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Volume Title: Conference on Business Cycles

Volume Author/Editor: Universities-National Bureau

Volume Publisher: NBER

Volume ISBN: 0-87014-193-7

Volume URL: http://www.nber.org/books/univ51-1

Publication Date: 1951

Chapter Title: Historical Approach to the Analysis of Business Cycles

Chapter Author: Joseph A. Schumpeter

Chapter URL: http://www.nber.org/chapters/c4762

Chapter pages in book: (p. 149 - 162)

HISTORICAL APPROACH TO THE ANALYSIS OF BUSINESS CYCLES*

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I

To protect the following comments from a not unnatural misunderstanding, I want to make it quite clear right away that I have no wish to advocate the historical approach to business cycles at the expense, still less to the exclusion, of theoretical or statistical work. As my own attempts in the field amply prove, I am as much convinced as anyone can be of the necessity of bringing to bear upon the study of business cycles the whole of our theoretical apparatus and not only aggregative dynamic schemata but also our equilibrium analysis. As should be evident from the last sentence, by theory I do not only mean explanatory hypothesis – as our lamented Mitchell did – but also and even principally the tools that theory puts at our disposal. Not less am I convinced that statistical investigations and methods (including all the methods that cluster around the handling of time series) are essential means for making headway. Further on I shall indicate how theory and statistics fit in with the historical approach as visualized in this paper. For the moment I only wish to emphasize that nothing is further from my mind than any desire to start the kind of methodological discussion which the logic of the modern scientific situation has definitely made a thing of the past.

II

Economic life is a unique process that goes on in historical time and in a disturbed environment. For this and other reasons there is an argument for historical or institutional study in almost any department of economics. But I mean something more specific than that. We talk about business cycles as a scourge (some of us call them the main scourge of capitalist life) and discuss possibilities of eliminating them altogether as if this were an unchallengeable end in itself, whereas it does not seem to me open to doubt:

* The paper here printed is the unrevised draft which Schumpeter prepared for distribution at the Conference. He was not satisfied with it and had intended to revise it but did not find the time to carry out his intention before he died.

a) that the darkest hues of cyclical depressions and most of the facts that make of business cycles a bogey for all classes are not essential to business cycles per se but are due to adventitious circumstances and

b) that these adventitious circumstances might be eliminated and those darkest hues banished without interfering with the cyclical mechanism itself. May I submit it is possible to devise a program of policy which though not foolproof would be effective in reducing the remaining discomforts incident to business cycles to the range within which the ordinary welfare provisions of the modern state are adequate. This fact, if it be a fact, is important both scientifically and practically. From both standpoints it at least merits to be looked at more closely. But this calls for historical analysis into every cycle on record: in the first instance and before everything else cycles must be treated as historical individuals.

III

The fact envisaged may be illustrated by every major crisis in history, but it will suffice to illustrate my meaning by the depression that broke in 1929. It is my opinion that a depression of unusual intensity and duration was in fact due. But, speaking from the standpoint of the United States alone and neglecting, therefore, the relatively minor repercussions of foreign troubles, I challenge anybody to deny that the whole course of events would have been entirely different and that all the phenomena of crisis-neurosis that were such potent factors in shaping subsequent history would have been avoided if there had been no banking epidemics, if the mortgage situation had been normal, and if the speculative mania had been kept in bounds (including in the phenomena that make up the speculative mania the practice of living on unrealized speculative gains). The statistical evidence of the quantitative importance of these three factors is as overwhelming as their demoralizing effects are obvious. Nor do I think that it can reasonably be contested that in their absence there would indeed have been 'business as usual' though on a reduced scale. Now the point is that those three factors were logically separable from the underlying process from which they arose and that they were practically avoidable. To establish this fully would take all the time at my disposal and more. It is only in order to give some measure of precision to my meaning that I venture the following remarks:

First, by saying that the factors mentioned were practically avoidable I mean no more than that a sufficiently powerful and intelligent government assisted by a properly organized banking system could have avoided them. I do not wish to be understood to hold that the American government as it then was and to some extent still is and the American banking system could have avoided them. Now since the power and the constitution

of a government and of a banking system are of course not products of chance, it would be very easy to reply that these factors were inevitable after all, but it is perhaps unnecessary to show explicitly why this objection fails.

