

Bulletin 45
1940

CRITIQUES OF RESEARCH
IN THE SOCIAL SCIENCES: II

'An Appraisal of Frederick C. Mills'
The Behavior of Prices

BY

RAYMOND T. BYE

SOCIAL SCIENCE RESEARCH COUNCIL

230 PARK AVENUE • NEW YORK

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With a rejoinder by Frederick C. Mills,
a panel discussion, and a commentary
by Read Bain

SOCIAL SCIENCE RESEARCH COUNCIL

230 PARK AVENUE • NEW YORK

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The Social Science Research Council was organized in 1923 and formally incorporated in 1924, composed of representatives chosen from the seven constituent societies and from time to time from related disciplines such as law, geography, psychiatry, medicine, and others. It is the purpose of the Council to plan, foster, promote, and develop research in the social field.

CONSTITUENT ORGANIZATIONS

American Anthropological Association

American Economic Association

American Historical Association

American Political Science Association

American Psychological Association

American Sociological Society

American Statistical Association

FOREWORD

THE SOCIAL SCIENCE RESEARCH COUNCIL, in seeking to discover and define its proper role of usefulness for the seven disciplines represented in its membership, is laying increasing emphasis on intellectual leadership toward improving the quality of social science research and in promoting better organization among scholars for carrying out such work. In pursuing the first of these objectives, improvement in the quality of research, the Council in September 1937 set up an Appraisal Committee charged with the task of seeing what might be done toward ascertaining and explicitly setting forth the standards which are currently applied by professional workers in these fields, with a view to their reexamination, reformulation, and more effective use. The Committee embarked on an exploratory program designed "to see whether objective standards can be inductively arrived at for judging research product and for putting such standards to practical use." Such standards, it was postulated, "should be tentative and growing, not rigid and pontifical."

In accordance with the inductive procedure which the Appraisal Committee adopted, it sought to place the responsibility for defining and defending standards of work on social scientists, reflecting as widely as possible current thought and practice in this field of work. The first step was to get a representative group in each of the seven disciplines to list the publications which have been most generally accepted as "outstanding contributions" by American authors since the World War. On the basis of this poll, though not necessarily limiting its considerations to the numerical vote, the

Committee selected a volume for intensive study by some competent individual, who was commissioned to prepare an appraisal report in which he should scrutinize the author's ways of working, with a view to identifying more or less standard procedures, modifications of standard methods, and distinctive innovations; evaluating the appropriateness of such methods to the particular material in hand or objectives sought; and appraising their trustworthiness, fruitfulness, and possible range of application in other studies. In case some years have elapsed since the publication of the research study, it might be possible to present evidence that the distinctive methods of the given worker had, by others following in the pioneer worker's footsteps, been found stimulating and helpful or perchance had led to results which were either sterile or harmful.

With this appraisal essay in hand, the next step was to assemble a small group of highly competent specialists to give intensive consideration to the issues raised by the appraisal essay and by the principal reviewers of the given book. After a full day devoted to such discussion, the stenotyped record of the conference was to be edited for brevity and coherence, summarized, and printed, along with the appraisal, rejoinder, and significant reviews.

The first of these undertakings to be completed in accordance with this procedure was an appraisal of Thomas and Znaniecki's *The Polish Peasant in Europe and America*. The product appeared as a bulletin of the Social Science Research Council on July 30, 1939. In the *Foreword* to that publication, President Edmund E. Day, first chairman of the Appraisal Committee, called attention to the twofold problem that confronts research. We need to allow the lively curiosity of the human mind to range freely enough so that we may be assured of the widest range of observation and investiga-

tion of phenomena which may ultimately be found to have social significance. But at the same time, we must find ways of reducing the raw data thus amassed to manageable dimensions and of so manipulating them that their significant characteristics and interrelationships may be discovered, effectively set forth, and, so far as possible, reduced to quantitative expression.

For many large areas of knowledge, he said, answers to questions as to the bearings of new information on existent knowledge and questions as to how or whether a particular body of new material adds to what we already know,

can be attempted only by those who belong to the select company of the expert. . . . Under these circumstances what is to prevent the development of a self-contained and self-recruiting cult perpetuating inquiries and reports of no substantial social value, either in promise for the future or relevance for the present? . . . The solution of the problem raised by this question would appear to lie in the acceptance by the experts of two fundamental propositions:

First, inquiry in any field should recognize the necessity of continuous appraisal of the nature of the knowledge being acquired. Does it consist of the record of observed uniques the reappearance of which cannot be anticipated, or does it at least suggest generalizations which may be expected to illuminate and to some extent make more manageable later experience under similar circumstances? Is one of the primary purposes of inquiry to reach such generalizations, or is this a matter of indifference? Questions such as these should be answered by those in the particular field of investigation.

Second, the experts are under obligation to communicate to the public, in language the educated lay public can understand, the answers to questions such as those just stated. The lay public cannot be expected to follow the intricacies of technique necessary for practically all fields of specialized scholarly and scientific endeavor, but it can be led to understand the ends which are sought by the workers in a given field, as well as the nature of the results that are being obtained. The public is entitled to know on what basis the experts distinguish between success and failure, between the good and the bad, in their own work.

But if the public is to be enlightened as to the basis on which experts distinguish between success and failure in the task of giving a scientifically rigorous quality rather than

mere irresponsible ingenuity to the product of research activity—which today makes increasing demands upon our time and our funds—the so-called experts must come to reasonable agreement, or at least moderately limited and specifically defined disagreements, as to their own ways of working and the criteria which they find it useful and necessary for each to apply to the work of others if he is to use their product in his own subsequent endeavors.

At the meetings of the Social Science Research Council at Skytop in September 1937, Dr. Day threw fairly at the feet of his colleagues the challenge whether they had any clearly defined idea of what social *science* really is—or should we say, would be?—and whether they were making any sustained, systematic, or comprehensive effort to find out. While we do not wish to claim more than is justified for the exploratory ventures being conducted by the Appraisal Committee, they at least represent a succession of “trial borings” in different parts of the field, designed to shed light on the different ways which scholars have devised in seeking to distil truth from the observed phenomena of human beings associated in society.

In our initial venture—in the field of sociology—the material concerning the Polish peasant was so subjective, imponderable, and evanescent that quantitative procedures were essentially inapplicable. Indeed, the problems of deciding even whether particular material was to be admitted in evidence, or how it was to be classified, evaluated, and generalized emphasized the importance of previous technical training, wide experience, and personal aptitudes on the part of the preparator and clinical researcher rather than the usefulness and reliability of teachable techniques or mechanical devices for manipulating the material once it had been amassed. To the Committee, it seemed that the title *Insight*

and Proof would have been an illuminating and proper descriptive title of that volume, serving to focus attention on one distinctive aspect of social studies. But this designation was not found acceptable by the author and by some of the participants in the conference. Hence it was dropped.

Even had we attempted to discover some apt characterization of the issues raised and discussed in each of our successive explorations, we should have been hard put to it to phrase the label with which our second study should be tagged. The book selected was Mills' *Behavior of Prices*—rating high in the poll of both economists and statisticians. In taking it for study, therefore, the Committee was mindful of the original and continuing interest of the Council in interdisciplinary studies. At the same time, it raised questions of procedure sharply in contrast to those raised in the study of *The Polish Peasant*.

The data with which Mr. Mills was dealing are among the most concrete and superficially precise with which the student of social phenomena is ordinarily confronted. Wholesale prices are quoted in cents or fractions of cents down even to one-hundredths. The layman is constantly reading them from his newspaper, trade journal, call board, or ticker tape and being guided by them in the conduct of his business affairs. But to manipulate such data in sufficient mass and with sufficient precision to answer the questions which the social scientist considers significant and even vital presents grave difficulties. Quotations of individual prices have long ago been sublimated into the unreal form of indexes, and these index figures have been standardized into certain patterns of presentation. As Thomas and Znaniecki turned to psychology for the checking, verification, or refutation of their concepts of "social becoming," so Mills turned to the techniques of statistics—enriched by his own skill and in-

genuity—to answer certain questions of “price behavior” in which, as an economist, he was deeply concerned.

How far the result was a significant addition to human knowledge or merely to the burden of scholarship is a question on which only trained experts can have an intelligible opinion, and, at the frontiers of knowledge, even the best of doctors disagree. Such disagreement, however, should fall within certain fairly well defined limits. The disagreement of doctor with doctor is something quite different from the divergence of thought between doctor and layman. And effort may be directed toward the clarification of differences between professionals and further analysis of the bases on which they rest, with a view toward maximizing the area of agreement as rapidly as possible.

This appraisal study would seem clearly to indicate (if the conferees are a fair sample) the agreement of economists and statisticians on the complementary relationship of inductive and deductive method. Perhaps also it reveals substantial agreement as to the impossibility of making any mere manipulation of empirical data without benefit of antecedent theory yield a strictly inductive generalization. At the same time, there was refusal to admit that useful theory must be limited to propositions amenable to quantification. Likewise, dissent was registered against the proposition that statistics has no role other than the ancillary task of verification, refutation, or some less conclusive type of illumination of hypotheses originated by some different order of thinkers known as “theorists.”

While a great deal of time was consumed in getting the conferees aligned in the positions which they in fact accept or assert with reference to the interplay of deductive formulation and inductive buttressing, refutation, and reformulation of theory, substantial consensus resulted. Would it not be possible to set forth this position in the space of a paragraph

as something accepted as "known" by a large majority of workers? Would this not afford a definite base from which the divergence of the minority could be measured or at least the character of this divergence clearly perceived?

Had this been done prior to *The Behavior of Prices* conference or in the first few minutes of discussion, it would probably have been easier to focus attention on whether Mills started from a body of knowledge generally regarded as established, to the effect that prices are not discrete phenomena but are enmeshed in a comprehensive set of systemic relationships. Presumably, nothing more than lay observation would be necessary as an adequate foundation for such a hypothesis and it would then have been possible to proceed with mutual understanding to examine the techniques which Mills employed to discover the nature of certain of these relationships. I do not find in the transcript any evidence of doubt on the part of any of the conferees that the method selected by Mills and elaborated by his own inventive genius yielded results which are accepted as making definite accretions to the existing store of knowledge about price relationships. On the other hand, no one seemed to find it desirable or possible to summarize precisely the nature and extent of the new knowledge won through this arduous undertaking.

One is moved to wonder, therefore, whether the progress of knowledge in the field of economics (and in other social sciences as well) would be promoted by the attempt to formulate more sharply the precise tasks undertaken in the various research projects and the claims made for results, to be submitted to some formally constituted agency within the profession for the purpose of ascertaining what consensus may be secured among at least such specialized groups as are working in a particular field. Are the materials and the methods used in this social science field so different in character from those of the natural sciences that it is not possible to arrive

at any reasonable consensus among workers on any point? Is some such formulation necessary if the character, point of departure, and bearing of new hypotheses are to be clearly recognized and evidence in support of them as effectively presented as in the natural sciences?

This conference illustrates with painful clarity the fact that the difficulty of arriving at consensus with reference to the validity and value of any original contribution to our store of knowledge is involved with the looseness of our current practice with reference to the definition of concepts and the verbal symbols that we attach to them. It was hardly possible for the several participants to make sure that they were talking about the same thing when they were discussing price "systems," "behavior," "stability," "institutionalism," "normal," or "equilibrium." How complacent are economists or how complacent should they be permitted to be about going on in the present rugged individualism in which each writer uses these terms in his own particular and often unclarified meaning? Is consensus as to the meaning of such verbal symbols possible? Would it be desirable? Or would "standard usage" in the social sciences be a bar to creative genius in this field of endeavor? Is ours a literary game of coining new words and creating new meanings for familiar words with such speed and ingenuity as to baffle intellectual pursuit?

In the quotation from Edmund Day we are warned against the danger of becoming a self-contained cult. If our practices tend to shrink the bounds of that cult to narrower and narrower limits till the lone worker becomes a cult unto himself, the danger is great indeed. Nor is this a purely imaginary danger today. If the growth of knowledge is to be as healthy and vigorous as possible, the individual mind must be allowed to range as widely and freely as it may be moved to do by its quest for truth; but if it is to make these explora-

tions fruitful for society, the product of such untrammelled thinking must be put in a form that is intelligible to one's fellows. If we wish to refer to our fields of study as "disciplines," it is perhaps worth while to consider the nature of the intellectual discipline which such professional groups should exercise on the practices of members in the preparation and issue of "scientific findings" for current circulation by a public who, in the nature of the case, are incapable of measuring "weight and fineness."

In raising here the question of the possible usefulness of some kind of consensus procedure, there is no intimation that the Appraisal Committee contemplates turning its efforts in this direction. It is merely a possibility occurring to an individual after contemplating the work of a particular conference. The Committee has simply been following one line of experimentation, which in time may yield suggestions for other and perhaps more fruitful lines of attack. The course which will be followed (including perhaps abandonment of the whole undertaking) will depend ultimately on the response of the profession as a whole. Opinion is still divided as to the value of the steps thus far taken. It remains to be seen how much attention will be paid by the rank and file of the respective professional groups to the material which is being prepared for them and submitted to them by the Committee. If anything useful is to be accomplished in this direction, the Committee should have the guidance of explicit expressions from those who find these conferences and the ensuing publications stimulating and useful, those who regard them as a waste of time and effort, and those who derive from them or are provoked by them into suggestions of a more fruitful direction of the Committee's efforts.

EDWIN G. NOURSE

November 22, 1939

PUBLICATION OF THE BEHAVIOR OF PRICES

The Behavior of Prices was published by the National Bureau of Economic Research in December 1927. No attempt was made to find a commercial publisher since the National Bureau itself publishes all of its researches. The Behavior of Prices was the eleventh monograph the Bureau had published since its organization January 29, 1920. Since 1927, some thirty additional volumes have been published.

An edition of 2100 copies was printed. The price was \$7.00 per copy. Except for the small number of copies which the Bureau has kept in reserve, the edition was exhausted in 1932 and has not been reprinted. Most of these copies were purchased by libraries and scholars here and abroad. The book has exerted considerable influence upon both the methodology and theoretical point of view of scholars working in this particular field of research. It is probably also true that it has had some effect upon the trend of research in economics outside the price field.

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Part One

A *CRITIQUE*

by

Raymond T. Bye

of

The Behavior of Prices

by

Frederick C. Mills

Part One

The Appraisal

Critique BY RAYMOND T. BYE OF Mills' *The Behavior of Prices*

The Position of Mills' Study in Price Literature. In an economy characterized by specialization and exchange, prices are the center in which most economic phenomena come to a focus. It is no accident, therefore, that prices have occupied the predominant position in economic analysis ever since Ricardo. From time to time, there have been protests against this concentration of interest but they have been of no avail. Economists have continued to be engrossed in the price system and its ramifications. The result is an extensive literature developed around this general theme. The present essay will attempt to interpret and appraise the contribution to this literature embodied in Frederick C. Mills' *The Behavior of Prices*.¹ Before proceeding directly to that task, however, it may be well to survey broadly the various kinds of price studies that are extant, in order that the position which Mills' work bears to this whole body of writings may be better understood.

The literature of prices can be grouped, for the most part, into two general classes, deductive and inductive. The deductive studies can be further broken down into those which are concerned with the analysis of individual prices taken

¹Frederick C. Mills, *The Behavior of Prices*, The National Bureau of Economic Research, New York, 1927. 598 pp.

singly, those which attempt to describe the system of prices as an interdependent whole, and those which are concerned with the purchasing power of money as reflected in the general level of prices. The inductive studies parallel these subdivisions in a general way, as will presently appear. There is a third kind of studies which cuts across this general classification. It is concerned with practical price policies and makes use of both a priori and empirical data.

The individual price approach dominates the neoclassical school of economists and finds its best expression in the writings of Alfred Marshall and his disciples; the interdependent approach is embodied in the writings of the general equilibrium school, which derives its inspiration from such men as Walras, Cassel, and Wicksell. The chief problem which engages the attention of Marshallian economics is the theory of value, the objective of which is to ascertain the determinants of particular prices, the analysis running in terms of demand and supply, with the familiar apparatus of curves, margins, and equilibrium. Pushed a little further, the explanation thus developed merges into the theory of distribution, in which the costs of value theory become distributive shares, which are again explained by the mechanics of demand and supply and marginal comparisons. The general equilibrium school objects to this individual price approach on the ground that the price of a good is not determined in isolation from other prices but in combination therewith. Hence, the price system must be conceived as an interrelated whole, each price being dependent upon all the others. The distributive shares, being the prices of productive agents, are part of this interdependent system. Because of the complexity of these relationships, this type of analysis must take the form of simultaneous equations in which prices are represented as the unknowns for which a solution is to be found,

and the choices of consumers, the quantities of the agents of production, and the technique of industry (which determines the proportions in which those agents shall be combined), constitute the known, determining, factors. Although the two schools thus stand somewhat in opposition to each other, they are not mutually exclusive. The equilibrium school studies the whole while the Marshallian school studies the parts. Both positions are logically tenable and can be reconciled. Therefore, they are properly to be regarded as complementary rather than contradictory. It is noteworthy that both schools emphasize discovery of the determinants of prices as the ultimate objective of their analysis. Knowledge of these determinants is important for any application of economic theory to the purposes of price control, and, indeed, for control of the whole economic system for which prices are the guide.

Deductive analysis of the general level of prices is best represented by the writings of Irving Fisher and the quantity theorists. It is concerned neither with the determinants of particular prices nor with the interrelations among prices, but with the relation between monetary factors and the average level of all prices. The determinants of the price level are traced, through the equation of exchange, to the underlying factors which affect the monetary circulation on the one hand, and the transactions of exchange on the other. Proceeding from the analysis of this problem, there has arisen the idea of stabilizing the general price level through monetary devices, and various proposals have been formulated looking to that end. Here, again, the importance of the theoretical analysis consists in the applications which can be made of it for purposes of control.

Alongside this extensive literature of deductive price theory, there has been growing, especially in recent years,

a large body of writings devoted to the inductive and quantitative study of prices.² These writings can be subdivided into three broad types, which roughly parallel the types of deductive studies already outlined: (1) empirical analysis of the factors of demand and supply for particular commodities; (2) analysis of the relationships prevailing among all prices taken collectively; (3) measurement of the general level of prices. For the present purpose, it will be convenient to defer discussion of the second of these groups until after the first and third have been described briefly.

Those writers who are devoting their efforts to an empirical analysis of the factors of demand and supply affecting the prices of particular commodities have based their work directly on the deductive analysis of the neoclassical school. One group of investigators in this field has attempted to employ data of prices, production, and consumption for the construction of statistical demand and supply curves approximating those of the theory of value. Moore, Schultz, Pigou and Leontief are among those who have done pioneer work of this character. Although hampered by the fact that their observations are necessarily spread over periods of time in which schedules of demand and supply may have changed, they have achieved results which are both interesting and suggestive. The United States Department of Agriculture is making practical use of such studies in connection with its crop control program. A second group of investigators in this field has been occupied, not with the construction of actual demand and supply schedules, but with an intensive study of the price-making influences at work in the case of particular commodities. Here the object is to examine the

² Mills has given an account of this literature, much more detailed than the brief description here following, in his article, "Price Data and Problems of Price Research," *Econometrica*, Vol. 4: 289-309, Oct. 1936. His classification of inductive price studies differs somewhat from the one employed here.

characteristics of the market for the commodity concerned and the nature of the conditions surrounding its production and consumption. Examples of this are Wright's study of sugar in relation to the tariff ³ and Cassel's recent study of fluid milk prices.⁴ Finally, the exigencies of rate regulation and the price-fixing activities of the American and other governments during the World War have produced a considerable body of data relating to costs and other aspects of prices in certain branches of industry, but the use and interpretation of such data have not generally been of a very scientific character. Neither has it always been guided by economic theory. Nevertheless, it is an attempt to get at the influences of demand and supply operating in the case of given commodities. Therefore it can be classed as paralleling, on the empirical plane, the Marshallian analysis on the abstract plane.

The statistical measurement of price levels is likewise in harmony with the work of deductive theorists. Here the objective is to construct index numbers which will show quantitatively what changes may have occurred in the general level of prices during a given time period. Thus, they give mathematical precision to the concept of the price level developed by a priori thinking. The scholarly work of Fisher and Bowley, both deductive theorists, is representative of this kind of study. Index numbers of wholesale commodity prices are now published periodically in many countries. Sometimes they are official figures published by the government; in other cases, they come from private sources. The recent historical studies of the International Committee on Price History have pushed such index numbers back many decades in the past, so that there are now becoming available fairly

³ Philip G. Wright, *Sugar in Relation to the Tariff*, New York, 1924.

⁴ John M. Cassel, *A Study of Fluid Milk Prices*, New York, 1937.

good measures of commodity price level changes over a long period of years. At first confined largely to wholesale commodity prices, index numbers are now being developed for other price series, such as retail commodity prices, living costs, and wages; while at least one investigator (Snyder) has attempted to construct a more general index, which would be a better measure of the purchasing power of money than could be obtained from prices for any particular category of goods. Index numbers are used not only to measure changes in prices, but, by suitable correction, to give some indication of "real" quantities. For example, figures of money income, corrected by a general index of commodity prices, afford a rough measure of changes in real income, and figures of money wages, similarly corrected by cost of living indexes, show the movement of real wages. Some writers have made interesting efforts to test the quantity theory of money by the use of index numbers. Cassel's work along this line is well known; the studies of Warren and Pearson, which purported to show the dependence of world prices upon gold, constitute an interesting chapter in recent American history because of the influence they exerted upon the monetary policy of the American government.

Inductive studies of the second type mentioned above, dealing with the relationships among all prices taken together, are an outgrowth of problems met with in constructing index numbers. Recognition of the distortion in a general index which may be caused by extreme fluctuations of a few prices led statisticians to weight the prices of different goods according to their relative importance, and the search for suitable criteria for such weighting created problems calling for more detailed analysis of certain aspects of prices. Furthermore, it soon became apparent that index numbers of wholesale commodity prices were inadequate for measur-

ing prices outside of wholesale markets, so that special indexes, such as those of retail prices and of wages, had to be constructed for special problems. Yet it was apparent that these various indexes were not entirely independent of each other. So, the inductive workers were led to recognize what had already been discovered by the general equilibrium theorists, namely, that the price system is not a simple thing which can be adequately comprehended by a single measure, but is a complicated net-work whose representation in quantitative terms calls for a new technique. Mills gives credit for this new approach to Wesley C. Mitchell who described the system of prices to some extent in his first book on business cycles; but its fuller development is the work of Mills himself, in the volume to which this essay is devoted, and to some extent in his subsequent writings. Though Mills' book pays some attention to inductive price analyses of both the other types described above, its major concern is with the price system viewed as an interdependent aggregate and it is here that its chief contribution is to be found. It is distinctive also in that, unlike the other types of empirical price studies which have been described, it does not build upon the foundations of deductive theory. Although the concept of the price system here involved strongly resembles that of the general equilibrium school, it is treated by Mills in a very different way. The reasons for this difference lie partly in the requirements of statistical technique, but for the most part, they are due to the attitude which Mills takes towards economic theory. A correct interpretation of his work, therefore, requires some discussion of his general point of view.

Before proceeding to that discussion, this rather cursory survey of price literature should be rounded out by a brief description of the third general class of price studies which was mentioned above—that is, studies which deal with price

policies. Here an effort is made to get at the actual practices of business enterprises in setting the prices at which they will sell their wares and to trace the effects of these practices upon the functioning of the economy. Hence, the approach is empirical rather than theoretical but some use of deductive theory may be made in interpreting the findings. Studies of the monopoly problem in its price aspects belong in this category. Much of the literature of rate regulation, cited above as an example of the inductive analysis of particular prices, should perhaps be included here. Then there are such recent works as the surveys of corporate price policies made by The Brookings Institution and by Hamilton and his collaborators, Burns' study of the decline of competition, and the discussion of price flexibility and inflexibility which has been agitated by Means and other writers.⁵ In his later writings, Mills himself has shown a marked interest in problems of this kind, especially in the matter of price rigidity; but this interest does not appear in *The Behavior of Prices*. Hence, although this part of his work will be discussed, it will not occupy a dominant place in the present essay, whose major task is to explain and interpret Mills' effort to develop a new method of price analysis. It has already been stated that, for this purpose, it is important to understand the general point of view from which he approached his problem.

Mills' Method of Approach. Although Mills does not appear definitely to have labeled himself such, it is fairly evident that he is a member of the institutional school of economists. This is revealed, not only by the methods which he employs in *The Behavior of Prices*, but more clearly by the confession

⁵ E. G. Nourse and H. B. Drury, *Industrial Price Policies and Economic Progress*, 1938; W. H. Hamilton and others, *Prices and Price Policies*, 1938; A. R. Burns, *The Decline of Competition*, 1936; G. C. Means, *Industrial Prices and Their Relative Inflexibility*, 74th Congress, Senate Document No. 13, 1935.

of faith which he has made in certain of his other writings.⁶ In view of the fact that he is closely associated with Wesley C. Mitchell in the work of the National Bureau of Economic Research, this is rather to be expected. In common with the institutionalists generally, he regards economic theory of the traditional type as of rather limited usefulness. Although he admits, somewhat grudgingly, that

the search for principles relating to the behavior of rational beings confronted by conditions of scarcity, and the exploration, in abstract terms, of the concept of economy have been, and, of course, remain, fair fields of economic inquiry, [he argues that] these studies can never be more than a first stage in the attempt to deal with the actual problems of economic life. The progress of economic science has been retarded by the prevalence of misapprehension as to the place which such studies occupy in the real work economists have to perform.⁷

As he sees it, the neoclassical type of economics has made two mistakes: it has made the choices of individuals its data, instead of the economy as a whole, and it has failed to take account of economic change. He does not believe that such mass phenomena as market prices, wages, and interest rates can be explained satisfactorily by the analysis of individual decisions or responses, for "the whole has properties of an order fundamentally different from those of its parts, and the behavior of the whole is not to be explained in terms of the behavior of the individual elements which make it up."⁸ The second of these criticisms is somewhat different from the usual institutional attack on deductive economics, but in his strictures against the failure of traditional theory to recognize

⁶ See his remarks on "The Theory of Economic Dynamics as Related to Industrial Instability," presented in a round table discussion at the meeting of the American Economic Association in December 1929, published in the *Amer. Econ. Rev.*, supp. to Vol. XX, pp. 30-35, March 1930, and his essay "On the Changing Structure of Economic Life," in *Economic Essays in Honor of Wesley Clair Mitchell*, 1935.

⁷ "On the Changing Structure of Economic Life," *op. cit.*, p. 358.

⁸ *Ibid.*, p. 360.

the importance of change, he is on more familiar ground—the static character of deductive theory has long been a favorite target for the shafts of its opponents. To segregate change in the pound of *ceteris paribus*, Mills declares,

is to remove from every economic problem that element which lies at its very heart. It is to extract the Dane from *Hamlet*. A static economy is conceivable, but the laws which describe its working have no meaning in the world into which economists are born and with which their theories should deal. . . . Abstractions concerning a static economy are of use only if they throw light on the actual problems of an economy in operation.⁹

Here again there is a slight concession to the neoclassicists.

For certain purposes, of course, abstraction from reality in the study of relations existing under fixed conditions is an entirely legitimate procedure. Some degree of abstraction must inevitably be present in any attempt to deal systematically with the tangled processes of a modern economy. But unrefined and excessive abstraction, which forgets that it is abstraction, is not a means to fruitful thinking.¹⁰

Therefore, he concludes that a new approach to the study of economic phenomena is necessary; and he apparently believes that the majority of economists would agree with him in this judgment, for he says,

Probably no economist would deny today the need of a mode of attack better adapted to the study of contemporary conditions than is that provided by the body of theory which had its origin in the conditions and problems of the nineteenth century and in the preconceptions of nineteenth-century thinking.¹¹

What is to take the place of this futile, nineteenth-century abstraction? Mills believes that the economics of the future must avoid the two mistakes just described by focusing its attention, not on the individual elements of economic activity, but on the economy as a whole, and by showing the economy in its processes of continual change.

⁹ *Ibid.*, p. 361.

¹⁰ *Ibid.*

¹¹ *Ibid.*, p. 358.

The true and measurable data of economics, not the hypothetical data concerning the behavior of individual economic men, relate to attributes of a population, of a system as a whole. It is these attributes which are the concern of economics.¹²

By "true and measurable data" Mills presumably means such facts as can be dealt with statistically, and it is by statistical methods that he himself proceeds with his task. By this approach, he hopes there may eventually be evolved

a general theory of economic change, if such may be conceived, [which] would represent an integration, after testing, of all those separate and particular hypotheses to be found as implicit or explicit in the multitudinous forms of economic activity. Whether integration into a complete and consistent system is possible remains to be seen. But approaching these separate hypotheses from the point of view of the economy as a whole and centering the issue about problems of change, one would attempt to give these theories, in combination, the degree of unity the facts possess.¹³

So, it would appear that the body of economic doctrines built up by the long process of evolution which began with Adam Smith and which constitutes the subject matter of economics as embodied in the standard treatises of today is to play a very subordinate role henceforth, while a new economics is to be built up by statistical methods and historical studies.

These quotations from Mills are important because they constitute the key to an understanding of what he has tried to do in *The Behavior of Prices*. *The significance of his work consists in the fact that he has attempted a new approach to the study of the price system differing fundamentally from that of the traditional economists*. Other workers have applied statistical methods to price analysis, but, for the most part, they have sought to apply existing theories to their data, or to give quantitative precision or inductive verification to

¹² *Ibid.*, p. 360.

¹³ "The Theory of Economic Dynamics," p. 34, *loc. cit.* in note 6.

those theories. Mills deliberately avoids building upon neo-classical foundations. Completely ignoring the existing body of doctrines and making no use of its hypotheses or conclusions, he sets out to make a fresh attack on the price system, relying almost exclusively on statistical tools. It is a bold and interesting attempt to apply new methods to the central problem with which economists for several generations have been concerned, in the hope of reaching scientific generalizations of an entirely different character from those which have hitherto been developed. With the exception of Mitchell's exhaustive, life-long analysis of business cycles, it is perhaps the most ambitious, constructive undertaking upon which any institutionalist has thus far embarked.

This broad purpose is to be inferred from Mills' various expressions of his point of view, as quoted above, and from the fact that the methods employed in *The Behavior of Prices* definitely harmonize with those confessions of faith. In writing *The Behavior of Prices*, however, the objectives openly avowed are somewhat more limited in character.

The objectives of the present study are, first, to secure a fuller understanding of the behavior of individual commodity prices and, secondly, to increase our knowledge of the working of the price system and of the interrelations between its component elements. . . .¹⁴

Elsewhere in the book he says,

The immediate objectives of the present study are the development of a method of analysis and the accumulation of a body of facts which may conduce to an understanding of the price system. . . . No attempt has been made . . . to support a specific thesis.¹⁵

From these statements, it is clear that Mills was bent upon studying prices by methods different from those which had hitherto prevailed and that he hoped to derive from such

¹⁴ *The Behavior of Prices*, p. 31.

¹⁵ *Ibid.*, pp. 437-438.

methods useful generalizations concerning the behavior of prices, which generalizations would presumably be different from those developed by the methods of deductive analysis. Here, then, is a challenge to economic theorists, and, at the same time, an answer to the argument which they have advanced that institutional economics consists only of destructive criticism and produces no constructive contributions to economic science. *The Behavior of Prices* was apparently designed as such a contribution.

The study is confined, for the most part, to wholesale commodity prices. This is not because Mills conceives of these as constituting the whole of the price system. He definitely recognizes that retail commodity prices, as well as "the prices of human services, of land, of capital and credit, the prices of business enterprises themselves, constitute elements of the price system, and each of these has unique features."¹⁶ Therefore, the volume is not to be regarded as a complete description of the price system, but only as an analysis of a very important part of it.

In that analysis, Mills has performed a Herculean labor. Using as raw material, price quotations for several hundred commodities over a period of years ranging from 1890 to 1926, he has applied to them a variety of statistical techniques and derived tables which it must have been a prodigious task to construct. No doubt he had considerable assistance in the routine aspects of this work, but even so, the labor of directing and interpreting it was undoubtedly very great. In performing this task, Mills has shown himself to be, not only an indefatigable worker, but a statistician of very great skill. He has exercised great ingenuity in handling his figures in such a way as to make them reveal the characteristics which he wished to investigate. The particular measures employed

¹⁶ *Ibid.*, pp. 213-214.

in the study are on that account of considerable interest to statisticians. The present essay, however, will not attempt to review these techniques in detail nor, for the most part, to criticize or appraise them. That may be left to others more competent¹⁷ for the present writer is not qualified as a statistician. The purpose of this essay is to view the work in its broader aspects as a contribution to the literature of economics. This calls for an appreciation of its use of inductive methods as a means of arriving at generalizations and for an appraisal of the generalizations themselves, but it does not require much technical analysis of its statistical methods.¹⁸

A Topical Outline of THE BEHAVIOR OF PRICES. Before proceeding to analyze *The Behavior of Prices* in greater detail, it may be helpful to give a very general sketch of its contents. The work is divided into four chapters, or parts, whose arrangement and scope are indicated by the following topical outline.¹⁹

Chapter I. Measurable Characteristics of Individual Commodity Prices

- (a) Variations of prices from month to month and from year to year (price variability).
- (b) Secular trends of prices.
- (c) Cyclical movements of prices (cyclical variability).
- (d) Relations between prices and price-determining factors (price flexibility).
- (e) Relations among price characteristics.

¹⁷ Theodore O. Yntema has written a brief review of the book from the statistician's standpoint in the *J. Amer. Statist. Assn.*, 23: 453-455. There is also a criticism of one feature of Mills' statistical technique, by Dorothy C. Bacon, "The Significance of Fixed Base and Link Relatives in Studies of Price Stability—A Comment on *The Behavior of Prices*," in the same journal, 23: 274-281. Bacon's criticism will be discussed later in this essay.

¹⁸ The present writer desires to acknowledge the valuable help which he has received from his colleagues, Raymond T. Bowman and Simon S. Kuznets, in connection with some of the statistical problems raised by Mills' work. If this essay betrays any evidence of statistical competence, the credit is mainly theirs.

¹⁹ The wording and arrangement of this outline differ slightly from Mills' own titles and subtitles, but they do not misrepresent his work.

Chapter II. Regional Differences in Individual Commodity Prices.

- (a) Regional differences in prices.
- (b) Regional differences in price behavior.

Chapter III. The Measurement of Price Instability: Prices Studied in Combination.

- (a) The price system. Price stability and instability.
- (b) Changes in the general level of prices.
- (c) Changes in the dispersion of prices.
- (d) Changes in the order of arrangement of prices (price displacement).
- (e) Characteristics of frequency distributions of prices, with reference to curve types (prices viewed as a "population").

Chapter IV. Measures of Price Behavior in Combination.

- (a) Group characteristics, as shown by frequency distributions, of the measures of individual price variability and trend developed in Chapter I.
- (b) Group characteristics, as shown by frequency distributions, of the measures of individual cyclical price movements developed in Chapter I.

The next four sections of this essay will attempt to describe, in general terms, the kind of measures used and the more important findings reached in each of the four chapters above outlined. This will give the reader some idea of the contents of the volume. He will then be in a position to follow with more understanding the discussion of some of the significant issues raised by Mills' study, to which discussion the remaining portions of the essay will be devoted.

Measurable Characteristics of Individual Commodity Prices. The prices dealt with in *The Behavior of Prices* are nearly always relative prices (or price relatives, as Mills prefers to call them). That is, the price of each commodity is shown, not in the dollars' and cents' quotations of the market, but as a percentage of its price in some year (or month) chosen as a base. Sometimes a fixed base is used, at other times a moving one, in which case the prices are shown as link relatives. The first chapter of the volume is concerned with measuring four types of change in such relative prices,

these types being designated by Mills as *variability, trend, cyclical variability, and flexibility*.

By *price variability* he means changes in the amount (amplitude) and frequency of changes in prices from month to month and from year to year. The amplitude of monthly variations is measured by the mean deviation of prices from the average price for each year; year-to-year changes are measured by the mean deviation of prices from their annual average, either in the prewar period of 1890-1913, or in the longer period of 1890-1924. Frequency of price changes is measured by an index which, in effect, shows the number of monthly changes in the price of a commodity during a given time period as a percentage of the number of months covered by the series to which the measure refers; so that, if a change occurred every month, the index would be 1.00, but if there was a change in only half of the months recorded, it would be 0.50. Such measures of amplitude and frequency, for both month-to-month and year-to-year changes, are given for more than two hundred commodities for the years 1890-1925, inclusive. In order to show how these changes compare with those in other types of economic phenomena, comparisons are made with the variability of six general economic series, namely: the yield of railroad bonds, the American Telephone and Telegraph Company's index of general business, the Dow-Jones index of industrial stock prices, pig iron production, the discount rate on call loans. As is to be expected, considerable differences are found in both the amplitude and frequency of price changes. Not only is there marked diversity of price variability from commodity to commodity, but certain group differences are noticeable. That is to say, the prices of farm products vary more than those of other commodities at wholesale, and wholesale prices vary more than retail. Mills regards these wide differences in variability as

/"evidence of the diversity of forces which are responsible for fluctuations in the prices of economic goods."²⁰ He points out that they mean price disparities which upset economic equilibrium, and which cause profits and losses. It is interesting to note that, before the World War, the variability of prices appeared to be declining, but the disturbances of the war period were reflected in an increase in the amplitude and ✓ frequency of price variations which continued for a long time after. Later in the volume, there appear other evidences of price disturbance, particularly during the war and postwar years, and of the responses of the price system thereto, which shed light on the general question of price stability and instability in which Mills is primarily interested. Fuller discussion of the significance of this whole problem will, therefore, be reserved for a later section of this essay.

In showing the long-time *trends* of relative prices, Mills makes use of a simple straight-line average of their annual rates of change over the period 1896-1913. For this purpose, the relative prices in 1896 are taken as a base, so that the trend lines, when plotted in the form of a graph, show a fan-like spread from their common point of origin in that year, thus portraying strikingly the different rates at which the prices of the various commodities have fallen or risen over this period of nearly two decades. Trends were computed for 223 different commodities. By correcting each of these figures by an index of the general level of prices during the corresponding period, it was possible to measure the changes which occurred in the purchasing power of each commodity, per unit of product. These changes in purchasing power are indications of the changes in values with which economic theory deals. Here we find, at one extreme, rosin, which rose at an average annual rate of 10.2 percent during the period

²⁰ *The Behavior of Prices*, p. 48.

covered by the study, and, at the other extreme, wood alcohol, which declined at an average of 3.4 percent, with all the other commodities ranging at various points in between. These changes in values are of interest because they denote basic changes in economic relations. Their usefulness is limited, however, by the absence of other data which might shed some light on the reasons for the changing price trends. The economic interpreter would like to know something about the movements of costs of production and changing outputs in the industries concerned, but Mills does not give this information. In some of his more recent writings, he has made some progress along these lines by investigating cost tendencies and volume of production for important groups of commodities, but this is not done in *The Behavior of Prices*.

The trend figures raise another question of interest to the theorist. This has to do with the presence or absence of normal relations between the prices of different commodities. Mills argues that the diversity of trends indicates that there is no normal relationship, though he believes that we may, perhaps, conceive of fairly regular rates of change as indicating normal tendencies. But the term "normal" smacks too much, no doubt, of neoclassical doctrines to suit his institutional temperament; therefore, he prefers to designate these tendencies as "expected" rather than "normal." This is another question which will come up again for discussion in a later paragraph.

There is one other finding, in connection with Mills' trend analysis, which should be recorded. It is that steep trends are correlated with great variability of prices; that is, those commodities whose trend prices showed the greatest degree of change over a long period also showed greater frequency and amplitude of changes from month to month and from year to year than did the prices of other commodities. This indi-

cates, perhaps, that secular changes in prices are not accomplished smoothly but are accompanied by a certain amount of erratic movement in the prices of the commodities affected.

Since Mills' work has been done as part of the program of the National Bureau of Economic Research, it is natural that a considerable part of his attention should be directed toward the cyclical aspects of his subject matter. Hence, he has developed this part of his study in considerable detail. He uses eight measures to describe the cyclical movements of prices. These are as follows: (1) date of the lowest price preceding a revival; (2) duration of the rise in price during revival and prosperity; (3) percentage of rise in price during revival and prosperity; (4) date of the highest point reached preceding a recession; (5) duration of the period of declining price during the recession and depression; (6) percentage of decline in price during recession and depression; (7) duration of the entire price cycle; (8) amplitude of the cyclical movement. Summary tables, showing all of these characteristics for each of 209 commodities in each of ten complete cycles which occurred between 1890 and 1925, were prepared by him, but since they were too voluminous to be reproduced in their entirety, they are given in the volume for only 24 of the more important commodities. These eight measures, however, are too unwieldy to afford a simple means of comparing the cyclical characteristics of one price with another. A more concise measure is needed. Therefore, Mills has derived from these measures an *index of cyclical variability*, which, in effect, measures the percentage by which a given price rises and falls during each cycle.²¹ Such indexes are given in an

²¹ This index is computed as follows: the rise in price of a given commodity during a given revival is expressed as a percentage of the peak reached in the ensuing upward movement, and the amount of decline in each recession is expressed as a percentage of the high that preceded it; these two percentages are then averaged to obtain the index of cyclical variability.

appendix table for all of the commodities studied in all of the cycles. Elaborate tables are also presented showing, for each of the 209 commodities: the number of cycles which characterized their price movements; whether their prices, on the average, remained constant, sagged below or rose above the general price movements of the corresponding periods; the average percentage of rise and fall in their prices; the average duration, in months, of their cycles; and the number of months by which their price increases or decreases preceded or lagged behind the major turns of the general price index, with a measure of their inconsistency in this respect.

The findings of this portion of the study indicate that there is considerable difference in the behavior of different prices with respect to the timing and amplitude of their particular cycle movements. They do not differ quite so much in the duration of such movements. The periods of revival are, in general, longer than those of recession. Some interest attaches to the conclusions reached concerning the sequence of cyclical price changes. It would be of considerable value to students of business cycles and to those who are engaged in the practical work of business forecasting, if the prices of some commodities were known to follow consecutively after certain others with regularity from cycle to cycle. With a fairly consistent order of price movements once definitely established, prediction of business recession and recovery would be on a much more reliable footing than it has been hitherto. As is to be expected, Mills found considerable diversity of sequence among cyclical price changes, but more consistency in recession than in revival. He did find, however, a group of 43 commodities whose sequence of cyclical change was fairly consistent in both revival and recession, and a group of eleven commodities whose behavior was still more

consistent. Of course, this is historical evidence, dealing with ten cycles which are past. One may be skeptical whether these eleven commodities will exhibit the same consistency of behavior in future cycles; but if they prove to do so, the discovery will be important.

Correlations between various cyclical measures made by Mills indicate that there is a common pattern among cyclical price movements, which, he believes, is not the result of chance. For example, those commodities whose prices rise and fall first in revival and recession are also the ones whose prices fluctuate most widely; those prices which rise first have the longest period of rise, and those which rise last have the longest periods of decline. In spite of some marked deviations, the sequence of price changes in revival and recession tends to follow a common pattern in different cycles. As in the case of trend changes, cyclical variability is found to be somewhat correlated with yearly and monthly variability. Apparently, prices which show tangent tendencies in one respect are likely to prove independent in other characteristics also. These conclusions Mills regards as significant generalizations.

In the concluding section of his first chapter, Mills deals with the relation between prices and price-determining factors. Here he touches on a subject of great interest to economic theorists, for it is with the problem of price determinants that the theory of value is primarily concerned. This is the field in which Moore, Schultz, Pigou, Leontief and others have been working in their attempts to derive demand and supply curves from statistical data. In comparison with the work of these writers, however, Mills' treatment is very disappointing. He is content to describe briefly the coefficients of correlation, determination, and flexibility which have been employed by other writers in this field, and to develop such coefficients for two commodities—hay and potatoes. Thus,

his treatment of the subject is illustrative rather than exhaustive. The concept of *price flexibility*, with which he here deals, is analogous to that of elasticity of demand and supply in value theory. It denotes the relation between changes in the price of a commodity and changes in the amount of it marketed. Mills finds, from his coefficients, that 53 percent of the variations in the price of hay from 1890 to 1925 and 88 percent of those in the price of potatoes from 1890 to 1913 were due to variations in the production of the two commodities. He concludes, therefore, that the price of potatoes is more flexible than that of hay.

Obviously, the correlation between the price of a commodity and the amount of its production does not get us very far in tracing the factors which determine prices. If the analyses of deductive theory in respect to this problem are to be supplemented by inductive studies, such matters as costs, consumption habits, human choices, technology, and the changing proportions of productive factors will have to be investigated. The paucity of data adduced by Mills in this phase of his work is in striking contrast to the elaborate pains he has taken to analyze other aspects of price phenomena. Perhaps it is because the data are not available, or, it may be that he believes that the results to be obtained by this approach are not as promising as those which are offered by his own type of analysis. He does say that "the conditions which would insure perfect comparability (in the use of the above coefficients) are difficult to secure and, accordingly, comparisons of coefficients of flexibility of price and elasticity of demand (and of the related measures discussed above) must always be made with great caution."²² Or, possibly he felt that others were working along these lines so effectively that he had best turn his attention to unexplored territory. What-

²² *Ibid.*, p. 151.

ever the reason, he lightly passes over the quantitative aspects of a problem which has been the major center of interest of economists for a century and a quarter.

Regional Differences in Commodity Prices and in Price Behavior. Economists have long recognized the differences in prices which prevail between countries; indeed, such differences occupy an important place in the theory of international trade. By comparison, scant attention has been paid to price differences in different regions within a single country. For instance, it is customary to construct indexes of wholesale commodity prices for entire countries, such as Germany, France, or the United States, as though prices were uniform throughout each of these areas. In his second chapter, Mills calls attention to the fact that there are considerable differences in prices in different parts of the United States. To demonstrate this, he uses wholesale price quotations for certain building materials and gasoline, retail prices for a number of foods, coal, gas, electricity, and dry goods, and farm prices for certain agricultural products. These data are derived from various cities (ranging from 10 to 50 in number, according to the particular commodity studied) for nonfarm prices, and from different states (ranging from 13 to 45 in number) for farm products. In order to afford a basis for comparison with noncommodity prices, similar measures of discount rates in different financial centers are likewise recorded. The differences in all these prices are measured in terms of the mean deviation of the regional prices from the average for the country as a whole, expressed both in absolute and in percentage form. The findings indicate that there are considerable regional differences within the United States in the prices of building materials, fuels, dry goods, and farm products. This is true even for such well standardized commodities as Portland cement and gasoline.

Regional differences change in magnitude from time to time. Absolute differences of farm prices were increasing slightly for twenty-four years prior to the World War and they rose sharply during the war period. Relative differences were moving downward before the war (except in the case of cotton) because the absolute prices were rising while the spread of prices from state to state was approximately stable. In the postwar years, this tendency was reversed; absolute prices fell, while differences from state to state remained substantially unchanged, causing the relative spread to increase.

The comparison of commodity price differences with those of discount rates is interesting. Discount rates might be expected to show very little regional difference, because of the ease with which capital funds can be transferred from one financial center to another. Such, however, was not the case. Differences in discount rates were greater than those in the prices of many commodities at retail, and not materially lower than the price differences of certain staple products at wholesale, although they were more uniform than farm prices and most wholesale prices.

In addition to direct comparisons of prices from region to region, Mills analyzes the regional differences in those aspects of price behavior for which measures were developed in the preceding chapter. This analysis is confined to a few sample commodities. He shows that there are important regional differences, both from country to country and within different parts of the United States, in the degree of price change between different dates, in the amplitude and frequency of year-to-year and month-to-month variability of prices, in commodity price trends, in cyclical commodity price movements, and in the flexibility of commodity prices. Price variability seems to be greater in the United States than abroad, although this is more true of some commodities than others. Price

trends differ from state to state and from country to country, due to differences in the development of productive methods, transportation facilities, and other fundamental economic changes. Mills feels that there is an important field for the study of the regional and industrial incidence of business cycles, which field has hitherto been neglected.

Mills' treatment of regional differences, although not as sketchy as his discussion of the relations between prices and price-determining factors, is, nevertheless, less complete than his development of other topics and is suggestive, rather than exhaustive, of the possibilities in this direction. Much more intensive studies of regional differences in the prices of particular commodities have been made by other writers. The recent study of fluid milk prices by John M. Cassel (cited in the fourth footnote, above) is a case in point, and Mills refers to certain others. However, Mills' chapter sheds some light on the general problem of regional price differences, and it perhaps justifies the broad conclusion to which he comes, viz.,

... that the United States cannot be treated as a single homogeneous market in a study of the structure of prices. If the United States is to be treated as a unit in measuring changes in the price level and in dealing with other aspects of the behavior of prices in combination, it can only be done on the basis of adequate regional sampling, with full recognition of existent regional diversities.²⁸

It will presently be developed that Mills is also skeptical of the value of general measures of the price level on other grounds. This is one of the interesting general problems raised by his study.

The Measurement of Price Instability: Prices Studied in Combination. Attention has already been called to Mills' belief that there is little to be gained by the analysis of individual elements in the economic organization. It is his view

²⁸ *Ibid.*, p. 210.

that it should be studied as a whole. The major interest of *The Behavior of Prices* centers, therefore, in the last two chapters of the volume, in which he describes certain group attributes of the price structure. He says,

... prices constitute a unified, coherent system. This conception emphasizes the fact that no one price is an isolated, independent phenomenon. From each price, lines of relationship run out to all other prices coeval with it in time, and back to previous prices, while similar lines run forward to affect future price quotations. Every price is connected by immediate or remote bonds to every other price.²⁴

This system is never at rest. It is a changing organism, with its parts constantly altering their relations to each other. An account of the working of the price system must, therefore, be a dynamic one. Change is of the essence of the relations which are here studied.

The remainder of the present volume and all of the volume which will conclude the present study deal with the system of prices. For a study of the behavior of prices in combination is a study of the price system.²⁵ . . .

He conceives that one of the most important aspects of this changing system of prices consists in its tendencies toward stability and instability. Therefore, his efforts are devoted mainly to discovering and describing those tendencies. He attaches three meanings to the idea of price instability. The first, which he calls *external instability*, is the concept of the general level or average of prices, a concept common enough to monetary and general economic theorists, as well as to statisticians and the public generally, and which, therefore, needs no further description. The second, he terms *internal instability*, by which he means changes in the relations among the various prices which make up the system. Some prices are rising while others are falling, and still others remain unchanged. These internal changes conceivably might take place without any alteration in the general level of prices. Therefore, the two concepts are essentially different. The third aspect of price instability has to do with what Mills

²⁴ *Ibid.*, p. 213.

²⁵ *Ibid.*, p. 214.

calls *the population of prices*. This refers to the group characteristics of the system of prices in its entirety as exhibited by such statistical devices as frequency distributions. The study of prices as a population consists in an analysis of such frequency distributions to discover the way in which prices are scattered about their central tendencies, and to ascertain the degree of their conformity or nonconformity to the normal Gaussian curve, or to the other curve types described by Pearson. Mills believes that price instability of these three types is closely related to the general question of economic equilibrium. Therefore, this part of his work has a bearing, not only on the concept of equilibrium as conceived by economic theorists, but it is equally important to the student of business cycles. Hence, the study of the tendencies to stability and instability in the system of prices is of very great importance to economists of all schools of thought. The significance of Mills' findings in relation to this general problem must presently be discussed, but the immediately following paragraphs will be devoted merely to a summary of Mills' methods and conclusions.

External instability, as manifested in changes in the general level of prices, is commonly measured by index numbers of the familiar type. This kind of instability has been so fully dealt with by other investigators that Mills devotes relatively little attention to it. His discussion is confined mainly to the technique of index number construction and is somewhat of a digression from his main theme. Six different types of index numbers computed on a fixed base, and six on the basis of link relatives, are listed, charted in graphical form, and compared. The differences in the measures yielded by these several types of indexes are noted and their standard errors computed. Mills comes to no conclusion concerning which type of index is the best, but he shows some interest in a different

type of index number which he here introduces. This consists of a twelve-month link index in which prices are expressed as a percentage of the prices prevailing twelve months previously. While he does not regard this as a substitute for the usual type of index number, he believes it advantageous for certain purposes because it eliminates the influence of seasonal changes. It records the occurrence of changes sooner than indexes of the conventional sort, and it reveals cyclical movements of prices very clearly. The most significant part of this section of his work for the general economist is his judgment that no general index number really tells the story of price changes, and that, therefore, a real understanding of movements in the price system calls for a study of the frequency distributions to which these index numbers relate.

The remainder of Mills' book is devoted to such a study. It is here that he investigates the phenomena of internal instability which were briefly referred to above. To measure internal instability, he makes use of two statistical devices which he terms the *index of dispersion* and the *index of displacement* respectively. Dispersion is used, in its commonly accepted meaning, to denote the distribution, or scatter, of the data about their average. In this case, the data consist of relative commodity prices. Their scatter varies from year to year, so that a series of annual frequency distributions can be compared with each other to determine what changes in the general structure of the price system are revealed thereby. The relative prices to be subjected to such an analysis may be computed in a number of ways. They may be expressed in arithmetical or logarithmic form; they may be weighted or unweighted; they may be calculated with reference to a fixed base (the prices prevailing in a given year) or on a moving base (the method of link relatives). With characteristic thoroughness, Mills handles his data in all of these ways and com-

compares the advantages and disadvantages of each. He renders a verdict in favor of logarithmic rather than arithmetically expressed relative prices because he finds the logarithmic movements less erratic in years of marked price changes. He also seems to prefer the method of link relatives over the method which expresses prices as a percentage of those prevailing in a single base year, although he also makes some use of relative prices of the latter type.

His *index of dispersion* is based on the standard deviation of the logarithms of relative prices, and is so computed as to show the percentage limits within which approximately fifty percent of the relative prices in a given period differ from their geometric mean. For example, an index of ten percent indicates that roughly half of the prices differ from their mean by more than ten percent and half by less than that amount. The significance of this index is that it measures

... the intensity of the disruptive forces, the forces which are operating, not to change the price level, but to change individual commodity prices in unequal degrees. The less direct the incidence of the force which is acting upon the price level, and the greater the relative importance of the host of specific price-making factors which affect individual commodities, the more widely dispersed will the price relatives be. The validity and the significance of any index number depend, therefore, upon the dispersion of the price relatives upon which it is based, and a measure of dispersion is a necessary complement to such an index number.

The disruptive forces possess interest and significance in their own right. For every inequality of movement affects the buying and selling relations upon which the movement of goods depends. Every inequality of movement introduces some element of instability into the price system....²⁶

Yearly indexes of dispersion derived from the relative prices of a large number of commodities (varying from 195 in some cases to 345 in others) are given for the years 1891-1926 and monthly indexes of the same type for the years 1906-1908 and 1920-1927. In each of the three periods, 1891-1902, 1902-

²⁶ *Ibid.*, p. 254.

1913, and 1913-1926, into which the figures are grouped, the dispersion index of fixed-base relatives moved generally upward. However, whereas from 1891 to 1902 the rising trend continued unchecked right up to the end, in both the other two periods the rate of increase slowed up toward the close; so that in 1902, prices were more disturbed with reference to their 1891 base, than were the prices in 1913 and 1926 with reference to their respective bases in 1902 and 1913, despite the violent price upheaval of the war and postwar years. The dispersion figures do, indeed, indicate that from 1914 to 1924 the disturbance of prices exceeded that of any other years in the series, but following 1924, there was apparently a quick return toward stability which led to relative tranquillity of prices.

The figures show that the index of dispersion can continue to increase over a long period of years, but this is true only when the relative prices dealt with are computed on a fixed base; it is not true of indexes of link-base relatives. Apparently, then, the steady increase of dispersion is due to the diverging secular trends of prices which have already been noted, and not to progressively more violent short-period disturbances (for the former would appear in fixed-base relatives over long periods, whereas link-base relatives would reveal mainly the latter). Mills believes that "there are *a priori* reasons for expecting that, in the absence of such exceptional disturbances as the war years brought, the dispersion of fixed-base relatives would increase at a decreasing rate, tending finally toward approximate stability at a relatively high level."²⁷ He does not state what these *a priori* reasons are, but he suggests that there may be economic factors, tending to check the indefinite scatter of prices, which are analogous to the tendencies toward reversion to type which

²⁷ *Ibid.*, pp. 266-267.

are formed in biological variation. There is evidence, in Mills' figures, of a rhythmically increasing and decreasing dispersion of prices, which indicates a tendency for the price system to become greatly disturbed and later restored to what may be construed as more normal relationships.

Mills' second concept of internal instability in the price system is designated as *price displacement*. This term refers to the shifting order of arrangement, or ranking, of the prices of different commodities from one period of time to another. For instance, if all the prices in a base year, such as 1913, be expressed as 100, a year later they will have different values, such as 125, 110, 90, and 75. If the commodity whose relative price is highest in this second year be designated as *a*, the next one as *b*, and so on, the alphabetical sequence *abcd*, etc., will represent their price ranking. At some later time, such as 1916, the positions of these commodities, in respect to their prices, will have changed, so that they will no longer rank in the same order. Commodity *a* may have fallen, price *b* may have risen, and so on, so that the order might now read *badc*. This shift in relative positions is what is meant by price displacement. If the positions of various commodities are completely reversed, so that the ranking becomes *dcba*, displacement will have been complete. Mills has here demonstrated his ingenuity in the use of statistics by contriving an *index of displacement* which measures these changes. This index consists of a coefficient of rank correlation, computed from either fixed-base or moving-base relative prices in such a way that a value of 2 would indicate complete reversal of previous price rankings, a value of 0, identity with previous rankings.

