DISCUSSION

JAMES W. ANGELL: Professor Schumpeter has observed that "analyzing business cycles means neither more nor less than analyzing the economic process of the capitalist era." Both of the papers just presented make important and constructive contributions to the solution of this problem of comprehensive analysis.

I have been asked to comment especially on Professor Gordon's paper, though I shall also venture a few more general observations presently. Professor Gordon's paper, as he points out, is a first progress report on the elaborate study of American business cycles in the interwar period, which he now has well under way. Examining it in much detail, either with respect to its obvious merits or with respect to points of possible controversy, might therefore be unfair, and in any event is debarred by limitations of time and space. I should, however, like to say something about certain general aspects of Professor Gordon's paper, which are revealed both in the list of unanswered questions which he poses, in his discussion of various alternative methods of going at the central problem, and in his preliminary conclusions on the interwar period.

First, Professor Gordon recognizes clearly the necessity for what he calls initial theorizing; that is, for setting up working hypotheses. But he does not indicate here what role this initial theorizing is to play in his own work. Nor does he suggest any set of "explanatory" hypotheses, or any more general body of business-cycle theory, which might form the initial theoretical structure of his own work and be tested by it. This is a serious apparent gap, but one which is doubtless explained by lack of time on the present occasion for any substantial statement.

Second, he is concerned throughout, not only with the cyclical elements of general business fluctuations, in a narrow sense of the term "cyclical," but also with longer-run or secular forces and movements, and with the interrelations between them and the cyclical elements. This line of approach seems prima facie reasonable and realistic. We can hardly hope to understand any one part of a complex phenomenon without some grasp of the thing as a whole. Moreover, it would be pointless to ignore possibly valuable information before its significance had been fully appraised; and in the early stages of a research study, it seems only common sense to avoid making any very rigid distinction between what may be thought in advance to be secular factors and those which may be thought to be cyclical. Yet this procedure—or, perhaps better, this point of view—raises a serious question as to how Professor Gordon really conceives of so-called "business cycles," and as to what his own initial hypotheses are with respect to the nature of the cyclical components of general economic fluctuations. Does he think of business cycles as really being economic entities, in some defensible sense of that term, corresponding to reasonably stable response and behavior patterns in the economy? Or are they simply the more or less fortuitous result of the fact

that longer-run forces may vary from time to time in their strength, and they may operate somewhat irregularly? His view does not seem to me entirely clear. I will return to this problem of underlying conception a little later.

Third, he proposes to examine with great care the behavior, during cyclical fluctuations, not only of broad national aggregates, but also of the various major segments of the economy, especially at the turning points. As I interpret him, he will endeavor to construct explanations for each segment which at the outset are as far as possible internal to the particular segment itself. This latter procedure, too, is a way of trying to ensure that all potentially significant information is utilized. It is also a procedure which will be especially attractive to those who have a suspicion that many, if not most, major economic fluctuations somehow get started in or by particular parts of the economy rather than suddenly bursting out at many points simultaneously. The danger must not be ignored, however, that an essentially particularist rather than aggregative approach may blind one to the possible existence of explanations of widespread economic fluctuations which are both more generalized and also more significant.

Finally, Professor Gordon examines two of the methods by which other students have attacked the problem of empirical business-cycle analysis. These are the statistical approaches of the econometric group on the one hand and of the National Bureau of Economic Research on the other. Both are rejected, for a variety of specified reasons. Professor Gordon contends chiefly that the econometric account of what we may call the “causes” of cyclical fluctuations is thus far unconvincing. Although the correlations obtained are high, the variables used are so broad in character, are so extensively intercorrelated, and are so relatively few in number, that the apparent “explanations” might be equally consistent with a number of hypotheses other than (and perhaps partly inconsistent with) those specifically postulated. I think Professor Gordon does rather less than full justice to the so-called “econometric method,” however. Even though substantial force be granted to his contentions, the very essence of scientific procedure surely consists in doing just what it, as well as various other methods, attempts; namely, formulating the best possible initial hypotheses relative to the problems set, testing these against the available data, revising the hypotheses where necessary, again testing, and so continuing on until the possibilities of the existing data and of available scientific imagination are exhausted.²

The National Bureau technique is rejected for what is in a sense the opposite reason—that in Professor Gordon’s view it does not take us very

²The shortcomings of the results obtained by the econometric method to date arise at least in part from the incompleteness of the available data and in part from difficulties of statistical technique in handling substantial numbers of intercorrelated variables. In the end, however, they may also prove to be due in part to the very nature of economic behavior itself. It may turn out that those types of uniformity in terms of building blocks and of behavior patterns which have made progress possible in the natural sciences cannot be demonstrated in economic societies; and hence that economic behavior cannot be shown to have sufficient stability in a sufficient number of its important elements to permit many generalizations that are both accurate and valid over a period of time long enough to make them useful.
fear in arriving at any explanation at all of the interwar period. Its emphasis on 
average or typical behavior entails ignoring what he regards as wide and 
significant differences among the probable causal relationships prevailing in 
different individual cycles. Moreover, he thinks it has yielded rather little, 
at least to date, in the way of explanatory generalizations; and its choice of 
the "short" cycle as the cyclical entity means that many of the results 
become inapplicable if the whole period 1921-33 is taken as the unit for 
study. Comment on these views, however, may properly be left to Professor 
Burns.