Second, this objection apart, it is easy to see that the virulence of the banking epidemics was due to the existence of a host of inefficient Lilliput banks and to mismanagement in some of the big banks. As regards the Lilliput banks, there is of course no reason why the process of concentration so effective in other fields should not have produced a dozen or so huge banks (each with a system of branch offices) that would have been as impregnable to the depression as were the English Big Five, except that politics and public opinion in what I believe to be unreasonable fear of big business ('money trusts' and the like) offered resistance that proved invincible. As regards the cases of mismanagement, they link up with the mortgage situation and with the speculative mania and, through these, with partly inadequate quality of leading personnel. But if so, we should immediately have to add that inadequate leading personnel is not an essential feature of modern financial processes.

Third, it cannot be repeated too often that the catastrophic as distinguished from merely depressed conditions in the agrarian sector were due *only* to the farmers' debt situation and that a nation that will not permit farmers' financial transactions to be controlled has only itself to blame for the consequences. The reform that would have prevented this situation is too obvious to detain us. Nor was it an essential feature of the cyclical process, that is, a feature without which this process could not do its work, that urban real estate credit was in still worse case.

Fourth, speculative manias are quite obviously one of the reasons why in any historical or statistical description business fluctuations are so much more marked in the United States than in Europe. I think most people will agree that it is not possible to control speculative excesses by interest-rate and credit-rationing policies; but there are other means of controlling them.

This analysis could be made very complete but so could the analysis of any other crisis on which we have sufficient material, that is to say, from at least the 18th century on. Intellectually speaking it is evidently no very deep problem to explain why John Law's ventures went wrong and why their failure had the consequences it did have. All this may be very trite. I believe it is. But – I have taken care before that this should not be misunderstood – as guide to depression policy historical analysis of cyclical vicissitudes is all the same worth a ton of dynamic schemata. As I have said, the policies to which they point are not foolproof. There is no tablet, the swallowing of which will immunize the system against events such as

those which we have glanced at. But neither are such policies difficult to devise. They need sound sense, moral courage and, of course, an environment that does not fall from one hysteria into another.

IV

It seems to me important, scientifically and practically, to bring out that all these phenomena are accidental in the sense defined, yet play a role that may very well decide the fate of capitalism. Since this can be done only by detailed historical case studies, the argument for the historical approach to business-cycle research seems established. In reality, however, we have established only half of it and, so far as the scientific aspect is concerned, the less important half. For historical research is required not only to elucidate the nature and importance of the nonessentials dealt with so far, but also to elucidate the underlying cyclical process itself. This underlying process, as depicted in the more important time series, suggests indeed dynamical schemata that may be framed in such a way as to fit practically any contour. In particular, it is obvious that the ups and downs of aggregate investment will by themselves account for most of the surface phenomena we usually associate with business cycles.[1] And these ups and downs lend themselves beautifully to description by means of dynamic models, especially if we introduce the now well established distinction between induced and autonomous investment. Familiar properties of differential, difference, mixed difference and differential and integro-differential equations then give us all, or almost all, we seem to need in order to 'rationalize' our material. They give us oscillations the possibility of which the untutored mind would never suspect and historical description would never suggest – the most striking instance being afforded by the theory of 'oscillators'.[2] But not only is it necessary to look to the historical material

[1] Including the behavior of aggregate expenditure on consumption. Mere precedence or lagging of absolute quantities is of course not necessarily significant. See e.g., Ragnar Frisch, Propagation and Impulse Problems in *Economic Essays in Honour of Gustav Cassel* (London, Allen and Unwin, 1933). The distinction between propagation and impulse problems will presently be used below. Meanwhile we note the methodologically interesting fact that it may be – I think it is – true that it is in the sphere of production rather than in the sphere of consumption that we ought to look for explanation of the cyclical movement and at the same time wrong to infer this from the observation that the maximum in the production of capital goods usually precedes the maximum in the production of consumer goods. (Not everyone will approve of this formulation; it is chosen for the sake of brevity.)

