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Chapter 25

Forecasting Short-Term Economic Change

Economic statisticians do not enjoy an untarnished reputation for accurate forecasting. We have managed, over the years, to come up with some memorable failures. While we have also had our share of successes, they are not as well remembered or as numerous as we should like. Recently, however, we have begun to pay more attention to the record, and a substantial body of evidence on forecasting performance has accumulated. In this chapter I propose to review this record, to try to arrive at a balanced appraisal, and to offer some suggestions for improvement.

To put the economic forecasters among our members in a properly humble mood, let me cite a few of the incidents that have cast doubt on our forecasting abilities. Back in 1929, few economists were pessimistic about the outlook, and fewer still were as pessimistic as would have been appropriate in view of the Depression that left the nation prostrate. One of the statistical casualties was a system of forecasting known as the Harvard *ABC* curves, developed in the 1920s by Warren Persons—a former president of this Association—and others, including a young man who was destined to become our next president, Ross Eckler. The three curves—*A* representing speculative activity—

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that is, stock prices; *B* measuring business activity; and *C* reflecting monetary ease or tightness—were used in formulating periodic reports on the business outlook. Historical studies of the pre-World War I period had shown that series of the *A* type tended to move early in the business cycle, the *B* type next, and the *C* type last, with the lagging upturns in the *C* series preceding downturns in the *A* type. The economic logic of the sequence was that tight money and high interest rates led to a decline in the prospects for business expansion and a drop in stock prices, whereupon businessmen cut back or postponed their commitments for further expansion, causing a recession in business activity. This in turn led to an easing of money and lower interest rates, which eventually improved business prospects, whereupon stock prices turned up, then business activity, and finally interest rates again in a new round.

Such, in brief, was the theory. In the depression of 1920–1921 the Harvard economists had some success in applying it. But in 1929 they maintained an optimistic view and failed to foresee either the downturn or the debacle. This failure dealt a death blow to the *ABC* curves as an influential forecasting scheme—a fate that was not altogether deserved, as I shall note.

A second prominent forecasting failure occurred in 1945, at the end of World War II. The expected curtailment of military spending and the return of soldiers to the civilian labor force led many economists to predict a serious postwar depression and mass unemployment. They failed to anticipate adequately the resilience of the private economy, the power of the pent-up demand for consumer goods, and the wherewithal provided by accumulated liquid assets. Certain econometric models were among the casualties. Based on relationships that fitted the prewar period, they were unable to cope with the transition from a war to a peacetime economy. The reputation of model builders suffered a setback from this failure. Nevertheless, they displayed a resilience that rivaled that of the economy itself and quickly shook off any sense of defeat, while striving to learn from the experience. Model building has since become a flourishing industry.

The next failure on my list is dated 1948–1949. Late in 1948 signs of a recession began to appear, but there were also signs of inflation. President Truman and his Council of Economic Advisers concentrated their attention on the inflationary threat. It failed to materialize, while the recession did. This forecasting failure found the government, during the first half of 1949, fighting the wrong war.

Another significant delay in recognizing the onset of recession occurred in 1957. The Federal Reserve Board failed to foresee the recession that began in midsummer, continued its policy of restraint by raising the discount rate in August, and began to take antirecessionary measures only in November. Again, as in 1949, the forecasters were concerned about inflation when the problem was recession. In 1965, on the other hand, a forecasting error of the opposite kind occurred. Administration economists failed to foresee the powerful inflationary pressures that were developing and urged policies that would continue to stimulate aggregate demand.

These are some of the better known exhibits from the economic forecaster's chamber of horrors—or should I say errors.[2] They reveal fallibility and the need to strive for better results, but they also demonstrate the importance of forecasting in guiding public policy. At the same time, these examples present a one-sided picture. We need a fairer and more systematic review, indeed, a statistical review, of forecasting performance.

Some work recently undertaken at the National Bureau of Economic Research provides a basis for such an appraisal. This is not, of course, the first such effort. The work of Cox [2] in the 1920s and the more recent studies of Okun [9], Stekler [11, 12], Suits [13], Theil [14, 15], and others have contributed to our knowledge of the validity of general economic forecasts. But the National Bureau study has produced a new and more extensive body of data of this sort, reflecting the great proliferation of forecasting activity in recent years.[1]

TURNING POINT FORECASTS

The National Bureau's collection of short-term general economic forecasts pertains largely to the period since World War II. It covers various economic aggregates such as gross national product and its major components, industrial production, employment, unemployment, and price levels, as well as business cycle turning points. The forecasts were made by economists in private business and financial firms, in government agencies, and in universities and research institutions. Some of the forecasts have been regularly published and widely disseminated; others were limited to private use. The record provides materials for analyses of the frequency and magnitude of error, of the factors contributing to error, and of the potential value of techniques for reducing error. It can also be used to analyze the

ways in which forecasts or expectations are formed and how the phenomenon of forecasting itself contributes both to the generation of business cycles and to their amelioration.

I shall refer to only a small part of this record, both for lack of time and because studies of it are still under way. First, let us consider what it tells about the forecasting and recognition of business cycle turning points. Suppose the objective of the forecaster is to foresee reversals in the direction of change in the annual level of gross national product—that is, to determine whether GNP will decline between this year and the next if it has been going up or whether it will rise if it has been going down. Such reversals in annual data are not frequent, but when they do occur, it is important to know about them since a downturn may mean the onset of a recession and an upturn the beginning of recovery.[2]

Victor Zarnowitz has assembled the record of 126 such forecasts made toward the end of the calendar year for the year ahead, mostly covering the period 1953 to 1963.[3] If the forecasts had accurately predicted the first official estimates of GNP made immediately after the year being forecast, there would have been forty-three turning point predictions or about one for every three forecasts. In fact, thirty-four turning point predictions were made or about one for every four forecasts. This is better, of course, than assuming that next year will always produce a turning point; it is better than the almost equally naive assumption that gross national product is a series of random numbers (in which case approximately eighty-four turning points would have been forecast); it is better than the slightly more sophisticated assumption that the change in GNP is random (which would have produced about sixty-three turning points); and of course it is better than assuming that no turning point would occur at all.[4] In short, the performance is clearly better than pure guesswork.

The forecasters not only had some success in predicting the total number of turning points, but were also fairly successful in judging when the turns would occur. Of the forty-three turning points that should have been forecast, thirty-two were predicted and only eleven were missed. Of the thirty-four forecasts that turning points would occur, only three were in error. Hence there were only fourteen turning point errors, which is a record of 89 percent accuracy in identifying years that would mark reversals in the movement of GNP. Table 25-1 gives these results in the form of a contingency table, together with some measures of association and a test of significance.[5]

Table 25-1. Forecasts of Turning Points in Gross National Product, Annual Data, 1947–1965.

1. Forecast Made Near End of Preceding Year

		Forecast: No TP	Forecast: TP	Total
Actual	No TP	80.5 (60.3)	2.5 (22.7)	83
	TP	11 (31.2)	32 (11.8)	43
	Total	91.5	34.5	126

Correct forecasts as percentage of
all forecast turning points (C_1) = 93 (34)
all actual turning points (C_2) = 74 (27)
all years covered (C_3) = 89 (57)
Cross product ration (α) = 93.7
Correlation coefficient (r) = 0.76
$\chi^2 = 72.8$
$p < 0.001$

2. Forecast Made Near Middle of Preceding Year

		Forecast: No TP	Forecast: TP	Total
Actual	No TP	6 (5.1)	2 (2.9)	8
	TP	3 (3.9)	3 (2.1)	6
	Total	9	5	14

$C_1 = 60$ (42)
$C_2 = 50$ (35)
$C_3 = 64$ (51)
$\alpha = 3.0$
$r = 0.26$
$\chi^2 = 0.95$
$p = 0.33$

Note: Forecasts or actual observations of no change are counted as half turning point and half no turning point. Figures in parentheses are the expected values on the assumption of independence and fixed marginal totals.