He gives such indexes, derived from the price rankings of 216 commodities, in various ways, so as to permit comparisons over monthly, annual, and biannual periods, with reference

to price rankings in either the immediately preceding periods or in base years more or less remote. In this way, he is able to show the yearly displacement of relative prices during and after the World War, from their prewar ranking, and the displacement of prices each year or each biennium. He also indicates the month-to-month displacement. The latter he finds too sporadic and irregular to be of much significance, and to some extent this is also true of the changes in ranking from one year to the next; but Mills believes that displacement over longer periods is of considerable significance, as showing alterations in the structure of prices or the "set of price relations." He reasons that changes in the ranking of prices from one year to the next reflect largely accidental and temporary forces; but that if changes be computed in relation to a remote base year, they will show more fundamental shifts in a well-established set of price relations. For example, relative prices on an 1891 base would, by 1914, reflect mainly long-time changes in costs and demands; the ranking in 1914 of such relative prices can then be used as a basis for comparison in the years from 1915 to 1926. Thus, one is able to ascertain how much of the structure of prices endures over long periods and to how great an extent it has been disturbed.²⁸

²⁸ Dorothy C. Bacon, *op. cit.*, objects to Mills' methods in the treatment of both dispersion and displacement. She argues that the use of a fixed base makes all prices look alike for the base period (because all prices in the base year are represented as 100), and thus obscures the real structure of prices. "No fixed base can catch the price structure in equilibrium." If price relations in the base year are abnormal (and she does not believe the years 1891 and 1902, which Mills uses as bases, were years of equilibrium conditions), prices may move toward more normal relationship in succeeding years, yet this movement in terms of indexes of dispersion and displacement will appear as a disturbance by comparison with the base year. "From the fact that some relatives are high and some are low and that dispersion is great, one can infer nothing as to the stability of the new price structure." She does not believe that the device of link relatives is a satisfactory escape from this difficulty. As an alternative, she suggests that price dispersions and order of ranking in given years should be compared with the positions they would

Yntema (in the review referred to in footnote 17) questions the usefulness of the concept of price displacement. He does not dispute the validity of Mills' measures of this phenomenon, but he doubts whether the displacement index adds much to the picture of fluctuations in the price system which is afforded by indexes of the price level and of price dispersion. Although the shifting relations among prices are of great interest to economists, the index of price displacement is like the dispersion index in that it shows only the aggregate movement; it does not reveal which prices have changed.

normally be expected to occupy in those years, as indicated by their secular trends. In other words, her method would be to express relative prices as percentages of trend prices. This suggestion will be discussed somewhat further in a later paragraph of this essay. It may be said here, however, that Miss Bacon's criticism hardly applies to price comparisons with a remote base year, especially in dealing with price displacement. It has previously been shown that the secular trends of relative prices spread out fanwise with the passage of time, as they diverge from their common point of origin in the base year. Therefore, in years far removed from the base, they will be rather widely separated. When this separation has been achieved, minor variations of prices, which might produce serious displacement in years near the base, will be relatively small in relation to the general position of prices which has now been brought about by the long-period forces; they will not be great enough to cause much displacement unless there has been a real shift in fundamental price relationships. Even if the base year selected is one in which prices were temporarily disturbed, it will make no difference, for these disturbances likewise will be too small to affect the position of relative prices prevailing many years later and, therefore, will not seriously distort the picture. This reasoning can be illustrated by a simple example. Suppose the relative prices of commodities *a*, *b* and *c* in the base year are each 100, and that one year later they are 102, 100, and 98, respectively, so that their order of ranking is *abc*. Now, if in the following year there are changes of only a few points in these prices, these changes may cause marked displacement, even though they may be of only a seasonal or cyclical character. For instance, if the prices are now 99, 101, and 102, the previous ranking will have been reversed, so that it will now be *cba*. Twenty years later, however, the secular forces at work may have brought the price of *a* up to 200 and the price *c* down to 50, leaving *b* at 100, where it was in the beginning. Now a change of 5 or even 10 percent in any one of these prices will not alter their ranking. Nothing short of a very fundamental shift would cause a displacement. It may be concluded, therefore, that when the relative prices whose ranking is being studied are computed with reference to a remote base year, the ranking must reveal whatever persistent and stable relations may exist in the price system, and that displacement of such ranking is indicative of basic changes therein, as Mills believes.

The question is whether two such aggregate measures are needed. While it is conceivable that there might be considerable price displacement without an increase in price dispersion, it is hardly likely that this would occur in the actual economic world. The two indexes would be expected to move together. Therefore, the index of dispersion, taken in conjunction with that of the price level, may be a sufficient indication of instability in the price system, without the use of the index of displacement. Mills finds that dispersion and displacement are, in fact, closely correlated, and the conclusions which he reaches from his analysis of the two phenomena are similar. Both indexes show a marked disruption of prices during the World War, followed by an apparent tendency of the price system to return toward greater stability in the postwar period.

It would be interesting to know whether this same rise and fall of instability was repeated in the depression of 1929-1933 and the subsequent partial recovery. Evidence on this point would be particularly desirable because of the importance which Mills attaches to the alleged tendencies to stability within the structure of prices, but, unfortunately, these later years are not included in Mills' study. Moreover, although his latest volume deals with prices in prosperity and depression during this very period, Mills' interest in the later work is centered in somewhat different matters, so that his findings do not shed any further light on this particular problem.

The index of displacement yields some evidence on another question. Some writers have held that greater price maladjustments develop during periods of revival and prosperity than during recession and depression. This, indeed, would be expected on a priori grounds; for a business collapse is presumably the result of disparities created during the preceding boom, and the subsequent depression would, therefore,

appear to be a time of readjustment, when economic relations are returning toward normality. Mills points out that his data do not confirm this. The indexes of year-to-year price displacement "do not indicate that maladjustments with reference to the immediate past develop during prosperity." He qualifies this, however, by pointing out that "since the indexes now under discussion relate only to year-to-year shifts, they would throw no light upon possible maladjustments with reference to an earlier situation."²⁹ The qualification is perhaps important enough to vitiate the conclusion, for displacement of prices with reference to an immediate past can hardly be taken as a criterion of their departure from normal relationships. Moreover, displacement of this sort might be expected to be as great in depression as in prosperity, because the return toward normality involves just as great a break with the price relationships of the immediate past as the departure from normality which preceded it. Hence, Mills' findings on this question must be regarded as inconclusive.

Some interest attaches to the relation between price dispersion, price displacement, and changes in the general level of prices. Some writers have offered evidence that prices become more dispersed when the price level is rising and less so when it is falling. Mills finds that it is the *amount* and not the *direction* of changes in the price level which affect the degree of internal instability in the price system. When the average movement of prices is violent in either an upward or a downward direction, both dispersion and displacement of prices become greater. Apparently, marked changes in the price level, caused perhaps by monetary factors, seriously disturb the ordinary price relationships between different commodities. This could be inferred from general reasoning, because the incidence of monetary changes would hardly be

²⁹ *Ibid.*, p. 304.

instantly and evenly diffused throughout the system of prices. Mills calculates, by coefficients of correlation, the extent to which price level changes are responsible for internal instability of prices. The figures indicate that from 40 to 66 percent of internal price disturbance is due to changes in the level of prices, and from 34 to 60 percent to other causes. These figures will have significance for the later discussion of the importance of the concept of the price level and of the proposals to stabilize it.

The third type of instability in the price system as analyzed by Mills, relates to *the population of prices*. The array of relative commodity prices in any given year, when arranged in frequency distributions, can be plotted in the form of graphs whose configuration tells a story to the statistician. In addition to the normal, or Gaussian, curve whose symmetrical, bell-shaped contour reveals the even distribution of data about their central tendency, Pearson has described a number of other curve types which are classified according to their degree of skewness and kurtosis (peakedness). It is with these curve types as applied to price data that Mills is here concerned. He has plotted the frequency distributions of his relative prices, expressed arithmetically and logarithmically, weighted and unweighted, on fixed bases and in the form of link relatives, for a long period of years. He has studied these frequency distributions from year to year to ascertain whether they conform to any of the Pearsonian types, and what degree of consistency they display in their curve patterns, if any. The presence of stability in the system should reveal itself, he says, by a more or less consistent adherence to a certain type of curve; therefore, if such consistency is not found—if the curves are not typical—it may be inferred that the system of prices is unstable.

He finds that price relatives seldom conform to the normal

law of error. The majority of the distributions are of Pearson's Type IV but there is no one type to which they adhere with any reliability; therefore, he concludes that price populations must be classed as erratic and extremely unstable.³⁰ This is especially true during periods of violent price upheaval such as the years of the World War. At such times, the disturbances which affect the price system produce marked swings away from the normal Gaussian curve—sharp rises or declines distort the curves in the direction of positive or negative skewness. After the period of disturbance, however, when comparative economic stability has been regained, the distributions swing back toward the normal curves; and this is equally true whether the period is one in which the general trend of prices is rising or falling. The clearly defined regression to the normal curve between the years 1916 and 1923 furnishes a striking illustration of this kind of reestablishment of equilibrium in the price system after a period of violent disturbance. This tendency of normal frequency distributions to reassert themselves after the forces of distortion have receded confirms the tendencies toward the reestablishment of stability revealed by the indexes of dispersion and displacement.

Measures of Price Behavior in Combination. In the first chapter of *The Behavior of Prices*, it was shown that individual prices exhibit certain characteristic types of movement in respect to which they differ considerably among themselves. These types of movement as classified by Mills, were month-to-month and year-to-year variability, secular trends, and cyclical fluctuations.³¹ In the final chapter of his volume, Mills examines these movements in the mass in order to ascertain

³⁰ *Ibid.*, pp. 334-336.

³¹ The relation between prices and price-determining factors (price flexibility) was also mentioned, but it will be recalled that it was very slightly dealt with. Mills make no further reference to this phase of price movements in his fourth chapter, which is now under discussion.

what are their group characteristics. In the third chapter, which was reviewed in the preceding section, he was also concerned with the group characteristics of price behavior, but there he was dealing with price movements in general, or rather with all kinds of price movement taken collectively; now he breaks the movements into these three types and examines the peculiar group attributes of each. For this purpose he combines, in the form of frequency distributions, his measures of variability, trend, and cyclical variation.

It was observed, in the description of month-to-month variability, that individual prices differ considerably in respect to both amplitude and frequency of their price changes. As to amplitude, he now finds a heavy concentration of frequencies at the lower end of the scale, which indicates that the majority of commodity prices do not fluctuate very greatly from month to month; but there are a few which show an extremely high degree of variability. On the whole, monthly variability was declining throughout the period from 1890 to 1913. The same tendency for the mass of commodity prices to be fairly stable, and a few very irregular, was found in respect to the amplitude of year-to-year variability. Distributions of prices arranged according to frequency of change yielded U-shaped curves, which indicates that a small number of prices show almost no change, a small number great frequency of change, with the bulk lying between these two extremes. He gives graphs of this type for five successive periods from 1890 to 1925 and finds this U-shape to be characteristic of all of them. However, during the World War the number of commodities in the group showing infrequent changes declined and those showing frequent changes increased, as was to be expected. It is interesting to note that Gardiner C. Means has found U-shaped distributions comparable to those of Mills for frequency of price changes in

1926 to 1933.²² The significance of these differences in the frequency of price changes for different commodities will be discussed in a later section.

The average trend of all of the prices included in Mills' study had a generally upward movement from 1896 to 1913, the average increase being 1.94 percent yearly. It was previously observed that there was considerable difference in the trends of individual prices; it is now revealed that these differences are systematically grouped about their mode. In view of this symmetry and the consistently U-shaped distributions of price variability, it appears that frequency distributions of price variability and trend conform much more closely to Pearsonian curve types than the distributions of relative prices themselves, as dealt with in Chapter III. The distinction between the latter and the frequency distributions now under consideration must be kept in mind. In the previous chapter, the frequency distributions dealt with the scatter of relative prices as they were found in successive years. The present chapter deals with the scatter of the measures of variation (amplitude, frequency, and trend) in those prices. It appears that the latter characteristics show more consistency of behavior than the movements of the prices themselves. That is, though prices may fluctuate rather erratically from one time to another, the monthly and yearly frequency and amplitude, as well as the general trend, of those fluctuations, taken in the mass, show considerable consistency. Commenting on this contrast, Mills says,

There is reason to think that heterotypic distributions contain certain elements of instability, that the populations to which such distributions relate do not cohere as closely or are not as homogeneous as are populations from which distributions of more orthodox types are drawn. Judging from this evidence one would conclude that prices are least stable, are

²² Gardiner C. Means, *Industrial Prices and Their Relative Inflexibility*, 74th Congress, Senate Document No. 13, 1935.

most exposed to the influence of unbalanced and disruptive forces, in respect to the degree of change between specific dates. Distributions which are much more stable, when tested in terms of their positive moments, are secured when the population of prices is sampled in regard to general variability, or in respect to the rates at which prices change over a period of years.²³

In analyzing the group characteristics of cyclical price movements, Mills has 1110 observations of prices, during ten cycles which occurred from 1890 to 1925, to draw from. The study of these many cyclical price movements in combination gives a picture of the way in which the forces making for business cycles impinge upon and permeate the price system. The picture is, however, somewhat futuristic in its outlines, so that (as Mills himself confesses) the findings are difficult to summarize. The data show that the prices of the great majority of commodities are affected by general cycles in business. Of those studied, the proportion whose prices rose and fell in consonance with the general cyclical movement constituted 71.1 percent, on the average. A few commodities, however, showed independent tendencies; and those that did conform to cyclical movements showed considerable differences among themselves in respect to the timing of revival and recession and to the duration and amplitude of their cyclical swings. These effects were revealed to a certain extent by the study of individual cyclical price movements in the first chapter; their study in combination shows how the cycle moves through the price system as a whole.

According to this analysis, revival (or recession) usually begins with a rise (or a fall) in the prices of a few commodities, followed by the mass of prices, then tapering off with those that lag behind, thus having a rather symmetrical pattern. The movement is more concentrated in recession than in revival. Although different cycles conform to this general

²³ *The Behavior of Prices*, p. 386.

pattern in the main, they nevertheless differ considerably in details. Frequency distributions of the duration of price rises during revival are generally positively skewed, but they become more symmetrical when revival is more prolonged, indicating that "when the general price advance continues for a sufficiently long period, the wide variety of forces which initiate and check the advances of individual commodity prices produces a semblance of balance in their cumulative action. . . ." ³⁴ The average period of the rise in prices was 22.2 months. Frequency distributions of the duration of price declines were also positively skewed, but more compact, and the periods of decline were shorter, on the average. The average degree of price rise during the ten cycles included in the study, computed as a fraction of the following high peak of prices, varied from 20.6 to 66.1 percent, the general average being 32.8 percent. The distribution of these figures was again positively skewed. The degree of variation from the mode did not vary much from cycle to cycle. The average percentage of price decline was 28.9, less than the corresponding figure for price rises, because of the generally upward trend of prices which prevailed throughout the years covered by the data. The duration of the cycle for the 1110 items had a mode of 35 months, with a fairly symmetrical distribution about it. This varied, however, from cycle to cycle. There were greater differences among commodities in respect to the amplitude than in respect to the duration of their cyclical price movements. However, when marked price changes were general, as they were during the more severe cyclical movements, those commodities which usually showed more variability did not stand out as exceptional—their irregularity was obscured by the general disruption which is characteristic of the period. In the usual cycle, the average range of fluctuations for those

³⁴ *Ibid.*, p. 407.

commodities which participated in the cyclical movement was from 25 to 32 percent of the high values which they reached in those cycles. On the whole, the frequency distributions of cyclical price movements in combination showed more uniformity than any others in the entire study. They conformed generally to Pearson's Types I or IV. They differed also from the distributions previously encountered in being flat-topped. Most of Mills' other frequency distributions showed a rather peaked character.

The Price Level and Its Proposed Stabilization. Having now reviewed briefly the contents of Mills' book, the ground has been laid for consideration of some questions of general economic interest which are raised by his study. Although he is content, for the most part, with findings of statistical fact, avoiding much interpretation or controversy, his work nevertheless raises issues both of theory and its application, and it is clear, from such cautious ventures into this field as Mills allows to creep into his pages, that he regards his book as yielding evidence upon such matters. One of the problems on which he has made his position fairly plain has to do with the general level of prices. Some indication of his views on this has already been given, but the subject warrants further discussion.

Mills holds that any general average of prices obscures the infinite variety of the movements which characterize the economic process. He has given such abundant proof of the diversity and intricacy of the price system that no further argument on this point is needed. He interprets the fact that different methods of constructing index numbers yield different results as indicative of "the inherent difficulty of describing by a single measure the complex price movements which take place between given dates."²⁵ The weakness is

²⁵ *Ibid.*, p. 233.

not one of statistics, he believes, but of the concept which the statistics are designed to measure. The true story of wholesale price changes cannot be told by index numbers, but only by the frequency distributions of the prices to which the index numbers relate. This must not be taken to mean that the concept of a general level of prices is of no value at all but only that it is of limited usefulness and that too much must not be expected of it. Taken in conjunction with his other measures of price instability, he believes that index numbers of the price level have practical importance but they are of little value taken by themselves. If index numbers are to be used, they must be made as accurate as possible. Hence, his recommendation that such numbers, for a geographical area as large as the United States, must take account of the demonstrated regional differences in prices, by having the quotations from different sections of the country which go into these index numbers weighted by their geographical importance. Impressed, as he is, by the heterogeneity of the price system, Mills is skeptical of any general index number of the purchasing power of money, such as that which has been attempted by Carl Snyder. He believes that progress is to be made rather in the construction of special index numbers for special purposes. So he would favor the development of indexes of the cost of living, of wages, of the prices of foreign and domestic commodities, and so on, which are now being worked out by statistical specialists in the various fields.

It cannot be denied that for some purposes the concept of a general average of prices is too broad. For the problems of international trade, of labor, of living standards, and for many others, index numbers measuring the prices of the particular groups of commodities concerned are needed. But for certain other purposes the idea of a general level of prices, and an

index which measures that level, are quite useful, provided their general character is fully understood and they are not applied to problems for which they are unsuited. They are most useful perhaps in monetary theory, where one is concerned with the purchasing power of money. It is hard to conceive of any other measure of monetary value than the broadest possible kind of an index of prices, including not only commodity prices, but those of services as well.

The issue here is most clearly joined when the proposals of those theorists who believe it possible to stabilize the general level of prices by monetary measures come up for discussion. Mills views all such proposals with skepticism because he believes that the stability and normal functioning of the economic system depend more on the maintenance of balanced relations within the system of prices than on the maintenance of a stable general level of prices. In short, external stability, as he uses that term, can never achieve internal stability—and it is the latter, not the former, which is important. These views, expressed incidentally in *The Behavior of Prices*, are set forth more explicitly in another article³⁸ where he emphasizes the diversity of prices and points out that monetary factors alone cannot right the disparities that arise in so complex a system. "Action on the monetary front . . . is inevitably shotgun action." He argues, further, that the monetary authorities cannot tell how large a degree of expansion or contraction of the monetary circulation will be brought about by a stated absolute change in the quantity of money or credit, that the incidence of monetary expansion or contraction on the price system is beyond the control of monetary authorities, and that prices differ so much in the flexibility of their response to monetary measures that disparities would be likely

³⁸ "Price Aspects of Monetary Problems," *Proc. of Acad. of Polit. Sci.*, 16:3-10, April 1934.

to result. Hence, although he does not go so far as to say that measures of monetary stabilization are valueless, he does not believe much can be accomplished toward stabilizing the price system by such means.

These arguments are not new, and they do not depend on the inductive evidence of such studies as those of Mills for their validity. They have been forcefully stated by theorists who oppose the monetary stabilizers. Mills has helped the opposition, however, by showing graphically and convincingly the complicated nature of the price structure with which monetary measures of control would have to deal. He has, however, proved too much; for some of his evidence goes against him. He himself shows that violent changes in the price level lead to disparities within the system of prices. It will be recalled that he found a definite correlation between measures of internal instability and marked movements of the price level, either upward or downward. From this, it would appear that, if external instability could be controlled by a program of monetary management, one important source of internal instability would be thereby eliminated. Moreover, if Mills' coefficients of correlation are to be relied upon, the degree of internal stabilization that might be accomplished by such means would be considerable, for his coefficients indicated that from 40 to 66 percent of internal price disturbance was due to changes in the price level.

The proponents of plans for stabilizing the general level of prices are fully aware that different prices move up and down unevenly, in response to the myriad influences of demand and supply that affect particular commodities and they know better than to offer their proposals as a panacea which would correct all price disparities. This has been made perfectly clear in the writings of Irving Fisher and others. But the stabilizers rightly regard fluctuations in the level of prices

arising from monetary changes as one serious cause of internal price disturbance. Mills' findings support rather than upset this view. On the whole, therefore, his work does not put the coup de grâce to the cause of price level stabilization, but, contrary to his own intent, when correctly interpreted, it rather helps that cause along.

His analysis sheds some light, also, on the controversy among advocates of price level stabilization, as to whether, in a program of monetary management, the price level should be kept perfectly stable, should be allowed to rise slowly, or should be forced gradually downward. Mills' statistics indicate that it is not the direction, but the violence, of price level movements that causes trouble. Therefore, so long as such movements are kept within reasonable bounds, the desired result will be achieved by any one of these policies. This is not to say that there may not be reasons on theoretical grounds for preferring one of these policies above the others.

Stability, Instability, and Equilibrium of Prices. If *The Behavior of Prices* can be said to have a general theme, it centers in the discussion of stability and instability in the price system. It has been shown that Mills regards the tendencies toward stability and instability within the system as among its most important aspects. The one outstanding generalization of his book is that, when the price system becomes generally disrupted, as it does from time to time, it tends thereafter to return toward more stable relationships.

The evidence of this is historical in character and based mainly on two episodes—the price phenomena during and after the World War, and the depression of 1921. The measures of variability, dispersion, displacement, and curve type all showed a violent upheaval of prices during these two periods, followed by a return toward stability thereafter. It has already been shown that Mills regards the curve types,

which he found in his frequency distributions, as especially significant in this connection. Failure of frequency distributions to group themselves in an orderly way about their central tendencies, he interprets as an evidence of marked instability in the underlying data. Conformity to recognized types of frequency distributions, on the other hand, is an evidence of stability. Therefore, he is convinced that the apparent tendency of his curves to swing back toward the normal type following the World War and the depression of 1921, in both of which periods his dispersions defied classification along Pearsonian lines, indicates that there were stabilizing forces at work in the system of prices which were reasserting themselves.

It is a curious and dramatic thing, this march of the successive Beta points back to the Gaussian point, as the conditions of balance and independence and multiplicity of causal factors which are characteristic of the normal law, and which had been so markedly absent during the war years, were gradually re-attained in the price system. It exemplifies in striking fashion the emergence out of chaos of that form of order which the play of sheer chance brings. Here is nature forming habits.³⁷

Mills, therefore, definitely believes that there is a potential set of price relations which, if it prevailed, would preserve balanced adjustment in the economy; and this set of prices is more than a mere hypothesis—it is a vital force whose presence is revealed by inductive analysis, and which is strong enough to manifest itself after a period of disturbance in a manner than can be measured statistically.

This is of tremendous interest to the economic theorist, for the conception of a fundamental set of price relations which tends to prevail in the long run, in spite of the vicissitudes of temporary disturbances, and which tends to keep the economy in balanced adjustment, has been the cardinal doctrine of the deductive theorists almost from the beginning

³⁷ *Ibid.*, p. 347.

of economics. In the Marshallian system, each commodity is represented as having a normal price which tends to prevail over long periods of time, and about which market and short-period prices fluctuate. A static state is conceived, in which all prices would conform to their normals and the economic system as a whole would be in perfect equilibrium. The theory of general economic equilibrium is substantially the same. It envisages a set of prices so related to each other that the entire system would be kept in balance. It is undoubtedly something of this sort that Mills has in mind when he speaks of price stability. In fact he says,

There is, of course, a close relation between the problems of price stability and that central problem of economic theory which is concerned with general economic equilibrium. In studying price instability we are dealing with the monetary aspects of this classical problem.⁸⁸

His measures of the behavior of prices in combination may, therefore, be construed as an attempt to show in quantitative terms the extent to which the actual system of prices deviates from complete equilibrium. It is a fascinating theme.

Mills' treatment of it, however, offers some difficulties. Although he nowhere clearly defines what he means by price stability, one can infer what his conception of this term is by his description of instability. Internal instability (with which he is mainly concerned) he defines as

that form of price instability which develops when a set of established price relations is disturbed. When buying and selling relations at any point in the market are altered, economic equilibrium is disturbed, and some element of instability is introduced.⁸⁹

This would appear to identify stability with the absence of all change. Some justification for this conception of price stability is to be found in theoretical literature. It has been

⁸⁸ *Ibid.*, p. 217.

⁸⁹ *Ibid.*, p. 215.

argued forcefully by economists of ability that the only tenable conception of economic equilibrium is that of the stationary state from which all change has been eliminated. In recent years, however, the more realistic notion of a moving equilibrium has been put forward, in which change is not absent, but in which the system is so perfectly adjusted to change that market prices always conform to normal prices and balance is preserved. The term "static," rather than "stationary," is frequently applied to this idea of equilibrium. J. B. Clark pictured the static state in this way; something resembling it is to be found in the writings of Alfred Marshall; and a similar idea is involved in the picture of a steadily progressive economy drawn by Gustav Cassel.⁴⁰ Certainly, if the counterpart of economic equilibrium is ever to be found in the actual world, the conception must be a dynamic, not a stationary one, for the actual world is a world of change, and it is a moving equilibrium toward which the underlying forces of that world tend. If this be true, then Mills' interpretation of any departure of prices from a position of rest as a manifestation of instability is not strictly correct. To picture the extent to which the system deviates from a moving equilibrium, a slightly different method would have to be followed.

Suppose that an economic system could be attained in which market prices would always be in exact conformity with normal prices, but in which certain forces were at work causing normal prices to change gradually over a period of years. Technological progress might be reducing the costs, and hence the prices, of manufactured goods; or, the growth of population and the consequent pressure on natural resources

* See J. B. Clark, *Distribution of Wealth* (1899), Chapters VI and VII; Alfred Marshall, *Principles of Economics* (7th ed., 1916), Book V, Chapter V, paragraph 3; and Gustav Cassel, *A Theory of Social Economy* (English translation, 1924), Chapter I, paragraph 6.

might be raising the costs of agricultural produce. Such a state of affairs would correspond to the notion of a moving equilibrium. The changing prices of such a world would appear in statistics as secular trends, when computed with reference to a fixed base year. These might be straight-line trends similar to those drawn by Mills, or they might be curves showing changing rates of increase or decrease. How would Mills' indexes of dispersion and displacement and his curve types behave under these conditions? If the changing trends of relative prices followed straight-line paths, the scatter of prices about their mean would steadily increase under the conditions assumed, yet this increase would not be indicative of increasing instability. If, however, the index of dispersion showed irregularities of movement, departing from the expected spread now in a positive and now in a negative direction, it would reveal the existence of cyclical or sporadic disturbances which were interfering with the maintenance of equilibrium. On the assumption, then, that normal prices move in straight-line trends, regular change of the index of dispersion in one direction would not necessarily be an indication of instability, but irregularity of change could be so construed. If the secular trend lines were curved, it is impossible to state how the index of dispersion would be affected. Conformity to or departure from normality would have to be determined by computing expected indexes of dispersion from a prediction of the curve trends, and then comparing the actual indexes therewith.

The behavior of the index of displacement would likewise depend upon the nature of the normal price trends. If the latter moved fanwise in straight lines, as Mills pictures them in his data, the index of displacement would not change, for the price of each commodity would occupy the same place in relation to the others, no matter at what time the observa-

tions were made. On the assumption of straight-line trends, therefore, it would be legitimate to interpret any displacement of prices as an evidence of instability (that is, of departure from normality) in the system. Again, however, if the trend lines were curved, they would presumably cross each other, and some displacement would then be a normal occurrence, not at all inconsistent with the maintenance of stability. The index of displacement, without reference to secular trends of prices, therefore, is not a certain criterion of price instability.

How the curve types of frequency distributions would behave in a moving equilibrium depends on the nature of the forces at work. If the forces making for reduction of some prices were not offset by exactly opposite forces making for an increase in other prices, the distributions would be skewed negatively. If the rising prices predominated, the skewness would be in the positive direction. If the two tendencies exactly offset each other the curves would remain unchanged in form. In some parts of Mills' discussion, he implies that, under conditions of stability, Gaussian distributions of relative prices are to be expected. At other times he seems to hold the view that consistent adherence to any one pattern would be indicative of stability. The second of these positions is more tenable than the first. Certainly, a priori reasoning would not lead one to suppose that normal price changes would be distributed evenly. Symmetrical distributions occur when some strongly predominating stable factors control the data dealt with, the individual variations in such data being relatively small, numerous, and independent of each other, so that their scatter is a matter of chance. For instance, strong biological influences are evidently at work tending to bring the height of adult male human beings toward a certain average. Individual deviations from

this average are numerous but slight and no force appears to be present giving them a bias in a particular direction; therefore, frequency distributions of such heights, if a sufficiently large number of samples are included, will take the form of the Gaussian bell-shaped curve—as Mills himself explains. But these conditions do not prevail in the case of prices. There is no dominant economic force tending to make all commodity prices the same, or nearly the same. On the contrary, there are numerous forces pulling in different directions. Moreover, variations of prices are not independent; they are definitely connected. For instance, changes in methods of agriculture tend to pull a whole group of prices in the same direction during the same period. Inventions in a certain branch of manufactures may pull another group of prices in another direction. Therefore, there is no reason why distributions of relative prices need not have several modes, nor why they need conform consistently to the Gaussian arrangement. Failure to find such conformity can hardly be interpreted as evidence of instability, nor, by the same token, of departure from normality. On the other hand, if, on observation, a definite type of distribution were found to predominate, temporary departures from this pattern might reasonably be construed as evidence of some disturbance. Mills did not find any such tendency of one curve type to predominate. Indeed, he seemed impressed with the lack of such conformity in his data; but, if the above reasoning is correct, such lack is hardly surprising.

In view of these considerations, it appears that Mills' analysis of price instability would have rested on a much sounder basis if he had begun by establishing a definite and logically tenable criterion of equilibrium prices. In a competitive economy, the concept of normal price would offer such a criterion, but it would be difficult to apply it. The price of

a commodity is normal when it is equal to the representative (or perhaps the optimum) costs of producing it; but adequate data on costs are not at present available, and even if they were, it would be no easy matter to decide which costs were representative or optimum. Moreover, our economy is not entirely a competitive one; it is characterized by a great deal of imperfect competition and monopoly. Neither monopoly nor imperfect competition are incompatible with equilibrium, but the prices which would maintain balance in such a system are not the normal prices of economic theory. What, then, is a statistician to do? Is there any criterion which he could employ?

Perhaps the secular trends of prices, if carefully calculated over a period of time which is sufficiently long, and which is reasonably free from upheavals of unusual violence, would come very near to indicating the prices that would lead to equilibrium. These trends would reveal the long-run forces actually at work in the economy—forces which assert themselves in spite of the ups and downs of short-period fluctuations. Something approaching a balanced adjustment in the economy would probably prevail if all prices conformed to the figures indicated by such trends. Therefore, if the statistical worker would begin with trend prices, and then calculate for each year the amount by which relative prices deviate therefrom, he would have a reasonably good basis on which to calculate the amount of unbalance, or instability, in the system. Indexes measuring the dispersion of these deviations could readily be prepared and the distributions thereof studied as to curve types. Even this procedure, however, is fraught with difficulties as will be shown in a later paragraph.⁴¹

Just why Mills did not use the method here suggested is

⁴¹ See the first paragraph on p. 64.

not entirely clear but several reasons occur as possible explanations. It has been pointed out that he has little sympathy with deductive theory. Therefore, he was evidently bent upon reaching his conclusions by strictly empirical methods and did not want to introduce into the analysis any theoretical concepts or abstractions. The concept of a normal or equilibrium price is theoretical and even a secular trend is something of an abstraction. It was shown in an earlier section of this essay that he definitely did not want to construe his trend figures as normal prices. He does not believe it is justifiable to assume that prices in any one year, or that average prices over any period of years, stand in a normal relation to each other. He says, "The only normal factor bearing on pre-war price relations (if we mean by normal a situation sustained in a fairly regular fashion over a number of years) was the existence of fairly constant rates of change. . . ."42 He looks upon the trend figures as indicating "that which is expected as a result of past experience," rather than that which is normal. Yet, in spite of himself, it is evident that he is toying with something very much akin to the equilibrium prices of the equilibrium theorists.

This is more clearly revealed by the discussion of "price disparities" which appears in his *Prices in Recession and Recovery*, where he defines price disparity as

the conditions prevailing after a shift in price relations to which there has not been complete adaptation among elements of the economic system at large.

In further elaboration of this definition, he explains that, by adaptation, he means

such adjustment . . . as may be necessary to a working balance of economic elements with an effective utilization of available productive resources. . . . This general definition of price disparity and of economic maladjust-

⁴² *Ibid.*, p. 74.

ment suffers because definite criteria of disparity and of maladjustment are not available. The definition goes back to a rather vague conception of a state of balance or equilibrium or mutual adjustment among the working parts of an abstract entity called the economic system.⁴³

In all this, Mills is obviously groping toward the very theoretical concepts from which he desires to escape. And so long as he is attempting to deal statistically with concepts which are theoretical, there is no use in trying to avoid elements of judgment and abstraction which may be necessary to give quantitative expression to them. For this purpose, carefully calculated lines of trend seem better suited than any other statistical data available.

The trends which Mills gives in *The Behavior of Prices*, however, would hardly be adequate—which may be one of the reasons why he did not use them. It will be recalled that his trends were all of the straight-line variety. It seems unlikely that the basic forces predominating in the price movements of all the commodities included in his survey were operating continuously in the same direction throughout two decades. Mills admits that

in some cases the straight line on ratio paper does not give as good a fit as would some other function. Even in these cases it has been employed as a measure of the average annual change during the period in question, and the r derived from the fitting process has been compared with and combined with similar figures for other price series. *If our present purpose were the measurement of cyclical and accidental deviations from trend this procedure would not be justified* in such cases, but the immediate object is something quite different.⁴⁴

It is precisely the “measurement of cyclical and accidental deviations” from trend that is advocated as desirable by the present writer. It is evident from the admission contained in the quotation that, had he followed this procedure, his trends would have had to be calculated differently. This is reinforced

⁴³ *Prices in Recession and Recovery*, p. 34.

⁴⁴ *The Behavior of Prices*, p. 63, footnote. Italics by Raymond T. Bye.

by the reasoning above, to the effect that curved trends in a moving equilibrium will affect price dispersion and displacement very differently from straightline trends.

Even had Mills fitted his trend lines more carefully, however, it is doubtful whether they could have been acceptable as indicative of equilibrium prices, because the period of time covered by his study was one of unusual disturbance. The economic upheaval of the War and postwar periods was so great that lines of trend were very likely distorted. In view of this fact, it is questionable whether Mills could have obtained from his data a really definitive picture of the tendencies to stability and instability in the price system by any method.

It must not be concluded from this discussion that Mills' analysis of internal instability is worthless. Even if he had done nothing else, he would be entitled to great credit for having done pioneer work in the statistical investigation of price equilibrium and disequilibrium. He has opened up an interesting field for further statistical exploration which may prove very fruitful in the future. But he has done more than this; for, in spite of certain weaknesses in his data and his methods, his findings do shed some light on the tendencies to instability in the price structure. Certainly, the changes in the index of dispersion and displacement which occur from year to year or from biennium to biennium are not primarily due to changes in trend. They must, therefore, be roughly indicative of cyclical and irregular fluctuations which constitute disturbances of the price structure. In the opinion of the present writer, however, he has not perfected a method by which it can be reliably determined whether the changes in prices in any given period are moving away from or toward more balanced relationships.

Price Variability and Price Rigidity. Mills' measures of

price variability are of interest because of the attention which has recently been given to the problem of flexible and inflexible prices.⁴⁵ Although current interest in this question has been especially awakened by the writings of Gardiner C. Means,⁴⁶ Mills appears to have anticipated him in pointing out the importance of the problem. Mills has made much of this issue in his last two books, and the groundwork for it was partly laid in the measures of price variability in *The Behavior of Prices*. It has already been recorded that he found marked differences in both the amplitude and frequency of price changes of different commodities and that he constructed U-shaped graphs of distributions showing frequency of price changes for five different periods between 1890 and 1925. His graphs were similar to those constructed by Means for the period from 1926 to 1933. Means has gone considerably further by adding other data which show the relationship in this last period between the frequency and amplitude of price changes and also by contrasting changes in prices with changes in production for important categories of goods.

It is the contention of both writers that there is a tendency toward increasing rigidity in the price system which prevents a rapid adjustment of the prices of certain groups of commodities to changes in the conditions of demand and supply, and which thereby lessens the adaptability of the whole economy to changing circumstances. It has already been pointed

⁴⁵ In *The Behavior of Prices*, Mills reserves the term "price flexibility" to denote the relation between the price of a commodity and the quantity of that commodity marketed, following the precedent set by Henry L. Moore, whose "coefficient of flexibility" was designed to measure this relationship. In the current discussion, the term "price flexibility" is used to denote either frequency or magnitude of price changes. This is equivalent to Mills' term "price variability." Mills himself speaks of price flexibility and inflexibility in this second sense in his later writings.

⁴⁶ See the essay cited in footnote 32. See also the more popular presentation of Mr. Means' ideas in *The Modern Economy in Action*, by Caroline F. Ware and Gardiner C. Means, 1936.

out that the equilibrium of economic forces in a dynamic world must be a moving one. It is through price readjustments that this changing balance must be achieved. Therefore, failure of prices to move in response to altered conditions of demand and supply is a source of maladjustment which may throw the economic system out of balance or which may prevent the speedy restoration of equilibrium after a disturbance. The relation between agricultural and manufactured goods has been especially stressed in this condition, it being alleged that agricultural prices move freely in response to changing conditions of demand and supply because they are governed by competitive market influences but that the prices of manufactured goods move less freely because they are produced under monopolistic or semimonopolistic conditions which make it possible for large corporations to exercise considerable control over the prices that are to prevail. It is said that these corporations find it good policy to prevent frequent and large readjustments of the prices of their products. So there may arise a disparity between the prices of farm products and manufactured goods which so upsets the conditions of trade between city and country as to interfere with the smooth functioning of the economic system. It has even been held that this disparity is an important, if not the primary, cause of business depressions although Mills does not commit himself definitely to this proposition.

The present writer is unable to find any evidence in the statistical material of *The Behavior of Prices* to support this belief in the tendency for rigidity in the price system to increase in recent years. Mills' distributions of measures of frequency of price change show graphs of strikingly similar pattern in all the five periods (1890-1897, 1898-1905, 1906-1913, 1914-1921, 1922-1925) into which he divides his data, except during the war years when changes in prices became

much more frequent; and it has already been noted that Means' graphs for the still later period 1926-1933 are, again, of the same general character. These distributions are distinctly U-shaped, indicating a group of prices at one end of the scale which is very variable and another group at the opposite end which is very rigid, with the mass lying in between. Evidently, then, these two extreme groups of prices have existed in our economy at least as far back as 1890, and it is not convincingly demonstrated that inflexible prices are any more prevalent today than heretofore.

Mills himself confesses, in his later writings, that the existence of increasing rigidity in the economic system is an inference for which quantitative proof is at present lacking. His belief that our economy is losing flexibility is based on the general observation of such phenomena as the growth of the corporate form of business organization, the increasing importance of fixed costs in industry arising out of the use of proportionately larger amounts of fixed capital, the growing strength of labor organizations, the increasing regulation of prices by government, and the evident tendencies toward greater intervention in international trade and foreign exchange transactions. So, the statistician who rejects the generalizations of economic theory in favor of those which he hopes to see develop out of quantitative studies, himself resorts to interpretations based on just those types of general observation which the deductive theorists have always used as a starting point for their analyses! But this is not to be found in *The Behavior of Prices*; it is a characteristic of Mills' later writings, of which more will be said below.

Here is another place where the introduction of theoretical analysis, of that very value theory which the institutionalists regard as so useless, would be helpful. A study of demand and supply curves under various hypothetical conditions should

reveal whether, on a priori grounds, some prices might reasonably be expected to show much greater variability than others, and some conclusions might be drawn concerning the significance of these differences in variability for the maintenance of general economic equilibrium. The problem is one in which inductive data ought to be able to show definitely whether or not rigidity in the system is increasing, but in which only theoretical analysis can interpret the significance of this fact, when once it has been established. The studies of Mills, Means, and others on this problem have, so far, merely opened up an interesting field for exploration. A final judgment on the questions thus raised must wait until there has been more empirical and theoretical work along the lines suggested.

It is interesting to note that whereas, in *The Behavior of Prices*, Mills seems impressed with the tendencies toward *stability* in the price system, in his later writings he emphasizes the tendencies to *rigidity*. Now these two things are by no means the same, for stability, as interpreted by Mills, is identified with equilibrium, and rigidity with disequilibrium, in the economy. Therefore, it would appear that Mills has shifted his position considerably. In a letter to the writer, he admits that

What looks like stability when conditions are relatively unchanging may appear as inflexibility when adaptability and not inertia is required. Some of the phenomena that in 1927 appeared to reflect the play of 'normative' influences, operative again after the disturbances of 1914-1921, appeared in 1932 as evidences of rigidity in the face of conditions that called for change, for adaptation to fundamental alterations in circumstances of economic life.

In the same letter he points out, however, that the stability with which he was concerned in *The Behavior of Prices* is an attribute of the price system as a whole, whereas the concept of rigidity refers to particular prices, or groups of prices. This

raises the question of how the presence of a group of inflexible prices in the system would be likely to affect Mills' indexes of dispersion and displacement, and the curve types of his frequency distributions. Given the presence of changing conditions in the economy, calling for adaptation, it would appear that the lack of flexibility of particular prices would be likely to increase both dispersion and displacement, and to distort the distributions away from the symmetrical. For, if some prices are moving while others are not, it is probable that the scatter of prices will be increased, their order of ranking will be changed, and their distributions will be driven askew. In other words, increasing rigidity ought to reveal itself in increasing instability. This would be consistent with the view that stability is indicative of balance, and rigidity indicative of unbalance, in the system.

Mills does not make use of frequency distributions in his later writings, so that it is impossible to compare the characteristics of the group attributes of the price system for the years of depression and recovery covered by his latest volume (1929-1936) with the measures given in *The Behavior of Prices* for earlier years. Such a comparison might have afforded some statistical evidence of the tendencies to rigidity that Mills believes to exist, but he did not avail himself of this possibility. So far as the years 1891-1926 are concerned, Mills' evidence indicates that tendencies to instability were present during the World War and in the depression of 1920-1921, but there is no indication of secular increase in instability of the system during this period as a whole. On the contrary, Mills interprets his data as indicating the predominance of stabilizing factors. One would infer from this that the adaptability of the system to change during this period was not permanently impaired, and that, if there has

been a pronounced change in this respect, it must have occurred since 1926.

If Mills and Means are correct in their view that prices are becoming less flexible, it would weaken the reasoning of a previous paragraph, that lines of trend might be taken as a criterion of equilibrium prices. How serious this weakening would be depends on the length of the period over which prices are prevented from moving. Infrequency of price change will not in itself distort the trend lines, provided in the long run there do occur changes of sufficient magnitude to work out an eventual adjustment. Moreover, if not too many prices are fixed, equilibrium could be achieved in spite of them, but it would be of a somewhat precarious character. Stable equilibrium would exist only if all prices could be kept at their normal. A statistical analysis of the extent to which such an equilibrium prevails would have to begin with the study of costs, the difficulty of which procedure has already been mentioned. It will probably be a long time before empirical workers will be able to give satisfactory quantitative precision to the interesting but elusive concept of general equilibrium. But this is no reason why efforts should not be made to approximate it as closely as possible.

Mills' Book Considered in Relation to Its Objectives. From Mills' own statement of his objectives and from a study of the book in the light of his general attitude on economic science, as revealed in the writings referred to in an earlier section of this essay, it can be said that he had two principal aims for *The Behavior of Prices*. In the first place, he sought to accumulate a body of data relating to the characteristics of individual price movements which would contribute toward a fuller understanding thereof. In the second place, he hoped, by an analysis of the behavior of these prices in combination, to derive scientific generalizations about the price system and

its interrelationships. It is perhaps not unfair to say that he wanted his book to be a pioneer contribution to the development, by purely inductive methods, of a body of scientific generalizations which might replace the traditional theories of value which he regarded as unsatisfactory.

There can be no doubt about his success in attaining the first of these objectives. His work is especially valuable as a compendium of data which describes many different aspects of the behavior of prices, both individually and in combination. These data are embodied in twenty-seven tables, constituting an appendix of 143 pages that contains a wealth of statistical information on wholesale commodity prices that will be of great value to investigators and interpreters of economic phenomena for a long time to come. Most of these tables relate to 200 or more commodities, some of them to over 400. The period of time covered by the figures in most cases is from 1890 to 1925, more than three decades. Here are measures of monthly and yearly variability, showing both the amplitude and frequency thereof, of secular trends, of various aspects of cyclical behavior, and of the characteristics of the frequency distributions of prices when combined in various forms. Many of the tables give a surprising amount of detail. The enormous amount of work that must have been entailed in the preparation of these tables has already been commented upon. The provision of this raw material in itself constitutes an outstanding contribution to the literature of economics. Taken in conjunction with Mills' subsequent two books, it perhaps justifies the statement by the Committee on Recent Economic Changes (under whose auspices the last two volumes were published), in its introduction to *Prices in Recession and Recovery*, that

these three works, covering the relationship and movement of prices since the beginning of the century, represent a monumental undertaking

in economic research in which the Committee has been happy to participate. They form an objective exploration into the realm of prices and their nature and influence, by an economist aloof from the pressure and the prejudices of business or politics; and they comprise a record that doubtless will serve as source material for generations.

A few illustrations of the uses to which Mills' data can be put will suffice to show their value. One such illustration has already been developed in the previous section where the relevancy of his measures of price variability to the problem of flexibility and inflexibility in the economic system was explained. These measures of variability, taken in conjunction with Mills' indexes of dispersion and his other criteria of instability in the price system, may afford material in the future for tracing the extent to which the tendencies toward adaptability or lack of it in our economy are gaining the ascendancy.

The importance of price trends for tracing the prices which would make for balance in price relations has also been commented upon. Such trends might be used in any program of control designed to maintain parity between the prices of important groups of commodities. The program of crop control embodied in recent farm legislation is a case in point. Both the AAA law and the recent Farm Act of 1938 set up, as their criterion of farm price parity, the relations that prevailed between the prices of farm products and other goods in the prewar years of 1909-1914. In view of the divergent trends which Mills' data disclose, the fallacy of this concept of parity, and the misguidance of agricultural production which will result from it, are convincingly demonstrated. Although Mills' trend lines are open to the criticisms previously stated, an agricultural program designed to keep farm prices in line with the trend prices shown by his calculations would be infinitely better than one aimed at the reestablishment of the price relations prevailing on any given date or

over any given period of years; and it would not be difficult to compute trends which would be more accurate.

The various measures of cyclical behavior so painstakingly developed by Mills must be of great value to students of business cycles. In a letter to the writer, Wesley C. Mitchell gives an illustration of this. He says,

Index numbers conceal almost as much as they reveal of the cyclical behavior of economic processes. By using Mills' elaborate appendix tables, showing the fashion in which the wholesale prices of many individual commodities respond to cyclical fluctuations in general business activity, I have been able to test certain working hypotheses more effectively than by using any other materials on prices with which I am acquainted. For example, one commonly overlooked but exceedingly significant line of demarcation in economic analysis runs between those commodities that are produced under conditions that make possible effective business control over the physical volume of current output, and those commodities produced under conditions that hamper such control. Products of mines and forests, also most highly fabricated commodities, belong in the first class; farm products and petroleum belong in the second. The distinction is especially important in business-cycle work because it is concerned with a rather "short run." This is just one example of the numerous notions that occur to an investigator from time to time and that can be tested only by using data of a detailed sort.

Finally, one might refer to the proposals for general economic planning which are now being so actively discussed. When it comes to planning the details of economic life, the planning authorities will have to have just such accurate information concerning price variability, trends, and cyclical fluctuations as Mills has here assembled. It is evident from passages in Mills' later writings that he has the problems of economic planning in mind and that he rightly regards the gathering of statistical data concerning all phases of economic activity as an essential tool to the control and guidance of economic activity. While for many problems much more information about prices than Mills has yet made available will be needed, the work he has so far done along these lines makes an important beginning at this task.

With respect to his second objective, it is the opinion of the present writer that Mills has been less successful. It must be kept in mind that he was attempting to establish scientific generalizations about the price system by a strictly inductive approach. Although his work is extremely suggestive and interesting, his results were necessarily restricted both by insufficiency of data and by defects of method.⁴⁷

In the first place, the study is confined to wholesale commodity prices. It does not include the prices of highly fabricated goods, of commodities at retail, of services (such as house rent, for instance), nor of the primary factors of production—land, labor, waiting, and business enterprise. In view of this restriction of subject matter, no trustworthy conclusions can be drawn from it concerning the price system as a whole. The omission of factor prices is especially serious. It is certain that there is a close interrelationship between the prices of commodities and those of the factors of production. No price analysis which leaves factor prices out of the picture can go very far toward an understanding of the price system. In omitting this phase of price phenomena, Mills has neglected a very fruitful line of approach to his major problem.

Equally serious is the fact that he has viewed prices apart from other aspects of economic life. He treats the price system as an independent entity, as though it worked in isolation from the other aspects of the economic process. This is abstraction as unreal as any ever made by the deductive theorists. The price system is undeniably an extremely important part of the economy but it is not independent of the other parts.

⁴⁷ A number of the difficulties herein developed have been anticipated by Jacob Viner in his article entitled "Mills' Behavior of Prices," which appeared in the *Quart. J. Econ.*, 43:337-352, February 1929. Although Viner's review is perhaps unduly severe in its criticisms, it nevertheless constitutes an able and penetrating analysis of the weaknesses inherent in Mills' treatment. This review is reprinted in this volume, pp. 165-180.

Prices are intimately related to such nonprice phenomena as consumption habits, productive processes, and monetary institutions. It is one of the major tasks of economic science to explain the relation between prices and these things. In this respect, traditional theory has done better than Mills, for it has emphasized the interdependence between the prices of commodities and the conditions of demand and supply which characterize them. The superficial treatment given by Mills to the relation between prices and price-determining factors which was noted in one of the preceding paragraphs of this essay as one of the most disappointing features of the book is in marked contrast to the painstaking thoroughness with which he has attacked certain other aspects of his problem. Any generalizations reached by a study of the price system entirely abstracted from all other economic phenomena bearing upon prices, as Mills' study is, must be rather superficial. It will appear presently that Mills' generalizations do, in fact, leave much to be desired.

The usefulness of Mills' study is somewhat further limited by the fact that the part of the price system which he does study is treated as a whole, to the exclusion of its parts. This was apparently deliberate on Mills' part. It will be recalled that he believes that traditional theory erred in approaching economic problems from the standpoint of their individual elements, arguing that the whole has properties of an order which cannot be described in terms of the behavior of its particular parts. So, although he has worked out various measures of individual prices, he does not attempt to follow up these measures with a detailed examination of the behavior of particular prices, or even of price groups. He makes use of this data only to combine them into frequency distributions and the like, in order that he may analyze their mass characteristics. Such an analysis is certainly legitimate—as far as it

goes. The study of dispersion, displacement, and curve types reveals certain aspects of price phenomena which might not be inferred from a study of the behavior of individual prices, just as the simultaneous equations of the general equilibrium theorists give us a view of the price system which is not obtained from the neoclassical method of describing the conditions of demand and supply for particular commodities. But many economic problems center in individual phenomena. For the solution of these problems, a description of mass phenomena does not suffice; an understanding of how the individual elements of the economy behave under given conditions is required. In the realm of prices, this is well illustrated by the case of agriculture. An important aspect of the agricultural problem in this country has to do with the relatively low prices of agricultural products as compared with the prices of manufactured goods. For an understanding of this problem, and the working out of a program for improving the economic status of farmers, a knowledge of the forces which cause particular prices to move upward or downward is required. No better method of setting forth the general principles which apply in such cases has yet been developed than the deductive reasoning of neoclassical theory. Yet that alone does not suffice. Before these principles can be applied to the particular case of agriculture, we need to know what are the particular factors of demand and supply which have been actively operating in this country in the past few decades to pull down the prices of farm products. The discovery of these factors is a task for technical experts and statisticians; but for this purpose the approach to price problems employed by Mills is useless. It does not reveal the general principles involved in the making of individual prices, nor does it show what factors are operative on the prices of the particular group of commodities with which the problem is concerned.

Therefore, one is led to the conclusion that it omits a phase of price analysis which is particularly important for the understanding of certain economic problems and for programs of economic control. This does not mean that the study of prices in the mass does not constitute an important contribution, but it does indicate that such a study is not a substitute for studies of individual price-making forces.

Finally, Mills' work appears to be somewhat hampered by the lack of a theoretical background. By studiously avoiding the principles of economic theory, Mills lacked a chart to guide him. Believing that the most fruitful approach to an understanding of economic phenomena is to seek generalizations by the actual observation of statistical data, he laid himself open to the criticism that he merely took a mass of facts and applied to them almost every kind of statistical technique at his command in the hope that he might hit upon some significant relationships. However, this is too severe a criticism; Mills did not set out to study the world of prices without a problem or hypothesis in mind. He was evidently interested in the problem of price instability and upon this he centered his investigations for the most part. The type of data he selected and the statistical methods he employed were no doubt suggested by their appropriateness to this problem. His unwillingness to employ the traditional type of theory in his handling of this problem, however, detracted from the usefulness of his work in the manner that has already been shown. By failing to carry over into his analysis the concept of an equilibrium price, he clouded his notion of stability with some ambiguity and used measures of instability which are not as reliable as the computations of trend which he might have employed. Moreover, by ignoring the value problems with which theoretical price analysis has been so much concerned, he passed by a fruitful field of investigation in which

inductive studies might have led him to some very interesting results.

These weaknesses of data and of methods necessarily reduced the possibilities of arriving at significant generalizations concerning price phenomena. The confining of the study to wholesale commodity prices would not prevent the reaching of conclusions which would be valid for this important group of prices, but the isolation of price from non-price data, the exclusive concentration on mass phenomena, and the failure to direct his analysis to problems suggested by theoretical considerations, were seriously limiting factors. The one outstanding generalization which emerges from the study is that there are tendencies to stability in the price system which tend to reassert themselves after periods of disturbance. The merits and defects of this part of Mills' analysis have already been sufficiently commented upon. The other generalizations are of a rather disappointing character because the insight into the workings of the price system which they give is too superficial. This can be made clear by the following illustrations, which are very typical of the kind of generalizations reached in *The Behavior of Prices*:

Farm prices show greater differences in the amplitude of their fluctuations than wholesale prices, and wholesale prices greater differences than retail.

The periods of revival of cyclical price movements for individual commodities are, in general, longer than their periods of recession.

There is a common pattern among cyclical price movements, that is, some consistency of sequence in commodity responses to cyclical influences, which is not the result of chance.

Cyclical variability is somewhat correlated with yearly and monthly variability.

Commodities with steep secular trends are more variable than those with less pronounced trends.

Dispersion and displacement of prices are correlated with the violence of movements of the general price level, but not with their direction.

Price dispersion is closely correlated with price displacement.

The dispersion of prices in revival and recession is rather symmetrically distributed.

Cyclical movement of prices is more concentrated in recession than in revival.

When revival is prolonged, the rise of prices is more symmetrical (hence better balanced) than when it is short.

The order of price recession in a given cycle exercises a stronger influence on the order of the succeeding revival than the order of revival does on that of the succeeding recession.

To Mills, these are illustrations "of just those uniformities for which the scientist searches in attempting to reduce masses of facts to understandable terms. . . . In the existence of these regularities lies our hope of achieving a fuller understanding of the working of the system of prices."⁴⁸

Certainly these generalizations add something to our knowledge of price behavior, but they do not constitute principles of very far-reaching significance. They fall far short of giving us an understanding of the influences which are at work in causing prices to behave as they do. With the single exception of the broad generalization concerning the existence of tendencies toward stability in the system of prices, not one of them goes as far toward describing the basic characteristics of price behavior as any of a number of the principles developed by deductive analysis. Contrast with the above, for instance, such propositions of theory as, that the price of a commodity tends to the level at which the demand for it equals its supply, that the price of a commodity in the long-run tends to equal the costs of production of a representative firm, or, that the general level of prices depends on the relation between the circulation of money on the one hand, and the volume of trade on the other ($P = \frac{MV + M'V'}{T}$).

