A CROSS SECTION OF BUSINESS CYCLE DISCUSSION

By Jacob Marschak*

"The purpose of the criticism . . . was not so much that the people did not like what [the painter] was doing as that they wanted to know exactly what was in his mind."—A Bell for Adano

The task of Professors Ellis and Haberler, and of the committees over which they presided, was like the task of directors of an art gallery, cramped into a small house and open to a wide public. The collection must be small yet representative, therefore broad. It must be broad but not shallow, that is, every piece must be significant. But, while significant, no piece should puzzle the busy layman who strays into the gallery to rest and enjoy.

The editors could not have accomplished the task more perfectly. As one revives in one's memory a twenty years' crop of blue, green, and red economic periodicals, one finds that no bouquet could be gathered with more taste and sense of proportion—yes, with more love. This impression is strengthened by Professor Somers's excellently classified bibliography of some 800 articles "on business cycle theories." About one in every forty of these articles has been reprinted in the volume under review. I doubt very much that more than one or two substitutions could be suggested to make the selection more weighty or more representative.

The volume is so representative that any reader interested in the progress of economics is rightly tempted to regard it as a sample of the present state of our discipline. What results have been achieved? What tools are being handed to the next generation (for the volume is also intended as a textbook) for further achievement?

* Cowles Commission Papers, New Series, No. 9. The author is professor of economics at the University of Chicago.

1 Readings in Business Cycle Theory, selected by a Committee of the American Economic Association, Volume II of the Blakiston Series of Republished Articles on Economics, with a Preface by Howard S. Ellis, chairman, General Committee on Republications; an Introduction by Gottfried Haberler, chairman, Selection Committee; and a Bibliography of Articles on Business Cycle Theory compiled by Harold M. Somers. (Philadelphia: Blakiston, 1944. Pp. xvi, 494. $3.50.)

When referred to for the first time, the articles included in the volume will, in general, be quoted only by the author's name and the year of first publication. Page numbers refer to the pages of the volume, not to those of the original publications.
I

I hear that the progress of seismology may be due, in the main, either to better instruments and theories, or to frequent earthquakes. In our case, the earthquakes did most of the job. The 1930's experienced depression. The 1940's experienced full employment. To be sure, the present volume contains little evidence of the direct utilization by economists of those contemporary experiences (an exception is an article by John Williams, 1941; while the factual material used in Alvin Hansen's article, 1939, chosen for the volume happens to refer to population and other trends rather than cycles). Yet it is difficult to imagine that the revolution in our outlook has been due to the tools: they are too blunt! or to the theories: they are too inarticulate! This has happened: the greatest among us, and especially the genius of Keynes, have sensed history and have had the courage and intuition to recast theory accordingly. The historical facts have been so forceful that no particularly clear and consistent theory and almost no measurements were needed to carry the conviction, or at least the feeling, that old economics could not explain all the facts and ought not to direct all policies.

Rather symptomatically, the field of discussion itself has not been clearly defined. The present volume, for example, purports to deal with "business cycle theory." Yet, as in many university courses of the same name, much more is attempted in its component articles than explaining business fluctuations. There is, of course, no harm in a name, provided distinct matters are named distinctly. Four articles (by Schumpeter, 1935; Kondratieff, 1926; Mitchell, 1923; and Tinbergen, 1940) do attempt to explain, or at least describe, the phenomenon of quasi-periodical changes in economic variables; they form the volume's Part I (Over-All Picture of the Business Cycle and Method of Analysis). Part VI on Special Commodity Cycles contains Mordecai Ezekiel's article, 1938, on the cobweb theorem. Of the remaining sixteen articles, hardly more than four or five deal with the trade cycles proper, or at least with the process of adjustments (or explosions: so-called cumulative processes) in response to external trends or shocks: in short, with economic dynamics. Faithfully and fairly reflecting the present state of discussion, the bulk of the volume is devoted, in essence, to the static question: vis., assuming that equilibrium is always reached

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4 Hawtrey, 1926.
5 J. M. Clark, 1917, and P. A. Samuelson, 1939, on the acceleration principle; J. M. Clark, 1939, on "compensatory devices"; F. A. Hayek, 1935, on price expectations, monetary disturbances and maladjustments.
somehow, what will it be under given conditions? For example: assuming that the money value of the supply of all goods (identical with the total money income earned in their production) has somehow reached equality with the money value of the demand for all goods, what will be the money value of this supply (another word for money income), if the wage rate, or the interest rate, or the rate of investment has a given size? This is the kind of discussions inaugurated by Lord Keynes’s discoveries, of which J. R. Hicks said: “The General Theory of Employment is a useful book. But it is neither the beginning nor the end of economic dynamics.”

