929, he would have used a sharply rising trend line instead of a sharply falling one *for the same years.* This looks to be a disastrous procedure. Prof. Tinbergen is quite aware of the point. In a footnote to p. 47 he mentions that "the trend chosen for the American figures (post-war period) may be somewhat biased by the fact that the period starts with a boom year and ends with a slump year." But he is not disturbed, since he has persuaded himself, if I follow him correctly, that it does not really make any difference what trend line you take.

(6) I pass in conclusion to a different department of the argument. 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 quantitative guide to the future, is it worth while? For, assuredly, it is not a very lucid way of describing the past.

Thirty years ago I used to be occupied in examining the slippery problem of passing from statistical description to inductive generalisation in the case of simple correlation; and to-day in the era of multiple correlation I do not find that in this respect practice is much improved. In case Mr. Loveday or others may nurse inductive hopes, it is worth pointing out that Prof. Tinbergen makes the least possible preparation for the inductive transition.

Put broadly, the most important condition is that the environment in all relevant respects, other than the fluctuations in those factors of which we take particular account, should be uniform and homogeneous over a period of time. We cannot be sure that such conditions will persist in the future, even if we find them in the past. But if we find them in the past, we have at any rate some basis for an inductive argument. The first step, therefore, is to break up the period under examination into a series of sub-periods, with a view to discovering whether the results of applying our method to the various sub-periods taken separately are reasonably uniform. If they are, then we have some ground for projecting our results into the future.

Now, this is what Prof. Tinbergen never attempts. It is true that his series are broken up into post-war and pre-war periods, but this seems to be done, not on purpose, but as a result of the exigencies of the available statistics. For his pre-war investigations he takes a period of about forty years and makes no attempt to break it up into sub-periods. If he had done so, would his regression coefficients, calculated for each decade taken separately, differ somewhat widely from those calculated as the best fit for the whole period? This is worth examination. For the main *prima facie* objection to the application of the method of multiple correlation to complex economic problems lies in the apparent lack of any adequate degree of uniformity in the environment.

Inductive difficulties arise not only from the lack of uniformity in the factors of which no specific account is taken. It arises also in the case of those which are included in the scheme. For, owing to the wide margin of error, only those factors which have in fact shown wide fluctuations come into the picture in a reliable way. If a factor, the fluctuations of which are potentially important, has in fact varied very little, there may be no clue to what its influence would be if it were to change more sharply. There is a passage in which Prof. Tinbergen points out (p. 65), after arriving at a very small regression coefficient for the rate of interest as an influence on investment, that this may be explained by the fact that during the period in question the rate of interest varied very little.

These many doubts are superimposed on the frightful inadequacy of most of the statistics employed, a difficulty so obvious and so inevitable that it is scarcely worth while to dwell on it. Taking everything into account, the successful application of this method to so enormously complex a problem as the Business Cycle does strike me as a singularly unpromising project in the present state of our knowledge.

This does not mean that economic material may not supply more elementary cases where the method will be fruitful. Take, for instance, Prof. Tinbergen's third example—namely, the influence on net investment in railway rolling-stock of the rate of increase in traffic, the rate of profit earned by the railways, the price of pig iron and the rate of interest. Here there seems a reasonable *prima facie* case for expecting that some of the necessary conditions are satisfied. But even in this case a formulation rather different from Professor Tinbergen's might be required. It is evident, without any particular inquiry, that the demand for new rolling stock will mainly depend on the growth of traffic. Moreover, profit is not independent of traffic, but is largely the growth of traffic over again. To get a separate factor it is necessary to segregate that part of profit which is due to growth of traffic from that part which is due to better freight rates relatively to wages and other costs. What we want to know is not the obvious point that the demand for rolling stock is considerably affected by the growth of traffic, but how far this dominates the situation as compared with more subtle factors such as (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.

I hope that I have not done injustice to a brave pioneer effort. The labour it involved must have been enormous. The book is full of intelligence, ingenuity and candour; and I leave it with sentiments of respect for the author. But it has been a nightmare to live with, and I fancy that other readers will find the same. I have a feeling that Prof. Tinbergen may agree with much of my comment, but that his reaction will be to engage another ten computors and drown his sorrows in arithmetic. It is a strange reflection that this book looks likely, as far as 1939 is concerned, to be the principal activity and *raison d'être* of the League of Nations.

J. M. KEYNES

THE PROCESS OF CAPITAL FORMATION

Statistics relating to Capital Formation. A Note on Methods by the Committee[1] of Statistical Experts. (Studies and Reports on Statistical Methods No. 4.) League of Nations : Geneva, 1938. Pp. 22. 1*s*.

