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REFORMULATION OF CURRENT BUSINESS CYCLE THEORIES AS REFUTABLE HYPOTHESES

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The business cycle is largely a measurable phenomenon, showing itself in the movement of many economic variables. It is a coherent movement but not rigorously cyclical. A large number of theories aim to explain its nature. Most of those current in economic textbooks and literature run in qualitative terms. The need for testing these theories is now generally felt, because of their multiplicity, their mutual inconsistency, and their incompleteness or indeterminacy from the quantitative point of view. Testing will hence be of great significance, both for our factual knowledge of economic fluctuations and for the further development of the theories concerned.

1 PURPOSE AND NATURE OF CURRENT THEORIES

Before discussing how business cycle theories may be tested, it seems useful to analyze briefly not only the nature of current theories, but also their *purpose*. Evidently their proximate purpose is to make the mechanism of economic fluctuations understandable. The immense effort devoted to business cycle research during decades would hardly have been justified, however, by scientific curiosity alone. One further purpose is to meet the need for forecasting economic depressions; another, in the author's opinion more important, is to uncover an appropriate 'business cycle policy', e.g., practical means of influencing economic fluctuations. It is of some use to state this ultimate purpose, since theoretical investigations seem hardly germane to it, a point to which we shall return later.

If we want to describe the *nature* of business cycle theories, it appears to be appropriate to use certain concepts introduced by econometrics. When speaking of business cycles and their explanation, we have in mind a number of phenomena, variable in time, which for short we call 'variables'; e.g., incomes, consumption, import duties, crops. Some of these variables are considered to be given to the economists. They relate to phenomena outside the economic sphere or outside the part of the economic sphere that is under consideration, e.g., one country. They may be called data, independent variables or exogenous variables. The other ones, the subject of economic explanation, may be called dependent or endogenous

variables. Between the variables relevant to economic fluctuations there is, in the opinion of the theorists, a network of causal connections. Given movements in the data therefore cause movements in the endogenous variables and it is the task of business cycle theory to show that the characteristics of observed movements in endogenous variables may be explained either by the given movements in the data or by the properties of the causal network. It is an open question how far the causal connections imply a completely exact determination of the movement in the variables they explain. Deviations from complete determinacy may be either explained by the omission of an unknown additional cause or by a fundamental uncertainty in the influence exerted by the explanatory variable on the variable to be explained (or of the 'cause' on the 'consequence').

Business cycle theory has to answer in particular 4 questions: What variables are relevant? How are they interrelated? How are observed movements to be explained? How can these movements be influenced by changes in structure or policy?

The usual procedure in the formulation of a theory consists of picking out a certain relevant variable and telling a story about the factors, e.g., movements in endogenous and exogenous variables, causing that relevant variable to move. Other variables are therefore also brought successively into the picture and a more or less coherent mechanism of the whole cycle develops; meanwhile special attention is given to what the theory believes to be the crucial point. Only the mathematical theories give a *complete* list of the phenomena included and of the causal connections accepted as existing among them. A systematic description of the network of causal connections is usually lacking. Some of these connections are stated explicitly, others are taken for granted. The explanation of the observed movements usually emerges as the consequence of a *combination* of the direct causal connections among the variables. Movements of income during the upward cumulative process, for example, are explained by stating that rising incomes make expenditure rise, and, in turn, rising expenditure causes income to rise further. This is a very simple example of the combination of 'direct' causal relationships (or 'structural' or 'autonomous relations'). One might say that such combinations create new indirect causal relations or derived relations. A well known recent example is Mr. Harrod's theory where he combines the multiplier relation with the acceleration principle.

Since the combination of many direct causal relationships may soon lead to almost intractable combinations, some theorists have rightly deemed it useful to introduce *derived variables* of a more abstract character, which in essence are combinations of several other variables. Such combinations may appear to play an important role in the mechanism and its understanding; e.g., the concept of the natural rate of interest as introduced by

Wicksell. The use of this concept may be seen primarily in the simple formulation of credit policy which, according to this school, should be followed in order to avoid business cycles: if at any moment bankers would only take care that the market rate of interest should equal the natural rate, no development of a cumulative process need be feared. Whether or not one would, with the present state of knowledge, still adhere to the idea that credit policy would be a sufficient instrument to stabilize business cycles, we may note the convenience of such a 'derived variable'. One could very well imagine that similar concepts would be used in the description of public expenditure policy or wage policy.