It is true that the discussion cannot help stumbling upon dynamic relations. For example, “cumulative processes” are described. Or, to take another example, the relationship between income and consumption is ascribed to a “multiplier principle” working over a hypothetical sequence of undefined “periods,” during which sequence of periods the variables converge to certain values called equilibrium values. A third example of rudimentary dynamics is the Robertsonian “day,” used by many authors of this volume: an undefined period of time introduced to justify giving different names (savings and investment) to two dated values of what is regarded as one and the same variable. To be sure, a truly dynamic theory will not neglect hypotheses which those authors may have in mind: the hypothesis, for example, that a divergence between two certain quantities leads to processes which increase the divergence (as in the case of “cumulative” processes of Wicksell and his school), or decrease it (as in the case of Mr. Kahn’s multiplier theory in which the difference between actual employment and the employment compatible with a given total demand gradually fades out); or the hypothesis that a significant time-lag separates consumption decisions

By “demand” and “supply” we mean the variable quantities demanded or supplied, not the functional relations between each of these variables and other variables such as prices. If the latter terminology were accepted, we should have to replace the simple expressions “demand equals supply,” “demand exceeds supply by a given amount,” etc., by more cumbersome ones. The relevance of these expressions for dynamic analysis will appear below.

J. R. Hicks, “Mr. Keynes and the Classics; a Suggested Interpretation,” Econometrica, Vol. 5, No. 2 (Apr., 1937), pp. 147-59. Among the authors included in the present volume, the distinction is recognized by Ellis, 1942 (p. 406); but also by Lerner, 1939 (p. 167), who says: “Mr. Keynes’s greatest fault is perhaps his failure to point out with sufficient emphasis that he is in the main concerned with equilibrium analysis.” Haberler, 1936, seems to go even further and to accuse Keynes of feeding us not even with statics but with “barren” identities. This is another misunderstanding, and reveals Keynes’s second greatest fault, viz. (to paraphrase Mr. Lerner), “his failure to point out with sufficient emphasis that he is not concerned with mere accounting”; some parts of the General Theory (although not as often as does the Treatise with its “Fundamental Equations”), do merely state identities; but not all relations of Keynes’s theories are identities! See the present author’s “Identity and Stability in Economics: A Survey,” Econometrica, Vol. 10, No. 1 (Jan., 1942), pp. 61-74.
from income changes. But, to apply such hypotheses seriously, the concepts and relations must be stated with greater precision than that given them by the original authors and by most of the interpreters. The present ambiguities are well illustrated by Fritz Machlup, 1939, who lucidly enumerates and classifies the various "periods" confused by various authors. The prevailing carelessness in discussing processes suggests that it is the equilibrium values and not the process of reaching them (nor the question of whether they are reached at all) that has interested most economists, at least after Keynes's book of 1936 was read and digested. This is also shown by Somers's bibliography of "business cycle theory": hardly one-sixth of the listed articles deal with the explanation of business fluctuations.7

II

Across the distinction between statics and dynamics cuts another one: that between aggregative or "macro"-economics, and the "micro"-economics of a single firm or household. The contributions of the volume are essentially macro-economic. Some contributors (Schumpeter, Mitchell, Williams, Haberler) emphasize the error involved in unduly sweeping aggregation. But this has been so far almost entirely negative criticism. Little has been done to indicate the size of the error under various types of aggregation and, hence, to find the "optimal" extent and method of aggregation; though probably nobody but a purist will deny that some aggregation, and hence some error, is inevitable because a theory in a million variables can be neither verified nor applied. The relation between "micro"- and "macro"-propositions, e.g., between the individual's and the community's "marginal propensity to consume" (a distinction emphasized by Haberler, p. 198) depends, of course, on the frequency-distributions involved: in this case, on the income-distribution in particular. As another example: Mitchell observes that the changing dispersion of profits has cyclical effects.8 This suggests that two frequency-moments of profits (dispersion and average) and not only one (average) must be studied. Such studies bridge the gap between "micro"- and "macro"-economics.