THIS brief study contains a modest and tentative approach to a subject of great importance and considerable difficulty. Its immediate object is to provide the methodological analysis which must be the preliminary to the collection of significant and consistent statistics of savings and investment; and its ultimate object is to furnish the basis for the collection of such statistics in every country on a uniform basis and with an agreed use of terms which will allow international comparisons.

The Committee are, clearly, feeling their way, and have not themselves settled down as yet to any rigid formulation or consistent use of a scheme or set of terms. This is probably wise in a preliminary study. But it makes rather obscure reading. Definitions of fundamental terms are scattered through the report, often in footnotes. One has the impression of reading a text, the first draft of which was not free from inconsistencies and logical errors, which, when they were detected by one or another member of the Committee, were corrected, not by radical re-drafting, but by the insertion of a footnote or a parenthesis.[2] The reader's first impression is one of considerable haze and doubtful logic. But a closer reading shows that this does the Committee's work an injustice. In the ultimate outcome they have been successful in avoiding the logical errors with which this subject is beset, subject to the one important criticism which will be made below.

[1] "This Sub-Committee was composed of the following members of the main Committee : Sir Alfred Flux, formerly Chief of the Statistical Service of the Board of Trade, London; E. Cohn, Director of the Statistical Department, Denmark; Dr. O. Morgenstern, formerly Director of the 'Osterreichisches Institut für Konjunkturforschung', Vienna; and the following outside experts : Dr. E. Ackermann, Head of the Statistical Office of the Swiss National Bank; Prof. H. Clay, Economic Adviser to the Bank of England; J. Denuc, of the National Economic Department, Paris; Dr. E. Lindahl, professor of the University of Lund, Sweden; F. Ravizza, Director of the International Thrift Institute, Milan; W. W. Riefler, of the Institute of Advanced Study, Princeton, New Jersey; J. J. Vincent, Director of the Economic Intelligence Service, National Bank of Belgium. The presence in Geneva of Mr. D. Robertson, Reader in Economics at Cambridge University, enabled the Sub-Committee to avail itself of his assistance at its first meeting."

[2] *E.g.*, the treatment of "capital gains," which seems all wrong in the diagram on p. 9, and ambiguous on p. 12, until the position is finally saved by a correct qualification given in a footnote to p. 16, from which it is clear that this item should never have appeared as a separate category on p. 9.

Their main contribution to the methodology of the subject is concerned with what they call "the process of capital formation." "Capital formation" is defined (in a footnote to p. 6) as follows :—

> "Throughout this report the term 'capital formation' is intended to cover the whole process from the constitution of funds by savings, etc., to the acquisition of capital goods, whether the funds are used to provide additions to invested capital or for maintenance and replacement of old capital. The term 'capital goods' is intended to cover :
>
> (1) Capital equipment for agriculture, industry, commerce, transport, including buildings and works of construction, etc. (referred to as 'Producers' equipment');
>
> (2) Producers' materials;
>
> (3) Durable equipment for consumers, including houses for private occupation (referred to as 'Consumers' capital goods')."

Thus the "process" of capital formation leads up to a final stage, which is concerned with what is sometimes called "gross investment," including maintenance and replacement. But this is preceded, according to the Committee, by two previous stages. The first consists in the setting aside of savings out of current income; the second stage in streams of "funds" becoming "available for investment"; and the third stage in the actual outlay of money for the acquisition of capital goods. The Committee envisage this complete process of the "formation" of a given capital good as taking place over a period of time subject to time-lags of undetermined length. Members of the public refrain from spending on consumption some part of their current income; subsequently, let us say three months later, these savings form one ingredient (the others will be mentioned in a moment) of the "funds available for investment" which are thereupon transferred to the entrepreneur who will be responsible for employing them; and finally, after another three months let us say, these funds are disbursed by the entrepreneur for the purpose of acquiring a capital good. The process of the "formation" of the capital good is then complete. The Committee point out that at any given moment of time the funds which are being "saved" out of income, the funds which are becoming "available for investment," and the funds which are being actually devoted to the "acquisition of a new capital-good" relate, not to the *same*, but to *different* "processes of capital formation." They regard current savings as contributing, not to the funds required by current investment, but to the amount of "funds becoming available for investment " a certain number of months later, which again supply the funds actually expended on the creation of new capital goods some months later than that.

