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# RECENT DEVELOPMENTS IN SHORT-TERM FORECASTING

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## *A. General Considerations*

THE CONDITIONS OF OPERATION as well as the short-term forecasting problem are largely set for the forecaster by the demands of the practical situation in which he finds himself. An appraisal of the outlook is wanted at once, to indicate the nature of the situation that will prevail at some future date, say a year hence. The tools and the information he can use are limited to what is at hand at the moment, or at least to what can be picked up without undertaking any new research. It is from this standpoint that the methods and procedures which have been developed in recent years will be considered.

In this review I shall confine myself to general economic forecasting. There are too many ways of forecasting movements in particular industries, commodities, or prices to permit consideration of them here. Although some of these specific methods are indeed clever and useful, most of them either require an overall forecast for arriving at a solution, or leave a wide margin of doubt in the answers given without one.

There are also many methods of general forecasting. They vary greatly in complexity and in the degree of skill needed for their utilization. Each has advantages, at least to persons who can understand and utilize the method readily. Each has deficiencies; and most involve some compromise with necessity, so that they do not quite conform to the standards of the theories underlying them.

Without getting into the details of procedure, I shall take up what seem to me the methods most commonly employed. These divide into two main groups—those variously described as survey, interview, or judgment techniques, which can be utilized, though not always efficiently, by the forecaster without extensive technical training; and analytical techniques, which require at least some mathematical or statistical skill. Specifically considered in the latter group are statistical indicators based on business cycle analysis, econometric models, and statistical analysis in terms of the gross national product and income data.

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Although it would seem that any forecasting procedure should be expected to produce meaningful results, too many "forecasts" are so indefinite as to be hardly recognizable as forecasts at all. When a forecast is so vaguely stated and so hedged about with qualifications as to avoid almost every possibility of error, it may well be disregarded. Many of the more reputable forecasts, however, also tend to lack significance—in timing, in content, or in specification of the direction of change at the terminal point.

Projecting a stable series like gross national product one month ahead would generally be regarded as inconsequential. Projecting the next quarter's average level at the beginning of the quarter represents substantially the same accomplishment, but this fact is not so generally recognized.

Estimating the gross national product for the year at the midpoint of the second quarter is hardly more of a feat. It definitely is not a process of forecasting seven and one-half months ahead; the averaging necessarily involved in such an estimate reduces the possible margin of error to only a fraction of what it might be at the year's end. Furthermore, if by this forecast date there has already been an increase of 15 per cent from the previous year's level, it would be a bad forecast, and not a good one, that showed a year to year increase of 13 per cent in comparison with an actual increase of 17 per cent.

I point this out with no intention of attempting to review and evaluate actual forecasts. My suggestion is merely that anyone desiring to make such a review set up criteria for judging performance which will prevent some rather poor forecasts from posing as good jobs. Still, I am not at all sure that such a project could produce a constructively useful result. Forecasting will probably never be perfect. There is real justification for the skepticism with which forecasts are generally received.

### *B. Survey, Interview, and Judgment Techniques*

#### 1. THE SURVEY OR INTERVIEW APPROACH

Economic surveys were in the past usually undertaken as a means of getting information. The information desired was usually quantitative, preferably to be taken from records of one kind or another. Advances in sampling technique have in recent years made surveys much more efficient, so much so that the scope of the information asked for could be greatly broadened without making the conduct of the survey overly expensive. More numerous and complex ques-

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tions frequently have made longer interviews necessary; and interviewing technique has also been improved to meet the needs of the new demands put upon it. With "results" so easy to obtain, it is perhaps not strange that this device should be turned to forecasting use.

The survey or interview approach can be made as broad or as narrow a forecasting procedure as may be desired, simply because it is possible to ask questions about anything. When used as a general forecasting device, the questions may deal broadly with the movement of the general economy, or with the movements of enough general indicators like production and prices to amount to practically the same thing. When used to forecast particular items, the inquiry may be limited to the specific questions that are considered relevant, and the list of respondents may be restricted to a group that is supposed to be able to answer those particular questions effectively.

Inasmuch as no group knows about the future of the general economy, it apparently seems quite proper to ask any group about that subject. Sometimes such inquiries are made in an effort to study other aspects of group behavior; more often just to get the information or to check up on the respondent's reliability as a forecaster.

The notion behind some of these surveys seems to be that if you can get enough people to answer a question they do not know the answer to, you will have the answer. This is not true of all forecast surveys, of course, and I shall return later to the discussion of some surveys which are designed to obtain specific information that the respondents may reasonably be expected to have.

The use of the survey as an economic forecasting device has in large measure grown out of the new "psychological" theories of economic activity. To my mind there is strong justification for a movement toward socio-psychological analysis. I believe that the realistic economics needed as a basis for forecasting may properly be regarded as a branch of social psychology, belonging in the general area known as group dynamics. Most of what is currently being said on the subject, however, does not strike me as fitting into such an approach. It is rather a kind of individualistic psychology, with motivations as the primary focus of attention. It barely touches the subject of group action, which, as has frequently been pointed out, cannot be considered merely the expression of the aggregate of individual motivations.

In discussing problems of economic activity with advocates of the

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psychological approach, I have often found myself confronted with a considerable semantical difficulty. There is a psychological counterpart to every type of human behavior, of course, and it is as appropriate to view economic behavior from the standpoint of the psychologist as from that of the economist. Viewed in this way, there can be no dispute about the role of psychology in analysis of business fluctuations. The difficulty arises when the "psychological factors" are presented as separable elements, capable of being objectively defined and measured, which exist in their own right and exert force on the economy. If the validity of such a view of specifically psychological forces is challenged, there is a retreat into the more general concept of a broad psychological perspective describing essentially the same economic phenomena from another point of view. There is a need, apparently, for careful definition of concepts and of the research techniques appropriate to each.

From the strictly economic point of view the psychological approach has a background in phenomena or processes which seemingly cannot take place without decisions by the various persons or groups involved. Thus investment in plant and equipment requires that management make a decision to expand. Such decisions, made in the course of the business cycle, sometimes prove to be "mistakes," because the investments undertaken at the peak of the boom may earn no profit in the depression years that follow. It would seem, therefore, that a realistic theory of economics should explain these mistakes. Obviously, the people who made them did not know they were mistaken at the time. They expected their investments to turn out better. Evidently, expectations are at the root of the difficulty, possibly at the root of the cycle itself. This proposition leaves something of a logical gap, but from it a whole theory of the cycle may be evolved—in terms of expectations, confidence, fears, and waves of optimism or pessimism.

At least, it is said, since expectations intervene in the process of investment, we have but to tap in at this point to determine what the future course of investment, and thus of business generally, will be.

There is, however, little evidence to support the theory that expectations are self-fulfilling. The initial findings of Modigliani's project on "Expectations and Business Fluctuations," to be published shortly, clearly reveal the disparity between what is expected and what happens.

The view of the future held by important decision-makers only

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partly influences their decisions. And their decisions only partly determine the outcome. Many things happen that nobody decides, wants, or expects.

On the consumer side many activities take place without thought or question, and many plans or projects go by the board. Preliminary results of a continuing consumer panel being conducted by Robert Ferber at the University of Illinois indicate that only about half the people who say they are going to buy actually buy in the period indicated. But the total sales are approximately as high as indicated, because others who did not intend to buy nevertheless fill the gap.

From an economic point of view it is the behavior of the whole individual that is important and not his state of mind at any given moment. There are times when fear obviously influences his behavior. He may whistle in the dark as he passes a cemetery at night, and if he thinks he sees ghosts, he may even lose his appetite for the midnight snack he usually enjoys; but by morning he will probably be as hungry as ever. He may think clothes are overpriced, but he will buy a new suit rather than lose prestige by appearing in the old one. He may think house prices are going down, but he will risk the loss if it is necessary to get suitable shelter for his family.

The operation of business enterprises is not essentially different. The businessman may expect a decline, and he may cut his inventories, but he will produce enough to fill the orders he receives; and as soon as the expectations of a decline prove to be mistaken, he will again rebuild his inventories—to increase his sales and to operate his business most conveniently for himself and his customers. When demands are strong, so that he is operating under pressure with no reserves of capacity, he will add to capacity even though construction costs are high and financial terms not as attractive as he would like.

In short, hopes, fears, anticipations, and other purely psychological factors have only limited economic effects. There are certain basic and continuing needs which constitute the primary goals of economic behavior; and while waves of pessimism or optimism may produce deviations from the rates of activity determined by these more basic economic factors, such deviations are limited in amount and in duration.

Considered from this standpoint, the whole psychological theory of the business cycle appears to be hardly more than an inversion of the real causal sequence. Expectations more nearly derive from objective conditions than produce them. The businessman both ex-

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pands and expects that his expansion will be profitable because the conditions he sees justify the expansion. Moreover, as he goes ahead, he helps to produce a situation that justifies his view and justifies a similar view on the part of others. It is not the wave of optimism that makes times good. Good times are almost bound to bring a wave of optimism with them.

On the other hand, when the decline comes, it comes not because anyone loses confidence, but because the basic economic forces are changing. Once let the real support for the boom collapse, and all the optimism bred through years of prosperity will not hold the line. Typically, confidence tends to hold up after a downturn has set in. In the spring of 1930 optimism produced an abortive recovery in the stock market, in construction contracts, and in various other phases of economic activity; but it merely concealed for a time what was to follow. Similarly a decline initiated by lack of confidence will not long continue if it is not reinforced by other factors. In the spring of 1947 the business community was swept by a wave of pessimism, but it failed to overcome the basic strength.

The effects of temporary bulges resulting from such psychological states are largely limited to the field of speculative buying and selling and related changes in inventory policy. To forecast how far a speculative movement may go, how far prices may be bid up or slashed, it would be desirable to know whether businessmen or security holders have a predisposition toward such action. If there is a bias of this kind, a forecast might properly be slanted one way or the other, but only during the temporary period in which the bias might be expected to persist; for such attitudes are subject to quick reversals. Any information obtained by the survey method would apparently have to be very current to do any good, and even then it would probably not be suitable for calling the turns, but only for modifying the projection of the ensuing movement in its early stages.