There exists, in fact, an awkward gap: that between the theorems which the undergraduate is taught to derive from the rational behavior of single firms and consumers, either in perfect or imperfect markets,

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7 These articles are mostly classified under I: Over-all Discussions and Development of Business-Cycle Theory (54 titles); and II: Dynamic and Econometric Business-Cycle Analysis (65 titles). Of the first group, only 14 were published after 1937; of the second, 361 (See below, Section VII of this review.)

8 "As prosperity approaches its heights... a sharp contrast develops between the business prospects of different enterprises" (p. 56).
and the rather crude and sudden assumptions of the "macro"-discussion on, say, "investments as a whole." How is the formula "investment are high when profits are high" linked with the formula "a firm employs as many machines as would make the value of their marginal product equal to the marginal expense"? (To use the last formula makes investments depend, *inter alia*, on the ratio of machine prices to wage rates.)

Or: How is the rise of costs during, but not before, the latter part of the boom (Mitchell, p. 50) connected with the production and cost functions of firms? If such links are not established, the theory of the firm as taught in colleges may degenerate into a "mark of a gentleman's education" like the fencing and archery taught to would-be mandarins. Yet this need not happen: rationality of firms' behavior is probably not a bad first approximation and can be well utilized in realistic analyses of the economy as a whole.

**III**

To say that the bulk of the volume, or of the discussion it reflects, is on macro-statics does not mean that the book or the discussion is useless. New equilibrium values corresponding to changed conditions, including changed policies, are sometimes reached so quickly that useful policy can sometimes be performed without paying much attention to the adjustment process. Thus, considerable practical knowledge can be derived from a relatively simple theory.

Unfortunately, the usefulness of macro-statics has been lowered by the crudity of our tools. We are spending too much time on misunderstandings, and we misunderstand each other because we talk carelessly.

That our concepts are not unambiguous is well known from the savings-investment controversy (of which the volume contains top specimens in the articles by Ohlin, 1937, Friedrich Lutz, 1938, and Lerner). Had there been more attempts to measure the quantities under discussion economists might have found themselves compelled to more precision.

Williams's plea to give a systematic place to "monetary" versus "realized" savings—both measurable—is a case in point (although his distinction between "oversaving" and "underinvestment" is again a puzzle). Another case in point is the Robertsonian "day." If it had 24 hours, the difference between the values of any variable taken "yesterday" and "today" would be trifling except on a Black Friday; and it is not too meticulous to ask just how long, approximately, is the lag in question. It is, in fact, the lag of spending habits behind changing incomes that matters: an empirically given amount of time, not an arbitrarily chosen unit of time-measurement.⁸⁸ As an alternative, the

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⁸⁸ Mr. Robertson would probably agree with the second, but perhaps not the first, part of this sentence. See his *Essays in Monetary Theory* (London, 1940), p. 83.
thorists may have in mind a relation, with or without time-lag, between a variable and the rate of change, per arbitrary unit of time, of another variable. (Examples of such relations will be given in the next section.)

These doubts of a sympathetic reader are given here merely to suggest that some current concepts and assumptions could well bear a fresh and unambiguous restatement.

The volume contains a well-known article by Oscar Lange, 1939. It expounds the relevant behavior relations (economists’ “schedules”) with a degree of clarity which other static articles of the volume have attained only in regard to certain accounting identities such as are specified in articles by Ohlin, Lerner, and Lutz. By counting relations of both kinds, it can be seen whether one has really explained what determines the variables (if there are as many of them as there are independent relations); or only a half-theory, like the half-scissors of Marshall’s simile (if the variables are more numerous than independent relations); or, thirdly, whether one has perhaps contradicted oneself (if there are more variables than there are independent relations). People afraid of symbols can describe each relation in words like “consumption depends on income,” then count all such sentences and thus reach those rather important results. People who do not despise God’s gift of equations can easily get even more interesting results. Hicks (in an article already quoted and similar to Lange’s in scope) has used such a static system to study the effect of changing interest rate; one could similarly study the complete effect of (much discussed, rarely clarified) cuts of wage rates.⁹