Professor Gordon therefore proposes to adopt, instead, what he calls the 
quantitative-historical approach, which he has just outlined, and which clearly 
presents a number of very attractive features. I shall not attempt here to 
appraise the detail of the proposed quantitative-historical approach. I am, 
however, much concerned about certain questions of general conception which 
are raised by Professor Gordon's outline of the method and of the way in 
which he wants to use it and, also, to some extent, by Dr. Koopmans' paper. 
These questions can be summarized in the single query, what is it that 
we are talking about when we discuss business cycles or what is it that we 
are trying to explain?

By this I do not mean, of course, to question the reality of the wide 
fluctuations which appear, recurrently if irregularly, in the general business 
activity of all advanced private-capitalistic societies. Nor do I mean to 
question the well-established fact that a number of important observable 
characteristics are common to virtually all the so-called "cycles" of such 
fluctuations in any one country, at least in recent decades. What I am driving 
at is the nature of the differing conceptions of what it is they are studying, 
and what it is they are trying to explain, which different students either 
state explicitly or imply by the character of the results they present. It 
seems to me that a good deal of current business cycle research has become 
so engrossed with individual trees that it has almost forgotten about the forest 
itself. At the risk of merely repeating the obvious, I should like to call 
attention to certain basic problems of conception, the answers to which— 
whether pro or con—current research perhaps takes too much for granted.

First, a question raised above: are the constellations of phenomena which 
we loosely call business cycles really economic entities, in some defensible 
sense of the term "entity"? If so, they must, of course, show some fairly 
stable array of observable characteristics. Moreover, if this observable array 
is to be regarded as the result of anything more than a series of historical 
accidents, the array must be the product of some reasonably stable set of

1 For a penetrating examination of the theoretical concepts lying behind two recent 
statistical investigations, see Edward Ames, "A Theoretical and Statistical Dilemma—The 
Contributions of Burns, Mitchell and Frickey to Business-Cycle Theory" (Econometrica, 
October, 1948). For a more general review of current ideas, see William Fellner, "Employ-
ment Theory and Business Cycles," in A Survey of Contemporary Economics (edited by 
Howard S. Ellis, 1948).

* Or perhaps of a statistical illusion. Thus it is familiar that many irregular series of some 
size can be decomposed into any one of a number of sets of linear and/or wavellite 
components, with pleasingly small and neatly distributed residuals.
economic behavior patterns of human beings, acting either as individual consumers or as business managers or both. And these behavior patterns, which must be distinguishable from other sets of economic behavior patterns, must in turn be explainable either in terms of expectations and other subjective psychological considerations, of institutional rules, of technological transformation functions, or of some combination of these or similar factors. Professor Gordon's paper gives little indication of his views on these matters. Yet unless such economic mechanisms can be established and can reasonably be expected to produce economic fluctuations having all the principal characteristics of something we agree to call business cycles, it is clearly doubtful that the term "business cycles" should be used as relating to any economic entity in the real world.  

Second, if the trial hypothesis be adopted that business cycles are economic entities, then what puts them into motion? Are they essentially self-generating, given the general nature of modern business organization and perhaps some initial exogenous disturbance in the past; or do they represent in the main responses—presumably dumping or even one-cycle in character—to a series of "shocks" which are external, in their own origins, to the factors and processes included in the phenomena of business cycles themselves; or is some combination of shocks and self-generating elements responsible? Neither Professor Gordon nor Dr. Koopmans indicates his own view. But here again the explicit and implicit answers to be found in the current general literature differ widely, and here again few if any of the initial hypotheses thus far set up have been at all adequately tested, or even stated in forms making clear that they are hypotheses to be tested.

Third, are the so-called "major" and "minor" cycles both results of essentially the same economic process, which sometimes manifests itself only in short or minor cycles but sometimes produces major ones as well; or are the two also fundamentally different, not only with respect to merely duration and amplitude, but also in a range of other major characteristics? Much of the current work, apart from that of Professor Schumpeter, seems rather to sidestep this problem.