Most current theories, in particular of the qualitative type, are partial and incomplete. They do not include all the relevant causal relations among all the relevant variables, but concentrate upon some that they think most important or that have not been stressed by others. Some theories focus on the fluctuations in demand for investment goods, others on the means available for their financing; some theories pay special attention to the influence of interest (both natural and market) rates on investment activities, others to the echo principle or the acceleration principle or the influence of inventions. Still other theories emphasize monetary circulation and its limits or agricultural fluctuations. Their very incompleteness saves many of these theories from being mutually contradictory; some may very well be combined into one or more complete theories.

Sometimes, however, they are indeed contradictory. The best known example is that of the over-investment and underconsumption theories. Their contradiction in simplest form is this: According to the over-investment theory the upward turning point is caused by a shortage of new credits, i.e., the impossibility of financing investments. According to the underconsumption theory, however, it is caused not so much by lack of financial means as by the decline in the propensity to invest; a decline which is explained by a decrease in demand for consumer goods.

2 METHODS OF TESTING

The methods of testing, so far used, may be classified as qualitative or quantitative. Qualitative testing may, e.g., take the form of inquiries into the motives and deliberations at the basis of certain reactions. Interesting recent examples are to be found in the inquiries by the Oxford Institute of Statistics into what factors determine investment decisions. It would seem that, in particular, decisions based on calculations might be investigated this way. As to decisions of an intuitive character, which are not based on conscious balancing of advantages and disadvantages, some doubt may be expressed concerning the reliability of this type of research Apparently, people often really do not themselves know what their motives

for a certain action have been. This is very well illustrated by the failure of attempts to estimate in advance deferred demand for durable goods, one reason being that people did not correctly foresee how much money they would spend for nondurables, once they were available (ice cream, pictures, etc.).

In a sense it may be said that many 'theoretical' discussions between economists reflect the experience, direct or indirect, of economists themselves with similar decisions. Such discussions are of particular value when held between economists with personal experience in the matters they discuss, but even then there is danger of undue generalization.

Quantitative methods or measurements may be subdivided into measurement of timeless structures and measurement of time series. Studies for one period of such structures as household expenditures, cost composition, breakdown of national totals by industries, are very useful indeed, even indispensable for our subject. They have one danger, however: they may lead to illegitimate extrapolation into time. For example, statistics on household expenditures describe a certain relation between income and total consumer outlay, but it would be dangerous to assume that the fluctuations in time of consumer outlay depend on the fluctuations in time of total income in the same way; this 'short-run reaction' does not coincide at all with the structural difference in behavior between classes of households with different incomes.

By far the most important type of measurement in the field of testing business cycle theories, however, consists of measuring time series. Here we may distinguish between the study of single series, pairs of series, and groups of series. Certain of single statistical series have already contributed to the clarification of important aspects of the business cycle. Unemployment figures have shown that the classical hypothesis of permanent full employment is not valid. Population statistics show the almost linear course of population and make it impossible to attribute an upturn to an increase in population. Figures on inventories have shown how important their fluctuations are in comparison with those in investment generally. Time series on labor productivity and real wages reveal their noncyclical course. Figures on the gold reserves of central banks show that the limits of credit creation have not been attained at every upper turning point. Technical data on the lifetime of machinery make it plausible that the echo principle cannot be the only explanation of business cycles.

The second stage of research consists in comparing fluctuations in two series. It was found, for instance, that prices and quantities show parallel variations. A very important fact about the well known differences in amplitude of fluctuation — both in prices and in quantities — of raw materials and luxuries on the one hand and finished products and necessities on

the other hand was discovered in this way. Moreover, there appeared to be practically no lag between fluctuations in the production of investment goods and consumer goods, contrary to what the acceleration principle teaches us. Wages and interest rates lag behind production and prices; we found also that no single cosmic variable offers a sufficient explanation of the cyclical movement. One special type of comparison of two series may be called 'turning point analysis'. Confined to one phase of the cycle, it uses very short time units and tries to find out which of two series shows its turning point first.

Multiple correlation, the natural extension of the more primitive quantitative method mentioned above, is the sole method available so far to explain the fluctuations in one series by a combination of fluctuations in two or more 'explanatory variables'. It may be applied to explain fluctuations in one time series (partial or incomplete econometric method) or to explain simultaneously fluctuations in the whole system of variables (complete econometric method).