Source: Victor Zarnowitz, "The Record of Turning Point Forecasts of GNP and Other Major Aggregate," National Bureau of Economic Research (draft manuscript).

While economists can take heart from this performance, it must be recalled that these are forecasts for the ensuing year made in the late autumn or early winter. The turning point, which in annual data is conventionally dated at midyear, is then actually past. That is to say, if at the end of 1968 a GNP forecast showed 1969 to be lower than 1968, 1968 would be the turning point, but the year would be over. Late autumn is the season of the year when most forecasting is done; records of forecasts made earlier in the year are less abundant. The compilation of fourteen annual forecasts made near the middle of the year for the calendar year ahead presents a very different picture (panel 2 of Table 25-1). Six turning points should have been predicted and five were, but the timing was poor. Of the six turns that should have been forecast, three were not, and of the five predicted turns, two were false signals. The five turning point errors produce an accuracy score of only 64 percent, compared with 89 percent for the forecasts made near the end of the year. This is just barely better than guesswork.

Forecasts made in midyear must, of course, predict the rest of the current year as well as the next. In effect, these forecasts are prepared at about the time of the turn, insofar as it can be dated from annual observations. In terms of the monthly dates of business cycle turns in the postwar period, three occurred before midyear, two were in July, and three after July. The record of annual forecasts, therefore, suggests that those made shortly before or shortly after the monthly turn have not been very successful, while those made a few months later have been quite successful.

Zarnowitz' materials on annual turning point forecasts of other variables yield another interesting conclusion bearing on this point. It is that the accuracy of forecasts is greater for variables that lag in the business cycle than for those that move coincidentally or lead. Turning points in plant and equipment expenditures, which often lag, are forecast more accurately than those in inventory change, which generally lead. Consumer prices, which lag, are forecast more accurately than wholesale prices, which move more promptly. Hence the visibility of a turning point to a forecaster depends partly on how far ahead he looks and partly on whether the variable he is looking at moves earlier or later than others that he can observe and relate to it.

The National Bureau has assembled not only annual forecasts but also quarterly forecasts for several quarters ahead. These provide an additional test of forecasters' ability, since turns can be dated with greater precision from quarterly than from annual data. Quarterly forecasts of turning points depend, in the first instance, on forecasts of direction change. How well, then have forecasters predicted the

direction of change in GNP between the current quarter and the next, between that quarter and the one beyond it, and so on? A compilation of nearly fifty forecasts of this type (Table 25-2) suggests that forecasters are well aware that GNP generally increases from one quarter to the next, but possess little ability to predict when declines are coming. For example, out of thirty-eight predicted increases from the current quarter to the next, three-fourths turned out to be correct, while only a third of the nine predicted declines were correct. The combined percentage of correct forecasts—68 percent—is not much better than one would expect if forecasters knew approximately how often declines would occur, but tossed a coin to decide when they would take place. As for quarterly changes still farther in the future, the forecasters seldom predicted declines at all, and when they did, showed no ability to pick the right occasions.

The record for forecasts of semiannual GNP data (from a different group of forecasters) is somewhat better. This is shown in the semiannual Section B of Table 25-2. Ninety percent of the predicted increases and 44 percent of the predicted declines from one half-year to the next proved correct, for a combined percentage accuracy of 73 percent. From the next half-year to the one following that, the accuracy drops to 62 percent, which is not much better than random.

The data as a whole suggest that forecasters have yet to establish their ability to detect turning points in aggregate economic activity well in advance of the event. What they do demonstrate is an ability to recognize turns at about the time or shortly after they occur.[6]

Another type of turning point record has been compiled by Rendigs Fels and C. Elton Hinshaw [4]. Fels developed a system for scoring statements about the business outlook that he applied to the writings of analysts that regularly appear in certain widely read business or financial journals. Hinshaw applied the same system to the statements on the outlook recorded in the minutes of the regular meetings of the Federal Reserve Board's Open Market Committee. Figure 25-1 summarizes part of their results, in terms of the probability, as indicated by the statements, that a turning point soon will be, or has already been reached.

The figure shows, first, that there is a clear improvement in the ability to forecast or recognize a turn as the date of an actual turn approaches and further improvement after it passes. Second, the record is better at troughs than at peaks—perhaps indicating an optimistic bias or maybe that troughs are easier to predict and recognize. Third, the Open Market Committee—the group in whose hands the monetary policy of the nation largely rests—did slightly better than

Table 25–2. Forecasts of Directions of Change in Gross National Product, Quarterly and Semiannual Data, 1947–1964.

A. Forecasts of Quarter-to-Quarter Changes, 1959–1963

		0–3 Months			*3–6 Months*			*6–9 Months*		
		Forecast			*Forecast*			*Forecast*		
		Rise	*Fall*	*Total*	*Rise*	*Fall*	*Total*	*Rise*	*Fall*	*Total*
Actual	Rise	29 (28.3)	6 (6.7)	35	34 (33.8)	5 (5.2)	39	30.5 (31.0)	2 (1.5)	32.5
	Fall	9 (9.7)	3 (2.3)	12	5 (5.2)	1 (0.8)	6	11.5 (11)	0 (0.5)	11.5
	Total	38	9	47	39	6	45	42	2	44
		C_3 = 68 (65) α = 1.61 r = 0.09 χ^2 = 0.38 p = 0.54			C_3 = 78 (77) α = 1.36 r = 0.04 χ^2 = 0.07 p = 0.79			C_3 = 69 (72) α = 0 r = −0.13 χ^2 = 0.74 p = 0.39		

Table 25-2. continued

A. Forecasts of Quarter-to-Quarter Changes, 1959–1963

		9–12 Months			*12–15 Months*			*15–18 Months*		
		Forecast			*Forecast*			*Forecast*		
		Rise	*Fall*	*Total*	*Rise*	*Fall*	*Total*	*Rise*	*Fall*	*Total*
Actual	Rise	31 (31.1)	2 (1.7)	33	16.5	0	16.5	14	0	14
	Fall	5 (4.7)	0 (0)	5	6.5	0	16.5	2	0	2
	Total	36	2	38	23	0	23	16	0	16
		C_3 = 82 (82) α = 0 r = − 0.09 χ^2 = 0.31 p = 0.58			C_3 = 72 (72) α = 0 r = 0 χ^2 = 0 p = 1			C_3 = 88 (88) α = 0 r = 0 χ^2 = 0 p = 1		