Even at best, in other words, the psychological approach leaves the forecaster in a state of uncertainty. It does not solve his problem; it merely changes it from that of determining what will happen to that of determining what will happen to psychological states of mind as the situation changes in the way anticipated.

Surveys based on an approach so fundamentally misconceived cannot lead to good forecasts. There remains the possibility that we could use the bad forecasts reported as a basis for making good forecasts by working out suitable corrections and adjustments. The

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possibility of so adapting survey results must obviously remain experimental for some time.

### 2. ANALYSIS OF PLANS AND PROGRAMS

In contrast to this relatively pointless exploration of general views, the analysis of plans and programs may represent a procedure of real merit. In the case of the federal government, for example, we obtain our best clues to probable changes in expenditure and tax programs from the budget and related discussions in Congress. Here we are dealing with proposals that are really intended to determine the course of operations; for the apparatus of government administration usually attempts to perform to the full extent of its authorization. Perhaps part of the reason we can successfully utilize this source is that we are all in on the discussion, can view the pressures for or against the undertaking of any specific program, and can apply reasonable judgments to the question of how those pressures will work out into approved plans. Even here, however, plans and programs are subject to change, and economic developments greatly modify results, so that substantial errors in estimates of expenditures or receipts are common.

A role similar to that of the budget is the goal of surveys designed to get information on business plans for future operations or capital outlays. Most of these surveys, however, lack the advantages of the budget. They are not subject to open discussion. They are liable to quicker changes. They do not provide so comprehensive a statement of what is behind the indicated plans or what will support their completion. The reports received from the companies are usually confined to a bare description of what the programs provide. Sometimes the reports are not even in terms of definite quantities, but merely in terms of general comparisons with a previous base.

Most of the surveys that collect current data also obtain data on the recent past, for comparative purposes. These data are in some cases of substantial value in themselves and may justify the survey even apart from the projections into the future.

Looking at the performance of these surveys from a forecasting point of view, I do not see that it has been an impressive one. In the case of capital expenditures surveys, which hold the greatest promise because forward commitments and orders for future delivery provide a basis for estimating prospective expenditures, results are apparently not too far off in the short run, but they are obviously unrealistic in the longer run. When the McGraw-Hill Department of



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Economics surveyed plans for capital expenditures five years in advance, the results showed a steady and wholly unjustified decline.

Progressive discounting of the future may prove to be characteristic of this type of survey. At least, this hypothesis tends to be confirmed by my own experience in wartime surveys of various kinds—such as those concerning materials requirements, labor requirements, and production schedules. Beyond the immediate future these surveys typically underestimated, with the degree of underestimate increasing throughout the period covered. This bias was substantially modified whenever contrary policy considerations were involved, as, for example, when priorities for material were desired or when it was felt necessary to protect a plant's labor supply. But the general rule seemed to be that what business had not scheduled it did not report.

The survey of planned capital outlays conducted by the Department of Commerce and the Securities and Exchange Commission has also tended to produce low estimates. The estimates for the year beginning just prior to the survey date were within a tolerable margin of error but were low in every year except 1949. The let-down in business in that year had not been fully anticipated at the time the reports were filed. In 1947 and 1950 the estimates were distinctly too low. The estimate for 1947 was more or less in line with the pessimistic thinking of the early months of that year. The estimate for 1950 reflected the mixed views of the early part of the year, and of course the Korean war could not be taken into account at that time.

The preliminary quarterly estimates—estimating one or two quarters in advance of actual data—also have rather consistently run low. A minor point of interest in connection with these figures is the lack of close seasonal relationship between the preliminary estimates and the actual figures, which are presumably taken from the accounting records. The errors in the preliminary estimates are substantial, since one quarter is in any case a relatively short period over which to project a statistical series of this kind. It is not at all clear that the results obtained so far are any better than would be obtained by merely projecting seasonally adjusted lines on a chart in the light of what is known about current changes in the business situation.

Other surveys reporting anticipated sales or other operating items have, on the whole, worse records. It is sometimes suggested that instead of getting executive estimates on these items it would be better to ask the reporting companies for their operating budgets.

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Whether this would in fact produce superior results is a matter of some doubt. Perhaps this suggestion involves a misapprehension about these budgets—that they attempt to establish a fixed schedule for future operations. Actually, their basic purpose is not to predetermine operations, but to coordinate them; and it is primarily from that point of view that they attempt to set forth in as explicit a manner as possible the expected levels of operation. If conditions change, operating plans must change, and budgeting can do no more than help keep necessary adjustments in procurement, production, and sales schedules in line with each other. On the other hand, any systematic source of this kind may be better than mere opinions. What we ought to know first of all is just how the reports currently being sent in are prepared.

Experimentation in this whole area of research is unquestionably justified. No doubt some of the worst examples of inconsequential surveys, or survey questions, could be eliminated without loss, but an effective appraisal of the better surveys cannot be made until data are available through a complete business cycle under normal conditions, that is, under conditions less drastically affected by war or the threat of war.

One further word of caution may be inserted at this point. Whether the survey results are good, bad, or indifferent, there is a tendency to play them up as the final word. Partly because results are often presented in such a way as to attract interest to their most newsworthy features, there tends to be too much rather than too little acceptance of the results of these surveys. They seem to be relied upon to an extent not justified by the results produced to date. Until a more convincing demonstration of their validity is forthcoming, the forecaster may well continue to look upon these experimental findings with skepticism. As yet they provide no safe substitute for sound analysis.

### 3. GOOD JUDGMENT

I turn now to the method that has been the stand-by of the business executive, forced by practical necessity to make decisions affecting his company's operation. These decisions can be said to imply forecasts even though none is explicitly made. He has to make up his mind, and the only way he can do so is to make the best judgment possible in the light of his knowledge, however limited, of the general economic situation, the outlook for his own industry, and the position of his own company. Good judgment is

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usually considered one of the prime requisites of the efficient executive, though what constitutes good judgment in this sense is seldom defined or tested.

Forecasts made in this way, often with no apparent method, possibly on the spur of the moment, sometimes turn out to be surprisingly accurate. The seasoned and mature veteran of many business situations acquires the knack of what is popularly known as "playing by ear." He picks up cues from small snatches and isolated details, and may be able to piece together a fairly accurate representation of the complete score.

Where the methods of judgment are more specifically described and supported, we may distinguish at least two definite procedures. The first involves the use of historical analogy. Such analogies may be explicitly spelled out, as was done by Woytinsky in his forecast of 1946;<sup>1</sup> or they may lurk in the background of the forecaster's experience without conscious formulation. When a situation is identified as essentially similar to a known previous situation, the analogy is put to work to supply a forecast of future developments.

The difficulty with this procedure lies in the identification of sameness. Certain similarities are bound to appear in each business decline—that much is almost a matter of definition. But superficial likenesses are not enough, for the concealed differences may be of overriding importance.

One of the fundamental propositions of logic is that an analogy can at best establish only a probable inference. Even that is valid only to the extent that there has been a fair sampling of similar situations and that an essential identity of relations and conditions exists.

In this complex, dynamic world of ours, economic conditions are never the same. Even in two postwar periods, when they might be expected to be most nearly alike, there are important differences. Thus, while the initial postwar inflation was correctly forecast by analogy with 1919, serious declines were incorrectly forecast by analogy with 1920.

It is true that we rely on the past for an understanding of the processes by which economic change takes place. Analogies often provide clues or hypotheses that prove valid on further analysis. For any analogy to be more than a clue, however, it must fit into the unfolding pattern of real events. The future flows not from the past,

<sup>1</sup> W. S. Woytinsky, "Postwar Economic Perspectives," *Social Security Bulletin*, December 1945, pp. 18-29.

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but from the present—not from any repetition of previous experiences, but from the forces currently making for change. Analogies, therefore, will always have but limited usefulness in forecasting.

The second procedure consists of striking a balance between the favorable and unfavorable factors affecting current rates of activity. The selection of factors on each side may be systematic or unsystematic; it may cover the whole economy or merely certain parts that are considered decisive at the moment; it may take up major segments in their entirety or consider some of their subsegments only. Appraising the impact of the factors selected may also be systematic or haphazard.

At its worst the process produces highly indefinite and qualified forecasts, perhaps concluding with something like this: "On the basis of the considerations mentioned with respect to the factors selected, it appears that the course of business will tend to be downward." At best it may come very close to a sound quantitative analysis of emerging economic developments. The closer it comes to the latter extreme, the more likely it is to produce sound forecasts.

A balancing of factors has no validity in anything but quantitative terms. Otherwise, one factor may be sufficient to override all the others, and yet be played down as relatively unimportant; or all the important factors may be on one side, and yet be judged in qualitative terms as less consequential than those on the other side. If consideration is limited to certain segments only, those omitted may actually be the decisive elements. There can be no substitute for a complete accounting in quantitative terms of all important aspects of the economic picture.

Those who consider sound judgment to be the answer may wish to quarrel with this statement. Some will argue that in some informal, mysterious manner judgment accomplishes the same result. They may point to instances where judgment alone was successful. But all this needs something in the way of more logical support. Intuitive processes cannot be shown to perform statistical computations; and any test that omits consideration of failures cannot demonstrate the validity of processes that produce some successes. What seems the more likely alternative is that there can be no consistent judgment in the absence of a quantitative approach.

Consider the bases of sound judgment. The first is experience. But experience is always limited and partial. No matter how mature and well trained a person may be, he will suffer many doubts if his information is not quantitative. There are times when the situation

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looks a little like this, a little like that, and yet is somewhat different too; or this factor may possibly be more important than that. All of which leads many forecasters who rely primarily on judgment, including many of those whose reputations are the best, to hedge their forecasts with qualifications of various kinds.

The seasoned expert will, on the whole, do better than the novice. It has frequently been pointed out that the hunches of the expert are on an entirely different level of significance from those of the amateur. One of the main reasons for this is the expert's superior knowledge. He may also have special sources of information from which he can find an answer to the specific question that represents the key to the whole problem. But even the expert can form sound judgments only if he has found a means of getting and ordering information, of selecting the significant items from the mass of information confronting him and weighing them in relation to the items that appear on the other side of the question. When confronted with new situations, he may find himself completely at sea. When there are developments not encompassed by the scope of his experience, and not covered by his usual sources of information, he may be unable to cope with the problem. For only a systematic technique of analysis, working through principles rather than cases, can deal with the new situations that are continually arising.