The most helpful type of scientific generalization is that which describes relations of cause and effect, or, if the reader prefers to be very correct, relations of invariable sequence. The theoretical principles just referred to are of this char-

⁴⁸ *The Behavior of Prices*, p. 439. The list of conclusions is compiled from various portions of the book.

acter. They show relations between antecedent phenomena such as output, consumers' choices, costs, monetary circulation, or trade transactions, and consequent results such as prices. Mills' generalizations, for the most part, lack this quality for the simple reason that he has made no investigation of the phenomena antecedent to the occurrence of prices. His method was to isolate price data from the nonprice data to which they are related, whereas by a study of which relationship alone, he might have been able to develop a cause-and-effect analysis. A quotation from Mills' colleague in the National Bureau of Economic Research, Frederick R. Macaulay, is pertinent here. He says,

A statistical study that presents nothing but raw facts may sometimes be extremely valuable, while one that presents 'correlations,' or other evidences of empirical relationships, as though they were explanations, may easily become a delusion and a snare. . . . If much progress is to be made, examination of facts must be followed by a serious attempt to understand them, to think the matter through. Mere empiricism will not do. Real explanations come by way of shrewd conjecture followed by adequate testing—the old-fashioned road of intelligent hypothesis and rigid verification.⁴⁹

Jacob Viner, in the review which was alluded to in footnote 47, observes that one might conclude from a reading of *The Behavior of Prices*, and from the paucity of significant generalizations developed therein, that there was more of chaos than of order in the system of prices. In spite of the uniformities and tendencies toward stability which he describes, Mills does seem to be much impressed with the instability and heterogeneity of his data. This, however, says Viner, is not due to the lack of order in the price system, but to the defects of Mills' method of studying it. Had he been willing to be guided by groupings of the data and by the relationships between price and nonprice phenomena suggested by a priori

⁴⁹ *The Movements of Interest Rates, Bond Yields and Stock Prices in the United States Since 1856* (1938), p. 8.

reasoning, and had he directed his statistical researches to the testing and elaboration of plausible hypotheses, he would have obtained much more significant generalizations.

In defense of Mills, it should be stated that he recognizes some of the limitations of his work in *The Behavior of Prices*. It is doubtful if he would agree with Macaulay's dismissal of correlations as "mere empiricism," and he would probably not have much enthusiasm for hypotheses; but he does realize that there are limitations to statistical methods, and he frankly acknowledges that the particular problems which he considers in *The Behavior of Prices* do not cover the whole range of useful price investigation. Especially in his essay on "Price Data and Problems of Price Research,"⁵⁰ he gives evidence of a somewhat broader attitude—even toward deductive theory—than appears to have dominated him when he was at work on *The Behavior of Prices*. In that essay he says, "The demands of the speculative theorists have far outrun the lagging pace of observation," and he outlines a wide range of price problems which require the attention of the statistical worker. In the latter connection, he definitely mentions the need for more study of costs in relation to the selling prices of goods, and for investigation of the relation between prices and nonprice data. Moreover, in *The Behavior of Prices* itself he was frank to acknowledge that a complete picture of prices could not be obtained by viewing them in isolation from other economic phenomena, when he said,

Alterations in price relations may be due to . . . any of the thousand factors that affect economic processes. The causes of internal instability are not, of course, found exclusively in the price system itself. Perhaps the chief causes are external to that system. Back of changing price margins and shifting price ties are alterations in economic relationships, alterations which may be slight or profound. These are the ultimate objects of interest.⁵¹

⁵⁰ *Econometrica*, 4:289-309, October 1936.

⁵¹ *The Behavior of Prices*, p. 215.

And elsewhere, he has repeated that the system of prices is not an entity apart, but is "bound up with physical transactions of production and sale, as well as with legal bonds and business customs."⁶² His failure to develop some of the other aspects of price problems, therefore, may be explained as a perhaps justifiable centering of his interest upon a particular phase of those problems, a phase which seemed to him to be in particular need of exploration.

It must be borne in mind that *The Behavior of Prices* is only part of a more comprehensive program of price studies by Mills, which becomes more ambitious as it proceeds. Originally intended to be followed by a second volume which was presumably to conclude those particular aspects of price relations to which *The Behavior of Prices* is devoted, Mills has since been led into a series of price investigations, and is working upon still others which seem likely to represent a lifetime of endeavor devoted to this general theme. Final judgment on Mills' contribution to economic thought, therefore, cannot be based on *The Behavior of Prices* alone but must wait until his work is completed. In the meantime, it is appropriate to consider briefly what development of his thinking and what changes in point of view are evidenced by those of his subsequent price studies which have so far appeared.

Mills' More Recent Price Studies. It has been stated that Mills planned to follow up *The Behavior of Prices* with a companion volume which was to deal with the "isolation and analysis of price groups and the testing of various grouping principles,"⁶³ and to "define certain of the component elements of the price system."⁶⁴ In the analysis of price groups it was his expectation to "return to the subject of heterogeneity

⁶² "On the Changing Structure of Economic Life," p. 377, in *Economic Essays in Honor of Wesley Clair Mitchell*, New York, 1935.

⁶³ *The Behavior of Prices*, p. 219.

⁶⁴ *Ibid.*, p. 34.

of populations," and to "discuss methods of testing for heterogeneity."⁵⁵ Although these indications of the content of the proposed volume are somewhat vague, it appears that it was Mills' intention to employ methods of investigation very similar to those of *The Behavior of Prices*, except that, instead of viewing the commodity price system in the mass, he intended to break it down into related commodity groups. This work has never been completed. Although Mills still has some thought of returning to it, he explains⁵⁶ that "the exigencies of the National Research Bureau program" have diverted his attention to certain other types of price studies. The most important of these are embodied in his "Price Movements and Related Industrial Changes" (which appeared as Chapter IX of the *Report of the Committee on Recent Economic Changes in the United States* (1929)), *Economic Tendencies in the United States* (1932), and *Prices in Recession and Recovery: a Survey of Recent Changes* (1936). He is now engaged on a monograph for the National Bureau which will deal with the behavior of prices during business cycles. These writings reveal a considerable shift, both of interest and attitude, on Mills' part; therefore, some discussion of them is pertinent to the present inquiry. It will not be necessary to describe their contents in detail; it will suffice for the present purpose to point out the principal respects in which they differ from *The Behavior of Prices*.

The new emphasis on inflexibility, in place of the former stressing of stability in the price system, has already been discussed. There are other important differences. Perhaps the most significant of these is that these later writings are not concerned with the basic characteristics of the price system as such but deal rather with recent historical developments

⁵⁵ *Ibid.*, p. 320.

⁵⁶ In a letter to the writer.

of prices and their relations to certain other phenomena. They are particularly concerned with the fortunes of three major groups of producers: those engaged in the production of raw materials and farm products, those in the fabricating industries, and those who produce consumers' goods. To some extent, this carries out Mills' expressed intention to analyze price groups, but the general emphasis and method of these works is quite different from that of the behavior study, and is an evident departure or digression from the projected companion volume. Because of their historical character, the recent books constitute a record of past events and explanatory interpretations thereof rather than an attempt at the derivation by inductive methods of scientific generalizations concerning the price system. Therefore, they do not possess quite the same significance as *The Behavior of Prices* for the broad question of methods in the study of economics; but they do represent some progress in Mills' method of approach.

An attempt to explain the price occurrences of the postwar period necessarily requires that attention be paid to other than commodity prices. It also requires that the underlying causes in nonprice data be investigated. Therefore, Mills now extends his analysis in both of these directions. He goes into the matter of factor prices to some extent, studying especially wages and the rewards of ownership and management. In this connection, he traces the changes in these two broad classes of income and shows how the relative purchasing powers of the corresponding groups of people have been altered. He investigates such causal phenomena as population growth, changes in the volume and character of the different branches of production, and developments in the fields of finance and international trade. Similarly, an attempt is made to get at the relation between prices and costs. In this part of his study Mills is concerned primarily with the prices of fabricated

goods. Not having data showing directly the costs of producing the various commodities included in this category, he is forced to get at them indirectly. He does this by taking the movements of wages and the share of ownership and management in this branch of industry as indicative of the changes in costs therein. A widening spread between wages in manufacturing enterprises and the prices paid for the fabricating of goods (that is, the value added in the process of fabrication) is noted. This does not yield any information as to whether the prices of particular goods conform to their costs, nor as to whether the increasing share of ownership and management reflects rising overhead costs or excessive profits, but it makes a beginning at analyzing the relation between costs and prices in this important branch of industry.

Mills' explanation of the causes for the unbalanced economic conditions of the postwar period is of interest. He describes the events of this period as a manifestation of serious disparities which were developing in the structure of prices, partly as a result of changing relations in the underlying phenomena of economic life, and partly as a result of the growing rigidity in the economy which has already been referred to. He had observed in the prewar period, a tendency for the prices of fabricated goods to decline as a result of the phenomenal increase in productivity in this branch of industry brought about by technological improvements. During the World War this tendency was reversed, the returns to fabricators now increasing, relatively to the shares received by the other branches of production. Neither the postwar period nor the Great Depression entirely checked this relative increase in the prosperity of the fabricators at the expense of other groups in the community. Mills regards this as evidence of a serious disparity, because he believes that the prewar tendency to a declining margin between the prices of raw

and processed goods, on the one hand, and the prices of fabricated goods on the other, was a deep-seated and persistent movement, resulting from gradually falling costs incidental to improved processes, which should have continued into the postwar period if balance was to be maintained. The upward push to manufacturing industry during the war was not corrected afterward, because of the increased influence of the factors making for inflexibility. The failure of fabricating prices to decline upset the balance of purchasing power between the groups employed in the production of farm products and raw materials on the one hand, and those employed in manufacturing industry, on the other. Owners and managers also gained somewhat, in relation to wage earners. These and certain other marked shifts in economic relationships led to some disturbance in 1920-1921, but the period of this depression was too short for the attainment of fundamental readjustment. Immediately thereafter, there was a strong upward surge of productivity, probably exceeding that of the Industrial Revolution—involving changes in costs, new relations between producing groups, and displacement of workers—in a system already somewhat out of balance, growing in complexity and independence, and at the very time when loss of flexibility was making adaptation to change more difficult. The collapse of 1929 and the depression which followed it was the logical result.

The contrast between this type of interpretation and that of *The Behavior of Prices* is clearly marked. Here is no longer the strict statistician bent upon building a new science of economics out of empirical data but a more broad-minded interpreter who combines statistical evidences of fact with general observation and inference to explain a series of events. The explanation reached cannot be said to be entirely the product of inductive analysis, for one of its most important

premises, the existence of increasing inflexibility, is admittedly an inference not quantitatively demonstrated. Other investigators using the same statistical data might readily arrive at some other explanation. On the whole, it represents a more fruitful procedure. Mills in these writings shows a better balanced mind, a more judicious selection of relevant information, and a broader attitude on the problems of economics.

It is interesting to note that, in these studies, Mills apparently has found little or no use for the statistical devices he developed in *The Behavior of Prices*. Here there are no measures of price variability, no indexes of dispersion or displacement, no considerations of curve types. Mills finds simple indexes of the usual type, showing the level of particular groups of prices, the movement of wages, of population, of physical production, and so on, adequate for his purpose.

Some Problems of Method Raised by Mills' Work. It was stated early in this essay that the significance of *The Behavior of Prices* consists in the fact that it represents an attempt to replace the methods employed by traditional economic theory in the study of prices by an entirely different approach. Having now surveyed Mills' work and seen something of the results arrived at, it is appropriate to consider some of the broad questions of methodology which it has raised. These questions can be posed in the form of a number of antitheses, namely: the abstract versus the concrete, the parts versus the whole, statics versus dynamics, deduction versus induction, qualitative analysis versus quantitative analysis. In common with the more extreme institutionalists, Mills believes that little has so far been accomplished and not much can be hoped for in the future from traditional methods of economic analysis because they are abstract, individualistic, static, deductive, and qualitative in their approach. He visions the economic science of the future as one which will be concrete, which will

deal with the economy as a whole, which will describe it in its dynamic processes of change, and which will employ inductive and quantitative methods.⁵⁷ *The Behavior of Prices* is offered, not avowedly, but by implication, as an example of this newer type of approach. This attitude involves a misconception both as to the nature of scientific method and as to the essential characteristics of deductive economics.

The method of science is always one of abstraction from reality. Phenomena as they occur in nature are too complex to be described literally in terms that have any meaning for our finite minds; therefore, the scientist reduces the behavior of the universe to its simplest terms, abstracting the particular aspects of things which he wishes to consider from the disturbing elements which obscure its working, either by creating in his laboratory artificial conditions from which those disturbing elements have been removed, or by excluding them from his mind by assuming conditions in which they are presumed to be inoperative. The simplest propositions of science, such as the law of gravity or the Mendelian law of heredity, are unreal, in the sense that they do not manifest themselves in nature exactly in the way they are stated. This is true because other factors are always present to modify their effects. For many years, the atom of the physicists and the gene of the biologists were merely concepts—working hypotheses even more imponderable than the utility and the marginal product of the economists; yet they were powerful aids to progress in their respective sciences. Some of the propositions of physics are so abstract that they defy expression in

⁵⁷ The institutionalists, generally, would stand with Mills on all of these points, except, perhaps, the emphasis on studying the economy in its entirety rather than by the analysis of its individual component parts. While the institutionalists have expressed opposition to the emphasis on individual psychology in neoclassical theory, they have not, so far as the present writer is aware, insisted on the importance of studying economic phenomena in the mass in the way that Mills has done.

any but mathematical terms. Neither Mills nor any other statistician can avoid the use of abstractions equally unreal. The average is an abstraction and so are Mills' lines of trend, his indexes of the price level, of dispersion, and of displacement. They cannot be observed directly in the world of prices; they are constructs of the scientific mind, obtained by abstracting certain elements from that world for the purpose of special observation.

Much the same thing is true in regard to the study of the individual elements of phenomena as compared with their study in the mass. Just as it is the essence of scientific method to abstract particular aspects of things from their actual setting, so it is characteristic of that method to break a complex problem into its parts for analysis of the elements which compose it. Man cannot comprehend the whole of anything. His mind is incapable of such a feat. Therefore, he takes one part of it at a time for intensive analysis. The progress of science is almost a record of further breaking down of the phenomena of nature into smaller and smaller elements which are studied in detail. Mills proves too much when he claims, as he does in his essay "On the Changing Structure of Economic Life,"⁸⁸ that the center of interest in the physical sciences has shifted from particular entities, such as atoms, electrical potentials, and the like, "to qualities possessed by the system as a whole which cannot be split up and located—a little bit here and a little bit there." Long before the physicists had begun to look at the statistical properties of the minute elements of matter and energy with which they are now so much concerned, they had made tremendous progress toward an understanding of the physical world by careful analysis of such particular elements of matter and motion as drops, molecules, heat-waves, and velocities; and it may be that their

⁸⁸ In *Economic Essays in Honor of Wesley Clair Mitchell*, 1935, p. 360.

contemporary preoccupation with such mass phenomena as entropy, for instance, is due to the fact that they have not yet been able to devise means of breaking matter further down into individual elements which can be studied separately. Mills has not succeeded in studying the economy as a whole; he has concentrated his attention only on one element of the economy—the price system, and only on a part of that, viz., commodity prices—and only on a portion of those—wholesale commodities! This is as it should be; for, by meticulous separation of economic phenomena into their elements, we are likely to derive knowledge which could not be reached by any other means. This is not to deny Mills' statement that the whole has properties not possessed by any of its parts. It has such properties; and Mills has done useful pioneer work in describing the mass characteristics of a certain portion of the price system; but the description of these group characteristics will never constitute a substitute for the scientific investigation of the forces which determine prices taken singly. The two types of investigation are complementary, not mutually exclusive.

The controversy over static versus dynamic analysis in economics is equally futile. We live in a world of change and it is part of the task of economists to describe the nature of that change, which means to develop laws of change if possible. But this does not mean that nothing is to be gained by assuming, as a preliminary hypothesis, a state of affairs in which change is supposed to be absent. The very contrast between phenomena, as found in such a stationary state, and phenomena as they are observed in the world of reality, may help to reveal what dynamic forces exist and how they operate. The hypothesis of the static state is merely a part of that process of abstraction which has already been stated to be essential to scientific method and it is strictly comparable to the vac-

uum created by the physicist for the study of certain types of physical behavior or the controlled conditions created by the biologist for the observation of certain hereditary influences. Moreover, it is incorrect to represent the propositions of deductive economics as the result of exclusively static analysis. Marshall has given special attention to the dynamic aspects of value problems. His distinction between the value-making forces in day-to-day markets, in short-period equilibrium, in long-period equilibrium, and in secular change, is just as dynamic as Mills' study of the tendencies to stability and instability in the system of prices; and the generalizations developed by Marshall concerning the effects of changing demands and the response of supply influences thereto, of the rise and fall of business enterprises and the predominance of the representative firm, come nearer to constituting laws of change and growth in economics than any propositions yet put forward by the institutionalists.

The distinction between deduction and induction in scientific work is very largely one of emphasis. All sciences which deal with real phenomena must be to some extent empirical; on the other hand, the very process of reaching generalizations calls for a certain amount of deductive inference. Traditional economic theory is not derived, like geometry, from purely abstract premises, nor are the findings derived from such concrete studies as those of Mills obtained by a process of pure induction. The classical and neoclassical theorists were keen observers of economic phenomena. David Ricardo, who was perhaps the first to set the fashion of casting economic generalizations into the form of logical syllogisms, was a practical business man with a first-rate knowledge of finance. Alfred Marshall was not a mere arm-chair philosopher, who spun theories from the threads of pure intellect. He spent a great deal of his time in factories and other business establish-

ments gathering information at first hand about industrial processes and market practices. His practical knowledge of these matters is evident throughout the pages of his *Industry and Trade*. It is said of him, that he had so good a knowledge of English wage rates that when he saw a workman performing a certain task in a British factory, he could give a very close estimate as to the wages the man was being paid. But the classical and neoclassical writers, desiring to make their propositions appeal logically invulnerable, represented them as deductions from certain premises derived from the fashionable utilitarian psychology of their time, when, in fact, they were not developed from such premises at all. Actually, they were in large part derived empirically, from observation of what was actually happening in the world of markets and men. The law of demand and supply is a good illustration of this. The tendency for the price of a commodity to settle at the point which equates effective demand with effective supply is a generalization which could hardly escape discovery by anyone who thoughtfully contemplated the phenomena of market price behavior. Strip neoclassical theory of all its psychological premises concerning economic men, marginal utility, and the like, and most of the mechanism of demand and supply curves and equilibrium embodied in the neoclassical theory of value will remain unaffected thereby—because it is grounded in the facts of experience.

On the other hand, generalizations which appear to be reached by strictly inductive research frequently involve a considerable amount of deductive analysis. It is not often that a scientist gathers facts and analyzes them in a purely objective manner to find out what they will reveal without any preconceived hypothesis or expectations. Usually his attention has been called to some phenomenon of interest that has

suggested a problem and he probably has formulated a tentative theory about that problem which he uses as a guide in his further researches. The inductive work is for the purpose of testing the theory, rejecting it or modifying it, if need be, and perhaps leading to an entirely different explanation. Thus, the theoretical explanations of Einstein suggested new ideas concerning the nature of gravity and the transmission of light which led astronomers and physicists to fresh researches. Mills evidently had some ideas about tendencies to stability and instability in the price system when he undertook his study and it is probable that he expected to find inductive evidence of the sort he did in fact obtain, although it is likely that he was unprepared for as much heterogeneity as the data eventually revealed. Some of the best inductive work in the field of economics has found its basis in deductive theory or has made extensive use of deductive analysis. Witness the studies of Viner and Angell in international trade, of Moore, Schultz, Pigou, and Leontief in the statistical analysis of demand and supply curves, of Kuznets in the field of capital and income, and Macaulay's recent work on interest rates, bond yields, and stock prices. Moreover, the inductive worker, when he has once assembled his data, reaches generalizations therefrom by processes of deductive logic. Induction merely gives him raw materials for a further process of analytical reasoning. Where the investigator proceeds with a bias against the use of deductive logic, the results are likely to prove disappointing. Certain weaknesses in *The Behavior of Prices* which appear to arise from this cause have already been noted. Thus, once more, it would appear that the supposed antithesis between two methods of procedure dissolves into mist. They are complementary, not antagonistic. Neither economics nor any other science can make much progress

unless they are combined. The apparent hope of some institutionalists to build up a new science of economics by pure induction is therefore bound to prove illusory.

Much of what has been said concerning induction and deduction applies equally to the relation between qualitative and quantitative work. The two must go hand in hand. The economic analyses of the nineteenth century were mainly qualitative in character because the statistical data for any other type of investigation were not available, and the methodological techniques for handling such data had not yet been developed. The economist is still hampered somewhat by these difficulties but these handicaps are rapidly being overcome so that from now on economics may be expected to take on an increasingly mathematical and statistical aspect. Undoubtedly, science comes to its fullest fruition when it learns how to measure the phenomena with which it deals. Not until he could measure the voltage of an electric current and the resistance of the various conductors could the physicist give us electric light and power. Likewise must the application of economic principles to the solution of the pressing problems which beset the economic world wait upon the progress of quantitative economic research. Prices cannot be intelligently controlled until we have more information about costs and actual demand and supply schedules so that we may know how great a change in production is necessary to produce a given change in price and what will be the effects of a given change in price upon the possible consumption of the commodity concerned. Effective monetary management cannot be hoped for until we know in mathematical terms the relative importance of the different items in the equation of exchange so that we can tell with some assurance what will be the effect upon the price level of a given change in the quantity of money or credit. Full employment cannot be

assured in a price economy until we know just how many men will be employed in a given industry at a given rate of wages; but neither can these problems be solved unless we know, by means of theoretical analysis, what forces need to be measured and how they are probably related to each other. So, qualitative and quantitative economics, far from being opposed to each other, are complementary tools whose joint employment is absolutely necessary to the perfection of economic science.

Mills' *Behavior of Prices* is therefore significant both for what it accomplished and for what it failed to do. It has not provided, and its method does not seem likely to provide, a body of generalizations which can take the place of the principles already developed by theoretical economists; but it has shown that there is a great deal of information available about prices which can be employed to give tangible evidence and substantial expression to the economic forces with which the theorist deals; and, further, it has demonstrated that in the hands of an ingenious statistician these figures offer possibilities of revealing the movement of subtle economic forces and of giving substance to such intangible concepts as the notion of a general economic equilibrium. Mills' later writings show, further, that when statistical methods are directed toward a particular problem and are supplemented by theoretical interpretation, they can contribute much toward the understanding of contemporary events which might otherwise remain obscure.

The above remarks must not be construed as belittling the importance of institutional studies in economics. So long as such studies are not represented as the open sesame to the understanding of all things economic but are recognized as complementary to other methods of investigation, they can be very useful. Institutional studies are necessary for the

testing and verification of deductive theories and for their modification and restatement if the facts are found to be inconsistent with the theories. This is an important part of scientific work in every field of study. Also, the solution of problems of practical policy requires just such knowledge of social institutions and of the probable effects of proposed measures as institutional and statistical studies can give. But these studies may do more than this. They may yield information about certain kinds of economic phenomena which would not be discovered by other methods. For instance, it is hardly likely that a priori analysis, even though based on fairly good observation, would ever develop a complete and satisfactory explanation of business cycles. A careful chronological and statistical record of the sequence of events in successive cycles is necessary to their full understanding and it is through the analysis of such records, checked and rechecked against theoretical interpretations, that a final solution to the puzzle of business cycles may eventually be found. Mills' data on the sequence of price movements and the facts which he has gathered about the behavior of individual prices during business cycles probably constitute one contribution toward this end. Even the hope of the institutionalists to develop dynamic laws of change comparable to the laws of evolution and heredity in biology is not necessarily illusory. It is quite possible that the empirical study of economic phenomena in movement may lead to generalizations of this sort which will constitute a useful supplement to such dynamic principles as are afforded, for instance, by Marshall's time-period analysis. Mills' generalization that the price system tends to return toward stability after disturbance is perhaps a tentative statement of such a law. However, in the development of scientific generalizations from empirical investigations, it is certain that there will have to be some resort to

deduction and inference—much after the manner of neo-classical theory. The greatest discoveries of science do not spring perfected from the mere observation of historical or statistical facts; they require flights of the imagination and the exercise of rigorous reasoning. The researches of Mills and his fellow institutionalists may well prove to be one of the cornerstones on which the structure of economics in the future will be built but it will rest equally on the foundations laid by the painstaking analyses carried on by deductive theorists for the past century and a half.

COMMENTS BY FREDERICK C. MILLS ON BYE'S *CRITIQUE* AND THE REVIEWS

On the Objectives of Economics. The world of the social scientist has long been a troubled one. The natural scientist could look out upon an objectively existing nature with characteristics of which he might learn through observation but which were essentially independent of man's activities. Natural processes might be shaped to man's ends but the attributes of electrons, atoms, stars, and nebulae were not determined by his behavior. Although he might be introspectively concerned with the mechanism through which sense impressions of nature were obtained, he was a detached observer of natural processes. The phenomena with which the social scientist has been concerned have had objective reality, too: but in the nature of things they have been outgrowths of man's activities; they have been intimately related to man's value-judgments; and they have been directly influenced by behavior standards set by man.

A certain duality of approach has been characteristic of the social sciences as a result of the positive part played by human valuations and humanly established norms in social processes. There has been, on the one hand, the same concern that is

found in the natural sciences with the attributes of externally existing entities such as social and political groups and the organized activities that constitute an economy. Since, however, the attributes of these entities are resultants of human judgments and directed human activities, in a sense not at all true of the attributes of an atom, or a chemical combination, or a solar system,¹ such attributes are subject to substantial changes over time. But the influence of the human factor goes further. Conclusions relating to social phenomena themselves involve value-judgments and normative considerations. The findings and generalizations of the social sciences, therefore, have differed from the bodies of knowledge built up in the natural sciences. Generalizations about the attributes of externally existing social entities are clouded by the fact of constant change in these attributes: descriptive (and explanatory) accounts of observed phenomena are intermixed with appraisals influenced by judgments of the socially desirable.

I do not say that this situation is to be deprecated. The study of economics can probably never be detached and aloof, divorced from human valuations and standards. Inevitably existing systems and procedures will be compared with possible alternative systems and methods. Yet there is a place in economics, and an important place, for the objective study of the system of related activities that constitutes a modern economy. Of course, the questions with which one approaches such a study will grow out of problems involving human needs and subjective standards; the organization and operating characteristics of the system under review will reflect the valuations, the choices and the decisions of millions of human beings, and will be influenced by deliberately exercised group

¹I do not suggest that all natural phenomena are immutable. But human activities do not play the role in variations of weather, or geological change, that they do in social processes.

compulsions; but the system and its elements are open to study as things divorced from the observer, as entities with characteristics capable of objective definition, just as are the attributes of the material universe.

Although this type of investigation constitutes but one of the approaches employed by economists, it has the promise, I think, of promoting that accretionary growth of knowledge that has been so difficult to achieve in the social sciences. The findings relate to an externally existing system; they are based upon observations immediately open to checking and verification by others; the conclusions themselves are valid only to the extent that they may be derived independently by other observers.

These aims are not new in economic science. There have been many contributions directed toward the goal I have just described. *The Behavior of Prices* serves, in some degree, to exemplify this approach. The subject matter will, of course, vary from field to field, but whatever the content, such study of a modern economy, conceived of as a system of related activities extending over time, will have two objectives.

The first is the study of organization or structure. The aim of such study is to develop a systematic description and explanation of the order discernible in the system viewed at a given point in time-order to be found in the interrelations among the elements of economic life. The second is the study of change over time in the relations among these elements and in the attributes of the system into which they enter, a study that may be concerned with comparisons of structural forms existing at specific dates, or with the patterns of change² observable in the temporal modifications of economic life.

²No familiar term conveys the idea of *order in change*. We have in mind regularities in the processes of economic life. *Patterns* has a static connotation, though *patterns of change* is a serviceable phrase. The concept of motricities—"flowing types of order"—is exactly what is here in mind, but the term is an unfamiliar one.

An examination of an economy at a given point of time reveals much that is apparently haphazard and incoherent but principles of order are also discernible. Some degree of form characterizes the agglomeration of men and things that enter into economic activities. Successive generations of economists have sought to define the arrangements of the elements of this entity and to find reasons for the orderliness that gives to this agglomeration the attributes of systematic structure. Various conceptual schemes have been used in these attempts, schemes providing frameworks through which reality may be viewed and its data organized. Many of the traditional categories of economics, such as the fourfold classification of factors of production, represent constructs through which aspects of organization may be apprehended. The system of equations defining the conditions of general equilibrium constitutes another construct designed to serve the same purpose—to permit the identification of regularities that give coherence and order to the economic system existing at a given time.

The second objective is a natural and inevitable outgrowth of efforts to attain the first. The order to be found in economic life at a given time is subject to change, to disturbing modifications. There are serious obstacles to the application to different periods of time of generalizations relating to economic organization. If we wish to take adequate account of the time dimension of economic activity, a somewhat different approach is required. Regularities that unify movements over time are sought. Here the task is one of finding continuities that are discernible amid the flux and confusion of change, determining the manner in which these regularities are impressed upon economic events and discovering the reasons in the nature of things economic for the persistence of order over time. The constructs and concepts that

guide us in organizing the data of economic change must be adapted to these purposes.⁸

If the universes⁴ that exist at different times were completely unrelated, attempts to find principles of order in changes over time would, of course, be vain. If, however, there is some degree of continuity in the process by which the economic structure existing at a given time passes into another structure, if order is impressed upon economic activities by the facts of life—by production techniques, the character and distribution of natural resources, business enterprise, consumption standards, legal institutions—the transitions in question will mark out patterns, motricities, regularities in change. Such regularities may take the form of cumulative or evolutionary change, or of periodic change. So long, however, as the change is orderly, regularities capable of definition would appear. These are of equal concern with organizational form, and the circumstances that lead to the appearance and persistence of motricities are of equal importance with the circumstances that determine structure. Indeed, these two aspects of order are closely related phenomena, to be distinguished, perhaps, for convenience of research and exposition

⁸ The categories here suggested are of course related to the familiar concepts of statics and dynamics but they involve somewhat different treatment of the time variable. The structural view is one that cuts across time; it involves the study of organization at a given time. The concept of statics in the work of Mill, J. B. Clark, and others, envisaged processes extending over time but without change in the conditioning factors—consumers' demands, population, supply of capital, technical procedures, organization of industry, etc. Any process that extends over time carries us from the structural to the developmental in the categories here suggested. No distinction would be made between movements toward static and dynamic norms if such norms could be defined. Each would constitute change over time.

⁴ The term "universe" is here taken to mean a "universe of inquiry," in the sense in which that phrase is used in statistical inference. It denotes a population of things or phenomena having stated characteristics in common, a population generally thought of as infinitely large, since it comprehends all the things or events that would be created if the forces generating the particular things or events were to operate indefinitely, without change.

but fundamentally manifestations of common forces. In studying these forces, we are concerned with the major problem of economics which is, I venture to say, that of defining and understanding the developing structure of economic life.

I do not assume that this development follows laws of purposive evolution or compulsive laws tracing patterns of inevitable change. It is not the unfolding development of Comte, or Spencer, or Marx that is in question. The patterns here referred to are those regularities in economic change that reflect internal compulsions growing out of the wants, natural resources, technologies, productive and distributive organizations of the economy, and external compulsions of social or political origin. They may be patterns of short-term or of long-term change; they may be restricted to minor elements of the economy, or may be uniformities in the broadest aspects of economic change; they may be regularities barely discernible among the discontinuities of actual events, or uniform movements of high consistency. They may have strong or weak bases in a priori reasoning. Knowledge of these multiform, varied patterns, separately and in interrelations, is sought in the study of an economy in operation.

This simple summary of the objectives of economics—the definition and explanation of elements of order in economic life in its twofold aspect—is not, I think, a distorted statement of purposes that have been the concern of economists since the science first took form. The conceptual schemes set up by men of different generations as guides to the organization of observable data have varied with variations in the physical circumstances of life, in the problems men have grappled with, in the mixed product of direct experience, accumulated learning and individual mental processes from which men derive their “a priori” contributions to the operation of research. There have been variations, too, in the uses to

which such conceptual frameworks have been put. The exploration of the logical implications of the elements of such schemes of ideas has furnished a proper and natural basis for dialectic. Again, when a confused and intractable reality offered discouraging obstacles to generalization and understanding, pedagogue and student alike found something of a haven in the orderliness of these conceptual schemes. But always, too, they have been guides to research, outlines or "sights" through which reality might be observed and its data inspected. (And herein, they have been, of course, instruments to their own improvement.) Dominant in all generations have been the need and the desire to find principles of order in the complex of activities that minister to human wants. Knowledge of economic organization and knowledge of regularities that persist over time in the processes of economic life have offered to men means of understanding and control. Such knowledge has been the objective of economic inquiry.

In this section, I have referred to certain of the objectives of economics and have emphasized the study of objectively definable attributes of a system that changes over time. This does no more, indeed, than outline a framework within which deductive and empirical processes of a study may operate, and within which data relating to the organization of economic elements in a functioning system may be assembled and interpreted.

The Methods and Objectives of The Behavior of Prices. There is today in economics, as in any field marked by constant and cumulative change in phenomena, debate and division of opinion concerning the concepts that shall guide investigation. In so far as these differences reflect personal preferences in the selection of subjects of study and instruments of analysis, there is little profit in discussing them.

Since, however, *The Behavior of Prices* is the subject of a case study in the Social Science Research Council's review of research procedures and results in economics, it is proper that the objectives, methods, and results of that investigation be clearly defined, and that the point of view with reference to which the study was planned and conducted be set forth in some detail.

I would not reopen the threadbare controversy over deduction and induction, the a priori and the empirical. There can be no question that it is through a combination of reason and experience that knowledge of the world in which we live is attained and extended, but there are so many possible combinations of these two ingredients, so many variations in the manner in which the mind may go back and forth between the conceptual and the perceptual, that a wide field for difference of opinion remains. In some degree, the choice of a working combination may be a private matter, depending on personal predilection and cast of mind; in greater degree, perhaps, the effectiveness of various combinations of the a priori and the empirical depends upon the subject matter of inquiry and upon the state of development of a given body of knowledge. The history of any science records swings of emphasis in these respects, with intellectual fashions, the dominance of outstanding practitioners, the exigencies of the day and the fruitfulness of prevailing conceptions shaping the activities of workers in that field. With so many variables affecting the results of given procedures, it would be a hazardous thing to suggest to any generation or to any individual worker an optimum combination of the a priori and the empirical or to deny to any worker the choice of his own combination. I do not attempt this.

A review of the history of economic thought and research procedures reveals, in any period, variations in the degree

to which different students emphasize the excogitation of theories and the examination of objectively discernible processes. At any time, one type of work may dominate, sometimes to an extent that restricts the healthy growth of knowledge. The dialectics of medieval scholasticism represented a perversion of the process of ordered discourse which has a rightful place in scientific inquiry. There were dark ages in medicine when there flourished "systems" "so beautifully imagined that they made further search for the causes of disease an impertinence." There have been times when something of the same sort has been threatened in economics, with the elucidation of an accepted body of doctrine tending to constitute economic inquiry, and the study of economics in danger of becoming the study of a body of theory not open to testing or rejection in terms of observable data. I think it fair to say that the dominant economic thought in the second half of the nineteenth century tended in this direction. The greater attention to empirical work that came with the twentieth century, the emphasis on concepts open to empirical testing, was an altogether salutary movement.

In reaffirming that the worker in economics must avail himself of both the a priori and the empirical good, I should stress certain points that seem to me important in the present development of economics. All attempts to gain knowledge must be guided by rational considerations, by conceptions providing a framework for the organization of observations, but observation, perception, experience, must play a major part in economic inquiry. "It is impossible," says Sir Arthur Eddington, "to have a priori knowledge of an objective universe." The body of economic knowledge must be empirically founded and empirically verifiable. Determination of the relevance of hypotheses and the testing of hypotheses must be based on the data of observation, but the two aspects of

investigation are closely related. The interplay between the empirical and the conceptual in an investigation is so close and vital, is such an integral part of the process by which knowledge is acquired, that sharp separation of the functions of "theorizing" and data-utilization cannot conduce to the advancement of knowledge. Further, I would suggest, the concepts that guide research, that provide rational bases for the organization, transformation, and utilization of observations, may vary widely. They may be sharply defined, true-or-false hypotheses; they may be constructs that provide a framework for the organization of data; they may be intimations of the existence of regularities. It is a mistake to think of them as necessarily of the first type, that is, as specific questions to which a yes-or-no answer is sought. More is said below on this matter.

A final point referred to in the preceding section should be here emphasized. It is that the universe of inquiry with which economists are concerned is subject to fundamental changes over time. These changes include cumulative alterations in the organization of economic life itself and modifications in the compulsions from without the economic system to which economic activities are exposed. The operating characteristics of the economic system and, consequently, the data of the science are subject to nonrecurring changes. The science itself, therefore, must remain unfinished, incomplete, with its practitioners seeking always to keep the body of their generalizations from falling too far behind the march of events. Of course, economists will seek enduring principles, and some generalizations of the science may be rooted in relatively unchanging physical and social conditions, but so long as organized economic life is subject to change, the principles of economics will be impermanent and incomplete.

There is no question, then, that inquiry into the nature of

economic organization and of economic processes must involve the interaction of the conceptual and the observational; that the mind must have guides, leads, clues, in seeking principles of order in economic phenomena. Where are economists to look for the intellectual leads, the clues to principles of order, the framework of hypotheses that are to channel researches and provide categories for the organization of the data of observation? To the accumulated economic doctrines of the past, beyond question, to the extent that these are relevant and promise to be fruitful today. That some of these are outmoded is clear; that some were never framed in a manner permitting empirical verification will be granted. That others provide useful starting points for the study of contemporary problems and constitute a frame of reference having clear utility today is unquestioned. The concepts and analytical tools that we find in the bodies of neoclassical and mathematical theory should be utilized to the full. If value is to be had from them we should approach them, of course, as instruments to be used in grappling with contemporary problems, not as theories to be handed, reverentially unchanged, from one generation to another. As instruments, it would be uneconomic and altogether unwise not to utilize them. However, the body of thought labeled neoclassical economics does not constitute the only source of ideas that may be fruitful in approaching contemporary economic problems. A disservice is rendered to economics and to students of economics by those who would restrict the working tools of economic research to those that may be derived from the economic thought of nineteenth century England and a few modern appendices thereto or to any similarly limited body of doctrine.

In refusing to accept the view that the only respectable and proper starting point for economic theory is a rather

tight, formal body of theory that is expounded in textbook and classroom, one does not have to reject that body of theory. I would say that the inquirer into the organization of economic life today should be free to draw from wider sources in seeking concepts promising to be fruitful in research. Contemporary philosophy, anthropology, biology, and morphology, sociology, the logic of Peirce and Dewey, and modern psychology have much to offer the economist. The understanding of the processes of economic life, not the development of a closed, consistent body of theory, is the objective of economic research. If diverse, even inconsistent hypotheses provide helpful clues to this understanding, the economist should be free to employ them.

The Behavior of Prices does not open with a pro forma statement of hypotheses. It presents, however, what I believe to be a fairly clear indication of the conceptual framework into which the factual study was designed to fit and which provided the guiding principles of the enterprise. I shall here review this conceptual background, repeating certain statements made in the book and making more explicit some of the specific issues that concerned me in that study.

I have referred above to the varying bases of scientific inquiry. An investigation may be directed toward the testing of a quite specific theory, e.g., the hypothesis that recurrent periods of prosperity and depression are due to variations in the intensity of sunspots. It may be directed toward the more exact determination of some constant, such as the atomic weight of an element or the speed of light. Such origins lead to sharply pointed studies, clearly defined and unambiguous. Again, as I have noted above, an investigation may represent the exploration of a somewhat general concept, a concept involving a framework of hypotheses, some capable of sharp definition, some representing, perhaps, merely intuitive ap-

prehensions of regularities believed to be open to study and determination. We unduly simplify the actual character of scientific inquiry if we conceive of all such inquiries as directed toward the acceptance or rejection of precisely formulated hypotheses. A wide diversity of ideas, adumbrated generalizations, conceptual situations may provide the starting points of inquiries concerning the natural or social world in which we live. These initial concepts may remain unchanged during the investigation, or they may suffer major alterations as the study proceeds and as the existential situation reacts on the conceptual.

The inquiry discussed in *The Behavior of Prices* was not concerned with the supporting of a specific thesis but it was directed toward certain definite goals.¹ The concept of prices as a complex system of related parts, a system possessing some attributes of unity, some persistent regularities of behavior, some orderliness, and much variation, provided the rough outlines of the mental construct that was to be explored and set against the data of observation.² Here was a system existing objectively, marked by some known uniformities, and playing a vital part in the economic processes of production, exchange, and distribution. The further definition of elements of order in this system and of regularities that persisted over time promised to aid our understanding of economic processes in general. Determination of the degree and character of variation in this system would add, similarly, to our understanding of it.

To this general objective, certain more specific purposes were added, purposes that provided direction in the selection, arrangement, and interpretation of observations relating to price behavior. Variations over time in the level of prices, in the buying and selling relations that define the terms of physical exchange between individuals and groups, and in

the attributes of prices in the mass, reflect (and in some degree cause) alterations in the interrelated economic activities of an economy. The study of stability and instability in economic organization and economic processes at large may be approached through the examination of these variations. Prices constitute one of the most readily observable aspects of economic life, and changes in prices and in price relations are intimately related, both as effect and cause, to changes in the physical processes of the economy. The study, therefore, is focused on stability and instability in the system of prices. This problem was broken into three parts—instability of the price level, instability in the relations among prices, and instability in commodity prices viewed collectively and appraised with reference to their group attributes.

The results of the investigation bore upon a number of questions not mentioned above, but the guiding principles of the study were provided by the concepts and problems I have mentioned. As is clear from the preceding general statement, and from Mr. Bye's summary of the contents of the book, not all of these questions are pointed directly toward problems discussed in neoclassical economics, but all the questions explored are of importance in seeking to understand the organization and working of a modern economy.

This brief statement of the concepts and problems with which the study was concerned would be incomplete without reference to the methods employed. In this opening study, considerable emphasis was placed upon experiments with procedure. I believed that study of the problems of economic instability, toward which the enterprise was pointed, involved the development of appropriate procedures as well as the accumulation of observations and the formulation and testing of hypotheses. Accordingly, I sought methods by which both the individual and group behavior of commodity prices might

be defined with precision. In good part, this phase of the study was concerned not with the truth or falsity of given conceptions, but with the efficacy and fruitfulness of certain experimental procedures with which I was working. Improvement of the procedures through which the data of observation may be organized and analyzed is no less important in the development of a science than is the elaboration of the body of hypotheses that make up the conceptual scheme. I share here, fully, the view Mr. Mudgett has expressed in his review. The diversification and enrichment of methods of attack on economic problems can contribute substantially to the advancement of economic knowledge.

The specific objectives of the investigation require somewhat more precise formulation. The Social Science Research Council, in making plans for this and similar discussions of contributions to the social sciences, has placed emphasis on the definition of the author's objectives. I venture, therefore, to supplement the preceding general remarks with a statement of the immediate purpose of the study of which one phase was reported upon in *The Behavior of Prices*.

A memorandum submitted to the Executive Committee of the National Bureau of Economic Research in explanation of proposed price studies, and appearing in the minutes of that committee for January 12, 1925, contains the following statements:

The proposed studies fall in two closely related classes. Those in the first group deal with the price system as a separate economic institution. It has been recognized that prices as a whole constitute a coherent, unified system, with well defined characteristics and distinct component parts, but the internal structure of this system and its relation to the production and movement of goods are as yet imperfectly understood. It will be the aim of research in this field to supplement existing knowledge of the structure and functioning of the price system.

Studies in the second group relate to the prices of individual commodities. There is need for quantitative analysis of the process of price determination, with the object of measuring the relations between

market prices and the various cost and quantity factors which determine these prices. Such investigations, carried on in a number of lines, should provide a realistic account of the interplay of price-determining forces, and should throw light on the general economic problem of value.

The same memorandum contained more detailed outlines of these two fields. The first outline related to the subject matter of the report later published as *The Behavior of Prices*. The following topics were noted in the second outline:

- II. A study of price determination.
 - A. Statistical analysis of the relations between prices and quantities produced, consumed, carried over, imported, etc., for a number of important commodities.
 - B. Study of relations between costs and prices.
 - C. Grouping of commodities into significant classes, based on demand conditions and other price-determining factors.
 - D. A consideration of the accepted theory of value, in the light of empirical results.

The Introduction to *The Behavior of Prices* states:

The objectives of the present study are, first, to secure a fuller understanding of the behavior of individual commodity prices and, secondly, to increase our knowledge of the working of the price system, and of the interrelations between its component elements. (p. 31)

The concluding chapter contains these statements (pp. 437-438):

The immediate objectives of the present study are the development of a method of analysis and the accumulation of a body of facts which may conduce to an understanding of the price system. This volume has dealt with the following aspects of the general problem:

1. The behavior of the prices of individual commodities, and the relations among different attributes of commodity prices.
2. Regional differences between commodity prices, and differences from market to market in the behavior of prices of individual commodities.
3. Certain problems related to the measurement of changes in the general level of wholesale prices.
4. The measurement and significance of price dispersion and price displacement.
5. The effect of changes in the price level upon relations among the prices of individual commodities.

6. The characteristics of the population of prices, as revealed by the behavior, in combination, of price relatives and of measures of variability, trends and cyclical movements in the prices of individual commodities.

No attempt has been made, in presenting the results of this study, to support a specific thesis. The investigation has been looked upon as part of a general attack upon the problem of charting the price system, defining its elements, determining the nature of the changes that occur in the price system with the passage of time and with changes in general economic conditions, and of describing more exactly the part which the system of prices plays in economic processes. This view of the relation of the present study to the broad task of surveying the price system has conditioned the plans for the investigation and has determined the form in which the results appear.

It is proper to say that the above statement of objectives covers a study broader than that represented in *The Behavior of Prices*. When the work had proceeded for something less than two years, it seemed desirable that the results then in hand and suited to separate publication be released. These results were of two types. They included a variety of measurements relating to aspects of the behavior of prices of individual commodities at wholesale. Some of the interrelations among these measurements had been explored and some attention had been given to behavior differences among commodities and markets. These individual measurements of aspects of price behavior having economic significance for a variety of purposes were thought of as the basic materials for use in the investigation to follow. Although we had as yet only partially exploited these measurements, it seemed well to make them generally available.

The other results related to combinations of the entire body of observations available to us. The general system of prices, as I elsewhere suggest, possesses many component parts, all interrelated, but each with attributes of its own. Among these component parts, one of marked importance for the student of market processes is that which is made up

of commodity prices in wholesale markets. Although lines run from this system to the system of security prices, retail prices, factor prices, etc., there are close interrelations among wholesale prices, and there is justification for treating this body, for some purposes, as a homogeneous entity. (In its turn, of course, the system of wholesale prices possesses subordinate parts, with special attributes. These are referred to below.) Accordingly, one stage of the study outlined was to deal with the characteristics of wholesale commodity prices treated as an entity. It is this system that is discussed in Chapters III and IV of *The Behavior of Prices*.

The results published in that book, therefore, did not deal and did not profess to deal with two fields of study included in the original plans. These were the study of price determination and the study of component elements of the general system of wholesale prices.

The phases of the proposed study of price determination, with reference to value theory, have been noted above. It became clear very soon that this enterprise could not be carried through on a scale that would yield measurements relating to a number of commodities and suitable for combination in the study of the attributes of the price system. The investigation of price determination called for intensive cultivation of limited areas, a procedure of clear and unquestioned value, but not in the direct line of attack on the first objective I had set myself, i.e., consideration of selected attributes of prices in combination. Accordingly, the presentation of materials in *The Behavior of Prices* included merely illustrative measurements of the flexibility of prices and related measurements defining price-quantity relations. This limitation was definitely set forth in the book.⁵ There was the more reason thus

⁵ "This section [i.e., the section on relations between prices and price-determining factors] differs from those which have preceded it in that no compilation of measures relating to a number of commodities is attempted. A brief

to limit the attention given to price-determining factors in that Henry Schultz was then beginning the brilliant series of studies culminating in his book on *The Theory and Measurement of Demand*. Schultz's studies have led to the derivation of statistical demand curves for sixteen commodities.

The original plans of the National Bureau regarding cost-price relations have been pushed subsequently, from one direction in the analysis of the prices of manufactured goods,⁶ from another in the special studies of cost functions being made for the National Bureau by Joel Dean.

The second subject deliberately excluded from *The Behavior of Prices* was the study of various elements of the system of commodity prices at wholesale. In the Introduction, I wrote:

In a later study the materials assembled in the present volume will be used, with other data, in seeking to define certain of the component elements of the price system, and in attempting to trace relations between these elements. (p. 34)

Elsewhere appears the statement:

The final stage of the study, and the most important from the point of view of one interested in the elements and internal structure of the price system, is the isolation and analysis of price groups, and the testing

account is given of methods which have been developed elsewhere and several examples are included as illustrations of procedure. So much is necessary in any complete account of measures of price behavior. A collection of measures of the type here described, suitable in quality and quantity for a study of group behavior, waits upon the future" (p. 140).

"In the treatment of this aspect of price behavior, the inadequacy of the data renders it impossible to secure measures relating to a considerable number of price series" (p. 160).

This is a clear and conscious limitation. Mr. Bye's reference to "the superficial treatment given . . . to the relation between prices and price-determining factors" and his statement that this subject was "slightingly dealt with" are criticisms for not doing something I did not attempt or profess to do. His statement that "The study of prices in the mass . . . is not a substitute for studies of individual price-making forces" is right, but curiously irrelevant.

⁶ Cf. *Economic Tendencies in the United States and Prices in Recession and Recovery*.

of various grouping principles. The data described in the present volume furnish some of the basic materials for such an analysis. The detailed account of this part of the study is deferred to a second volume. (p. 219)

There was thus no effort to apply grouping principles to the individual measurements derived in the first chapter and dealt with in combination, as undivided wholes, in the last two chapters.⁷ This was looked up as a task upon which a later report would be made. In fact, no single subsequent report comparable to *The Behavior of Prices* has been made upon this subject. A considerable number of experimental classifications had been set up at the National Bureau when *The Behavior of Prices* was being written and considerable use has been made of these in later publications.⁸ Their use, in a comprehensive survey of the component elements of the system of prices, waits upon time and circumstance.⁹

⁷ In the appendices and certain of the charts, the detailed measurements presented were listed, for convenience, in accordance with the classification of the U. S. Bureau of Labor Statistics but this use did not involve any testing of the principle upon which that classification was made nor any acceptance of it as necessarily significant.

⁸ These classifications, based on considerations relating to the forces acting upon goods of different types, from both supply and demand sides, include such groups as: raw materials and manufactured goods; raw materials, semi-manufactured and finished goods; durable and nondurable goods; producers' goods and consumers' goods; goods destined for use in capital equipment and goods destined for human consumption; crops, animal products, forest products and mineral products; American farm products, and commodities other than American farm products; agricultural and nonagricultural products; organic goods and inorganic goods; foods and nonfoods; commodities of which the short period output is subject to a high degree of control (by their producers), a moderate degree of control, and slight control; manufactured goods in the fabrication of which wages constitute less than 10 percent of value of product, from 10 to 25 percent, and more than 25 percent; manufactured goods in the fabrication of which "value added" constitutes less than 25 percent of value of product, from 25 to 50 percent, and more than 50 percent; commodities of American origin and commodities of foreign origin; commodities of which less than 2 percent, from 2 to 20 percent, and more than 20 percent of the American output is exported.

Various cross classifications of these categories yield a very large number of subgroups having stated characteristics in common. The extent to which study by cross classification is possible is limited, of course, by the size of the sample available.

⁹ Mr. Bye speaks of my unwillingness "to be guided by groupings of the data,"

The preceding account of the conceptual background and the more direct objectives of the study here in question may be read in connection with Mr. Bye's discussion of my approach and purposes. I do not believe that any detailed rejoinder is required but it is in order to comment on three of the points made by Bye. The following ideas appear in various forms in his *Critique*:

1. Mills is an institutionalist.¹⁰
2. The investigation reported in *The Behavior of Prices* was planned and conducted without benefit of deductive theory. It was strictly empirical, purely inductive, uninfluenced by theoretical concepts or abstractions.¹¹

and states that I do not attempt to examine the behavior of price groups. Mr. Viner says ". . . when [Mills] deals with prices in combination, he bases his subgroupings only on chronological criteria or on differences of quantitative behavior. With the minor exceptions referred to above, he avoids any other grouping of prices that might be suggested by economic theory or hypothesis, and he refrains from any search for relations between the economic characteristics of particular commodities or subgroups of commodities and their price behavior." (Pp. 167-68, Viner's review. Page references are to Viner's and Mudgett's reviews as paged in this volume.)

In view of the several statements in the book (and a further statement by letter to Bye) to the effect that the application of grouping principles and the study of component elements of the system of prices was definitely contemplated, but was not undertaken in the study in question, it is difficult to understand these judgments.

I discuss, later, the validity of treating the system of wholesale prices as an entity.

I should note that Viner's statement that I based subgroupings "on differences of quantitative behavior" is not accurate. Measurements for individual commodities were listed in order of magnitude, and, of course, the construction of a frequency distribution involves quantitative distinctions along both scales, but no principle of grouping was based on quantitative differences. The quantitative bases of the classifications listed in the preceding footnote, which are primarily designed to reflect qualitative differences, do not relate to "differences of quantitative behavior."

¹⁰"Although Mills does not appear definitely to have labeled himself such, it is fairly evident that he is a member of the institutional school of economists." (Bye's *Critique*, p. 10 above).

¹¹". . . the term 'normal' smacks too much, no doubt, of neoclassical doctrines to suit his institutional temperament" (p. 20).

"[Mills] in common with the more extreme institutionalists" (p. 81).

¹²Various quotations from Bye's manuscript bear on this allegation: "Mills has little sympathy with deductive theory. Therefore he was evidently bent

3. The real purpose of the study was the building of a new science of economics, the derivation of scientific generalizations of an entirely different character from those developed in the past, the replacing of the traditional theory of value by a new body of scientific generalizations.¹²

I am somewhat baffled by the charge that, although I have never avowed it or known it, I am an institutionalist. I am afraid that I must disclaim the honor, not least because I do not know what responsibilities it entails. Such tags may have their advantages for certain purposes but they serve, also, to interject irrelevancies, to cloud discussion, and, for those who have tropismatic reactions to such terms, to lessen objectivity.¹³

upon reaching his conclusions by strictly empirical methods, and did not want to introduce into the analysis any theoretical concepts or abstractions." (p. 56).

"... a new economics is to be built up by statistical methods and historical studies." (p. 13).

"... he was attempting to establish scientific generalizations about the price system by a strictly inductive approach." (p. 68).

"Here [i.e., in later works] is no longer the strict statistician bent upon building a new science of economics out of empirical data" (p. 80).

"Where the investigator proceeds with a bias against the use of deductive logic" ... [as appears to Bye to be the case in *The Behavior of Prices*]. (p. 87).

Mills "would probably not have much enthusiasm for hypotheses." (p. 75).

"By studiously avoiding the principles of economic theory, Mills lacked a chart to guide him. Believing that the most fruitful approach to an understanding of economic phenomena is to seek generalizations by the actual observation of statistical data, he laid himself open to the criticism that he merely took a mass of facts and applied to them almost every kind of statistical technique at his command, in the hope that he might hit upon some significant relationships." (p. 71).

¹² Mills "wanted his book to be a pioneer contribution to the development, by purely inductive methods, of a body of scientific generalizations which might replace the traditional theories of value, which he regarded as unsatisfactory." (p. 65).

"... the hope of reaching scientific generalizations of an entirely different character from those which have hitherto been developed (p. 12).

"... bent upon building a new science of economics" (p. 80).

¹³ My disinclination to use the term 'normal' to define a certain concept (a concept far from identical with Marshall's normal value) seems to Bye, curiously enough, to have been evidence of an institutional cast of mind. If this be the test, I find some satisfaction in the thought that the mantle of institutionalism is broad enough to include the editor of the *New Yorker*, a notable ally in any great cause. He writes, commenting on the ambiguity of a reference to 1926 as a 'normal' year, "Normalcy, like love, is old yet ever

The second count deserves more extended consideration. As there are degrees of sin, so are there differences among research workers in the emphasis they place upon empirical elements in the complex of procedures by which advance of knowledge is sought. Bye and I would stand at different points on this scale.¹⁴ But the statements I have quoted from Bye, given in the flat and unqualified form in which they appear in most sections of his essay, fail to represent my thoughts. My own view has been made explicit in the book under review, and elsewhere.

The general framework of *The Behavior of Prices* is set forth in the Introduction to the book, and again, in the opening pages of Chapter III. It is not the kind of framework Bye has been accustomed to, nor is it in the main centered about problems of the type he has been familiar with. But to say that a frame of reference is lacking, that this is strict empiricism, pure induction, giving evidence of a bias against deductive logic, is seriously to misapprehend the record available to him.

The methods employed in *The Behavior of Prices* were not, as Bye implies, selected at random in the hope that significant relationships might turn up. Being concerned with instability of the price level, internal instability (arising out of shifting internal relations in the price system), and with possible group instabilities arising out of the interplay of

new. It is the imponderable, haunting element in the statistical pudding. . . . Normalcy is a memory, a wisp, a piece of old lace, a crushed petal between the pages of a book." *New Yorker*, July 9, 1938.

¹⁴It is pertinent to note that Bye has, in the past, favored the separate development of 'pure' and 'descriptive' economics. He has said, speaking of the better organization of statistics-gathering facilities, ". . . the knowledge of facts about employment, efficiency, income shares, prices and other phenomena that it gives us will afford the material for an applied science of economics that is not now possible. . . . This applied economics must be developed parallel with but distinct from pure economics as a merely descriptive science." "Some Recent Developments of Economic Theory," in *The Trend of Economics*, edited by R. G. Tugwell, New York, 1924, p. 299. The advocacy of such a separation seems to me to be wholly unwise.

forces influencing the diverse component elements of the group, the selection of measurements to employ followed naturally. It was purposive not random selection. Price relatives of the ordinary type, measures of variability, measures of the direction and rate of secular change, measures of the timing, duration and amplitude of cyclical fluctuations, measures of price flexibility and measures of regional differences in prices and in price behavior were of clear significance in relation to the general purposes of the study.