3 CRITIQUE OF THESE METHODS

There is no fundamental difference between measuring one series, comparing two, or comparing several series. Every measurement runs the risk of error; moreover, in principle even the simplest comparisons are correlations. If a quantitative comparison is made (such as the statement that sub-series A shows a decrease, whereas sub-series B does not) the rules of econometrics apply. A special danger, for instance, in turning point analysis consists in the strong influence of random deviations on short series. In a sense therefore the econometric method is much more universal than its critics sometimes seem to think. Nevertheless, there are other methods, as indicated above (qualitative methods), and it may perhaps be said that the selection and splitting-up of series constitutes a separate method of research. Moreover, it should be admitted that the results of the econometric method must be negative if one wants unconditional ones. Any positive result can be obtained only at the expense of a certain loss of generality by accepting certain conditions.[1]

There is of course a general danger of a too mechanical application of mathematical methods. Sometimes it will be necessary not to use linear relations only; sometimes it will be necessary to use a different relation for different phases of a cycle, etc. Econometrists are well aware of these possibilities and it seems somewhat unjust to employ this argument against them. We shall come back to examples of such procedures.

[1] Cf. T. C. Koopmans, The Logic of Econometric Business Cycle Research, *Journal of Political Economy,* April 1941.

4 REFORMULATION OF THEORIES

In discussing the reformulation of current business cycle theories we speak of 'a set' of refutable hypotheses since most theories, if they are to be tested, will have to be expressed as a combination of assumptions. This is necessary since the movements of a group of variables cannot be deter-mined unless we know all the relations among them, and these relations must be tested simultaneously. This latter point is a well known conclusion of Haavelmo's.[2]

Not all theories, of course, or not all relations of one theory need be reformulated. But most of them have to be.

Let us first discuss why reformulation is necessary. Current theories often neglect certain relevant variables or fail to explain the relations among them. Certain theorists neglect part of the cycle and concentrate on a few phases. Practically all theories are incomplete because they do not indicate the numerical values of the coefficients in their relations; the same qualitative theory may or may not explain cyclical movements, according to the numerical values of the coefficients.

Another reason for reformulation is that business cycle series are some-times not very clear about the true character of the relationships they include. Sometimes their authors use expressions which have to be trans-lated before the theories can be tested. Examples of such translations will be given below.

In our opinion some theories also use concepts that are superfluous since they may be brought back immediately to combinations of variables already included, or since it is very improbable that the actual course of events should be influenced by them. Examples of the first type are forced savings and the velocity of circulation; examples of the second type are 'marginal costs' and the 'roundabout way'. According to the Oxford in-quiries it is very probable that the average business man does not use the idea of marginal costs in making his decisions and it is questionable whether the introduction of the notion of 'roundabout way' is very useful in describing the business cycle mechanism. Both concepts are of great importance to static theories, however. Another concept that may be superfluous is the distribution of income, at least as long as Engel curves are approximately straight lines.

Coming to our positive task we may state that a first requirement for reformulation is to include all relevant variables and the relations neces-sary to explain their fluctuations. In both respects there must be complete-ness, which may be helped very much by a certain 'administration' of these

[2] T. Haavelmo, The Probability Approach in Econometrics, *Econometrica,* July 1944, Supplement.

variables and relations, i.e., symbols and formulae. It will be necessary, e.g., to list imports and exports as variables, perhaps also stock prices; the decision about including or excluding a variable depends of course upon its relevancy.[3] This is a question of taste and of trial and error. We have to compromise between accuracy and comprehensibility.

We shall not present in this paper a complete model of reformulation. Such models have been presented in the literature, and the reader will be reminded of them in Sec. 5. But it seems useful to indicate by examples how some well known elements of business cycle theories may be translated into elements of these econometric models.

Such a notion as the elasticity of the money supply has an analogue in the coefficient attached to the interest rate in the supply equation for money. Similarly the concept of 'structural maladjustment', as handled by Haberler, may be traced in the coefficients of some equations. He speaks of a structural maladjustment if consumer goods industries tend to grow less rapidly than investment goods industries. Supposing that we have an equation in which the demand for consumer goods is a function of national income, and another equation in which the demand for investment goods is a function of national income, we may say that there is a threat of structural maladjustment as soon as the elasticities of these two demand equations with respect to income are widely different.

The concept, introduced by Professor Mises, of a "deliberate action of bankers", which would be responsible for lowering interest rates and hence for a boom, may be dealt with in the following way. Evidently the opposite of a "deliberate action" may be seen in a "systematic action" based upon the circumstances in which and the institutional motives by which the bankers are acting. One could imagine, for instance, that bankers base their decisions as to the interest rates they charge on their balance with the central bank, on the central bank rate, and possibly on the balance of payments. If, in trying to explain the course of interest rates in terms of these variables, we observe a high correlation, it seems plausible that bankers do not act "deliberately", but "systematically"; if, however, we get a low correlation, the probability is that bankers act more or less deliberately.

