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Volume Author/Editor: Arthur F. Burns

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Chapter Author: Arthur F. Burns

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Current Research in Business Cycles

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 . . . 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 conditioned 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

Discussion of two papers presented at the Christmas 1948 meetings of the American Economic Association, "Business Cycles in the Interwar Period: The 'Quantitative-Historical' Approach," by Robert A. Gordon, and "The Econometric Approach to Business Fluctuations," by Tjalling C. Koopmans. Reprinted by permission from *Papers and Proceedings, American Economic Review*, May 1949, pp. 77-83.

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 helps 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 stood 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 varying 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 technique. 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 1920-1921 "led to only the most temporary impairment of the business community's 'propensity to invest'" with a drop of 67 per cent be-

tween 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 pertinent, 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 generalizations as, or more 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 the 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 1929-1932. 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 1921-1933, 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 inter-war period stand out as individuals, without pushing the interpretation of particular events further 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 1921-1933 may, perhaps, be facilitated by putting 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.

¹ See a forthcoming study by Ilse Mintz [*Deterioration in the Quality of Foreign Bonds Issued in the United States, 1920-1930*, published by the National Bureau in 1951].

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