[2] An oscillator is a quantity that does not oscillate or act intermittently itself, yet produces oscillations in some other quantity or set of quantities on which it acts – I take this opportunity to point out two errors into which the builders of exact models are prone to fall in the excitement of the chase: First, if they are proof against the layman's error that some factor, to qualify for a 'causal' role in the cycle, must itself

for verification of the postulates involved and to give it an opportunity for suggesting others to us; it is also necessary to bear in mind that those ups and downs of (corrected or uncorrected) investment expenditure are themselves merely a surface phenomenon and that we must try to see what there is behind it – which means that we must investigate historically the actual industrial processes that produce it *and in doing so revolutionize existing economic structures.* Unless we do this, investment, especially autonomous investment,[3] is a mere label for a blank space and if we fill this blank space by some such thing as 'expectations' we are filling a blank with another blank.

It is tempting to define the resulting relation between the two 'indispensables', the historical analysis and the dynamic model, by means of the concepts, Impulse and Propagation Problems. But it would not be quite correct to say that historical analysis gives information as regards impulses and dynamic models as regards the mechanisms by which these impulses are propagated through the system or, to put it differently, as regards the manner in which the economic resonator reacts when 'irritated' by the impulses. Very roughly this is so and I shall be quite content if my audience accepts the thesis that the role of the econometric model (which includes the statistical element) is to implement the results of historical analysis of the phenomenon and to render the indispensable service of describing the mechanics of aggregates. But the econometric models do more than this – they 'explain' situations which in turn 'explain' or help to 'explain' impulses. And the reverse is also true. For instance, it may be possible to show – personally I believe it is – that *at least* the major prosperities and depressions from the last decades of the 18th century on can all be 'explained' without appeal to endogenous oscillations of the elastic type though the existence of the latter is of course not denied. But whether this is so or not can be established only by finding out what actually happened in each instance in the economic organism. It is even likely that a great many 'waves' of entirely different nature are running along simultaneously, some being oscillatory and others not.

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be of an oscillatory nature, they often imply that such a factor must be aggregative in nature because cyclical situations are defined in terms of conditions obtaining in the whole economy; second, they often take pride in the fact that theories embodied in an exact model will 'explain' *all* the phases of cycles, including turning points, by a single argument and make it possible to dispense with separate theories for the individual phases – this *may* be an advantage but it may also result in misconstructing the whole phenomenon.

[3] But induced investment also because it is decisively influenced by autonomous investment.

V

Nothing has been said so far as regards the nature of the historical information required. In a sense, the compilation of long time series (and it is extremely important to note that for fundamental problems of analysis only long time series are of any use and that time series that start in 1919 are almost completely valueless) is in itself work in economic history. This work is indispensable of course, but it is not what I wish to draw attention to. Apart from the measurements it yields, a set of time series does not so much solve any problem as state in quantitative terms what problem there is to solve. In another sense, it might be thought, annalistic description of business situations and other events that might conceivably have any relevance (the annals of the National Bureau of Economic Research even include the marriage of Queen Victoria!) is all that is required. Such annals have their value. They facilitate the interpretation of the time series and are useful as a check upon theoretical analysis. But they tell us little about industrial processes of change and their effects upon the structure of the economy.