Finally, must the explanation of business cycles which any one investigation finally arrives at also be capable of explaining the larger part of the total "reality" of a given set of general economic fluctuations? The dilemma here is familiar. If attention is focused primarily on individual cycles taken separately, as Professor Gordon is apparently doing at the outset, the explanation given for any one cycle is likely to gain in richness and convincingness. It will account for a larger part of the total phenomena of the selected time period. But then the danger also increases that the particular explana-

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\(^{3}\) If this cannot be done, then any "business cycles" apparently suggested by statistical or other observational data must presumably be due either to a series of fortuitous circumstances or to short- and medium-term irregularities in the operation of the same forces that produce "secular" or "long-wave" movements. A variety of models, both literary and mathematical, which will produce cyclical fluctuations can be found in the literature, but their adequacy as explanations of the real world has yet to be satisfactorily established.

\(^{4}\) Schumpeter, op. cit., pp. 101-175.
tion will lack generality and will not lead to any significant hypotheses relating to cycles at large. If, on the other hand, attention is focused on the factors common to all or many cycles, the opposite danger increases; namely, that the "generalizable" elements of explanation will in many cases constitute a relatively unimportant part of the total phenomena of any one cycle.  

I have tried above to state some though by no means all of the problems which arise over the basic conceptions with which investigators of business cycles necessarily start, whether consciously or not, and which they necessarily carry along in their work. Students differ in the answers they give or imply; and establishing such answers is, of course, a large part of the object of the game. I shall not attempt here to offer solutions of my own. I do, however, want to urge that these and other similar problems of underlying concept be recognized more explicitly than is often the case, and that the preliminary answers to them be themselves treated as vital hypotheses which it is of prime importance to test. One always has such hypotheses, anyway, whether consciously or not. If one embarks on what attempts to be an objective and unbiased process of collecting so-called "facts" but with unrecognized hypotheses which happen to be bad ones, the resulting collection of facts itself is also likely to be defective. It is likely to give inadequate or even wrong answers to certain questions and no answers at all to others that are even more important. The investigations which Professor Gordon, Dr. Koopmans, and their associates are now making should, however, carry us a long step forward toward satisfactory statements and solutions of these problems of fundamental concept.

ARTHUR F. BURNS: All students of business cycles owe a debt of gratitude to Professor Koopmans and to Professor Gordon for laying bare their approaches to the study of business cycles. Koopmans has sketched what he calls the econometric approach; Gordon has outlined what he calls the quantitative-historical approach. Both routes have been pictured attractively and persuasively by their authors, and I suspect that many members of this audience may now be struggling to decide whether it is best to follow the Muse of History or the Queen of Mathematics. An alert participant in the discussion might take advantage of this delicate uncertainty by staking out a claim for still another approach to the understanding of business cycles. I shall not succumb to this temptation. For I think that the paths being followed in business cycle research—whether by Koopmans, Gordon, or others—are not so far apart as may appear.

It is clear that both Gordon's and Koopmans' approaches are statistical, in the sense of involving extensive use of quantitative data. But this is not the only point of agreement. I notice that in listing the essential characteristics of the historical approach, Gordon notes that it may involve "a variety of statistical techniques—including econometric studies." In turn Koopmans observes that the econometric approach is not "a competitor of the historical approach but, on the contrary, a supplement to it."  

The econometric method, so far as it places weight on its "explanatory" variables, may run either risk!  

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Footnote: The econometric method, so far as it places weight on its "explanatory" variables, may run either risk!
approach, but . . . an important instrument of it." I notice, too, that both Koopmans and Gordon assign a strategic place to economic theory in their respective approaches. Gordon states that the historical method "entails initial theorizing—setting up working hypotheses," and that one of its essential characteristics is "full use . . . of qualitative as well as quantitative information." Koopmans likewise observes that the econometric technique is conditional upon initial theorizing; that it is "not a substitute for theory, but one of the servants of theory." If, therefore, I have understood our two authors correctly, the approach of each can be described as theoretical, statistical, historical, and mathematical. I might even add "psychological" to the list, for both Gordon and Koopmans confess to an "unseen hand" in their operations. Gordon notes that in the historical approach "causal inference depends upon personal interpretations and judgment"—to be sure, "after detailed examination of the available evidence." Koopmans is no less explicit. He tells us that "intuitive considerations" play a large role in setting up econometric models; and that while there is "an optimum degree of detail" in excursions of this type, "we are far from knowing at which point . . . this optimum . . . is reached"—which means, I take it, that matters of this sort must be resolved by personal interpretation and judgment.

As I see it, then, the methodological approaches of Gordon and Koopmans have much in common. Not only that, but both seem to be concerned primarily with business cycles in a brief segment of history—the period since World War I. They further agree in suggesting that their approaches—whether to the business cycles of this period or some other—will not necessarily yield a complete and final solution to the puzzle of business cycles. Koopmans asserts that the "true calling" of the econometric approach "is not to answer all questions," and "that in certain circumstances it may leave important questions unanswered." Gordon in his turn notes that in the historical approach it is frequently "impossible to arrive at convincing judgments regarding the actual magnitude of various forces which we may be able to isolate as probable causes of particular fluctuations"—which means, I take it, that in certain circumstances the historical approach may leave important questions unanswered.