Before we can assess the value of this analysis, some further explanations must be given. We have seen that the final stage of capital formation is concerned with *gross* investment and includes expenditure on the maintenance and replacement of old capital goods. Savings, however, are defined *net* (it would be clearer if this were made a little more explicit at the outset) and do not include sums set aside by entrepreneurs to meet depreciation and current repairs. This discrepancy is made good when we come to the second stage, namely of " funds available for investment," which will be (generally speaking) larger than the savings of the preceding period since they will be augmented by streams from other sources. These additional streams, which are added to the stream of funds arising out of the net savings of the preceding period to make up the total flow of funds available for gross investment, consist mainly of the provisions set aside for maintenance and replacement,[1] of dishoarding and of credit expansion. (We need not complicate the argument with other items which the Committee rightly bring in for the sake of completeness, such as capital import, loans for consumption and their repayment,[2] and public loans for purposes other than investment.)

Now, up to a point this is an interesting and instructive way of analysing the course of the circulation of money, to which, subject to what follows, I see no logical objection. But there is a further corollary to their use of terms which the Committee might have added. They are concerned with the amount of saving set aside out of current income at a date appreciably prior to that of the current investment which they have in view; and they point out, quite correctly, that there is no reason to expect equality between such saving and such investment (after correcting the latter for the fact that it is gross and not net). But they do not point out that it follows no less clearly from the definitions which they have adopted that the amount of saving which is taking place *at the same time* as the investment must be exactly equal to it (both being reckoned net).

This corollary is not merely a neat truism. For unless it is

[1] It is not clear whether these, like savings, are subject to a time-lag before becoming available for investment, or whether they are reckoned as becoming available simultaneously with being set aside.

[2] Instalment purchases of consumption goods are not deducted by the Committee in arriving at net saving, though this is not stated explicitly. I infer, however, that business losses are so deducted, though this also is not stated explicitly.

kept in mind, the reader is very likely to be led to false conclusions. For example, he might naturally suppose—for anything the Committee say to the contrary—that the right way to prepare for an increase of investment is to save more at an appropriately prior date. But the corollary shows that this is impossible. Saving at the prior date cannot be greater than the investment at that date. Increased investment will always be accompanied by increased saving, but it can never be preceded by it. Dishoarding and credit expansion provides not an *alternative* to increased saving, but a necessary preparation for it. It is the parent, not the twin, of increased saving.

It also leads up to the fundamental criticism to which the Committee's schematism seems to me to be open from the statistical side. According to the Committee funds for investment can only become available either from prior saving or from dishoarding and credit expansion. Does not this suggest to the reader that something must be wrong? By taking account of dishoarding and credit expansion, the Committee's scheme allows for additional investment as the result of an increase in output. But it excludes altogether an increase in investment arising in the old-fashioned way as a result of producing more capital goods and less consumption goods, total output remaining the same. Where do the funds for increased investment come from when this happens? Their scheme suggests that an increase of investment beyond the savings (and investment) of a previous period requires dishoarding or credit expansion to supply the necessary funds. But why should this be necessary if the total output is unchanged? Moreover the Committee's scheme assumes that the *whole* of an increase in output will be devoted to the output of capital goods, which (unless there is a change in the propensity to consume) must result in unbalanced production and an inflationary rise in the price of consumption goods. For if there is an appropiate increase in the output of consumption goods *pari passu* with the increase in the output of capital goods, this will use up some of the funds which the Committee have earmarked for investment.

The Committee have overlooked the fact that *spending* releases funds just as much as saving does, and that these funds when released can then be used indifferently for the production either of capital goods or of consumption goods. And they have also overlooked the fact that the production of consumption goods requires the prior provision of funds just as much as does the production of capital goods. The diagram on p. 9 would do just as well if at the top "spending" was substituted for "saving." Prior saving has no more tendency to release funds available for subsequent investment than prior spending has. It is not an increase of investment as such which requires an immediate increase in " available funds," but an increase of output whether for investment or for consumption, or more strictly an increase in the turnover of transactions for any purpose whatever. If there were to be an increase in investment, without there being any change in total output, there would be no need either for prior saving or for an increase in dishoarding or credit expansion. Money which is spent on prior consumption flows into the same pool of available funds as money which is saved, and is available to finance at the next stage the acquisition either of capital goods or of consumption goods. In the former case, the liquid funds for the subsequent acquisition of a capital good are, in effect, provided *beforehand* by the subsequent saving and reduced spending which is impending. Thus the Committee's list of sources of funds potentially available for investment is incomplete. As soon as it is understood that the available funds arise from the *whole* of the money income earned at a previous date, whether saved or spent, supplemented by dishoarding and credit expansion, and are then employed for the *whole* of production (or other monetary transactions) at the subsequent date whether for investment or for consumption, their schematism breaks down completely in so far as it purports to relate the funds arising from savings at a previous date to the funds required for investment at a subsequent date.