To let the murder out and to start my final thesis, what is really required is a large collection of industrial and locational monographs all drawn up according to the same plan and giving proper attention on the one hand to the incessant historical change in production and consumption functions and on the other hand to the quality and behavior of the leading personnel.[4] The rationale of this requirement can be very briefly stated at the expense of my being more dogmatic than I have any intention to be. First of all it can be established, but it may be taken as intuitively clear, that no society would display *the kind of fluctuations* we usually identify as business cycles if consumption and production functions remained unchanged over time. To this proposition, it is, however, necessary to add two qualifications: (a) Such a society would still be exposed to wars and other political disturbances and to the occurrence of good and bad harvests. Any investigator who is prepared to base his explanation of the business cycle on either or both of these sets of factors would of course not agree with my proposition and I in turn, placing myself on his standpoint, would agree with him. (b) Among short-run fluctuations there is the one that used to be called the 40-month cycle (I usually refer to it as the Kitchin cycle); these fluctuations and perhaps still shorter ones may possibly be explained by some such schema as Metzler's inventory cycle. Hog cycles, sheep cycles, coffee cycles and other such phenomena may be included in the same category. To all this my thesis does not apply either, or rather I cannot aver that it does, because it would lead too far to discuss my doubts on this point.

[4] 'Consumption function' is used here in a sense too obvious to require explanation; it has nothing whatever to do with the Keynesian consumption function.

But all cycles that do not belong in either category can reasonably be assumed to be absent from a society of the kind envisaged. It has often been said that there may not be much left of cycles if we do exclude these. But whether this is so or not is again a question that can be answered only by detailed historical inquiry that yields generalizations through making us understand individual situations. In any case, if there are factors inducing cycles of a different sort, these factors must be connected with changes in the consumption and production functions or, if we do not think much of the autonomous change in taste, of the production functions alone. Now in order to construct a schema that will describe the modus operandi of these changes, it is necessary to refer to industrial history which, when the analytical work is done, will again provide verifications, checks, amplifications, qualifications, and also tell us where we might expect oscillatory movements to play a role. Theoretical and statistical analysis is in this task as necessary as is historical work. In fact they are inseparable because there is an incessant give and take between them. But I am not contradicting this if I say that the most serious shortcoming of modern business cycle studies is that nobody seems to understand or even to care precisely how industries and individual firms rise and fall and how their rise and fall affects the aggregates and what we call loosely 'general business conditions'.

Also, it should not be forgotten that many individual problems about business cycles can be solved only on the basis of such understanding. An instance is the question of the effects of given degrees of monopoly in an economy upon the processes of prosperity and depression. Without the kind of experience that industrial history supplies, all we have to say about this question reduces to reckless assertions or trivial speculations.

COMMENT

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With the two major points Professor Schumpeter stresses I do not have any quarrel. The first — that from an historical study of successive business cycles we can learn much concerning factors extraneous to the cyclical mechanism itself — seems to me indisputable. I cannot judge whether Professor Schumpeter is right in assuming that such study will yield sufficient information to induce policies that will be intelligent and effective in dealing with the more catastrophic consequences of business cycles, but I do

not have any basis for denying such a possibility. On the other hand, the usefulness of an historical study of successive cycles is so obvious on many grounds that one can only deplore its relative neglect. If it could perform only one service – impress upon economists and other students of social problems the rapidity with which determining factors and their weights shift, and the variety of the forces that seem crucial in affecting the course of business cycles as they unfold over a sufficiently long stretch of historical experience in any given country – that service alone might be worth the effort. For it would prevent over-facile beliefs in the stability of multipliers and of other aggregative ratios, and would intensify the demand that any generalization from historical experience be properly labeled with the time and space coordinates from which it was derived and to which it is supposed to apply. In thus subscribing to Professor Schumpeter's call for historical study of business cycles, I would expand its scope, as surely he intended to do, to the particular shapes and forms the cycle mechanism itself took on. This means establishing which industries happened to be the leaders in the expansion and the loci of speculative investment activities; the changing structure of business and other enterprises in the fields of production as well as of finance; the attitudes and policies of government and of quasi-public institutions; the current state of international relations; the impacts of cyclical developments in a given country on those of its most important customers and vice versa; and the like. That this task is difficult but nevertheless is worth doing; that some common frame of analysis and reference is essential so that historical description of successive cycles would attempt to answer a similar set of questions with reference to these individual events; that a combination of quantitative and non-quantitative data would have to be so employed that the statements could be subject to check – all these are observations too obvious to warrant stressing.