Shall we conclude, then, that the approaches of the two authors may turn out in the long run to be very similar? I think that as far as the present evidence goes, this is an entirely permissible conclusion. Koopmans might begin, for example, by constructing a simple model with very few linear equations, estimating the parameters by using annual data. If the model yields unsatisfactory results, he might add additional equations by breaking down the endogenous variables of the system, or by shifting exogenous variables to the endogenous category, or by dipping into the random catchall for variables hitherto neglected. If the results are still unsatisfactory, he may substitute quarterly or monthly data for annual, or devise methods for handling nonlinear parameters, or modify the distributional hypothesis underlying the treatment of the random variance. If it should turn out in the meantime that the estimating techniques recently devised by the staff of the Cowles
Commission are in practice no better than the techniques used by a Schultz or a Tinbergen, Koopmans may abandon his criteria of simultaneous fitting and thereby win the freedom to work with a larger number of equations. If none of these devices help sufficiently, he might put historical boundary dates to the model, devise different models for different periods or cyclical phases, perhaps even experiment with a different model for each phase of each business cycle. If Koopmans should undergo this evolution, he would come very close indeed to Gordon's position—if the latter in the meantime stayed still. But I have no more reason for supposing that Gordon will remain still than that Koopmans would, and it is therefore equally possible that Gordon will come out at Koopmans' mathematical pole. If any here should think that what I am saying is fanciful, I can only plead that they may be relying on information outside the two papers before us—something I am scrupulously trying not to do.

The cold fact is that discussions of business cycle methodology, carried on in the abstract, are merely intellectual exercises in which experience, philosophical insight, and temperament mix in variable proportions. To appraise different methodological approaches responsibly, it is essential to scrutinize the actual findings or results to which the different approaches lead. The critical question is never whether a method is quantitative or qualitative, mathematical or historical, elegant or pedestrian, theoretical or statistical. In 1913 Wesley Mitchell's *Business Cycles* appeared; one year later Henry L. Moore's *Economic Cycles* was published. Mitchell used no special apparatus apart from ordinary charts and tables, but that did not prevent his reaching generalizations about the cyclical process of economic life that stood up well in the next generation. At the same time, Moore's elaborate mathematical techniques did not prevent his results from being discredited by later research. It is possible to cite illustrations of an opposite tenor, but they would only reinforce my point, which is simply that the merits of a technique cannot be judged in the abstract. The purely personal element in the scientific process is sometimes more important than anything else. A method that yields reliable results in the hands of one investigator may produce nightmares when tried by another investigator of comparable intellectual stature.

The important question about business cycle methodology, or for that matter any other body of techniques in economics, is simply whether it does or does not lead to dependable answers to significant questions. Unhappily, this pragmatic test can hardly be applied to the papers presented at this meeting, since both Gordon and Koopmans are still in the early stages of their research. It would be manifestly improper to use the tiny samples of results that the two investigators have put before us as a basis for appraising the merits of their approaches to the vast problem of how business cycles are generated. If a pragmatic criterion is to be applied at all, we must restrict ourselves to the issues underlying the particular results illustrated by Gordon and Koopmans. What I have to say on this subject must be brief.

Koopmans has cited two illustrations of results yielded by the econometric
One relates to the influence of liquid assets on consumer outlay, and here he tells us that the econometric approach has failed to yield a definite conclusion. Gordon has not taken up this complicated subject, but I do not think I am being reckless in asserting that the historical approach is capable of yielding a similar result. Koopmans' second illustration is Tinbergen's negative verdict on the acceleration principle as an explanation of fluctuations in investment. Since Gordon has not discussed this subject, a direct comparison is again impossible. But I can testify that the National Bureau of Economic Research has reached results similar to Tinbergen's, indeed of larger scope, by using an approach that is similar to Gordon's. Furthermore, if the validity of the acceleration principle really hinged, as Koopmans states it does, "on the implied assumption that productive capacity is at all times in substantially full use," then anyone who had doubts on this issue could bring the acceleration principle to a critical test merely by examining some statistics on the degree of utilization of productive capacity—a procedure so simple and straightforward that there is no need to dignify it by any special name. Finally, while I can readily agree that the acceleration principle misrepresents the play of forces on investment in the short run, it seems to me that Koopmans overlooks an important point; namely, that the acceleration principle is sometimes the key to movements of investment over long periods.