The possible influence of the rationing of credits may be detected in the following way. Suppose we could explain the fluctuations in demand for investment goods with the help of explanatory variables, such as profit rate, interest rates, and the price of investment goods. If during a boom actual figures on the demand for investment goods should fall short of cal-

[3] Confusion arises from the use, by different authors, of different symbols for the same variable. Uniform symbols are very desirable and in many respects Frisch's proposal, as given in his Ecocirc system, should be followed. I venture to propose that for imports the Russian symbol И be chosen.

culated figures according to this correlation, the deviation could be attributed to a rationing of credits. If we do not find, on the other hand, such a systematic deviation in booms it would seem less probable that rationing of credits exerted a strong influence.

Bottlenecks, such as the impossibility of supplying all the steel that is demanded because of the limit put by the capacity to produce, could be detected in a similar way. As far as such bottlenecks caused very high prices of steel the equation determining steel prices would show a curvilineal dependence on the quantity delivered.

A typical feature of the over-investment theory, that during the upward phase more investment goods are demanded than can be financed from current savings, may also be tested by comparing savings with figures for demand derived from a multiple correlation for other phases.

As far as expectations are assumed to play a role in the demand for certain goods they may be introduced by asking what factors determine expectations themselves. Expected prices, for instance, may be based on movements so far observed. Expected profits may be based on profits observed after certain corrections (such as corrections for a changed level of interest rates).

A different behavior in different phases of the cycle may be translated by either assuming separate equations for one phase and for other phases or by specifying the reason for different behavior. Suppose the reason for different behavior may be seen in the direction in which prices are moving: then it would be possible to introduce the rate of increase in prices as an additional explanatory variable.

Finally some more general aspects of business cycle theory may be considered. First, let us ask how we may distinguish between endogenous and exogenous theories of the turning point. This can be done only by the process of elimination. After this process we are left with the minimum number of simultaneous equations determining the endogenous movements of relevant variables. The movements may be cyclic or noncyclic. If they are cyclic, an endogenous explanation of the turning points appears to be possible; if they are noncyclic, turning points will have to be explained by exogenous factors.

One last word about the rather sweeping statement, sometimes misunderstood, that the business cycle is only "a cumulation of random deviations". Some people think that for this reason all business cycle research is futile. The misunderstanding is in the word cumulation. The way in which the random deviations are cumulated may be very different and of considerable practical importance. It depends on the system of causal connections between the separate economic variables. And it is this very structure that is the subject of business cycle research. Hence there is no question here of a controversy: everyone agrees that random shocks play

an important role in the causation of cycles, but how they cumulate remains a question of importance.

5 MODELS SO FAR TESTED; A PROGRAM

During the last few decades attempts have been made to reformulate one or more theories and to test them statistically. In addition, mathematical theories have been put forward without having been tested. I shall indicate the first group briefly. The work done under the auspices of the League of Nations consisted among other things of constructing a model for the American economy for 1919-32. Some 40 variables were included and a special role was played by stock prices, which in our view contributed essentially to the instability of the American economy at that time. Similar attempts were made to construct models for the United Kingdom (1870-1940) and Holland (1921-33). In these models quite a number of self-evident relations were included about which there would not be much difference of opinion. But some of the most important reaction equations represented merely first attempts to combine existing theories and to select what seemed relevant. L. R. Klein, who also constructed a model for the United States, introduced important improvements.[4]

One of the most important consisted in the addition to the explanatory variables for investment activity of accumulated previous investments representing the total capital stock in existence. The government sector was also treated far more carefully by Klein, which may be partly explained by the considerable increase in state activity since 1933. On the other hand he did not seem to be very much impressed by the role stock prices played according to my model. Another attempt to build a model for the United States has been recently published by Colin Clark, who follows Klein on the point of the investment equation and in addition pays special attention to investments in commodity stocks.[5] He uses Modigliani's discovery about the influence of the highest income previously earned on consumption outlay. Quite recently Goodwin seems to have applied, when constructing another model for the United States, the idea of a curvilinear investment equation, expressing the fact that there are a lower and upper limit to investment activity, the lower one being determined by the indispensable level of replacement goods and the upper limit by the capacity to produce investment goods. During the last few years J. J. Polak and T. C. Chang have done important work on international systems at the International Monetary Fund.

Generally speaking the first models, consisting of some 40 equations,

[4] The Use of Econometric Models as a Guide to Economic Policy, *Econometrica*, April 1947, p. 111.