Nor can one gainsay the usefulness of Professor Schumpeter's second major recommendation – "a large collection of industrial and locational monographs all drawn up according to the same plan and giving proper attention on the one hand to the incessant historical change in production and consumption functions and on the other hand to the quality and behavior of the leading personnel". The crux of the difficulty here, as in the case of histories of individual business cycles, is partly in the availability of data and partly in the feasibility of establishing "the same plan" so that the results can be expected to be cumulative rather than discrete; and so that the effect might go beyond the specific 'story' elements of a succession of historical events. This *caveat* is somewhat more relevant to industry and locational studies than to historical studies of successive business cycles. The scope of each business cycle is so much wider, the conse-

quent demonstration of the interrelations among a wide variety of economic processes so clear that despite the danger that the results will not cumulate into a series of comparable and additive units, the effort at historical treatment, in the generic meaning of that term, is still worth while. That the same could be said about a monograph on the history of the spittoon-making industry, for example, is not so clear, unless the monograph were part of a wider undertaking so organized that the results would surely be comparable and combinable.

But while joining heartily in Professor Schumpeter's first proposal, and subscribing with some qualms to his second, I must confess to much reluctance in urging the historical approach in the field of business cycle research — as I would hesitate to urge any specific approach in a wide and complex field. The grounds for hesitation are presented here, since they raise certain fundamental questions. The first ground is general: whenever we have a complex field of study, dealing with historically bound experience, various approaches can be employed, and it can always be argued that one will yield some results that others will not. But what is the specific basis for selective emphasis? What evidence can be adduced that one approach is likely to yield more than another, short of an attempt to use both and compare the results, or to evaluate their yields on the basis of past performance? More specifically, can I rely on what is essentially a personal judgment in agreeing with Professor Schumpeter's recommendation, even though he himself warns at the very beginning that he advocates the historical approach as supplementary to, rather than in lieu of, others currently practiced? If we view the situation realistically and consider the limited intellectual resources available for basic work in the field, is not advocacy of a given approach always to some extent at the expense of others? And if so, are there any objective grounds for decision?

This question, to which I have no ready answer, becomes especially acute in the area of business cycle research. If an historical approach means dealing with the succession of individual business cycles, or with the succession of changes in the production-consumption functions and entrepreneurial quality in various industries, we must recognize that at least the former has been practiced by earlier generations of empirically minded scholars. Such scholars as Juglar, Tugan-Baranovski, Spiethoff, Bouniatian, Aftalion and, in some of his early writing, Wesley Mitchell, used historical accounts of individual business cycles in one or several countries of the western world as evidence. Indeed, such descriptions are the early form records in historically bound areas of experience take. They have been displaced in relative importance, if not completely abandoned, in favor of statistical analyses which, however different the underlying hypotheses and the resulting techniques, have in common an attempt to liberate

the observations from subordination to specific coordinates of space and time, to free them from their historicity. In urging the historical approach, even as supplementary but within any realistic set of conditions as partly exclusive of other approaches, we are in fact asking for a reversal of the trend, for a return to a more primitive state of the empirical evidence — even though this does not necessarily mean a return to historical descriptions quite similar to those common, say, in 19th century literature. Are we justified in demanding such a reversal, be it only partial? That I frame the question in this way does not necessarily mean that I am inclined to answer it in the negative. Merely raising it expresses my uneasiness about the advocacy of the historical approach, and the need for somehow testing one's judgment – if such testing is at all possible.