IV

Yet, even the clearest static theories usually have one misty spot. They assume equilibrium between, say, the supply of and demand for a single commodity; or between the value of all goods produced (another word for national income) and the value of all goods demanded. This implies unchanged inventories. This assumption tempts one to use the same symbols for supply and for demand, neglecting to state the assumption itself as a separate relation. Actually the relation is seldom valid. Under the names of inflationary (or deflationary) gaps we have recently familiarized ourselves with aggregate-demand-supply differences of spectacular dimensions. These differences show up in the depletion (or rise) of inventories and later cause a change in prices and in production.

⁹The technique, known as “differentiating implicit functions” is useful in economics because the functions (“schedules”) involved are seldom known except for some general features, like the signs of derivatives. Hicks, *Econometrica*, Vol. 5, p. 157, footnote. See also my article on “Wicksell’s Two Interest Rates” (*Social Research*, Vol. 8, No. 4 [Nov., 1941]) for the use of an elementary technique which assumes linear relations.
The existence and the dynamic effects of excess demand or supply were well known to the founders of modern market theory such as Walras and Marshall. To explain the reaching of market equilibrium, they referred to the fact that excess demand (or supply) caused a rise (or fall) in price: a relation between a variable and the rate of change of another variable. Together with the two static relations (demand and supply schedules), this dynamic one constituted the market theory of a single commodity. Three and not two relations are, in fact, necessary to explain the movement (of which the convergence to equilibrium is one possibility) of the three variables, supply, demand, and price. This did not easily fit the two-dimensional blackboards of our classroom and was consequently forgotten. Yet, a market theory consisting of the three propositions “demand depends on price,” “supply depends on price,” “excess supply makes the price fall,” is both clearer and nearer the truth than a market theory in which the third (dynamic) sentence is replaced, without further explanation, by “demand equals supply.” Strictly, the latter sentence is only correct if the price falls and the consequent adjustment of demand and supply (hence, of inventories) is infinitely rapid: a special, limiting case. The suppressed and replaced sentence helps to describe just this process of adjustment.

National money income which, for the theory of a single commodity, is taken as an exogenous cause of “shifts of demand schedule,” becomes an endogenous variable (i.e., one to be explained) in problems of employment of national resources. National money income is the sum of all money incomes earned, and is therefore another word for the money value of total supply. Again, the equation “total supply = total demand” is only approximately true; quite unlike the equation “total supply = total income,” which is an accounting identity. The total demand (for consumption goods plus investment goods) is therefore not identically equal to total income. To be sure, no harm arises from thinking otherwise, as long as equilibrium is assumed to have been reached somehow: no different consequences arise from the proposition “supply is assumed to equal demand” than from the proposition “supply is another word for demand.” But the confusion shows up as soon as one approaches the facts, the changing inventories, changing prices. One has then to discard the “supply = demand” assumption, and to substitute for it, say, “supply = demand plus a random quan-

Footnotes:
40 Macro-theories of unemployed resources often choose to neglect prices and to discuss the deflationary gap in terms of deflation of physical supply only. We have assumed this simplistic attitude in the rest of the paragraph merely for brevity's sake.
tity," the latter quantity being an accidental (and exogenous) failure of producers to adjust inventories; or going further, one may substitute for it a dynamic relation such as "excess supply slows down supply" (possibly including again, in addition, a random quantity). The savings-investment controversy would not have been so tiresome and inconclusive if, at the time of the controversy, the contributors to the volume under review (this includes Messrs. Lutz, Lerner and Lange) had made clear that the proposition "money value of aggregate demand = money value of aggregate supply," far from being an accounting identity, was not even true, except as a limiting case—viz., the case of infinitely quick adjustments—of some more general dynamic statement. Professor Samuelson’s dynamic restatement, on somewhat similar lines,39 of the Keynes-Hicks-Lange system might well have deserved inclusion in the volume and would supplement Ezekiel’s discussion of the cobweb, a form of dynamics of single commodity markets. By facing dynamic facts, full understanding is reached of the nature of the static model.