Most people, lacking the perspective of the expert, cannot even understand the significance of current information unless it is put into a context in which it is properly appraised in relation to other things. From the forecaster's point of view such a context involves a framework for quantitative analysis. Understanding in the significant sense is understanding in quantitative terms; and this is the primary basis for good judgment.

Judgment is still needed to obtain a practical solution. It is, in fact, required at all stages of the forecasting process. It must be used to interpret data and to select and appraise techniques of analysis. But judgment in a process of quantitative analysis is quite a different thing from judgment used to pull a general answer out of nowhere.

Beyond this, judgment must be independent and impartial. The only authority accepted is the logic of unbiased intelligence. It is the fitting of any fact or idea into the broad perspective of past and current economic events that provides its forecasting significance. Each alternative, regardless of source, regardless of special interest or policy implication, must be logically evaluated. If there were a

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short way to summarize the qualities of good judgment, it would be to say that they combine informed experience and a scientific attitude of mind.

### 4. RELIANCE ON EXPERT OPINION

The use of a panel of experts as a basis for forecasting may be either a survey or a good-judgment method, depending upon the manner in which it operates. It is the former when results are merely tabulated and accepted as given. It is the latter when the reasoning behind the experts' views is examined and an effort is made to reconcile and synthesize those views.

In either case the panel may be selected in various ways. One way is to get together a group who are all, for one reason or another, considered to be good consultants or forecasters in general. Another way is to select a group who are specialists in various branches of economics, or represent various functional points of view, such as labor, finance, industry, and agriculture. Selecting the panel in accordance with the first of these criteria presumably involves the difficulty that certain branches of activity or certain functional areas in which important developments originate may be overlooked through preoccupation with general developments. In the second case the difficulty is that the specialists may not be broadly enough informed on general economic conditions to cast their views into the wider perspective needed for successful forecasting. The two kinds of criteria may, of course, be interwoven.

When the survey technique is used as a means of obtaining a composite view, all that can be said for the results is that they represent the average opinion of the particular group of experts canvassed. These experts have qualifications of various kinds, which may or may not be explicitly indicated. As a rule only a limited number of them make any systematic analysis of the situation. Most of them merely put down numbers which seem to be approximately consistent with their general view of the future. The consensus thus tends to portray the unsupported judgment of the group. If some of the experts should happen to work out answers by detailed analytical techniques, these answers tend to be buried in the process of averaging.

Although there is little in the whole history of forecasting to encourage its use, this procedure is very commonly employed. The postwar period is crammed with instances of forecasts in which the consensus was mistaken. Let me quote from J. A. Livingston, who

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conducts such a panel for the *Philadelphia Bulletin*. In the report published January 1, 1950, summarizing the returns from "38 of America's top-ranking economists," he points to a swing to the optimistic side and then goes on:

"The optimism is a recent development, encouraged by the improvement in business since July. Which proves that economists not only influence business, but are influenced by it. They reflex as well as reflect.

"A striking instance of this was 1946. In June, the economists were bullish. But after the September break in the stock market, they turned bearish. When the business collapse didn't come, they gradually became more optimistic.

"Again in 1949, events governed opinion, rather than opinion events. At the outset of the year, four out of 10 were moderately pessimistic. By March, after business started to drop, six out of ten were bearish. By June, eight out of ten. Yet, at the height of the bearishness, the inventory recession was over. Production had turned up. Fluctuation in the economists' optimism is shown in the following table:

	Per Cent Expecting Business to:	
	Decline	Rise or Remain Stable
December 1946	92	8
December 1947	61	39
December 1948	41	59
March 1949	56	44
June 1949	76	24
December 1949	36	64"

Results like this may well give pause to the thought that economists are the country's experts on the business outlook; but let it pass as one of the mysteries of how reputations are made. Presumably, a panel of this kind could be improved by dropping those of its members who are least successful. This, however, is hardly ever done. It would introduce the problem of getting suitable replacements; and in any case it would require a rather extended period of testing to ensure that those being dropped were not the victims of the particular circumstances affecting the period in which the initial surveys were made.

I am inclined to the view that a group opinion of this kind is always likely to express the dominant bias of the times, just as busi-

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nessmen lean to optimistic ventures in the boom phase of the cycle. Current conditions tend to set the theme, which gains acceptance through the visible evidence of those conditions themselves; and the theme is then repeated and hammered home, day after day, by radio and press, to such an extent that it becomes difficult for anyone to maintain the objectivity which independence of judgment requires.

If any conclusion can be drawn from this, it is that the best practice is to disregard the consensus, but to seek the reasoning behind the views of the various panel members and to test and utilize the most promising by means of the same techniques of quantitative analysis that might be used to work out an original forecast.

At this point we have arrived at the discussion of the second method for utilizing a panel of experts. In this case the panel is organized into a kind of seminar for an exchange of views among panel members. The burden of judgment is then thrown back on the sponsor, or the person conducting the seminar. He is usually presented with a number of divergent views, each supported by at least a semblance of logical argument. Sometimes the facts themselves are in dispute. The sponsor must then sort out from among the facts, hypotheses, and arguments presented those which make up a convincing picture of possible developments.

In this approach the panel becomes a source of information and ideas, but cannot be considered the source of the forecast itself—or of any policy or operating decisions which grow out of it. It may be a fruitful approach for the sponsor who is able to use it effectively. The procedure is likely to be costly, since a group of experts cannot be assembled without considerable expense. Against this must be weighed the sponsor's own time limitations and a possible lack of other opportunities for obtaining information and ideas that may be of overwhelmingly greater importance than the expense involved. With the growth of great organizations, the conditions for the use of panel discussions appear to be expanding, in business as well as in government. The validity of results is obviously dependent upon the quality of the panel and the good judgment of the sponsor.

### *C. Analytical Techniques*

#### 1. THE BUSINESS CYCLE APPROACH

There has recently been a revival of interest in statistical indicators of business cycle movements as a result of the National



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Bureau's work in this field. The most comprehensive exposition of the new techniques is Moore's *Statistical Indicators*.<sup>2</sup> There presented are the preliminary estimates of forecasting efforts based on years of painstaking and detailed analysis of the business cycle.

The studies of the National Bureau are, of course, far more sophisticated and scientific than earlier work in the field. There is none of that fixed-periodicity, invariable-pattern type of projection so characteristic of older business cycle analysis. Among the recent works in this older tradition is Dewey and Dakin, *Cycles: The Science of Prediction*,<sup>3</sup> which is quickly becoming an intellectual curiosity of the postwar period. Nor is there any seeking for that simple, perfect barometer which will always indicate changes sufficiently in advance to make profitable action possible. These ghosts of the past are, in fact, being laid to final rest by the National Bureau's research.

We have here, in other words, the rare spectacle of an approach being revived and discredited at the same time. The former, because stability of relations can apparently be discovered in statistical compilations of indicators selected from a large number of historical series. The latter, because analyses of the entire collection of statistical series available reveal marked variation and lack of any permanent reliability in the behavior of most indicators.

The essence of Moore's approach is to select not one but a substantial number of statistical series that have in the past typically led the movements of business at the cyclical turning points. These are compared for purposes of confirmation with the behavior of other series that lag behind or closely conform to the general turns. Many series were examined, and more than two-thirds of them were rejected because their past behavior did not consistently display any regular pattern in relation to the general cycle. Thus from among all the available series were selected those that appear to be dependable on the basis of past action; and the turns in these "dependable" series are observed as indicators of approaching turns in general business.

The process of selection of these indicators is indiscriminate in the sense that no logical appraisal of the reasons for observed differences in timing is attempted. Indeed, it has sometimes been held in earlier works that knowledge of causal relations is irrelevant or

<sup>2</sup> Geoffrey H. Moore, *Statistical Indicators of Cyclical Revivals and Recessions*, Occasional Paper 31 (National Bureau of Economic Research, 1950).

<sup>3</sup> E. R. Dewey and E. F. Dakin, *Cycles: The Science of Prediction* (Henry Holt, 1947).

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even that knowledge of what the series represent is irrelevant, because the only important fact is that the observed behavior of any indicator in relation to the cycle is consistent. Such indiscriminate methods were used to provide a basis for the artificial methods, cute tricks, and queer devices of the past. Ordinarily, they have had to be abandoned, because the whole history of invariant sequences is that at some time or other they explode in the user's face. Since so much has been done to show that purely statistical constructs are without meaning, is not a degree of skepticism justified in this instance also?

At the moment I am not convinced that a reassuring answer to this question can be given. It is true that similar conditions tend to call forth similar economic responses—a fact that lies at the heart of the business cycle and at the heart of any effort to forecast. In each specific situation, however, there are not only similarities but also differences. What is needed is a way of telling when the differences are becoming more important than the similarities, when new variants of behavior may be overriding the established patterns.

It is true also that the leading or lagging tendencies exhibited by the selected indicators do show considerable stability over a number of cycles, creating a substantial probability that at least some of the relationships are governed by more than pure chance. If one looks beneath the surface, reasons can be found to show that this is actually the case. A fairly large number of indicators involve causal relations that justify their use in an index designed to anticipate the turns of the cycle. For example, one of the most dependable of the "leading" series is construction contracts. Here is a series which by the nature of its relation to an important segment of economic activity is entirely suitable for inclusion in an index of leaders.

In any forecasting system the construction-contracts series should provide one of our best measures of future activity. But to use such a measure merely as a statistical indicator only serves to divert attention from its essential significance.

Not all the series included in Moore's list can be considered as having this character. Any list of indicators selected without regard to their causal significance must of necessity be partial. It will be partial in the sense that it covers only part of the significant sources of change in the economy; among the important types of activity not represented in Moore's indexes are government expenditures, inventories, and exports. It will be partial also in the sense that it will serve some situations well and others badly. When there is

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coincidence between the forces moving the economy and the series included in the index, the index may behave in a "normal" manner—defining normal in terms of the observed past sequences. When those forces are quite different—and new situations are always emerging—there is no reason to think that the index will provide the basis for a sound forecast.