Mr. Bye has discussed the guiding principles of the investigation as he understands them. His presentation falls somewhat short of giving an accurate reflection of my views on method in economic research. I venture to quote certain general statements concerning methodology that were made in other publications of mine that Bye consulted or was familiar with.

. . . pure deduction and pure induction are alike unworkable as scientific methods . . . there is an inductive residue in even the most abstract piece of scientific reasoning . . . the foundation of a universal generalization upon the mere multiplication of instances is quite invalid.¹⁵

A statistical induction resembles a universal induction in that it can never carry complete confidence unless there be present some a *priori* element, some think in addition to the mere statistical evidence. (*Ibid.*, 65).

¹⁵ "On Measurement in Economics" in *The Trend of Economics*, edited by R. G. Tugwell, 1924, p. 54. Mr. Bye was a contributor to this volume. The following sections of this essay dealt with the relative importance, in given fields, of deduction and induction. These conclusions were suggested:

"The mechanical concepts of direct causal relationship and of invariant law do not accord strictly with facts in any scientific field. Infinite knowledge and perfect certainty being impossible, some elements of ignorance and doubt must always be present in scientific reasoning and prediction. But in certain of the physical sciences these elements are reduced to a minimum, and the concept of mechanism comes close to actual realization. In such fields, where the notions of perfect causality, absolute sameness and certainty most closely approximate the truth, the amount of inductive evidence needed to support a train of deductive reasoning will be small. Such inductive evidence will always be required, but the amount of such evidence necessary as a foundation or a verification of an argument will vary directly with the number of causes in operation and the degree of variation in nature.

To accept . . . the view that in this field it is the function of the economist to solve the significant real problems that arise as a result of economic change is not to say that there is no place for theory. It is to turn attention to a search for different kinds of theories. . . . It is to look for many theories, not one. . . . There is . . . a very real danger in this direction, a danger that the body of dynamic theory should be nothing more than a medley of *ad hoc* hypotheses. A theory should aim to provide an integrated view of the working economy as a whole. . . . That theory is erroneous which is posited upon a degree or type of unity which the facts do not possess; that theory is inadequate which fails to take account of real relations among the phenomena in a given field. ("The Theory of Economic Dynamics as Related to Industrial Instability," *Amer. Eco. Rev. Supp.*, March 1930, p. 34). The discussion here cited is referred to by Bye in his *Critique*.

The instrument of logical deduction may yield an orderly theoretical structure, the counterpart of which it is difficult to find in reality, while a frontal attack on reality may reveal nothing but disorderly and disconnected phenomena. In steering between these two extremes economists must lean heavily upon their scientific inheritance. But precious as this is, something more is required. Probably no economist would deny today the need of a mode of attack better adapted to the study of contemporary conditions than is that provided by the body of theory which had its origin in the conditions and problems of the nineteenth century and in the preconceptions of nineteenth-century thinking. ("On the Changing Structure of Economic Life," in *Economic Essays in Honor of Wesley Clair Mitchell*, New York, 1935, p. 358). Mr. Bye quotes the last sentence of the preceding extract, in the *Critique*, but fails to note the bearing of the sentences immediately preceding upon his general discussion of my attitude toward method.

There is . . . in every statistical induction an *a priori* element. The statistical conclusion can never stand completely on its own feet. It must be endorsed by reason and judgment if it is to carry conviction.

"It follows from this that as scientific method invades fields in which large errors are involved in applying the mechanistic concept, in which a multiplicity of causes operate and in which there is a high degree of variation in the data, a much larger body of inductive evidence is needed. The premises will be tinctured with a larger element of doubt, the initial degree of probability will be less, and therefore more facts and more varied facts will be needed to establish a reasonable degree of probability for the conclusions. Induction must play a commanding role in investigations in these fields in which the elements of approximation and probability bulk large. Where we must phrase our laws in terms of "tendencies," where "other things must be equal," where laws must be qualified because the effects of single causes cannot be isolated, and where the operation of many factors in a given case must be recognized—in other words, where the statistical view of nature applies in its fullness—induction can never be merely a halting companion to deduction but must play a dominant part in scientific investigation." *Trend of Economics*, 54-55.

If a high positive coefficient of correlation were secured from the study of a sample relating to banana importations and the suicide rate, this would not furnish convincing evidence of a causal relation, or a relation of contingency, between these two variables. There would be no reasonable basis for assuming that, in the larger universe of phenomena from which the sample was drawn, there would be uniformity with respect to this relationship. (*Statistical Methods*, New York, 1924, p. 551.)

I should note that a number of these statements antedate the investigation discussed in *The Behavior of Prices*, which was begun in 1925, and ended, so far as that book was concerned, in 1927.

Bye's procedure, in discussing the inductive-deductive, empirical-rational aspects of the investigation in question is rather equivocal. He makes numerous statements to the effect that the methodology employed was purely empirical, purely statistical, quite unguided. Near the end of his essay, however, he qualifies the earlier remarks. The brief and muted qualification, coming after the numerous contrary statements, does not serve to dispel the impression given by the earlier remarks. On page 71 following the statement I have already quoted at the close of footnote 11, above, Bye writes:

However, this is too severe a criticism; Mills did not set out to study the world of prices without a problem or hypothesis in mind. He was evidently interested in the problem of price instability, and upon this he centered his investigation, for the most part. The type of data he selected and the statistical methods he employed were no doubt suggested by their appropriateness to this problem.

This is comforting, but it is hard to understand how Bye reconciles these statements with the many preceding that picture the investigation as unhallowed empiricism, mere statistics, pure induction.

The explanation doubtless lies, in part, in the fact that theory is identified by Bye with neoclassical theory, deduction means reasoning from the premises of neoclassical theory,

and the only conceivable instruments for use in guiding economic analysis and research are the concepts that make up neoclassical economic theory. The introduction of unfamiliar concepts is disturbing. Indeed, when 'theory' means to one a particular body of doctrines, research guided by other concepts is by definition nondeductive.²⁶

In respect to the third point I have noted, I believe Mr. Bye has also presented a distorted picture of my purposes. The objectives I had in mind were broad enough, to be sure, but they were modest in comparison with the grandiose objectives Bye is able to discover—the building of a new science of economics, the derivation by pure induction of a new body of scientific generalizations. It is true, he cites my 'openly avowed' objectives (Bye, p. 14) but he is able, by inference, to discern revolutionary under-cover designs.

The objectives and general approach of the study have been discussed above. Bye's assumptions concerning the more general objectives of *The Behavior* study are not, I believe, justified by anything said in the book itself or in other publi-

²⁶ I would not have these statements interpreted as a repudiation of neoclassical economic theory. As Bye has indicated, I have asserted that many of the problems of contemporary economic life lie outside the bounds of that body of theory. Within the field of problems to which that body of theory is relevant, the instruments it provides are capable of improvement. So much, I think, is really not open to question. But nowhere have I repudiated that body of ideas, nor intimated that I considered it 'futile, nineteenth century abstraction' (Bye, p. 12). I do not (as Bye says in quoting me) 'grudgingly admit' (Bye, p. 11) that "the search for principles relating to the behavior of rational beings confronted by conditions of scarcity, and the exploration, in abstract terms, of the concept of economy have been and of course remain fair fields of economic inquiry." I assert it unequivocally. But I believe that such activities constitute only one phase of the economist's work in attempting to describe and explain contemporary economic life. There is much in the organization and operation of modern industrial systems that cannot be understood through the explication of the behavior of economic men rationally weighing utilities and disutilities.

Bye's detachment in respect of these matters is in some degree impaired by an ultrasensitivity to comments hinting at possible limitations of the body of traditional theory. Those pledged to "systems," says Dr. Peyton Rous, suffer much for conscience's sake, and lead uneasy, contentious lives.

cations of mine. I am not and was not bent upon building a new science of economics out of empirical data. Nor did I seek by purely inductive methods to build a body of scientific generalizations that might replace the traditional theory of value. Of course I sought new knowledge; of course, I sought to make a 'constructive contribution to economic science.' Naturally, I thought the existing body of economic theory and the existing fund of economic knowledge were subject to considerable improvement, both by way of modification and addition. But I must disavow the naïveté that Bye would credit me with. It is fair to ask that the book be judged with reference to my own more modest objectives.

On the Study of Prices as a System. In several papers that appeared subsequently to the publication of *The Behavior of Prices*, I discussed some problems of economic research and considered the relation of certain traditional doctrines to research procedure. One of the points made has a bearing on the interpretation of the work here under review. I quote from an essay printed in 1935:

A more fruitful starting point for economic theorizing may be afforded by the economy, in operation, than by the economic man and the utilities and disutilities which have been alleged to motivate him.

The formal systems of economic thought developed by nineteenth-century economists had their roots in the individual. It was with the pleasures and pains of the individual, or with the utilities and disutilities attaching to consumption and exertion, that the chain of economic reasoning began. From the interactions of all the individuals in a given economy, each aiming to secure the maximum of utilities and the minimum of disutilities, a theory explanatory of the structure and functioning of the complete economy was evolved.

It is true that the economy, not the individual, has always been the major object of interest to economists. . . . But in the search for ultimate causes theorists were led back to the individual, to his economic actions and to the psychological motives which impel him. The potential actions of the individual, as represented by individual demand schedules and individual supply schedules, furnished the bricks with which the structure of economic theory was erected. Since this logical process started thus from the individual, and worked out from the individual choice

to the ultimate structure of the economy as a whole, it was inevitable that the derived body of principles should not apply rigorously to the actual data of economic experience.

There are two essential weaknesses in this procedure. The first is that the argument started with one set of data (individual choices), but was directed toward the explanation of a completely different set of data—market prices, wages, interest rates. . . . As a direct result of this procedure, many accumulations of statistical materials bearing on the working of the economic system were rendered irrelevant to the doctrines of economic theory. . . . The appeal to facts, the basic resource of a scientific theory, was denied, because the facts available were largely irrelevant.

The second weakness of a theory thus working out from the individual choice is equally fundamental. The data of economics (the price of potatoes on a given day, the rate of interest on a certain date) are the result of a great number of individual actions, and it is the mass, not the individual, which concerns us. The mass must be studied as a whole, and its properties as a functioning unit ascertained. . . . The true and measurable data of economics, not the hypothetical data concerning the behavior of individual economic men, relate to the attributes of a population, of a system as a whole. It is these attributes which are the concern of economists." To which was added a footnote:

This is not to say that the desires of individuals and changes in these desires are of no interest to the economist. They are of the greatest interest. But for the purposes of realistic economics these phenomena must be studied and measured in the aggregate, in their effects upon the direction of expenditure of the national income, upon the production, consumption, and prices of specific goods, and upon other economic processes.¹⁷

These quotations have some bearing on the procedure employed in *The Behavior of Prices*, and Mr. Bye quite properly refers to the concepts set forth in this article. However, he has been led into a rather serious misconception which affects his interpretation of the study he is reviewing. His first references to the point at issue are accurate (see p. 11). He notes that by "mass phenomena" I mean such things as market prices, wages, and interest rates, and that the criticism of neo-classical theory here is that it has made the choices of individuals its building blocks. In his later references, however,

¹⁷ These paragraphs are extracts from an article "On the Changing Structure of Economic Life," in *Essays in Honor of Wesley Clair Mitchell*, New York, 1935, pp. 358-60.

Bye takes my various references to individuals—meaning individual persons—to mean component elements of the economy. He speaks of “Mills’ belief that there is little to be gained by the analysis of individual elements in the economic organization. It is his view that it should be studied as a whole” (pp. 27-28). Later, Bye speaks of the necessity of breaking a complex problem into its parts, “for analysis of the elements which compose it” (p. 83), and urges that the study of prices in the mass “is not a substitute for studies of individual price-making forces” (p. 71). He refers (p. 69) to the fact that:

that part of the price system which he does study is treated as a whole, to the exclusion of its parts. This was apparently deliberate on Mills’ part. It will be recalled that he believes that traditional theory erred in approaching economic problems from the standpoint of their individual elements, arguing that the whole has properties of an order which cannot be described in terms of the behavior of its particular parts. So, although he has worked out various measures of individual prices, he does not attempt to follow up those measures with a detailed examination of the behavior of individual prices, or even of price groups.

Three comments are in order with respect to these statements.

(1) I have said nothing to the effect that individual elements of the economy should not be studied. The choices of a hypothetical economic man seemed to me to be a poor starting point for an explanation of how a modern economy works. I did not believe that such choices constituted “elements” accessible to objective study. However, it is fantastic to think of a science taking only an aggregative view of all phenomena within the field in question, or an economic science having as “data” “the economy as a whole.” (Bye, p. 11). Obviously, analysis will carry beyond the aggregate to its elements. The statement that the behavior of the whole is not to be explained in terms of the behavior of the individual elements that make it up, that the attributes of the whole are not to be arrived at by summing the attributes of the sepa-

rate parts, does not mean that study should be restricted to the whole, "to the exclusion of its parts."

(2) No element can be properly appraised, nor its working understood, without reference to the entity of which it is a part. Research may be focused on a particular part of the total but the effectiveness of the consideration of that part will be greater if it is viewed in perspective, as an element of a total. It is a risk of specialization, in economics as in medicine, that the organism may not be seen as a whole.

(3) I do not understand Bye's statement that I deliberately excluded the study of parts of the price system—that the behavior of individual prices or even of price groups was not part of the enterprise. On August 29, 1938, in response to an inquiry from Bye, I wrote him:

Later sections (of *The Behavior of Prices*) aimed at the definition of some of the attributes of prices viewed collectively—attributes of the price system. An obvious next step was the description of the elements of that system, the determination of groups of commodities marked by distinctive modes of behavior. The first book (i.e., *The Behavior*) did not contain any of the results of this group study, although this has been from the beginning an essential part of the undertaking. In both the books that followed, I made considerable use of the results relating to group behavior—but no systematic account of the elements of the price system has yet been published.

Indeed, Bye's own essay contains a statement of my intentions to define certain elements of the price system, and analyze price groups (p. 76). I have elsewhere¹⁸ made the point that "The aim of that economic research which would be most fruitful in a practical and immediate way must be the limited generalizations which relate to specific groups and specific conditions." To say that study of specific conditions must take account of the whole, of which these conditions are a part, and that knowledge of the attributes of any entity which is compounded of subordinate parts must be sought through

¹⁸ "On Measurement in Economics" in *The Trend of Economics*, p. 70.

study of the entity and not through the summation of the attributes of its parts, is far from saying that only the whole is worthy of investigation.

The preceding discussion has relevance to one of the most important issues raised by Mr. Bye, and before Bye by Mr. Viner, in an early review of the book. This has to do with the validity of the procedure employed in *The Behavior of Prices* in treating commodity prices, and more particularly wholesale prices, as a system possessing economically significant attributes and open to investigation by means that do not directly bring in other elements of the price system and nonprice factors in the economic system.

Both Bye and Viner recognize my own specific statements to the effect that prices do not constitute a system apart, and that wholesale prices are but one element in the total price system. It is in order to cite one or two relevant quotations:

The prices of individual commodities, in their changes, stimulate or retard production and consumption, and, in turn, reflect changes in production and consumption. *The Behavior of Prices*, p. 31.

Alterations in price relations may be due to changes in the quantity of money in circulation, to changes in manufacturing and marketing methods, to a temporary shortage or surplus of goods, to any of the thousand factors that affect economic processes. The causes of internal instability are not, of course, found exclusively in the price system itself. Perhaps the chief causes are external to that system. To the extent that external causes are responsible, prices constitute merely the medium in which changes in market relations are reflected. Back of changing price margins and shifting price ties are alterations in economic relationships, alterations which may be slight or profound. These are the ultimate objects of interest. *Ibid.*, p. 215.

The elements entering into the complex net which is the price system are not, of course, restricted to commodity prices. The prices of human services, of land, of capital, and credit, the prices of business enterprises themselves, constitute elements of the price system, and each of these has its unique features. The present study is confined, with minor exceptions, to the system of commodity prices. *Ibid.*, pp. 213-14.

There is, of course, a close relation between the problem of price stability and that central problem of economic theory which is concerned with general economic equilibrium. In studying price instability

we are concerned with the monetary aspects of this classical problem. *Ibid.*, p. 217.

These statements are explicit. Mr. Viner takes them to mean that I "concede . . . that there are other things in the economic order than prices," but he believes it necessary to state that

To the price theorist, wholesale commodity prices are *an* element in the economic system, as are other classes of prices,—prices of the factors of production, retail prices, prices of services, and as are also demand and supply functions, market structures, the technical conditions of production, and ultimately the wants and aversions, the habits and customs, of mankind. [And that] The theorist would agree with Mills that "no one price is an isolated, independent phenomenon" (p. 213), but he would assert it with equal vigor of wholesale commodity prices in combination. . . . (p. 168, Viner review.)

Happily, Viner and I are here in complete agreement. Neither of us believes that the price system in general, or wholesale commodity prices in combination, or the price of an individual commodity, exists as an entity apart. And we both assert it with vigor.

There is more ground for difference of opinion on other points. The critical comments here in question fall under two heads: (1) those relating to the general attempt to define some of the attributes of wholesale commodity prices, treated as a system, and to trace some of the interrelations and uniformities existing within this system; (2) those relating to the procedure employed in this attempt.

Mr. Bye and Mr. Viner both believe that it was a central weakness of *The Behavior of Prices* that it was restricted to wholesale commodity prices, and that the study did not bring other elements of the price system and related nonprice elements of the economic system directly into the investigation. Their views should be before us.

Bye's comments on this point are somewhat equivocal. He

suggests that "In view of this restriction of subject matter (to wholesale commodity prices), no trustworthy conclusions can be drawn from it concerning the price system as a whole" (p. 68). In particular, he deprecates the omission of prices of the primary factors of production—land, labor, waiting, and business enterprise.

No price analysis which leaves factor prices out of the picture can go very far towards an understanding of the price system. In omitting this phase of price phenomena, Mills has neglected a very fruitful line of approach to his major problem.

Equally serious [he adds] is the fact that he has viewed prices apart from other aspects of economic life. He treats the price system as an independent entity, as though it worked in isolation from the other aspects of the economic process. (p. 68).

[Later (p. 76), Bye notes my own reservations concerning the limitations attaching to the study, and suggests that] His failure to develop some of the other aspects of price problems . . . may be explained as a perhaps justifiable centering of his interest upon a particular phase of those problems, a phase which seemed to him to be in particular need of exploration.

Viner's remarks sum up to the following, as regards the present issue:

[The theorist] . . . would agree . . . "that all prices are connected"; but he would insist that the bonds are never immediate, that prices are never related to each other directly, but always by means of a connecting tissue of demand and supply functions. He [the theorist] would probably be skeptical . . . of the possibility of getting much insight into the character of the relations between prices in the absence of any reference to this connecting tissue. (p. 169).

Viner's conception of a fair analogy to studying the attributes of wholesale commodity prices and their mutual interrelations, without direct inclusion of other prices and of nonprice factors, is that of a biologist dealing with "the behavior of heights," arbitrarily limiting his investigation to heights of persons under a given age but without further differentiation according to age, basing his grouping of heights

merely on chronological order and degree of change of his height data, making use of no biological theories, and seeking by such means to demonstrate the existence of a "system of heights" (pp. 169-170).

Viner's other comments deal partly with the method employed, partly with the evidence bearing on the validity of the concept of a system of prices. Both of these matters are discussed below. Throughout, however, runs a clear scepticism concerning the desirability, or possibility, of studying an aggregate of price measurements without taking direct account of the "connecting tissue," in the form of demand and supply functions.

The price system, as I have used that term, comprehends "all quotations on commodities, services, equities, and other disposable values which are expressed in pecuniary terms."¹⁹ The individual elements of this system constitute a number of subordinate systems, such as security prices, wage rates, prices of land, commodity prices at retail, commodity prices at wholesale, interrelated as parts of the total but marked also by characteristic features. Such groups are to be distinguished, of course, on the basis of rational considerations, with the classifications subject to empirical tests of significance. *The Behavior of Prices* dealt only (except for incidental references to other categories) with commodity prices, and among these, only with the subcategory of wholesale prices. My exposition, therefore, relates only to the system of wholesale commodity prices.

A point that is perhaps obvious may be made explicit. Commodity prices are not taken to be 'representative' of the price system in general, nor are wholesale commodity prices

* "On the Changing Structure of Economic Life" in *Essays in Honor of Wesley Clair Mitchell*, New York, 1935, p. 371.

taken to be 'representative' of all commodity prices. It is pointed out in *The Behavior of Prices* that there are numerous 'minor price systems' in the system of prices and that each of these has 'characteristic features and modes of behavior' (p. 213); each has 'unique features' (p. 214). An understanding of the price system calls for the study of each of these minor systems 'individually and in relation to the whole system' (p. 213). One does not, obviously, pick out one element of a total, an element possessing unique features and characteristic modes of behavior, and use it as 'representative' of the total.²⁰ There is no such suggestion in *The Behavior of Prices*. Using available quotations at wholesale, I sought such knowledge as I could get of the characteristics of the system of wholesale commodity prices.²¹

The justification for treating prices as a system is found, in part, in considerations of the same order as those that support Viner's belief in a "connecting tissue of demand and supply functions." In each case, some external evidence supports an inner conviction resting on rational considerations. Some of the external evidence is referred to below in connection with the discussion of generalizations based on the study. Other considerations supporting the validity of the concept may be briefly noted.

1. The ties between the prices of individual commodities that may be direct, as between goods of the same type at different stages of the productive-distributive system, ties that may be maintained through the agencies of competition, ties that derive from joint supply or joint demand, ties over time

²⁰ Viner's suggestion that the heights of persons under ten are no less representative of the heights of all persons than are wholesale commodity prices of all prices is based upon misconception of my purposes and procedures.

²¹ For brevity of exposition I frequently speak, in the book, of the "system of prices" or "the system of commodity prices" but the context clearly indicates that conclusions based on this study relate to commodity prices at wholesale, in combination.

that bind today's price quotations to those of yesterday and tomorrow, are too numerous and familiar to need recording.

I assume that there is no doubt concerning the reality of these connections. Empirical evidence in a mass that would exceed the compass of the present essay could be adduced in support of these statements. Indeed, the Walrasian law of demand

$$D = f(p_1, p_2, p_3 \dots p_n)$$

in which the demand for any commodity is defined as a function of the prices of all commodities, is explicitly formulated in terms of a system of interrelated prices. The objection to the concept does not arise, I take it, out of doubt concerning these relations.

2. The ties among the individual elements and among the larger component parts of the price system follow various channels. Some are as direct as those between cattle and beef; some are indirect, involving relations of competition, substitution, and the judgments and choices of buyers, and sellers. Viner's reference to the connecting tissue of demand and supply functions suggests the character of the relations that we suppose to bind prices together into a system. But I do not believe he would hold that we can learn nothing of value about the interrelations among prices unless we can define these functions in detail, unless we can trace the routes and measure the impulses by which stimulations making for price rises or declines are transmitted from commodity to commodity. No one knows better than Viner that his tissue of supply and demand functions is largely outside the sphere of possible observation and definition. We can sometimes roughly guess the degree of elasticity of supply or demand, but the number of commodities for which we have even approximate knowledge of the character of demand functions

falls short of two dozen. Fortunately, we can make progress without waiting for this number to be multiplied by a hundred or a thousand. It is enough for many purposes to deal with observed relations even though the routes by which forces are transmitted are hidden from us. Of course, we seek to trace the factors, to explain observed co-variations, to guide, supplement, and reinforce observation by all that reason and judgment may tell us concerning the hidden routes and the forces that travel them, but study of the observable relations among commodity prices cannot and need not wait upon prior knowledge of the numerous demand and supply functions that may define price-quantity relations.

Indeed, there can hardly be any real dispute over the paramount importance of prices as prices, prices as channels through which forces originating in such nonprice factors as human wants, physical scarcity, surplus inventories, abundant harvests, are transmitted, prices as the nervous system of the economic order. They constitute the observable surface of economic life. We study them knowing that they are not economic ultimates, but believing that their movements and their interrelations reveal important aspects of economic processes at large.

The role of prices in economic life and the supreme importance of the relations among elements of the structure of prices viewed as a system of related parts are well defined in the following statement:

. . . as a matter of fact, prices are to the economic structure at least as important as the circulation of blood is to the human body, and the whole stable and effective working of it, at something near its capacity, rests on the adjustments between the various parts of the price structures. That isn't necessarily true of any economic system, but it is true of the American system, where the bribe of a prospective profit acts as a substitute for the edict of authority.

The quotation is from Mr. Viner's testimony before the Pub-

lic Service Commission of Wisconsin, in a case involving the rates of the Wisconsin Telephone Company.²²

3. There is another aspect of price relations that calls for brief note. Prices as prices, and without reference to the wants and disutilities that may lie back of them, define fundamentally important terms of exchange among producing groups. These relations are simple and familiar. With wheat at one dollar a bushel and shoes at five dollars a pair, the ratio, 1:5, defines the purchasing power of a bushel of wheat in terms of shoes. If the price of wheat falls to 50 cents, the ratio becomes 1:10, with obvious concurrent changes in the physical relations involved. The same relations hold among relative prices. If p be the price relative for a given commodity and P be an index of the prices of goods bought (for consumption, let us say) by the producers of the commodity in question, the ratio p/P defines changes in the per unit purchasing power of that commodity.

The more general significance of this relation is brought out when we deal with aggregates for a given producing group. If p , as above, be the price of a certain product, expressed as a relative, and q be the physical output of that commodity, also as a relative, changes in the aggregate value of the product are defined by pq . If P be an index of the prices of the goods purchased by the group of producers in question with the money received from the sale of their product, the ratio pq/P defines changes in the aggregate purchasing power of this group. It is suggestive to think of this quantity as a measure of changes in their aggregate physical

²² The statement is from page 1238 of the edited testimony on the case in question. Mr. Viner's testimony here and his discussion elsewhere of price rigidities as factors of prime importance in business cycles (Cf. "Business Cycle Theory: Can Depressions Be Tempered or Avoided" in *Lectures on Current Economic Problems*, U.S.D.A. Graduate School, November 1936) accord precisely with the conception of a price system that shaped the investigation here in question.

rewards, or their real income. This quantity pq/P may be set against q , the measure of changes in the aggregate physical contribution of this group to the stream of economic goods available for use. The ratio of pq/P to q thus defines, for a given group, the relation between changes in their aggregate physical remuneration for productive services and changes in their aggregate physical contributions. That is, the ratio defines alterations in the terms of exchange between groups, with reference to aggregate contributions and aggregate real income. It is a measure of obvious importance in defining the shifts and alterations that occur in an exchange economy.

The point here to be emphasized is that $pq/P:q$ reduces to p/P . The measure of per unit purchasing power serves also to define a highly important relation among physical aggregates. It is a price relation pure and simple, yet it reflects the play of all the forces, physical and other, that influence the market and affect exchange relations. This holds for domestic relations, among the elements of a given economy. It holds also for the highly important trading relations among nations. The ratio between changes in import and export prices defines alterations in international terms of exchange.

The importance of this lies in the fact that price movements and price relations possess significance beyond the monetary terms in which they are directly expressed; that in dealing with shifting price relations, we are in fact dealing with the consequences of fundamental alterations in the real forces that lie behind prices. It would be a profound mistake to ignore the story told directly by changing price relations, on the ground that we cannot trace the details of the interconnections—psychological, physical, political, social—through which impulses to change are transmitted among prices. Concepts concerning the nature of these interconnections may help us to interpret the changes directly observable

and for many purposes such concepts may and should be called upon, but to dwell in that conceptual world as though it were ultimate reality is not the part of wisdom in economic research. Actually, few if any economists do so, faced as they are by real problems and situations that call for the utilization of the resources available to them.

In discussing the concept of a system of prices, reference should be made to Viner's statements concerning the heterogeneity of the materials analyzed. It is his view that

run-of-the-mine price data, ungraded or graded only according to a few arbitrary characteristics, display in high degree lack of homogeneity, instability in their criteria over time and space, absence of mutual independence, and during any particular disturbance of moment, subjection to control by a few particular variables of predominant but unbalanced importance. The data to which Mills applies his analysis seriously violate, therefore, every one of the conditions requisite to the production of reasonably good expectancies by the application of the calculus of probabilities (p. 179).

There follows his suggestion of groupings, in order that subgroups more homogeneous than the aggregate may be set up (Viner review, pp. 179-180). The point Viner raises is an important and interesting one.

The concept of homogeneity is not a simple one. Aggregates of persons, things, or measurements are not to be classed, arbitrarily, as homogeneous or heterogeneous. Homogeneity is a relative matter. Aggregates that may be homogeneous with reference to certain generalizations, purposes, or principles of classification, may be heterogeneous with reference to others. The term has meaning only with reference to given purposes or generalizations.

The human race is a homogeneous entity for certain purposes and from certain viewpoints. All men have a great many biological characteristics in common. Many generalizations hold true of all men and of the aggregate they compose. In respect of many characteristics, individual members

of the human race differ and their characteristics may be graded along a scale. There are differences in heights, weights, pulse rates, cephalic indexes, and other bodily proportions—the list can be extended indefinitely. For many purposes, to the student of man, the characteristics of aggregates of these measurements are of unquestioned importance, in seeking to determine the group attributes of *homo sapiens*. All pulse rates, all cephalic indexes, all heights, constitute aggregates of scientific importance in the study of man.

But clearly the matter doesn't end there. Knowledge may be sharpened and redefined by examining the elements of these aggregates. We may ask: 'Are adult male members of the human race a homogeneous entity with respect to height, when rational principles of classification are applied? Or, with reference to one such principle: Are adult males of different racial groups members of a homogeneous entity, in respect of height? Again, with reference to a smaller subdivision of all adult males in the world: Do occupational groups in the United States differ significantly in respect of the heights of adult males? Narrowing the problem further: Do male factory workers in the United States, of different racial stocks differ significantly in height? The process may be carried on almost indefinitely. All adult males in the world may be treated as a homogeneous unit for some purposes; for others, all adult males in the United States may be treated as a homogeneous group; for other purposes, all adult male factory workers in the United States may be conceived of as a homogeneous group. But each of these may be subdivided, and greater homogeneity sought by distinguishing subgroups alike in more respects than are the members of the parent group.

If we should refuse to make general statements about any aggregate that could, on rational principles, be broken up

into smaller and more homogeneous subgroups, we should sharply curtail the generalizations that enter into the total of human knowledge. Of course, more sharply defined and, for some purposes, more useful statements may be made about small groups the members of which have many common attributes, but the general attributes characteristic of larger aggregates are of equal and, for some purposes, of greater concern. Knowledge of these attributes is a proper objective of men seeking an understanding of the nature of things.

All prices of disposable equities constitute a homogeneous aggregate, with respect to some generalizations and for certain purposes, but we may, by applying certain principles, differentiate among the elements of this aggregate. Wholesale commodity prices may be set off from retail prices, from security prices, from land prices. Generalizations about each of these subordinate divisions are justified even though smaller subcategories differing significantly among themselves may be set off within each of these divisions. The nature of these distinctive subcategories is, of course, of vital interest, but so also are the attributes of the entity which these subcategories, in combination, make up. The behavior of security prices as a whole concerns us; of land prices; of retail prices; of wholesale prices. The fact that each of these is heterogeneous with reference to certain principles of classification does not lessen our interest in the aggregate, or invalidate generalizations about the attributes of that aggregate. Indeed, the very fact of heterogeneity, of the existence of subgroups subject to the influence of different forces, is one of the circumstances that determine the attributes of the whole viewed as an entity.

There are rational grounds for treating the aggregate of wholesale commodity prices as a system whose characteristics are of concern to us. That system plays a vital role in the pro-

duction and distribution of goods. The importance of this role would not, I believe, be questioned. The general acceptance of an average of wholesale commodity prices as an index of price behavior is relevant evidence. There is good reason to believe that various elements of this aggregate—the prices of raw materials, finished goods, consumption goods, perishable goods—are subject to the play of special forces. Goods differing “in their organic nature, in the uses to which they are put, or in any other important economic characteristic” may be expected to differ in their price behavior, as is noted in *The Behavior of Prices* (p. 212). These attributes of special groups we hope to explore later, but the whole system is a factor in the organization and processes of economic life. Its attributes, also, are of concern to us. It was these attributes that were dealt with in sections of *The Behavior of Prices*.

In treating these attributes, I repeat, since the fact has escaped the attention of several reviewers, I was not ignoring the necessity and desirability of the next step.

The final stage of the study, and the most important from the point of view of one interested in the elements and internal structure of the price system, is the isolation and analysis of price groups and the testing of various grouping principles.²³ The data described in the present volume furnish some of the basic materials for such an analysis. The detailed account of this part of the study is deferred to a second volume. (*The Behavior of Prices*, p. 219).

Some Remarks on the Results of the Study. The results of the investigation with which *The Behavior of Prices* deals are to be appraised, of course, with reference to the objectives of that study. The nature of these objectives was indicated in the book itself; they have been restated in the opening pages of the present essay. I aimed at “the development of a method of analysis and the accumulation of a body of facts

²³ Viner's suggestion that “such groupings are unlikely to be come upon by chance, or to be found even by exhaustive experimentation with all the permutations of price-to-price relations alone” (Viner's review, p. 179 is at once valid and gratuitous.

which may conduce to an understanding of the price system." The "accumulation" was guided by certain concepts concerning the nature of the price system and its role in economic processes. It was directed particularly toward aspects of economic instability. The procedure as a whole was thought of as experimental and the classifications of price attributes employed in deriving the basic measurements were conceived to be neither rigid nor exhaustive (p. 37). A body of evidence, ordered in accordance with certain conceptions concerning the nature and role of the system of prices and concerning the types of instability manifest in price movements, and a procedure for the systematic accumulation of more evidence were primary aims in this task.

In respect to these objectives, a few general statements are in order.

1. In so far as I sought to generalize, I sought limited generalizations relating to specific groups and specific conditions. I sought no all-embracing laws. I aimed at no cosmologies. I conceive of knowledge in economics, as in other spheres, as resting in the main upon a body of verifiable observations. I sought, therefore, to summarize results in objective statements definitely related to observations and capable of being verified or disproved by similar observations of others. Only so, I believe, can economic knowledge become accretionary. Only so, may we contribute to a heritage of objective, ordered knowledge, capable of cumulative growth from generation to generation, rather than to a heritage of 'systems,' conflicting theories, and disputed rationalizations. The fact that the study was being conducted under the auspices of the National Bureau of Economic Research confirmed my own desire thus to deal with objective aspects of price behavior and to point the research toward limited and verifiable conclusions.

2. I hoped that the results of my own work might in their turn become data in the work of others.

3. If the procedures, ordered evidence and limited generalizations of this study should provide further hypotheses and leads to additional investigation, I should be the more content with its results.

The results, therefore, take the form of an experimental method for the study of the behavior of prices, separately and in the mass, and a body of evidence concerning the characteristics of the behavior of wholesale commodity prices in the United States between 1890 and 1926, organized with reference to conceptions concerning three aspects of price instability. These results do not lend themselves to ready summarization. Their nature is suggested in the sections of Bye's *Critique* that deal with the content of *The Behavior of Prices*. As regards the method in general and the detailed evidence obtained, it is not necessary to attempt further summary here.

Bye's detailed account of the study, division by division, gives a reasonably accurate impression of the procedures employed and of the nature of certain of the detailed findings. When, however, he selects "typical generalizations" (pp. 64-76), he is not uninfluenced by the position he has taken with reference to the study as a whole. I think it in order, therefore, to discuss certain of the generalizations to which the study led me. Since this discussion will necessarily involve reference to Bye's conception of the nature and significance of scientific generalizations, I shall first refer to certain of his general remarks on this topic.

Bye cites (p. 73), as examples of principles developed by deductive analysis, three propositions of theory:

. . . the price of a commodity tends to the level at which the demand for it equals its supply.

. . . the price of a commodity in the long-run tends to equal the costs of production of a representative firm.

. . . the general level of prices depends on the relation between the

circulation of money on the one hand, and the volume of trade on the other.

[He proceeds] The most helpful type of scientific generalization is that which describes relations of cause and effect, or, (if the reader prefers to be very correct), relations of invariant sequence. The theoretical principles just referred to are of this character. They show relations between antecedent phenomena such as output, consumers' choices, costs, monetary circulation, or trade transactions, and consequent results—such as prices (pp. 73-74).

Bye objects to my generalizations because they lack this quality of showing relations between antecedent phenomena and consequent results.

These statements, which throw light on the outlook and standards of judgment of the reviewer, are of interest in several respects. Bye's conception of the nature of an economic generalization has a counterpart in a statement by Cairnes²⁴:

Alike in the case of the physical and of the economic world, the facts we find existing are the results of causes between which and them the connection is constant and invariable. It is, then, the constant relations exhibited in economic phenomena that we have in view when we speak of the "phenomena of wealth," and in the exposition of these laws consists the science of Political Economy.

The relations of invariant sequence that Cairnes envisaged and that Bye seeks in political economy have to do, so far as economics is concerned, with a purely conceptual realm. It would be comforting if the working of such laws could be observed in the kind of economic system we possess, but that is not possible. In the physical world, the limit of perfect relationship is approximated under certain conditions but even in that world perfect relationship has the attributes of the conceptual rather than those of the observable and verifiable. Since Clerk Maxwell wrote

. . . those uniformities which we observe in our experiments with quantities of matter containing millions of molecules are uniformities . . . arising from the slumping together of multitudes of cases, each of which is by no means uniform with the others,

²⁴ J. E. Cairnes, *Character and Logical Method of Political Economy*, London, 1857, pp. 19-20.

concepts of invariant sequence, of mechanical relationships, of simple and definable lines of cause and effect, have, in many fields of thought, given way to concepts of probability, multiplicity of causes, average relationships, concurrent variation. Particularly is this true in the social sciences, in which it is impossible so to isolate selected variables that their interactions are uninfluenced by disturbing causes. In so far as we are realists, we deal perforce with variation, with a multiplicity of factors, with tendencies, with relationships that hold only on the average. Bye's view represents an interesting survival of an outlook that did at one time dominate thinking about social and economic relations. When consciously applied to ideal situations in which all disturbing factors may be held constant and when there is due recognition of the ideal nature of the situation, a mechanical theory may be fruitful and valuable, but the mechanical view is not applicable to the *phenomena* of economic processes in the manner suggested by Bye.

Bye's own examples of "invariant sequence" are in curious contradiction of the term itself. If the price of a commodity "tends" to a level at which demand and supply are equal, or if price "tends" to equal cost of production, the idea of invariance, applied to phenomena, is obviously negated; and the invariance that involves so intangible a factor as "costs of production of a representative firm" is indeed a notable concept.

Somewhat more important in its bearing on the validity of Bye's general conception is his idea of the relation between "antecedent phenomena" and "consequent results," in the examples cited. He speaks of output and consumers' choice as antecedent, prices as consequent results. Causal relations, of course, run in both directions. Output and consumers' choices are affected by prices, as prices are affected by output and consumers' choices. To suggest that costs are always antecedent,

prices consequent, is to take a curiously naïve view of contemporary producing and distributing operations. Bye's third principle ($MV + M'V' = PT$) is equally capable of a two-way cause and effect interpretation. Not necessarily is the monetary circulation antecedent, with price changes consequent. Under contemporary conditions, a change in the volume of credit outstanding may result from changes in the prices of individual commodities, changes that may in their turn be due to any of an infinity of forces affecting demand or supply conditions, e.g., as when an advance in productivity lowers costs and prices, and reduces, perhaps briefly, perhaps for a longer period, a manufacturer's current credit needs.

Earlier statements in this essay have touched upon my own conception of the types of generalizations to which research may lead and which may be fruitful in summarizing knowledge and advancing further research. I do not think of them as necessarily involving relations of cause and effect, or of invariant sequence, or of necessarily showing relations between antecedent phenomena and consequent results. We seek statements of uniformities, sequences, tendencies, and correlated changes—regularities in the events observable in the external world. In seeking to discover and define such regularities, we are guided by rational considerations, by past experience, by analogy, by reason playing on observed phenomena. Anything that may shape ideas and illuminate experience may help in this search for order. The regularities thus defined may vary over a wide range in a universe marked by the interplay of multiple causes, perhaps reaching only conceptually the limit of complete certainty that permits unqualified prediction. These conceptions and those of Bye differ rather materially.²⁵

²⁵ This difference is exemplified by Bye's criticism of my generalizations for not showing relations of invariable sequence between antecedent phenomena and consequent results (Bye, above, p. 74). The major difficulty under which

Bye cites eleven generalizations, said to be typical of those reached in *The Behavior of Prices*. These are presented by Bye as of limited significance. Yet each one,²⁶ I think, repre-

Bye labors is here manifest—he does not distinguish between the ideal constructs of the mind and the world of nature with reference to which these constructs must ultimately be judged. The necessary limitation of universal generalizations when applied to existential circumstances has been repeatedly noted. Years ago Jevons wrote, "I have no objections to use the words cause and causation, provided they are not allowed to lead us to imagine that our knowledge of nature can attain to certainty" (*Principles of Science*, p. 226). More recently, J. M. Clark has written, "So far as economic theories are to serve as hypotheses for inductive studies to verify, it follows that these hypotheses must be such as the workers in the field will use. They must be verifiable. . . . This rules out certain kinds of propositions of the deductive economics: namely, those which insist on being universally true, with the result that they become tautologies, so that any kind of observed behavior will fit them as well as any other kind. For such statements, an inductive economic theory can have no active use, and the more closely economic theory gets in touch with inductive studies, the more will its statements take on new forms, becoming less tautological and more quantitative." ("The Socializing of Economics" in *The Trend of Economics*, p. 76.)

* With one exception. The first of the eleven does not state a general conclusion reached by me. Bye's formulation is as follows:

"Farm prices show greater differences in the amplitude of their fluctuations than wholesale prices, and wholesale prices greater differences than retail." (Bye, p. 72).

No comprehensive sampling of farm prices was attempted. Indeed, I wrote, ". . . the comparison of actual values (i.e., of measures of price variability) in the farm price and in the wholesale price lists is not justified. The farm prices, being averages for the United States, would tend to be more stable than would wholesale price quotations in a single market. On the other hand, the farm prices (utilized in this study) are December 1st prices, while the wholesale prices are averages for the year." However, I did set up for incidental comparison measurements of the year-to-year variability of the December wholesale prices of six farm products, as quoted in the Chicago markets, and similar measurements for the same six commodities based on December farm prices in six selected states. I concluded, with reference to these commodities and these markets, that "the year-to-year variability of December farm prices in the chief producing states is greater than the year-to-year variability of December wholesale prices in Chicago." (*The Behavior of Prices*, p. 55). I said nothing about differences in the amplitudes of price fluctuations, nor did I generalize (as was clear from the statement itself and from the context) beyond the specific commodities and markets included.

No general sampling of the retail price field was attempted and no conclusions were drawn about retail prices in general. I did, for illustrative purposes, cite measurements relating to the year-to-year variability of the retail prices of thirteen food products and remarked that "the range of values within which these (thirteen) measures fall is distinctly smaller than that for wholesale prices" (p. 53). This was not intended to be a "generalization."

sents the results of an attempt at just that kind of accumulation and ordering of evidence for the critical appraisal of other students that promises truly to advance economic knowledge and to place economic generalizations on a foundation of fact rather than of opinion. Believing this, I venture to take two of the generalizations that Bye depreciates and discuss the issues involved, the meaning, significance, and limitations of the conclusions reached, and the nature of the evidence on which they rest. These two relate to cyclical movements in commodity prices.

There is a common pattern among cyclical price movements (that is, some consistency of sequence in commodity responses to cyclical influences) which is not the result of chance (Bye, p. 72).

The order of price recession in a given cycle exercises a somewhat stronger influence on the order of the succeeding revival than the order of revival does on that of the succeeding recession (p. 73).

In introducing the discussion of these statements, it is worth noting that doubts concerning the reality of business cycles have been expressed from time to time and that a year or so before *The Behavior of Prices* was published, a noted American economist had pronounced the business cycle a myth.²⁷ The question at issue is whether there is a pattern that repeats itself in successive cycles—a pattern marked by regularities in the timing of the changes occurring among different economic elements and in other interrelations among the movements that appear to be associated with “cycles” in general business. The manifestations of business cycles are not, of course, marked by relations of “invariable sequence.” There are wide diversities from cycle to cycle in the timing, amplitude, and duration of the movements of economic series. It is of central importance, therefore, to determine whether the complex of varied movements from

²⁷ Cf. Irving Fisher, “Our Unstable Dollar and the So-called Business Cycle,” *J. Amer. Statist. Assn.*, June 1925.

cycle to cycle may be attributed to the mass of undifferentiated forces we call chance, or whether true regularities may be discerned.

Various theories of business cycles rest upon presumed uniformities, and the work of Wesley Mitchell, Henry L. Moore, Warren Persons, and others, had given evidence of the existence of objectively discernible regularities. Yet no systematic attempt had been made to define the patterns of price behavior during business cycles, or to apply to patterns thus defined, tests of significance, i.e., tests designed to determine whether or not the observed results were attributable to the play of chance.

I quote from my own summary of Chapter I of *The Behavior of Prices*, inserting certain of the evidence upon which the various conclusions rest:

A number of significant relationships among the measures of cyclical price movements have been observed.

a. The commodities which rise first on revival are subject, in general, to wider cyclical fluctuations than are the commodities which lag on revival. Similarly, the commodities which decline first on recession are marked by wider fluctuations than are those which lag (p. 158).

This conclusion is based upon observations relating to 1110 cases of price decline during cyclical recessions, 1110 cases of price advance during cyclical revivals, and 1110 completed cycles for which measurements of the amplitude of cyclical price movements were available.²⁸ A coefficient defin-

²⁸ The measurements were obtained from monthly price quotations for 149 commodities for the period 1890-1926. The analysis included 209 commodities in all. The sixty excluded from the present measurement of relationships included 34 which passed through less than five complete cycles during this period (ten recognized business cycles occurred) and 26 agricultural products and goods whose price fluctuations appeared to have no connection with cycles in general business. The conclusions relate, therefore, to about 7/10 of the commodities included in our sample of wholesale prices. Different elements of the economic system reflect "cycles" in general business in varying degree and some elements of the economic system appear to be independent of such fluctuations. Our conclusions deal with interrelations among those elements of the wholesale price system—about 70 percent of the total—that

ing the correlation between the timing of revival, i.e., the sequence of revival, in the wholesale prices of individual commodities and the amplitude of cyclical fluctuations has a value of .29.²⁹ A similar measurement relating to the sequence of recession and the amplitude of cyclical fluctuations has a value of .31. Each of these measurements is more than ten times its standard error, which means that the odds are infinitely small that chance factors could account for the relationships observed. A higher coefficient (.50) is derived when, for each of 149 commodities, a measure of the average timing of revival during 36 years is paired with average cyclical variability over the same period. The corresponding measurement, when average timing of recession is paired with cyclical variability, is .47.

b. The commodities which rise first during revival tend to decline first on recession, but the correlation between the sequence of change on revival and the sequence of change on recession is far from perfect (p. 158).

The coefficient of correlation between 1202 pairs of observations relating to the timing of recession and revival is .27. This, also, is almost ten times its standard error. If the sequences of price change in revival and recession were independent of one another, such a value would occur as a result of chance only once in millions of trial. When the average timing of recession is correlated with the average timing of revival, a coefficient of .72 is obtained.

c. When measures relating to individual cycles are paired, it is found

are marked by and appear to participate in the swings we call cyclical. They are to be interpreted with this fact in mind. The nonconforming series would yield neither measures of timing nor measures of cyclical variability, so are not, of course, available for inclusion.

²⁹ I omit the signs of this and the following coefficients. For technical reasons related to the system of measurements employed, the signs vary from case to case in a manner that might confuse the reader. It is absolute values that are here significant.

that the correlation between the timing of recession and the timing of revival on the next succeeding turn is significantly higher than the correlation between the timing of revival and the timing of the following recession. The order of price recession in a given cycle exercises a stronger influence upon the order of the succeeding revival than the order of revival exercises upon the order of the succeeding recession. There is evidence in both cases, however, of a significant relationship. Although each phase of a price cycle bears the imprint of novel factors, there are definite bonds which tie it to the phase that has preceded it (p. 158).

Continuities extend through time. Recession prepares the way for revival and places its imprint upon the character of that revival. Similarly, the nature of the recession is in some degree determined by the nature of the preceding revival. But are these continuities of equal strength? This is a question of degree since there is no reason to believe that the cyclical process has a clear beginning and sharp end, but it is of great importance to the student of business cycles to know which phases of the cycle are bound together by the strongest ties. The preceding quoted statement presents the answers obtained from the study of price relations between 1890 and 1926. If we begin with the sequence of revival, we find a correlation of .27 (based on 1202 observations, and with a standard error of .027) between the pattern of revival and the pattern of the succeeding recession. But if we begin with recession, and measure the ties between recession and succeeding revival, the coefficient rises to .40 (1192 pairs of observations, with a standard error of .024).⁸⁰ This is a notable difference, and points to a highly suggestive con-

⁸⁰ These coefficients of correlation are not high, for differences between cycles are in many ways more marked than the resemblances. However, we are now asking the question: Are the events of successive cycles independent of one another? Is each cycle a unique event, as regards the interrelations among movements of economic elements? The significant fact here is the degree of departure of a coefficient of correlation from zero, judged with reference to the sampling errors involved. These departures from the zero values that would denote complete independence, complete absence of a common pattern, are many times greater than they would be if chance factors were responsible.

clusion. Recession sets a more marked imprint upon the succeeding recovery, in so far as the timing of price movements is concerned, than revival does upon the succeeding recession.

d. There is a clear tendency for the sequence of revival to follow a common pattern during different cycles. The same tendency is observed in studying the sequence of recession in different cycles. The common pattern is slightly more in evidence for the data relating to recession than in the figures measuring the time of revival. Other evidence bears out this suggestion that the consistency of price movements during different periods of recession is greater than it is during revival (pp. 158-159).

Statements (b) and (c) above indicate that the sequence of price movements during recession was not, during the period covered by this study, independent of the sequence of price changes during revival. The statements under (d) were designed to answer the questions: "Is there a common sequence of price movements which prevails, without much variation, from revival to revival? Is there a standard sequence of recession, a pattern to which different periods conform more or less closely?" (*Behavior of Prices*, p. 134.)

Answers to the first question were derived in two ways. Measurements defining the average sequence of revival (i.e., the average order of change in the prices of individual commodities in the recovery following a period of depression) were correlated successively with measurements defining actual sequences in each of eleven periods of revival. The calculations for each period rested upon measurements for all the commodities whose prices reflected the revival in question. The observations ranged in number from 105 to 149. The results appear in Table 46 of *The Behavior of Prices*. The coefficients ranged from $+.28$ to $+.73$. Evidence of a second type was obtained by comparing the sequences of price change in successive revivals. Ten such pairings gave nine positive coefficients of correlation, ranging in value from $+.06$ to $+.37$ (See Table 47, *The Behavior of Prices*). Of

these nine, eight were significant, in the sense that they appeared to be definitely too great to be attributed to chance. The conclusion is unmistakable. During the period covered by this study, something of a common pattern tended to reappear in the timing of price changes in successive revivals. Recurrent regularities are impressed upon these cyclical changes.³¹

Similar evidence was adduced in answer to the second question, relating to the existence of a standard pattern of price recession. Using the average sequence of recession as a criterion, measurements of correlation relating to ten dif-

³¹ Obviously, the matter is not closed by such a finding. The nature of the economic forces that impress these regularities upon price changes calls for study, and the questing mind will follow all the clues it can find—whether empirically suggested or rationally derived—in seeking to understand them.

I should perhaps say that the technique of correlation was used in seeking to determine whether regularities existed in the sequences of cyclical price movements simply because it was appropriate. I have presented the results above in these technical terms because there is no substitute device that can so effectively summarize relations of the type here in question.

Mr. Bye has something to say of correlations as empiricisms, citing Macaulay's characterization of a coefficient of correlation as an evidence of an empirical relationship. A coefficient of correlation, by itself, is a form of observation, as empirical as a photograph of a spiral nebula or a measurement of the speed of light. It is a convenient summarization of a series of related measurements. It proves neither cause, nor effect, nor a definite sequence, nor the existence of a relationship that will be found when new observations are included in the study. I know of no competent person who employs these techniques who would say otherwise.

But when this technique is guided by rational considerations, when the coefficient is used to give precision to ideas concerning relationships that are based upon other knowledge, to test clues that are suggested by reason, intuition, analogy, or whatever be the source of the "light of the idea," the empirical observation becomes relevant evidence. So it is, I think, in the uses made above. We have not the knowledge of the complex relations that prevail among all the elements of the economic system during successive recessions in general business that would enable us to construct a complete, rational explanation of these movements. It is reasonable to believe, however, that something of a common order is found in the incidence of the forces of recession, that the sequences of economic change in successive recessions have common elements. This hypothesis could be made more concrete by bringing in further ideas based on such knowledge as we have of the cyclical role of investment, of speculation, of inventories, of orders for future delivery, of the nature of demand for goods of various types. These would support the

ferent recessions varied from .39 to .61 (See Table 48, *The Behavior of Prices*). Nine similar measurements relating to the sequences of recession in five successive cycles were all positive, ranging from .18 to .36. Of the nine, six were clearly significant (See Table 49, *The Behavior of Prices*).

I quote now the brief summary statement from *The Behavior of Prices*, page 159:

The evidence upon which the preceding statements are based is of considerable general significance in showing that there are true economic regularities in the price movements which accompany cycles in general business. In cycle after cycle, there has been observed a degree of uniformity in the sequence of revival and recession in commodity prices and in other aspects of price behavior. If cyclical movements represented the play of mere variability about a mean or a trend, one would expect the sequence of price change in each cyclical swing to be unique, except for certain chance resemblances to movements at other times. Yet the odds against chance alone accounting for the regularities we have found are infinitely great. In period after period, there is a recurrence of price movements which have something in common, in respect to sequence of change, amplitude, and duration. These changes in different periods are far from showing perfect uniformity, but there is unmistakable evidence that the observed resemblances would not be found if the cyclical movements of individual prices represented random fluctuations alone. The phenomena of business cycles show just those regularities and uniformities which it is the business of the scientist to discover, trace and, if he can, explain.

hypothesis of a persistent sequence. But they, in turn, would call for detailed testing and it is not necessary to specify these in testing the general hypothesis of orderly and repetitive change. This hypothesis, whether in general form or restricted to wholesale prices, has sufficient basis in our knowledge and in a priori reasoning to warrant the seeking of empirical evidence bearing on it. And that is all that the coefficient of correlation is. When the hypothesis to which it relates is supported by reason, when it accords with other ordered and verified observations, when it has been tested for significance with reference to the sampling fluctuations to which it is exposed, then weight attaches to the coefficient. But it is never other than an observation (or a summary measurement descriptive of relations among a number of observations). I assume that all observations are, by definition, empirical.

I might have specified in a formal manner the precise "hypothesis" to which each measurement of this type cited in *The Behavior of Prices* had reference. I had, however, indicated elsewhere my conception of the part played by such summary measurements and had not foreseen misunderstanding.

This evidence has various limitations. The individual series were analyzed and their cyclical turning points located by a standard procedure, but some elements of personal judgment were inevitably present in deciding borderline cases. Neither seasonal nor trend movements were eliminated prior to the analysis of cyclical fluctuations. This was in part due to a deliberate desire to work with the series as they actually stood, to avoid abstractions that might remove elements integrally related to cyclical changes. Seasonal, trend, and cyclical movements are not independent, mechanically combined elements of economic change. Limitations of time and money contributed, also, to the decision not to modify the reported series, prior to the cyclical analysis. Again, the individual price series compiled and reported by the U. S. Bureau of Labor Statistics possess defects. These are recognized by that Bureau and improvement is being effected. Better series would have improved the results. Further, the measurements defining cyclical regularities relate only to that portion of the system of wholesale prices that appears to participate in, or reflect, the swings in the level of wholesale prices that accompany the semiperiodic fluctuations of general business.

I have ventured upon this extended statement relating to two of the generalizations that Bye refers to, because it seemed necessary to indicate the nature of the objectives, the procedure and the evidence, and the true character of the conclusions reached. These conclusions have clear limitations. I do not question that both procedures and basic materials could be substantially improved. Indeed, we are now attempting to extend and improve the measurements in question. But I believe that the problems investigated were problems of high importance, that the questions to which answers were sought were questions of proper concern to persons who seek

an understanding of modern industrial processes. To the extent that the conclusions are valid, the same significance attaches to them.

In discussing certain of the results obtained from the study of cyclical price movements, I have not attempted to deal with Mudgett's very suggestive criticism of "the conception of a *general business cycle* as a gargantuan force, causal in character, and bringing in its train the cycles in individual series" (Mudgett review, p. 187). It is undoubtedly true that various statements in *The Behavior of Prices* seem to rest upon such a conception. Were I rewriting the book, I should aim to avoid such statements. For I agree with Mudgett that "it is only the cumulation of effects on individual commodities that produces the thing we call a general business cycle" (*Ibid.*, p. 188). It is true, of course, that there are variations in the intensity with which the "effects on individual commodities" cumulate, that demand and supply conditions for thousands of individual commodities are subject to common influences, making for expansion or contraction, or to related influences that may stem from a limited number of common causes. Mudgett's phraseology perhaps underemphasizes the role that may be played by forces that impinge upon a great many commodities making for some degree of uniformity in their behavior during business cycles but he is unquestionably right in stating that the "general business cycle" is merely the cumulated behavior of masses of economic series and that the understanding of cyclical movements will be advanced through the study of conditions affecting the behavior of individual series.

It is not desirable to deal in great detail with the evidence lying back of other "generalizations" cited by Bye. These citations, I believe, fail to give adequate indication of the nature of the conclusions reached. To some extent these

inadequacies are offset by more detailed statements in other parts of Bye's *Critique*, but the general impression given by his summary statement of eleven generalizations (Bye, pp. 72-73) I think is a misleading one.