[5] A System of Equations Explaining the United States Trade Cycle, 1921 to 1941, *ibid.*, April 1949, p. 93.

seem to have been too complicated. It is almost impossible to understand
their working. Here we find ourselves faced with a fundamental scientific
difficulty. Most economists when criticizing econometric models were
pressing toward including many variables. This very inclusion, however,
makes the model unintelligible. For that reason the Keynesian school pre-
fers to work with very simple models, which certainly appeal much more
to the imagination. They have not, however, convinced everybody. One
way out of these difficulties seems to be to construct a model consisting of
an inner circle of relations between the most important macro-economic
variables and a series of supplementary relations meant to specify and
analyze the inner-circle relations. The inner-circle relations might be
relations using only such broad concepts as total national income, total
expenditure, total imports, total exports, or the general price level. One
might be the relation explaining the total demand for goods and services
(national expenditure) as a function of national income, highest previous
national income, price level, and perhaps the rate of increase of the first
variable. Corresponding to this one inner-circle relation there could be
supplementary relations explaining the demand for separate groups of
commodities and services, for instance, for consumer goods or for invest-
ment goods. The demand for consumer goods could in turn be split up into
the demand for food, textiles, housing, etc. Similarly an inner-circle rela-
tion for total imports (as a function of national income, national price
level, world price level, etc.) could be 'backed' by equations for the imports
of raw materials, finished products, investment goods, etc. Each inner-
circle relation could in this way be illustrated and tested, and possible
deviations between observed and calculated values of the macro-economic
variables 'localized', i.e., it could be found out whether some deviations in
total imports are to be attributed to imports of raw materials or of finished
products, etc.

Another reason for the development of residuals in macro-economic
relations is a possible change in structure leading to deviations between
constant-weight indexes and the true aggregates they represent. These re-
siduals could be identified also by a system of supplementary relations.

These supplementary relations therefore would make the base of our
knowledge firmer and would enable us to test the regression coefficients in
the inner-circle by specifying them in the supplementary relations. This
way helps us also to interpret in economic terms the residuals in the inner-
circle relation, an interpretation of especial importance.

It seems to me that current reporting on the business cycle situation
could gain very much by the introduction of this method, if business cycle
reports were based on the periodic extrapolation of inner-circle and sup-
plementary relations. Fragmentarily this method has been applied by

Wagemann and quite recently by the *Survey of Current Business*. It could be made more systematic and more scientific if it were applied to a complete set of inner-circle relations, with as many supplementary relations as seem practical.

COMMENT

TJALLING C. KOOPMANS, *Cowles Commission for Research in Economics*

Since I have little disagreement in principle with Professor Tinbergen's illuminating paper, I should like to use this opportunity to make comments that connect Professor Tinbergen's remarks with the discussion of Mr. Christ's paper.

Attempts by econometricians to develop theories of economic fluctuations screened by systematic analysis of time series and other observations had their origin in attempts merely to formulate dynamic economic theories. Even after a concern for confronting hypotheses with observations developed, 'theory', that is, postulated behavior relationships not derived from the data used in the confrontation, continued to be recognized as an indispensable element in the analysis. I agree with Professor Friedman that the economic literature does not offer us anything like a systematic dynamic theory to work with, but rather a variety of incidental ideas, as yet full of gaps and ambiguities. This was precisely the difficulty of the econometricians who looked toward theory as a frame of reference from which to interpret systematic observations. They often had to supplement or even produce theories in order to advance in the direction they took.

As an example of the type of theoretical construction needed, let me mention the distinction between a short-run and a long-run demand curve, which was drawn in the preceding discussion. These cannot be two disconnected and independent explanations of quantity demanded in a given price situation, because if such a notion were entertained, the short-run curve might call for a different quantity of demand from that consistent with the long-run curve. The appropriate concept of a relationship is not one or two curves at all, but what the mathematicians call a 'functional'. This is a relationship indicating how quantity demanded depends on the entire history of price (or of prices) up to the time demand is to be 'explained'. A particular example of such a functional is given by the hypothesis advanced by Duesenberry and by Modigliani, in which consumption

expenditure depends on the historical maximum of preceding incomes as well as on current incomes.[1]

The gaps and inconsistencies in available dynamic theory were revealed more clearly as a result of the econometricians' insistence on a *complete* theory, in the sense that as many relationships should be postulated as there are economic variables to be explained. This requirement was a theoretical and logical one, not initially related to problems of statistical estimation. The counting of equations and variables has long been a stock in trade of mathematical economists in the study of static equilibrium. It is a surprising and, to me, unexplained phenomenon that the same completeness requirement penetrated business cycle theory only at a much later date. The sole explanation I can see is that the mathematical formulation of theory in this area was attempted much later, but this fact itself is also difficult to understand.