As I see it, the basic deficiency of this approach is that it contributes so little to an understanding of the current situation, which is almost the first requisite of a good forecast. When the index provides an indication that something is happening, or is about to happen, it does not tell why, or how far the move is likely to go. When, for example, the majority of the indicators have switched from the expanding to the declining side, the economy may well be at the point of moving down. But there is nothing in this fact to indicate the probable magnitude or duration of the decline—whether it will be a short, sharp decline as in 1921 and 1938, a moderate recession as in 1927 and 1949, or a severe depression as in 1929-1933. These quantitative differences are clearly important elements of a forecast. Unless some picture of the quantitative effect of the various factors influencing the current situation can be put together as a starting point, there will be no basis for projecting the rapidity or extent of the next movement.

There are also practical difficulties in the application of this method. The mere inclusion of a large number of indicators, which itself affords a means of protection because no situation is likely to invalidate all of them at once, multiplies the burden of statistical review and adjustment. The difficulties of identifying turning points in many series at the time they occur are often nearly insurmountable. Without hindsight it is frequently impossible, because of the magnitude of erratic short-term fluctuations, to tell whether a series is still rising or has begun a cyclical decline. In the analysis of movements of stock prices, for example, one of the difficulties of using such a seemingly simple device as the Dow Theory is that of identifying the secondary movements; and of the current leading indicators presented in Moore's last chapter, stock prices are one of the better-behaved series.

Moore and his associates at the National Bureau have thoroughly covered the ground and are well aware of all these difficulties. In fact they specifically bring them to the reader's attention. When we reach the last chapter of Moore's study, we are disappointed to find so much of the promise of what has gone before written off. Such

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an acknowledgment of shortcomings is praiseworthy as a sign of scientific faith, but it is of little help to the person who has to make a forecast whether he wants to or not. What is needed is a convincing demonstration that these statistical indicators can be used as a practical aid to realistic forecasting in an emerging situation.

To deal exclusively with the practical aspects of the subject would, however, be begging the broader question. These statistical indicators are but one phase of a more comprehensive approach to the problem of business fluctuations—the business cycle approach.

In commenting on this broader question, let me make clear that I am attempting to appraise business cycle research only from a forecasting point of view. That research has theoretical and statistical values which are well known and in no wise being questioned here. From this narrower viewpoint I have already indicated that the National Bureau's work is far too advanced and well informed to be caught in the older fallacy of a search for invariant sequences. Yet something of the same character enters into the mere process of setting up basic reference dates of cyclical decline and advance; for the reference cycles themselves tend to become devices for looking away from things that may be important.

Once a set of cycles is established as a frame of reference, those cycles tend to dominate the orientation of further research. What happens in relation to them becomes part of what makes business move. What happens in such a way that it cannot be fitted into the cyclical pattern is an exception, an erratic element, something of separate interest perhaps, but not necessarily a thing to be taken into account at the moment.

In contrast to this, there is an alternative approach which seeks to get at the causal relations behind these business swings, to identify and measure the factors responsible for each decline and recovery. This approach, which I shall refer to as the forecasting approach, starts with the assumption that there are no business cycles as such, that there are merely various forces operating on the economy, which may be recognized as partly independent and partly inter-related, and that more general movements develop in the direction which the interaction of these forces dictates. Any conjuncture of forces that is sufficient to get a general movement started will in turn bring other forces into play. In each case the outcome depends, not upon any fixed relationships, magnitudes, or sequences in timing, but upon the specific nature of the forces that become operative in the specific circumstances.

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This does not in any way deny the existence of business cycles. On the contrary, it stipulates that the interrelations among some of the factors are such as to make business cycles almost inevitable. Thus whenever a decline occurs, inventory liquidation will set in and lend force to the other factors responsible for initiating the downturn; and whenever a movement to liquidate inventories sets off a decline, the decline gains support from such other factors as reduction in capital expenditures and liquidation of consumer credit. Viewed in this way, inventories are not thought of as lagging one cycle and leading another. In the one case their movement is responsive to changes initiated by other factors. In the other it is the decisive factor initiating the whole movement.

Many of these interrelated movements tend toward some regularity in timing and sequence. An inventory movement, beginning at a time when inventories are not seriously out of line and remaining virtually unsupported by other factors, is likely to work itself out in half a year or so, as in 1949. More typically, it gains support from other factors, prolonging the movement perhaps two or three times, and in some instances a much longer time, as in the great decline from 1929 to 1933. After such a movement has run its course there is quite likely to be a reversal rather than just a cessation of the original movement, because the mere termination of accumulation or liquidation itself involves an adjustment of production and incomes in the opposite direction.

In the forecasting approach any number of such "specific cycles" may be recognized. Many of the strategic factors have characteristics that make for regularity in timing, these characteristics being largely tied up with the length of their periods of production and consumption. In practically any line of industry we have the resources to raise production above long-run rates of consumption, and then it is only a matter of time until surpluses accumulate, bringing on the reversal that culminates in a compensating period of liquidation.

In this respect many of the other "specific cycles" are not unlike the "inventory cycle." In the case of inventories we tend to focus attention on the stock of goods held and ignore the process of production by which it is accumulated. In the case of houses or capital equipment we focus attention on the activity of production and ignore the stock of goods held. Both perspectives are necessary to a correct viewing of the swings between boom and depression.

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It may be suggested that the short cycle of several years' duration is essentially the "inventory cycle" and that the long cycle of some fifteen to twenty years' duration is essentially the "construction cycle," with both types of movement typically supported by related shifts in durable goods investment on the part of both producers and consumers. The general level of activity around which the shorter cyclical swings occur is thus primarily determined by the rate of long-run investment established by the construction cycle. In booms construction activity sustains the economy against short-term depressing fluctuations; in depressions it provides little aid to incipient revivals. Construction and related activities that predominate in the long cycle exhibit short-term movements that are much more limited than the swings of such short-run factors as inventories and government expenditures. Hence the turning points of all cycles tend to be determined by the short-run factors, and calling the turns over the short term is primarily a matter of forecasting inventory movements, government programs, and other factors that may be subject to decisive short-term variations, such as net foreign investment. To this extent the emphasis put upon capital expenditures as a factor in short-term forecasting is almost wholly misplaced.

Everything that can be explained as a phase of the business cycle fits perfectly into forecasting theory; but many things that cannot be cast into the cyclical mold also form integral parts of the forecasting approach. Here analysis of each situation is directed toward the specifics of that situation, toward determining the decisive forces at play and explaining how the manner of operation and effectiveness of these forces differ from those in other situations. From this point of view it seems almost useless to establish reference dates and compute average patterns of change within such dates. It is worse than useless, it is destructive of good analysis, to leave out of account so important a factor as government programs because its behavior does not fit into established cyclical patterns.

The essence of the forecasting approach is that propositions are always put to the test of actual events, so that their validity or falsity is soon revealed. Mistakes in forecasting will undoubtedly continue to be made; but such mistakes represent the crucial testing of hypotheses about what makes the economy move. It is my firm conviction that greater understanding of business fluctuations will develop from this process than from any detailed statistical analysis of recorded business cycle data.

## 2. ECONOMETRIC MODELS

Perhaps the youngest of the forecasting methods is the use of econometric models. These models are systems of simultaneous equations adapted for forecasting purposes.

The word "model" is often loosely used. There are many kinds of models, including other types of econometric models and models that have little if any connection with econometrics. "Model-building," as generally conceived, is basically a separate and distinct process from forecasting. There may, in fact, be not the slightest element of forecasting in model-building.

Model-building suggests the construction of a model which, like the model airplane in the wind tunnel, could be used to test the working of the actuality itself. In economics, however, no such exact replica is possible. Most model-building consists, therefore, of attempting a partial representation of economic conditions on the basis of certain stated assumptions. The model represents the situation as it would be if the assumptions were fulfilled. It can have no more validity than the assumptions underlying it.

This is quite different from forecasting. In forecasting the basic objective is to avoid any assumption that might predetermine the result. It is simply a matter of determining where we are going from where we are. In essence it attempts to do this without limitation as to unforeseen contingencies and developments. In actual practice limitations frequently cannot be avoided, and a forecast has to be made conditional in order, for example, to abstract the possibility of all-out war. Sometimes actual forecasts are so hedged as to be almost meaningless. But conceptually, at least, a forecast deals with reality in all its complexity and variability.

Pointing up this distinction so sharply is not intended in any way to belittle the value of model-building. The procedure of setting up models and tracing their consequences is extremely useful as an aid to understanding certain kinds of situations and processes. It is rigorous in defining the relationships that are thought to exist between the various factors under consideration and it thus represents a valuable tool of theoretical research. It may be decisive in policy discussions by revealing possible consequences if certain proposed courses of action are adopted. All this, however, has little to do with short-term forecasting.

In the case of long-range forecasting the distinction tends to fade, because such forecasts so often have to assume certain conditions

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or abstract the possibility of certain kinds of disturbance. Many so-called long-range forecasts are, in effect, not forecasts at all, but rather "models" based on presumed conditions more or less explicitly stated by the author. For example, it might be "forecast" that at full employment in some future year, production will reach such and such a level—completely dodging the key question for the forecaster: Will full employment prevail in that year?

Similarly, some of the so-called models used in forecasting are no more than statements of the way in which certain variables may be estimated from other variables, and as such are perfectly proper tools for the forecaster's use. This is true, however, not because such statements compose a model, but because they represent tools of analysis which may be of value apart from the model as well as in it. Some of them may indeed be valuable tools; but none of them, I am sure, will be so fool-proof that it can continue to be used without question over a period of time.

There is no doubt some justification for efforts to escape the vagaries of human judgment. All of us are aware of its frailties. But in setting up models we do not escape judgments; we merely transfer them to other points in the process of working out a solution. It is a very relevant and important judgment to decide that such and such a mathematical function represents a relationship in the sense appropriate to a solution of the problem at hand. It makes no difference that the econometrician does not assume that the volume of capital expenditures will reach a certain level at a specified time, when such an assumption is in effect built into his system in an equation that will automatically bring it to that level under the conditions projected at that time. The rigor of mathematical language may even force greater rigidity into the solution for such a variable than the econometrician would be willing to advocate on his judgment of the facts. The effort to eliminate judgment merely takes on the character of a kind of quest for certainty—a certainty that can never be found in practical affairs.