In my *General Theory of Employment, Interest and Money* I was seriously at fault in omitting any discussion of what the Committee call " the process of Capital Formation." Under the spur of criticism I have since endeavoured to remedy this omission in an article published in this JOURNAL (December 1937, pp. 663–9). I there introduced a conception serving the same purpose as, but not identical with, that of " funds available for investment " under the name of " finance " which still seems to me to be a convenient term to use. For it covers equally the use of the revolving pool of funds to finance the production of capital goods or the production of consumption goods or (*e.g.*) an increased turnover on the Stock Exchange. In the same way the conception of the rate of interest as being determined by liquidity preference emphasises the fact that *all* demands for liquid funds compete on an equal basis for the available supply; whereas the conception of a *separate* pool of " funds available for investment " suggests that the rate of interest is determined by the interaction of investment demand with a segregated supply of funds earmarked for that special purpose irrespective of other demands and other releases of funds. It may also help to clear up misunderstanding to point out that whilst saving takes place concurrently with investment (in the sense of the first acquisition of a capital good by a entrepreneur), the flow of funds (*i.e.*, of money) available for investment (in the sense of the first acquisition of this capital good by a permanent holder) takes place subsequently; the bridging of this time-lag by "finance" (*i.e.*, by the supply of money) being the function of the credit system (which is solely concerned with finance and never with saving).

I suggest, therefore, that the Committee might begin by limiting their inquiry to the final stage, namely, to the amount of funds which are being devoted to the acquisition of capital goods. I do not deny the great interest and importance of tracing in detail where the ultimate demand for the permanent holding of these capital goods comes from (we know already that it is there in the aggregate). But when investment is increased by more than consumption is falling, the funds for taking up the increased investment permanently *must* "become available" *subsequently* and not prior to the production of the new capital goods—unless, indeed, hoarders or the credit system become permanent investors: and at any rate it is no good looking for them in the fruits of prior saving. The rate of prior saving only tells us how much of the current investment can find a permanent home beforehand without upsetting the liquidity position and the long-term rate of interest, and without time-lag. Subject to these conditions, the *increment* of current investment over prior investment (or saving) can only be cared for *permanently* out of the increment of *current* saving; and the period during which current savings are kept liquid by their owners must be bridged by an increase in the revolving fund of "finance," *i.e.*, of liquid funds provided by the banking system or by dehoarding. It is the rôle of the credit system to provide the liquid funds which are required first of all by the entrepreneur during the period before his actual expenditure, and then by the recipients of this expenditure during the period before they have decided how to employ it. We have been all of us brought up, like the members of this Committee, in deep confusion of mind between the demand and supply of money and the demand and supply of savings; and until we rid ourselves of it, we cannot think correctly.

J. M. KEYNES

The League of Nations Monetary Review[1]

The political misfortunes of the League of Nations have not affected its usefulness as a centre of comparative economic studies. The latest review of monetary and banking developments has the same qualities of balance and comprehensiveness as its predecessors. The annotated statistics of national monetary and banking systems, brought in most cases to the end of 1938 and covering forty-four countries, are relegated to the second volume. The first volume can thus be devoted to an analytical survey of developments. It contains four studies, the first two reviewing exchange developments and credit policy in the chief countries; the other two aiming at a less topical treatment by eliciting from published statistics the changes in banking structure since 1913 and the decline in the importance of the Bill of Exchange. A word or two may be said on each of these.

In the field of foreign exchange the chief feature of the year was the devaluation of the franc in May, which, after the usual temporary reflux of money, appeared to have failed, until the budgetary reforms of November had a more lasting effect. Sterling was vulnerable at the beginning of the year because the world was long of sterling. Political fears produced a sharp fall in March, and, in spite of a temporary recovery in June, due to rumours of an American intention to devalue, a steady, and after September rapid, fall was arrested only at the end of the year. The review (p. 12) defines the Sterling Area by two criteria—maintenance of a fixed rate on sterling and (a consequence of the first) the keeping of monetary reserves in London. Should not these be reversed? The Sterling Area consists of those countries which have so much to pay in sterling that they keep their surplus balances in London; there is room for only three or four centres in the world in which an economical use of such balances is possible, and they choose the one in which they have most payments to make. They naturally then try to keep their currency at a fixed rate on sterling; but Australia and New Zealand did not cease to be members of the Sterling Area when they depreciated on sterling. As is pointed out, most of the "sterling currencies" are at a discount on sterling compared with gold standard parities. The matter has some importance, since it follows that any Sterling Area country can correct what it regards as an over-valuation of