The general grounds for substantiation are easily stated. They are the grounds for distrusting generalizations from historical experience when the underlying framework is changing rapidly. The very definition of business cycles used in National Bureau studies clearly associates them with the preponderance of business enterprise in the economy; and the inference is that the shift in such preponderance would in and of itself shift the basis of business cycles. When wars and other major exogenous factors are prevalent; when a large part of the world is governed by a system in which business enterprises do not predominate; when within the industrially advanced countries of the western world there are apparently rapid shifts away from relatively free business enterprise systems toward more planning under the aegis of the government and more government control, there is a genuine question to what extent one could hope to find recurrent general characteristics of business cycles to warrant anything except a purely historical approach. Yet such general grounds are merely impressions and can be misleading.

Perhaps we can go further and shift the discussion to somewhat more specific and tangible grounds. The distinctive feature of the historical approach is its emphasis on the uniqueness and nonrecurrence of the observed phenomena, which does not, of course, prevent the phenomena from being widely observed in space. And we can give formal expression to this emphasis by saying that the historical approach would stress the secular movements of a nonrecurrent character, within, say, a type of economic system, regardless of its locus, e.g., the system of industrial capitalism; or within the economy of a given country, say, the United States; or within a given industry (whether these secular and structural changes are in the production and consumption functions, in entrepreneurial capacity, or some other aspect). These secular movements are unidirectional and unique as compared with the reversibility and repetitiveness of business cycles. The need for an historical approach, and the acuteness

of such need, may, therefore, be discussed in the following terms: can we understand and generalize about business cycles without taking into account the changing secular framework within which they occur?

The answer is not as obvious as it may seem: the very recurrence of cycles is indicative of some independence of the phenomena from the ever changing framework of secular movements. But we can deal more specifically with the question if we emphasize its relevance to reference cycles, to use the NBER terminology, thereby restating it in terms of cycles recognized as affecting general economic conditions of a country rather than those observed in specific processes. In subsequent discussion business cycles will be understood in this sense.

Such business cycles may first be envisaged as an aggregative phenomenon. This means that we see them as fluctuations in one or at most a few aggregative measures of economic activity, e.g., total employment, total income flow, or total volume of all transactions. These aggregative measures are presumably weighted totals of various sectors (industries, areas, etc.) within the economic system of a given country. We know also, with a fair degree of reliability, that secular movements in the relative weight of these various sectors are quite pronounced. For example, there are marked shifts away from agriculture and toward certain types of urban industry, from the individual to the corporate form of enterprise, and so on. We are not equally sure that within each sector the response to business cycles displays marked secular movements; but we do know that these sectors show different average responses. Given significant secular shifts in the weight of these various sectors, the following possibilities may be considered. First, assume that there are no marked secular shifts within each sector in responsiveness to business cycles. Then, the cyclical fluctuations in the aggregates would probably display marked secular shifts, as long as there is any association between the secular trends in weights of sectors and differences among these sectors in their average responsiveness to business cycles. For example, a decline in the weight of agriculture, combined with a lack of responsiveness of agricultural output to business cycles, would mean, other conditions being equal, a widening of business cycle amplitudes, as far as they are measured in terms of total output. Second, assume that there are secular changes in the responsiveness to business cycles within each sector. They may or may not be associated inversely or positively with the secular shifts in weights; e.g., the diminution in the secular weight of many industries (especially when they become standby) may be associated with a widening in the amplitude of their response to business cycles.

I do not intend to argue that because there are marked secular shifts in the weights of various sectors of the economy, each sector distinguished by

an average different response to business cycles, relative constancy of business cycles in the economy at large is impossible. It would be easy to envisage a scheme by which, even with no secular movements in the responsiveness of individual sectors, a constancy in business cycles would be possible. E.g., a decline in the secular weight of agriculture may be offset by a rise in the secular weight of service industries – a sector whose output also does not respond sensitively to business cycles. All that is argued here is that the existence of secular shifts in the distribution of employment, of output, of transactions among various industries or among various types of business organization, or between domestic and foreign markets, etc. creates a major problem in establishing how there can be secular stability in business cycles, conceived as fluctuations in aggregative totals. It is a problem whose solution would seem to call for direct examination of these secular movements concurrently with the study of business cycles, and a careful scrutiny of any possible trends, compensating or re-enforcing, in the intra-sector responsiveness to business cycles. As far as such secular movements are examined, the historical approach re-enters the field – for secular movements of economies are precisely what economic historians study (even if they do not use the name). They are the unique, nonrecurrent movements that have to be carefully marked as to time and place, and can be divorced from these historical fetters only when they themselves become recurrent or repeated elements in some generalization of a higher order.