Econometric model-building is in this respect similar to economic theorizing in general, where the main effort has been to establish principles whose validity, because of the nature of the principles, is not dependent upon the exigencies and confusions of actual affairs. Study of the theory of equilibrium may be essential to the progress of the student who begins by knowing almost nothing of the workings of the economy, but the whole concept is one that may well be ignored in analyzing actual short-term economic developments. Sim-



ilarly, the notion of a distributive system that allocates returns to various factors in proportion to their marginal products can contribute little to a portrayal of what lies ahead.

The forecaster must inevitably operate in a world of reality—a world of social beings, of politics and sentiment as much as of profits. Many of the factors that affect economic conditions in the actual world are not readily subject to mathematical treatment. The political victory that will sway the balance of power sufficiently to change the distribution of income in favor of farm receipts, or wages, or profits, may be the result of conditions almost wholly unrelated to the economic issues it affects.

But perhaps all this is laboring the obvious. To come more directly to the point, let us ask just what is needed to set up a system of equations that will produce workable answers. As I see it, the conditions are four: (1) enough variables to represent all the important forces affecting the economy, including all those which may at some time or other become important; (2) a set of equations correctly expressing the relationships between the variables; (3) good data, not necessarily without error, but with errors that are nonsystematic and reduced to tolerable proportions; (4) a basis for solving the equations as applied to the period to be forecast, involving a basis for estimating at least some variables as of that period.

Can these conditions be said to be satisfactorily fulfilled for practical forecasting purposes?

1. As for the number of variables to be included, it would seem that we should be able to agree on a list that is not unlimited, but is nevertheless sufficiently comprehensive to enable us to move ahead. However, it may be pointed out that the requirement for completeness is much more severe when a mathematical solution is to be made than when some simpler alternative approach is being followed. Things that could be taken into account by rather simple adjustments and allowances in an alternative approach must be explicitly represented in a mathematical equation. At best we shall have to deal with a substantial number of variables—so many that at times a solution may be impractical by reason of the sheer burden of work alone.

2. The determination of appropriate functions for use in these models is still in the experimental stage. Conceptually, it is possible to take account of almost anything in such functions. There could be dynamic models in which the parameters vary through time, or

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even models in which the parameters vary as functions of other variables. In practice, however, we are usually limited to relatively simple relationships, using fixed parameters. Testing such models is difficult, and comparing functions to select the "best" is almost impossible.

The standard errors of estimate, where they have been computed in the past, are large—in fact so large that the probable range of a forecast of such a variable as gross national product becomes so wide as to make the forecast meaningless to most people who might have a use for it. This does not mean, of course, that the probable error of a forecast will be any less simply by reason of not being computed. Other bases must be relied on in judging the relative merits of results produced by the various techniques; and for this very reason the computation of standard errors tends to lack practical value.

Any lack of correspondence between goodness of fit and forecasting accuracy merely emphasizes the point. As part of his *Study of Aggregate Consumption Functions*,<sup>4</sup> Robert Ferber made comparisons of the actual consumers' expenditures of the postwar years, 1947-1950, with the estimates of consumers' expenditures obtained from a large number of functions that had previously been developed by research in this field. Among the results of his study was a finding that there was little relation between the coefficient of determination of a function and its predictive error. Indeed, considering all the functions together, there was a slight positive correlation between goodness of fit and forecasting error; and even segregating the various functions by type and by period of observation brought little decline in forecasting error with improved goodness of fit.

Furthermore, the available evidence points overwhelmingly to the fact that there are few invariant functions in economic behavior. The wide postwar swings in personal savings give warning of the important variations that can occur in the consumption function alone. Although this is considered to be one of the more stable economic relationships, the variations in actual results are so large as to make it impossible to generalize the results of actual comparisons like those made by Ferber as a final test of the validity of the various functions in other periods. The specific postwar years used in the test were abnormal in many ways; and the recent new "disturbances" make it unlikely that we shall be able to "prove" the validity of any of the various consumption functions for many years to come.

<sup>4</sup> Technical Paper 8 (National Bureau of Economic Research, 1953).

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Again, recent emphasis is being placed on private capital expenditures as a determinant of the level of economic activity, and models like that constructed by Hicks<sup>5</sup> seem to offer improved explanations of the cyclical changes in capital outlays. The essence of this approach lies in the shifting of capital-demand schedules at critical points in the cycle. Yet it may be doubted that even a series of cyclically shifting functions can meet the need. To meet an increase in orders from customers, producers may, when operating near standard one-shift capacity, adopt various expedients. They may (a) increase the number of employees and rearrange work assignments to speed up production; (b) increase working hours, operate extra shifts in the case of bottleneck equipment, or even put the entire plant on extra shifts; (c) install additional bottleneck equipment or efficiency mechanisms, such as automatic control devices or materials-handling systems, to expedite the flow of work through the plant; (d) undertake major remodeling of facilities, increasing both capacity and operating efficiency; or (e) build new plant. There are obviously great variations in the investment and time required to make each of these alternatives effective. Which alternative will be adopted depends upon the particular circumstances existing at the time, so that the volume of capital expenditures called forth in any particular situation may vary widely from that derived by means of any preconceived functional relationship.

3. The problem of data is one of the most serious that currently has to be faced. The kind of back data needed to establish sound relationships is largely nonexistent. Even the current data leave more than a little to be desired. Recently, in attempting to reconstruct a past forecast for illustrative purposes in terms of revised data, I was struck by the fact that revisions in the data were in many cases larger than the errors of forecast. I relate this without implying any criticism of the organizations compiling relevant data; on the contrary, I have only profound respect for those who recognize the importance of devoting themselves to this essential part of our common task.

In the process of simultaneous solution the effects of errors in data, like those of inappropriate equations, carry all through the solution and often affect results in curious and unpredictable ways. It is all very well to say that we are improving our data all the time and that each year adds to the historical record. How much it adds

<sup>5</sup> J. R. Hicks, *Contribution to the Theory of the Trade Cycle* (Oxford University Press, 1950).

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is, of course, the important question; and again using the consumption function as an illustration, the entire record of the postwar period has added little that would enable us to choose from among the various formulas supported by one analyst or another. People who want forecasts now, for the year or so ahead, cannot afford to wait for the unfolding of historical records.

4. The conditions for a solution of the equations are likely to be of critical importance. Some models rely on the relationships themselves to project the key variables, as by difference procedures, rate-of-change projections, or lead-lag relationships. These, however, can hardly be considered satisfactory for more than brief intervals at best. Most variables are only partly determined by what has gone before, or even by what is currently going on. Most have some element of autonomy in them, and projecting them by a fixed formula is destructive of the whole principle of autonomy in strategic variables. There is nothing in the economic relationships of today, for example, that will enable a sound projection of the important government sector.

It is necessary, therefore, to make forward estimates of at least some of the variables by other methods; and the forecast as a whole can have no more validity than the specific estimates of the variables selected for independent projection. Making such estimates is the critical part of the whole forecasting process; and unless there is a technique for working back from the final solution to the initial projections of those variables, to test their consistency with each other and with the whole, the entire forecast rests on a very shaky foundation.

In short, none of the conditions needed for a satisfactory utilization of the econometric approach is sufficiently fulfilled to make it a satisfactory working tool. The attempt to use mathematical procedures rigorously introduces too many inflexibilities into the forecasting process; and these rigidities enhance the possibility that the model will misbehave and produce wholly unrealistic forecasting results, at least in some of the significant variables. The procedure is for the time being, and perhaps will remain indefinitely, an impractical approach to the forecasting problem. In presenting this pessimistic point of view, I recognize that it is a question on which there may be reasonable grounds for difference of opinion.

### 3. ANALYSIS OF NATIONAL INCOME AND PRODUCT DATA

As the review proceeds, the disadvantages of relying on any one

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method become increasingly evident. I have, indeed, placed more emphasis on the disadvantages than on the techniques themselves or their advantages. Forecasting is not an easy task, and it is almost unavoidable that some difficulties should be encountered in the use of any method.

What seems to be needed is a means of utilizing the best features of the various methods while holding the disadvantages inherent in their use to a minimum. We have to take into account the valid information provided by current surveys. We want to draw upon the interchange of views and ideas among a group of experts as a source of fruitful hypotheses. We want to gain what understanding we can of the current phase of the business cycle from all available statistical indicators. We require a framework of analysis and tested methods of drawing conclusions from quantitative data, like those being developed by econometric research. Throughout, we must rely on the best judgment we can muster to adapt what we know or discover to the practical necessities of the moment.

We come nearest to this, it seems to me, in a kind of flexible statistical analysis carried through in terms of the gross national product and national income data. In our enthusiasm for the newer, more powerful techniques, we sometimes tend to lose sight of the fact that the older techniques of extrapolation, projection of trends and cyclical patterns, and correlation can still produce good results for the practical statistician.

The term "flexible," as used in connection with techniques for obtaining future estimates, implies that throughout the process of analysis all aspects of statistical technique and all results arrived at are subjected to scrutiny and critically evaluated before they are accepted as part of the forecast. The experienced analyst knows at a glance that certain results, or even certain reported data, must be in error. He has at his disposal many sources of information and an established body of knowledge against which he may appraise the new. What does not fit in must be checked—with the result that ironing out a small inconsistency sometimes becomes a research project in itself. It is essentially a pragmatic process. The tests of each step are, Does it work? and Is there reason to believe it will continue to work?

One aspect of this flexible approach is a preference for the simpler methods. The straight line is usually taken to represent the shortest path to a good forecast, though evidence of nonlinearity will not be ignored. Simple correlations are also preferred, but never to the

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point of overlooking the primary objective of getting a good relationship. Graphic methods of curve fitting and correlation are frequently used, not only because they give equally valid results, but because they facilitate the making of necessary adjustments. These are, of course, preferences that derive in part from personal experience. The sum total of my own experience seems to indicate what might be stated as a general rule: The more a function is complicated by additional variables or by nonlinear relationships, the surer it is to make a good fit with past data, and the surer it is to go wrong at some time in the future.

Another aspect of the flexible approach consists in adjusting results whenever there is reason to believe that the computed values obtained from the statistical relationships would not in themselves be satisfactory. In these cases we depart from the relationship in the same way that the marksman aims off the target in order to hit the bull's eye in a cross-wind. The necessity for such adjustments has long been recognized by practical statisticians.