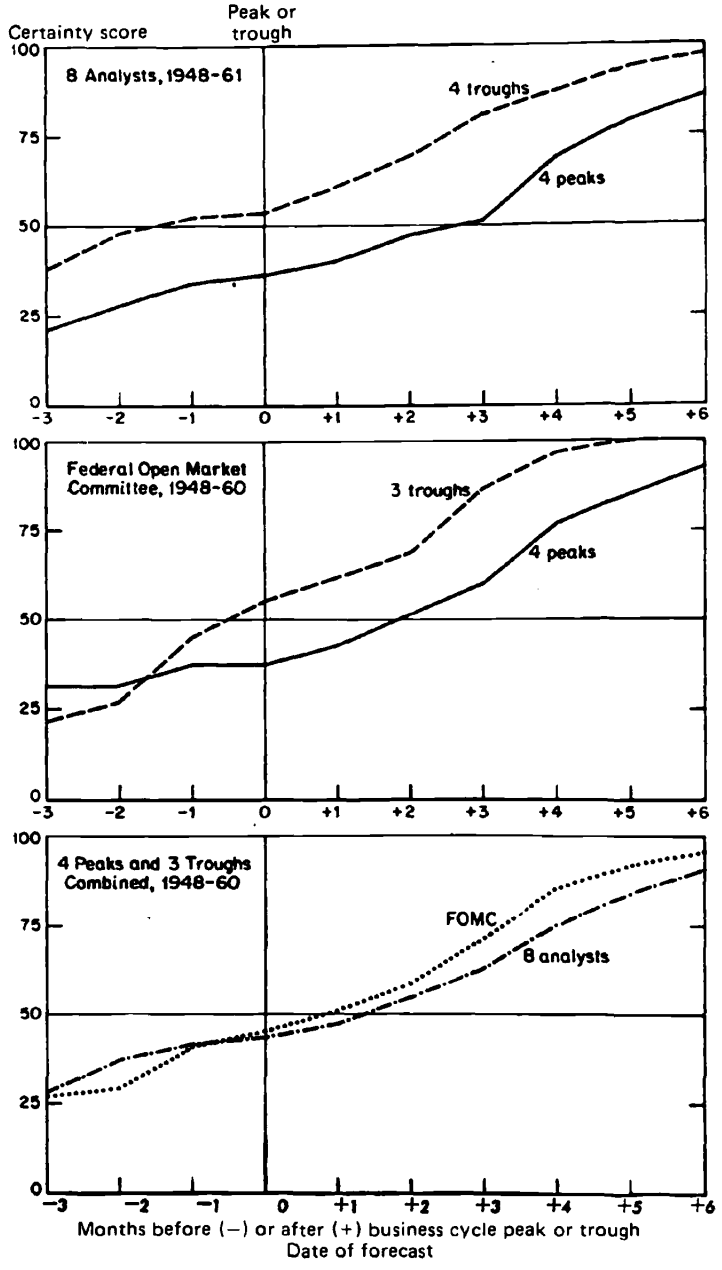
Table 25-2. continued

B. Forecasts of Semiannual Changes, 1947–1964

		0–6 Months			*6–12 Months*		
		Forecast			*Forecast*		
		Rise	*Fall*	*Total*	*Rise*	*Fall*	*Total*
Actual	Rise	26 (22.6)	9 (12.4)	35	23 (22)	10 (11)	33
	Fall	3 (6.4)	7 (3)	10	7 (8)	5 (4)	12
	Total	29	16	45	30	15	45
		C_3 = 73 (58) α = 6.74 r = 0.38 x^2 = 6.50 p = 0.01			C_3 = 62 (58) α = 1.64 r = 0.11 x^2 = 0.54 p = 0.46		

Note: For definitions and source see Table 22–1. The span 0–3 months refers to the change from the current quarter to the one ending three months hence; 3–6 months refers to the change from the quarter ending three months hence to the one ending six months hence; etc.

Figure 25-1. Recognition Scores at Business Cycle Peaks and Troughs.



Source: Fels and Hinshaw [4].

the average of the business analysts. The committee's record in avoiding false signals—not shown in the figure—was markedly better than that of the analysts.

Finally, the Fels-Hinshaw study, like Zarnowitz', suggests that there is only a modest ability to anticipate turns. Before the date of the turn, the indicated probability attaching to the occurrence of a turning point averages less than 50 percent. A month or so after the turn it is still only a fifty-fifty proposition. The 75 percent level, or odds of three to one, is reached three or four months after the turn. Not until five or six months after the turn is there virtual certainty (nine to one odds) regarding the event.

In view of the lags in the availability of data and the fact that major economic indicators rarely reach their turns at the same time, this is not necessarily poor performance. Moreover, recognizing a turn in the business cycle a few months after the event does imply some ability to forecast. One must not only be aware of the change in the direction of movement of aggregate economic activity that is taking place, but also judge its likely magnitude and duration. Recognizing business recessions is not the same as recognizing every wiggle on the curve. Major, lasting movements must be distinguished from minor interruptions of the trend. The evidence collected shows that forecasters have achieved some success, if not in anticipating the turn, at least in recognizing it rather promptly.

FORECASTING MAGNITUDES

Most forecasters regard the prediction of turning points as one of the significant challenges in their work. It is easy enough, they say, to anticipate continuing growth. The crucial test comes at the turning point. That is why I began this review by examining turning point forecasts. On the other hand, it is also true that the chief concern of forecasters is not predicting turning points but predicting the magnitude of change.[7] True, predicting a turning point in the business cycle implies something about the magnitude of change. But this is not entirely satisfactory. Missing the turn of a mild recession, say in 1960, is not as serious as missing a 1929. Moreover, between 1961 and 1968 we have enjoyed a period without any business cycle turning points as commonly defined, so the only mark of a forecaster's skill in this respect was his ability to avoid characterizing the minor dips, notably in 1962 and 1966, as recessions. It behooves us, therefore, to look at the record of forecasts of magnitude of change.[8]

One of the most important such series of forecasts covers only the recent period of expansion. These are the forecasts prepared by the

Council of Economic Advisers and published each year beginning with January 1962 in the *Economic Report of the President.* The forecasts pertain to gross national product for the year ahead, in current and in constant prices, and the implicit price deflator, which is one of the broadest measures of the general price level. With respect to GNP in current prices the *Report* has been explicit. The forecast has been stated as the midpoint in a range of plus or minus $5 billion, which since 1962 has been roughly equivalent to plus or minus 0.75 percent of GNP. The *Report* has generally been less explicit with respect to the price level and real GNP, though when the numbers are not stated, the approximate rates of change implied in the forecast can be inferred. One of the conditions implicitly and often explicitly underlying the forecasts is that the President's budget and economic program will be enacted. Hence the forecasts should, strictly speaking, be judged only after taking account of the extent to which these conditions were met. The record in Table 25-3 does not, of course, do this.

During four of the six years when the forecasts can be compared with actual changes, the rate of price increase was underestimated, a tendency particularly marked in 1965–1966, as I noted earlier. On the other hand, the forecasts somewhat overestimated the rate of growth in real GNP. As a result, the average forecast change in current dollar GNP over the period as a whole was almost exactly right.

Nevertheless, the errors in individual years were not inconsequential. For each of the three variables, the errors, on the average, were roughly 20 to 25 percent of the mean rate of change. In five years out of six, the actual value of current dollar GNP fell outside the range within which it was considered likely to fall. A more reasonable range in the light of experience would have been about twice as large—namely, plus or minus one and one-half percentage points, which is plus or minus $12 billion at current levels of GNP. Even this wider margin of error would have been exceeded in two of the six years.

A common way of appraising a set of forecasts is to compare the results with what could have been achieved by some simple method of extrapolation. One such method is to assume that the change in the coming year will be the same as the change last year. The mean errors resulting from this procedure are shown in the table, and they exceed the mean forecast errors for current dollar and real GNP, but not for prices, where the simple extrapolation yields approximately the same results. There is reason to suppose that it should, since the *Report* has often stated its expectation regarding prices in terms of extending the recent trend. In 1965–1966 the forecast explicitly

Table 25-3. Forecasts of Annual Percentage Changes in Gross National Product and in the General Price Level, President's Economic Report, 1961–1968.