V

In the volume under review, two or three simplified dynamic models (involving an equal number of relations and endogenous variables) and half-models (e.g., a single relation in two or more endogenous variables) are discussed articulately: such are the works of Ezekiel, Samuelson and J. M. Clark. They do not claim to explain the business cycle. Authors who have tried to do so faced a more difficult problem, yet applied more deficient tools. The criticism which Tinbergen in his reprinted article makes of most business-cycle theories applies to his neighbors in the volume: Schumpeter, Mitchell, Hawtrey. Are they not, in Tinbergen’s words, “telling stories, not making theory”? The relations they have in mind may be sufficient to explain the cycles, but this is difficult to check because the system of relations is not clearly stated. They may be too numerous, or there may be not enough of them. Even if there are just as many relations as variables, the relations may or may not be able to explain turning points, i.e., periodic movements instead of non-periodical “cumulative processes” or of non-periodical convergence toward equilibrium. The method chosen is to tell how one situation breeds another, and many striking facts are observed and plausibly interpreted from step to step. But the reader, aware of the many simultaneous, entangled threads of economic causation, is left to puzzle why, at any juncture of the story, one particular thread is chosen and not any of the others. One is merely made to feel, with Tony Lumpkins’s genteel but vague friend, that there is some “concatenation accordingly.”

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Tinbergen's methodological article does therefore fulfill an important function in the volume. Unfortunately, the article itself does not illustrate Tinbergen's economics. Specific economic relations had to be treated in that article cursorily and formally. It would be useful to acquaint students with Tinbergen's actual hypotheses, and to provoke criticisms. For example, they might ask (as we did above, in Section II) for a fuller use of the results provided by the theory of single firms; or they might suggest getting at the economic causation of some of the trends which Tinbergen imposes from outside—e.g., reduce some trends to the fact that resources or markets approach exhaustion.

VI

As mentioned earlier, empirical tests or measurements appear in the volume rather casually. Kondratieff's statistical-historical article on long waves is an exception. This is an empirical discovery, with little pretense of a theory. The author forcefully rejects exogenous explanations (inventions, wars, new markets, increase of gold stocks are, to him, themselves caused by the economic conditions); but he does not try to give an endogenous one.

The great merit of Tinbergen's approach is, of course, the knitting together of theory and measurements. His tests by fact are indeed necessary since the number of possible, logically watertight theories is infinite. This is not to say that the tests are sufficient: as in all science, we can only narrow down the set of possible hypotheses by rejecting some of them; we can never be sure that we have hit the uniquely true one. But is it not important enough that, for example, Tinbergen (p. 81) has shown the "acceleration principle," so attractive to theorists, to be of doubtful account in actual fact?

Tinbergen estimates, separately for each economic relation, the size of parameters such as "marginal propensities," price-elasticities of demand, time-lags, etc. The size of those parameters makes the system periodic (with wave frequency of plausible size), or otherwise. With the exception of time-lags, the parameters are estimated by the partial regression coefficients (familiar to statistical workers) of the single relations. The student will, however, be misled by Tinbergen's statement, now five years old, that "Various pitfalls in this field have recently been discussed... fairly completely... it is now more useful to apply the method to concrete cases" (p. 81). Recent researches have shown that estimates of parameters, obtained by separately fitting each of the theoretical equations to the data, are, in general, biased estimates if the data are of non-experimental origin. This is the economist's case. His data are produced by the interplay of simultaneously valid relations. He does not have any controlled variables. Accordingly, other
more general and more suitable methods are being developed;¹³ though in certain cases, especially where lags and exogenous variables are involved, the old method may also lead to correct results.

VII

Whether macro- or micro-economics; statics or dynamics; purely hypothetical or empirically tested, no economic theory—however interesting or true—is really useful if it does not help policy. To nip depressions in the bud, is it necessary to know why they arise? We know that a given total demand would guarantee full employment of resources—if not immediately, then after some reasonable time needed for adjustments. Let us, then, keep total demand at the desired level, correcting it upward or downward whenever it slips down or up. This is the policy of the steering wheel.¹⁴ It dispenses with both theories and measurements of business cycles. All that is necessary is to watch current (not past) facts pertinent to employment, and to act quickly. It is not necessary to know the causes of the trouble or of its alleged periodicity. If the room is too hot we open the window though the trouble may have been caused by the radiator, not the window; nor need we ask ourselves whether the room warms up and cools off periodically, and why. To the practical man, this short-cut is precious. It is interesting to note¹⁵ how, since the publication of the General Theory of Employment by a statesman-economist, the output of articles on business cycles proper has slowed down. Except for the finishing of a few monuments started before the day of revelation, and excepting the work of harmless econometric drudges, victims of despised "scientism," the professional interest shifted to macro-statics!