Although it is not here possible to discuss in detail other conclusions of the study or to indicate the nature of the evidence on which they rest, the treatment of instability calls for some further comment. The third chapter of *The Behavior of Prices* dealt with price relatives in combination. I discussed these materials with reference to the problem of price instability. That term, as I pointed out, is ambiguous. For the purposes of my own treatment, I distinguished three types of price instability open to investigation in terms of the materials available to me. Instability manifest in fluctuations of the price level is a familiar type. Less familiar are the concepts of internal instability and group instability.

"Internal instability," I said, "is that form of price instability which develops when a set of established price relations is disturbed" (p. 215).²² Bye thinks that this is "not strictly

²² This is, of course, a definition selected because it lent itself to the type of study of price dispersion and price displacement that could be made from the data at our disposal. It is deliberately limited to price relations and does not distinguish those changes in price relations that may reflect equilibrating movements from those that may reflect disturbing movements in the economy at large. With these limitations in mind, I pointed out that measures of internal instability were strictly to be viewed as "measures of the degree of change in price relations" (p. 215), and that significance attached to the relative magnitudes of such measurements. "Instability," therefore, was not defined in any absolute sense by these measurements, although variations over time in their magnitude could be expected to reveal "abnormally" high rates of change in price relations. We suffer, of course, from the lack of objectively definable criteria of equilibrium, or stability, or of "adjustment" among the working parts of the economic system. In default of such criteria and of means of getting at the underlying changes in costs and in quantities supplied and demanded which might be made to yield such criteria, we may utilize the historical record as a guide in the interpretation of contemporary changes.

This problem appears in a slightly different form in connection with the concept of "price disparities." Some of the difficulties attaching to the identification of such "disparities" are briefly discussed in *Prices in Recession and Recovery* (pp. 33-37).

correct," favoring a procedure which would measure deviations from a moving equilibrium. Such a moving equilibrium, he thinks, might be approximated with reference to the secular trends of prices. "Something approaching a balanced adjustment in the economy would probably prevail if all prices conformed to the figures indicated by such trends." (Bye, p. 51) Instability or unbalance would be measured in terms of deviations from such trends, and the dispersion of prices and characteristics of curve types would be determined with reference to similar deviations.

This is an interesting suggestion.³³ There may be question as to whether statistically determined trend lines would approximate the "normal" prices of Marshallian theory and there may be further question as to the logical separability of trend movements and those "deviations" which Bye conceives to be manifestations of instability. One may well doubt, too, that "stable equilibrium would exist—if all prices could be kept at their normal" (i.e., their trend values) (Bye, p. 64). The actual prosecution of such a study would raise questions and difficulties of other types. Bye notes some of these in connection with possible changes in price flexibility. However, the procedures and the concepts back of these procedures would be utterly different from those of the study under discussion. They would be different investigations, not alternative studies. His criticism of method here comes down to the proposal of a quite different type of study involving both logical and practical hazards that may not be lightly dismissed.³⁴

³³ Reference should be made, in connection with it, to the general concept of moving equilibrium developed by Henry L. Moore in *Synthetic Economics* New York, 1929.

³⁴ In *The Behavior of Prices*, I discussed the possibility of defining variability in terms of deviations from a line of trend and noted reasons for rejecting such a measure as not relating to "significant manifestations of variability" (p. 50).

There is reference in *The Behavior of Prices* (footnote 2, p. 76) to the

In his discussion of group stability, Bye's comments lead me to doubt that he correctly interprets the treatment of frequency distribution types in *The Behavior of Prices*. A frequency curve of a given type does not reflect the existence of a particular set of price relations. The appearance of a stable distribution type does not necessarily, or even probably, represent the restoration of some one "potential set of price relations which . . . would preserve balanced adjustment in the economy." (Bye, p. 49). At least, this is not true if the term "set of price relations" has the meaning normally attached to it. The relative prices making up a Gaussian distribution may be mixed, internally, in any one of an infinity of possible combinations. The price relative for commodity *X* may be higher than, lower than, or equal to that for commodity *Y*. When, with Marshallian or Walrasian concepts in mind, we speak of "a set of price relations," we think of certain internally ordered relations, with the prices of specific commodities standing in definite relations to the prices of other specific commodities. This, if I mistake not, is Bye's conception of . . . "a fundamental set of price relations which tends to prevail in the long run . . . and which tends to keep the economy in balanced adjustment" . . . (Bye, p. 49), a conception he identifies with the doctrines of deductive theorists. My reference to the relation between the problem of price stability and the problem of general economic equilibrium was not meant to imply that the presence or absence of such a set of price relations could be determined by observations on frequency distribution types.

theory of crises developed by Emanuel H. Vogel, *Die Theorie des Volkswirtschaftlichen Entwicklungsprozessen und das Krisenproblem*, 1917. Vogel's theory, very briefly summarized in that footnote, is that "the moving equilibrium between economic processes which is necessary to prosperity is ruptured periodically—because of differences in the rates of growth of the various elements in this equilibrium." This view is to be contrasted with that which conceives of empirically derived trend lines as defining the normal prices of neoclassical value theory.

The relations within a stable frequency distribution have to do with the character of the actual distribution of observations but all identity of individual observations is lost. It is stability of another type, not the stability of particular price relations, that was under discussion in this part of *The Behavior of Prices*.

In this study of group stability, we are concerned with what can be learned by indirection about the hidden forces that play upon the individual members of a population. Are these individuals subject to the play of balanced forces, with the tendency toward individual variation held in check by stabilizing factors, or are they influenced by forces limited in number and unbalanced in their incidence? One may learn something about the underlying forces by studying the types that emerge when frequency distributions of the individual observations are constructed.

Not much has been done in this field, although there has been some discussion of the "normality" of distributions of price relatives. *The Behavior of Prices* presented rather extensive materials bearing on these questions. An extraordinarily large percentage of the distributions (90 out of a total of 190) were 'heterotypic,' in Pearson's sense, reflecting the presence of marked instability. Only 18 out of the 190 distributions studied could have been considered sufficiently close to the Gaussian type to have been described by the normal law of error. (Almost too generous allowance has been made for possible sampling errors in including as many as 18 in this group.)³⁵

³⁵ Distributions of Type III and (more rarely) the normal type would be expected to emerge when a large number of independent factors, none of predominant importance, are influencing the changes measured by relatives for individual commodity prices. Intercorrelations play such a role in price movements, however, that these conditions are the exception rather than the rule. They are more closely approximated when a considerable time period intervenes between the base year and the given year than when a short period intervenes. Thus, relatives for 1924 on the 1891 base constitute a distribution of the stable Type III.

Perhaps most interesting of the results obtained in this part of the study was the evidence relating to the behavior of a series of frequency distributions of price relatives on the 1913 base. From a point of extreme divergence from the normal type, in 1916 (a divergence greater than that of any distribution of observations, in any field, of which I have been able to learn) an extraordinary sequence of changes carried the distributions for subsequent years in an unbroken movement toward the Gaussian type. By 1923, a distribution conforming to the normal law of error had emerged. To a person who is somewhat familiar with the phenomena here being studied and with the fact that the Gaussian distribution is but one of an infinite number that may result from the interaction of complex forces, there is matter for wonder here. That the particular type of order exemplified by the Gaussian distribution should have been restored after the unprecedented disturbances to which the price system was subjected during the War and immediate postwar years is a manifestation of a remarkable tendency in the system of prices. Mr. Viner is able to draw upon a priori sources of knowledge that place this, with many other observations, in the category of things altogether to be expected. I find this still, at twelve years' remove, a dramatic and extraordinary thing.

I have referred in preceding sections to certain other points made in Viner's review. I should add a comment on his statements concerning the evidence of order and regularity in the price system that was presented in *The Behavior of Prices*. To Viner this evidence is unimpressive. This, of course, is to be expected. The constructs of pure theory have as chief characteristics symmetry, logic, order, invariable sequence. The confusions and disorders of reality do not introduce disturbing incoherencies into such systems. Knowledge is sharp

and clear.⁸⁸ A generalization not capable of empirical testing is not open to disproof or rejection. The investigator who works with actual observations foregoes the niceties of comfortable order that are to be found in the realm of pure conception.

Far less of order than is provided in the constructs of pure theory is to be expected, therefore, when data of observation are organized and interpretations sought. Yet numerous evidences of persistent tendencies, correlated variations, regularity in change are available. The introductory study upon which report was made in *The Behavior of Prices* did not purport to exhaust the materials presented, far less to explore all the possibilities open to the general application of methods there discussed. It did, I think, yield clear evidence of the existence of the kinds of order to be expected in phenomena reflecting the influence of many forces.

The objectively existing economic system with which we are concerned will always be marked by order intermixed with disorder, by continuities marred by discontinuities, by uniformities overlaid and distorted by irregularities. Knowledge of this system cannot be complete or definitive. It must be knowledge intermixed with doubt and uncertainty. It may be useful to conceive of fixed mechanical relationships, and even to base systems of thought or rules of procedure upon such concepts, but observable reality will fall short of the limit of complete uniformity and of certainty to which such mechanical relationships correspond. However, there

⁸⁸ Dr. Peyton Rous, speaking of medical knowledge, refers to a similar situation. "The student sees knowledge as exquisitely sharp and clear because his instructors have not had time to dwell upon its blurred outlines and nuances." In medicine, however, "This confidence that knowledge is precise cannot last more than a year or two." In medicine, the exigencies of practice force upon even the most reluctant an appreciation of the irregularities and uncertainties of reality. Cf. "Nature and the Doctor," *Science*, Nov. 25, 1938.

may be order, verifiable order, far short of this limit, order definable in terms of probabilities, but nonetheless order. Of course, we hope to push these probabilities, higher and higher, to find means of obtaining greater homogeneity, holding constant more and more of the disturbing causes, ruling out more and more of the forces that make for heterogeneity. I have no quarrel with Viner on this score. The use of appropriate principles of classification, the following of leads suggested by a priori considerations coming from any source whatever, the employment of all possible means of getting rid of the irrelevant and reducing the area within which uncontrolled and undefinable chance plays are means to this end.

Viner was necessarily limited in the space he could give to the substantive results of the study, in the development of this point. He does refer, however, to various statements and conclusions which he interprets as evidences of "failure to produce results." I think it well to give specific attention to certain of these "negative" results.

On page 100 of *The Behavior of Prices*, I gave measurements for 15 commodities, indicating the average timing of revival in their wholesale prices, after business depressions. The figures cited were averages of measurements relating to the time of revival in a number of cycles—10, or 11, for most of the commodities listed. Thus, pig iron prices showed a lag, in the pick-up after depression. Taking account of 11 periods of revival, pig iron prices reached their low points and started to advance 5.1 months after the general index of wholesale prices had started up. But, of course, the price of pig iron did not, in each of the 11 revivals, rise exactly 5.1 months after the general price index. There was variation among the 11 observations that had been averaged to obtain the figure 5.1. A measure of this variation was an obviously desirable

supplement of the average. As a measure of this variation, or an index of "consistency," I used the standard deviation of the 11 individual measurements. The value of the standard deviation for pig iron was 4.5 months.

I referred in the text to these measures of inconsistency. It happened that for two of the 15 commodities discussed at that point—pig iron and sole leather—the standard deviations measuring variation in the timing of price revival were smaller than similar measurements for a number of economic series usually considered excellent barometers of cyclical conditions—such as industrial stock prices, pig iron production, and the American Telephone & Telegraph Company's index of general business conditions. Although there was variation in the cyclical revivals in the prices of these commodities, yet their movements were more consistent than were those of the important nonprice series listed. Some of the other price series showed relatively low measurements of inconsistency and many were relatively high. There was no doubt as to the importance of the variation discernible in these figures. In any use of the averages, due account should of course be taken of the variation thus revealed. I called attention to this, saying

Even where there is a distinct tendency to precede or lag behind general prices, there is considerable variation in the degree of lead or lag. [adding] This is true . . . of most economic relations, and does not mean that principles of order cannot be found in the working of the price system. It means, only, that relations between prices are statistical and not mechanical in character, and that the interpretation and use of the data secured from studies in this field involve the use of the calculus of probabilities, rather than the rigid formulas of mechanics (pp. 101-02).

This is the example Viner cites (Viner, p. 178) of a 'failure to produce results.' My subsequent comment cited above is quoted by him as indicating how support is found for the method even in its failures. It is not captious, I think, to suggest that his remark indicates a misunderstanding of the nature and significance of variation.

As "negative results," Viner also cites statements to the effect that "individual commodities differ materially in the matter of price variability" and that "the variability of specific commodities has changed from period to period" (Viner, p. 173; p. 46, *The Behavior of Prices*). I believe that one of the suggestive results of the study was in the evidence it presented of a secular prewar decline in the degree of internal disturbance in the price system. The change in degree of price variability from period to period, therefore, far from being "negative," is a positive indication of considerable significance. That the recognition of differences between commodities in respect of their month-to-month variability and the numerical definition of the magnitude of this variability for individual commodities so that precision might be given to such comparisons should be looked upon as a negative result suggests an extraordinary attitude toward research. Of course, if the only type of order recognized as order is that which is uncomplicated by variation, the point of view is understandable. But this is to make extreme demands of the world of reality.

Again, Viner includes among the "negative results," evidences of the pronounced differences found among measurements of the average annual rates of change in price between 1896 and 1913. This particular characteristic of price changes over time, a characteristic revealed by studies of other periods as well as that of the period 1896-1913, is an important aspect of economic development and the finding is relevant to any attempt to generalize concerning the patterns of change over time—cyclical change as well as secular change. Equally important is the light it throws on the concept of "normal" economic relations. It points to the conclusion that there is no state of normality as regards price relations, no state that may be used as a standard of reference in defining degree of

departure from "normal," and to which return may be expected after divergence. Degree of rapidity of change among prices is suggested as a basis of study, rather than degree of departure from any hypothetical state of normality. Standards of 'parity' based upon any cross-sectional view of the course of economic change lose significance, in the light of this evidence.¹⁷

The persistence of the concept of "normal relations" among prices, a persistence reflected in the idea of parity prices for agriculture and in the argument of Bye (based in part, apparently, upon a misconception of some of my own conclusions) on a related point, justifies emphasis on this clear evidence of persistent divergence as the "normal" or "to be expected" condition among commodity prices.

I have discussed in some detail above the kinds of regularities found in the cyclical behavior of commodity prices. It is perfectly clear that there are wide divergences from uniformity in these cyclical movements, but it is equally clear that there are regularities of a number of clearly distinguishable types in these fluctuations. These regularities are unquestionably significant, in the sense that the play of random factors could not have yielded results of the type observed. Here,

¹⁷ The evidence for this finding consists of measurements based upon price series for 223 commodities during the period 1896-1913. The rates of change in these series are cited in the book and graphic illustrations of a limited number of series are given. Reproduction of charts for the 223 series would have been expensive and difficult. The original data are available, of course, in the *Bulletins of the U. S. Bureau of Labor Statistics*.

Viner questions the conclusion because of my own qualifications concerning the adequacy of exponential growth functions as trend lines for certain of the price series included. The function in question gives a good fit, in the trend sense, for the great majority of the series included. Even where it did not, however, there is justification for the use of r as a measure of the *net* change, in the cumulative sense implied in the use of the exponential curve, in the price series in question. Divergence among these measures of net change is here in question, not the measurement of cyclical or accidental deviation from lines of secular trend. The purpose in mind is admirably served by the growth function employed.

again, Viner refers to the divergencies as "negative results." True, they are, in a court where only bull's-eye shots and invariant relationships have standing.

I have already referred to the characteristics of frequency distributions of price relatives, have noted the types that emerge and have suggested certain of the conclusions concerning the stability of wholesale prices viewed in the aggregate, as populations of phenomena reflecting the play of diverse forces. The variations of type, and the existence of forms deviating very widely from the stable Gaussian and Type III distributions, were among the results to which attention was called in the study. Viner, who was led on a priori grounds to anticipate just such a remarkable emergence of the Gaussian distribution after the disturbances of the War as actually occurred, is also prepared, on a priori grounds, for the appearance of these widely variant distributions. To him, these also constitute evidences of purely negative results.

If I say that variation is omnipresent in the biological and social worlds, that the measurement and interpretation of variation and of co-variation are central tasks of research workers in these fields, that much of our work consists of the search for order amid variation, I say nothing that is not entirely familiar. Revelation of the fact of variation, and the giving of precision to measurements of individual attributes and to corresponding measurements of group variability, do not constitute final stages of any study. They mark, usually, the early steps, the initial operations that raise questions and reveal problems calling for further study. *The Behavior of Prices* did something, I think, to reveal the existence of order amid variation, to indicate certain of the co-variations that tend to confirm, or suggest, ideas relating to causal interconnections, to point to some of the patterns of change that appear to exist in the cyclical fluctuations of prices. But I am

content to think that it raised many more problems than it settled. In defining in quantitative terms a number of the attributes of commodity prices—variability, frequency of change, trend over time, cyclical behavior—it presented just those problems that evidence of variation always raises. Why are there differences among commodities in respect to frequency of change in price? What are the economic consequences of such differences? Do these differences change over time? Are they related in measurable ways to other factors involving labor costs, overhead costs, quantities produced and demanded, degree of competition in the markets for these products? These and many other questions are raised by the mere existence of measurements defining with precision this aspect of price behavior, and similar questions are raised by measurements relating to other aspects of price behavior. Some of these questions have already been explored by others. Many of them call for further study. With reference to these questions, *The Behavior of Prices* did not profess to do more than take a first step toward their definition and solution.

Conclusion. I have sought to make clear the objectives and methods of the study upon which report was made in *The Behavior of Prices* and to present some indication of the nature of the findings. Some of the clear limitations of the study have been alluded to in the preceding discussion. In part, these limitations were imposed by the plans for the study and the nature of available data. In part, they are faults that might have been avoided with better instruments and better judgment.

As defects of data are removed, far better studies of price behavior will be made and our knowledge of the price system materially enhanced. We know that many of the series of quoted prices of individual commodities are inaccurate as indicators of the actual prices at which goods change hands.

Allowances on sales at contract, discounts and various concessions from quoted prices and undercover price-cutting are generally not reflected in the current quotations that make up available price lists. If these concessions represented constant percentages of quoted prices, the relative movements might not be greatly affected, but we know that the concessions vary from time to time. For commodities affected by such concessions (these, of course, do not constitute a major proportion of the commodities studied), our records of price behavior are inaccurate. Again, certain of the series included may be accurate in themselves but inadequately representative of the prices of similar commodities in the general market. Since price studies must be conducted on a sampling basis, there should be assurance that a representative sample has been obtained. This has further implications. It means not only that the series employed must be checked to ensure that they reflect price changes in the commodity groups they represent but that all important commodity groups in the universe of inquiry should have representatives in the sample under investigation.

I am more keenly aware now than I was in 1925 of defects of coverage and inaccuracies in the individual price series included in the sample we employed. This sample was (as it remains today) the only large and reasonably well authenticated body of wholesale price quotations available in this country. It was an obvious choice for study in an enterprise concerned with the behavior of prices in the mass. Without far more resources than are available to a private research organization, it would be impossible to develop an alternative body of data. Yet extension of the list and more intensive appraisal of the individual series would have been desirable. It is a source of satisfaction that others are doing better, that individual price series are being critically appraised, and

more accuracy sought in these all important records. The Bureau of Labor Statistics is itself taking the lead in a program of improvement. Investigations based on these improved series may not substantially alter the general picture derived from the use of less adequately tested data, but the record of price behavior will be more accurate in detail and more confidence will attach to the general results.

Further intensive study in one direction and the extension of such studies in two other directions are desirable. These needs have been pointed out by reviewers and have been clearly recognized from the beginning of the investigation. Intensive work is needed, of course, in the analysis of the body of data relating to wholesale commodity prices. Significant categories of goods marked by distinctive modes of price behavior are to be sought and their attributes determined. I have spoken above of past work and future plans relating to such analysis. But the price system is far broader than the system of wholesale prices, important and strategically placed though the latter be. Factor prices, retail prices, prices of securities, call for detailed study, looking toward the determination of their special characteristics and of their relations with other elements of the general system. Work has, of course, been pushed in these fields over many years but there is still much to be done in a systematic program of study. Equally important is the extension of such investigations to include non-price factors. In detail, and for a limited number of commodities, this has been done by students of demand. General studies of economic change have taken account of the related movements of prices, production, stocks, profits, and other phenomena, viewed in the large. An investigation aiming at the compilation of detailed measurements of price behavior in relation to changes in associated nonprice factors, measurements that could be studied in the mass in getting a picture

of economic change having the perspective of an over-all view and the advantages of accuracy in detail, faces extraordinary difficulties. Measurements of elasticity of demand and flexibility of price, both simple and partial, would provide some of the technical information about correlated changes necessary in such a study, but relatively little has as yet been done in the actual derivation of these measurements for individual commodities. I do not know how far such attempts may take us. However, in this or other directions, we must work toward the further development of methods adapted not alone to the study of the behavior of prices but of the behavior of the elements of economic life in combination.

The need of rational direction in such studies, of suggestive concepts and fruitful leads to the organization and interpretation of data, is not open to question. There is more need today to emphasize the other aspect of economic research—observation and open-mindedness to what may be learned by observation. Economics has suffered and still suffers from ingenious ideas, appealing systems, excellent conceits that offer marvelous opportunities for discussion but lend themselves not at all to the accretionary development of knowledge. The ingenious fabricator of such conceits, who barely completes the elaboration of one system before he discards it for more stately mansions, has, doubtless, a place in the scheme of things. But system building, facile generalization, and precipitate explanation of phenomena not yet adequately perceived may actually impede progress. Energies may be wastefully expended, observations may be hampered, and inquiry may be checked by a false but comfortable sense of understanding. Reasoning that transcends experience has an important part to play in the advance of economic knowledge, but the ultimate realities of economics are those revealed by observation.

REVIEW OF *THE BEHAVIOR OF PRICES* BY JACOB VINER

This¹ is a systematic study of the behavior of wholesale commodity prices, and of the various statistical devices for measuring and otherwise describing such behavior in convenient summary form. It gives evidence throughout of that high degree of technical competence and of inventive ingenuity to be expected in any statistical work by Professor Mills. For individual price series and for price averages, the following aspects of price movements are studied in detail: variability about various means; trends; and timing, duration, and amplitude of cyclical changes. For the behavior of price relatives in groups, the following aspects are studied: index numbers; frequency distributions and their criteria; dispersion; displacement; variability; and cyclical changes. The book is a valuable repository of laborious computations in terms of these devices and methods for some two hundred wholesale commodity price series for the period 1890-1925. It is a contribution of great importance to the history of prices in the United States since 1890. It makes an impressive addition to the laudable record of achievement of the National Bureau of Economic Research.

Appraisal of the contribution of this volume to the technique of statistical description, the reviewer must leave to competent statisticians. The significance for economic theory of the technique employed and the results obtained is the only concern of this review. The book presents a wealth of raw material for the theorist to ruminate upon. Hypotheses clamoring for a test spring up for almost every section. A rich field of theory, namely, the statistical study of the objective behavior of prices in relation to other economic factors, needs for its further exploitation just such techniques of statistical description as are here admirably expounded and per-

¹ *Quart. J. Econ.*, February 1929, Vol 43:337-352.

fect. In this sense, the volume makes an indisputable contribution to economic theory, in addition to its contributions to economic history and to statistical method per se.

But the book sets up claims for more—to having made a specific and direct contribution to economic theory. For the objectives of this study are stated to be:

First, to secure a fuller understanding of the behavior of individual commodity prices and, secondly, to increase our knowledge of the working of the price system and of the interrelations between its component elements. To attain the first objective we shall make use of various methods of measuring the characteristics of individual price series. In seeking to attain the second, we shall be searching for uniformities and regularities in the behavior of prices in combination. . . . We shall be seeking principles of order in the realm of prices. [Page 31.]

The concluding paragraph is also pertinent in this connection:

The second fact of importance is the existence, among the diversity of price movements, of just those uniformities for which the scientist searches in attempting to reduce masses of facts to understandable terms. The present investigation and those which have preceded it in this field have gone only a little way in the search for principles of order among the variations within the price system, but there have been revealed numerous interrelations and uniformities, and there have been found many clues to other regularities. In the existence of these regularities lies our hope of achieving a fuller understanding of the working of the system of prices. [Page 439.]

Professor Mills promises a second volume, but the only indication of its contents is the statement that “the remainder of the present volume and all of the volume which will conclude the present study deal with the system of prices. For a study of the behavior of prices in combination is a study of the price system.” [Page 214.] This suggests that the contents of the second volume will not be different in kind from those of the second half of the present volume. Moreover, Professor Mills makes definite claims for the contribution of the present volume to the explanation of price relations. It

seems, therefore, that the examination and appraisal of the adequacy of his methods for the purpose of throwing light on price relationships can be made without the reservation that the author may regard the work so far accomplished as, for this purpose, merely preparatory to a different type of analysis in the forthcoming volume.

For the price theorist, the points of outstanding interest in this volume are: the faith expressed by the author in the existence of a "price system"—"a system infinitely flexible in detail yet stable in the essential balance of its interrelations, a system like a living organism in its ability to recover from the serious disorders into which it periodically falls";² his description of the characteristics of such a system; and his confidence that his method of analysis is adapted to, and that his investigation has contributed to, the demonstration of its existence.

The outstanding contribution which the volume achieves is the reduction to precision and measurability of concepts that economists generally use vaguely and loosely. It is therefore unfortunate that the reader is given so little help in determining just what constitutes a "system" of prices and how one is to know when the "order," the "regularities and uniformities," which are apparently the criteria of a "system," have been discovered in sufficient degree to warrant confidence in its existence. With a few minor exceptions with which he deals only parenthetically and briefly (namely, short studies of elasticity of demand and of regional differences in prices, and occasional illustrative references to economic series other than commodity prices), Mills confines himself to the study of the relation of prices to prices; he deals only with wholesale commodity prices; and when he deals with prices in combination, he bases his subgroupings only on chrono-

² Quoted by the author (p. 31) from W. C. Mitchell's *Business Cycles*, New York, 1913, p. 31.

logical criteria or on differences in quantitative behavior. With the minor exceptions referred to above, he avoids any other grouping of prices that might be suggested by economic theory or hypothesis, and he refrains from any search for relations between the economic characteristics of particular commodities or subgroups of commodities and their price behavior. He does not deal at all with the one type of price behavior that indisputably and obviously is closely related to fluctuations of a factor external to prices, namely, seasonal fluctuations.

Mills concedes, of course, that there are other things in the economic order than prices, but it appears to be his position that the "price system," though a part of a broader whole, has an independent existence of its own as a system, marked by ascertainable principles of order and regularity. He refers to Walras, apparently in support of his own position: "Leon Walras, in describing the conditions of general economic equilibrium, defined in mathematical terms certain characteristics of a system of related prices. In the Walrasian system, prices are one element in general economic equilibrium." [Page 213, note.] To Walras and other price theorists, it may be hazarded, the second sentence in this reference would have been vital as in some degree corrective of an ambiguous note struck by the first sentence. To the price theorist, wholesale commodity prices are *an* element in the economic system, as are other classes of prices—prices of the factors of production, retail prices, prices of services—and as are also demand and supply functions, market structures, the technical conditions of production, and ultimately the wants and aversions, the habits and customs, of mankind. The theorist would agree with Mills that "no one price is an isolated independent phenomenon" [page 213], but he would assert it with equal vigor of wholesale commodity prices in combination, and he

would deny that "an individual price has significance only in its relation to other prices" [*loc. cit.*], if this means that its relations to other prices are, by themselves, sufficient data from which to appraise its significance. He would agree that "every price is connected by immediate or remote bonds to every other price," in the sense that all prices are connected; but he would insist that the bonds are never immediate, that prices are never related to each other directly, but always by means of a connecting tissue of demand and supply functions. He would probably be skeptical, also, of the possibility of getting much insight into the character of the relations between prices in the absence of any reference to this connecting tissue. It is interesting to see, therefore, what light can be thrown on the relations between prices, and on the existence and characteristics of a quasi-independent system of prices, by a method of analysis which deliberately leaves these considerations out of account.

The essential characteristics of the method are the restriction of the data investigated to commodity prices and time, the avoidance of use of any hypotheses or rationalized theories, and the search for evidence of regularities in price phenomena which would reflect the existence of principles of order in price relations inter se. If a biologist were deliberately to inhibit himself from searching for associations between distinct and obviously nonhomogeneous classes of phenomena, such as height and race, but were to deal only with the "behavior of heights"; if he arbitrarily limited his investigation to heights of persons under a given age—ten or fifteen years—but without further differentiation according to age;⁸ if he based his groupings of heights merely on chrono-

⁸ It might be objected that the suggestion here of a sample that is clearly unrepresentative is unfair to Mills. But are heights of persons under ten less representative of heights of all persons than are wholesale commodity prices of all prices?

logical order and degree of change of his height data, without reference to any special qualities of the different persons or the different units of time; if he scrupulously avoided making use of any biological theory or hypotheses; and if he sought by such means to demonstrate the existence of a "system of heights," the investigation would seem to approximate in method that undertaken in this volume. Mills' application of this method to wholesale commodity price data is indisputably thorough and able. The character and quality of the results obtained should provide a fair test, therefore, of the usefulness of the method as a method of price theorizing.

In examining the results of Professor Mills' investigation for indications of order and regularity in price phenomena, it will be convenient to follow the division made by him between measures of the behavior of individual prices and measures of the behavior of prices in combination.⁴ The first indication of regularity in the behavior of individual commodity prices noted by Mills is the existence, before the war,

of fairly constant rates of change in individual commodity prices, rates of change which differed materially from commodity to commodity and from group to group. What was constant in the prewar price situation was not a set of fixed price differences but relations which changed at fairly regular rates year by year. [Page 74.]

This finding is unsupported, however, by any evidence beyond the bare statement that "the exponential curve gives, in general, a good fit to most price series for the years 1896-1913." [Page 67.] Decision as to what weight should be given to this finding obviously calls for some indication of the number of price series for which this curve gave a good fit, and the standards of goodness of fit that were used.

⁴Of the four chapters in the present volume, the first two deal with measures of the first sort, and the last two deal with measures of the second sort.

For cyclical behavior of individual price series, Mills has found significant correlation between a number of aspects of the timing, duration, and amplitude of rises and of falls of the individual price series.⁸ As indicative of the successful results obtained, the following verbal summaries may be quoted.

The commodities which rise first on revival are subject, in general, to wider cyclical fluctuations than are the commodities which lag on revival. Similarly, the commodities which decline first on recession are marked by wider fluctuations than are those which lag. [Page 158.]

There is a clear tendency for the sequence of revival [for each individual commodity price series] to follow a common pattern during different cycles. The same tendency is observed in studying the sequence of recession in different cycles. [*Ibid.*]

The evidence upon which the preceding statements are based is of considerable general significance in showing that there are true economic regularities in the price movements which accompany cycles in general business. In cycle after cycle there has been observed a degree of uniformity in the sequence of revival and recession in commodity prices and in other aspects of price behavior. If cyclical movements represented the play of mere variability about a mean or a trend, one would expect the sequence of price change in each cyclical swing to be unique, except for certain chance resemblances to movements at other times. Yet the odds against chance alone accounting for the regularities we have found are infinitely great. In period after period there is a recurrence of price movements which have something in common, in respect to sequence of change, amplitude and duration. These changes in different periods are far from showing perfect uniformity, but there is unmistakable evidence that the observed resemblances would not be found if the cyclical movements of individual prices represented random fluctuations alone. The phenomena of business cycles show just those regularities and uniformities which it is the business of the scientist to discover, trace and, if he can, explain. [Page 159.]

These are the only regularities in the behavior of individual price series which Mills claims to have disclosed by his analysis. With the exception of the constancy of the rate of change of individual price series before the war, they all relate to cyclical behavior.⁹

⁸ See his Tables 42-50. It should be pointed out that while Mills refers repeatedly to "business cycles," the subject matter of his analysis is price fluctuations and not business fluctuations.

⁹ The coefficients of correlation found by Mills between the various measures

To some extent, the degree of consistency found in the cyclical measure of behavior of the individual series may be imputed to the persistence throughout successive cycles of some economic characteristics of individual commodities, whose nature is not revealed by analysis such as this, confined to price behavior alone. To this extent, such measure of success in finding regularity as Mills has here attained may be attributed to the fact that the method of analysis used, *in spite* of its failure to take the economic characteristics of the commodities expressly into account, permits such of these characteristics as are persistent to exert an influence on the results. To the extent that these persistent economic characteristics are known or suspected to be related to their cyclical price behavior, Mills has achieved the occasional discovery by chance, and without any contribution to explanation, of relations which, *a priori*, he could have discovered with less waste effort and in more conclusive form if he had applied his technique of analysis to both price and nonprice data, selected with the aid of known and plausible hypotheses with respect to the relations between commodity characteristics and their cyclical price behavior.

In order adequately to appraise the value of this method of attack, account should also be taken of the analyses which failed to produce evidence of significant relations. It is im-

of cyclical price movements indicate the degree of regularity of cyclical behavior, not of all the price series investigated, but only of a series of prices selected on the basis of the conformity of their monthly variability to the general cyclical framework, outlined by the author. The omitted series include: (1) the prices of 26 commodities classed as "exceptional" because their failure to conform to the standard schedule of cycles was pronounced (p. 81); and (2) 34 other commodities, which passed through less than five complete cycles between 1890 and 1925. This leaves only 149 out of 209 price series to which the analysis was applied. [Page 128.]

Even for the included series, only such parts of the series were used in computing these coefficients as conformed in their cyclical behavior to the cyclical behavior of the wholesale commodity price index number. For each cycle, on the average about 115 of the 209 series were used.

possible here to list all the instances in which the analysis admittedly produced no evidence of regularity. A few quotations from Mills will indicate the character of these negative results:

It is clear from Table 4 that individual commodities differ materially in the matter of price variability and, also, that the variability of specific commodities has changed from period to period. [Page 46.]

The wide differences between the measures for different commodities give evidence of the diversity of forces which are responsible for fluctuations in the prices of economic goods. [Page 48.]

Measures [of average annual rate of change in price] have been computed for 223 commodities in all. The pronounced differences between these measures indicate the degree of change which was taking place in the relative positions of different commodities. [Page 69.]

There was no normal pre-war relation between prices, in their absolute form. [Page 74.]

There are wide divergences from any common pattern in the [cyclical] behavior of individual commodity prices. [Page 81.]

The evidence of order and regularity found by Mills in the behavior of prices in combination is, relative to the extent of his investigation, even less impressive than the evidence of regularity in the behavior of individual prices. Here the prices of all commodities are merged in some form of average or of distribution, so that year-to-year comparisons are of the changes in averages or distributions of all the prices, and not of changes in the prices of particular commodities kept distinct from each other. For the types of measures of behavior here used, persistent economic characteristics of particular commodities will therefore be less likely to result in a tendency to stability of behavior of the measures than where, as in the first part of the study, each price relation forming the basis of an observation is the relation between the prices of *the same commodity* at different dates.

A significant correlation ($+0.614$ for 35 pairs of observations) is found between degree of change—regardless of direction—in the price level and degree of dispersion of prices. [Page 284.] All that it is necessary to adduce in explanation

of this correlation is that, in addition to the many influences affecting particular commodities which operate to bring about dispersion of prices even when the price level is constant, active pervasive factors tending to affect all prices will further increase dispersion if different commodities have varying degrees of resistance to their influence.

In distributions of fixed base relatives, there appears a tendency to depart from what Mills, following Kelley, calls "stable" frequency types during years of violent price disturbances and to approach more closely to them when price conditions became more stable.

It is a curious and dramatic thing, this march of the successive Beta points back to the Gaussian point [during the years 1919 to 1923 inclusive]. . . . It exemplifies in striking fashion the emergence out of chaos of that form of order which the play of sheer chance brings. Here is nature forming habits. [Page 347.]

Mills himself points out that the price relatives on a fixed and *distant* base have been subject in the course of years to the cumulative and joint effects of a great many different factors; that in quiet years the price relatives computed from a distant base would reflect the influence of a great number of minor factors instead of the dominant influence of a small number of pervasive and powerful factors; and that the interrelations between particular factors would have weakened during the passage of time. In quiet years, the frequency distributions of price relatives on a fixed and distant base would, therefore, meet the requirements for the production of good expectancies on the basis of the law of chance more closely than is usual for undifferentiated price data such as are here dealt with. They would, therefore, be more likely to approximate one or the other of the so-called "stable" types. That this should have suggested a sense of the dramatic indicates how rare is the occurrence of situations adapted to the fruitful application of the calculus of probabilities to price data,

without previous recourse to economic analysis and classification of such data.

In the frequency distributions of logarithmic weighted year-to-year link-price relatives, pronounced positive skewness is generally found during years of rising prices and sharp negative skewness is usual during years of falling prices. [Pages 349, 350.] In other words, in years of rising prices a greater proportion of the prices are likely to show very high relative increases than correspondingly low relative increases (or decreases) over the preceding year, and in years of falling prices a larger proportion of the prices are likely to show very high relative declines than correspondingly low relative declines (or increases) as compared to the preceding year. This holds for almost every year since 1891, and the exceptions appear to be mainly years in which there was a sharp reversal of trend of prices within the year. This result is interesting, but is what should be expected, since there is reason to anticipate, on a priori grounds, that some commodities will be highly sensitive to a general price-disturbing factor, and thus will tend to bring about a skewness in the direction of the general change.

On the basis of his measures for the period 1890-1914, Mills finds a notable degree of year-to-year stability in the mean monthly variability of the measures for individual commodities:

The relative stability of the averages from year to year, prior to the war-time disturbances, is worthy of note. . . . In 20 of the 25 years [the average] is between 4.0 and 5.0. We may judge from this considerable sample that except in times of such extreme price disturbances as were brought by the war, the amplitude of the fluctuations of individual prices, viewed collectively, does not vary greatly from one year to the next. The cyclical swings of the general price level are hardly apparent in these averages, which take account of all price changes. [Page 374.]

In the absence of extreme price disturbances and of cyclical

changes in prices, there would be no apparent reason to expect a lesser degree of stability than that indicated by the data.⁷ After the elimination of pervasive factors—of which extreme monetary disturbances and cyclical price-level changes are the important ones—which tend to produce large yearly variability in all prices, the seasonal, trend, and myriad casual factors should, on probability grounds, be expected to leave the average monthly variability substantially unaltered from year to year.⁸ The main point demanding explanation, therefore, is the failure, which Mills emphasizes, of the annual averages of monthly variability to reflect the recurrent cycles of rise and fall in the general wholesale commodity price level. The guess may be hazarded that a partial explanation lies in the mutually counteracting effects, on gross monthly variability, of upward or downward trends, on the one hand, and of seasonal fluctuations, on the other. If the price series of commodities particularly subject to seasonal fluctuations in prices were separately grouped, it might be found that their average monthly variability was less during periods of rise or of fall in the general price level than during periods of stability in the price level.

The frequency distributions for individual price series of (a) duration of price rise during periods of revival and prosperity, and (b) duration of price recession during periods of recession and depression, both show positive skewness, but the skewness is less for (a) than for (b). [Pages 405 ff.] There seem to be obvious explanations both of the positive skewness in these distributions and of the difference in degree of

⁷ The measure of year-to-year variability found by Mills in the annual average of the month-to-month variability of the 206 individual series does not appear to me to be strikingly low. Thus, for the period 1900-14 it is 10.1, which is higher than the average year-to-year variability of at least 137 of the individual series for the period 1890-1913. [Page 375, and Table 216, p. 377.]

⁸ Unless some factor, such as, for example, improvement in storage facilities, should be tending over time to lessen the importance of seasonal fluctuation in prices as a contributory source to the gross average monthly variability of prices.

skewness. Any frequency distribution of performances requiring stamina or skill—as, for instance, endurance tests, where low values are the easy values and high values become progressively more difficult of attainment while still continuing to be possible—would tend to show positive skewness of the type found here, that is, a long tail in the direction of high values (in this case, *long* duration). Continuous repetition of price changes in a given direction would, in a variable world, appear to be of this nature. The general upward trend of prices during the period under examination would explain adequately the greater positive skewness of the distributions of duration of price rise than of price fall. During this period a commodity was more likely to maintain a very long-continuing upward price trend than a very long-continuing downward one. If the trend were to be taken out of the data, no pronounced difference between the two distributions in degree of positive skewness should be expected.

These are the only indications of order and regularity in the behavior of prices in combination, found by Mills, which are pronounced enough to call for comment. They do not seem impressive, in the face of the abundant evidence of apparent instability and irregularity in price behavior to be found in this volume. Indeed, the most striking regularity disclosed is the regularity of irregular and seemingly erratic behavior. In general, Mills himself is struck by the absence of the degree of order and stability which similar investigation in other fields could be expected to reveal.

A distinguishing feature of distributions of price relatives is that they do not conform to any one type, but undergo marked changes with variations in price and business conditions. Most distributions of homogeneous data from biological, anthropological or other scientific fields, tend to conform to a common type, without sharp variations from sample to sample. [Page 354.]

Distributions of price relatives are of an erratic and extremely unstable character. [Page 368.]

We are dealing with a population which seems to contain inherent elements of instability. This population is subject to violent internal changes which are reflected in radical alterations from year to year in the form and character of the frequency distributions. [Page 340.]

Evidence of irregularity is itself, of course, a sort of result. But when sought in fields in which there are no a priori grounds for expecting to find regularity, it is the easiest sort of result to obtain. In any case, it is not evidence of this kind that is the objective of Mills' analysis.

For price theory as such, the immediate harvest from this elaborate investigation seems scanty. The main significance of its results for theory may perhaps consist in its new and empirical basis for skepticism as to the wisdom of reliance on empirical trial-and-error methods, unaided by hypothesis; and in its unintended contribution to a revived faith in the usefulness of price theory in explaining the relations of prices to each other and to other economic factors. But Mills finds support for his method even in its failures to produce results. Having found large measures of inconsistency in the timing of price rises and recessions of different commodities during business cycles, he makes the following comment:

This is true, however, of most economic relations, and does not mean that principles of order cannot be found in the working of the price system. It means, only, that relations between prices are statistical and not mechanical in character, and that the interpretation and use of the data secured from studies in this field involve the calculus of probabilities rather than the rigid formulas of mechanics. [Page 101.]

Toward the end of the eighteenth century there was, in certain advanced circles in France, somewhat of a trend toward the belief that the possibilities of the traditional non-experimental methods had been exhausted (not, however, that they had never had any possibilities), and that further progress could be made only by resort to the calculus of probabilities.⁹ There seems more ground for such belief in

⁹ Cf., for example, Condorcet: "Peut-etre meme, dans plusieurs branches des

the twentieth century than there was in the eighteenth. But at its best, immediate application of the calculus of probabilities to unanalyzed data is a *pis aller*, a makeshift, to be resorted to only when no means are available of finding information or plausible hypothesis about the relations of these data to each other and to other classes of data. Mills' own investigation demonstrates that run-of-the-mine price data, ungraded or graded only according to a few arbitrary characteristics, display, in high degree, lack of homogeneity, instability in their criteria over time and space, absence of mutual independence and, during, any particular disturbance of moment, subjection to control by a few particular variables of predominant but unbalanced importance. The data to which Mills applies his analysis seriously violates, therefore, every one of the conditions requisite to the production of reasonably good expectancies by the application of the calculus of probabilities. In working with empirical data where perfect experimental control is wholly out of the question, recourse to methods involving some degree of reliance upon probability theory is necessary. But the data must be so prepared that they are suitable for analysis by such methods. Data which in their original form do not meet the tests of homogeneity, independence, and freedom from control by a few powerful and unbalanced factors, can be transformed, by proper groupings, into data meeting these tests adequately. Such groupings are unlikely to be come upon by chance, or to be found even by exhaustive experimentation with all the permutations of price-to-price relations alone. The suggestions for such groupings must be sought, in the main, in the propositions and by

sciences politiques, approchons-nous du terme ou tout ce que la raison peut faire seule sera epuise, ou l'application du calcul [des probabilités] deviendra le seul moyen de faire de nouveaux progres." *Rapport et Projet de Decret sur l'Organisation General de l'Instruction Publique* (new edition, Paris, 1883, p. 104; original edition, Paris, 1791).

the modes of analysis of some generalized body of theory; a theory formulated necessarily in quasi-mechanical terms because it is only in such terms that we know how to summarize, organize, and synthesize our detailed knowledge relating to many interdependent variables. Resort must then be had to probability, in order to appraise the significance of the deviations of the observed data from the inferred quasi-mechanical law. The fact that actually observed relations never reveal the rigidity and perfection of those posited by the mechanical theory neither impugns the validity of that theory in its own universe of thought nor warrants lack of faith in its fertility as a source of promising hypotheses which are to be tested in the realm of statistical relations. Disappointment with the degree of light thrown on price relations by Mills' analysis does not rest on his failure to find rigidly perfect mechanical relations; for these are not in question if they existed here, and would not be revealed by this type of statistical analysis. It is the meager showing of statistical "probability" results of the sort Mills is himself looking for, of the appearance of order in data selected at random, which is disappointing. Professor Mills has made a valuable contribution to the statistical tools useful in the search for answers to questions concerning economic relations, once the questions have been asked. From the point of view of economic theory, the only criticism to be made of his work is his apparent satisfaction with a technique ill adapted to finding out *what questions to ask*.

REVIEW OF *THE BEHAVIOR OF PRICES* BY BRUCE D. MUDGETT

We¹ are prone to think of progress in science in terms of its significant achievements, the sign posts of new roads—

¹ *Proc. of the Casualty, Actuarial, and Statist. Soc. of Amer.*, May 24, 1929, 15:265-272, pt. II, no. 32.

those phenomenal generalizations that alter fundamentally the direction of scientific effort: Newtonian gravitation, Einstein's relativity, or the discovery of vitamins. Without in the least discounting their importance or the personal achievements of their discoverers, we should not forget that they do not stand in isolation but result in general from the cumulation and sifting of vast amounts of evidence, that out of many failures comes one success. The accumulation of evidence and the development of new methods of handling it are oftentimes the forerunners of new and significant achievements.

It is with some such thoughts as this in mind that one must approach the task of evaluating Professor Mills' book *The Behavior of Prices*. There has been no attempt here to form broad generalizations about price behavior, indeed very little of interpretation in any degree; but a vast accumulation of evidence and a development of method. There is implicit in all these pages the thought that the history of prices contains much of importance for the understanding of economic activity and also for the progress of economic science. There have been great developments in the measurement of price levels; there have been attempts, such as that of Mitchell, to see into the complex interrelations of prices in a coordinated *price system*; and there have been brief studies and suggestions for further study of price dispersion, e.g., Fisher, Flux, and Crump. These efforts reflect a growing conviction that prices needed to be studied in greater and greater detail, that averaging was not enough but must be supplemented by other characteristics of price populations or by studies of individual price series. *The Behavior of Prices* is a significant achievement, born of the conviction of an able student of prices that these further things needed to be done, and brought to successful completion by the aid of the National Bureau of Economic Research. The unaided efforts of a single worker

could not have completed such a study as this short of many years of labor—much of one man's working lifetime.

The author states, in one of the last sentences in the book, that "the immediate objectives of the present study are the development of a method of analysis and the accumulation of a body of facts which may conduce to an understanding of the price system" (p. 437); and he had said in the beginning (p. 37), "The present classification (of the characteristics of price series) is an experimental one—and is not to be looked upon as rigid or exhaustive." No judgment concerning the book as a whole or concerning particular methods of analysis can therefore be passed without constant reference to this fixed purpose; or again, one may feel that a particular bit of analysis has not carried far in a useful direction, and yet be unable to offer anything in its place. Such is the path-breaking character of this study. In most cases, the author has applied known standard methods of analysis; the newness lies in the application. But in other instances he has had to develop entirely new methods.

As said above, little has been done previously in the study of price behavior beyond the construction and study of price indexes with some attention given to dispersion of the group that is averaged. This study goes back first and fundamentally to the individual price series and seeks to analyze their behavior before considering the behavior of groups of prices. The basic material upon which the study is made is found in the wholesale price bulletins of the U. S. Bureau of Labor Statistics. Chapter II, 160 pages, is devoted to the analysis of individual series and measures of the amplitude and frequency of price change, of trends, and of cyclical fluctuation are developed; and the individual series compared with respect to each.

Two measures of the amplitude, or variability, to use the

author's term, of price movements are constructed, one for monthly and one for annual series. The first, the average deviation of the monthly items of each year divided by the year-average and expressed in percentage terms, is calculated for each year 1890 to 1925, the complete results being shown in the appendix for a small number of commodities; for the complete list of commodities, results are shown only as averages of this coefficient for groups of years, 1890-97, 1898-1905, etc. To measure year-to-year variability, the author employs the mean deviation from the mean of link relatives of average annual prices—two such measures being given for each commodity, one for the years 1890-1913 and one for 1890-1924.

The chief consideration in the choice of the first measure, that for monthly prices, was that "price changes themselves are facts of immediate concern to producers and consumers 'and such a measure' may be accepted at its face value and compared with similar measures for other commodities" (p. 41) and on this point there would be general agreement; but the author says also that "Each annual measure relates to price variations within the year, reflecting movements due to all causes, secular, seasonal, cyclical or accidental" (p. 40), the accuracy of which statement is in some doubt. Monthly variability within the year as measured by the average deviation of monthly items may vary with the secular trend but when this measure is divided by the mean annual value of the monthly items, the influence of trend upon the resulting coefficient is insignificant. And again, this base, or divisor of the ratio, may contain considerable or cyclical or accidental elements, so that the final percentage probably contains nothing more than residual elements of the cycles or accidental fluctuations in the series, and again, when the period averages are taken, there is almost certainly a cancelling tendency for these residuals, with the result that the variability index in

question measures with a high degree of probability the whole of the seasonal influence plus an unknown portion of cyclical and accidental influences and no secular at all. With the author's decision not to attempt to separate these various elements of variability in his coefficient, there is no objection and there is agreement with his main defense of the coefficient; but in this explanation of it, possibly a minor point to him, I believe that he is in error. And to me, the point is not minor, for before we have reached ultimates in our analysis of price behavior, we must deal analytically with these separate effects of separate causal forces. An earlier comment, however, is pertinent here: there is nothing better at present to offer in its place, and, regardless of its interpretation, the coefficient supplies important information as to the facts of varying average monthly variability.

The yearly coefficient, as the author states, in effect measures variation over a period of years from a constant percentage rate of change, since if the link relatives had a constant value, the average deviation would be zero. This index, he says, reflects movements due to forces other than seasonal and very short time, that is, essentially, due to trend and cycles. As with the monthly measure, this index is supported mainly on the ground that actual changes from year to year are the ones which mostly affect business dealings and economic relations; and it is preferred to its chief alternative, deviations from a line of trend. It is true that deviations from a hypothetical trend may involve bold assumptions about the measurement of trend, but one may question the statement (p. 50), "Actual fluctuations, not departures from a hypothetical trend are the significant measures of variability," since variability that is cyclical is significantly different from variability that is secular. But as a matter of fact, the major contribution to this annual variability coefficient is cyclical and not secular, for

a large share of the secular element is necessarily removed by the process of taking link relatives. One cannot help a regret that the author did not grapple in a more fundamental way with the important problem of trend. A coefficient based upon deviations from trend would have been a measure largely of cyclical influences, but with some trend influences present due to failure correctly to measure trend; and that, as I see it, is just what the present coefficient is, with the limitation that the trend influence is probably greater than it would have been had approximate trends been adapted to each individual series, whereas the present method in effect rigidly hypothecates a simple exponential trend in every case. The coefficient as it stands is an *approximate* measure of the effect of cyclical influences upon each of the two hundred odd commodities, and as such gives a good basis for comparing them with reference to the importance of cyclical factors over the periods of comparison.

For a measure of the *frequency* of price change, the author takes the ratio of the total number of changes (applied to monthly data only) to the total number of months less one, the coefficient thus having limits of variation from zero to unity. He calls attention to the desirability of a shorter time interval for a comprehensive investigation of frequency of price changes and to the difficulties inherent in this measure resulting from the fact that many series used are averages of several constituent series, or of similar series in several markets or of different days in the month. He is right, of course, that the fundamental measure of frequency of price change should be confined to a single series in a single market and should be based upon a complete record of all actual changes that have occurred. For these reasons, it seems to me that this coefficient must be ranked in importance far below the measures of monthly and annual variability.

The next subdivision of Chapter II is devoted to a study of the trends of individual price series, for the years 1896 to 1913. While a comprehensive treatment of the problem of trends would necessitate special study of the growth factor in each series and the probability of many different methods of measuring their various trends, the author's purpose has not been this, but rather such a study as would make possible comparisons of the different series so as to bring to light a fundamental shifting of price relationships that continues through time. And for this purpose, his measurement of trend is sufficient. He has fitted to each series for the period 1896 to 1913, a period as, he says, of relatively constant growth for most price series, an exponential curve with a constant rate of growth. The growth comparisons for different price series thus afforded furnish the basis for one of the truly significant discussions of the entire book, a discussion that deserves to be read and pondered by every compiler of a price index number and especially by those who persist in the demand for a normal price period as base, or for normal weights. The upshot of his discussion is that the only normal thing about prices is their persistent tendency to change, some up and some down, and thereby to bring about permanent and fundamental shifts in economic relations.

An entirely new procedure has been developed for the measurement and study of cyclical fluctuations in commodity prices. The usual method of correcting each series for trend and seasonal was discarded, first, because the author says he was not trying to measure true cyclical but rather to describe individual price movements during general swings of prices; and second, because of the technical difficulties of measuring true trend and seasonal and the practical difficulties of insufficient time or materials. The method followed compares each series in its fluctuating movements with the selected

highs and lows of a standard series, the standard used being the wholesale price index. By a careful comparison of each series with this standard, he is able to calculate in months the duration and percentage of rise and fall, the timing of these movements with reference to the standard, an index of cyclical variability, and other useful measures. An enormous amount of descriptive material for the comparison of individual price series is thus obtained, and certain conclusions readily appear. Thus, "The sensitivity to cyclical movement . . . varies greatly as between different commodity groups" (p. 94). "Even where there is a distinct tendency to precede or lag behind general prices there is considerable variation in the degree of lead or lag" (p. 101). Many "price changes did not appear to have any necessary connection with the business cycle" (p. 102).

No review can do justice to all the descriptive material thus assembled or to all the relationships brought to light among the commodities studied. There is nevertheless one feature running through all this analysis that I find distinctly unsatisfactory. It appears in the selection of a standard series for comparison and in the selection of arbitrary reference dates of this series for deciding whether cycles have or have not occurred in the various individual series. Antecedent to this whole procedure lies the conception of a *general business cycle* as a gargantuan force, causal in character, and bringing in its train the cycles in these individual series. Were space to permit, it would be possible to point out many bits of evidence produced in this book, and some of its interpretations as well, that are against this hypothesis and yet the author never quite succeeds in freeing himself from it. See for instance, p. 215, "internal instability is in part, at least, a result of an unstable price level"; p. 361, note, speaking of "changes of the price level, as cause, and disturbance of

internal relations, as effect"; or p. 369, "the causal relationship runs from changes in the price level to variations in the degree of internal disturbance. . . ." To be sure, he states this position conservatively, as a hypothesis, or an assumption; but the conception is deeply rooted in current thinking, though a voice is raised against it now and then. I believe it is time to assert positively that no such causal relationship exists; that the working of causal forces must be sought first in the demand and supply conditions for individual commodities and that it is only the cumulation of effects on individual commodities that produces the thing we call a general business cycle, if and when such cycle occurs.

This objection to the author's procedure in measuring price cycles does not, of course, bring about a general condemnation of his results. A different procedure would no doubt produce significant differences in the cycles of some series but not of all. Much of the timing relationships discovered would still remain; and it must not be forgotten that the author claims only experimental justification for his work, nothing final. The differences between this and a procedure based upon the improved hypothesis will lie in interpretations, in ultimate explanations and the present procedure must be credited with an important start in that direction.

Chapter III contains, in my belief, the most significant results of the whole six hundred pages, for it studies the frequency distributions of price relatives with the main object in mind of discovering evidences of stability or instability in any set of price relationships. The earlier part of the chapter, devoted to the development of indexes of dispersion and displacement, is of least importance. Without going into too technical details, the index of dispersion is a measure of the spread of a given set of price relatives based upon the standard deviation and one conclusion stands out, that it cannot be

used, as has been thought by some, to forecast changes in the price level. The index of displacement, designed to measure shifts in the relationships of sets of price relatives, is $(1-P)$, P being the coefficient of rank correlation.^a Space limitations forbid any detailed consideration of either of these indexes, in view of the much greater importance, indeed fundamental significance, of what follows. This is a study of curve types and characteristics of the many groups of price relatives, using the method of Karl Pearson. The criteria of curve type, β_1 and β_2 , together with other constants in the Pearsonian procedure, are calculated for frequency distributions of relatives, both fixed base and link, weighted and unweighted and for both arithmetic and logarithmic distributions. On the assumption that the normal curve and Type III curve represent stable types, the degree of departure from these types, or of breakdown in the conditions of stability, is ascertained for all these distributions, though the author recognizes the tentative character of conclusions about stable frequency distributions based upon these criteria (See p. 318). The possibilities of the method are well illustrated by the charts and data of Figures 43 and 47. By plotting Beta points, as he calls them, in a plane, the coordinates of each point being respectively the β_1 and β_2 of a given distribution of relatives and by locating the positions that represent stable or near-stable conditions, he is enabled to trace over certain chronological periods the trend toward unstable or stable conditions. Thus the data shown at the bottom of Figure 43, the figures being too large to show on the chart, trace the growth from 1910, and especially from 1914 to 1918 of highly unstable conditions in wholesale prices; while Figure 47 even more eloquently displays the return from the conditions of greatest instability in 1916

^aAs the author has no doubt already discovered, the formula for P , by a typographical slip, has been given incorrectly in the book. See p. 288.

to approximate normality (in the Gaussian sense) by 1923. This type of analysis seems here to show great possibilities and *deserves to be carried much further.*

Space limitations in a review already over long necessitate omitting any discussion of Chapter II on regional differences in price behavior and the final Chapter IV, on the study of frequency distributions of the various measures developed earlier for individual price series. It has been impossible to do more than call attention to some of the outstanding features of the book. It has gone so much farther than any previous study of price behavior that we are confronted by a vast array of new technique or new applications, and it would not be surprising therefore if many of the procedures used failed to command universal approval, and a few exceptions have been registered in this review. It is all the more necessary to recognize therefore the author's accomplishment as an outstanding contribution to our knowledge of price behavior, and, as said earlier, a path-breaking adventure. It is proper also to state that the work could not have been done in its present form without the assistance of an organization such as the National Bureau of Economic Research and that it has been financed by that organization wholly in the interests of impartial science. The sale price of the book undoubtedly covers a very small portion of the total cost.