	Percentage Change								
	GNP in Current Dollars			*GNP in Constant Dollars*			*GNP Implicit Price Deflator*		
Year (1)	*Forecast* (2)	*Actual* (3)	*Error* (4)	*Forecast* (5)	*Actual* (6)	*Error* (7)	*Forecast* (8)	*Actual* (9)	*Error* (10)
1960-1961	—	3.3	—	—	1.8	—	—	1.5	—
1961-1962	9.4	6.7	2.7	8.0[a]	5.3	2.7	1.5[a]	1.4	0.1
1962-1963	4.4	5.4	−1.0	3.5[a]	3.8	−0.3	1.0[a]	1.5	−0.5
1963-1964	6.5	6.6	−0.1	5.0	4.7	0.3	1.5	1.9	−0.4
1964-1965	6.1	7.5	−1.4	4.0[a]	5.4	−1.4	2.0[a]	1.8	0.2
1965-1966	6.9	8.6	−1.7	5.0	5.4	−0.4	1.8	3.0	−1.2
1966-1967	6.4	5.6	+0.8	4.0	2.5	1.5	2.5	3.0	−0.5
1967-1968	7.8			4.3[a]			3.3[a]		
Mean, 1961–1967, disregarding sign	6.6	6.7	1.3	4.9	4.5	1.1	1.7	2.1	0.5
Mean error of extrapolating preceding year's change, 1961–1967			1.8			1.6			0.3
Correlation coefficient, forecast and actual			0.34			0.48			0.72
Correlation coefficient, actual and preceding year's actual			−0.04			−0.44			0.73

Source: Economic Report of The President, January 1962 through February 1968. The actual changes are based on the first official estimates given in the report following the year for which the forecast was made. Changes in constant dollar GNP and in the price deflator are based on estimates in 1954 dollars for 1960-1961 to 1961-1965 and in 1958 dollars thereafter.

[a]Inferred from statements in the report. All other entries are based on figures (dollar levels, dollar changes, or percentage changes) given in the report.

extrapolated the preceding year's change and thereby made its largest error. This extrapolative method applied to annual data will, of course, always be a year late in recognizing an acceleration in the rate of inflation or in anything else.

The GNP forecasts in the President's *Economic Report* achieved a positive correlation with subsequent actual changes, which again is superior to the simple extrapolation of last year's change, since the latter was if anything inversely correlated with the actual change. But the correlation attained by the forecasts is modest. By this measure (that is r^2) the forecasts account for only a small fraction of the variance in actual rates of change—12 percent for GNP in current dollars and 23 percent for GNP in constant dollars. (For a more recent analysis of this record, see Chapter 26.)

I turn next to some other forecasting records. The mean error of forecasting the annual rate of change in gross national product by various groups of economists, according to eight sets of records assembled by Zarnowitz, is set forth in Table 25-4. The records begin generally in 1952 or 1953 and extend through 1963 (more recent figures are available but have not yet been incorporated in summaries). Most of these mean errors range between one and one-half and two and one-half percentage points, and hence are consistently larger than those of the *Economic Report*. However, the period covered is

Table 25-4. Summary Measures of Error in a Collection of Business Forecasts of Annual Percentage Changes in Gross National Product in Current Dollars.

Forecast Set (1)	*Month Forecasts Made* (2)	*Period Covered* (3)	*Mean Absolute Error (percent)* (4)	*Correlation Coefficient, Forecast and Actual Change* (5)
B	October	1952-1963	1.9	0.73
D	October	1955-1963	1.7	0.75
E	October	1952-1963	3.0	0.58
A	November-December	1953-1963	2.3[a]	0.79[a]
H	October-January	1953-1963	2.2	0.81
C	November-January	1957-1963	1.9	0.80
F	January	1952-1963	1.4	0.90
G	January	1952-1963	1.6	0.84
Mean, 8 sets			2.0	0.78

Source: Victor Zarnowitz, unpublished tabulations. For descriptions of the samples of forecasts covered and analysis of the results, see Zarnowitz [16].

[a]The figures for the period 1946-49, 1953-1963, for columns 4 and 5, respectively, are 4.0 and 0.19.

different. The records in Table 25-4 encompass three recessions and hence about twice as wide a range of variation in the rates of change to be forecast. Or, to put it somewhat differently, the errors involved in a simple extrapolation of the preceding year's rate of change were about twice as great, on the average, during 1952–1963 as during 1961–1967. Relative to the variation with which the forecasters were faced, therefore, the forecasts in the *Economic Report* do not represent as good a performance as that achieved by most of the forecasting groups in Zarnowitz' sample. This is indicated more directly by the correlation coefficients, which range from 0.6 to 0.9 in Table 25-4 compared with 0.3 in Table 25-3. The collection of forecasts in Table 25-4, therefore, though they made larger errors, captured a substantially larger fraction of the variation in rates of change in GNP than did the forecasts in the *Economic Report*. Unfortunately, we have not yet checked this result by a direct comparison covering the same period, which is necessary if the forecasting hazards are to be matched precisely (See Chapter 26).[9]

Another forecasting record where such a check can be made is shown in Table 25-5. This pertains to forecasts of the annual rate of change in GNP in constant dollars rather than in current dollars and is based on the econometric model originally formulated by Lawrence Klein and Arthur Goldberger and subsequently developed by Daniel Suits.[10] The forecasts have been presented in November of each year at the Conference on the Business Outlook at the University of Michigan.

During the six years, 1961–1967, the errors in the Suits' model forecasts were larger than those in the *Economic Report* in four years and smaller in two, with the mean error exactly the same. The model forecasts and the *Report* forecasts were quite closely correlated with each other, though the correlation between forecast and actual change was somewhat smaller for the model than for the *Economic Report*. However, this difference cannot be given much weight because the sample period is short.

In the earlier period, 1952–1963, as well as in the period 1952–1967 as a whole, the model forecasts were much more highly correlated with the actual changes than during the past six years, though the mean error was somewhat larger. Both the higher correlation and the larger error may be due to the fact that the variability in rates of change in real GNP was substantially larger in the earlier period. In any event, both in the earlier period and more recently the model forecasts have been substantially better than those of a simple extrapolation of preceding year changes. For the fifteen year period as a whole, the model reduced the error of extrapolation by more than

Table 25-5. Forecasts of Annual Percentage Changes in Gross National Product in Constant Dollars, Suits' Econometric Model, 1952–1968.

	Percentage Change, GNP in Constant Dollars		
Year *(1)*	*Forecast* *(2)*	*Actual* *(3)*	*Error* *(4)*
1951-1952	—	2.1	—
1952-1953	4.8	3.7	1.1
1953-1954	-2.6	-3.1	0.5
1954-1955	0.4 (-0.3, +0.1, +1.4)	6.2	-5.8
1955-1956	2.5	2.5	0
1956-1957	2.3	0.8	1.5
1957-1958	0.1 (-0.6, 0.4, 0.5)	-3.1	3.2
1958-1959	5.3	6.7	-1.4
1959-1960	0.7 (-0.6, 2.0)	2.7	-2.0
1960-1961	1.9 (1.6, 2.2)	1.8	0.1
1961-1962	5.2	5.3	-0.1
1962-1963	3.0 (3.0, 5.4)	3.8	-0.8
1963-1964	4.9 (2.7, 4.9)	4.7	0.2
1964-1965	3.3	5.4	-2.1
1965-1966	4.0	5.4	-1.4
1966-1967	4.3	2.5	1.8
1967-1968	4.6 (3.4, 5.1, 5.3)		

Summary

	1952-1963	*1961-1967*	*1952-1967*
1. Mean, disregarding sign			
a. Forecast percentage change	2.6	4.1	3.0
b. Actual percentage change	3.6	4.5	3.8
c. Error in forecast	1.5	1.1	1.5
d. Error in extrapolating preceding year's change	4.2	1.6	3.4
2. Correlation coefficient			
a. Forecast and actual percentage change	.72	.12	.71
b. Actual and preceding year's actual percentage change	- .37	- .44	- .20

Source: Forecast percentage changes are computed from published reports of the Michigan Conference on the Economic Outlook and data supplied by the University of Michigan Research Seminar in Quantitative Economics. Alternative forecasts based upon different data or policy assumptions are given in parentheses. For 1962-1963 and 1963-1964 we use the alternative corresponding to the policy adopted (the 1964 tax cut); for other years we use a simple average. Actual percentage changes are based on the first official estimates given in the *Economic Report of the President* for the year following the year for which the forecast was made. Forecast and actual changes are based on estimates in 1939 prices for 1951-1956, in 1947 prices for 1956-1957, in 1954 prices for 1959-1965, in 1957 prices for 1957-1958, and in 1958 prices for 1958-1959 and 1965-1968.