It is not too conceited to call this shift a boon to mankind, involving as it did a change in the attitude of administrators and, though much slower, a change in public opinion. Certain tenets of fiscal and monetary policy, rational in their time but mere magic taboos under changed circumstances, are giving place to new rules, appropriate to higher productivity levels. There is disagreement about details only: about the way to circumvent this or that prejudice, or the feasibility of dispelling it; about the distribution of benefits of the policy between the various classes; and about the probability that the steering wheel will have to


¹⁵See above footnote 7 for some figures.
be resorted to more often or less often. Robertson’s article, 1940, classifies the tastes or fears at the base of those disagreements; articles of Williams, 1941, and of J. M. Clark, 1939, are examples of understanding and weighing the difficulties. The principle itself is, if possible, even more generally accepted now than it was five years ago: witness the current discussion of post-war employment.

We do not think, however, that the general acceptance of the (static) principle of effective demand, and of the steering wheel policies implied in it, makes business cycles theories, economic dynamics useless. Cars must not only be steered, but also repaired and built. A car must not be built to swing wildly and to require exasperated steering at every turn or slope of the road. A system able to absorb or “dampen” external impacts is more “stable” than one whose “damping-ratio” is small (Tinbergen, p. 79). That ratio is a well-defined quantity. (It is, roughly speaking, another way of measuring the speed with which equilibrium is approached.) It depends, of course, on the structural characteristics of the system, i.e., on its parameters such as the various elasticities, lags, reaction velocities, etc. Would a lowering of the elasticity of labor supply (i.e., a higher flexibility of wages) make the system more stable or less stable, and by how much? What effects on the stability of the system have the current regulations on bank reserves? on dealing in securities? on governmental storage of agricultural commodities? These are problems in comparative dynamics, just as the effect of a changed consumption propensity, or of changed government spending, on the equilibrium income, are problems in comparative statics. The latter studies the factors affecting the equilibrium values; the former reveals the factors affecting the speed with which equilibrium values are reached. Both kinds of factors can be instruments of a policy that aims at high and stable income for the nation.

To answer the problems of dynamics, one has to go into the past and learn from previous fluctuations: one has to have business-cycle theories. In addition, one has to study the past even if one merely wants to “steer the wheel”: to avoid a drunken course of the car, one has to estimate in advance the presumable effect of various external, or “spontaneous” changes in the structure of production or demand (Neisser, 1934), or the effect of, say, profits on investments. But it is erroneous to think that these relations can be measured in isolation (see end of Section VI). Therefore, even if applied statics should be the only aim, the necessary empirical study of economic relations would imply the study of the dynamic system of the economy as a whole. If, however, in addition to steering the wheel of the system we want the system to have a stable structure, we need the knowledge of what makes systems
stable or sharply fluctuating. Hence, in both cases, theory of business cycles is needed.

VIII

Economic theories are made to explain what determines quantities such as prices, or employment, or interest rates at any given time. Any economic theory therefore involves relations between quantities. If economic policy applies theory it uses these relations: it tries to give some of the quantities, such as the national income, desired value by influencing other, more directly controllable quantities, such as tax rates. A theory involving false relationships cannot help, and may harm, policy. A theory that consists of vaguely stated relationships can help only if the relationships meant were true and, by an accident, happened to be understood as they were meant; or if the relationships meant were false but, by an accident, have been misunderstood in an appropriate way. More useful than to rely on such accidents is to have theories as true as possible, and to formulate them clearly.