Let us turn next to the illustrations cited by Gordon of the results yielded by his approach. To me the most interesting finding is that a severe depression seems to have been followed as a rule by a "submerged" cycle, but I doubt if this suggestion will stand up under critical examination. Gordon's sketch of the cyclical contours of the interwar period I can confirm in large part, though I cannot accept some of the detailed findings. I find it difficult, for example, to square the conclusion that the depression of 1920-21 "led to only the most temporary impairment of the business community's 'propensity to invest'" with a drop of 67 per cent between October, 1919, and December, 1920, in the floor space represented by construction contracts, or with a drop of 91 per cent in machine tool orders between January, 1920, and September, 1921. At this point, as at some others, I think that Gordon has been misled by using annual data on investment expenditures instead of monthly data on investment undertakings. But I do not wish to press criticism along these lines or even to note Gordon's omissions. He has put his results tentatively and with great candor, and I have confidence that his historical sketch will vastly improve as the investigation progresses. I find it essential, however, to observe that Gordon's illustrative results deal largely with the magnitude of certain ups and downs, in contrast to Koopmans' illustrations which deal with questions of causation. As things stand, the number of variables handled by Gordon is small and well within the econometrician's range even if the latter worked mechanically, which of course he need not do. I fail to see why the kind of economic history Gordon has sketched could not also be written, if someone thought it worth while to take the trouble, in mathematical curves with explicit equations; though it is only proper to add that some of the
questions raised by Gordon have no obvious mathematical equivalent. This is about as far, I think, as a pragmatic test applied to the papers by Gordon and Koopmans can take us. If we are to go further in appraising their methodological approaches, we must revert to speculations. It seems reasonable to suppose that if Gordon and Koopmans persist in their present emphases, their results will be cast in different forms—one mathematical, the other literary. That may impede understanding for a time, but economists have become inured to this sort of inconvenience. Even the nonmathematical literature of economics does not lack identical theories expressed in different idioms, to say nothing of different theories expressed in identical words. Thus economist A may assert that, ceteris paribus, demand is a monotonically decreasing function of price, while B states that under stable conditions demand increases as price diminishes. Or economist A may claim that in a competitive market the rate of interest equilibrates the amount of money that households and firms seek to hold with the amount of money in existence—i.e., the amount they do hold—while B asserts that the rate of interest equates the demand for money loans with the supply. Again, economist A may asseverate that if intended investment exceeds the propensity to save, the national income expressed in a wage unit is to the left of its equilibrium position and will therefore rise to its equilibrium value, while B may assert that if the aggregate profits of business firms exceed expectations, they will tend to increase their working forces. Thus the extraordinary richness of the English language has brought its joys and embarrassments. I think that Koopmans and Gordon may at least take comfort in the thought that, if it should turn out that they impose a linguistic ordeal upon one another and upon the rest of us, they may do so in no greater degree than have economists conversing in different varieties of English.

Of course, it is possible—perhaps even likely—that Koopmans will present us with a single, comprehensive generalization, while Gordon will end up with as many or more generalizations than the number of business cycles he covers. But this outcome need not mean that their results will be contradictory. To the extent that Gordon tracks down variables treated as exogenous in the econometric model or secreted in its random variances, his work might prove complementary to Koopmans'. To the extent that Gordon neglects the common features of business cycles, Koopmans' work might prove complementary to Gordon's. Furthermore, I take it as a matter of course that, although Gordon is now chiefly concerned with the features that differentiate business cycles rather than with the features they have in common, he is intensely interested in the latter and will go as far as he can to account for them. I therefore see a basis for hope that Koopmans' and Gordon's results may prove not merely complementary, but actually confirm one another.

In any event, we may look forward eagerly to what they turn up. I anticipate a stimulating account of the interwar period from Gordon's pen. While there is a greater continuity in business-cycle experience before and after World War I than many students realize, there can be little doubt that certain structural changes in world economy did occur around that time.
The period surely deserves intensive study, especially if the background of earlier business cycles is not neglected. Between the 1870's and 1914 the fluctuations of economic activity in the leading commercial nations of the world—Great Britain, Germany, France, and the United States—moved in unison, except for the fact that American experience was occasionally diversified by extra cycles. After 1919 the business cycles of different countries tended to drift apart, though practically all shared in the catastrophic contraction of 1929-32. There can be little doubt that the international gold standard tied together the business fortunes of different nations before World War I, and that monetary individualism is imprinted on the divergent business fluctuations of different countries in later years. The United States emerged as an international creditor after the war, and both foreign lending and foreign trade assumed a new significance in our economy. Exports, which conformed poorly to business cycles before 1914, later fell into step with business cycles. Perhaps the most dramatic evidence of the economic unity of the period 1921-33, which Gordon has described as a major cycle, is to be found in our record of foreign lending. Up to about 1925 the volume of foreign loans placed in this country was substantial. Yet the loans were on the whole of sound quality, as attested by later experience. The rest of the decade witnessed a further expansion in the volume of foreign loans, and a very sharp deterioration of their quality.1 The speculative craze was not confined to foreign bonds, but expressed itself also in the real estate and stock markets. Consumer credit shared mightily in the upsurge, the largest part being devoted to the purchase of durable consumer goods. For decades before the outbreak of World War I the share of consumer durables in the total value of finished commodities had fluctuated around an average of about 10 per cent. In the twenties the percentage doubled, and this swift and momentous change in the nation's consumption habits brought a new element of potential instability to our system.