50 percent and had a mean error of about one and one-half percentage points and correlation between forecast and actual rates of change in real GNP of 0.7. The record was marred especially by the forecast for 1954–1955, which substantially underestimated the rate of expansion actually experienced. Excluding this one year, the mean error would have been approximately one percentage point and the correlation 0.9. Although a direct comparison with the collection of forecasts in Table 25-4 is not possible, since the latter refer to current dollar GNP, where somewhat larger errors might be expected because of the difficulty of forecasting prices, the model forecasts appear to stand up comparatively well.[11]

I conclude that it is reasonable to expect, on the basis of past performance, that economic forecasts made near the end of the year for the year ahead will predict the percentage rate of change in gross national product with an error averaging about one and one-half percentage points. One can expect, also, that the predicted rates of change will be positively, though far from perfectly, correlated with those that actually occur. In both respects this is a much better result than can be achieved by simply extrapolating last year's change in gross national product.

I cannot undertake to examine the results of forecasting the magnitudes of change in GNP by quarters, to consider how forecasting errors increase with the span of the forecast or with the distance of the forecast interval from the present, or to review the available data for other variables. These are fascinating topics, full of instruction for the practitioner as well as for the user of forecasts, but I pass them by in order to discuss standards of forecasting accuracy and ways to improve performance.

ACCURACY STANDARDS

The availability of a fairly systematic record of past forecasts of turning points and of magnitudes of change make it possible to formulate standards by which to judge future forecasts derived by new methods. The record tells us what it is reasonable to expect and what can be considered superior performance. One of the great merits of such a standard is that it has a realistic, historical basis, not a hypothetical one. Another is that it is not subject to the bias of hindsight.

Nevertheless, it does have several limitations. First, the past period may not be comparable with a future period with respect to ease or difficulty of forecasting. Although adjustments can be made for this, they cannot be entirely adequate. A second limitation is that the past forecasts are frequently conditional and the conditions may or may

not have been met. A straightforward comparison with what happened, as in our appraisal above, either may not do justice to the forecasts or may treat them more favorably than they deserve, but in any case does not tell as much as we should like to know about what standards to apply to conditional forecasts in the future. A related point is that as the timeliness, power, and appropriateness of governmental actions based upon forecasts improves, the forecasts themselves may be negated. And as forecasts become more widely publicized and more uniform—witness the common phrase "standard forecast"—the reactions of private firms and individuals may place the forecasts in jeopardy or help to bring them about. A simple historical record of accuracy, then, may no longer provide an appropriate benchmark. Finally, a standard based upon the past record may not provide a proper stimulus to improvement. We must seek to do better, not merely to equal our past achievements.

In this last respect, the standard provided by the past record seems to be an improvement over the so-called naive models or extrapolative methods. As we have seen, the record of most forecasters is superior to at least one of the naive models—the extrapolation of the most recent change. Recent work by Zarnowitz, Mincer, Theil, Cunnyngham, and others shows that this "same change" model is inferior to more sophisticated autoregressive models. But many sets of actual forecasts, at least for short periods ahead, beat even the best autoregressive models. In this sense the past record provides a higher standard, as well as a more realistic one.

Recently I have been experimenting with a standard, based upon leading indicators, that incorporates some elements of the historical and some of the extrapolative, and I should like to describe its properties briefly. First, a composite index of leading indicators—those that have generally reached their peaks and troughs at an earlier date than business activity as a whole—is constructed, using methods developed by Julius Shiskin [10]. It is then assumed, as suggested by past evidence, that this index leads various specific measures of aggregate economic activity—such as gross national product, industrial production, or employment—by six months. This implies that the percentage change between fiscal year averages of the leading index should be closely correlated with the percentage change between the subsequent calendar year averages of the aggregates. That is, the change in the leading index between the fiscal years ending June 30, 1967, and June 30. 1968, is associated with the calendar year change in GNP from 1967 to 1968, and so on. It turns out that on this basis, the percentage changes in the leading index, appropriately adjusted by a simple regression, provide fairly accurate esti-

mates of the turning points and the percentage changes in calendar year data for GNP and other aggregates.

If we could wait until just after the middle of the calendar year to make our forecast for that year, by which time fiscal year averages for the leading index would be available, this method, would, in the postwar period, have provided forecasts correlated with the annual percentage changes in GNP to the extent of about 0.8, with an average error of something less than one and one-half percentage points. Unfortunately, forecasts must be made earlier than this. The simple device that we have resorted to is to use whatever part of the current fiscal year figure for the leading index is available as an estimate for the entire fiscal year. For example, if only the initial quarter (July, August, and September) is available because the forecast is being made, say, in October, the change between that quarter and the preceding fiscal year average is used as an estimate of the change between the full fiscal years. Since this in effect shortens the span over which the change is measured and at the same time increases the assumed lead to something like ten and one-half months instead of six, the regression adjustment needed to forecast calendar year changes in GNP is different than before. The mean error on this basis for the postwar period is approximately two percentage points and the correlation coefficient is 0.6.

As more data for the current fiscal year become available, they can be used to revise the forecast. In late January, for example, data for the first two quarters of the fiscal year could be used. We have calculated the forecasts for the postwar period on the alternative assumptions that one, two, three, or four quarters of data are used (this could also be done for successively larger groups of months instead of quarters, since the leading index is available monthly). The regression equations and summary measures of error are given in Table 25-6, for GNP in current and in constant dollars. The table shows how the correlation improves and the mean error is reduced as more recent data for the leading index are used in the forecast.[12]

Table 25-7 compares the actual forecasts referred to above with these results, making use of data on the leading index for only the initial quarter of the current fiscal year—that is, through September of the year preceding the one being forecast. For the most part, the forecasts of change in GNP based upon the leading index compare favorably in terms of magnitude of error and degree of correlation. The experiment suggests, therefore, that this mechanical use of the leading index can produce a standard of forecast accuracy which is not easy to surpass. I have shown previously a modification of this method and an application of it to foreign trade forecasting.[13]

Table 25-6. Regression Analysis of Annual Percentage Changes in Index of Leading Indicators and in Gross National Product, 1951–1967.

	Dependent Variable				
	Calendar Year Percentage Change in GNP in Current Dollars				
Independent Variable	*r*	*a*	*b*	*t*	*MAE*
Leading index, per cent change[a]					
1. Fiscal year to III *Q*	0.62	4.19	0.44	2.98	1.84
2. Fiscal year to III + IV *Q*	0.76	3.95	0.42	4.39	1.47
3. Fiscal year to III + IV + I *Q*	0.82	3.79	0.37	5.42	1.29
4. Fiscal year to fiscal year	0.83	3.64	0.34	5.57	1.28
	Calendar Year Percentage Change in GNP in Constant Dollars				
Leading index, per cent change[a] from					
5. Fiscal year to III *Q*	0.58	2.16	0.43	2.70	2.01
6. Fiscal year to III + IV *Q*	0.73	1.88	0.43	4.05	1.63
7. Fiscal year to III + IV + I *Q*	0.81	1.67	0.39	5.22	1.38
8. Fiscal year to fiscal year	0.83	1.48	0.36	5.66	1.31

Note: Linear regressions were fitted to percentage changes in the revised GNP data, 1951-1967, as available in May 1968, and in the reverse trend-adjusted index of eighteen leading indicators (see n. 12). *r* is the correlation coefficient; *a* and *b* are the regression coefficients; *t* is the *t*-ratio for *b*; and MAE is the mean absolute error in percentage points. The mean absolute error is adjusted for the loss of two degrees of freedom used in fitting the regression (by dividing the sum of the errors by $N-2$ instead of by N). However, this adjustment probably results in an overstatement of the mean absolute error, since the regression was fitted to minimize the mean square error, not the mean absolute error.