Part Two

**Proceedings of the Conference
on
Bye's *Critique***

Part Two

The Conference

MORNING SESSION

A Conference called by the Committee on Appraisal of Research convened at ten o'clock, May 14, 1939, in the offices of the Social Science Research Council, New York City. E. G. Nourse, chairman, and the following conferees were present: Raymond T. Bye, Morris A. Copeland, A. F. Hinrichs, Edward S. Mason, Frederick C. Mills, Wesley C. Mitchell, Bruce D. Mudgett, O. C. Stine, Jacob Viner, Theodore O. Yntema. Of the Appraisal Committee, F. W. Coker, A. M. Schlesinger, and W. S. Thompson (besides Chairman Nourse) were present—as observers rather than participants in the discussion. Read Bain acted as rapporteur.

CHAIRMAN NOURSE: In spite of the thoroughness with which the documentation furnished members of the Conference may have served to clear the ground and focus the issues, one day is all too short for dealing with even the most important questions. As I read these documents, I conclude that most of the vital points to be threshed out fall in the field of economic theory rather than statistical method. There is so much agreement in praise of the originality and thoroughness of the author's work in devising and using statistical techniques that I am hopeful that any further discussion of the selection and manipulation of data can be kept within the limits of perhaps an hour and a half so that the rest of our time may be devoted to the more controversial issues of economic theory.

The Conference is entirely in your hands, but I think you are all aware that, in a conference, the topic which gets up first, if it is a fruitful one, is likely to absorb most of the time. Mr. Mudgett has raised a number of questions very clearly in his review, and it occurred to me, therefore, that perhaps what the Conference feels is really important in reference to the statistical techniques might be terminated before the end of the morning session, thus leaving us fairly ample time for the discussion of economic theory.

I repeat, the Conference is in your hands. You are a round-table now. Mr. Mills is here to take part in the discussion whenever he or you may think it desirable. Several members of the Committee sit on the sidelines. Mr. Bain is here as reporter of the Conference.

We expect to publish as a pamphlet the appraisal essay, the selected reviews or excerpts from them, the rejoinder of the author, and the reduced report of this Conference. Each speaker's contributions will be submitted to him for approval before it is published. We do this so that the profession at large may have the benefit of this discussion of standards and methods of workmanship in economic research.

Before the meeting is thrown open, I will give Mr. Mills an opportunity to make any supplementary statement or comment as to his approach to this work. Do you have something of that sort, Mr. Mills?

MR. MILLS: Nothing of any great importance. What I had to say is in the written statement. I think it would be a mistake for me to deal now with the substance of that statement or the substance of the critiques or reviews. The Chairman suggested that it might be in order to say a word or two about the origin of the study. That can be very briefly said.

Some time about the year 1923 or 1924, I had been thinking of some such general attack upon the study of prices as is

exemplified in *The Behavior of Prices*, and some time in 1924 I had a long talk with Mr. Mitchell about plans for that work and the work I had already done. He suggested that I prepare a prospectus which might be considered by Mr. Gay and himself, who were then joint directors of research at the National Bureau.

I welcomed that, of course, because the job I had in mind was one which involved a good deal of routine statistical work which would have been very difficult for an individual to carry through. So I prepared such a statement. This was approved by the National Bureau, and work was begun early in 1925.

The general plans envisaged, roughly, work in two related fields: one, dealing with the system of prices as a whole, and the component elements of that system; the other, a study of value or price determination, centering on individual commodities.

The work was carried on through 1925 and 1926; the first volume was published some time in 1927. It was then my intention to carry along the study much as it had been originally outlined, but there were demands from the outside which did not exactly necessitate a digression, but rather, attention to certain aspects of the work which I had not intended to develop at that time.

If you recall, there was a general study of economic changes during the nineteen-twenties sponsored by the President's Committee on Unemployment, which was later termed the Committee on Recent Economic Changes. The National Bureau undertook to do the substantive work for that committee. I was one of the participants and that drew me for a time from the main line of work I had contemplated. Out of the work on Recent Economic Changes, to which I was one contributor, a later study of my own developed which appeared as *Economic Tendencies in the United States*.

This did not constitute a real digression from the work in which I was mainly interested, but it did involve some change in my plans, so that the volume or volumes which logically should have followed *The Behavior of Prices* were not written. I still hope to work in that general field, although I have thought of the work I have done since as contributing very directly to the project I had first planned.

My written statement naturally deals with issues raised in the reviews and critiques, but I certainly do not want to appear here as one who believes that *The Behavior of Prices* is without defects. It possesses many defects of which I was aware at the time, and more of which I have become aware more recently.

If I understand the Committee's purpose, it is not our main job to review the substance or the method of any one book; rather, we are to take this book as a point of departure for considering the method and scope of economic research. I am happy to be a participant in a discussion of the points raised in the book and in the critiques and reviews of it, but I do not wish to appear primarily as a defender of a particular work completed some time ago.

MR. COPELAND: Did the fact that you worked first on the general aspect of this problem, as a system of prices, rather than on an analysis of particular commodities in relation to factors that might be said to be price-determining, indicate anything of the relative ease of tackling the one problem, rather than the other?

MR. MILLS: Probably the over-all job is easier. However, the over-all job probably can be better done after a detailed job has been done. When we have more detailed measures of individual commodity price behavior, we shall probably have to return to the main job and do it over.

You know the difficulties of measuring elasticity of demand

or flexibility of prices in the technical sense. One can spend years on a single commodity. My concern here was with mass behavior. It would have been impossible to develop a mass of measurements of elasticity of demand or flexibility of price, which would have been useful in a general study. However, a sufficient number of measurements defining the behavior of individual prices were readily available, or could be derived readily, to permit an initial attempt at the mass attack.

MR. BYE: If we exclude for the moment the questions of statistical technique to which you referred, it seems to me that Mr. Mills' study raises three general types of problems. One is the very broad problem of the relation between inductive and theoretical interpretation. A second question is what particular problems of price can best be studied quantitatively. Third, there are certain questions, which perhaps are side-issues raised by Mr. Mills' study, such as the question of general equilibrium and the statistical approach to it, the general level of prices, and the flexibility of prices.

I don't know whether it is possible to shape a general discussion like this around such a program but it occurred to me that it might be profitable.

MR. VINER: I should be very much surprised if the first one proved profitable. I do not think the issue is whether there is any scope for induction. I think we all agree there is. We ought to avoid framing the issue as I think Mr. Mills does, and justifiably so in the light of Mr. Bye's report, as between some particular kind of deductive approach and induction. It seems to me the issue raised by the book is, Was it a fruitful method of induction?

I do not think anybody here thinks there is much profit in trying to measure what percentage of our attention should be given to a theoretical, a priori, conceptual approach, on the one hand, and to the quantitative, on the other. Nor do I

think any reviewer of Mills' book wanted to discourage induction. The nearest thing I saw to it was perhaps Mr. Bye's plea for theoretical investigation. I do not think Mr. Mills' book justified any such position, but as the book was written, I think it justified a claim for another kind of theoretical approach than the one Mills used. So I see no profit in a discussion of inductive versus non-inductive theory, but it might be profitable to discuss kinds of inductive theory.

MR. MITCHELL: You spoke of non-inductive theory. You don't really mean that any theory can dispense entirely with induction?

MR. VINER: I should say every theory starts with certain things that are taken to be known. Mr. Mills quotes Eddington that some sorts of knowledge cannot be a priori.

MR. MILLS: "It is impossible to have a priori knowledge of an objective universe."

MR. VINER: If he just stopped at "knowledge," that would be a better statement—"No a priori knowledge." The a priori work starts with previous knowledge.

MR. MITCHELL: That is right.

MR. VINER: We ordinarily think that in deduction you start with a set of axioms or premises about which you think there can be no reasonable dispute, matters of common observation or historical knowledge (they may be false, of course), and build up by conceptual methods elaborate systems which can be tested in terms of their inner consistency.

MR. MITCHELL: May I ask for another slight revision in your phrasing? It is often a very useful intellectual procedure to start with assumptions about which there may be very grave doubt as to whether they represent anything that can be observed. It is often very useful to explore situations that do not conform to anything we think represents knowledge.

MR. VINER: That cannot be a purely deductive method.

The minute you grant that the premises you start with are doubtful or conjectural or contingent, the question of what you should do with your results depends on checking them in terms of observation. That is a preliminary to systematic induction. That is not the pure deductive method. The pure deductive method would stand by itself.

MR. MITCHELL: The pure deductive method is a certain stage in an investigation and has observation before it or after it.

MR. VINER: The pure deductive method starts with what is alleged to be observation, and the observation stops there. The method you have outlined is preparatory to carrying out an investigation to see whether or not a hypothesis works.

MR. MITCHELL: So pure deduction is just one stage and does not give us results that we think of as scientifically established. It is an indispensable tool.

MR. VINER: You are introducing another term, you see, "scientifically." The man who believes in his premises, and believes in his logical capacity, believes that he gets results from his pure deduction.

MR. MITCHELL: Yes, but the question then arises, Where does he get his belief and his premises? That is where observation has to come in.

MR. VINER: Yes. Some men go along for quite some time without realizing that they have to go through that stage.

MR. COPELAND: It is not merely observation; it is induction. You cannot observe a general situation. An observation is specific. It is followed by some kind of induction.

MR. VINER: The major premise of a syllogism is ordinarily an inductive inference.

MR. BYE: May I explain that, in stating my first problem, I was careful not to phrase it in terms of induction versus deduction. I stated it as a question concerning the relation

between induction and theoretical interpretation; that is, the two things must be combined. Mr. Viner is quite right—it is not a matter of one versus the other, but there is still controversy over the proper relation between them. There is still quite a divergence between those who have followed the traditional type of analysis in economics and those who are working by inductive methods. I think it is extremely desirable to bring those two methods of work closer together, if possible. One reason I stressed those points in the *Critique* was because it seemed to me that Mr. Mills' study was attempting to separate the two entirely.

This may have been due to his feeling that existing theory does not lend itself to statistical treatment. I have heard statisticians say theorists ought to frame concepts and generalizations in terms that will permit the statistician to test them quantitatively. To me, that seems the wrong approach. The theoretical interpreter should try to discover the fundamental things in economic life. Then it is up to the statistician to find ways of measuring those things.

MR. VINER: What if the statistician thinks he is a discoverer of important problems?

MR. BYE: He has a right to think that, and to put out his ideas for general analysis, but he must not discard existing theory merely because he finds it difficult to treat statistically. Let me give one or two illustrations. When the physicist first conceived a molecule or an atom, I do not think he had the faintest idea that anyone would ever be able to measure those things. They were pure constructs for which empirical evidence was entirely lacking; yet the time came when molecules, atoms, and electrons could be measured.

The statistician must not expect the economic theorist to reformulate his concepts in terms of what the statistician is now able to do, but the statistician must devise means of

measuring the things he thinks are really important. Whether the theorist agrees is another question.

MR. COPELAND: I am somewhat disturbed by that statement. It seems to me that one of the issues here is what is a useful and effective division of scientific labor. Is it desirable to have the theorist on one side, and the statistician on the other? If you accept the proposition that it is desirable to combine the empirical and the a priori, the same individual ought to be concerned with both of them. Hypotheses should be framed which are capable of being tested empirically.

Mr. Bye's statement seems to imply that empirical testing is limited to statistical procedures. It seems to me it should be stated in broader terms, namely, that it is desirable to frame theories to show their relevance to empirical knowledge and so they can, if possible, be checked empirically.

I have not made a detailed study of the history of science, but I am not convinced by Bye's statement about physicists. I find it difficult to imagine that they did not have quite definite reasons for wanting to think in terms of atoms, rather than something else. You start to puzzle about various aspects of the physical universe, and you come to some conclusions that may lead in the direction of atoms. From some little inquiry into the theories of ancient Greek physicists, I conclude that they were thinking very definitely in terms of how the atomic theory was related to observable events. You may say that electrons could not be measured in the early stages, but that is not quite the question. The question is: Did the atomic hypothesis fit the known empirical facts better than some other theory did?

MR. MILLS: It is fair to ask Mr. Bye why the concept of phlogiston was dropped.

MR. BYE: Now you have me, because I am not familiar with the concept.

MR. MILLS: The concept that all combustion involves a single material substance, phlogiston; the theory was dropped because there was no way to verify its existence empirically while the part played by oxygen could be demonstrated empirically.

MR. VINER: I would not accept for a moment the idea that we find out what the physicist does, and only then know what we ought to do in economics.

MR. MILLS: I agree with that fully.

MR. VINER: We should learn from them but nevertheless remain autonomous and decide how much of their procedure is useful to us, and how much not, and whether we may not have to use procedures which, because of the nature of our data, might seem unscientific to physicists.

MR. BYE: I agree heartily with much of what Mr. Copeland said. We ought to look forward to economics becoming a thoroughly quantitative, empirical science. I urge my graduate students to take a thorough training both in mathematics and in statistics.

MR. COPELAND: Is mathematics empirical? I think it is the most a priori of all sciences.

MR. BYE: You need mathematical technique to do empirical work.

MR. COPELAND: I think we ought to discuss the degree to which mathematical analysis is a useful device in economics.

MR. BYE: All sciences become increasingly mathematical as they progress. To use the example of physics again, higher physics is almost entirely mathematics. Einstein's concepts can be stated only in mathematical terms.

MR. MITCHELL: You might make the same remark about economic statistics.

MR. BYE: The economic world is one of such infinitely complicated relationships that the mathematical symboliza-

tion seems the ideal language in which to express the interdependence of those many variables.

MR. COPELAND: Perhaps I should state more clearly what I wanted to say. One of the questions raised by this book, and a very important question in modern economics, is this, Does the nature of our data warrant the use of any very elaborate mathematical techniques? How far beyond arithmetic should we go?

MR. VINER: I do not see how that question relates to the behavior of prices. I think that is an independent subject. We have no documentation on it here.

MR. COPELAND: I think it quite definitely relates to the procedures tried by Mr. Mills. He emphasized the point that our present measurements of economic phenomena are very far from being satisfactory. Particularly, the techniques of measuring price observations are in a very primitive state.

MR. HINRICHS: Perhaps this accounts for some of the disagreement between those who work predominantly with measurement and those who use a more generalized approach. In all of the modern sciences with which I am familiar, a large proportion of the original work has involved little more than actual classification. Large amounts of economic data so far do not seem to lend themselves to clear-cut classification. When one tries to say, "This belongs here rather than there," the phenomena appear as joint phenomena. Business enterprises are not clearly in one field or another. They are in several. Prices—wholesale prices—pervade the whole economic system.

We start with relatively well classified materials. Beyond that, anyone who has tried an over-all examination of any set of related economic phenomena is aware that such shreds of evidence as are available may be so faulty that very frequently the errors of guessing what would be likely to happen

are much less than the errors of your observations, if you were depending on strictly inductive and verifiable materials.

That absence of classified materials, that absence of measurement at vital points, makes it imperative, if you are attempting to get a broad view, to make use of logical reasoning as an absolutely indispensable tool. If you then turn to the type of work Mr. Mills was doing, the inadequacies of the materials, both because of their original classification and doubtful validity, mean that such a study is likely to fractionalize into such a mass of details that a person with a generalized interest, or a generalized question, is likely to say at the end, "So what?"

Therefore, the end product of a large amount of inductive work is nothing more than an improvement of the original materials with which we began to work. Such studies are not necessarily a contribution to economic theory, but rather, to an improvement of the materials so that later inductive workers can get better results because the materials have been improved by the earlier studies.

It seems to me that much of the controversy and impatience is due to the failure to appreciate the fractionalizing process that a large-scale inductive study necessarily goes through in trying to shape the original observations into more useful forms.

MR. STINE: Before we pass entirely from the discussion of induction versus deduction, I should like to make one observation. I found this last sentence of Mr. Mills' statement very suggestive: "Reasoning that transcends experience has an important part to play in the advance of economic knowledge, but the ultimate realities of economics are those revealed by observation."

MR. VINER: What does it mean?

MR. STINE: Mr. Bye said that the statistician asks the econo-

mist to formulate his concepts so that the statistician can measure them, but, going to the other extreme, I frequently hear statisticians say, "I know only what the figures tell me." That is, the appearance of the curve, or the apparent relationships between two curves, seems to be the last word in interpretation. I think the statistician should be criticized who does not bring to his particular set of figures a wide enough field of observation to make useful interpretations possible.

From this point of view, I can follow Mr. Copeland, namely, that often the figures of statistics are not a satisfactory basis for any generalization. Working with the figures becomes a game, like a boy playing with a string, seeing how many patterns he can make, but contributing nothing of significance.

MR. VINER: I take it that you like the first part of Mr. Mills' sentence. What about the second part?

MR. STINE: I would rephrase it just a little.

MR. YNTEMA: I should be interested in your phrasing. (Pause.)¹

MR. BYE: I should like to return to the thought that some of the apparently subtle concepts of the theorists can be measured quantitatively if we go at it with sufficient intelligence and patience. I remember that Irving Fisher once made an effort to measure marginal utility. I am not sufficiently familiar with it to say whether he did a good job, but it illustrates what I have in mind.

To me, the most interesting and perhaps the most signifi-

¹When reading the transcript, Mr. Stine prepared the following note: "I would suggest rephrasing the second part of Mills' sentence quoted above to read: '*but such reasoning must be verified by observation.*' 'Pure theorists' seem to me to be often expressing hypotheses which are merely flights of the imagination without verification. Thus, propositions that ought to be stated as hypotheses become assertions of fact. Progress requires that all economic theories or principles be treated as only partially verified, if verified at all, and that we be continually examining observations for refinement and re-statement of principles or economic laws."

cant thing in Mr. Mills' book was the way in which he grappled with the rather subtle concept of a general equilibrium among all his price variables and ingeniously worked out devices for measuring the tendencies towards equilibrium in the system. I did not altogether approve of the concept of equilibrium that he employed, but I was much impressed by the possibilities that his handling of the statistics showed.

Now, we will take again the concept of a normal price which some statisticians do not like. Mr. Mills does not like to use that term. He prefers to use "expected." I believe the concept of "normal price" is very significant and could be measured statistically. I would like to see the statisticians pay more attention to trying to give reality, empirical reality, to some of these things theoretical economists have dealt with for so long.

MR. MASON: If you mean by "normal price," a long-run equilibrium price, or something of that sort, you would not pretend that you can ever recognize by any sort of statistical or empirical investigation when that price is normal, or even what the normal price is, given all the statistical data you would be likely to accumulate?

MR. BYE: You are asking me, a theorist, who has very little knowledge of statistical technique, to answer a statistical question. The economic theorist can define what he means by normal price. The statistician must answer the question, "Can normal price be measured statistically?" I think the concept has reality; if it has, I should think it could be found.

MR. VINER: No; the theoretical concept is not supposed to be in a statistical world, an actual world. It is a timeless concept. It is abstracted from phenomena in the world. If you could find its exact counterpart in fact, it would discredit the concept.

MR. MILLS: Should a person working with empirical data

take a concept such as "normal value," and assign that concept to a quite different measurement? Marshall used it in a very special sense.

MR. VINER: Marshall did not invent the word "normal." You and Mr. Mitchell do not like the term, but you constantly use it, nevertheless. You sometimes put quotes around it. That means you once condemned the use of the term by somebody else, but have found no better term.

MR. MILLS: My position is that the term "normal," "normal value," has a definite meaning. Mr. Bye objected to my using the word, "expected," rather than "normal," for something that was quite different from Marshall's "normal value." It would have been confusing to use "normal" for a trend value.

MR. VINER: If you use "normal price," with no explanation, I think the economist would think you had reference to Marshall's concept; therefore, in the interest of intelligibility, you should not use the term.

MR. MILLS: I do not object to using the term "normal value" as Marshall used it.

MR. VINER: There are lots of other uses. It is always a dangerous term, I think, because of its variety of accepted meanings—but so will any substitute be.

MR. YNTEMA: What is normal price for a durable good—for steel, for instance?

MR. VINER: In an actual world? If you freeze the world, then I can define the term with reference to the price of steel, but I cannot get it statistically.

MR. YNTEMA: But the world is not frozen.

MR. MASON: The fact that a term has no reality does not mean that it has no utility.

MR. VINER: I think it has lots of reality.

MR. MASON: It certainly has utility. It has the same sort

of utility that all concepts have. They help us to ask the right questions of the data we want to investigate. Obviously, by mere economic reasoning you cannot come to any conclusions that will approximate the real world. You cannot do that because economic theory sets up a simplified model of markets, and reasons on the basis of a small number of elements. What it does by means of this "mental experiment" is to formulate certain questions which it then becomes important to ask of the data. In this connection, I agree with Mr. Copeland that it is very important for economic theorists to formulate conceptions that are capable of empirical investigation.

What I am leading to is this, What questions did Mr. Mills ask of his reality? Economic theory does provide certain questions to ask. For example, a study of the structure of industrial markets leads one to suppose that probably the number of sellers has a good deal to do with how prices are determined in that particular market. If Mr. Mills was concerned merely with a description of how prices behaved over a definite period of time—1890 to 1925—you presumably do not have to ask any questions of that sort, but, if you are interested in why prices behave the way they do, or the consequences of price behavior for the functioning of the economy, then I think you have to ask some questions of the kind which economic theory provides.

Let me put the question to Mr. Mills this way: Were you merely interested in describing how prices behaved, or were you interested also in the further question of why they behave the way they do? And possibly, What are the consequences of price behavior for other variables in the economic system?

MR. MILLS: Since a good part of my written statement deals with phases of that question, I hesitate to try to answer it in a sentence, but I think the answer to the first part of your question is "No." I did not think I was dealing merely in a de-

scriptive way with what had happened. I had a very definite frame of reference. I made one point in the written rejoinder that I would re-emphasize here: I do not think the concepts with which one works, the hypotheses, perhaps, that suggest an attack, need be in the form of questions to which a Yes or No answer can be given. They may be more general concepts to which no Yes or No or other definitive answer can be given.

MR. MUDGETT: A probability answer, for instance?

MR. MILLS: Yes. One may think of trying to explore the attributes of a system of prices—

MR. MASON: What do you mean by that? That you have a lot of prices, any change in one of which will affect others?

MR. VINER: Does it mean that much? It needn't, in a large part of the book. It could be a system of prices in the same way that you have a system in other fields to which economic theory never has been applied. I should like a discussion of the concept of "system of prices." That is one of my stumbling blocks.

Though I now must make important qualifications in the light of Mills' later work and his statement that what he did later was already in his mind when he wrote *The Behavior of Prices*, in the light of the book itself, I should say he was taking over a concept of system from other fields of economics and applying to it purely noneconomic techniques of analysis, applying statistical techniques, if you like, that could have been applied by a statistician absolutely devoid of interest in economics for its own sake who had been given a set of data and told, "Here are numbers. Analyze their statistical behavior." Mills analyzed the data—and he had a theory. I find lots of theorizing in your book, Mr. Mills, but it is probability theory; it is not economic theory.

Here and there is a trace of economic theory, but you show,

I think, by statements and methods of handling data, that really you are not seriously interested in economic theory. I give one concrete illustration—your treatment of the regional behavior of prices. An economist attacking the problem with economic interest would say that there is a certain set of prices that has a lot to do with the regional behavior of other prices—namely, the prices of transportation—and yet you discuss regional prices without mentioning freight charges. An economist with economic interest could not have done that. It is psychologically impossible. The prices of transportation are tied up with regional differences in prices; you cannot separate them and yet do economic analysis of regional price differences.

MEMBER:² I think of prices as economic barometers. They define the attributes of the system of economic activity. They are measurable. We can get at them. We are not interested in prices as prices, but as measurable aspects of the working of the whole system; the relations and changes among prices are factors of great importance. They reveal information about the system of related activities through which we produce and distribute goods.

In reference to Mr. Viner's last point about regional differences, the study of transportation costs would have constituted a very nice addition to what Mills did. I mean seeking a further explanation of those differences, one would necessarily have dealt with transportation costs. But the geographical structure of prices is revealed in part by the actually existing prices of potatoes or cotton or wheat or whatnot in different parts of the country. Costs of transportation are additional measurements that fit into the picture and help complete it.

² The member to whom this statement was attributed denied it, but since it seems pertinent to the discussion it is included in spite of the stenographic error. One other similar case occurs on page 274.—R. B.

MR. VINER: They help to make your results economic results. I may be speaking more dogmatically than I am thinking. Let me put it as a question: What is there of economic significance in your results as to regional prices? What bearing has it on our understanding of the economic process? You used the word "naturally." I would say that "naturally" an economist would start with the freight relationship between prices of the same commodities in different regions. You did not start there, and I said that it is natural for a statistician who is not seriously interested in the economic phases to start from a set of numbers that change through time, and apply statistical tests and statistical criteria to the changing behavior of these prices through time.

I still say that your book, as I understand it, was a manual of techniques very valuable for the economist, but it was not an application of these techniques to economic analysis. Of course, that is perfectly legitimate. You had a perfect right to do it that way, but why did you call it what you did? Why did you give it the title you did? It is not the behavior of prices. It is certain aspects of the behavior of prices measured without reference to economic phenomena.

MR. MILLS: I think we are still thinking on different planes.

MR. VINER: I said that there is lots of theory in your book.

MR. MILLS: It is not the kind of theory you are thinking about.

MR. VINER: In my own review, and still more now, I carefully avoid suggesting that I have the right to impose upon you a certain theoretical approach to the analysis of economic behavior. My only argument is that you must have some type of machinery for creating hypotheses, *economic* hypotheses, if you are to examine economic phenomena fruitfully, and that you did not use any such system in your book. It is not a question of using my system, or Mr. Bye's system,

or Marshall's. You picked me up once because I say that prices are tied to each other by a system of demand and supply functions, and you assume that I meant you should analyze the demand and supply functions. I do not mean that. I, myself, do not believe that you can get very far in analyzing the economic system as a whole merely by studying demand and supply inductively. But the recognition of that connecting tissue would lead you to a sort of economic grouping of your prices, so that you would not be taking 250 series selected partly in terms of the way in which they behaved statistically, noneconomically, and then seeing what patterns you could throw them into in terms of probability analysis alone.

If you really had been driven by economic questions in your mind at the time—some economic problems for which you burned to get a solution—you would have been driven to introduce some economic tie-ups between different prices, and not purely frequency distributions, where the individual prices were not given any identity. It did not matter in your patterns whether the prices were those of peanuts or steel.

I repeat once more—I am not going to say it again, but I do want to make this clear—that I accept your statement that this was a preliminary study. I read the book as honestly as I knew how, but I now see that I missed certain places where you did forecast the nature of your second volume, but even now, I should say that some of these statements were *very* ambiguous.

Let me refer you to another statement which indicates where I think we still would differ. You speak somewhere of the thousand forces that determine individual prices, as distinguished, let us say, from your half-dozen techniques for analyzing the behavior of prices. When somebody refers to "a thousand factors" influencing something, that means he does not think he can analyze these factors individually, but

can only apply probability analysis to these thousand factors; that he did not have in mind any outstanding economic forces which could stand individual investigation of an economic sort.

MR. MILLS: I think that is misinterpreting what I meant.

MR. VINER: That is the impression I got from your book. It seemed to me that you not only did not apply economic analysis to the data, but that you had certain objections to doing so.

MR. MILLS: That is distinctly wrong, if you are thinking of individual price determination. When I say "thousand factors," that is strictly true, of course. The demand for one commodity is a function of the prices of a great many commodities, but obviously three or four factors may be of dominant importance and therefore subject to measurement. The plan for a study of value determination was in my mind from the beginning.

MR. BYE: Mr. Viner seems to be agreeing with what I said in my *Critique*, namely, that all scientific analysis should be striving for relations of cause and effect. Mr. Mills does not like my concept of cause and effect. However, whether you describe cause and effect relations in terms of statistical probability or in terms of mechanical sequences is really unimportant. The important thing is to link up all phenomena we investigate with antecedent circumstances. That is what Mr. Viner means, I think, when he says that in studying regional price differences, you want to link it up as far as you can with costs of transportation. To me, costs of transportation would be an antecedent circumstance, a cause, and price differences would be a result, or effect. That is what I call a cause and effect relationship; and I believe that in the study of prices, as of all other economic phenomena and all scientific investigation that is the kind of knowledge we want. I think *The Be-*

havior of Prices did not push the analysis back far enough, though as a preliminary to such analysis, it may be all right.

MR. COPELAND: Mr. Chairman, I think Mr. Viner and Mr. Bye are in the curious position of objecting to abstraction. To me, to take these price series and see what generalizations you can make about them, abstracted from changes in physical volumes, is a perfectly legitimate procedure.

MR. VINER: Who disputes that?

MR. COPELAND: And it is a perfectly good procedure for an economist. The fact that you may use a curve that came from some other discipline does not mean that you are not going at it as an economist. Mills did not find as many nice generalizations as some of us might have hoped, but nonetheless, it was a perfectly legitimate procedure. It is desirable in scientific exploration to try any reasonable procedure and see what can be done with it.

MR. VINER: Most of us admit that any procedure is legitimate in that sense; certainly, I do not dispute it. If I object to anything, it is that the study purports to be a study of the system of prices, a study of the behavior of prices. There is no qualification in the title. The word "system" is nowhere defined.

MR. COPELAND: That is customary among the older theorists, too.

MR. VINER: Then let us attack them, too.

MR. MILLS: The Marshallian system was nowhere defined.

MR. VINER: In one or two places, I think you speak of the price system.

MR. MILLS: I indicated clearly what part of the system I was dealing with.

MR. VINER: You might have called it, *A Fraction of the System of Prices*.

MR. BYE: He pointed out that he was dealing only with

wholesale commodity prices, and that he was henceforth going to speak of that group of prices. He is perfectly justified in using a short-cut term to avoid cumbersome language.

MR. VINER: It is a question of terminology. I interpreted the book to be an exposition of the system of prices, of the behavior of prices, meaning the economic behavior of prices; and I felt that it was a selection, or a fragment, an arbitrary fragment, of the price structure, and that his picture might look drastically different if he included other parts of the price structure.

CHAIRMAN NOURSE: I sympathize very much with what Mr. Bye said, that Mills was justified in taking a short cut in naming the book. What has bothered me is, What would the full name have been? I went through the same difficulty with *America's Capacity to Produce*. In the last chapter, by way of reminding the reader of the scope and limitations of the study as set forth in the introduction and reiterated throughout the text, I put down a 29-word caption which would constitute the full title of the book, although it had to be abbreviated to go on the backbone of even a 600-page book.

While I think the title of this book is well phrased and is justified, I do not think Mr. Mills has made as clear—as I attempted to—just what the full title should have been. After playing around with various possible phrasings, I finally decided the full title might have been something like this: *Certain Aspects of the Behavior of Certain Wholesale Commodity Price Quotations—A Descriptive Preface to an Analysis of the Systemic Relationships of Prices*.

MR. VINER: "As Reported by the Bureau of Labor Statistics."

MR. MITCHELL: And a few other sources.

MR. MILLS: For certain selected markets.

CHAIRMAN NOURSE: Well, of certain wholesale commodity

quotations as reported by certain agencies and in certain regions; but then the subtitle, which is a challenge as to whether he would have accepted any such limitation—"A Descriptive Preface to an Analysis of the Systemic Relationships of Prices."

MR. MILLS: Well, "descriptive" is a sort of fighting word.

CHAIRMAN NOURSE: It was not intended as a fighting word, but as a provocative word. I wanted to get your reaction. You have referred to description and analysis as features of the book.

MR. VINER: I should object to "descriptive" as misleading. I do not say that *you* should not have used it, but Mills should not have used it. He was doing analysis.

MR. COPELAND: The word, "description," I think, needs to be opposed to "appraisal."

CHAIRMAN NOURSE: Mr. Viner asked what it was that you were burning about when you started, Mr. Mills. You did not tell us clearly in your initial statement here today. One possibility is that you regarded as inadequate the portrayal of prices from which the economist has started his analytical efforts at explanation, and that you were trying, on a purely descriptive basis, to show prices in action. Would that have been it?

MR. MILLS: I am not very apt at phrasing anything off-hand, but it was something like this: The producing and distributing of goods and the allocating of services are conditioned by the existing system of prices, by prices and their interrelations. Those prices, apart from merely reflecting economic activities and economic choices, have come to play, I think, a certain directive role themselves. That is, there is interaction between the physical processes and the price measurements. The price measurements, in some sense, react upon physical production, and the attributes of that organized set of prices needed more accurate defining.

I set myself the task of attacking, first, the technical problems involved in improving our measurements of prices; and, secondly, in a preliminary way, I sought to bring those measurements together to learn what I could about the behavior of this system of prices in the past. With more time, I could improve this statement and probably make it less open to criticism, but perhaps it suggests what I was aiming at.

MR. COPELAND: To me, one of the most significant contributions of this book is the title. It gets away from the static approach to the study of prices which attempts to understand a price in relation to certain other substantially simultaneous facts without any direct reference to the time factor. You may talk about long-run and short-run adjustments, but I do not think that brings in the time factor as Mr. Mills has done. His title suggests that we must study the relationship between prices as a system, and, by implication, that we must study the relationship between price changes and other economic changes. I think it is important to have phrased the problem in that way.

MR. HINRICHS: Perhaps I should not shift the discussion at this point, but I want to raise some questions in connection with the original mandate—an attempt to find out what is good scientific work in economics. Whether one attempts to determine that from the defects of this, or any, particular piece of work, or from its consequences, as Mr. Copeland suggests, it is safe to say that in the present state of economic knowledge, and especially from the point of view of its measurement, no piece of work produced either by an economist or a statistician will be very adequate.

It seems to me the test of good scientific work is whether it stimulates the author or others to do further work along the same line. Mr. Mills' book meets this test in several respects. Mr. Copeland has indicated the importance of the title.

The development of the terms, while perhaps not setting forth a system of concepts entirely new to the economist, did make the interrelationships of price, whether directly or through a complicated tissue, much more evident and fruitful to anyone who attempts to assemble quantitative materials and analyze their relationships.

The Behavior of Prices revealed the inadequacies in the basic materials on wholesale prices as such. It has raised the issue of revising many of our concepts of wholesale prices. It has tended to make those working with mass observations shift their attention from a rather single-minded concern with central tendency to the importance of dispersions and the behavior of items within a frequency distribution of related activities. We really want to find out what are the points at which a work stimulates, blocks, or misleads subsequent research.

MR. MITCHELL: In line with Mr. Hinrichs' remarks, I may say that I found the results of Mills' investigation of great value in trying to understand business cycles. Ordinarily, people who have considered prices with reference to business cycles have employed index numbers of retail and wholesale prices, wages, and, to some extent of interest rates.

Index numbers are unquestionably very useful, and, for certain purposes, are probably the best data available, but in working on business cycles, one finds that index numbers conceal a great deal of the information one needs to ascertain. Notably, they conceal a large part of the amplitudes that are characteristic of price movements when you take them commodity by commodity. In business cycles, the differences between the fluctuation amplitudes of different commodities are of high importance.

By taking some two hundred price series and analyzing their percentages of rise and fall, leads and lags, in connec-

tion with business cycles, and by showing the dispersions of those movements and enabling us to get averages that represent commodities classified in different ways, Mills has prepared a body of materials that is exceedingly enlightening when applied to the analysis of business cycles.

CHAIRMAN NOURSE: It was precisely in that sense that I used the word "descriptive." It seemed to me that Mills thought price indexes did not adequately portray prices as they actually function in our economic system, and that he did a great deal toward portraying them in a much more adequate and useful way. In that sense, do you accept "descriptive" as describing your intention?

MR. MILLS: Yes, but I should like to think that it went a little beyond that. It was an incidental concern, and a not unimportant one, but I do not think that was my chief objective.

MR. STINE: To me, the most significant contribution of the book is its presentation of dispersion and diversity in price movements, not its explanation of anything in particular.

MR. MUDGETT: I should like to return to the question of normality for a minute. I think the most important result of Mills' book is what he did in Chapter III. I am sure he has not said the last word on the subject, but he did show that statistical methods can do at least one thing with respect to this question of normality.

If we have framed a theoretical concept as to what is normal, I believe we can get a statistical measure that is related to it. It would not be an exact measure, but an approximation which is the only thing statistics can do. I don't know whether it is necessary to define normality in great detail, but at least it is related to the stability of prices. Statistical procedures, and the particular ones Mills used in this case, have given us one of the best pictures I have seen of tendencies toward stability and away from it.

MR. VINER: Stability of what?

MR. MUDGETT: Stability in the system as a whole. I would assume that Mills was talking about a Walrasian conception of an equilibrium system.

MR. VINER: Stability in that sense would imply no changes of any sort.

MR. MUDGETT: Quite true; and the moment the system became unstable, it would tend to return to a stable condition. You would never get it in reality.

MR. VINER: You get a tendency to return to the original system, if the disturbance is fugitive and casual, if there has been no fundamental change in the situation; but Walras would never have applied it to the year 1910 in relation, say, to the year 1900.

MR. MUDGETT: Also, when a new factor is introduced into the system, such as an increase of capital, a new system arises.

MR. VINER: You have two different universes in a system the minute you introduce another factor, whereas Mills treats it all as one system, so he must have a different concept of stability. Why bring in Walras? I don't see any relationship.

MR. MUDGETT: If you want to leave out any definition of equilibrium or stability, all right.

MR. BYE: I think you have to bring in the theory of general equilibrium, not necessarily as Walras understands it, but as the ordinary economic theorist does.

MR. VINER: You had better not if you are comparing 1890 with 1910 or 1920. The economist's price data themselves will not reveal what ought to be the theoretical relation of the actual price structure of one of those years to that of one of the other years.

MR. BYE: You are introducing limitations in the statistical method that are not there. It can reveal the changing equilibrium. That is just what the statistician ought to do for us.

I think Mills has made a very interesting beginning in that direction.

MR. COPELAND: It seems to me that you merely *say* the statistical technique can reveal the changing equilibrium. Some hypotheses are merely *obiter dicta*. I think this concept of equilibrium is in that category. You can find a characteristic distribution of the prices, or price relatives, or some other aspect of the situation, which has a certain uniformity or "stability," but you do not have to say that the designated factor tends to return to it except in those times characterized as "periods of disturbance," or to say that this involves equilibrium in the Walrasian sense. Your finding is merely a description of the price behavior. That is your generalization. Anything beyond that, it seems to me, is an *obiter dictum*.

One of the objections to this word "normal" is that it means different things. One should avoid it, or else specify his meaning. One concept of "normal" is very close to the concept of "average," merely a descriptive aspect of a situation; but "normal" also has the meaning of "normative," of a situation as it ought to be.

MR. MITCHELL: It also can be used in a third sense, as in Marshall's analysis, namely, as a factor in an imaginary situation about which you can reason on the basis of certain assumed premises. There are really those three meanings, and one that certainly ought to be avoided, I think, is the one that connects "normal" with "average." I grant that a great many people do use it in this sense, if you are talking about current practice.

MR. COPELAND: That third, or Marshallian sense, seems to be partly a descriptive term. As such, it is unobjectionable when used carefully, but unfortunately, it often both is descriptive and carries an implication of the "natural" or the "normal" in the normative sense.

MR. MUDGETT: Have you any objection to "stability"? Let us leave out "normal" completely.

MR. COPELAND: "Stability," if it simply means stability in the statistical sense—"this is a stable type of distribution"—is quite safe, but I think "stability" often has suggestions of economic stabilization about it.

MR. MUDGETT: Do you think there is any relationship between stability in the statistical sense, and the stability you would expect in a purely conceptual equilibrium system?

MR. COPELAND: I see no reason to assume so.

MR. MUDGETT: I should expect there would be a very close relationship.

MR. BYE: I think there is a danger in identifying stability with the absence of change. I felt that was what Mills did in *The Behavior of Prices*, and I do not believe that is what we ought to mean by "stability" in the economic sense. To me, "stability" means adjustment. I can only define it in terms of normal relationships.³

MR. COPELAND: Which of the meanings of "normal" are you using now?

MR. BYE: The Marshallian.

MR. COPELAND: I think the Marshallian "normal" is both description and appraisal. It has both concepts in it.

MR. BYE: What do you mean by "appraisal"? There is no implication in Marshall that normal prices are desirable prices, if that is what you mean.

MR. VINER: Does Marshall think "normal monopoly price" is desirable?

MR. COPELAND: He might think it is undesirable.

³"It is lack of balanced adjustment to change rather than change itself which constitutes an unstable situation. This would be revealed by significant deviations of market from normal prices. If normal prices are changing over a period of time, but market prices are nevertheless substantially in agreement with these changing normals, I would not call the situation unstable."—Raymond T. Bye.

MR. VINER: Because of "monopoly"; not because of "normal."

MR. MITCHELL: Not necessarily because it is normal, nor because it is monopoly price—not necessarily.

MR. VINER: You mean for Marshall? I agree with Mr. Copeland that the classical economist had some kind of subconscious association between the desirable and the normal.

MR. BYE: I don't think that is true of Marshall.

MR. MITCHELL: I think there is a shade of it in Marshall.

MR. MUDGETT: Here is the particular point I have in mind with respect to instability. The best evidence in Mills' entire book, in my judgment, on a system that is thoroughly unstable is the series of price distributions that he showed for the years 1915, 1916, 1917, 1918.

MR. VINER: Unstable with respect to what? They may have been very stable with respect to cost relationships or actual utilization of resources or the value of the dollar.

MR. MUDGETT: Perfectly normal for wartime situations; but you can agree with this, that there is no single conception of "normal" in Marshall. I can quote you four definitions.

MR. VINER: It would be desirable if the term "normal" were killed, but it springs up again. You can't kill it, so you have to explain in just what sense you use it each time you do use it.

MR. MUDGETT: We were going along on an even keel—we had plenty of economic trouble, but an even keel—up to 1914; but in came a complete shifting of demands for commodities.

MR. MITCHELL: All sorts of alteration in prices, also.

MR. MUDGETT: Many of them coming immediately in response to these demands. Those terrific changes are reflected in the whole situation. They are reflected in this system of prices immediately—they are part of what I apparently unfortunately called a Walrasian equilibrium system, but they

show up in the distribution of prices. If there is anything statistical method can do, it can show you a distribution of anything on a quantitative basis that is subject to a great multitude of relatively small and unimportant effects—forces, if you will.

MR. YNTEMA: When we have such an unstable distribution of price changes, it does not show whether we started from an unstable situation, or ended in one.

MR. VINER: We may have attained stability.

MR. MUDGETT: The only thing the statistical method can do is to distinguish between more or less stabilization.

MR. VINER: Which was more stable—1914 or 1920—and how do you know?

MR. MUDGETT: Well, 1914 was the more stable, but the minute the war was over, we had a shift to something else.

MR. VINER: Maybe 1914 was a disturbance year. How do you know we weren't shifting back after 1914 to something we had before 1914?

MR. YNTEMA: It seems to me that all frequency distributions of prices reflect is that we do not have the kind of condition which gives rise to a normal curve—nothing about the starting point or the ending point.

MR. MUDGETT: Perfectly true.⁴

⁴ Mr. Mudgett prepared the following note with reference to the discussion appearing on the preceding pages of the Transcript.—R. B.

The concept of equilibrium in the economic system as a whole (whether it be a Walrasian equilibrium or some other sort) furnishes a valid point of departure for the measurement of tendencies to maintain or to depart from equilibrium—tendencies toward stability or its opposite. There is in the real world, of course, no equilibrium to match the theoretical concept, but even the latter is not the situation of a "frozen world." It involves a ceaseless round of production and consumption. Only certain elements in the system are frozen—tastes and consequent demands; resources, human and other; techniques.

Now the ceaseless changes of the real world by hypothesis may be classified as (1) fugitive and casual (the statistician would call them random); and (2) specific or casual. The latter is not an entirely satisfactory term, but it refers in this connection to the "frozen" elements, or the "given" data in the

MR. MITCHELL: Take a concrete example. According to the War Industry Board's *History of Prices During the War*—a very large collection of data—the prices most out of line with previous experience were the prices of dye stuffs. These very high quotations for dye stuffs, we know from supplementary inquiry, did not represent the full increase in prices because anybody who was engaged in buying dye stuffs for use on textiles knew he was paying ten or fifteen times the prewar price for dyes that had been adulterated. The amount of dye stuff in an ounce of material a user bought would probably be a fifth or a tenth of what it would have been before the war, so the price per unit of coloring matter was extraordinarily high.

Anybody who knows what the situation was accounts for the high prices readily in terms of the cutting off of the source of supply, and by the fact that dyes, bought even at extraor-

conceptual system of static equilibrium. This classification of changes into random and specific is the point at which statistical technique may come to the aid of the pure theorist. If changes in the real world are essentially (and approximately) random in character, then a given system (equilibrium) is being approximately maintained; if, on the other hand, the changes are more than random, then there is evidence of evolutionary movement—a change in the givens."

Now, recalling that a frequency distribution of "prices," as used in Mills' Chapter III, is a frequency distribution of price relatives, or of price change from one period to another, these distributions provide the raw material of one kind of change in the real world. If the change can be explained in terms of random variation in homogeneous material, there is evidence of stability; if more than this, there is evidence of instability. With respect to almost every frequency distribution that is analyzed, it is a case of more or less; and there is no reason to believe that gradual evolution could not take place even though successive frequency distributions (of, say, each year on the previous year base) showed variation of random character only. Here, at least, is a means for statistical technique, working on a framework of basic theory, to assist in explaining what is going on in the real world—a means of differentiating between changes that occur without serious derangement of the system and those which bring more serious and dangerous consequences in their train. Be it noted, finally, that the technical procedure has fitted itself into the problem set by the theory of economic equilibrium. The economic "concept" of the world was not framed with respect to what might be measurable statistically.

dinarily high prices, are a small part of the cost of finished products, so that prices of dyes could rise enormously without raising much the selling prices or cutting down the demand for the goods into which dyes entered.

We knew the situation was unstable, for we were tolerably sure that existing conditions would not last long. Either the war would end and abundant dyes could be had from the old source, or the production of dyes would begin in other places than Germany, and the extremely high prices would stimulate an increase of supply.

This conception of instability, of course, is one that ties up with our economic theorizing—that is, we account for this situation in terms of the altered conditions of supply and demand. We say “instability” because we look forward to a new situation, particularly with reference to the supply. Of course, our statistical presentation of the extraordinary scatter of prices in 1918 and early in 1919, with these exceptionally high figures for dye stuffs, does not tell us how the situation came about.

MR. VINER: If your statistical investigation includes a series indicating the percentage of domestic consumption of dye stuffs which came from domestic and foreign imports, and you started in 1914, and imports become impossible, there you get a statistical explanation.

MR. MITCHELL: You get a statistical explanation only in the sense that, by means of your economic reasoning, you can tie up these things. It is like your case of the transportation. If you got full figures on the cost of transporting potatoes from Maine to New York, and other sources of supply to New York, that would not, by itself, explain the differences between prices in Maine and in the New York market.

MR. VINER: It would go a long way toward it.

MR. MILLS: It would carry your argument one step further

back, just as knowledge of dye imports and exports would.

MR. MITCHELL: You would still be resorting to reasoning on the familiar lines. You would have a clearer view of the situation if your reasoning were accompanied by data that reasoning showed to be pertinent.

I do not see that we have any serious differences of opinion. We merely approach these questions with somewhat different habits of mind, different schemes of emphasis. Perhaps it is wrong to suggest that we have no serious grounds for controversy, but that is the way it looks to me.

MR. STINE: I think there are significant differences in evaluations. It is a question of emphasis, of how you should deal with particular problems.

MR. VINER: If it is merely a difference of emphasis, the three or four thousand economists in the country can take care of any problem, because if one underemphasizes it, another overemphasizes it, and you can take an average.

MR. COPELAND: Take an average between the a priori and the empirical?

MR. VINER: The view that certain approaches are not valid, that one approach is all that is necessary, becomes negativistic.

MR. MITCHELL: My guess is that there is not a man here who would take the view that any one approach is adequate.

MR. VINER: Let each man speak for himself.

MR. MILLS: You don't find anything in *The Behavior of Prices* that suggests such a view, do you?

MR. VINER: Yes, I do. I don't find anything like that in F. C. Mills, because I know him. I want to make that clear. Some of your work since then is in the manner I should have liked to go at the problem, myself, if I were competent to do it. Let us be clear on that.

MR. MILLS: I should certainly like chapter and verse on that. If there is anything in the book that implies—

MR. VINER: I have already made the qualification that you pointed to passages to which I had given insufficient attention, perhaps overlooked, but your book indicates—to the careless reader, let us say—that you think you have gone much further in the description of the behavior of prices than I think you actually went. Your reference to Walras, I think, was unfortunate, because the Walrasian system was clearly a system in which causal relationships were carefully brought into the discussion, relations of human desires and volitions, the demand and supply functions.

This reference to the “thousand other factors” which you would have to stress, if you departed from your approach, by implication means that another approach would not be a fruitful way. As you and I know, a thousand factors cannot be studied.

There are other points where you were not very explicit about what you wanted to do. You do say, of course, that in the second volume, you are going to make other groupings.

MR. MILLS: Groupings. Not *other* groupings. None were made in *The Behavior of Prices*.

MR. VINER: I would dispute that, that there are no groupings in *The Behavior of Prices*.

MR. MILLS: I used the B.L.S. groupings for cataloguing purposes.

MR. VINER: No, you apologized for them. I would have wanted you to use them more, because the B.L.S. groupings are economic groupings. What I object to is that you did not have economic groupings.

MR. MILLS: I did not apologize. I merely said that I neither accepted nor rejected them.

MR. VINER: They were not an integral part of your analysis.

MR. MILLS: No groupings were an integral part of my analysis.

MR. VINER: You say that I do you an injustice when I say that you grouped, that you did have some groupings.

MR. MILLS: Based on quantitative difference.

MR. VINER: That you had groupings by quantitative criteria. I said the only subgroupings you had were by quantitative criteria, with a few exceptions. Look at page 99, I say these are groupings.

MR. MUDGETT: You mean groupings with respect to the variations?

MR. VINER: Yes, with respect to their degree of variation.

MR. MILLS: Let me see this. Where do you find it? I don't see it.

MR. VINER: This is perhaps where I will get some statistical education. I may not know what grouping means. Aren't these groupings? You have broken up your things into percentages which behaved in one way, and percentages which behaved in another way.

MR. MILLS: This shows the number of commodities that participated in the cycles we attempted to study.

MR. VINER: That is a grouping.

MR. MILLS: On the basis of quantitative differences—no. Some commodities simply were so nonconforming, failed so completely to conform to the cycle, that we could not include them. How many did we include?

MR. VINER: You have five groups here according to their cyclical behavior. You see "constant"—isn't that a quantitative concept?—"rising," and "sagging"?

MR. MILLS: I deny that I have made a classification based on quantitative differences of behavior.

MR. VINER: Well, "constant," "rising," "sagging"—these are quantitative concepts.

MR. MILLS: Some went up slightly, let us say, through a cycle, but there are no quantitative differences in behavior.

MR. VINER: Suppose one price does not change at all?

MR. MILLS: You mean no qualitative differences? Perhaps we are using the word "quantitative" differently. I use the word to mean "measured quantitatively."

MR. VINER: As I used the term, and as I thought you used it, a qualitative difference would be between pork and beef, and a quantitative one would be prices with high and prices with low variability from month to month.

MR. MILLS: If I had thus divided my prices, and had studied each, I would have said that was a classification based on quantitative difference of behavior.

MR. VINER: When you classify them according to "constant," "rising," and "sagging," you don't say that is quantitative? It would be no defect if he had gone further with that grouping. I don't know why Mr. Mills seems to think that was a negative criticism.

MR. YNTEMA: No defect! It is one of the most promising things in the book.

MR. VINER: I am all for it. Classify them by your test, group them into highest and lowest, and then see if they have any common economic characteristics. That is what you did in your study of price rigidity.

MR. MILLS: Yes, but I have not started with a quantitative difference in behavior as a basis for classification. "Quantitative," I suppose, applies to this difference here in one sense, but it was not a series of measures that provided the basis for the classification.

MR. VINER: I do not use it in that sense. I know there is ambiguity; it was not a criticism; it was just an attempt to describe what you were doing.

MR. MITCHELL: Do I understand correctly what Mills is saying when I put it this way, that his interest was in that part of the system of prices which he was considering—namely,

wholesale commodity prices, as represented by the materials he was able to gather?

MR. MILLS: Yes, that is right.

MR. MITCHELL: Rather than in breaking down that whole into various categories? Am I again right in taking this sample as a whole? Your interest does extend to representing diversities of behavior within it?

MR. MILLS: Yes.

MR. MITCHELL: In that sense, you are grouping, aren't you? You are bringing out these differences in behavior.

MR. VINER: When he denies that, he says, "Unless frequency distributions are meant," but I said he was grouping, even aside from frequency distribution.

MR. MILLS: There is something rather irrational about taking commodities, let us say, based upon degree of flexibility, and finding all those with high flexibility and all those with low flexibility, and generalizing upon those. I think there is no rational basis for such a division based merely on the measurement.

MR. VINER: I would say, "group them that way." Then you get clues for further search. You look for common characteristics in these groups.

MR. MASON: That is quite a fruitful line of approach.

MR. MILLS: Only as a preliminary basis for finding a rational basis of classification, but in itself, it has no rational justification.

MR. VINER: That is what bothered me about your book as a whole.

MR. YNTEMA: Factor analysis does exactly that. It attempts to group these variations so as to find similarities.

MR. MILLS: I would employ that, if at all, only as a preliminary basis for finding significant differences. (See pp. 245-246 for a further comment on this point.)

MR. VINER: Is my review here? I said your book was going to be extremely valuable for the theorist. What you did was to prepare a statistical manual for use by theorists who want to analyze quantitative price data.

MR. MILLS: This dichotomy between statisticians and economists! You say that economists—

MR. VINER: That they are not competent statisticians. Theoretically, they might be both. The dichotomy is a nonuniversal distribution of the two skills, so that some have more of one, and some have more of the other; and certainly, if I ever did any inductive work on prices, I would want to check with the statisticians. I would not want to rely on my own statistical competence; I would use Mills' techniques. I was interested in your measures of variability. While I may not have written anything dealing with them, I have certainly used them in my thinking, and I certainly did not say that your book was not useful to the theorist. I only discussed your explanation of the significance of your results. Even if I thought I was competent, I would not have any basic criticism of your book on the score of the invention and application of new tools of statistical analysis.

MR. BYE: I should like to comment a little further on Mr. Mitchell's statement that we have no fundamental differences of opinion. Intellectually, we admit the need for bringing the empirical and theoretical approaches together, but actually, we react away from it. When I read Mills' rejoinder, I was much impressed by the evidence he adduced from his own writings to show that he does not take the attitude toward theoretical work which I had attributed to him in my *Critique of The Behavior of Prices*. It seemed very convincing. However, later passages seemed to indicate an attitude exactly the opposite of what he had tried to show.

I am going to read two quotations from his rejoinder: "I

sought, therefore, to summarize results in objective statements, definitely related to the observations, and capable of being verified or disproved by similar observations of others. Only so, I believe, can economic knowledge become accretionary" (p. 135). That is, only by quantitative methods, you see, can economic methods—

MR. COPELAND: He didn't say that. You should not identify an observational or empirical verification with quantitative methods.

MR. BYE: I have not finished reading. "Only so, may we contribute to a heritage of objective, ordered knowledge, capable of cumulative growth from generation to generation, rather than to a heritage of 'systems,' conflicting theories, and disputed rationalizations." That last phrase seems to me to be a veiled condemnation of the theoretical approach.

Then again, on p. 164, he says: "Economics has suffered and still suffers from ingenious ideas, appealing systems, excellent conceits that offer marvelous opportunities for discussion, but lend themselves not at all to the accretionary development of knowledge. The ingenious fabricator of such conceits, who barely completes the elaboration of one system before he discards it for more stately mansions, has, doubtless, a place in the scheme of things."

MR. VINER: That is how Veblen would have presented it.

MR. BYE: "But system building, facile generalization, and precipitate explanation of phenomena not yet adequately perceived may actually impede progress. Energies may be wastefully expended, observations may be hampered, and inquiry may be checked by a false but comfortable sense of understanding."

MR. VINER: I would agree.

MR. BYE: I can quote similar passages from the writings of Mr. Mitchell, which imply that the theoretical approach, at

least heretofore, has been utterly sterile. I felt that in Mills' treatment, he had definitely been influenced by that attitude and that he deliberately avoided any attempt to base any of his findings on the kind of theoretical analysis that previously had been done. I would appeal for the abandonment of that attitude. I do not know any theorists who do not think that empirical work is necessary and desirable as a complement to theoretical analysis, but it does seem to me that the empiricists have gone out of their way to give the impression that theoretical work of an a priori character is utterly futile, and has been entirely sterile. That seems to me an unfortunate attitude, and not the way to make progress.