Frequently, the problem appears in the form of a deviation from a well-established statistical relationship, based on many years of historical data, that has been used with good results for some time. The question is, What is the significance of this development? Is it merely a temporary deviation of no further significance, or must the validity of the whole relationship be questioned? I shall not attempt to answer this question here, except to indicate that I do not think there is any final answer. Each case must be treated as a separate subject of research, to discover the causes of the deviation and to determine as far as possible whether those causes will carry future results further away from or back into line with past experience.

What is concealed in a straight comparison of computed values and actual results, as in Ferber's study of consumption functions, is that good results can be obtained from most of the functions tested if suitable adjustments or corrections are applied to the initial computations. In both of my articles dealing with this subject I used such procedures in making forecasts of consumer expenditures and effected distinct improvements in the results.<sup>6</sup> It might even be said that unless some such procedure is used, consistent results cannot be obtained from any relationship.

<sup>6</sup> "Consumers' Expenditures in War and Transition," *Review of Economic Statistics*, August 1946; and "No Bust in 1947," mimeographed (Department of Commerce, March 1947), the relevant portion of which was republished in *Barron's*, April 21, 1947, under the title "Consumer Spending Still Going Up."

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The adjustments made cannot be purely arbitrary, of course. Anyone who wants a purely arbitrary result need not waste time with statistical procedures. But when there is a logical basis for doing so, a relationship may be modified to take account of any special factors known to be operating toward an abnormal result.

All these rules of statistical procedure, including the liberties taken with them, it may be pointed out, have been known for some time. Why, then, should such statistical analysis be considered an innovation in forecasting? The answer to this lies in two comparatively recent developments. The first is the publication of the gross national product and national income data. The second is the development of an improved theoretical framework for the analysis of business fluctuations, beginning with the publication of Keynes' *General Theory*. These developments are, of course, interrelated.

The national income and product data have all the characteristics of good statistical data. They are the most comprehensive measures of economic activity; they are consistent in concept and comparable through time; and they may be related, with suitable adjustment, to almost any other economic measures. Like everything else in this field, they fall short of being perfect, but I shall ignore their weaknesses at this time, for their statistical qualities are less important than their analytical advantages. These advantages arise in part from their own internal relationships and in part from the fact that they are constructed from the standpoint of the goals of human action.

The basic approach I have mentioned proceeds through an analysis of the forces making for economic change. These forces are, of course, almost innumerable; they include forces of nature, such as weather, and group actions, such as war, not ordinarily regarded as economic. Basically, however, economic activity is expressed in the patterns of human behavior that represent what people do to achieve their goals, to meet their needs, or to protect themselves against possible adverse developments. These patterns are defined most clearly in end results rather than in intermediate stages or processes. In economic terms the end results are represented by the goods and services flowing to final users. People want the goods and services that will provide the kind of living they desire; and they strive for a surplus of income to save as a measure of future security. Business wants the facilities that will enable it to function effectively. Government decides upon programs it will carry out in the light of the circumstances with which it is faced. Foreigners cannot

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realize certain objectives without our exports. These are precisely the things measured in the national product and income data.

Other statistical series used to measure overall activity include items appearing in all the various stages of production. Indexes of production include various raw materials and components; employment data also include work in those earlier stages; and bank debits not only fail to separate payments for such commodities or labor from those for end products, but are powerfully affected by financial transactions. Nobody wants or can use a ton of steel in ingot form. Consumers, business, government—all have very definite needs for the products into which it may be fabricated. Those needs are the driving force of the economy. Except for accumulation of goods in the form of inventories, to which I shall return shortly, they are the basic determinants of economic activity. Being cast in those terms gives the gross national product data special advantages as a starting point for the forecaster.

The national income and product data also fit nicely into the theoretical framework that has been developed as a basis for analysis. The new structure of theory rests on a firm foundation and is being expanded and improved year by year. In this framework certain factors are identified as strategic and others as mainly derivative. Among the former are plant and equipment expenditures, housing, net foreign investment, government expenditures, and all the nonconsumption expenditures that operate with leverage effect to expand incomes throughout the economy. Among the derivative factors are savings, retained profits, taxes, and the bulk of all the leakages from the income stream that have to be balanced by nonconsumption expenditures. As I have already indicated, there are many exceptions to these generalities, so that judgment is needed to revise and correct the relationships in the specific circumstances in which a forecast is being made. Yet, by and large, there is sufficient basis for most of the relationships so that this general technique of analysis provides a sound approach for the forecaster.

This approach begins with provisional forecasts of the strategic factors, in terms of the specific considerations appropriate to each, and works through the various succeeding stages to derive the complete income and product accounts. It is a sound approach because it directs the forecaster's attention toward both the important forces affecting activity and the derivative changes that the operation of those forces will call forth.

The solution is arrived at through a series of approximations. One



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of the most important aspects of this process of working through successive approximations lies in the fact that it reopens initial forecasts of the various factors for reflexive adjustment. Once a result is obtained the validity of all the initial judgments and computations can be checked for consistency and corrected before the next approximation is taken. Most important of the checks for consistency is the circular check on consumer expenditures as gross national product is translated into income and income into expenditures. Then there are the numerous checks of segments with the total and with each other. I shall not attempt to go into the details of this process here. What it requires is an understanding of how each factor, and particularly how each of the strategic income-producing factors, behaves in relation to other factors in the kind of overall situation represented by the expected changes in the total.

The procedure as a whole is flexible enough to allow maximum scope for imagination and judgment and yet ties the forecaster down to the necessity for balancing the overall accounts and observing all the known interrelationships. The end result is a forecast internally consistent and incorporating all that is known about the special forces affecting each important segment.

It may be pointed out that numerous errors have been made by forecasters using a procedure similar in many ways to that described. The facts on this point are too well known for argument. The last thing I should want to be accused of is claiming infallibility for any forecasting method. As I look back upon the postwar years it seems to me that those errors may be ascribed, basically, to two causes. The first consisted of attempts to make the forecasts subservient to policy considerations. The second consisted of too much rigidity in carrying over the relationships of an earlier period into a changed situation.

One of the primary requisites of good forecasting is an impartial attitude of mind. If bias is introduced, the forecast will prove incorrect. Attempting to make a forecast serve the ends of policy is an obvious form of such bias. Attempting to use a forecast to cover up past errors is similarly destructive. In every case the effort must be to see the situation as it is, to put aside the mistakes of the past and the hopes for the future, and let the analysis go where the logic of the facts dictates.

The forecasters of the postwar period approached the problem with all confidence in their new techniques of analysis. They felt they held the keys to a true understanding of economic processes,

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and they proceeded vigorously, if not too intelligently, to put those techniques to use without regard to the many elements in the situation that were new and essentially different.

Partly as a result of these early failings, there has been considerable recent criticism of the procedure itself. Burns, in his "Keynesian Thinking,"<sup>7</sup> attributes the basic difficulty to the aggregative approach and suggests that it is necessary to break down the aggregates in order to realize results. This position has evident merit in some particulars, but nothing could be of greater disservice to the forecaster than to generalize it with respect to all aspects of his work.

It is true, of course, that dealing with the economy in terms of broad measures involves overlooking a considerable amount of detail. This disregard of detail is in fact a necessity for the forecaster; for he must reduce his job to manageable proportions by sifting out the important facts about the current situation from the welter of information that is constantly being forced on his attention. To overlook important details may lead him into error. To attempt to deal with too much detail will certainly bog him down in such a mass of irrelevancies and errors as to make his task well-nigh impossible.

To illustrate, consider Abramovitz' work on inventories. According to Burns, "Abramovitz' great contribution consists in demonstrating that inventories are not a homogeneous mass, that their behavior does not lend itself to aggregative analysis."<sup>8</sup> Abramovitz himself says, ". . . no simple, general explanation of inventory fluctuations is valid. An adequate theory of inventory cycles must explain the disparate behavior of the several categories of stocks that move in significantly different ways."<sup>9</sup>

These statements are almost tantamount to saying that nothing can be said about the forest except by talking about the trees.

The fact is that practically nothing can be done in terms of specific types of inventories to project the inventory fluctuations which are most significant for overall forecasting. The initial fallacy in Abramovitz' position arises from the confining of his analysis to manufacturers' inventories. Basically, it makes little difference whether a given finished product is held by the manufacturer or by his distributor. Within manufacturing it makes little difference

<sup>7</sup> *Twenty-sixth Annual Report* (National Bureau of Economic Research, 1946).

<sup>8</sup> *Twenty-eighth Annual Report* (National Bureau of Economic Research, 1948), p. 15.

<sup>9</sup> Moses Abramovitz, *The Role of Inventories in Business Cycles*, Occasional Paper 26 (National Bureau of Economic Research, 1948), p. 21.

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whether steel is held by the steel mill as a finished product or by the auto manufacturer as a raw material. Much more important than the question of whose inventories are going up is the question of whether inventories in total are going up. If distributors are rapidly piling up stocks, they will soon pass the accumulation back to the manufacturer by cutting orders, and the piling up will continue until he reduces his production schedule. It is the movement of the aggregate and not the stage at which that movement is going on that is of primary importance.

The only sound approach to this problem that I have been able to discover flows from the definition of inventories. Inventories are goods produced but not sold to final users. Changes in inventories are determined basically by differences between rates of production and consumption—as indexes of production, consumption, and inventories that I compiled more than ten years ago reveal. The adjustment of production and consumption rates to each other, the relationship of total inventories to the rate of flow to consumers, and the probable changes in that rate of flow are the keys to inventory forecasting.

To put this another way: If we know the magnitude and the character of the current movement of inventories, how large aggregate inventories are in relation to the flow of goods into consumption, and what the implications of those facts are in relation to the movements of other factors, probable changes in the rate of inventory investment can be effectively projected. This applies to timing as well as to rates of accumulation or liquidation. Involuntary movements of inventories that occur because of unforeseen changes in demand and voluntary movements that are undertaken in expectation of a change in demand that does not materialize are bound to terminate quickly. Voluntary movements designed to bring inventories back into line or to adjust aggregate holdings to expected conditions affecting demand or costs will persist for a longer period, depending upon whether the changes in other factors prevent or facilitate the adjustment.