These and a thousand related facts will emerge from Gordon's study. I expect that he will make the business cycles of the interwar period stand out as individuals, without pushing the interpretation of particular events farther than the intrinsic complexity of individual experience or the quality of available records will allow. I look forward to an integrated interpretation that will test current understanding of the twenties and thirties—a period that is decisive in any attempt to form a reasoned judgment of the economic outlook over the next decade. But I think that if Gordon is to accomplish what fully lies within his power, he needs a more definite framework of analysis than he has presented. His marshalling of evidence on the major cycle of 1921-33 may, perhaps, be facilitated by following financial accounts side by side with national income accounts, and watching the shifts from one form of speculation to another, as well as the changing proportions between the speculative and industrial activities.

It is more difficult for me to appraise the prospects of Koopmans' investigation than of Gordon's. The attempt to describe the essential workings

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1 See a forthcoming study by Ise Mintz.
of the economic system in a comparatively small number of equations is a
new and magnificent conception. Whether the attempt will prove successful,
I have no way of knowing. I think, however, that the chances of success will
be improved if econometricians note carefully the results of systematic
factual studies of cyclical behavior such as Abramovitz' on inventories and
Hultgren's on cost-price relations. I think, too, that the econometricians' work
would be improved if they made an explicit effort to wrestle with the
historical problem of marking off the boundary dates to which their models
are supposed to apply. I think that further theoretical and statistical work
on short-run versus long-run economic functions is seriously needed, and that
the econometricians should experiment with timing relations that shift sys-
tematically over the course of a business cycle—a matter I believe I can
demonstrate is of some importance. I think, finally, that econometricians might
benefit from better record keeping. General econometric models are barely a
decade old, but simpler models go back to Moore and embrace a generation
of research in agricultural economics. As far as I have been able to discover,
no one is now keeping a reasonably full record of how well or how badly the
many different models constructed by econometricians have worked or are
working. Such a record would serve as a measure of progress, and at the
same time provide an instrument that might effectively hasten progress.
Imagine a file kept for each model, excluding of course those that seem too
absurd to follow or those that have turned out badly, for, let us say, a dozen
consecutive years. Once a year a trained analyst would go through the files
and see how well the prediction for an additional year compares with the
observed figure. Each year he would prepare an analysis for publication,
classifying the errors of the various models according to the type of equation
used, the method of estimating its parameters, the period covered by the
model, the economic terms it includes, and so on. Such an analysis would aim
to segregate the factors in econometric model building that seem to promote
success from those that promote failure, and thus pave the way for improve-
ments in the technique. I devoutly hope that someone will undertake this
arduous but necessary task of scientific verification and accounting.

If what I have said is not too wide of the mark, both Gordon and Koop-
mans are engaged in empirical investigations of high importance. True, neither
author has as yet specified the economic theory that guides him, or listed
the variables on which he deems it desirable to concentrate, or commented
on the quality of the available statistics or other information bearing on his
study, or discussed the influence that any of these matters has had or may
have on his methodological approach. But I infer from these silences, as I do
from the soul searching in which each has engaged and from the points of
agreement between them that I noted in my opening remarks, that the
approaches of our two investigators are still fluid. This fact promises well for
their inquiries. Experimentation is essential in the present state of our
knowledge of business cycles, and I see in the experimental cast of mind of
our two investigators the best of reasons for expecting that their researches
will prosper.
GOTTFRIED HABERLER: I confess that I found reading of the two main papers a rather depressing experience. Professor Gordon starts his contribution by formulating questions concerning the interwar period to which we have no satisfactory answer. These questions are so numerous and broad that they cover almost everything one would like to know about cyclical movements of the interwar period. It would seem to follow that our knowledge of the business cycle is identically equal to zero! I like to suggest, however, that this impression is to some extent the result of an optical illusion. If Dr. Gordon had formulated his questions a little more precisely and if he had had the time to supplement the list of open questions by a list of questions on which there is substantial agreement, the picture would have been less depressing.

The gloom emanating from Dr. Gordon's paper is hardly dispelled by Dr. Koopmans' contribution. He tells us that the econometric approach to business cycle research which he champions has just undergone a radical revision. This revision, he says, cannot yet be regarded as completed. I admit that I feel a little like a babe in the woods when confronted with the latest intricacies of the Cowles Commission approach. The fact that an authority in the field of mathematical statistics like Professor E. B. Wilson feels somewhat mystified makes me a little less ashamed than I otherwise would be.