I may perhaps insert here a marginal comment on the statistical aspects of the completeness requirement. As a first response to Trygve Haavelmo's fundamental work,[2] it was thought for a time, and this view is expressed also in Section 4 of Tinbergen's paper, that statistical inference (estimation of parameters or testing of hypotheses) concerning behavior equations is possible only on the basis of explicit formulation and simultaneous estimation of a complete equation system. It should be mentioned that later work by Wald, and by Anderson and Rubin has shown the possibility of methods of statistical inference which, while recognizing the postulate that a complete equation system describes the formation of the variables, require explicit formulation of only one or a few of the equations in question.[3]

Let us stop now to consider why the estimation of behavior equations, each possibly subject to random disturbances, is a desirable goal of business cycle analysis. One might object: Since such an equation system defines a stochastic process generating the variables, why not confine

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[1] James S. Duesenberry, Income-Consumption Relations and Their Implications, Lloyd A. Metzler *et al., Income, Employment and Public Policy: Essays in Honor of Alvin Hansen* (Norton, 1948), pp. 54-81; Franco Modigliani, Fluctuations in the Saving-Income Ratio: Problem in Economic Forecasting, *Studies in Income and Wealth, Volume Eleven* (NBER, 1949), pp. 371-443.

[2] *Econometrica*, July 1944, Supplement.

[3] Abraham Wald, Remarks on the Estimation of Unknown Parameters in an Incomplete System of Equations, pp. 305-310 in *Statistical Inference in Dynamic Economic Models,* Cowles Commission Monograph 10 (Wiley, 1950); T. W. Anderson and Herman Rubin, Estimation of the Parameters of a Single Equation in a Complete System of Stochastic Equations, *Annals of Mathematical Statistics,* March 1949, pp. 46-63, and 'The Asymptotic Properties of Estimates of the Parameters of a Single Equation in a Complete System of Stochastic Equations', *ibid.,* December 1950, pp. 570-82.

ourselves to estimating the characteristics of that process? Let me try to clarify this point by an example that may help to illustrate also the notion of a stochastic process.

Let us assume that I am in possession of six loaded dice (the nature of the loading being unknown to me). Besides the six numbers on the faces of each die, there is a number identifying each die, which runs from 1 to 6 as we go through the set of dice. I now take a die at random and throw it. I record the score x_1, and select for the next throw the die whose number equals the recorded score, and throw it, obtaining the score x_2. This process is continued for a large number of throws.

I now transmit the sequence $x_1, x_2, \ldots$ of scores to Professor Friedman, telling him only that they were obtained by throws of dice. He will soon deduce from a study of the observations that they were generated by a stationary stochastic process. That is, the series can be described as having been generated by a sequence of probability distributions, such that the distribution from which the next score x_{t+1} is a random drawing itself depends only on the preceding scores $x_t, x_{t-1}, \ldots$ but not otherwise on time t.[4] It is important to realize that my knowledge about the mechanism generating the observations, which was withheld from Friedman, does not give me any advantage in forecasting the next score by studying the same long series of observations.[5]

Much statistical work on business cycles is done by the method that through lack of information has here been forced on Friedman: study of statistical regularities exhibited by historical time series. Indeed, for purposes of forecasting the next few 'scores', more is not needed or even useful, provided we can rely on the assumption that the mechanism generating the observations is not changing.

To illustrate the limitations of that assumption, and of the type of analysis flowing from it, let me now assume that I replace the first die by a perfect die. When I inform Friedman that this particular change has been made to the die numbered 'one', he has no way to use that information to modify and improve his forecast of the next few scores. Such improvement can be made only if it is known precisely how the mysterious die numbered 'one' enters into the generation of the data.

To adduce an econometric example, consider the problem of forecasting the effect of a quantitatively given change in the personal income tax

[4] In this particular case, he will note that the process in question is a Markoff process, that is, that the distribution of x_{t+1} depends only on x_t, and not on earlier scores $x_{t-1}, x_{t-2}, \ldots$, but the processes we need to consider in dynamic economics will entail dependence on a longer history.

[5] For a shorter series I would have the advantage that no doubt need be entertained about the stationary (unchanging) nature of the process.

schedule. It is not possible to trace that effect from knowledge, however complete, of statistical regularities in the data generated before any such change took place. It is possible, subject of course to the usual and possibly wide margins of error, to make such a prediction if previously, with the help of 'theory', we have specified, identified, and estimated from the data, the behavior equation explaining consumption expenditure out of a given income, and possibly other behavior equations entering into the formation of income and expenditure, briefly the dynamic *structure* of the economy.