None of these movements are phenomena that can be analyzed in the detail of specific inventory series. This is a field in which errors of fragmentation easily become more important than those of aggregation. It is unfortunate that Abramovitz should have chosen to introduce this issue into the discussion. His work stands out otherwise as a fine piece of research and anyone who wants to understand what inventories are, how important they are in the business

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cycle, or how they behave in detail would be well advised to study his book.<sup>10</sup>

All this is not to say that aggregative analysis alone provides the basis for sound forecasting. Within the framework of an aggregative approach familiarity with detail can add greatly to the understanding that produces nicety of projection, both in magnitude and in timing. The necessity for working out preliminary forecasts of each strategic factor in terms of the specific forces and relationships significant for it has already been pointed out. As we move into the field of forecasting movements in particular commodities or industries, we encounter a number of situations where the specifics are all-important and the influence of the larger aggregates fades into insignificance. If there is any generalization that fits all cases, it is that the forecaster must apply himself in each without preconceptions, to deal with the problem at hand in the manner best calculated to produce a practical solution.

### *D. Summary*

To summarize briefly: The characteristics of good forecasting procedure are that it should be a form of quantitative analysis, that it should be flexible enough to permit wide scope for judgment and imagination, that it should draw on all available sources for information and ideas but accept only those that fit into the unfolding pattern of economic change. None of these characteristics is sufficient in itself. A mere system of quantitative analysis contributes little; judgment is needed, both in marshaling facts and in utilizing techniques of analysis; but without facts and a technique of analysis, no dependable judgment is possible.

As a process of developing the implications of the present for the future, forecasting is essentially continuous in nature, requiring modifications and adjustments with each change in the economic scene. The keys to a sound forecast are knowledge of the current situation in terms of what has made it what it is and knowledge of what reactions may be expected to the forces working to change it to something else.

To speak thus of a sound forecast does not necessarily imply that any forecast we may make will turn out to be correct. There are many reasons why we shall continue to make mistakes. But if we learn from our mistakes, forecasting will be instrumental in develop-

<sup>10</sup> Moses Abramovitz, *Inventories and Business Cycles* (National Bureau of Economic Research, 1950).

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ing the creative intelligence we need to deal effectively with the problems of an emerging future.

### C O M M E N T

ELMER C. BRATT, *Lehigh University*

I agree with Bassie that the knowledge of causal relations is far from irrelevant in choosing and analyzing business barometers. The way we should proceed may, however, not be entirely dependent upon our knowledge of aggregative causal relations. Considerable attention should be given to empirical uniformities, even if there is no presently available causal explanation of these uniformities in terms of overall aggregates. Such a procedure is desirable for several reasons, of which two seem particularly relevant. (1) Encouragement should be given to the discovery of empirical uniformities. After we find they exist we may find perfectly valid explanations for them, even though such uniformities might never have been guessed if known causal relations had been relied on to direct our empirical analysis. A good illustration is the positive correlation between price and quantity variation over the business cycle, which, when first discovered, was thought to demonstrate upward-sloping demand curves. (2) Our logical understanding of aggregative relationships has been unable to provide us with all the clues we need, probably because of the important part played by mutual causation in economic change. The leads which Moore finds in the percentage of series expanding are highly promising, but would not have been implied by *aggregative* logical relationships. The hypothesis on which Moore proceeds actually developed from long and careful consideration of the nature of the business cycle. Similar points can be made about my findings on amplitude variation over the business cycle.

Discoveries regarding relationships in economic change will sooner or later fit into our general understanding of logical forces operating in the overall economic aggregates, or they will necessarily be discarded. For the time being, there is no reason to believe that economic-change relationships are in any way inferior to those explained by theories in aggregative economics. The turnover on such aggregative ideas appears to be high. Our framework of thought will be widened by developing empirical relationships which were inspired by hypotheses on cyclical change, rather than, in all cases, beginning with aggregative logic.

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BENJAMIN CAPLAN, *Washington, D.C.*

What makes a good forecast? This is the problem with which Bassie deals. What emerges from his paper is that the determining factor is not the quality of the method used, but the quality of the forecaster. This is clear from his stress on flexibility and judgment, and from his generally eclectic attitude toward the techniques of forecasting. True, according to Bassie, there are better and worse methods of forecasting, but even the best technical method available is no better than the judgment of the forecaster. He personally prefers a "flexible statistical analysis carried through in terms of the gross national product and national income data."

Thus the difference is not so much between good and bad methods of forecasting as it is between good and bad forecasters. Hence forecasting becomes essentially an art where success depends to a major extent on the skill of the forecaster and his flair for feeling out the currently fundamental determinants of the trend of the economy. In a vital sense forecasters are born, not made, just as artists are, or tea-tasters, or any others whose proficiency depends upon innate propensities. This is what I conceive to be the implication of Bassie's discussion even though he probably did not intend to convey such a conclusion.

Such a proposition undoubtedly reflects the practical realities of the situation. But its import is most significant: we do not yet have reliable scientific forecasting, i.e. a reliable method invariant with respect to the forecaster. The conclusion is hardly surprising. We shall, as economists, continue to seek such a method because the prize is such a glittering one. But we have here a fundamental problem in epistemology: whether, as the mathematician Du Bois Raymond said in a similar connection, the problem is not that we do not know but that we shall never know ("non ignoramus sed ignorabimus").

But if forecasting depends to a major extent upon the skill of the forecaster, what is it that gives us confidence in the ability of the forecaster to forecast—his ability to make accurate forecasts, or his ability to analyze accurately the forces that led to the accurate forecast? The practical man, whether in business or in government, is generally not too much interested in the analytical underpinnings of the forecast. What concerns him much more is the accuracy of the forecast itself. The history of economic forecasting is littered with forecasts that turned out to be correct though based on what, in

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retrospect, were the wrong reasons. Indeed, much depends upon the psychological predispositions of the forecaster. Thus if we deal with the postwar period, it is sobering to reflect that an optimist who predicted that the economy would expand each year would have been wrong in only one year, 1949, and that would have been a rather minor error.<sup>1</sup> On the other hand, a pessimist, however highly trained in all the scientific techniques, would have had a sorry record indeed! How long must we wait to know whether the forecaster is using an improved technique or happens to be forecasting a trend that agrees with his psychological predisposition? Where such a psychological bias does not exist, there is frequently, on the other hand, the tendency for forecasters to forecast that which is happening. The major difficulty, as always, is to pick the important turning points.

We live in an open-end universe. If, as the philosopher Karl Popper said, the function of science is the refutation of hypotheses, then the function of economic change might be the refutation of forecasting methods. But at the same time, constituted as we are, we shall always seek to make rational forecasts if only because we wish to have some intelligent approach to the decision-making process.

In this connection it seems to me that Bassie's attempt to create a fundamental contrast between model-building and forecasting cannot stand up. In terms of scientific method they are both of the same kind. Both start from premises and work to conclusions. The difference is one of mood. The model says: *If* these conditions held, these others would follow. The forecast says: *Because* these conditions hold, these others will follow.

### REPLY BY THE AUTHOR

I am glad to have Caplan comment along these lines because he opens for discussion an important question that did not quite seem to belong to the original topic of my paper. As a teacher in the field of forecasting I have been concerned for some time with the question of how forecasters are made. I say "are made"—not "are born"—because I am convinced that the process is largely a function of training and experience, and particularly of the attitude with which experiences are received, rather than of any innate propensity or quality of mind.

<sup>1</sup> Written in 1950.

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The specific knowledge needed as the basis for an analysis of economic conditions can be obtained without too great an expenditure of effort; but it is evident that many who are well equipped with information and techniques cannot produce sound forecasts. Putting such knowledge to effective forecasting use is apparently difficult, particularly when the channels of communication are so cluttered with misinformation, misinterpretations, and misleading impressions.

There are obviously obstacles to be overcome before the individual gains the kind of judgment that will make him a good forecaster. In describing a scientific attitude of mind as an essential quality of such judgment, I referred to what is the essence of the scientific method in the social sciences—the application of impartial intelligence in the search for a solution to the problems encountered. Only through such an attitude—with its comparative freedom from preconceptions, prejudice, and bias—can error be avoided.

At some point in the process of personal development it becomes too late for the individual to acquire the scientific attitude. Many students have developed personal traits that make such an approach impossible long before they come to college. Others, even good students, lose the ability to pursue it somewhere along the way; they learn well, in an unquestioning way, and their teachers may never find out just how their minds really work, or where they ultimately end up. Only a limited group asserts its curiosity and insists on integrating all that it is taught, resolving conflicts in terms of impartial judgments of the relevant facts.

Most of this fits nicely, of course, into what Caplan has said. What I object to is any implication that there is something mystical, some purely subjective or unique quality about the judgment needed for forecasting that makes it the special endowment of the few. I firmly believe that it can be acquired through the ordinary process of learning by anyone who is open-minded enough—as we all start out being, surely, at birth.

What it all comes back to is a willingness to take things as they are and to accept the verdict of the facts regardless of where it leads. This does not mean that to be a good forecaster a person has to give up his own special interests, goals, or emotional attachments. On the contrary, it may be helpful to proceed with certain stated aims in mind, recognizing that conditions may not permit the realization of those aims. The process of observing their defeat may then become the basis for a sound education in forecasting. If defeat is



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accepted in this spirit, with the reasons for it clearly understood, and not just in a spirit of frustration, the next time a similar situation appears a better judgment of the probable outcome can be made in terms of the forces that are known to be operative. The effort must be not to give rein to, but to prevent being carried away by, one's own psychological predispositions.

Beyond this it is merely a matter of persistent effort and hard work. To make a comprehensive analysis of the whole economy may be a matter of hours for one who is exceptionally proficient and well informed. It is more likely to be a matter of days for anyone else. The easy way out is to make a guess on the basis of some hunch or impression—perhaps in accordance with the theory that you can't go wrong by following the crowd. The easy way, unfortunately, does not produce accurate results.

In short, the question of attitude seems to me to be all-important. That is why I take issue with Caplan's closing paragraph. Grammatically, the difference between forecasting and model-building may be only one of mood. Practically, it may be all the difference between working out the solution of a real problem and daydreaming about how things might be if only they were the way we thought of them. Insofar as model-building is a working tool of scientific analysis, I fully concur in its use. Only as it becomes a way of retreating from reality do I question its validity.