Dr. Koopmans cites one example of the type of problems that can be solved by means of the Cowles Commission method. It cannot be said that this example is calculated to inspire much confidence. Surely to refute the acceleration principle in its simplest form by means of the econometric apparatus is like constructing an atom bomb for the purpose of killing a mouse, which if not still-born has been killed so long ago that its body is by now in an advanced stage of decomposition!

Appropriately enough, research in the field of business cycles seems to move in cycles—or should we say in fashions? Once upon a time all sorts of business barometers were en vogue. The Harvard barometer was one of the best known, and it was copied, with more or less important modifications, by many other services in the United States and abroad.

This type of approach has been completely abandoned. It is completely discredited although it would be ungrateful and unjust not to recognize that business cycle research has been permanently enriched by Persons and his Harvard collaborators through the collection of data as well as the development of methods of time series analysis.

The econometric approach of the Cowles Commission seems to be petering out rapidly or not to be getting anywhere beyond extensive methodological discussions.

What we might call the consumption function approach which grew up on the basis of a crude, oversimplified Keynesianism, was very popular during the war and the immediate postwar period. It produced a large number of gloomy forecasts of an immediate postwar depression and has been entirely discredited by the complete failure of these forecasts. Although this type of

analysis marked a backward step in business cycle research, it left behind a sediment which probably constitutes a permanent enrichment of our economic knowledge.

What has become known as the National Bureau technique of analyzing business cycles, embodied in the imposing work, *Measuring Business Cycles*, by Mitchell and Burns, had rather rough sailing recently. It has been severely criticized by a long list of impatient though competent reviewers such as Hurwicz, Koopmans, Lerner, and Metzler. Still it seems to have greater staying power than the other approaches mentioned, partly at the expense of extreme caution and unperturbable silence on many burning issues.

Frickey's ingenious and highly original approach is largely descriptive. He does not go far in answering questions in which theorists of business cycle policy are interested. But as far as he goes, he puts firm ground under our feet.

Professor Leontief's input-output analysis is the *dernier cri* in the field of quantitative over-all economic analysis. It has become a tremendous success in the sense of being widely adopted and imitated and crowding out to a considerable extent some of the other approaches mentioned. It is primarily long-run structural analysis rather than dynamic business cycle analysis. But it certainly harbors possibilities for business cycle analysis proper. Will it share the fate of the other types of analysis which were just as popular at one time or another? It is too early to give even a tentative answer to this question.

Professor Gordon in his paper proposes a quantitative-historical approach. That approach has at least this to be said in its favor that it is not new but well tried. After all, the method of many of the classical writings on the business cycle may be described as quantitative-historical analysis. Juglar, Aftalion, Mitchell, Pigou, Robertson, Schumpeter—none of them was satisfied with pure theory or nonquantitative description; nor did they confine themselves to the utilization of statistical series, but made full use of nonquantifiable or not yet quantified historical knowledge.

Not only quantitative business cycle research but also the pure theory of the business cycle is in a rather unsatisfactory state. To be sure, great progress has been made during the last twenty years or so. Our stock of cyclical models (theories) has been much enlarged. In fact, it is embarrassingly large! It has become too easy to construct cyclical models. Every sophomore is being taught how to produce any kind of cycles. Give him a couple of lags and initial conditions and he will construct systems which display regular, damped, or explosive oscillations, or exponential movements up or down, as desired. However, the more models we have, the less we seem to know of the real business cycle.

The explicit and systematic introduction of expectations and anticipations has unavoidably complicated things enormously. It marks the passing of the mechanical age in business cycle theory.\(^2\)

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\(^2\)I cannot help feeling that Dr. Gordon's queries and strictures are frequently drawn up in a somewhat too mechanical spirit. He wishes to "see" or almost to "touch" the forces which cause business activity to fluctuate in cycles.
What conclusions shall we draw from this state of affairs? Surely we cannot return to the good old days twenty-five years ago; we cannot retrieve the mechanical age. The human mind cannot unlearn what it has once mastered. We simply cannot forget or ignore the many possibilities of generating cyclical fluctuations which we have been taught by recent model builders. We can only sift them, and reject many as unrealistic, on the basis of empirical (which is by no means purely statistical) evidence. This is inevitably a laborious and time-consuming process which does not hold out hope for reaching rapidly startling and unambiguous results.

But, as I said before, I do not believe that the area of disagreement is quite so large and our knowledge of the nature and causes of the business cycle quite so small as one is led to assume by reading the papers of Professor Gordon and Dr. Koopmans. The fact that there is a considerable amount of agreement on how to mitigate depressions seems to point in that direction. Or is it possible to know something about the causes without knowing anything about the causes? It might be said that in medicine it frequently happens that a cure is known while the cause of the disease or the way through which the medicine works is still a complete mystery.