In the light of these considerations, how should we evaluate the outcome of the Klein-Marshall-Christ experiment? Let us first consider it from the viewpoint of straight forecasting on the assumption that changes in structure can be ignored. Then we note that the method has performed no worse than the estimated standard errors, obtained by it, indicated that it would. At the same time, the method has performed no better, in forecasting the quietly prosperous year 1948 from data up to the similar year 1947, than the naive model that ignores all change. The situation can be likened to the attempt to build a tower in order to look beyond an inaccessible wall. It has been found that the tower is not yet high enough to look beyond. Does this mean we should abandon the effort or build further?

It seems to me that we need to build further precisely because the prediction problems arising in practice are those where the effects of policies need to be predicted. Both Friedman and Marschak have stated that the 'naive model' also incorporates a 'theory' of the effects of policies: the effect of any policy is nil. Let us consider the example of the German rearmament beginning with 1933. Given the knowledge that a large rearmament program was being put through, would our prediction of its effect on unemployment in Germany have to be that there is no such effect? Is not this heroic agnosticism somewhat artificial? Varying the children's game "I see, I see, what you don't see," this attitude might be described by "I don't see, I don't see, what you all see."

I do not think that we can long persist in such a preference. In assessing the effect of known or hypothetical changes in economic structure of a type not previously observed, statistical regularities by themselves provide no information. The choice is between theoretical models and theoretical models screened by systematic observation. I cannot see that our answers could get worse as a result of such screening. We should therefore use whatever data we can collect to reduce the number of alternative theoretical explanations in which we might place some confidence.

What then do we infer from Mr. Christ's results? The test of straight forecasting performance of the improved Klein model shows that, indeed, we are still quite far from a knowledge of the economic structure sufficient

to give useful forecasts. This may well be due to inherent limitations on forecasting possibilities which would apply even if much more complete knowledge existed. The very fact that human economic behavior is subject to random disturbances puts a limit on possible improvements in forecasting performance. Even if that limit were now already reached, we should still attempt to develop models that give us differential forecasts, i.e., models that enable us to estimate the difference of effects between two courses of action or two hypothetical developments.

However, it would be rash to conclude that the limit has already been reached. In a number of ways, it is possible to increase the amount and variety of data employed in the screening process. Both Tinbergen and Friedman give suggestions in this direction. We need to disaggregate in time, in space, and in the concepts measured by the variables. In time we can disaggregate by introducing quarterly data. This will also force us to recognize in our statistical methodology the presence of serial correlation in the disturbances, and thus to face the attendant identification difficulties to which Professor Samuelson has referred. We can disaggregate in space, as Rutledge Vining has proposed,[6] by regional breakdown of relevant variables. Most important, we can disaggregate in the choice of variables, by studying smaller and more homogeneous groups of decision makers. We can study investment in plant and equipment and investment in inventories, by industries or by industrial groupings. We can study consumption by important groupings of consumer goods. It is in these directions, among others, that further econometric work is waiting to be done.

[6] Methodological Issues in Quantitative Economics: Koopmans on the Choice of Variables to be Studied and of Methods of Measurement, *Review of Economics and Statistics,* May 1949, pp. 77-86.

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Does the business cycle exist?

In listening to the exchanges on the 'National Bureau' point of view versus that of the Cowles Commission, and also in reading various pronouncements on the subject it has seemed to me that several fundamental issues are never adequately touched upon. My point of view has already been anticipated, I believe, by some English economists, especially Pigou. Nevertheless the following comment may perhaps be of value.

I shall begin with a quotation from Alfred North Whitehead: "The true method of discovery," he writes in *Process and Reality,* "is like the flight of an aeroplane. It starts from the ground of particular observation; it makes a flight in the thin air of imaginative generalization; and it again

lands for renewed observation rendered acute by rational interpretation."

What Whitehead is criticizing is the notion that *mere* observation of a 'uniformities' or *mere* measurement are sufficient for the evolution of valid scientific law. Without our taking, he maintains, at some stage (in lucky cases a very early one) a flight into the "thin air of imaginative generalization", we are never going to accumulate anything more than a jumble. Thus far therefore his criticism has certain affinities with the ideas of Koopmans.

But what I think is vitally important for Koopmans and others to realize is that modern science and metaphysics are now *post*-Newtonian. I agree with Koopmans as to the necessity of a stage of theory at some intermediate point in the process of discovery, but I profoundly disagree with him in the notion that in discussing cycle theory we are dealing with a process capable of treatment in Newtonian terms; or, to put it more conservatively, I think we ought, at the least, to consider the possibility that we are dealing with a process to which the Newtonian presuppositions are not applicable.