MR. MITCHELL: I think it would be a most unfortunate attitude if anyone held it, but personally, I do not know anyone who does.

MR. VINER: What about these statements Mr. Bye just quoted?

MR. BYE: I can quote some from you, too, Mr. Mitchell.

MR. MITCHELL: I wish very much that you would. Mr. Viner has said things of that sort to me before, and I have asked him, two or three times, at least, to give me citations, but he has not done it.

MR. BYE: It was in an address you made before the American Economic Association. I am not sure whether it was your presidential address, or a previous one, in which you referred to all the bodies of economic doctrine as being ready to be thrown into the dust-heap. That was the expression you used.

MR. MITCHELL: I think you fail to recall exactly what was said. I certainly shall be surprised if I ever committed myself to such a reckless statement.

MR. BYE: I will look it up and send it to you.

MR. MITCHELL: Please do. I shall be greatly indebted to you.

MR. MILLS: I am delighted to see someone else play the role of defendant here.

MR. VINER: I want to end that. Let's come back to your passages.

MR. MILLS: I am content to have them passed upon. I do not think they repudiate a theoretical approach. They merely say that that approach can be, and has been, carried to excess.

MR. MASON: You say that no accretionary knowledge can accrue from theoretical research?

MR. MILLS: From this excessive type, which goes beyond experience or empirical verification.

MR. MASON: Then that brings the argument down to what is excessive, and what is not.

MR. MILLS: Mr. Bye did not read the first sentence of this paragraph. "The need of rational direction in such studies, of suggestive concepts and fruitful leads to the organization and interpretation of data, is not open to question." You can't leave that out.

MR. VINER: I would say that is irrelevant.

MR. BYE: You say that after the statistician has worked, we can begin to theorize, but all the past theory is poppycock?

MR. VINER: He means that the need for a system of manufacturing hypotheses is not vital.

MR. MILLS: No, I don't say that.

MR. VINER: Where do you say that theorizing is necessary?

MR. MASON: Mr. Mills is now reinterpreting the position. You are saying what you thought you meant, what you intended to mean by this passage.

MR. MILLS: What I did mean by it, and what I did say.

MR. MASON: What he did say, and what he does mean now, as rephrased.

MR. MILLS: I am talking about the excess of ingenious rationalization.

MR. MITCHELL: Let me point out just one thing. What we are doing here seemingly is—

MR. MILLS: Beating around a bush.

MR. MITCHELL:—trying to impute to one another ideas which, when so imputed, are not supposed by the men to whom they are imputed to represent their own thinking.

MR. MASON: That is the only way we can get an argument.

MR. MITCHELL: If there is a fair basis for any genuine difference of opinion here, it is concerning the implications that are to be found inside certain sentences that can be quoted from documents; and from my point of view, anyway, to insist that a man must mean by what he has put down something that he himself repudiates, is to raise an issue that may be very wholesome in the sense that it teaches us to write a little more clearly than we have, but that otherwise does not have any importance.

MR. VINER: I think Mr. Bye makes a valid point when he says that the way a man phrases propositions reveals what his subconscious position is. I could not have written that; it would have been psychologically impossible for me to write those phrases in order to mean what Mills says he means.

MR. MILLS: What conclusion should be drawn from that?

MR. VINER: That my bias is in the other direction.

MR. MILLS: My bias certainly is in favor of empirical research. I think the ultimate dependence of our theorizing must be upon empirical observation.

MR. VINER: How much faith have you in systematic theorizing?

MR. MILLS: A very great deal.

MR. COPELAND: Do you mean any one kind of system?

MR. VINER: A man may want to construct his own system for creating hypotheses. The question of faithfulness to a traditional pattern, I should say, is an altogether different

question. I would make them two different items for debate. I have not been discussing whether there is enough value in the traditional approach. It is not a question of how you shall go at it. Do you need to have, for fruitful statistical work, a fairly generalized, comprehensive, theoretical scheme, which you hold tentatively, ready to reject and modify and revise? If you do not have that, I say you will get a very nearly useless compilation of mechanical analyses that will be dead even before they are printed. But you do not have to use Walras or Marshall. You can construct your own.

In my own incompetent way, from the start, I have done quantitative work; I don't think I have a bias against quantitative work; I would have done more if I thought I was better at it; so I think I have no bias against it. As in all other fields that hope to be scientific, I think economics is going to become increasingly quantified, and I welcome that. But, if you do not have a general scheme, you will not know where to start, and that results in a tremendous waste of effort and money.

MR. YNTEMA: I should like to raise another question along the same line. Does the material used in the book have the degree of homogeneity that justifies the kind of methods applied to it, not in respect to individual items, but in respect to the whole group? Ordinarily, we resort to frequency distributions when our subclassification runs out. My opinion is that it would have been more fruitful to deal with recognized subclasses, rather than to apply shotgun techniques.

MR. MILLS: In the rejoinder, I attempted to express my point of view about the meaning of homogeneity in relation to the system of wholesale prices.

MR. YNTEMA: It is a fair question whether the material has enough homogeneity to make such measures useful. When you have a great mass of materials, you might want to measure

such characteristics as dispersion and displacement, and then use your results as finding devices to reveal unusual distributions. That is the primary use of such techniques. I am much more concerned with averages of homogeneous subgroupings than I am with dispersion when homogeneity is doubtful. If heterogeneity is suspected, you should search for homogeneous subgroupings.

MR. VINER: You can make the data homogeneous by working from percentages of change instead of from the basic data.

MR. MILLS: You may or may not.

MR. MITCHELL: The changes are no guarantee. They are homogeneous in form, since they are all percentages.

MR. MILLS: But not logically. You read, perhaps, the section of my rejoinder where I expressed the view that you often study an entity which is not homogeneous because it has significance for certain purposes. You may find more homogeneous subgroupings. That is a part of the study I yet hope to make.

MR. YNTEMA: My issue is the relative fruitfulness of that approach. I have a great deal of sympathy with studying broad groups to discover whether they behave in a similar fashion, but I do not see any great promise in studying measures of dispersion or displacement of items which you know beforehand are subject to major different influences. Frequency distribution analysis was never designed for that.

MR. MITCHELL: May I suggest some situations in which such analysis is useful? I am interested in cyclical changes characteristic of different economic processes, and it seems to me that one of the most fruitful fields of inquiry is the amplitude dispersion of the cyclical changes in the economy as a whole. I get my start on the analysis of the differences by trying to survey the field as a whole.

MR. VINER: It is a preliminary to finding the groups?

MR. MITCHELL: It is a preliminary.

MR. MILLS: It has significance in its own right.

MR. MITCHELL: Wait just a moment. I find some cases in which there is rather marked similarity in these amplitudes. They belong in one order of magnitude. After I have taken up the items that fall in that class, I conclude a given order of magnitude can be accounted for in several different ways. For instance, I find certain classes of data, such as commodity output, prices, employment, and so on, that show very small cyclical amplitudes. That may be due to any one of numerous different factors.

MR. VINER: But not a thousand?

MR. YNTEMA: It may be.

MR. MITCHELL: Each one of them might be the net result of a thousand factors.

MR. VINER: And no *one* would account for a large fraction of the phenomena?

MR. MITCHELL: I may be wrong, but that is the view I take.

MR. YNTEMA: Isn't that exactly what I am saying? My objection is to stopping with a measure of dispersion of the whole group. You ought to go on to the subgrouping stage.

MR. MITCHELL: An investigator may be in a perfectly defensible position if he does just part of a job, particularly when he himself recognizes there is a further job to be done.

MR. YNTEMA: But there is a responsibility on such an investigator to emphasize that the important work lies beyond.

MR. VINER: A necessary preliminary is important, too.

MR. MITCHELL: It is not a completed job. Mills gives no evidence of thinking that it is. The general implications of the book are that he was opening a field and wanted to do further work.

MR. YNTEMA: I still think a great deal more can be gained by studying the average behavior of relatively homogeneous

subgroupings than by saying, "This is the dispersion of a great mass of much less homogeneous material." The study of the frequency distribution of the nonhomogeneous material is very useful, because it suggests that there are important subgroupings, but we will get further by studying homogeneous materials.

MR. MILLS: My answer is twofold. I quite agree that one must go on to the homogeneous subgroupings, but I also believe the characteristics of the heterogeneous material may be extremely important, and the characteristics of wholesale prices, taken in the mass, are important, although they constitute a heterogeneous mass.

MR. YNTEMA: The distributions of changes in industrial stock prices show certain important characteristics which have some prediction value. In that sense, they may be useful. However, I don't think such distributions are so important as those for rational economic subgroups because you cannot use them as generally for prediction purposes. There is a difference between cause and effect sequences and this business of measuring something without knowing quite why it happens.

MR. COPELAND: The difference is an *obiter dictum*, I think.

MR. YNTEMA: There are differences in degree.

MR. VINER: The difference is much larger than that. Frequency distributions are important in themselves, if you are interested in frequency distributions.

MR. YNTEMA: No, no.

MR. VINER: I ask, "What is the importance?"

MR. COPELAND: Generalizations are important if you are interested in generalizations.

MR. VINER: If the other person isn't interested in them? They may be important for control purposes. What do they contribute for control purposes? Is control important? It isn't if you don't think it is. But I am saying that there may be

a special technical reason why a statistician may be gathering varieties of frequency distributions—intellectual curiosity, or the possibility that it will suggest different statistical techniques. I was not disparaging it. But I am asking, Is it economically important? Has it some obvious economic importance? Maybe it has, but I can't see it, myself.

For instance, suppose I know the character of the dispersion of wholesale prices in 1920 as compared with 1910, and I ask myself what that has to do with the standard of living of the American people, or the probability that capitalism is decaying.

MR. COPELAND: Or the theory of the price level?

MR. MASON: If you put these dispersion measures in a cyclical framework, you may get some predictive value from them.

MR. MITCHELL: There are some problems in which wholesale prices constitute a group by themselves. For instance, I should be much interested in seeing a comparison of the groupings of commodities at wholesale and the same commodities at retail.

MR. VINER: That is an economic grouping.

MR. MITCHELL: That is what I say, but it is a grouping in which wholesale prices would constitute one part of a larger whole.

MR. VINER: Oh, yes, I say that the grouping may go both ways. It may break up the wholesale prices, or it may compare these, as a group, with nonwholesale prices.

MR. BYE: I do not see why both types of analysis are not valuable. I do not see why a study of a very heterogeneous mass of economic facts, such as wholesale prices, might not reveal the presence or absence of stabilizing and disturbing factors in the economy at large. If you wanted to find out what the disturbing factors were, then you probably would have to group your data.

MR. VINER: You wouldn't know there were stabilizing and unstabilizing factors unless you grouped your data? Didn't we know that the war seriously disturbed the economy and made things different from what they were until such studies had been made?

MR. BYE: I don't think that is relevant to the question of whether you should group your data or handle them in the mass.

MR. STINE: It seems to me that such studies as Mills' do give more concrete form and better understanding of the movements of the mass.

MR. COPELAND: It gives empirical precision.

MR. VINER: How much are you willing to pay for that?

MR. COPELAND: How much will you pay for science? One purpose is to get some objective measure.

MR. HINRICHS: In some problems, where you are perfectly certain with reference to the qualitative results that are going to follow in a particular, limited field, isn't it true that the extent of the repercussions is of very fundamental significance in the determination, not of that limited area in isolation, but in the whole economy? I am thinking now specifically of problems that arise in connection with minimum wages and the repercussions that you find, viewing wages in a dynamic economy where changes that are taking place today for any one of a number of reasons are going to have repercussions tomorrow.

When you deal with such a problem, while your qualitative analysis is exceedingly fruitful, when it comes to predicting a series of events and appraising their importance, it is almost necessary, if it is humanly possible, to implement it with a quantitative measure.

MR. VINER: That isn't the question.

MR. HINRICHS: That becomes part of the question of de-

termining whether specific pieces of work are worthwhile. There are certain things, obvious to everyone in qualitative terms, concerning which measurements of magnitude are enormously worthwhile.

MR. VINER: I do not dispute that. Let us take an elasticity concept. You may know that every commodity has some degree of elasticity, but that would not be of much use. My question—and it was a question—was, Just what value would there be to increased precision of such measurement? It is not a question of measurement in general.

MR. BYE: I would go even further than Viner in respect to the importance of measurement. The very essence of science is measurement and I question the claim of economics to be considered a science until it can measure the magnitudes with which it deals.

MR. VINER: I would say, "All right, but I don't care."

MR. BYE: The statisticians should do more than just measure magnitudes. They should relate their measurements definitely to the generalizations already developed by a priori theory, or else give us a new set of generalizations. Some statisticians no doubt hope to do the second thing, and it may be that in the remote future, out of such studies as this one of Mills and others, little by little, through accretion, they will give us such generalizations.

On that point, I am frankly skeptical; I feel that a good deal of statistical work so far has been rather fruitless because it has not led to much interpretation or generalization. I believe you get much further when you take a specific problem of control like minimum wages or what the price of electricity ought to be in New York City and go at that statistically. Such results are much more valuable. This Conference might fruitfully discuss what we need to know quantitatively about prices.

MR. COPELAND: Would you be willing to say theory would be more fruitful if you focused it on a fairly concrete problem such as what the price of electricity in New York City should be?

MR. BYE: I think that has been done in theory to a considerable extent.

MR. COPELAND: I do, too; but I wonder if you would also make the generalization about theory that you made about statistics.

MR. BYE: I think the generalization will hold for theory, for the most part.

MR. VINER: That reform does not have to be made; theory already has pointed to the answer of specific and presumably significant questions.

MR. BYE: Theory has not done all that can be done in that direction, but it has done a great deal.

MR. STINE: I should like to follow up Mr. Bye's statement in connection with the experience of those of us who are working all the time with problems of particular commodities. We are in danger of thinking the price movements of a particular commodity are caused by something unrelated to that commodity which we have not discovered merely because we are specializing in that particular commodity.

In studying the operations of the future market in cotton, for example, we find things attributed to cotton which do not belong there at all, because we know the same thing happens to wheat and other commodities and therefore has no particular relation to cotton. It is a function of the economic process, and not of the commodity. We can measure what we consider the supply and demand forces operating on wheat, and for a period it goes all right; then we go off—absolutely off. Why? Because something entirely outside the relationships we have been measuring has changed the level. We see the great need

for studying these forces. They are not tied specifically to quantities nor to specific commodities.

I also think we use the terms "supply and demand" and "prices" very loosely. Mills' analysis is a good start in showing there is a tendency for prices to be driven in certain directions, that they scatter at one time and converge at another.

MR. COPELAND: Mr. Chairman, may I come back to the question Mr. Yntema raised about homogeneity? There is some reason to believe that Mills found a greater degree of homogeneity than might have been the case had he had a more comprehensive collection of price data. The recorded observations are in general those which can be most easily made. If we take branded goods, for example, and products where changes in design are of great importance, price data are very plentiful and are pretty unsatisfactory. If we had satisfactory measures of wholesale prices in these other fields, the degree of homogeneity might be less than appeared in the series that were available.

CHAIRMAN NOURSE: Do you have any comment on that, Mr. Mills?

MR. MILLS: This word "homogeneous" is a weasel word. Wholesale prices are homogeneous; they represent transactions that take place some time before the retail stage. By that definition, they are logically homogeneous. If we had more of them, it would not change that fact. If we had more of them, as Mr. Copeland suggests, we might find it possible to apply more significant principles of classification, and find more subgroupings with characteristic differences.

MR. COPELAND: You might find quite different distributions of the various attributes you investigate.

MR. MILLS: Let me clarify a point discussed a half hour ago. I think it calls for clarification. Mr. Viner suggested that I applied principles of classification based on quantitative

differences. I want to say why I took that to be a rather serious charge. If I am interested in knowing whether Swedes and Italians differ significantly in height, I can take a thousand Swedes and a thousand Italians, and I get a height of 5' 11" for one, and 5' 7" for the other. That may be a significant racial difference in height.

Suppose I take two thousand people at random, throw them into a frequency distribution based on height, and take the upper half of that and get 5' 11", and for the lower half, 5' 7". Such a test is not significant because there is no rational basis for the division of my sample. I am merely taking the facts as they come. I might search my upper thousand and find that most of them are Swedes, and my lower thousand, and find that most of them are Italians. That might suggest the principle of racial difference, but the division down the middle of the frequency distribution is based on quantitative differences, and so, as such, could have no significance.

MR. VINER: I see that, but what bothers me—again I am probably missing something—is that in your study of cyclical behavior, as I understood it, you rejected one group—a very large group—completely or partially, on the ground that they did not fit that pattern, and then you studied the remaining ones. Wasn't that a sort of grouping? And do your results apply to wholesale prices, or to a selection from wholesale prices?

MR. MILLS: To a selection.

MR. VINER: What is the significance of that? I am simply in doubt as to what it means when you find patterns of behavior in a selection of prices selected practically with a view to whether they do show them.

MR. MILLS: They hold for a group taken to conform to the general pattern of cyclical behavior, or cyclical moves, and must be so interpreted.

MR. VINER: I want to make clear that I do not pretend there is knowledge behind my question. But why wouldn't it have been interesting to study the distributions of your complete sample, including the rejected series?

MR. MILLS: It would. An average of the two, however, would not have been significant.

MR. VINER: No, but I was discouraged by the rejection of those which did not fit a pattern.

MR. MILLS: Your point is a good one, and certainly Mr. Mitchell in his work now is taking full account of those that fail to conform. I think you are perfectly right. But they should not have been included in a single system of averages.

MR. MITCHELL: In trying to understand business cycles, one of the most important tasks is to ascertain what economic processes conform to this general pattern in every business cycle, what processes show no regularity in conforming and what processes conform in intermediate degrees.

MR. VINER: I would go beyond that. You have rejected one of them as not conforming to the pattern.

MR. MITCHELL: We do not reject it.

MR. VINER: No—from this analysis. The next thing would be to find out, by listing the commodities, whether there is any explanation of their nonconformity.

MR. MITCHELL: That is what we do, and we find that the explanations are by no means uniform.

MR. VINER: No, no.

MR. MITCHELL: I think one can come fairly close to making a generalization about nonconformity, at least in the field of production, and I incline to think also in prices. When commodities are produced under conditions that prevent a producer from exercising substantial control over the volume of output, one finds nonconformity. The completer the control, the higher conformity tends to be. There may be several

types of conditions that prevent a man from exercising continuous control over his volume.

MR. VINER: Let me ask another question of the same type, which I could not have asked in 1927, because I then took the Bureau of Labor Statistics series as being all you could ask of a price series. Supposing, as we now know, that lots of these series are spurious as actual prices at which sales are made, how does that affect the results as to order and stability?

MR. MITCHELL: I don't suppose one can answer unless one knows which are the spurious series and how inaccurate they are.

MR. VINER: They did not have even the homogeneity of being the prices at which commodities were sold prior to the retail stage.

MR. MILLS: We now know that is more of a disturbing element than we thought then, but I don't think we should exaggerate. Most of those series are fairly faithful reflectors of actual quotations.

MR. HINRICHS: May I speak on that? The situation today is worse for this type of analysis than it was when Mills did his work. Very large additions to the list of prices were made in 1931. These were items whose omission had led to the criticism that the Index was an inaccurate measure of the general level of wholesale prices; but the very items that were needed from the point of view of the Index were among the most difficult to handle as valid in detail for the individual series. I do not mean that every series in 1926 was perfectly homogeneous, and some series we used then are now better than when Mills was using them, but the inaccuracies with which we are now so familiar were not so great then.

MR. VINER: I had forgotten that one of the important sources of these difficulties was this probably mistaken extension of the index.

MR. HINRICHS: Mistaken from the point of view of this type of analysis.

MR. VINER: For what type of analysis are bad price series useful?

MR. COPELAND: Are you asking about bad price series or series of observations which measure other things along with prices?

MR. VINER: I mean by a bad price series, a series which purports to list the prices at which sales actually occur, but which really lists something else, such as list prices, or prices that have changed so rapidly that they are not the real sale prices at the quotation date.

MR. COPELAND: The greatest difficulty is not so much with list prices as with prices where the specifications of the commodity are less constant, and where, consequently, you have measurements which measure something in addition to prices.

MR. VINER: I think, myself, that since the items were extended, the list prices are probably a larger portion of the added price series than they were of the old; but that is on the basis of a sampling which, in the main, Hinrichs made, and not I, but also on the basis of some individual samplings which I have made.

MR. HINRICHS: I think the very fact that these B.L.S. materials have been subjected to this kind of criticism raises some questions which are very sensible, with reference to wholesale price indexes as such, and how they can be modified, and at what points. You can find clerical errors in any agency of the Government. You can find misclassification. You can find everything.

MR. VINER: Oh, no, none of these errors are random errors. They all have a definite bias with respect to the kind of analysis that is being applied to them. They overestimate the degree of rigidity in the business cycle.

MR. HINRICHS: I agree. If I can carry that idea back to Mills' *Behavior of Prices* and apply criteria to determine the contribution it made, I should like to ask Mr. Mills whether it involves an entire misreading of his work to go into a comparison of price movements without anything further in the way of classification. I think there has been some disservice in throwing together prices subject to discrete influences such as agricultural implements and wheat or cotton, and saying, "Here is a rigid price," "Here is a flexible price or a sensitive price," and looking at the consequences in terms of production. It was not implicit in the work you did, but the fact that you couldn't do the job without classification and more refined analysis, means that while you stimulated something, the impact at certain places has not been desirable. Is it fair to criticize you for not giving us more stop signals?

MR. MILLS: The very next step was classification. Before the book was published, we had laid out twenty-six tentative bases of classification, with cross classifications that gave so many subgroupings that you did not—

MR. VINER: I wish you had put that in your preface.

MR. HINRICHS: I am sorry that I do not remember in detail. I didn't come away from your book with an impression (I have used it very carefully for a long period of time) that the classification was almost the next job that needed to be done in terms of analysis. But was it specifically lined out, so that it is a mistaken use of all the guide lines that you threw out, to jump into a problem of production and price relationships, for example, without any effort at all at classification except magnitude of change?

MR. MILLS: If the book gives the impression that classification is not necessary or desirable, then that is wholly unfortunate. I said in three or four places that group analysis was the next step, but I did not stress it as much as I am sure I should

have done. As a matter of fact, although the lines of the subsequent study were fairly clear in my own mind, I was reluctant to give hostages to fortune by saying, "This is the study I am going to carry forward and complete, and by March, 1930, this section will be out, and by March, 1932, this section will be out." I wanted to keep, as I still want to keep, some freedom of action.

MR. HINRICHS: There is some indication, then, that this kind of empirical approach, because it always has to be incomplete in terms of one human being's activities, needs to carry with it some of the stop signals.

MR. MILLS: I am sure that those signals should have been given.

MR. VINER: And "go on" signals.

MR. BYE: Mills is certainly right that he did mention in several places in the book his intention to analyze price groups in a subsequent volume. I have quoted him to that effect in my *critique*. It was there, although it was rather vague.

MR. MILLS: Purposely so at the time, but I think now, unfortunately so, in view of the misunderstanding that grew out of it.

MR. BYE: I don't think you anywhere indicated what you have here in your rejoinder as to the whole program, and as to the other part of the program, which was to seek to analyze the determinants of prices. I know that if I had been informed of that whole program when I read your book, I would have judged it somewhat differently.

MR. MILLS: I am sure I should have made it clearer. I would say, in general, that my approach is not the royal road to economic knowledge, or the only road. There are many approaches. The kind of approach Mr. Mason outlined in his Christmas paper—the study of special market situations, the attempt to classify them—I think is a very fruitful approach.

CHAIRMAN NOURSE: Are you beginning to think of lunch rather than more intellectual fodder? If so, we can adjourn.
The session adjourned at twelve forty-five o'clock.

AFTERNOON SESSION

The Conference convened at two-thirty o'clock, May 14 1939, Edwin G. Nourse presiding.

CHAIRMAN NOURSE: Are you ready to resume the discussion, gentlemen?

This morning we talked about the relation between induction and deduction in economic research. This study strongly emphasizes the inductive approach. As the discussion proceeded, I found myself wondering to what extent the types of induction which might develop from work set up precisely as this was, could be truly free and original. Starting with the frame of reference of existing cycle theory, would Mill be as likely to make a fresh and unconditioned re-examination of cyclical economic phenomena in the light of price behavior as he would have been able to do had he started from some other frame of reference?

This seems to relate to a comment in Mr. Mudgett's review (page 187 above). He seems to feel that the character of the induction which might develop from this approach is rather definitely circumscribed by the sort of cycle theory which is accepted and which he is inclined to challenge.

MR. MUDGETT: Is that where I made comments on the general business cycle?

CHAIRMAN NOURSE: Where you said "There is nevertheless one feature running through all this analysis that I find distinctly unsatisfactory. It appears in the selection of your arbitrary reference dates of this series for deciding whether cycles have or have not occurred in the various individual series," and so forth.

MR. MUDGETT: The point was whether the so-called general business cycle might not be an accumulation of many individual results. Wouldn't it be better to investigate each one by itself?

CHAIRMAN NOURSE: But the points of reference were those which had been given by a pre-existing analysis, were they not?

MR. MUDGETT: The mere fact of selecting one series as a point of reference might not influence the result. Take another one, and you might get the same set of ordered relations, but not necessarily the same thing on a given magnitude scale. Maybe I ought to reread what I said.

CHAIRMAN NOURSE: You more or less challenged the idea of the business cycle.

MR. MUDGETT: That is perfectly true. It is in the sentence where I quote comments on internal stability as being at least in part a result of the unstable price level. I still challenge that particular notion of a general price level. I don't know how much I disagree with the men present on the usefulness of the general price level concept, but there are many things about it I would challenge.

MR. MITCHELL: May I remark on that general problem? We have been studying business cycles empirically in the National Bureau for some time. Part of our work—all of which is looking forward to a general theoretical account of business cycles—is studying the cyclical behavior of different economic activities analyzed in a rather rough classification.

We mark off what we call the "specific cycles" found in each series representing these different economic activities. While there are technical difficulties, we can usually distinguish movements that we think are cyclical in character—what we call "specific cycles." We have analyzed and compared the turning points of the specific cycles in about 800 series for the United States. As I remarked this morning, we can

distinguish one group whose series move more or less in harmony with each other; however, the specific cycles of other series seem only slightly related to this congeries of activities that undergo similar fluctuations.

We think we are developing by this means a much more definite conception of general business cycles than we had before. We can tell rather definitely what economic processes are regularly involved in business cycles; that is, we can distinguish the commodities that move together from those that are relatively independent of the cyclical tides. And we can determine approximately the dates when business cycles reached their peaks and troughs.

Needless to say, there is an arbitrary element in taking particular months to mark these turns, but it does not seem to be much greater than the arbitrary element in selecting an average from given data that are moderately well grouped. We compare the timing of the specific cycles in a particular series, with these general reference dates.

This is very similar to Mills' procedure, except that he did not have any elaborate studies of specific cycles in other than price series to provide the points of reference he needed for clear presentation of leads and lags. It seems to me he adopted the best device then available for his purpose, namely, the turning dates found in the B.L.S. Index Number. These turning dates, whether on his basis, or on the rather more elaborate basis we are now using, have value primarily as points in time from which one can measure the leads and lags of different economic activities. One has to have definite points of reference.

MR. MUDGETT: Do you think you have obtained any fairly stable relationships in your major leads and lags? For instance, if a series leads another series now, will it do so in the future?

MR. MITCHELL: In some instances, there is considerable regularity in leads and lags. In others, they are scattered at random.

MR. MUDGETT: I would expect to find a certain amount of regularity. If you find constant relationships, essential parameters in an economic system, they should be true ten years hence, if the economic system remains relatively stable.

MR. MITCHELL: I think we are warranted in saying that some series show a high degree of regularity in the timing of their cyclical movements, but, of course, many series are very irregular.

MR. MUDGETT: The failure to find constant relationships was one of the least satisfactory parts of Mills' book from my point of view, and I feared you would have no better luck.

MR. MITCHELL: I think this study has considerable value. Many series show a high degree of regularity in making their cyclical turns in advance of certain other series, and the scatter in time of these leads or lags is in some instances not very great. However, I repeat, in many other series, the leads and lags vary very much from instance to instance.

MR. MUDGETT: Studies of this kind have not been very successful, and I have always thought this meant that what appeared to have statistical regularity really did not. In other words, the data appear to change very rapidly and fundamentally.

CHAIRMAN NOURSE: Mr. Mills, would it not be desirable to have the cycle theory to which you are referring more explicitly set forth?

MR. MILLS: I was trying to determine whether there was a pattern which recurred from cycle to cycle. I used the wholesale price index as a convenient reference base from which to measure time leads and time lags. I do think evidence of a persistent pattern emerged. There were regularities in revival

stages in cycle after cycle, regularities that could not be attributed to chance factors; and there were regularities in the timing or sequence of recession among several hundred commodity price series. In my written statement, I said this seemed very significant because the reality of business cycles had been questioned before this book appeared. I refer to Irving Fisher's paper on "so-called" business cycles. Therefore, it seemed rather important that my study furnished evidence of observable regularities repeating themselves through cycle after cycle, in a way that could not be accounted for by chance. No single theory of the business cycle was being tested; rather, the reality of the cycle concept was being tested, and I think the results were clear and positive.

MR. BYE: Mr. Mitchell, are you attempting to appraise or test the validity of the various cycle theories?

MR. MITCHELL: In so far as the theories can be tested for conformity to experience, yes.

MR. BYE: Do you expect, eventually, to be able to give a generalized explanation of how and why business cycles occur?

MR. MITCHELL: Yes. That is what we are working toward.

MR. VINER: Did you consider why certain theories are not susceptible to testing in terms of experience? This might show just what step in the theorizing makes empirical testing impossible.

MR. MITCHELL: If a theory of business cycles is based on assumptions that do not conform to experience, the theory is not refuted by the fact that conclusions drawn from those assumptions do not conform with what we find.

MR. VINER: I have something else in mind. You might find that a certain theory is framed so that it cannot be tested because the data required to test it do not exist, or possibly cannot exist.

MR. MITCHELL: That is right.

MR. VINER: Or there may be real data which are unavailable to the statistician.

MR. MITCHELL: Or not at the present time.

MR. VINER: I don't say you can't test the human volition cycle theory statistically, but certainly you cannot do it directly.

MR. MITCHELL: Not directly. Or take the theory that emphasizes savings—it is almost impossible to test it statistically because you can't measure savings.

MR. STINE: Are you discussing price cycles or business activity cycles?

MR. MITCHELL: We are trying to find out which economic activities are congruent with one another in cyclical behavior and which are not. Our investigation includes production in physical terms; construction work; transportation; prices of commodities; employment; individual incomes; changes in the conditions of business enterprises; dealings in securities; investments; foreign trade, merchandising, and financing; interest rates; changes in monetary stocks; total value of transactions, so far as we can trace it; and changes in the condition of the banks.

MR. VINER: But you are testing them all in terms of one cycle or pattern, in terms of timing. Some which don't fit your pattern might, upon independent analysis, be disclosed to have another cyclical pattern. What then? If your period is roughly a three and a half year period, but there are certain phenomena that have a seven year period, would you look for that?

MR. MITCHELL: Yes, indeed.

MR. VINER: When you throw it out of your pattern, you don't throw it out of our economic system.

MR. MITCHELL: We think the differences between different

cycles are due in part to the fact that certain nonconforming factors impinge in one way upon one cycle and in another way upon another. The largest nonconforming category I myself have analyzed thoroughly is the production of vegetable crops. They seem to have no regular relationship in time with general business cycles. These fluctuations in crop production are an important factor in the general business situation; therefore, we study them just as intensively as we study fluctuations in the output of steel or copper or automobiles.

MR. MASON: Mr. Mills, was there more about business cycle theory in your later book than there was in *The Behavior of Prices*? Wasn't there in the background, the notion that a certain set of price relationships are conducive to economic stability, and that a departure from that set of relationships was apt to accentuate economic instability, so that the price system and changes in price relationships exercise a causal effect upon general economic instability?

MR. MILLS: In the later work, among other things, I was interested in alterations in terms of exchange among important producing and consuming groups, the industrial producers against farmers, for example. Those relations changed rather moderately over a long period prior to 1914, then they were subject to rather extreme changes during the seven or eight years following 1914, and were subject to still more extreme changes during the five or six years following 1929.

I did not profess to have criteria of proper or desirable or equilibrium exchange relationships among these groups. So I took the evidence of the past and traced the violence of the modifications in the exchange relations among different producing and consuming groups.

MR. BYE: Didn't you try to get at changes in cost in relation to changes in price?

MR. MILLS: Yes, I included costs as a subelement among the classes or groups that I distinguished. I took not merely industrial and agricultural producers but a great many different categories, and sought to break down selling prices into component elements of labor costs, fabrication costs, material costs, overhead, and profits, and sought to trace the changes in those relations.

MR. MASON: Was it the implication that if these changes in price relationships had not been as violent as they were, the disturbance to economic stability would not have been so great?

MR. MILLS: I will put it this way, that when a violent modification occurs over a short period, adaptation to that change is difficult.

MR. MASON: Yes, but why can't the changes in price relationships be regarded as part of the adaptation itself? Does anything indicate that the rapidity of adaptation as mirrored by the changes in price relationship itself contributes to the accentuation of instability?

MR. MILLS: I attempted to bring in nonprice factors. I had quantity figures and some materials of inventories in stock and various other factors.

MR. VINER: Is your question, How does he distinguish between adjusting and disturbing factors?

MR. MASON: It is in the background of my mind.

MR. BYE: He used the word "disparity" in the book so he must have had some concept of parity.

MR. MILLS: I devote some space to disparity, which is about as vague and difficult to define as general equilibrium. If you will define general equilibrium in terms capable of empirical verification, I will define disparity.

MR. VINER: I define equilibrium as that which you don't find from statistical evidence.

MR. BYE: If you define general equilibrium more clearly, a way of measuring it statistically may be found.

MR. VINER: When you use two different methods of approach, it is very important to examine carefully the fundamental assumptions, and when they differ, to remember those differences all the time. I say the concept of theoretical static equilibrium is by its very nature not amenable to statistical demonstration.

MR. BYE: Why do you say that?

MR. VINER: A static equilibrium is based on certain assumptions, the size of the "givens." Time always brings changes in the "givens," so you get a different set of variables for every year. Therefore, while there may be an equilibrium appropriate to 1920, it cannot possibly be the same equilibrium appropriate in 1910.

MR. BYE: Why do you say such a concept is not susceptible to measurement?

MR. YNTEMA: Suppose you can measure the other factors which change, why can't you get something that encompasses them as well?

MR. VINER: I would say each year is a disequilibrium year in terms of static equilibrium—which never pretends to be a picture of the status quo. It is a picture of a constructed status quo toward which you would be moving if all the "givens" remained unchanged.

MR. YNTEMA: Why can't you have a picture of static equilibrium in terms of a function in which the values of the variables may be different from those at another time, but in which the same structure of relationships exists?

MR. VINER: You are assuming the functions are static. I don't know how you would find it out in the first place, and I don't see any a priori reasons to assume it. Everything

we know indicates that from 1910 to 1920 there would have been forces at work to change these functions.

MR. YNTEMA: If you include these factors in the functions, you can get a pretty reasonable explanation of the variations.

MR. VINER: Oh, explanation of changes! I don't say statistics can't give a partial explanation of changes.

MR. YNTEMA: But why can't your static equilibrium encompass these other factors? Why do you have to say Year 1 is different from Year 2 if you can get an explanation that includes them both?

MR. VINER: It isn't static equilibrium if it assumes changes in the size of the "givens."

MR. YNTEMA: You might say that your equilibrium is defined once you get your functional relationships. You might have different values for your variables.

MR. VINER: I have no reason to suppose the economic system is that kind of a universe. I know of no a priori argument to indicate that it is. Economic analysis never covers all the important phenomena; they are changing every moment.

MR. COPELAND: I don't see that you have answered Mr. Yntema. If you formulate a general theoretical statement and call it a static set of relationships between a price which is somehow related to the observed price and certain other factors such as production and attitudes of buyers, some aspects of which you can observe, I see no reason to assume it means anything except that some conceivable functional relationship exists among the observed variables. If this is true, it certainly can be got at empirically.

MR. VINER: I am not saying you can't study changes quantitatively, but they won't have any close relationship with the equilibrium concept of static theory. The first assumption of static equilibrium theory is that certain "givens" are constants through time, and you reply, "But what if they are not con-

stant in time?" I say we have no a priori reason to suppose that there are functional relations which are constant through time.

MR. COPELAND: Then you have no a priori theory?

MR. BYE: Don't you believe these tendencies toward equilibrium we talk about in economic theory are real?

MR. VINER: As descriptions of forces, yes, but before they realize their ultimate end, that is, a situation which they would bring about if they worked without restraint in an otherwise changeless world, there will be changes in the world which will prevent such a culmination.

MR. BYE: And because they are not culminated, we cannot measure them?

MR. VINER: I would say that.

MR. BYE: It doesn't follow at all.

MR. VINER: In terms of static assumptions, yes.

MR. BYE: I say they can be measured empirically. If they can't be measured, there is a question whether they are real. I will go that far with the empiricists.

MR. HINRICHS: That is simply hunch. Take cotton textiles, for example. This industry follows the behavior expected from straight theory closely enough so that I can use deductive theory to check the accuracy of statistical reports made to us and be right about four times out of five. I can even locate clerical errors by observing discrepancies between our records and theoretical expectancies.

MR. VINER: I don't dispute that at all.

MR. HINRICHS: If the theory of static equilibrium checks with observed values that closely, you certainly have an approximate measure of static equilibrium, even though you cannot measure it exactly. That is even more true in the case of cotton textiles because they are affected by rather large changes in price, quantity, and so on, over short periods of

time, to some extent independent of the rest of the economy. I am sure I do not know how to do it but it seems to me you are going too far when you say static equilibrium cannot be measured. Of course, it is granted that any equilibrium will change with time and external forces.

MR. VINER: Can you give a concrete illustration of something you think you can forecast in terms of static analysis?

MR. HINRICHS: Yes, very specifically. I have just been reviewing the shifts in employment that occurred presumably as a result of the twenty-five cent minimum wage established last October. Theoretically, it should have had certain consequences in a group of plants I was studying. It didn't in ten cases, but in eight of them my statistics were wrong.

MR. VINER: Let me see how you make your forecast. I would have to follow your reasoning to see how it is related to what I understand by static analysis.

MR. HINRICHS: In this particular case, it was a change in the relationship of the low-wage plants to the total market.

MR. MITCHELL: In respect to the number of people employed?

MR. HINRICHS: No, man-hours worked as a measure of approximate volume.

MR. VINER: The Minimum Wage Law raised the minimum wage for those plants, and you were able to forecast that?

MR. HINRICHS: I was trying to find out how much it affected the very low-wage plants in contrast with the movement for the industry as a whole. I had a fair-sized sample of low-wage plants to work with. In ten plants, there appeared to be an increase instead of the expected decrease of employment. An increase did occur in two cases. Eight of my cases were simply wrong statistics.

MR. VINER: What you think has been verified is the proposition that increasing cost to particular employers will lessen

their output while costs remain constant for other employers?

MR. HINRICHS: Yes.

MR. VINER: I would expect that, but it is not the same thing as measuring static equilibrium. You don't know what the new equilibrium is. Of course, I do not argue that static analysis is useless. On the contrary.

MR. YNTEMA: No, but that it has no counterpart whatever in statistics.

MR. VINER: No.

MR. YNTEMA: Then I didn't understand.

MR. VINER: Static analysis will give you a better prediction than random guessing.

MR. YNTEMA: Then it has some counterpart in statistics.

MR. VINER: If that is what you mean, I have no objections, but I still insist that you cannot describe statistically a 1910 or 1920 static equilibrium.

MR. COPELAND: Do you mean you would be able to deduce or induce from certain observations and certain limits what the equilibrium was? You might, for example, be able to say approximately what is the elasticity of demand in a static sense.

MR. VINER: You couldn't rely on the data, although I would still use it, if I had no other, and regard my results as probably better than a random guess. I wouldn't call the 1910 data a static equilibrium.

MR. COPELAND: A more generalized statement would be to say, When you have a curve which fits a given set of observations, it is hazardous to predict Y from X for any point other than those of the given observations. That is similar to the position of David Hume.

MR. MILLS: I can't identify a position of equilibrium. Can you identify it?

MR. VINER: It gives you a better basis for forecasting than

just guessing. All prediction is hazardous, but I still believe in taking the hazard because it is much less hazardous than doing without systematic prediction. I don't see what you gain by attaching the notion of static equilibrium to data taken from a universe substantially different from the universe in which you are doing your conceptual analysis.

MR. YNTEMA: It might not be substantially different in so far as one part of it is concerned.

MR. VINER: Between 1910 and 1920 there was a substantial change in population. That always has been left out of static analysis, and yet it is a change which ought to produce differences.

MR. YNTEMA: Granted that static analysis may not include all factors and influences which may be important, such as business cycle factors, for example, still, there are important segments of the economy in which static analysis is a very useful approximation to the facts and for which you can get a good statistical approximation.

MR. VINER: I don't know about that. I think the universe as a whole is orderly; but I also think economic analysis in its highest reaches never takes in more than a fraction of the universe.

MR. YNTEMA: Sometimes a big fraction.

MR. VINER: Well, I am not going to use quantitative terms, but there is enough left out when one uses ten-year intervals to interfere very seriously with your results.

MR. COPELAND: There is presumably more orderliness than we have yet discovered.

MR. VINER: I assume there is as much order in the economic part of the universe when related to the noneconomic part as there is in the astronomical world. What we call disturbances appear to be such because of the limited nature of our investigation. They are not disturbances in terms of a study of

the universe as a whole if we could comprehend the universe as a whole.

MR. BYE: If the order is there, it should be possible to measure it.

MR. MILLS: Can you define in terms capable of statistical verification the conditions either of a particular equilibrium or general equilibrium?

MR. VINER: Or of departure from equilibrium?

MR. MILLS: Can you define those in terms open to checking through observable data?

MR. YNTEMA: In some segments of the economy, yes.

MR. VINER: What, for example?

MR. YNTEMA: I am no expert, but I should say in some of the ranges of crop prices and production you would have a rough approximation. Wouldn't that be a fair statement?

MR. STINE: Yes.

MR. VINER: Well, then, you take a twenty-year series.

MR. YNTEMA: It doesn't have to be twenty years. The proposition was much more general than that.

MR. VINER: We want to have enough data. Take a ten-year series. Then you get a supply function, is that it?

MR. YNTEMA: You might get a demand function.

MR. VINER: How do you test its consistency through time?

MR. YNTEMA: Suppose you use it for forecasting and get excellent results?

MR. VINER: Lots of work has been done on supply and demand functions in agriculture. What studies have been made of the constancy of these values?

MR. STINE: I think that is what we need. We have been doing the particular things and not synthesizing them, but I feel quite confident that if we take the relations of prices of commodities, we will find a surprising degree of consistency

in those relationships over a period of fifty to a hundred years. Of course, there are some significant shifts.

MR. VINER: I would expect the relation of wheat prices to barley prices to be fairly constant over the past thirty or forty years. The relationship between the prices of No. 1 wheat and No. 2 wheat ought to be even closer.

MR. STINE: There is a close relationship between corn and wheat and corn and hogs.

MR. VINER: I would want to see the quantitative measures of that constancy and then form some judgment as to whether it is appreciable.

MR. BYE: There are at least two approaches to an answer for Mr. Mills' question. One, suggested, in my *Critique*, is to work out lines of trends; the other is data on costs. That is pretty hard to get; we need more data on costs.

MR. YNTEMA: For business cycle analysis?

MR. BYE: For a study of equilibrium prices. An equilibrium price must be a cost price, under competition. Under monopoly, difficulties arise immediately.

MR. MASON: But there is monopoly everywhere—

MR. STINE: And difficulties everywhere.

MR. BYE: You would get closer to it if you could trace changes in costs for particular commodities over periods of time; secular trends, if carefully computed, would give you an approach, because, if our theory is correct, market prices fluctuate around normal prices. The central tendency of market prices through time ought to approximate normal prices.

MR. VINER: Statistically, normal price would then be that around which the market prices fluctuate?

MR. BYE: That ought to be true. If our theory is correct, the central tendency of the variations ought to be pretty close to the normal of theory.

MR. YNTEMA: But business cycle deviations from normal are not of the sort contemplated by equilibrium economics. They are an entirely different sort of phenomenon.

MR. BYE: I wouldn't agree to that.

MR. YNTEMA: "Entirely different," is, I admit, an overstatement.

MR. BYE: The cycle fluctuation would be a fluctuation around the normal. In day-to-day prices, there are long-period and short-period fluctuations.

MR. MILLS: Don't normal prices undergo cyclical fluctuations?

MR. VINER: The normal is undergoing trends and changing through time.

MR. BYE: Take trend lines as indicative of change in the normal. If short-period fluctuations are about the normal, then the trend lines would be somewhere near the normal, wouldn't they?

MR. YNTEMA: That sort of normal was never contemplated in theory. The reason for these cyclical changes is not because there is some short-run deviation. It is because the demand and supply functions have changed.

MR. BYE: Oughtn't that to be reflected in the trend lines?

MR. YNTEMA: Maybe it should, but our normal price . . .

MR. BYE: Marshallian analysis envisages four types of price adjustments: secular change in the normal; the long-period normal; the short-period normal; and the day-to-day market fluctuations. A long-run secular change (or trend) in normal prices goes on. About that trend, there are long-period fluctuations—that is, the normal at different static times, as Mr. Viner conceives it, fluctuates around this secular change, the short-period price changes around the long-period normal, and the day-to-day price changes around the short period, so you have four movements woven together. I don't see why

it would not be possible to separate them by statistical techniques.

MR. YNTEMA: They do not encompass business cycle changes where the demand and supply functions themselves vary—not in the way contemplated in any of those four categories.

MR. BYE: I beg your pardon. I should think the cyclical changes in price would fit Marshall's theory of the short period. The price tends to the marginal cost during short periods, Marshall says. That ought to be what would happen during a business cycle.

MR. MITCHELL: Marshall's definition of the short period is that within which there is time enough to change the supply coming on the market but not time enough to change the facilities for production, and of course when the business cycle is long enough to change the facilities—

MR. VINER: In justice to Marshall, he also points out that all of these factors merge into each other.

MR. BYE: That is right.

MR. MITCHELL: I think Marshall's long-period would be closer to business cycle changes than his short-period.

MR. BYE: That may be, but in the actual phenomena, there are those interwoven Marshallian short- and long-period forces to separate.

MR. YNTEMA: That is on the supply side. What does he say about the demand side?

MR. BYE: Time-period shifts in demand have not been analyzed.

MR. YNTEMA: That is the point I am making, and business cycle phenomena are at least half demand phenomena.

MR. VINER: His interest in breaking down the supply concept and his complete lack of interest in breaking down the demand concept is interesting.

MR. MITCHELL: There are some references to it in Marshall, but he doesn't develop it very much.

MR. COPELAND: We got into all this argument, as I recall, over the question whether static analysis could be given any empirical test. Isn't that the question you were raising?

MR. VINER: No, I didn't say that. If it has no predictive value, it fails under empirical test; when you attempt to show a statistical picture of normal or static equilibrium, I think it requires such a great statistical power of verification that it takes you so far away from the theoretical concept of normal equilibrium that they are on two different levels.

Let us take the simplest case, that the price at planting in agriculture will have some bearing on the extent of plantation, and that there will be a correlation between the amount planted and the price. I would not expect price and planting to be correlated a hundred percent through time, but in a static society I would. But this does not mean that prediction is impossible.

MR. MITCHELL: Your static society would have to have static weather.

MR. VINER: We cannot do static analysis unless we assume that one variable, as distinguished from all the others, is not held stable. It is a disturbance. You can't do static analysis except by assuming conditions of stability and then introducing the one variable, the disturbance.

MR. YNTEMA: Let us take a case out of the physical sciences. Suppose you have a gas, and want to study the relation between the volume and pressure and temperature. You don't have to vary only one thing at a time.

MR. VINER: Or vary two.

MR. YNTEMA: Or vary five or six or seven.

MR. VINER: That is right.

MR. MUDGETT: If a particular price goes up one day and

down the next, and so on, you have pretty nearly a standardization with respect to that price, haven't you?

MR. VINER: In what sense?

MR. MUDGETT: If we want to measure in respect to that price, we would consider that variation is purely a chance, random variation, around that static price. Let's not put the trend in there.

MR. VINER: Let's say the trend is a horizontal line—but you find no horizontal lines.

MR. MUDGETT: This is a ratio of prices one time to price another time. We can measure dispersion of the prices from one month to the next, and if we did that for two or three months and found the same dispersion, then it seems to me we might have a pretty stable set of relationships between those three or four months; but the moment one of those prices started to go up, kept on going up, and the other one started to go down, you would find that dispersion changed, and you would find other evidences of instability appearing in your whole system. It seems to me that is a thing statistical analysis can do.

MR. VINER: No statistical analysis produces for me any evidence of constant relationships.

MR. MUDGETT: It gives evidence of random variation with respect to constant relationships. Where the price goes up today and down tomorrow but stays close to the same level all the time, that is where reality differs only slightly from the static of our conception. That is the closest we can get to our concept of the static.

MR. VINER: If you will produce for me a single statistical series that has a horizontal line for its trend . . .

MR. MUDGETT: The statistical problem is to pick out of the changing trend that part of the variation which is purely random and accidental.

MR. VINER: And shows regularity in direction? I know few discoveries of such regularities.

MR. MUDGETT: But I think the method has its possibilities.

MR. VINER: I don't think this universe produces that kind of regularities but I will yield when you show me an impressive collection of such regularities.

MR. MUDGETT: I will admit they have not been found yet.

MR. VINER: Why haven't they? There are many able men.

MR. MUDGETT: Let us assume a present demand and then let us assume a demand changing through time.

MR. VINER: Changing with some pattern of change?

MR. MUDGETT: Yes. We will have a lot of random variations around this pattern.

MR. VINER: But you cannot find a regular pattern of change of demand.

MR. BYE: How do you know?

MR. VINER: I say that statisticians like Mills and Mitchell and a lot of others have been working for twenty-five or thirty years, with fairly adequate resources, and the net crop of discovered regularities is pretty small.

MR. YNTEMA: They have not been working on that problem.

MR. VINER: They are all looking for regularities—can't help it. That is the only purpose of science.

MR. YNTEMA: But not a particular kind of regularities.

MR. VINER: I say any kind of regularity. I am not limiting it.

MR. YNTEMA: There is a great deal of regularity.

MR. COPELAND: Isn't it a corollary of your position that other hypotheses are more fruitful than the hypotheses of neoclassical theory for scientific investigation in economics?

MR. VINER: You use "scientific" as if it separated the sheep from the goats, whereas I say that even statisticians in the field

of economics should be wary of calling themselves scientists.

MR. BYE: What do you mean by regularity? I would say a trend line is an evidence of regularity if you can describe its movement by some equation.

MR. VINER: By a simple equation.

MR. BYE: It doesn't have to be a straight line.

MR. VINER: It has to be a simple equation. The number of terms in the equation describing it must be substantially smaller than the number of points of observation.

MR. BYE: It must not move at random over the graph.

MR. MILLS: You just make the game harder.

MR. VINER: If I join all the points of observation through thirty years, I suppose I have a trend.

MR. MILLS: Take an equation with thirty constants. That will give a perfect fit.

MR. VINER: That is right, and is that the trend?

MR. MITCHELL: No, certainly not.

MR. VINER: But the question is, how good a trend can you get?

MR. MUDGETT: If you can set up an appropriate conception of trend, plus an appropriate conception of cycle, plus an appropriate concept of the seasonal situation if it exists, plus other variations . . .

MR. VINER: The seasonal is about the only thing that is not doubtful.

MR. MUDGETT: Not in all price series. The statistician can break up his series into those four parts and then measure the random element.

MR. VINER: I say it has not been done. You can't get a simple equation that describes the cyclical fluctuations and one that describes the trend, and still have a good fit.

MR. MUDGETT: In one case, I think a random element of that sort was eliminated when the seasonal was taken out.

MR. VINER: Suppose you try it on a series that has thirty observation years and succeed in doing it for those thirty years, I wouldn't attach any importance to it (because you might have tried it on thirty thousand series and it only worked on one) unless you carry that series back ten years and find it still holds, and then carry it further, and it still holds.

MR. MUDGETT: I would insist on that absolutely.

MR. VINER: I don't know of very many cases where men have worked out equations for trends which haven't had to be modified when new data came along, backward or forward.

MR. MUDGETT: I wouldn't argue over what has been done; I am interested in the possibility of improvement.

MR. VINER: I am trying to learn from experience by watching the results of statisticians and generalizing from their work, which I assume is good. If striking results don't appear, I conclude it is not because of the deficiencies of statisticians, but because the universe they are describing is not the kind of universe that can produce the kind of results they are seeking.

MR. MUDGETT: There may be defects in the way they have interpreted the universe.

MR. VINER: Let me put the question very frankly to you as a statistician. Just what statistical regularities through time have you discovered? If statisticians make that sort of claim, and I think lots of them wouldn't, they ought to start listing the regularities they have discovered.

MR. YNTEMA: You find them in some cost functions.

MR. VINER: I don't know personally of a single cost function in existence, so I can't speak of that.

MR. BYE: Mills claims to have found some regularities in the pattern of cyclical movements among prices.

MEMBER: I believe something approximating a cycle exists, with ups and downs, with some bunching of the periods of

maxima and minima. I think that is the kind of regularity Mills found, and I accept it. What surprised me was the number of series he had to reject.

MR. BYE: Don't shift your ground, Mr. Viner. I am strong for a priori theory, but I think you are going too far. Probably Mr. Mitchell and others could cite a good many regularities they have found, and there is Burns' book on *Production Trends*, in which he finds there is a certain pattern of growth and decay among industries. I don't profess any wide knowledge of statistical literature, but I believe the statisticians could present a much more formidable list of regularities than you give them credit for.

MR. VINER: Then it would be a good thing for somebody to show how much regularity has been found in the whole field of economics.

MR. YNTEMA: I don't think you are fair in your appraisal of the facts.

MR. MITCHELL: Let's be a little more concrete. Take the sort of regularities we do find. We find, for instance, that when we measure the cyclical amplitudes of different processes, they are not uniform in successive cycles, but I believe that the amplitudes of cycles in the production of industrial equipment are regularly much larger than the amplitudes in the production of foods, bituminous coal, steel billets, and so on. That is, we can divide these amplitude measures into groups some of which invariably have a higher range than others. In fact, we can make four or five classifications according to magnitude and find that the same series will fall in the same class.

MR. VINER: Also in the same order of time?

MR. MITCHELL: That presents other regularities.

MR. VINER: That is the only kind of regularity I think we can hope to find.

MR. MITCHELL: You will find the same sort of regularity in conformity to business cycles—the duration of the period of expansion to the period of contraction in the specific cycle, and so on.

MR. VINER: Yes, there are other even more pronounced regularities you could find, for instance, the number of baseball tickets sold per day. I would expect to find more on Sundays and many more in summer than in winter, but I want to find constancy in these relationships.

MR. YNTEMA: What do you mean by constancy? If you find a relationship by which you can predict, there is some degree of constancy in it.

MR. MITCHELL: Let me say one more word on that head. One advantage of statistical methods is that they enable you to express rather accurately what you do find. You can measure the dispersion around whatever means you choose. I find some regularity in these degrees of dispersion. That is, one gets the idea that changes in different economic activities fall within certain ranges or are grouped around certain means and depart from them by tolerably regular degrees.

MR. VINER: The “degrees” part is what I am skeptical about. As for the sequence part, on a priori grounds, there should be many sequence regularities.

MR. MITCHELL: You will find, just as a matter of empirical fact, using the statistics we have, that these departures from the means sometimes show a considerable degree of regularity.

MR. VINER: Do you mean for particular items, steel in reference to wheat, for example?

MR. MITCHELL: Let's say retail prices in comparison to wholesale prices—the dispersion of price changes around their means.

MR. VINER: That is another thing; it is not the dispersion,

or relative shift, of a particular price as compared to another particular price. You might get two quite different kinds of results. If you get an almost identical frequency distribution of prices about the mean in 1910 and in 1920, I would say that would be one kind of result; but you would find there would be a great shifting if you were using prices of specific commodities. I would not expect to find much regularity there.

MR. MITCHELL: Neither should I.

MR. VINER: And yet the static theory has to do with relations between specific commodities and not with frequency distributions.

MR. MILLS: There has been a shift of the basis of discussion here. Didn't we start with Viner's statement that the concepts of particular and general equilibria were not capable of statistical verification? Then we passed to the question of whether there are regularities of any type in economic phenomena. The first issue is perhaps more important for our present problem. Can we use the framework of neoclassical theory with its concepts of particular and general equilibria as starting points for inductive work? I think we have not disposed adequately of that question.

MR. VINER: I think that is an important question, too.

MR. MASON: Those concepts are certainly useful, but they are different from the question you are now posing. Now you are saying, Can you find statistical evidence for the existence of equilibrium positions? That is quite different from saying these concepts of theoretical analysis are useful. Of course they are useful to help one to ask the right questions of the data, but they don't bear any simple relationship to reality because static equilibria take into account only a limited number of elements.

MR. HINRICHS: The statistician would have to insist that

static equilibrium be redefined so disequilibrium can be measured. That is, the phenomena you are measuring presumably would never be in equilibrium. Conceivably, there may be several significant criteria of equilibrium such as the relationship of market price and cost price, and at least by implication, the inflow of capital, volume of production, technically adequate facilities, and so on. If you can measure equilibrium or disequilibrium with reference to several variables, the concept begins to enter the statistician's realm.