However, the analogy with medicine is hardly valid. Medicine being a highly experimental science, it is often possible to find cures by purely experimental methods, by trying out hundreds of compounds until a potent drug is discovered. The economist is not in a position to do that. He must wait for history and politics to make experiments for him.3 Unfortunately, history or governments do not make experiments for scientific purposes. Hence conditions are never quite as “pure” as the economist would like them to be. Nor would that be possible, even if a curious dictator wanted to frame his policies with a view to discovering economic laws. For an economic system which is experimented with may behave rather differently than one which is left alone. Just think of possible anticipations. Experimentation in the large, with the system as a whole—and the business cycle involves the system as a whole and requires wholesale experimentation—is a different matter than experimentation in the small.4

If there is, then, a certain amount of agreement on depression policies, it must be based on some agreement concerning the nature and immediate causes of depression. This does not mean that these hypotheses (theories) cannot or need not be subjected to empirical tests. But it is probably safe to say that no depression policy has ever been agreed upon, let alone discovered, by experimentation (even if the word experimentation is taken in the broad sense of watching the experiments of history and policy).

What is it, then, that might be regarded as the area of agreement in our

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3 Of course, the difference between experimental and nonexperimental sciences must not be exaggerated. It is a difference in degree only and an anti-empiricist, a-prioristic methodology of economics (or any other science) cannot be based on the fact that it is impossible to make laboratory experiments with the economy (or society) as a whole.

4 In this respect again there is only a difference in degree between economics on the one hand and the natural sciences on the other. It has been often observed that in experimenting with nature, natural conditions are changed. Mere observation may influence the course of events and thus fail to reveal exactly what happens in an unobserved world.
field? No one of the existing general, full-fledged theories of "the business cycle" is entirely acceptable, and it is extremely doubtful whether it is sensible to look for such a solution. Is it at all plausible that all recorded cycles or even those within some restricted period, say the second half of the nineteenth and the first decade of the twentieth century, a period which may be regarded institutionally fairly homogeneous, must be explicable in terms of the same theory, be it one of the modern precisely stated models or of a somewhat looser type, as for example Professor Schumpeter's theory? My answer is no. At any rate, we should not start with such a preconception. The hypotheses on which many economists agree, often implicitly and without stating them precisely, and on which agreement might be reached and which should be looked for, seem to me of much more pedestrian nature: First, it is an interesting and suggestive fact that there is so much agreement on dating cyclical fluctuations. Cyclical calendars of investigators of so different background and approach as Mitchell, Robertson, Spicthoff, Pigou, Schumpeter, Frickey and Slichter, to mention only a few, coincide extremely well. The broad characteristics of the short cycle are practically the same for all these writers—fluctuations in output and prices, etc. (With respect to the so-called "long waves," there is much less agreement as to dates and characteristics.) Is it too much to say that there is also agreement that fluctuations in the aggregate flow of expenditures is the proximate cause of fluctuations in output, employment, and prices? To be sure, that is not necessarily the case. (If it were, the proposition would be reduced to a useless tautology.) It may be possible to find exceptions for particular industries, but for the fluctuations of business as a whole it seems to be fairly generally accepted.

Moreover, that investment expenditure fluctuates more than consumption, durable goods more than perishable ones, industrial output more than agricultural output, while industrial prices are stabler than agricultural prices, is well established and, I think, generally accepted.

We know fairly well, certainly much better than, say, twenty-five years ago, how all sorts of changes in the data affect upswing and downswing; for example, that changes in the propensity to save, government deficits or surpluses, changes in credit policies and interest rates, export or import changes, exert expansionary or deflationary effects is reasonably certain, although the intensity and hence the practical importance of some of these factors either in general or in particular cases is often a matter of doubt and dispute.

These types of hypotheses (theories) ought to be more fully exploited and utilized by empirical business cycle investigators. If the National Bureau is criticized for its lack of theory, the charge seems to me groundless or justified according to what is understood by "theory." Mitchell and his collaborators were right, in my opinion, in their refusal to start out, even if it were only tentatively and subject to later confirmation or rejection, from any one of the current models or theories of the cycle as a whole. But a fuller utilization of the more modest theoretical propositions indicated above would give their work a more useful direction. Moreover, the substitution for the vague and elusive concept of a "reference cycle" of the meaningful and
important concepts of income and employment⁸ would provide a clarifying focus for the work of the Bureau, would establish closer contacts between the Bureau's work and the work of other business cycle investigators, and would help to substitute cross-fertilization and constructive criticism for the rather sterile methodological bickering which is going on at the present time.

⁸ It should perhaps be pointed out that this is by no means a plea for "Keynesianization" of the National Bureau. In view of the short memory and ignorance of pre-Keynesian literature displayed by many ardent Keynesians, it is not superfluous to recall the fact that Pigou's theory of the cycle (which goes back to the first edition of his Economics of Welfare) has been clearly conceived and executed in terms of fluctuations in income and employment. The same is true of Robertson's Industrial Fluctuations, which has been recently reprinted by the London School of Economics.