[a]Percentage changes for lines 1 and 5 are calculated from the preceding fiscal year average to the third quarter (July-September) of the year preceding the calendar year being forecast; for lines 2 and 6, from the preceding fiscal year to the average of the third and fourth quarters of the year preceding the calendar year being forecast; and so on.

Such a standard has several points to recommend it, apart from its past record. One is that it depends largely upon economic information from outside the series that is being predicted. The various naive models depend only upon the previous history of the forecast series and usually only its very recent history. The outside information that the leading index contains is of a kind that can be expected, on a variety of theoretical grounds, to bear upon future changes in aggregate economic activity. New orders for equipment and contracts for construction have obvious anticipatory elements. Other leading indicators are connected with future activity by more complicated routes, ranging from those that explain why changes in the average workweek ordinarily precede those in employment or

Table 25-7. Summary Measures of Error in Several Sets of Forecasts of Gross National Product.

	Period Covered	*Mean Absolute Error (percent)*	*Correlation Coefficient, Forecast, and Actual Change*
1. Forecasts of Annual Percentage Changes of GNP in Current Dollars			
a. Mean, eight sets of business forecasts	1952-1963	2.0	.78
b. Index of leading indicators[a]	1952-1963	1.8	.74
c. Extrapolation of preceding year's change	1952-1963	4.1	−.43
d. *Economic Report*	1961-1967	1.3	.34
e. Index of leading indicators[a]	1961-1967	0.8	.66
f. Extrapolation of preceding year's change	1961-1967	1.8	−.04
2. Forecasts of Annual Percentage Changes of GNP in Constant Dollars			
a. Suits' econometric model	1952-1967	1.5	.71
b. Index of leading indicators[a]	1952-1967	1.6	.71
c. Extrapolation of preceding year's change	1952-1967	3.4	−.20
d. *Economic Report*	1961-1967	1.1	.48
e. Suits' econometric model	1961-1967	1.1	.12
f. Index of leading indicators[a]	1961-1967	0.4	.94
g. Extrapolation of preceding year's change	1961-1967	1.6	−.44

Note: The mean errors and correlation coefficients are calculated using as the "actual" change the first official GNP estimates published in January or February following the year being forecast. In Table 25-6, the corresponding calculations for the leading index, and the regressions as well, are based on the latest revised GNP estimates.

[a]Using the regression equations in Table 25-6, lines 1 and 5 (fitted to data for 1951-1967, based upon changes in the leading index from the preceding fiscal year to the third quarter). An alternative set of forecasts for 1961-1967, based upon regression equations fitted to data for the prior period 1949-1961, yields the following results for 1961-1967:

	Mean Absolute Error (percent)	*Correlation Coefficient*
GNP in current dollars	0.7	0.64
GNP in constant dollars	0.9	0.95

output to those that explain the delayed reaction of the economy to a change in the rate of growth of the money supply. Moreover, extensive empirical testing, carried out over many years, lies behind the selection of the individual leading indicators. Although no amount of theorizing or empirical testing can insure that past relationships will persist, it is only by these means that we build confidence in particular forecasting procedures.

The empirical evidence examined in selecting the leading indicators pertained primarily to the consistency of their timing in the business cycle. The degree of correlation between the magnitude of their movements and those of GNP or any other aggregate was not considered.[14] This means, then, that the correlations recorded in Tables 25-6 and 25-7 are not just reproducing a relationship used in selecting the leading indicators. To this extent the results do not depend on hindsight. Moreover, although historical evidence with respect to the leads or lags of the indicators at turning points in the business cycle was a vital factor in their selection, much of this evidence goes back to the period before World War II. Indeed, about a third of the eighteen indicators included in the index were originally selected in 1950 on the basis of prewar evidence, and a postwar index constructed from the 1950 list is not unlike the present index (see Chapter 24). Hence, we may have some confidence that the average timing assumed in the method will persist, despite considerable variability at particular turns in the business cycle.

In many respects the method is analogous to the reduced form of a system of econometric equations. In the reduced form, weights or coefficients are applied to the predetermined or exogenous variables in the system to allow for their direct or indirect effects on GNP in order to generate a forecast. In the leading index, weights are applied to various leading indicators to allow for differences in their amplitude of variation and other factors, and as a final step, a regression coefficient is applied to the weighted average change in the indicators to generate a forecast. The choice of variables is different and so is the procedure for deriving weights, but in the end both forms obtain a forecast by applying a reasonable system of weights to a set of variables believed to be related to the future course of GNP.

The procedure I have outlined for generating annual forecasts is easily kept up to date and readily applied to such series as gross national product, industrial production, employment, or unemployment. For states or for countries that are not abundantly endowed with comprehensive economic data and the analytical models that require such data, the method might prove particularly useful. It can

be used for shorter time units than a year—say, quarters or half years. By modifying the assumption regarding a six month lead, and perhaps with other modifications in composition or weighting, it might be applied to variables that lag in the business cycle, such as some price indexes or some interest rates. But it must be emphasized that the consistency of forecasts of different variables generated from a single leading index needs to be carefully checked. Forecasts of GNP in current and in constant dollars may or may not imply a reasonable forecast of the price deflator; forecasts of output and of employment may or may not imply a reasonable forecast of productivity; and forecasts of employment and of unemployment may or may not imply a reasonable forecast of the labor force.

In short, a simple and mechanical method cannot be expected to pass all the complex tests one can set for it. It is not a substitute for a carefully reasoned approach to the economic outlook, whether this approach takes the shape of an econometric model or of a less formal apparatus. All that the method does is help to summarize the information contained in a group of leading indicators regarding the near-term future course of GNP or other variables that are systematically related to the business cycle. Hence it can provide the forecaster with some of the information useful in developing his actual forecast, and it can be used as a standard by which to judge his past efforts, perhaps helping him to improve upon them.

IMPROVING PERFORMANCE

Forecasting is the activity of the economic statistician that is most visible to the public. The public is not likely to forget the dramatic failures, like those I mentioned at the outset. But these apparent failures require careful analysis. Sometimes the sequel alters one's view. The demise of the Harvard *ABC* curves after 1929, which I referred to earlier, is a case in point. Did the historical sequence upon which the scheme was based disappear in 1929 never to return? Was it a mere figment of a Harvard professor's imagination? The answer is no. Stock price indexes have continued to lead at business cycle turns as systematically since 1929 as they did before; various types of interest rates, though not all, have continued to lag; and the rise of interest rates and tightening of money during an expansion of business has been one of the factors tending, after a time, to curtail new commitments to invest, shift investor's sentiment from stocks to bonds, cause stock prices to turn down, and bring the business expansion to an end. The Harvard curves oversimplified the situation—it was not

as simple as *ABC*—but they did contain a kernel of truth about the way our economic system works.

One lesson I draw from my review is that the development of the science of forecasting depends crucially upon the accumulation and continuing analysis of a record of forecasts. Without a record one cannot evaluate the performance or tell how to improve upon it. All too often forecasts are thrown out and forgotten—or thrown out with the hope that they will be forgotten—as soon as the occasion for them is past. They are often inadequately annotated when they are made. They often fail to specify what the present level of activity is believed to be, what assumptions or conditions are laid down, what probability or range of outcomes is attached to the forecast under these conditions, what is expected to happen if the conditions do not obtain, and what method or information was used to arrive at the results. Sometimes, even, the forecasts are couched in terms that make them unverifiable. Since forecasts are always subject to revision as more information becomes available, a record of the revisions, the reasons for them, and their relation to the final outcome is an important part of the story.