To be true, a theory must be logically, or internally, consistent and must not contradict facts. (Although necessary, these conditions are of course not sufficient.) A theory about the determination of quantities can in general be expressed by a system of relations such as equations or inequalities, and the implications of such a system can be studied by rules of mathematics. If, in addition to being internally consistent, a theory about quantities wishes to claim consistency with facts, measurements must be made. In economics, measurements must in general be statistical estimates because numerous factors exist that cannot and need not be identified separately and that combine themselves into “random” influences. Economic propositions (except for definitions or identities) are in general statistical, i.e., they state that certain occurrences are more probable than others. An economic theory is in agreement with facts if it does not assign a high probability to occurrences which, in reality, prove to happen but rarely.16

Ill-defined concepts, relations, and systems of relations (called theories) can be more easily extirpated or improved, and the logical consistency and empirical reliability of propositions about quantities are more easily tested if mathematical tools are used. Occidental humanity has developed mathematics for those very purposes.

Economics is far flung and borders on law and ethics, psychology and history, on human biology and on technology. To reason by methods other than mathematical ones and to ascertain facts without sta-

tistics is legitimate or necessary in some of those fields or nearer their borders. Economic theory is not the only and perhaps not the most useful part of economics. Propositions of legal economics about the existing institutions, and statements of sociology or political science as to how institutions change, may be more reliable, and hence more useful, than the essentially quantitative propositions of economic theory and economic statistics about man's probable behavior in matters of supply and demand of goods and services. Nevertheless, propositions of the latter kind have been shown to possess some moderate degree of reliability, and this is why any sort of economic theory can exist at all. Why, then, neglect the tools appropriate to quantitative relations and thus diminish whatever usefulness economic theory may have? There is nothing inhuman about stating that, in given conditions, men behave in moderately persistent ways; there is nothing frivolous in counting men and measuring their ways. True, Providence did punish King David for taking a census: but that was a long time ago.

A giant can do with a hammer the work of a steamhammer. Ricardo and Keynes, using numerical examples and the short-cuts of powerful intuition, have achieved great things; but I doubt that a third name (Menger? Marx?) could be added to these. Marshall probably arrived at, and most certainly tested, his conclusions by mathematics. By hiding his tools in appendices, Marshall bowed to an old belief, a self-deception, I think: that in economics clear reasoning can be replaced by delightfully Ciceronian discourse and that the resulting agreement of minds is due to conviction rather than respect. Robertson's (1938, pp. 315, 312) purpose is "to give an account of events in language as nearly as possible approaching that of Reading without Tears." . . . "Latterly several helpful attempts have been made to express precisely in mathematical terms some of the points at issue. Has a stage now been reached when it is possible to sum the whole position up broadly in more ordinary language, indicating in a general way what departments of the whole tangled controversy seem to be primarily concerned with words and methods, and what with more substantial issues? I do not know, but I should like . . . to make the attempt."

As one who has perused Readings in Business Cycle Theory I must confess: this was not reading without tears. It was strenuous reading. If we study economics to remember the names of authors, their characteristic terms and phrases, conflicts and misunderstandings, and perhaps the social and psychological setting of those conflicts, a volume like the present one is both pleasant and useful: an admirable art gallery. But if economics is not about economists and their words and feelings but about how to explain or influence facts, then to extract economic knowledge from the articles of the volume is a most difficult
task. A giant’s disciple says (Lerner, p. 164): “Mr. Keynes’ presentation [of the multiplier concept] cannot escape the suspicion of the lack of clarity.” In the course of our present article we had cause to complain similarly of other authors. To read about triangles yet shed no tears, give us Euclid, not Cicero.

Precise language would make reading and writing slower, no doubt. Yet in the long run time would be saved. Do we not (and our students) read and write too hastily, anyhow? Do we not, just because of that haste, read and write too much? Economists praise the fruits of patience: the bee in the seal of the Royal Economic Society is there to buzz the glory of “roundabout processes.” Even non-economists know that to get along in England or in the United States it pays to learn some modest English. Our students begin to realize that, to master quantitative relationships that make up economic theory, it pays to learn some modest mathematics. The econometricians may not have been such harmless drudges after all.

The Readings reflect excellently the discussion of the last twenty years. The discussion was moved by momentous events and has achieved important results. The results were obtained by intuition rather than clear thought. The problems still unsolved are naturally the more complicated ones. Nevertheless, no better tools may perhaps be needed for their solution if—but only if!—the coming generation again produces a genius of intuition. But what if it does not?