MR. YNTEMA: Hasn't part of the difficulty been that when the economists framed their generalizations, they just didn't think about a lot of things that happened to be important?

MR. BYE: You mean important to the statistician?

MR. YNTEMA: No, important in real life.

MR. VINER: Important in determining the phenomena.

MR. COPELAND: In identifying uniformities of nature.

MR. YNTEMA: Take elementary economics when taught in terms of supply and demand. Supply and demand are functions of other variables, not constants. When you carry the analysis back to explain why the demand and supply functions shift, you sometimes can construct an explanation that fits a rather wide variety of data.

MR. BYE: You mean economic theory doesn't take account of the shifting of demand and supply functions? It certainly does take account of them, though perhaps not adequately.

MR. YNTEMA: It is not realistic in its explanations. It does not go into detail sufficiently.

MR. VINER: Theory cannot do that.

MR. BYE: We certainly picture a shifting of demand and supply schedules and their effects upon prices.

MR. YNTEMA: You say *if* they shift, this is what would happen. You do not explain that they *do* shift because of changes

in national income, changes in profit prospects, variations in optimism, and so on.

MR. MILLS: They change their form, also.

MR. VINER: In the body of doctrine you are referring to, I think a shift in demand would be taken as one of these external disturbances coming from heaven. Then you would study its effect upon your system, but that body of doctrine did not attempt to explain shifts, as a rule. There are some exceptions. For instance, classical economists tried to explain the shift of demand for land by referring to population growth. In this case, they did step out of the static framework.

MR. YNTEMA: That is not entirely a static framework. The generalization of demand and supply functions may be just as static as the framework you are talking about.

MR. VINER: If you would take the universe as a whole, all theory would be static. You would assume a constant universe, wouldn't you?

MR. YNTEMA: No, I don't think so. You also would assume rates of change. I don't think you can devise a static formula which would completely explain what happens, because part of what happens is a result of the rate at which other things have been happening.

MR. VINER: I am assuming that you are trying to develop your system so that it explains all phenomena.

MR. YNTEMA: It could not be static because every variable would be more or less dependent on rates of change, as well as status, of other variables.

MR. MILLS: You mean a useful concept cannot be static? I don't understand.

MR. YNTEMA: I thought Mr. Viner was speaking of a description of the whole universe. There are many segments of the universe in which you can get a static formula that is a

good approximation to reality, but I am not convinced that you can do it with business cycle phenomena.

MR. BYE: I don't see how economic theory could go very far in the direction of explaining shifts in demand and supply functions in general terms, because those things are dependent upon historical events that are not predictable. People's tastes change as a result of invention. The automobile supplants the horse and buggy. You can't lay down any general principle about a thing like that.

MR. YNTEMA: NO, but as their incomes go up and down, their demands for goods increase or decrease, and you can point out that cyclical variations in their demands for some goods will be different from cyclical variations in their demands for other goods.

MR. COPELAND: Assuming there is no uniformity in cultural evolution is a bit dangerous, if that is what is implied.

MR. BYE: I don't say that, but I doubt if it is predictable.

MR. COPELAND: There may be uniformity in cultural evolution, but none has yet been discovered? Is that your proposition?

MR. BYE: I don't think we can predict the future of cultural evolution. You might, by viewing the past, discover that there were uniformities. I don't know. I wouldn't say it has never been done, or never could be done.

MR. COPELAND: You wouldn't say that Karl Marx, for example, had predicted the course of the evolution of our economy?

MR. BYE: Marx? I am not so sure.

MR. VINER: I know some outstanding predictions which are almost the reverse of subsequent events.

MR. COPELAND: Some predictions were not right but some have been strikingly close to what happened.

MR. VINER: What would you say if I were to predict that

per capita consumption of wheat in ten years will be less than now in the United States? I could support my prediction on several grounds; by the empirical fact that this has been the trend over a period of years; on the ground that the changing distribution of population will reduce the number of bread-eaters; that increasing prosperity reduces the consumption of wheat in the United States; and that the decrease in the proportion of manual laborers in the total population will decrease the consumption of calorie-creating foods.

MR. MILLS: On what do you base these predictions?

MR. VINER: One of them I have indicated is extrapolation, a trend which has been going on for over thirty years. Partly, because I am assuming that white bread is the poor man's food. So my prediction would not be true for China or Germany.

MR. MILLS: You have not used neoclassical theory as a basis?

MR. VINER: No, I don't think I have. I was just trying to find out what Bye meant when he said you can't predict the future. . . .

MR. BYE: I wouldn't say you never could for a short period, but I was questioning the number of such generalizations that would help us very much in explaining economic phenomena. Maybe there are more than I think.

MR. HINRICHS: If you cannot predict, it has a tremendously significant implication for the whole of economic science. It presupposes that you are dealing with a field which is governed completely by random forces and is not subject to any substantial degree of control. Of course, in a controlled economy, you also will gain in ability to predict, and you also become highly dependent upon your ability to do so. However, in a relatively uncontrolled economy like ours, it seems futile to limit ourselves largely to the interactions of supply and

demand. To me, it seems imperative for us to give more attention to those aspects of economic behavior we have taken for granted in the past. We simply have to do it whether we like it or not.

MR. VINER: That has been a historical illusion common in American economics. There was a campaign against giving too much attention to supply and demand at a time when not over a dozen men out of several thousand economists were really devoting a large percentage of their time to theorizing on this subject. This campaign has been revived and is very active now, even though there are very few men over thirty who are now doing any theorizing on this subject.

MR. HINRICHS: I did not say too many people were doing it. I simply said that the implication of Mr. Bye's statement that the forces back of supply and demand are things to be taken for granted, is unnecessary.

MR. BYE: I would agree with you, Mr. Hinrichs, that the more light we can shed on those things, the better, but I was questioning the degree to which we can make predictions about changing schedules of demand and supply. From what some of you have said, I think I did underestimate those possibilities. Of course, if we have a controlled economy, the problem is very materially altered.

MR. VINER: In which direction? As to predictability?

MR. BYE: The ultimate justification of all science is the power it gives us to make things go the way we want them to.

MR. STINE: Or go with them advantageously.

MR. COPELAND: What is meant by "we" under these circumstances?

MR. BYE: Well, the collective organization, whatever it happens to be.

MR. MASON: A central statistical board?

CHAIRMAN NOURSE: One thing the Committee is particu-

larly interested in, and to which I referred this morning, was just touched upon and then abandoned. It is the question as to whether the book under discussion has been useful in stimulating fruitful additional work in this field, or whether you believe it has tended to misdirect subsequent work by raising false hopes and starting people down blind alleys. Mitchell says he has found it very useful. It seems to me that being oriented toward the cycle theory he has been working on, taking off from it, in fact, it naturally would circle back to it. How about its usefulness in other fields, either as to conclusions drawn from it, or as to whether it has elaborated new and useful techniques?

MR. VINER: I was especially interested in Mills' techniques for measurement of variability. Until his work, I knew of no precise formulations of the concept of variability nor of any precise techniques for measurement, and I think they have had an important influence. The question of variability has become a central point in theoretical discussion of price rigidity. I think that some of this rigidity discussion can be said to stem from Mills' work. I myself, in Mills' opinion perhaps, attribute exaggerated importance to the question of rigidity and flexibility in terms of whether it is a workable economic concept. As a tool of analysis, I attach great importance to the measurement of variability. I had no notion myself of how to compare the variability of prices at two different periods before his book was published.

CHAIRMAN NOURSE: Mr. Mason, do you have any comments on that? It falls in your field.

MR. MASON: I should quite agree with what Mr. Viner has said, although I share Mr. Yntema's doubts as to the fruitfulness of much further work on price flexibility if we are talking about the results of price behavior in the functioning of the economy. Much has been done to describe how prices

behave in the cycle, but little has been done on the question of how the economy would behave if prices were more or less flexible than they are. However, that is not Mills' fault; he was not grappling with that problem.

CHAIRMAN NOURSE: Do you have any rejoinder to that, Mr. Mills? Do you think further extension of that type of analysis would be more fruitful than Mason and Yntema seem to think?

MR. MILLS: I think the possibilities in that direction have not been exhausted. A much more accurate measure of flexibility would be that suggested by Henry L. Moore, namely, the reciprocal of the coefficient of elasticity of demand—flexibility of price with reference to one or more factors, with certain other factors held constant. I think measurements of flexibility will be used to supplement any type of analysis, including the one Mason refers to.

MR. MASON: I think we certainly can go much further in the measurement of what one ought to mean by price flexibility, that is, the measurement of responsiveness of price to changes in other variables in the economic system.

MR. VINER: You don't think it is important?

MR. MASON: I don't see how you can proceed from conclusions along that line to conclusions respecting the functioning of the economy. I think the crux of that whole argument is the relation of price behavior to changes in volumes of money expenditure. As yet, I can't see how to bridge that gap.

MR. STINE: I think it would be fruitful to try to understand the causes of variability.

MR. MASON: Oh, yes.

MR. BYE: I think the analysis of price variability or flexibility has raised some questions the theorist has not heretofore considered, such as why, according to our usual supply and demand analysis, some prices should move more freely than others. I don't think it has been explored fully.

MR. MASON: I should say a good deal of work has been done on the question of why certain prices are less flexible than others. I could cite a number of articles along that line, and I think such work will increase.

MR. VINER: But you don't think it matters?

MR. MASON: Perhaps it does, but I don't yet see how to develop techniques for dealing with the consequences of it.

MR. VINER: To me, the whole idea of price as a factor determining the working of the economy rests on the notion of flexibility, that prices govern the economy in so far as they change.

MR. YNTEMA: That is exactly the issue. I am not at all convinced that prices are an important equilibrating influence in restraining the downward movement of business cycles or getting us out of them. I feel very much as Mason does on that. Perfect flexibility may mean merely that all prices move together.

MR. MASON: That is quite possible.

MR. VINER: It is an inductive fact that the countries with the most large-scale operations and most rigid prices are the countries with pronounced business cycles.

MR. COPELAND: Is that a fact—China, for example?

MR. VINER: China does not have rigid prices.

MR. COPELAND: That is news to me.

MR. YNTEMA: What about the proposition that countries with the most durable goods have the pronounced cycles?

MR. COPELAND: To what prices do you refer when you say China doesn't have rigid prices?

MR. VINER: Well, let's say perhaps it doesn't have them.

MR. COPELAND: Which prices do you mean—wages?

MR. VINER: Oh, yes, I would include wages. Or take another example—France has much less rigid prices than we have.

MR. MASON: How do you know that? How can you say

whether the price system is more rigid in one country than in another?

MR. VINER: Except for a very few industries, the French produce a wide variety of products on a small scale. Each man produces a wide range of designs and sizes; consequently, it would be extremely difficult to set up any machinery to maintain rigid prices.

MR. MASON: That is a very intricate type of argument, though.

MR. COPELAND: The a priori way of arriving at an empirical conclusion.

MR. VINER: Also, France has had wide variations in its general price level.

MR. MASON: I should like to see some examination of the French price system.

MR. VINER: So would I. It might be very interesting. On the Chinese, my authority is Buck who has done a great deal of work on it. He says every Chinese price is a competitive price at almost every moment.

MR. COPELAND: Oh, you mean the Chinese do not have a one-price system or something perhaps approximating it?

MR. VINER: If there is a multiple-price system, it is impossible to have rigid prices.

MR. COPELAND: On the contrary, you will have an inert price structure on the whole—but my conclusion is as a priori as yours.

MR. YNTEMA: The proportion of equipment expenditures is less than in this country. If durable goods are the essence of the business cycle, no other country should have as big cycles as this one.

MR. VINER: It may be that rigid prices are correlated with durable goods and that those goods are more prominent in this country, but that is a subject for investigation.

MR. YNTEMA: I should like to refer to price flexibility again. I have a feeling that Mills' study of price inflexibility has misled a lot of people. If they had not just looked at price flexibility but also had tried to explain why there were such big shifts in quantity, whether or not there were big shifts in price, they would have come immediately to the conclusion that there must have been tremendous changes in demand, and they would not have stopped at flexibility or inflexibility. I believe emphasis on the price factor alone has stopped some thinking that may have gone beyond it.

MR. MILLS: The study of prices as prices should be supplemented by a study of prices in relation to consumption, production, stocks, inventory, and so on, but as long as prices play a role in the utilization of resources and the exchange of goods and services, measurements of the behavior of prices will have economic significance. We can refine them and improve them, we will measure aspects we have not touched upon so far, but they play such a tremendous role in modern economic life that I cannot conceive of that approach lacking significance.

MR. MASON: In that connection, Mills, I would like to repeat a question I asked a little while ago. Why isn't it proper to regard all the changes in price relationships during the business cycle as price adjustments to changes that have taken place elsewhere in the economic system?

Why use this term, disparity? Why isn't the primary disparity possibly a disparity between the yield on investment and the expectations which lead that investment to take place, and so on, and consequently why aren't all these price changes merely adjustments to that other type of disparity? Is there anything in the changes in the price system during the business cycle which causes or accentuates these fluctuations in the volume of output which you are primarily concerned with?

MR. MILLS: It is practically impossible to locate the source of causal influences, but I think we know enough about prices, about the conditions that tend to freeze certain prices, the role of overhead costs, the disinclination of producers to cut their prices when they have certain fixed obligations to meet—we know enough about those factors to know that there are barriers in the way of price adjustments.

MR. MASON: If you had a different type of price change then, that means you could be sure you could contribute to economic stability?

MR. MILLS: If we had a different kind of price adjustment? I am sure that the stickiness of prices is one of the factors that slows up adaptation to changed circumstances.

MR. BYE: That is a purely a priori observation, isn't it?

MR. MILLS: I don't think it is entirely a priori. I think it has an a priori basis, but not entirely. You can find evidence that supports it.

MR. COPELAND: Evidence that is consistent with that hypothesis and not with some contradictory hypothesis? If so, be specific.

MR. MILLS: No, that is too big an order for me to undertake now.

MR. COPELAND: That is necessary in order to establish it empirically.

MR. MILLS: I don't seek to establish it. I merely stated it as my viewpoint.

MR. MASON: The term price disparity tries to distinguish between a type of price change which is adjusting and facilitating and a type which is the opposite.

MR. MITCHELL: I had a talk with Walter Gifford some time ago. He said that during the great depression, the Bell Telephone experimented in some of its affiliated companies by reducing telephone charges to see whether that might not de-

crease the number of telephones that were being taken out. He said cutting the rates seemed to have no effect except to reduce the company's income.

MR. VINER: I would say that was not conclusive because I don't think the importance of price reductions at the beginning of a depression are in what they do directly to the volume of the price reducer.

MR. MILLS: They may well purchase elsewhere.

MR. MITCHELL: I think such experience is significant. In a business system organized like ours, there are certain classes of prices that will make more profit for a given corporation if they are kept stable. That may be one of the difficulties with our present economic organization. When one really gets into a study of the relation between sticky prices and the continued contraction of business, such experience as Gifford mentioned is important to think about. Presumably, there are also classes of prices, even in the fairly early stages of contraction, that will tend to increase the volume of business if they are reduced.

I should say one of the very important problems in price research in the near future is to seek clearer understanding than we now have, for particular commodities, of the relationship between change in price and change in volume that can be sold, and to have these facts related to different phases of the business cycle. It seems to me this problem is closely related to the attack Mills made upon price flexibility.

MR. MASON: I don't know whether you will get anywhere by this commodity-to-commodity approach. I don't think it makes any difference whether or not volume increases very much with price reduction because you have repercussions all through the economic system. Even though there is no increase in some sales, that price reduction effects other commodities, and so on.

MR. MITCHELL: Yes, but if you find that a reduction in prices does nothing except decrease the income of the corporation that cuts the prices . . .

MR. MASON: It increases the real income.

MR. VINER: It helps to explain the reluctance of manufacturers to cut prices. I think it does.

MR. MITCHELL: From that point of view, it is a pretty important factor in the business situation.

MR. MASON: This whole discussion usually starts out with the premise that you have a certain amount of money income. If the price of any particular product falls, the volume of employment will increase. But what happens to your money income with these shifts of prices? I don't see any way now of handling that particular problem.

MR. VINER: There is another mode of attack. You can approach it from the cash balance point of view.

MR. MASON: You have the same sort of problem.

MR. VINER: With volume falling off, they may reduce inventory and pay off debt.

MR. COPELAND: Approaching it from the cash balance point of view, the assumption is that you can infer what Mason is speaking of, a continuing, constant volume of income. You are using the cash balance to suggest that.

MR. VINER: You have to find out what happens to the size of the cash balances. If you approach it from the income side, you get a pessimistic attitude toward price flexibility as a cure, because, as payrolls are reduced, so is money income; if you approach it from the cash balance and you assume a constancy of cash balance, you get an optimistic approach; but both need qualification. The optimistic approach has to be qualified, because they may reduce their cash balance, or the banks may call in their loans.

MR. COPELAND: In fact, they ordinarily do.

MR. VINER: We don't know—

MR. COPELAND: I think that is one of the uniformities that is fairly well established.

MR. VINER: We don't know what they would do or how fast, if there were no rigid prices.

MR. YNTEMA: It would be very helpful to require businesses to publish their statements on a cash basis. Such data would aid greatly in studying business cycle behavior.

MR. MILLS: I refer to a point raised by Mason. Without assuming that prices are sticking or playing any causal role, I think they may have significance. Consider the relative prices of agricultural and industrial products. Assume that those two groups exchange products with each other. During eight months of severe recession, the prices of agricultural products fall to just half the level to which the prices of industrial products fall. That means that agricultural producers have to give twice the physical volume of goods they gave eight months before for the same volume of industrial goods. The most readily available parameter which defines that change is price, and it therefore has great significance.

MR. MASON: It has for the whole problem of distribution of income.

MR. VINER: And distribution of income has relevance for anticipations as to what government will be forced to do.

MR. MASON: Certainly it has.

MR. YNTEMA: However, we should be very careful in passing from that to some inference about the business cycle.

MR. MILLS: If the prices of steel products haven't fallen, those whose prices have been cut in half have to give twice as much for the steel products they buy.

MR. YNTEMA: The people who buy the farm commodities are paying half as much.

MR. MILLS: I assume that our discussion still runs in terms of physical units exchanged. Yntema's statement is true of those industrial producers whose own products have been reduced in price only half as much as have the prices of farm products. Actually, there is many-sided exchange among producing and consuming groups, and the selling and buying prices of these groups will have been altered in diverse ways. Relations between the physical contributions and the physical rewards, or real income, of these groups are definable in terms of relative price changes.

MR. COPELAND: That seems too broad a statement. I think you cannot conclude income parity from price parity. For example, if employment has radically fallen off, it seems dangerous to apply your parity theory.

Mr. Chairman, at lunchtime, Mr. Hinrichs mentioned a number of points which seem to bear on the question you raised a little while ago. I think he should repeat his remarks about some possible results of Mills' book.

MR. HINRICHS: As I said this morning, one of them was emphasis on the importance of price dispersion. I feel that Mills' analysis of the behavior of prices stimulated deductive analysis, for example, the concept of normal price, quite as much as it stimulated further inductive work. It compelled the rethinking of some important theoretical concepts.

Another very important contribution, now almost a necessary corollary for successful inductive work, was the questions it raised regarding the validity of the fundamental data. Any successful inductive research will be carried out as far as the existing data will allow and will be stopped partly for lack of hypotheses, partly for lack of materials. This is true of Mills' work. Successful scientific work ought to contribute a new refinement of our basic measures and indicate the need for further refinement and extension; it also should give a

basis for the formulation of further hypotheses. Mills' work also did this.

I think it also had an indirect influence rather difficult to evaluate. To give merely a personal illustration, I worked with Mills' book rather extensively for a period of three or four years. I have done very little work in the field of prices itself, and therefore I have had comparatively little occasion to work directly with his material. I find that my whole emphasis is on the analysis of the Bureau statistics, departing from crude averages to measures of individual variability, to regroupings, reclassification, if you like, but with an initial concentration on dispersion. Is this due to Mills' work, or would I have been doing it anyway, just as a natural consequence of seeing a body of materials? I think a good deal of our emphasis in our present analysis, departing from concentration on a central tendency, can be attributed to Mills' work.

I would like to ask Mills himself what there was in this work which was an essential foundation for his own further work. If one test of scientific work is its further usefulness, *The Behavior of Prices* should have been a sort of necessary prelude to his later work.

MR. MILLS: I should say the work I have done since 1927 was built on the work I did in the several years devoted to the study of this material.

CHAIRMAN NOURSE: Is there any implication in what you said a while ago concerning relative flexibility of prices, that to get its real usefulness and perhaps to protect it against possible misuse, the study would have to be made more comprehensive and deal not merely with commodity prices but also with agricultural prices, with prices of labor, prices of securities and other forms of property?

MR. MITCHELL: Volume of output.

MR. MILLS: It would have been a better study had it taken in all these things. I also would have been a much older person when the study was completed. It was impossible, within the limits of my time and resources, to cover all these other fields.

CHAIRMAN NOURSE: But is there any possibility of misunderstanding or misuse of it because of its limitation in these respects?

MR. MILLS: I tried to indicate the scope of the work clearly. Apparently there has been some misunderstanding as to how it fitted into a more general scheme. Mr. Viner misunderstood it in one or two points. He suggests that I might have avoided that with a little more care in qualifying the results. But to do it on a broader basis than I did, to take in volume of output, security prices, farm prices, retail prices, living costs—well, life is short.

MR. VINER: Do you think it ought to be done?

MR. MILLS: Indeed I do, though not in precisely the same way. I certainly have learned a lot since then. I presume others have.

CHAIRMAN NOURSE: But you don't think it may be misleading because of its present limitations?

MR. MILLS: I don't think so.

MR. HINRICHS: It seems to me that is a very dangerous thing even to suggest. You will never get a book out of Mr. Mitchell if you set up the finished work as the standard. The type of work he is doing cannot be finished in one man's lifetime. The danger of such work is that the man who does it will appear foolish to much less competent people twenty years later. If the work is any good, it will stimulate enough constructive criticism so that graduate students can do it better than it was done at first. So it seems to me fundamentally unsound for the Social Science Research Council to suggest that

anybody should do any more than indicate clearly the limitations that inhere in his work.

MR. COPELAND: I don't see how you can ever hope to protect yourself against most misinterpretations, particularly when abstraction is used. The attempt to make generalizations regarding observable items in our economy necessarily involves focusing attention on those items to the exclusion of some other possible functional relationships. You have to isolate in order to get forward. That is the essence of all scientific procedure.

MR. VINER: Granted that you have to select and abstract, the person who does it has a better opportunity to know what he has left undone than will the ordinary reader, and it ought to be his responsibility to state what he thinks he has discovered, and also what gaps he thinks he has left. The latter may be as important as the former for later readers. I don't think that contradicts anything Hinrichs or Copeland have said. It applies to a priori as well as to inductive work. One of the valid criticisms against a good deal of a priori theorizing is the failure to state frankly what is left out.

CHAIRMAN NOURSE: If you put such a statement in, it is not read; or if read, it is soon forgotten.

MR. STINE: A professor of mathematics used to tell me, don't explain your propositions merely so that they can be understood but so that they cannot be misunderstood. That is impossible, I find, in price analysis work.

MR. BAIN: That is what Mills thought he was doing. I think he thought nobody possibly could read the book and misunderstand the importance he attached to the other types of prices which he was not professing to study. Isn't that stated pretty specifically? I'm no economist, but I understood that.

MR. VINER: That was not the point of my criticism at all,

that he did not say he was studying only wholesale commodity prices. He didn't have to say that. What should be stated more completely was the inadequacy of a study of the price structure which considered only wholesale commodity price series.

MR. BAIN: I thought he made that clear, too.

MR. STINE: The book might have been much more enlightening had a few samples been chosen and worked through more specifically and fully—a study of the actual price-making forces operating on these particular items, and then perhaps go on from that to an analysis of selected groups.

MR. VINER: Any one of those would have taken a book the size of *The Behavior of Prices*.

MR. STINE: Not necessarily.

MR. YNTEMA: You couldn't get many of them in a book of that size.

MR. STINE: Select a few with significantly different characteristics. This would show the great variation in combination of factors that affect prices of individual items. You could also show from that the many factors that are related even though the commodities are significantly different.

Personally, I have used the book chiefly when considering measures of variability or dispersions in deciding what are good base periods against which to check disparities and parities. In my judgment, it is the only research that provides a starting point for such work. But of course, you cannot set up any base period and say that relationships between a given commodity and other commodities or between two groups of commodities will be constant. You have to check from time to time to see if any significant changes have occurred in supply or production resources or supply and demand relationships. The fact that these relationships are not constant does not condemn all parity relationships or all attempts at measuring parity relationships.

CHAIRMAN NOURSE: Are there any further comments on the contribution of this book?

MR. COPELAND: Mr. Chairman, it seems to me that it would be appropriate to emphasize the point which Mr. Bye made in his critique, viz., that Mills' work has an important bearing on the concept of the price level. It is very important to have the kind of analysis of dispersion of prices that Mills has provided to supplement the concept of price indexes. However, I do not quite think it follows that it aids the notion of stabilization programs to say that dispersion is correlated with a movement of the level. Dispersion is one factor only. In Bye's *Critique*, no investigation at all is made of how monetary policy might exercise such a control.

MR. BYE: But at least it does not close the door.

MR. COPELAND: No, there is no connection. I think investigation of dispersion is an essential part of any careful analysis of the level of prices. To talk intelligently about the level of prices, an average, you should have some idea of the adequacy of that average. The measurement of the dispersion gives this.

MR. BYE: The book challenges the theorist to state the exact meaning of general equilibrium. Certainly theorists should give this concept enough precision to make clear just what it means in a dynamic world. The relationship between theoretical and empirical work is at least twofold. First, the theorist should formulate problems and hypotheses for the empiricist to check, and, obviously, if he has not made his concepts entirely clear, the empiricist cannot check them. This suggests two difficulties: the concepts may be vague or they may be incapable of measurement. I am not convinced that the latter is often the case. For example, I would not go so far as Mr. Viner did and say that the concept of general equilibrium is incapable of measurement. However, I think the

whole concept of general equilibrium needs to be re-examined and its meaning made more clear. The same is true of the concept of normal price, if we expect the statistician to work with those concepts. I don't mean that the theorist must necessarily change his concepts to fit the statistical possibilities, but he should at least make it very clear just what he means.

The second relationship between the two types of work is to give quantitative precision to theoretical concepts so that they may be applied to particular problems. There may be other relationships, but those two seem to me very clear.

CHAIRMAN NOURSE: Are there any further points? I do not want to stop the discussion until you have given us your full views, nor to prolong it unduly. Does anyone want to be heard?

MR. HINRICHS: I have talked too much today. I am sorry. I have criticized the B.L.S. wholesale price index pretty severely, partly in order to stop its abuse. I indicated today that I had encountered a whole group of errors in our employment figures, and I should like to make it clear that there is no similarity between the employment work and the wholesale prices work, as far as I know. I spoke of eight errors. One was strictly clerical. Two others were errors of classification that had been made by me in my preliminary analysis. For example, I classified an establishment as a low-wage establishment when it was in fact low-wage only in the particular month in which it was being examined. It was really high-wage and behaved as a high-wage establishment should. I want to make it clear that I have not found anything in our employment figures that presents quite the same kind of problems that we have in prices.

CHAIRMAN NOURSE: On behalf of the Committee I want to thank all of you for your presence. We want to thank Mr. Bye

for the labor he put into the preparation of the appraisal essay and Mr. Mills, who put so much care and thought into his rejoinder. He did a little more than the Committee expected in that his introductory section has such a broad application to the problems of social science in general.

It would be appreciated if you would give me, for the Committee, any suggestions you may have, any comments on the value of this conference, and also whether you think it would be worth the outlay to do similar jobs on other books which have been selected as outstanding contributions in other fields by the respondents to our questionnaire.

Mr. Mitchell remarked to me that it would be quite useful if we could take another book of a quite different type. Clark's book on *Overhead Costs* had the highest vote, ten votes; there were two for *Social Control of Business*. Chamberlin's *Monopolistic Competition*, as I recall it, had nine votes. Knight's *Risk, Uncertainty and Profit* and his *Essays* had between them some eight or nine votes. So there were a number of other books that might have been considered. The Committee would be very glad to know whether you think any of those other books could profitably be submitted to the same sort of treatment as *The Behavior of Prices* has received.

The meeting adjourned at five-forty o'clock.

SUPPLEMENTARY STATEMENT BY WESLEY C. MITCHELL

Personally, I did not feel that the results of the panel discussion were of much value. One modest outcome for which I hoped may or may not have been achieved. I thought, and still think, that the differences of opinion relate not so much to scientific issues as to what the other side really thinks. Specifically, the men who regard themselves as representatives of economic theory seemed to believe that the men they thought of as representing statistical inquiry hold that eco-

conomic theorizing is altogether futile. The statisticians repudiated with emphasis the view imputed to them. What I do not know is whether their repudiation was believed or will be remembered.

Similarly, some statisticians seem to think that economic theorists ascribe small value to the contributions made by statistical work. If so, the professions of men like Viner and Bye should dispose of the imputation. But once again I am not sure that the people inclined to make such charges are adequately impressed by the repudiations.

This difficulty, over which the Conference did not triumph, has its roots in the time-worn controversies over economic method. Critics wanted to make Mills responsible for errors that have been actively debated for several generations. These unhappy memories from the past prevented the participants from concentrating upon a fruitful discussion of the significance and limitations of the book under consideration.

COMMENTARY ON THE MILLS-BYE CONFERENCE BY READ BAIN*

To achieve the maximum results from our appraisal of research in the social sciences, it seems advisable not only to present the salient points developed in each conference, but also to compare the differences and similarities of the methods used, results obtained, and issues raised. Since such cumulative analysis would tend to increase the length of each successive commentary, it is necessary to be as concise as possible even to the point of neglecting some topics that may be very important and of presenting others too briefly. It is hoped, however, that each commentary may help to define more clearly

* A first draft of this commentary was submitted to several members of the Conference for criticism. The remarks of Messrs. Bye, Copeland, Mills, Mudgett, Nourse, and Viner were especially helpful. If the commentary has any merit, it is largely due to these men. Its shortcomings, of course, are entirely due to the writer.—R. B.

both the nature of social science and some of the research methods by which scientific social knowledge may be obtained. It should also make clear at least some of the difficulties which beset such research in each field. The commentary, however, is in no sense a substitute for the careful reading of the documents and Conference report upon which the commentary is based.

In the sociology conference, there was general agreement that Thomas and Znaniecki's *The Polish Peasant in Europe and America* has exerted profound influence upon research in sociology and in adjacent fields. The distinctive contribution of this work was its use of human documents as data for inductive generalizations and the development of a theoretical scheme by which these documents could be interpreted. This was a distinct break with the method of most earlier sociologists who had used deductive conceptual schemes frequently far removed from any systematically collected empirical data. Most early sociology was more closely related to traditional philosophy than to natural science. Thomas and Znaniecki conceived their problem primarily as an attempt to formulate scientific laws which adequately would describe and predict the behavior of individuals and classes of individuals in changing, or transitional, cultures. Human document materials were regarded as objective data because they can be reanalyzed by other investigators. They also express or imply the "subjective factor" (attitudes) interacting with the "objective factor" (values) which the authors postulated as comprising all cultural reality. From the interaction of attitudes and values, they proposed to deduce "laws of social becoming" which would describe the behavior of persons and groups in changing cultures, thus defining and integrating social psychology and sociology.

The members of the Conference, including Thomas and

Znaniecki themselves, agreed with Blumer's conclusion that the theoretical scheme of *The Polish Peasant* does not adequately perform the task for which it was devised, that it is not wholly the product of the inductive analysis of the materials, and that the plausibility of the authors' interpretations of Polish peasant behavior depends upon their wide experience with peoples and cultures in general and with Polish peasants in particular—upon their insight and sympathetic understanding—rather than upon the methods of investigation employed or the theoretical scheme used. Yet it was generally agreed that this work shed great illumination upon the problems with which it deals and has produced valuable scientific and practical results—more because of what it started than because of what it actually did. That much sociological data, including some kinds of human documents perhaps, can be studied by methods of strict logic and the mathematical manipulation of quantified elements was brought out by several members of the Conference. This view was concurred in, with some reservations, by Thomas.

Mills' book also represents a decided break with deductive conceptual methods in economics although the empirical data used by him were much more objective than the kind of human documents used in *The Polish Peasant*. Mills developed methods of handling empirical price data which can be used by other workers so that their results can be compared with those of Mills, thus enabling researchers to check each other's work. In this way, Mills' work approximates the procedures used in the physical and biological sciences. His general objective was to open up a field which could be investigated by many men using similar methods so that useful scientific knowledge could be obtained regarding the structure and functioning of the price system in our economy. These remarks should not be taken to mean that Mills was primarily

interested in method nor that he minimizes the desirability of insight, wide experience, and creative intelligence on the part of men doing research in the social sciences. All good research men must possess these qualities irrespective of the methods or theories they use. It is also true that Mills was chiefly concerned with subject matter and hoped to produce factual results of significance to the empirical science of economics.

One contrast between the two conferences was the reaction of the authors and conference members to the critiques of the two works. The sociologists commended Blumer's analysis highly and the authors made only brief replies which in general accepted the validity of Blumer's criticisms of their work. Thomas even went much further than Blumer in emphasizing the importance of statistical analysis. Most of the discussion hinged on the question of verification of theories and hypotheses in the social sciences. Mills, on the other hand, made a lengthy reply to Bye's *Critique* and Viner's review taking issue with them on various points raised by their appraisals. The introductory part of his rejoinder is an essay on the logic and methods of social science research which is in itself a substantial contribution. All of the men in the Conference praised Mills' work as those in the sociology conference had praised that of Thomas and Znaniecki, but some of the economists felt that Mills' work was more in the field of probability theory than economic theory, that it was *statistical* rather than *economic* analysis as they understand the terms. Others, however, felt that it was both economic theory and analysis employing statistical techniques as tools.

There was substantial agreement in both conferences that "facts do not speak for themselves," that the investigator must either come to the analysis of his data with wide experience in the field or with a theoretical scheme previously elaborated as a result of empirical observations or research. Ordinarily, sig-

nificant and credible results will depend upon both of these factors. In both conferences, there was substantial agreement that the methodological issue cannot be phrased as insight *versus* proof, or deduction *versus* induction, but that these approaches are reciprocal relatives, that deductive concepts grow out of previous empirical observation and are used as starting points or interpretive devices for subsequent empirical research which in turn modifies, refines, and extends the theoretical concepts of the field in question. However, in both conferences there was little agreement as to just how deductive formulations, particularly of the large inclusive type usually connoted by "theory," can be verified by inductive research. It is perhaps evidence of the good judgment of the two conferences that they did not attempt to give any simple final answer to this very complicated question. In the sociology conference, it was discussed at some length and several views were expressed as to how large inclusive theoretical generalizations gain or lose general acceptance. They seem incapable of definitive proof or disproof; rather, they seem to be found useful, stimulating, plausible, suggestive, generally acceptable, etc., to scholars in certain cultural settings; later, they may lose these attributes. Both the acceptance and rejection of such large theoretical generalizations seem to depend largely upon the cumulative accretion of research upon specific delimited problems relevant to the larger generalization. If the mass result of such minute researches remains consistent with the general theory, i.e., if each one, or the great majority of them, may be plausibly regarded as a theorem logically deducible from the inclusive generalization, the latter tends to be accepted even though it is incapable of either proof or disproof in its most general statement. There was considerable consensus that significant inductive work is greatly handicapped, if not impossible, if the investigator fails

to make use of already existing theory in his field. The economists were more critical of Mills' work on this score than were the sociologists of Thomas-Znaniecki's. Even though Mills was using rigorous statistical methods on fairly well quantified data, his results were challenged with the charge that he had not used existing economic theory sufficiently, or some substitute for it.

Several members of the Conference intimated that Mills, at least at the beginning of his work, had thought price data could be made to "speak for themselves" to a greater extent than was justified by his results. He was accused by Bye of being an institutionalist, of lacking or concealing a theoretical framework, and of being contemptuous of a priori theory, all of which he vigorously denied. The general view of those criticizing Mills' procedure was that an investigator must have some deductive theories or hypotheses (which, of course, have been previously developed from commonsense observation or empirical research) in order to ask the most significant questions of his data and to devise methods by which to draw from his data the most useful results. This controversy was very similar to that regarding "planless and pointless empiricism" in the sociology conference. While Viner held no brief for any particular theoretical scheme, he insisted that a researcher should have some comprehensive and logically consistent set of concepts capable of furnishing hypotheses relevant to his empirical work if the latter is to have other than accidental value. This criticism of Mills' work is perhaps more valid than similar criticism of Thomas and Znaniecki's because economic theory, or more accurately, neoclassical economic theory, is a more consistent, coherent, systematic, and widely accepted body of doctrine than is true of any single systematic sociological theory. There are numerous "systems" in both sciences, but neoclassical economics has been productive of more em-

pirical and theoretical research and interpretation than has any single sociological system. Therefore, to adopt an extreme inductive procedure which seems to flout all deductive formulations is a more serious charge in economics (at least to neoclassicists) than similar procedure in sociology would be. It should be noted here that Mills does not regard his work as "extreme induction" because he had other guiding concepts than those of neoclassical theory. He holds that the problems and concepts involved in this investigation lie outside the central structure of neoclassical theory. Therefore, the neoclassical theories were largely irrelevant to his work. Even if this be granted, some members of the Conference felt that he had not made sufficiently explicit what his other "guiding concepts" were. This he has done in his rejoinder to Bye's *Critique* more fully than he did in *The Behavior of Prices*.

Mills felt, and I agree, that the preoccupation of the Conference with the old question of deduction-induction, empiricism-a priorism, tended to diminish the usefulness of the Conference. The same may be said of much of the discussion about the role of quantitative analysis. After all verbal misunderstandings, differences of emphasis, and necessary divisions of labor are taken into account, I think there is substantial agreement on these matters, but the result is that both in the *Critique* and the Conference not enough attention was given to the substance of the work under review, the economic problems with which it deals, and the specific techniques and methodological procedures which it employed.

Mills' procedure can be justified, I think, even granting that his critics properly appraised his work. He did not admit that they had done so, particularly in regard to his having no use for "theory," by which they usually seemed to mean neoclassical theory. His answer to this is that neoclassical theory

was largely irrelevant to his research, but that he therefore had no theory, and no use for theory in general, does not follow. Perhaps an analogy from common procedure in the physical and biological sciences will illustrate the usefulness of Mills' work even if we admit that it was as baldly inductive, that is, as lacking in theoretical orientation, as was alleged. A laboratory worker may carry out an investigation which is specifically designed to answer questions which he has formulated from his knowledge of work previously done, or from his familiarity with the deductive framework, or generalized and systematized knowledge of his field, which of course is the end result of previous inductive research and theorizing. On the other hand, he may investigate the behavior of animals or objects in experimentally varied situations without any previously formulated hypotheses, with no specific theoretical interests, but with a definite intention of allowing the observations to suggest significant lines of new inquiry. Mills apparently was experimenting with a somewhat similar procedure: starting with few or no conventional neoclassical presuppositions or a priori hypotheses, he was seeking to devise methods by which he and others might study empirically the behavior of various kinds of prices and arrive at valid generalizations relative to what he called a *system* of prices. Nourse, however, raised the question (page 252) as to whether the fullest possible success of this particular type of investigation may not have been defeated, not by too little deductive formulation of questions to be answered, but rather by too much implicit acceptance of the assumption of particular cyclical patterns in prices. These implicit assumptions might have prevented his ranging as freely in pure inductive behavioristic analysis as his title seemed to promise. Mills stated that he neither accepted nor had tried to prove the existence of

any particular cycle theory, but that his work did in fact show the existence of cyclical behavior in the types of prices he was investigating.

It appears that Mills' idea of "a system of prices" is a deductive theoretical concept, possibly even somewhat a priori even though there is, partly as a result of his work, considerable empirical evidence for it. Some skepticism was expressed regarding the "reality" of a system of prices apart from other nonprice aspects of the economic system. There was still more doubt as to the adequacy of Mills' behavioristic analysis of this "system of prices." For example, he was accused of treating wholesale commodity prices as if they were adequate data for determining the characteristics of the alleged system of prices, whereas such a system certainly would include all other types of prices (pp. 209 ff). Mills denied the first part of the above statement and freely admitted the second. He stated that he had tried to make it clear in his book, and still clearer in his rejoinder, that *The Behavior of Prices* was an exploratory study, a part of a larger project planned at the time the initial work was undertaken, which contemplated the investigation of other minor systems of prices, which, with all the types of prices in the entire economy, constitutes "the system of prices." He limited himself to wholesale prices because they constituted the largest and best body of materials, extending over a considerable time span, which was available at the time he began his work. He was also limited by the time and money which could be devoted to the research. He held that wholesale commodity prices are a very important part of the general system of prices. To some extent, they reflect or summarize the movements in other types of prices. Therefore, this seemed a reasonable point at which to begin the empirical investigation of the price system as a whole—particularly, since one of his major objectives was to develop refined technical

devices which might be useful in attacking problems of individual prices, price-determining factors, factor prices, and other aspects of the price system. He planned to make an overall investigation first, to develop methods and gain possible leads, and then go on later to the question of price determining factors and the behavior of individual "pure" price series (*Behavior of Prices*, p. 35) in respect to the system of prices.

Perhaps one conspicuous shortcoming of the Conference was its failure to discuss this concept of a *system* of prices more fully and to define more precisely the role of behavioristic analysis in dealing with economic phenomena, particularly in relation to a system of prices. All would probably agree that there is a "price system," a functional aspect of the total economy, and yet some might question a "system of prices" in the sense of an orderly and measurable interrelatedness between various types of prices. It is this latter aspect of the price system which Mills attempted to investigate. There was general agreement that prices constitute one of the most significant aspects of an economy like ours, but some might not agree with Mills' assumption that prices play a "certain directive role themselves" (p. 216) in the production and distribution of goods. They would doubtless agree that prices are a factor in this, but perhaps are not a "directive" one. Mills' view, however, is that it is this which produces the "system of prices." Otherwise, prices would be merely a reflection or end-result of other socioeconomic factors and therefore might not be as fruitful a field of inquiry as would be the case if they actually play this directive role. It was this apparent neglect of the influence of nonprice factors and other types of prices than wholesale which caused a good deal of the criticism, although Bye made it clear that he regarded Mills' procedure as legitimate. I do not believe the Conference explored the implications of the price-system concept adequately nor indicated the

methods (other than those in Mills' work) by which the role of prices could be determined. Such analysis probably could not be done in a conference, but it obviously is important.

There was some discussion of the general concepts of "normality," "stability" and "equilibrium" as applied to economic phenomena. It is obvious, of course, that all of these concepts are aspects of the problem of general equilibrium. At least three meanings of "normal" are current—the average, the desirable (normative), and the static, as in Marshallian analysis. Copeland held that Marshall's concept of normal carries the implication both of "natural" and "normative" (p. 32). Bye demurred. Mudgett said Marshall gives four definitions of normal. "Normal" as average is seldom or never implied by formal economic theorists, though "normal" as natural or desirable does seem to be suggested by some. Viner held that "normal" should not be used unless qualified, since it is commonly employed to represent significantly different concepts, all or most all of which are legitimate and useful. He would say the same of "stability" and "equilibrium." Mitchell was of the opinion that the statisticians' use of instability ties up with economic theory and Mudgett's note on pages 224-225 attempts to make this view more explicit. The sense in which Mills uses stability and instability with reference to prices is found on pages 214-17 of *The Behavior of Prices*.

The concept of stability is closely related to that of equilibrium. Viner pointed out that static equilibrium, by definition, cannot be measured statistically because it is merely a methodological concept which has great utility for analysis but has no empirical reality. Bye, Hinrichs, Yntema, and possibly Copeland, held that if static equilibrium exists, it can be measured, at least approximately, by using a sufficient number of functionally related variables. It was pointed out by Viner that such a procedure could not handle more than a

limited number of variables, but that this is beside the point anyway since such a concept of equilibrium is not the equilibrium of static theory at all, but rather is a concept of moving equilibrium, trend, or some other postulated or "taken" statistical starting point from which deviations can be computed. This seems to be Bye's position and he made it clear that he thought Mills' work had made an important contribution toward measuring these moving equilibria in the realm of prices. He appeared to think this tended to prove the validity of the concept of general equilibrium. This does not necessarily follow, I think, because general static equilibrium is not an empirical reality, but a conceptual construct, a mere abstraction.

Viner stated that both static and statistical analysis can furnish predictive generalizations that will give better results than mere random guessing, but he also insisted that there are very few demonstrated regularities in economic phenomena comparable to those which have been found by the physical and biological sciences. He assumes that economic regularities exist similar to those in the astronomic world, but in neither universe is there any such thing as static equilibrium, which is merely a useful methodological concept for dealing with a continually changing universe *as if it were static*. This frame of reference makes possible the formulation of hypotheses which can be tested empirically and which sometimes result in scientific generalizations, i.e., predictions more useful in practical affairs than mere guessing. Similar predictive generalizations also can be derived from static analysis. However, these facts do not prove the empirical existence of static equilibrium, though of course they are the basis for inferring the existence of order in the universe, whether economic or astronomic. Static equilibrium has *existence* and *reality*, but these attributes are conceptual, not empirical. Static analysis is

logical, whereas economic and all other natural phenomena are not static but dynamic and are known only through sensory experience.

It seems to me that Viner's general position, if I understand it, is unassailable. Static equilibrium, whether particular or general, can never be *proved* to have sense-observable existential reality by statistical or any other empirical means because it is merely a methodological postulate to aid in formulating questions presumed to be proximately answerable by empirical research. Whether it is a fruitful postulate is a matter to be determined by experience, the test being whether it generates hypotheses which provide predictive generalizations. In the last analysis, of course, all such tests must depend upon the manipulation of empirical data. In this sense, data themselves are abstractions from actually occurring, sense-observable natural phenomena which in turn are, or are assumed to be, orderly (with reference to certain implicit or explicit criteria) and are also certainly in continual flux, all being directly and indirectly connected by antecedent and consequent ("cause and effect") functional interrelations. All scientific generalizations are subject to the logic of this conception. Hence, there is some implicit or explicit theory in all natural sciences which is logically similar to the concept of static equilibrium.

It is possible to formulate concepts of equilibrium which are essentially empirical and therefore may be said to have existence in reality, i.e., they may have the same kind of "reality" that all scientific generalizations have which, as has been pointed out, are necessarily abstractions from some sense-observed data. Any system, whether physical, biological, or social, is in equilibrium when, after a foreign element has been introduced into it, thus modifying its functioning, the system returns to the condition it would have achieved had

the new factor not been introduced. Contrariwise, a disturbed system which does not regain the state it would have attained in the absence of the disturbing factor, is unstable or not in equilibrium. Thus conceived, equilibrium is a highly relative concept; a system might be in equilibrium with reference to some disturbing factors, but highly unstable with reference to others. The determination of whether a system is in equilibrium or is unstable, when specified disturbing elements are introduced, is a matter for empirical determination. Since the existence of such equilibrium can be proved or disproved, by ordinary empirical research, the equilibrium concept itself may be said to have the same kind of empirical reality that any scientific concept has. This view is similar to that expressed by Pareto and, I think, improved upon by L. J. Henderson.

The concepts of moving equilibria derived from quantified time series, trends (quantitative), etc., are also empirical equilibrium concepts. Fluctuations or deviations from the chosen or discovered base would be less ambiguous in such cases, I think, than the terms instability and stability. Such empirical equilibrium concepts as secular trend, or any measure of central tendency, are much less generalized than the concept of "any system in equilibrium" set forth above. Viner stated that "economic analysis in its highest reaches never takes in more than a fraction of the universe" (p. 265). This is true of all scientific analysis. The findings are valid only within the time and space frame of reference and the limits and accuracy of the data and methods being used. Theorists, empiricists, logicians, and men of common sense must agree to this. Such a view, which I interpret to be consistent with Mitchell's remarks on pages 225-227 and 275-277, seems to bring induction and deduction, equilibrium and change, stability and fluctuation, statistics and theory, etc., into

mutually interactive and logically consistent relations with each other—in theory certainly, and usually in actual practice.

Despite the criticisms of the book and differences of opinion on theoretical matters, there was considerable agreement that *The Behavior of Prices* has exerted considerable influence on economic research, perhaps more for the questions it has raised than those it has answered. Hinrichs thought Mills' work had greatly stimulated much needed research into price relations, and in particular, had revealed some inadequacies in wholesale price indices by transferring attention from central tendency to dispersions and frequency distributions. Mitchell said Mills' work had greatly aided him and his associates in their investigations of business cycles, particularly in showing the importance of amplitude fluctuations and developing techniques for measuring them. Copeland stated (p. 217) that he regarded the title of the book and the concept of "a system of prices" as one of its most significant contributions—that this had tended to bring the study of prices down to earth and to put it on a realistic empirical basis by emphasizing the time factor, and by implication, other economic factors. It represented a shift from the static to the empirical approach. Yntema regarded the work as faulty in that it did not deal with homogeneous materials; dispersion and displacement of heterogeneous prices do not seem as significant as dispersion and displacement of homogeneous prices. Mills referred to his rejoinder (pp. 131-133) where he showed that homogeneity and heterogeneity are relative concepts which depend upon the purposes for which the data are being used; he also stated that studies of the heterogeneous, over-all type are important in themselves, since they are useful for devising and testing procedures and for getting leads to more refined studies of homogeneous price

phenomena. *The Behavior of Prices* was a necessary first step, even though it necessarily was incomplete.

Viner regarded Mills' study of price variability as a very substantial contribution. Hinrichs said Mills' work had stimulated deductive analysis (for example, of the theory of normal price) and the rethinking of other important theoretical concepts: it also subjected the data used to rigorous criticism regarding their validity and reworked a large body of materials so that they are now available and valuable for further inductive research; it also developed several new and refined techniques of measurement; it laid the foundation for Mills' own later work; indirectly, it has influenced the work of many other men. Stine held that Mills' work is the only research that provides sound base periods as a starting point for checking price parities and disparities. Bye said the book challenges theorists to state the exact meaning of general equilibrium, but that Mills had adduced important evidence on the problem of the general price level and flexibility of prices and had shown great ingenuity in his attempt to measure tendencies to equilibrium.

Everybody admitted the statistical brilliance, originality, and thoroughness of Mills' work and marvelled at the magnitude of the labor it represented, but there was some criticism of the fact that he had investigated only one kind of price phenomena and had not shown clearly the limits of what he did do, and had not indicated its relation to extant economic theory. Viner went so far as to say that Mills' book is really an essay in statistical techniques and probability theory; that it is almost devoid of economic theory and analysis; that it is brilliant statistics, but for the most part is not economic analysis at all. Other members of the Conference did not agree with this view. Bye took the position that the economic theorist should formulate such generalizations as

seem reasonable in the light of extant theoretical and empirical knowledge and that statisticians should then attempt to test these theoretic hypotheses empirically. This idea of the statistician as a mere technical assistant to the theorist did not meet with general approval and Bye modified his position by stating that economists should also be good statisticians, that "the economic world is one of such infinitely complicated relationships that mathematical symbolization seems the ideal language in which to express the interdependence" (pp. 202-203) of the many variables involved. Therefore, he held that economics would tend to become more and more quantitative. Viner agreed. In spite of their criticisms, both Bye and Viner stated that Mills' development of statistical techniques applicable to the investigation of economic phenomena in general and price data in particular is a substantial contribution.

I believe the general question of the relation of statistical procedures to social research was not discussed as fully as its importance warrants, either in this or the first conference. It seems particularly apropos to a conference dealing with a book like *The Behavior of Prices*. It is obvious that statistical analysis is rapidly assuming a major role in an increasing proportion of research in the social sciences. Therefore, it would seem that the Appraisal Committee should not conclude its work without making a special study along this line. Statistical theory and techniques are extremely important in themselves, but their relation to social science theory and their utility and limitations in the analysis of various classes of social data are the factors which determine the characteristics of that phase of statistics now commonly called social statistics. Must we have economic statistics, sociological statistics, political statistics, psychological statistics, and so on? Or is general statistics, a branch of mathematics, adequate as a tool-tech-

nique in all sciences? Or must a special body of theory and techniques be developed for use in the social sciences—a special science of *social* statistics? Few people probably, would take such an extreme position. Statistical theory, and to a large extent, practice, is the same no matter what kind of data are being dealt with statistically. It is obvious that such terms as social, or economic, statistics must be referring to the subject matter, not to the statistical techniques or theories. The nature of the subject matter, the concepts used, the implicit or explicit theory, the data available or capable of being collected, may limit or exclude the use of current statistical techniques and may thus stimulate the development of new ones.

Statistics, however, is merely an analytical tool applicable to some aspects of all sense-observable data that can be counted. I believe it is an indispensable analytic tool for any kind of scientific research which goes very far beyond commonsense or a priori generalization. It is difficult to understand why so much ink has been spilled over the relationship between empirical, and especially, quantitative research and the logico-deductive type of theorizing. Scientific thinking—research—inevitably involves both. Without observed and communicable facts, no empirical research can be done, whether it be quantitative, observational, or experimental; but “no theory, no facts” is equally true. An observation, at least a communicable one, cannot be made without some explicit or implicit general knowledge—theories, hypotheses, postulates, frames of reference. Facts, in this sense, not only “do not speak for themselves”—they do not exist except in relation to postulates, assumptions, and previously constructed or formulated generalizations. Statistics is one of the most powerful symbolic tools thus far invented both for the formulating and testing

of scientific generalizations. It is even true that very little so-called laboratory experiment escapes the necessity of being put to the quantitative test.

If a general treatise on social statistics can be chosen for appraisal, it probably would be more profitable to submit the *Critique* and rejoinder to a number of competent social statisticians than to hold a conference on the documents. It might be still more profitable for the Appraisal Committee to formulate a number of searching propositions dealing with various aspects of the general problem, perhaps after all the other appraisal projects are completed, and submit them to a number of competent social theorists and social statisticians for analysis. I do not know how it should be done, but I feel strongly that it will be a serious defect in the work of the Appraisal Committee if this problem is not faced in some way.

In conclusion, it should be noted that Mills frequently was accused of saying things which he did not think he had said and of holding views which he explicitly denied in the book itself, in his rejoinder, in his other writings, and in the Conference. This was particularly unfortunate since the Conference time was so limited. The sociology conference was almost devoid of this defect, though it also wasted considerable time on what seemed rather unprofitable topics. The criticism of Mills' work on the ground that he investigated only wholesale commodity prices (pp. 214-215, 237-241, 246-249, Bye's *Critique* page 45, passim; and Viner's review) is particularly unjustified in view of his explicit statement in *The Behavior of Prices* (pp. 213-17) that they are only one factor in the system of prices, in his rejoinder (pp. 121-126) as to why he chose them, his careful recognition of their limitations, and his statement that this was only the beginning of a task which must employ the talents and time and money of many men

for many years. As he pointed out, and as the Conference members admitted, many of the data which he was criticized for not using were nonexistent at the time he did his work and to a considerable extent are still unavailable.

However, this criticism produced some useful by-products. The discussion dealt with a number of crucial questions relating to economic theory and empirical research—with what results must be left to the judgment of the reader. For example, there seemed to be unanimous agreement that price research is a central and basic problem in an economy like ours. So long as the essential structure and functioning of our economy remains substantially what it now is, economists must face this problem, particularly the price-determination aspect of it, using all the theoretical knowledge and empirical techniques now available and many others yet to be formulated and invented. Some of the techniques developed by Mills, and similar and better ones which they may suggest, will doubtless be useful for many years, though as Mills stated, his method is not the only way to investigate this field. The future of such research probably will depend upon the demonstration of the relations and interactions between the system of prices and such other economic factors as supply and demand, or the institutionalists' revision of these concepts. Some of the factors which certainly must be taken into account are: corporate control; capital structure; the activities of labor and other pressure groups; governmental regulation through tariffs, taxation, social legislation, crop control, conservation, etc.; changes in the industrial arts, including transportation and communication; the international situation; changes in population; changes in consumption habits; and many others.

Mills stated that there is no royal road to scientific knowledge of price phenomena. This equally is true of all scientific

knowledge, and particularly so in all fields of social science. First steps are usually faulty and faltering, but first steps must be taken before we can get anywhere with speed and certainty. Enough scientific progress has been made in the social sciences during the last twenty-five years, largely by the rise of empirical and especially statistical research, so that we now may proceed with self-confidence and celerity to create a body of scientific knowledge comparable to that attained in the other natural sciences—a body of knowledge which may be found applicable to many pressing problems of social reorganization and control. Such a body of verified scientific knowledge is obviously a necessary prerequisite if social engineering is to become more reliable than the muddled measures of hunch and hope which have so long characterized man's endeavors to manage his societal affairs.

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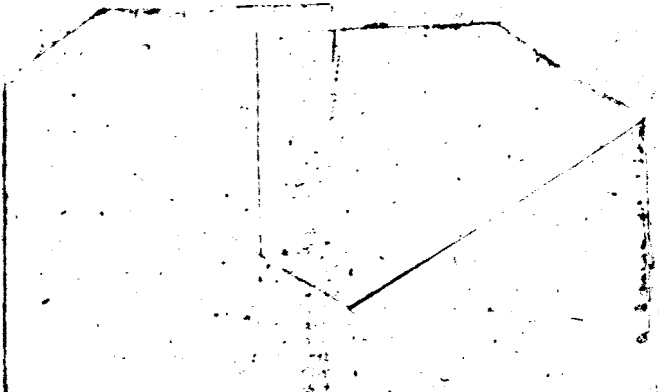
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