Econometric model forecasts, in principle, meet many if not all of these conditions. But econometric model builders are as human as the rest, as any attempt to resurrect an ex ante record of model forecasts will show. To judge from experience, it is too much to expect the individual forecaster to develop a scientific record of his work on his own initiative. The benefits that may accrue to him are uncertain—the record may even prove to be fatal!—and he always has plenty of other things to do. The benefits really accrue to the profession and sooner or later to society as a whole. In view of this, I believe that the American Statistical Association, and particularly its business and economics statistics section, has a significant role to play.

For a number of years the B&E section has conducted an annual outlook survey among its 5,000 members. Questionnaires are mailed out and the replies tabulated. Last year about 400 members responded, and perhaps half of them indicated that they regularly prepared forecasts. It seems to me that this survey could be developed so that it would become the vehicle for a scientific record of economic forecasts and hence be of far greater service both to the profession and to the public.

If it is to be this, the survey should be conducted quarterly and thereby provide an opportunity to record the frequent revisions that forecasters make. It should provide for the identification, though not

the disclosure, of the name of the forecaster, so that a continuing series for each respondent can be accumulated. It should specify, or allow the forecaster to specify, what are the most recent levels or rates of change in relevant series. It should record the assumptions attached to the forecast, allow for probabilistic or alternative forecasts, and call for a description of the methodology.

The questionnaire that was circulated in the July 1968 survey went some way to meet these specifications. Careful consideration must be given, of course, to how far one can go and still retain the cooperation of respondents. Moreover, to be useful the results must be analyzed and compared with subsequent developments, and provision for this on a continuing basis must be made. The analytical work should be closely tied to the survey itself, so that the needs and ideas of the analysts can be reflected in the questions asked.[15]

A systematic, analytical record of forecasts and a continuing review of the results is one step toward better forecasting procedures and results. Other steps must be taken as well. Improvement in the quality of the basic data is fundamental. A study [1] of the revisions in the provisional GNP estimates, upon which all forecasters depend, showed that about 40 percent of the mean error in forecasts was attributable to errors in the current data. Two-fifths is a substantial fraction, and it represents a part of the forecasting error for which forecasters per se cannot be blamed. On the other hand, the agencies constructing the provisional estimates cannot be blamed either, for the fault lies with the inadequacy of the information that they must use to produce estimates promptly. A considerable improvement in the accuracy of forecasts could be brought about by a massive effort to bring the less adequate types of data up to the level of the best. Better coverage of output, wages, and prices in the service industries; greater attention to obtaining transaction prices, both domestic and international, in contrast to list prices or unit values; prompter reporting of information on profits; reduction in erratic elements in anticipatory statistics such as housing starts, new orders, and construction contracts; classification of data on new orders by industry placing the order; and far more comprehensive statistics on job vacancies than are presently available—these are among the items that should be on our statistical agenda. Their development will not only improve analyses of general economic prospects but will also contribute to more enlightened public policy and private decisions at all levels.

Finally, we need to expand the scientific studies that inform us about how our economic system works and what have been the effects of policies and institutions upon its workings. This is not the

place to consider systematically the types of analytical studies most needed to support short-term forecasting efforts, but I should like to mention a few relatively neglected types of study. One is a careful review or set of case studies of past forecasting failures to explain what went wrong, how far it was avoidable, and what lessons for future forecasting can be learned from such episodes. A second need is for studies devoted to the forecasting of leading indicators. Their anticipatory value would clearly be enhanced if we could forecast their own movements; yet with few exceptions, little work of this kind has been done. In this connection a promising line of attack is to examine the behavior of various types of lagging indicators. Historically the behavior of many leading indicators, such as new orders or profit margins, appears to have been strongly influenced by the opposite movements of certain lagging indicators such as interest rates or unit labor costs, particularly when the relation of the latter to the level of aggregate activity is taken into account. A third type of study demanding attention concerns the relations between the problems of forecasting short-term fluctuations and those of forecasting intermediate or long-term growth trends. One of the first requirements is the compilation and analysis of a record of longer term forecasts. A great deal of such forecasting is done, it has a considerable influence upon investment and other decisions, and yet we know very little about its accuracy and what contributes to sound results.

All of these paths to progress—better records of forecasting, a sounder statistical base, and a more enlightened economic framework—are costly. But the potential benefits to society are great. The nation is devoting large resources to economic forecasting. Tardiness or failure to identify, measure, and anticipate the forces of inflation or of recession can affect the welfare of millions. Economic forecasters have, in my judgment, demonstrated an ability to forecast. But there is much room for improvement, the limitations need to become better known and more firmly established, and the most dependable techniques must be developed, demonstrated, and adopted. The statistical profession itself should take the leadership in bringing this about.

NOTES TO CHAPTER 25

1. The National Bureau study is under the direction of Victor Zarnowitz of the University of Chicago, and among those responsible for various parts of the project are Rosanne Cole, Jon Cunnyngham, Michael Evans, Rendigs Fels, Yoel Haitovsky, C. Elton Hinshaw, Jacob Mincer, and Julius Shiskin. Results of the project to date have been reported in references 1, 3, 4, 6, 8, 10, and 16. The

study has been supported by grants from Whirlpool Corporation, General Electric Company, Ford Motor Company Foundation, U.S. Steel Corporation, and the Relm Foundation, as well as by other funds of the National Bureau.

2. Because a year is a crude time unit for this purpose, the beginning or end or even the occurrence of a recession is not accurately identified by year-to-year changes in GNP. Indeed, although it is generally agreed that four recessions have taken place in the United States since 1946, only one decline appears in the revised calendar year data for GNP—namely, in 1948–1949. In the first official estimates published at the time, however, year-to-year declines occurred in 1953–1954 and in 1957–1958 as well (though not in 1960–1961). In comparing forecasts with what "actually happened," Zarnowitz [16] uses the first official estimates as most nearly representing what the forecaster is trying to forecast, and I have followed his practice. This choice avoids the conceptual changes contained in the revised estimates, but otherwise is debatable, since (1) the revised estimates, being based on more information, are more accurate than the first official estimates; (2) the latter are themselves forecasts of what the revised estimates will be and conceivably may be less adequate in this respect than the forecasts that antedate them; (3) the forecasts are usually based upon average relationships derived from revised data rather than first estimates; and (4) the averaging process itself abstracts to some degree from measurement error. See Rosanne Cole [1].

3. The record includes twelve sets of forecasts. Some go back to 1947 and some go as far forward as 1965, but the bulk of the forecasts (115 out of 126) pertain to 1953–1963. Six of the sets are forecasts made by individuals or a team; the other six are averages of separate forecasts made by the members of small or large groups. Hence the total number of forecasts included far exceeds 126. The averaging of group forecasts, it should be noted, tends to reduce the range of error.

4. This last assumption would not be wide of the mark if the revised GNP estimates (as of 1965) were the criterion of what happened rather than the first official estimates. Since the revised annual estimates contain turning points only in 1948 and 1949 (cf. n. 2), and few of the forecast records go back that far, the number of turning point predictions consistent with perfect accuracy in terms of the revised estimates would be three.

5. The test assumes that the observations are statistically independent, which clearly is not the case, partly because the occurrence of a turning point in one year has a bearing on its occurrence in the next, partly because the several sets of forecasts included probably have some influence upon one another and in any case cover the same period. These considerations are likely to mean that the statistical significance is exaggerated by the ordinary test.