[1] Money and Banking, 1938–9: Vol. I, Monetary Review; Vol. II, Commercial and Central Banks. (Geneva: League of Nations; London: Allen and Unwin, 1939. Pp. 173 and 202. 5*s*. and 6*s*.)

its currency without fear of retaliation, England being the only country which cannot depreciate on sterling. There is a very useful table of " devaluations " on pp. 36–7.

In discussing the sterling-dollar rate the report goes through a traditional exercise in the calculation of purchasing-power parities. This seems hardly worth the trouble, since it is pointed out that capital movements were the chief cause of the decline in sterling, and that the United States' balance of merchandise trade reflects mainly the state of industrial activity—and therefore of imports—in America. Further, " allowance should be made for changes in commercial policy " ; since the 'twenties the United Kingdom has turned protectionist and the United States has imposed on its industry " handicaps " in the way of social legislation, far more onerous than anything of which English industrialists can complain.

The survey of credit developments and policy is a model of objectivity and judicious comment. The writer appears, however, to be a little surprised by the facts he chronicles ; at any rate he is continually pointing out that monetary changes have had no effect when no effect was to be expected—monetary expansion under condition of " full employment " did not lead to any inflation of prices in Germany and Japan (p. 41) ; an important increase in cash did not have any striking effect on the other main accounts of the banks in America (p. 46) ; a substantial expansion of the quantity of money was accompanied in France by a marked falling-off in the rate of increase of commodity prices (p. 51) ; the aggregate volume of banking deposits continued to grow to a record level for months after business turned sharply down in the United Kingdom (p. 53). The examination of monetary figures is complicated in England by the unknown incidence of Exchange Account transactions ; it may be suggested that bankers' deposits with the Bank of England are bound to fluctuate more widely as Government transactions expand (and increase the importance of public deposits), so that total Banking Department Assets on total Banking Department Deposits is a more reliable index of credit conditions.

The sections on Structural Changes and the Bill of Exchange are interesting and timely. The former is of value particularly as indicating not only changes but also differences between national systems. British and North American institutional arrangements have dominated the text-books and influenced theory ; but the overwhelming predominance of commercial banks among deposit-receiving institutions in these countries is far from being universal.

The table on p. 79 (distribution of deposits and assets between different types of institution) and the tables in the appendix, IX (Cash Ratios) and XIII (Ratio of banks' own resources to their public liabilities), are particularly illuminating. The restriction on banks' power to increase their holdings of investments, imposed by the fear that depreciation will wipe out their capital, is commonly neglected in discussions of credit policy.

One or two small corrections may be noted. It is not strictly true to say that "the interest on deposits paid by building societies is exempt from income tax"; the societies compound, but pay the full estimated amount of tax due from their shareholders and depositors. The use of stamp duties as an indirect means of measuring the volume of commercial bills in the London market (p. 99) will perhaps serve to indicate fluctuations; but these duties are a very defective measure of absolute amount, since they include bills in any part of the Kingdom and not only London, they include promissory notes and moneylending bills, and they are graded by shillings (so that a bill of £101 attracts the same amount of duty as one of £200). From other indications a guess might be hazarded that they give a figure about double the true figure.

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London.

OBITUARY

EDWIN ROBERT ANDERSON SELIGMAN (1861–1939)

THE news of the death of Dr. Edwin R. A. Seligman, McVickar Professor Emeritus of Political Economy and Finance in Columbia University, at Lake Placid, in the State of New York, which was announced in *The Times* of June 20, came as a shock to a large body of economists all over the world. Fellows of the Society received the news with special regret, as Seligman contributed several papers to the JOURNAL, and was its United States correspondent from 1905. His passing at the age of seventy-eight removes another of the elder statesmen among economists who have for many years looked out from the economic watch-towers with a skill all their own. Several of these elder statesmen, however, are fortunately still with us, including Bonar, Ely, Taussig, Hollander, Irving Fisher and also the reviewers in this JOURNAL of Seligman's early publications on public finance—Bastable, Price, and Higgs. It is desirable