A. G. HART, *Columbia University*

"Somebody's always taking the joy out of life," and somehow I can't avoid the feeling that that is what Bassie is up to in this paper. Its general trend seems to be to pooh-pooh the view that anticipations matter and encourage economists to settle back into the curious universe of double talk in which we stress that economics is about the future—"bygones are forever bygones"—and yet never refer to the way people feel about the future.

To start with a point of argument, Bassie concedes the usefulness of observing business *plans* (together with orders, contracts, and the like) on the ground that people are actually trying to carry them out. But he rejects the usefulness of data on expectations as distinct from intentions.

I cannot regard this view as consistent. The papers for this Conference give us abundant data to show that plans are carried out in part, but that to a great degree people fail to do those things

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that (according to their plans) they ought to have done, and do those things that (according to their plans) they ought not to have done. We should not infer that there is no health in their plans. But we are bound to be interested in factors that lead to overfulfillment and underfulfillment of plans. Among these factors are surely the excessive optimism and pessimism of the estimates to which plans are attuned.

This discrepancy would stand out still more if we had reached the point where the plan data used in forecasting included a comprehensive sample of production schedules. These are inevitably linked with sales forecasts. In the manufacturing industries, sales forecasts may sometimes be based almost solely on orders, making orders the "actual" basis of production schedules. But in many branches of manufacturing—and almost everywhere in the merchandising world, where manufacturing orders largely originate—advance orders cover only a fraction of sales.

In a discussion of short-term forecasting it is odd to write off anticipations on the ground that resulting "deviations are limited in amount and in duration." The price and inventory policies of business in 1950-1951 (which in conjunction with bank credit gave us the inflation of 1950 and the breathing spell of 1951) are surely evidence of the potency of anticipations as a transmitter of exogenous disturbances. These swings were not so limited as to be beneath the dignity of the economic forecaster; and one should add that they seem likely to leave us with a permanently higher price level and a permanently sharper response of wages to price rises than if a different set of expectations—equally or more compatible with the "objective" data—had been set up by the "disturbance" of Korea.

Painful as it is to mention again the fiasco of the "8 to 10 million unemployed" forecast of 1945-1946, it is worth saying that adequate data on expectations could have safeguarded our projections. The forecast rested largely on the impression that massive reconversion unemployment would impair markets even for producers that needed no reconversion. If employers on the whole had *expected* mass unemployment, it might have been worth their while to lay off men in millions, reckoning to pick up a staff of better average quality when they were expanding again. But the event showed that employers took a different attitude. Apparently they feared a tight labor market. The termination (as of December 1945) of the excess profits tax made any loss through carrying men that might

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not be needed largely a loss for the Treasury, but any loss through being unable to recruit later largely a loss for the company. In any event gross layoffs at the time of reconversion were trifling, and the mechanism that was supposed to spread unemployment never got a chance to work. If we had collected evidence on employers' personnel plans and expectations about the labor market—or even used fragmentary data at hand—we should have been immune to the error of forecasting that caused the profession so much trouble.

### REPLY BY THE AUTHOR

I am amazed at the opening paragraph of Hart's comment—its concept of the "joy of living" and its implication that my views go back to some early version of neoclassical economics.

Hart correctly interprets my remarks as conveying a negative judgment of economic analysis carried out in terms of anticipations or other quasi-psychological theories of business fluctuations. My assignment was to put currently popular forecasting methods into perspective, and the best perspective I am able to muster reveals a considerable overemphasis on such theories, couched in precisely such terms as "the way people feel about the future."

Going on to the second paragraph, I find a compounding of confusion. The distinction I tried to make was between planned expenditures, projects, contracts, and orders—in short, measurable items that are themselves the early stages of activities by which things are done—and the vaguer forms of goals or intentions, which may be carried out if future conditions, developments, or circumstances warrant. Hart apparently insists on treating them as all alike; at the end of the paragraph the identification is complete. The sleight of hand with which this is accomplished is on a par with the notion that we are only indulging in parlor tricks for personal amusement, after all.

Hart cites the developments of 1950 and 1951 as illustrating the importance of anticipations. In this he is correct to the extent of interpreting the pattern of the post-Korean boom and letdown as deriving from the excesses of the initial upsurge and the subsequent reaction to those excesses. Anyone, as soon as the fighting began, could have predicted the upsurge, though not necessarily its full violence, whether or not he had explicit information about expectations. It was not so easy, however, to predict the subsequent decline. At the beginning of 1951 the prevailing view, which I believe was

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shared by Hart, was that inflation would continue through 1951. There was nothing in the current state of anticipations or expectations in January to suggest that the pressure would soon be off. The only way that it could have been predicted was by recognizing, as I stated, that such movements "are limited in amount and in duration." That it could be, and was, predicted on this basis may be verified by reading my testimony before the Joint Committee on the Economic Report, January 29, 1951 (pages 243-247).

Hart believes the failure of the forecasts of unemployment in 1946 could have been avoided by attention to data on expectations. It is clear, from the point of view of hindsight, that they could have been avoided on any number of bases. What is not clear is that expectations could have made a significant contribution along the lines indicated. Consider the reasons for the overstatement of unemployment:

1. The main source of unemployment was thought to be the discharge of millions of men from the armed forces for whom jobs would not be immediately available. Many of those who were discharged, however, were not counted as unemployed because they did not immediately enter the labor force. The group "not in the labor force" increased by some 5 million in the year following V-J Day. This was easily the largest source of error in the forecasts.

2. This problem was aggravated by the fact that the Census Bureau changed the methods of its survey of the labor force in July 1945. All the changes made were in the direction of minimizing unemployment, by classifying into other categories workers who might have been considered unemployed. The effects of these changes were not understood for some months; and most of the forecasters had made their predictions in terms of back data derived on the basis of definitions and methods that would have showed a larger volume of unemployment in 1946.

3. With the ending of the war, consumers loosened their purse strings, and for a year or so there was an abnormal outpouring of expenditures. Since durable goods were restricted in supply, these expenditures were concentrated in the nondurable field, that is, in industries with few or no reconversion problems. This high level of expenditures not only directly raised the level of production and employment in those industries, but stimulated accumulation of inventories at a rate that made a further substantial contribution to employment.

4. In the reconversion industries, manufacturers held employ-

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ment above levels that would have been strictly necessary, tolerating a corresponding decline in productivity and reducing unemployment correspondingly.

It is the last of these points that Hart chooses as the basis for his proposed means of avoiding the forecasting errors. Not only does he thus rest his case on an item that accounted for only a fraction of the error, but it is doubtful that any survey of personnel plans and labor expectations he could have made at the time would have revealed the essential character of the situation. It is easy to over-emphasize the intentional aspects of the overemployment in these industries. Most businessmen, like the economists, expected the labor supply to be ample and did not base their action on any fear of a labor shortage. What they were interested in was reconverting as quickly as possible, in order to beat competitors into the lush post-war market; and they were encouraged to incur a certain amount of waste for the sake of speed because of the large tax credits that had been built up during the war period.

I remain very skeptical about the possibilities of the survey approach at that time. Knowing what we do now, of course, we could direct attention to the specific points that would have helped us arrive at correct forecasts. In future situations we may again be at as much of a loss as we were then to devise and interpret surveys in such a way as to provide sound clues for a forecast.

I repeat that expectations cannot be considered self-fulfilling, and any forecasting procedure based on such a thesis is bound to produce frequent failures. The value of information about people's fears and inclinations lies in its contribution to the forecaster's knowledge of a complex situation, but such information is not necessarily best obtained by survey methods. Knowing the role of a "state of mind" in bringing about any given situation, and what is likely to happen to that state of mind as the situation changes, the forecaster will at times find himself with a sound hypothesis on which to predict the next move. This is particularly true on those occasions when psychological aberrations have temporarily carried the economy to an extreme not justified by the more fundamental factors—as in June 1949, when it was possible to predict that the decline was "already nearing an end." Identifying such situations provides one of the best opportunities for the alert forecaster.

### NOTE BY MR. HART

When operating as devil's advocate, one should be flattered to be

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told he is "taking the joy out of life." What I meant by this rather imprecise expression was that I thought Mr. Bassie, whose role at the meeting was to puncture complacency about the expectational approach, was succeeding, and forcing on us the pain of thought.

It is healthy (if not altogether a joy) to be reminded that expectational analysis led me, among others, into incorrect forecasts of renewed inflation for the latter part of 1951. On the other hand, there *were* expectational indicators of a possible leveling off. Better analysis of a fuller body of evidence might have provided a better safeguard than a hunch that "such movements are limited in amount and in duration."

Mr. Bassie is of course on unassailable ground in stressing factors that limit the self-fulfillment of expectations, and in stressing the difference between commitments to act, general intentions for future action, and still less definite impressions about the future environment. What bothers me about his approach is his tendency to score points off the people concerned with expectational evidence, and remain a debater to the last rather than seek for a synthesis.

CLARK WARBURTON, *Federal Deposit Insurance Corporation*

Four points in Bassie's paper seem to me to be especially pertinent to basic methodology in forecasting:

1. Psychological theory is an inversion of the primary causal sequence, for expectations more largely derive from objective conditions than produce them.

2. Statistical indicators do not tell why something is happening and provide little to indicate probable magnitudes of upward and downward movements.

3. There is need for testing hypotheses about what makes the economy move.

4. The estimates of national product and income and their components provide the most useful body of data available for analysis of basic economic conditions and their variation over time.

I should like to suggest that if we add to Bassie's conclusions the three elements listed below we might be well on our way toward providing a good foundation for forecasting. The three elements relate respectively to data, hypothesis, and institutional arrangements.

1. To the national income and product data add, first, a division of the changes in value of products between changes in quantity

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of output and changes in prices, and, second, information regarding the supply of money or circulating medium and its rate of use in purchasing the goods and services included in gross national product.

2. For a hypothesis select for careful testing the theory of the effects of disequilibrium in the quantity of money, which was developed a century ago along with the classical theory of equilibrium.

3. For institutional arrangements scrutinize carefully those which dominantly influence the circulating medium and are therefore responsible for turning points in the quantity of money.