Again referring to Whitehead I conceive that economic phenomena are more fruitfully to be considered in terms of *evolving organisms* than in terms of mechanical law. Professor Schumpeter states that each cycle should be treated as an "historical individual". This idea does not necessarily rule out the notion of a fundamental mechanically recurring process. It may only refer to adventitious external circumstances which somewhat warp the basic movement. When I first began to study the cycle I used the simile of a fish swimming upstream. Sometimes the current might be faster, sometimes slower. Thus the speed of the fish might alter though the effort he expended did not. But it would always be the same fish. Now, however, I begin to wonder whether that fish is there after all!

An economic boom or slump, as I see it now, is like a broken leg. You can get one in any number of ways. If there is any discoverable uniformity in the behavior of the system it is to be found, I submit, in what might almost be called its 'personality'. That is, certain types of culture are peculiarly liable to certain types of disturbance and may show some uniformity in reaction time. For example, a militaristic culture is liable to recurrent war damage and so on. Thus, again using an organic example, a man predisposed to drunkenness might get drunk fairly regularly, and, by using various mathematical techniques, we could easily chart what appeared to be a 'drunk' cycle. Undoubtedly in such circumstances there *are* progressive physiological changes that tend to precipitate a fairly regular crisis. But some drunks do reform and there is an organization called Alcoholics Anonymous.

Yet another quality of some organisms is the capacity for *novel* conceptual valuation. The individuals who compose a society, and *hence* a social system itself (we thus bypass the knotty problem of a 'social-mind'), are

capable of developing new concepts of social organization, new wants, and new ideals. And it is *of the essence* of the cosmological process, as I conceive it, that they do act in that way. But these new social valuations may profoundly alter the reactions of a society, may even stop it dead in its tracks, or they may greatly accelerate it.

Some people, for example Oswald Spengler, Brooks Adams, and Karl Marx, have tried to work out set rules for the shifting of ideals. They have tried to show that novel social conceptual valuations can be uniquely and mechanically related to some antecedent change in the basic data. Sociology and econometrics are thus merged — but we remain in a fully determined universe.

Personally, I happen to be a modified voluntarist. To me the past conditions the present but never wholly so. Always there is the elusive element of novelty and of self-determination. There cannot, I hold, ever be a perfect and complete science of history (in the sense of social forecasting). But whether one remains a philosophical determinist and holds that history is immutable — only that we have not yet learned its key — or whether one believes in an open system, it should be clear that the approach we have been sketching carries us far beyond the technique of either the Cowles Commission or the National Bureau. I simply do not believe that any set of econometric models, or any set of mathematical formulae, will ever suffice for reliable economic forecasting over any great length of time. The element of novel social conception is always breaking in.

The social process, I submit, is peculiarly unfitted for description in terms either of simple positivism or of mechanically imposed law. Such approaches result from the naive application of 'scientific' notions already out of date in their own field. Koopmans should realize that things have happened since the publication of Newton's *Scholium*. There is relativity, evolution, and the quantum theory. Economics today is striving to become more mechanical and determinate at the very time physical science is loosening its bonds. I feel therefore that the concept of the cycle as a mechanical wave, whether merely observed or whether 'explained' by various mechanical hypotheses, is both unnecessary and harmful. Now what is the bearing of this on practical procedure?

I began by saying that an economic boom or slump was like a broken leg. Clearly the complete forecasting of a 'broken leg cycle' would be difficult (to put it mildly). But if we lived in a society in which broken legs were both frequent and undesirable, it would certainly be helpful to catalogue the principal known conceivable ways of leg breaking in that environment. We might *then* compare these different possibilities with quantitative data indicating the approach of another conjuncture. But the catalogue would always remain to supplement the limitations of our own imagination.

Thus, speaking more concretely, we should realize that practically *all* theories of the business cycle could under some circumstances be correct, i.e., relevant. A slump, for example, can come because of too little purchasing power, but it can also come because of too much. Sometimes one force is the villain, sometimes another. And the logic by which forces succeed one another is never wholly ascertainable. The basic task not merely of cycle theory, therefore, but also of cycle forecasting, will be to compile *and* remember as large a catalogue of possible forces as may be, *then* to look at the 'facts', mathematically described or otherwise. Personally I do not believe our catalogue can ever be complete. But at least if our approach seems distressingly tentative it will perhaps save us from the more disastrous possibilities that lurk in arrogant certainty.

On the whole I believe the National Bureau comes off better than the Cowles Commission from this critique. The Commission's hypotheses dwindle to mere items in a vast catalogue and its basic world view is directly repudiated. But the National Bureau, even if some implications of its reference cycles seem disturbingly ambiguous and inchoate, nevertheless presents us with the recorded facts against which our flights must always be compared.