6. Just as forecasts of year-to-year changes made at the end of the year identify turning points conventionally dated some six months before the date of forecast, so forecasts for the next half-year made at the end of the half-year identify turning points dated three months before the forecast, while forecasts for the next quarter identify turning points dated a month and a half earlier. The implicit lag is longest in the annual case, and since this lag makes other information available to the forecaster, it probably explains why the annual results in

Table 25-1 (first panel) are better than the semiannual (Table 25-2, semiannual changes, first panel), and the semiannual better than the quarterly (Table 25-2, quarter-to-quarter changes, first panel). In addition, the longer time unit doubtless smooths away some of the unpredictable wrinkles, though it also implies a longer future span to be forecast. The annual analysis in Table 25-1, however, is not strictly comparable with that in Table 25-2, since the latter is based on directions of change and the former on turning points, or sequences (pairs) of directions of change. When the data for Tables 25-1 and 25-2 are put in the same form, the correlation coefficients are:

Annual	*Semiannual*	*Quarterly*	*Quarterly*
Forecasts made (x) months after the turning point	0.59 (6)	0.38 (3)	0.09 (1.5)
Forecasts made (x) months before the turning point	0.25 (0)	0.11 (−3)	−0.13 (−4.5)

7. Curiously, in developing and applying their methods (fitting equations, etc.), many forecasters pay close attention to magnitudes (as in the method of least squares) but pay no particular attention to turning points. This procedure does not seem well suited to producing good forecasts of turning points even though, in the end, this is considered to be one of the crucial tests of good performance. See, for example, the analysis of the Commerce Department's model by Liebenberg, Hirsch, and Popkin [5]. Would the use of lagged (postdated) variables get such heavy emphasis in econometric models if the forecasting of turning points were a desideratum? The lagged series may improve the fit, but it cannot forecast its own turning point, and it may (though it need not) prevent other variables from doing so.

8. The materials presented rely largely upon two summary measures of forecasting quality—the mean absolute error in forecasts of percentage change and the correlation coefficient between forecast and actual percentage change. The use of percentage changes rather than first differences as the form in which to express the forecasts has the advantage of facilitating comparisons among different variables. Also, where the variables experience growth or inflationary trends, it puts the most recent changes more nearly on the same level as earlier changes. Logarithmic differences would in principle be superior to percentages, because increases and decreases would then be symmetrical, but since most of the percentage changes here are small, this is not an important consideration, and percentages have the merit of familiarity. The mean absolute error is arithmetically simpler than the root mean square error and avoids the high weights assigned to extreme errors that squaring implies. In practice, the mean absolute error is highly correlated with the root mean square error and is usually about eight-tenths as large (in a normal distribution the expected ratio is 0.798; in a rectangular distribution, 0.866). The correlation coefficient supplements the mean absolute error, since a correlation between forecast and actual changes that is close to zero or is negative diminishes one's confidence in a series of forecasts even when the mean error is small. On the other hand, a high positive cor-

relation does not in itself represent good forecasting performance if the mean error is large, though it may mean good potential performance (via, say, a linear adjustment of the forecasts). The ratio of mean absolute error to the mean absolute actual percentage change, which simply measures the size of the error relative to the magnitudes that are being forecast, is analogous to the inequality coefficient proposed by Theil [15]—namely, the square root of the ratio of the mean square error to the mean square actual change. In Theil's decomposition of this coefficient, the correlation between forecast and actual changes is one of the factors accounting for the total error.

9. Since the above was written, results during 1961–1967 for two of the groups (A and D) in Table 25-4 have been compared with those of the *Economic Report*. Both groups tended to underestimate the rate of growth in current dollar GNP. The mean absolute errors were 1.9 percent and 2.1 percent for A and D respectively, and the correlation coefficients between actual and forecast changes were 0.29 and 0.23. The poorer performance may be partly attributable to the fact that these forecasts antedate those of the *Economic Report* by two or three months.

10. I am indebted both to Suits and to Jon Cunnyngham for information regarding the forecasts. The forecasts were produced not by a single model but by a system of equations that was altered in some respect almost every year.

11. This conclusion was also reached by Cunnyngham [3] on the basis of a direct comparison of constant dollar and current dollar GNP forecasts.

12. The index of leading indicators used in Tables 25-6 and 25-7 is based upon eighteen leading indicators and is "reverse trend adjusted" (see [10]). Virtually the same results can be obtained without the "reverse trend adjustment." Since these computations were completed the U.S. Department of Commerce has begun to publish a monthly index based upon twelve leading indicators (see the November 1968 issue of *Business Conditions Digest*). Regression analysis based on the latter index yields results similar to those given here. The regression equations based on the index of twelve leading indicators, 1951–1967, are as follows:

Independent Variable: Leading index, percent change from	*Dependent Variable: Calendar year percent change in GNP in current dollars*				*Dependent Variable: Calendar year percent change in GNP in constant dollars*			
	r	*a*	*b*	*t*	*r*	*a*	*b*	*t*
1. Fiscal year to IIIQ	0.60	4.55	0.41	2.78	0.56	2.51	0.40	2.55
2. Fiscal year to III and IVQ	0.74	4.34	0.40	4.12	0.71	2.27	0.40	3.82
3. Fiscal year to III, IV, and IQ	0.81	4.19	0.36	5.08	0.80	2.08	0.37	4.94
4. Fiscal year to fiscal year	0.81	4.05	0.33	5.26	0.82	1.91	0.35	5.37

Regression analysis is, in fact, not essential to the procedure. The method used in constructing the leading index can be adapted so that the index has the

same trend and average cyclical amplitude as the series being forecast. Further adjustment by means of regression is then superfluous and may indeed bias the forecasts.

13. Chapter 6 of this book.

14. Note, however, that it has been known for some years that the magnitude of change in various leading indicators during the early stages of recession is correlated with the severity of the recession itself (cf. [7]).

15. The B&E section promptly took up this suggestion and on November 29, 1968, launched the first of a series of quarterly surveys designed to accomplish the above objectives in cooperation with the National Bureau of Economic Research.

REFERENCES

[1]. Cole, Rosanne. "Data Errors and Forecasting Accuracy." In Mincer [6].

[2]. Cox, Garfield V. *An Appraisal of American Business Forecasts.* rev. ed. Chicago: 1930.

[3]. Cunnyngham, Jon. "Econometric Forecasts." *Forty-fifth Annual Report of the National Bureau of Economic Research*, June 1965, pp. 60–64.

[4]. Fels, Rendigs, and C. Elton Hinshaw. *Forecasting and Recognizing Business Cycle Turning Points.* New York: National Bureau of Economic Research, 1968.

[5]. Liebenberg, M.; A.A. Hirsch; and J. Popkin. "A Quarterly Econometric Model of the United States: A Progress Report." *Survey of Current Business*, May 1966, pp. 23–26.

[6]. Mincer, Jacob, ed. *Economic Forecasts and Expectations: Analyses of Forecasting Behavior and Performance.* New York: National Bureau of Economic Research, 1969.

[7]. Moore, Geoffrey H. *Business Cycle Indicators.* Vol. I, ch. 5. New York: National Bureau of Economic Research, 1961.

[8]. Moore, Geoffrey H., and Julius Shiskin. *Indicators of Business Expansions and Contractions.* New York: National Bureau of Economic Research, 1967.

[9]. Okun, Arthur M. "Review of Some Economic Forecasts for 1955–57," *Journal of Business*, July 1959.

[10]. Shiskin, Julius, and Geoffrey H. Moore. "Composite Indexes of Leading, Coinciding, and Lagging Indicators, 1948–67." Supplement to *National Bureau Report 1*, January 1968.

[11]. Stekler, Hermon O. "An Evaluation of Quarterly Judgmental Economic Forecasts." *Journal of Business*, July 1968.

[12]. ____. "Forecasting with Econometric Models: An Evaluation." *Econometrica*, July-October 1968.

[13]. Suits, Daniel B. "Forecasting and Analysis with an Econometric Model." *American Economic Review*, March 1962.

[14]. Theil, Henri, *Economic Forecasts and Policy.* Amsterdam, 1961.
[15]. ____. *Applied Economic Forecasting.* Chicago, 1966.
[16]. Zarnowitz, Victor. *An Appraisal of Short-Term Economic Forecasts.* New York: National Bureau of Economic Research, 1967.