But suppose that we view business cycles not as fluctuations in an aggregative total but as a complex of processes, emphasizing similarities and differences in their behavior; that these cycles are concentrations in time of similar movements in a variety of such processes. The implication of this, what might be called a process rather than an aggregative view of business cycles, is that in the final synthesis it is not the mere additive weight of various elements but something else that counts. This something else may or may not be measurable. The unifying thread may be a theory of entrepreneurial expectations and limitations, and the processes may be grouped in a scale of decreasing importance of venturesome expectations and increasing importance of mere adjustments. Or it may be some Keynesian or other version of the volatility and independence of decisions concerning investment. Or it may be some revised version of the acceleration principle. In all such cases it is not easy to see that the well known secular shifts in weights of various sectors of an economy raise the same question as in an approach to business cycles as fluctuations in an aggregative total. Yet one could argue that these shifts in weight are relevant even in a process approach, for after all any theory of the mechanism by which a business cycle develops through its successive phases must take into account the

magnitudes of the forces that are brought into operation; and at least one set of forces is reflected in the secular weights of the various sectors. But we could go even further and urge that *pari passu* with secular shifts in weights there are other equally prominent although not too easily measurable secular shifts: in the structure of business organization associated with the size and monopolistic character of firms; in the organization and behavior of the banking and credit system; in the consumption and savings practices of the individuals and households that comprise the economy; in the degree to which business enterprises and individuals are free to follow the dictates of the market, the profit motive, and the desire to maximize income. If one admits the existence of major secular movements of this character, the problem in the process approach to business cycles is the same as the aggregative approach: how, within such a framework of shifting and irreversible secular tendencies, a relatively invariant, recurrent set of business cycles can occur. And it would seem to me again that a proper analysis of the latter requires an explicit consideration of the problem, explicit attention to the secular trends and their accompaniments in the cyclical functioning of the several processes.

If the emphasis suggested here is at all justified, the implications for planning business-cycle research are important. The most important is the need for lengthening the historical perspectives of research. The elaboration of models of the type discussed by Mr. Christ, combined with the usual shortages in the supply of data as well as with the effort at prediction of the immediate future, results in a foreshortening of the time perspective of analysis that has dangerous consequences. The assumption that the relations found within a short stretch of historical experience possess a sufficient degree of invariance to be useful is obviously risky. The concentration on monthly data in an effort to get a more accurate picture of cyclical movements and the otherwise justified zeal for detail make it altogether easy to forget that the twenty or thirty years covered are a phase of a longer historical process whose transient elements cannot be discerned for lack of a longer stretch of experience with which to compare. One is forced to raise the uncomfortable question of proper balance between effort expended on precise and detailed analysis of a short period and effort expended on an adequate if rough picture of a much longer period of historical experience.

I must conclude by again emphasizing the elements of judgment involved. The historical approach means to me essentially an emphasis on the unique and nonrecurrent features of any segment of historically bound experience; and its chief implication is the need for explicit consideration of the unique framework within which recurrent business cycles manifest themselves. This means necessarily also extending the time horizons of

business cycle study, and a consequent reliance on somewhat cruder data than those limited to a short and recent period in our experience. It means also extending the study to a variety of secular frameworks, exemplified perhaps by different countries. In terms of specific recommendations, it calls for adding to Professor Schumpeter's two points, or perhaps substituting for his recommendation of industrial and locational study, an explicit study of secular movements in the structure of economies in their bearing upon business cycles. But these are not matters about which one can be dogmatic. The suggestions will be treated, I hope, in the spirit in which they are submitted – an honest uneasiness over the current emphasis in business-cycle research.