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Short-Run Forecasting Models Incorporating Anticipatory Data

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THIS paper represents a preliminary progress report on an attempt to determine the most reliable quantitative tools for short-term forecasting of the gross national product at the present stage of knowledge. Short-term forecasting is considered to cover time periods from about one quarter to one year ahead. The results we have been able to obtain so far are extremely limited, but we have decided to present them anyhow because we consider them useful even though incomplete.

On the basis of our reactions to the existing literature and to the state of the art, we started with the assumption—which we of course planned to test further—that a highly multiequational and multivariate econometric model is not likely at present or in the foreseeable future to give as satisfactory results for short-term forecasting as a simple model. There seemed to be little evidence that otherwise important niceties, such as production functions, demand-for-labor equations, labor market adjustment equations, interest rate and price equations, disaggregated consumption functions, or perhaps even distributive share and financial variables, added significantly to the short-term prediction of the gross national product and its major components. Moreover, it appeared plausible that a simple or small system of relationships would be associated with less proliferation of random and perhaps of systematic errors than a large or complex system. It might also be noted that with n equations in a model of income determination, and an average of m forms which can be fitted or tested per structural equation for each one finally selected, there are a truly impressive number of combinations possible if m and n are at all large,[1] whereas the best combination of a few simple equa-

NOTE: We are greatly indebted to Paul Taubman for his invaluable statistical assistance.

[1] For example, in a system of thirty equations and an average of ten forms per equation, there are, theoretically, 10^{30} possible combinations of the equations.

tions—which concentrate on a small number of basic rather than a large number of marginal variables—should be much more easily determined (and more easily tested). Similarly, it is much easier in a simple model to test the stability of parameters over time and to make any required changes.

This is not to say that eventually large-scale models of income determination may not be highly useful for understanding the broad dynamics of our economic system, for appraising the quantitative effects of changes in economic policies, and for forecasting the level and composition of the national income. We simply doubt that at least for short-run economic forecasting the large models have as yet proved themselves, though we propose to test this skepticism rather than leave it in this ex-cathedra form. Nor do we have any reason to expect that large-scale models will ever be superior to smaller systems for short-run forecasting. Thus, even in the constant dollar models discussed in this paper, it seems quite plausible that in the short run the supply of output accommodates itself to the demand for output. Therefore, production functions and demand-for-labor equations may be unnecessary in explaining short-term fluctuations in the gross national product. On the other hand, it is obvious that the same assumptions about flexibility in productive capacity, or about the existence of at least temporarily expansible capital stock and labor supply, cannot be made in the longer run.

The proof of all this is of course in the pudding, so long as it is a proper and not an ersatz pudding. The only really satisfactory test of the predictive ability of a model is obtained by comparing the forecasts made for some period ahead with the actual values for that period (adjusted, if necessary, for the conditional nature of certain forecasts and for changes in the basic data incorporated in the model). The degree of reliance placed on predictive performance would not only obviously increase with the number of periods so tested but also, normally, with the extent to which accuracy in over-all forecasts (e.g., the gross national product) is a result of accuracy in component forecasts (e.g., consumption, inventory investment, etc.). The difficulties of relying on *ex post* tests of predictive ability are well known. With sufficient resources and diligence, it should be possible to fit a structural equation (or a reduced form) in a model to past data reasonably well if enough forms are tried. Under such circumstances, the usual statistical tests of significance have serious limitations, and a chart or table showing the closeness of observed to predicted values may have no meaning at all. Even the common device of omitting some recent periods from the data used to derive the basic model, and then testing the predicted against the observed values for these periods, is not very satisfactory. If the model no longer "works" in this new situation, the analyst may make a few selective alterations—not to delude anyone, but to ensure that the model presented to the public is the best possible.

There are three additional points which should be made before considering the results presented in the rest of this paper. First, we have attempted a fairly comprehensive investigation of the utility of anticipatory data in the simple models of income determination on which we are concentrating our attention, though as yet only partial results are available. What may appear an excessive preoccupation with anticipatory or expectational data in areas where their performance in earlier studies has been less than remarkable (e.g., durables consumption and inventory investment) is due largely to the availability of new data. But it is also due to a feeling that plans, expectations, and the degree of fulfillment should be useful in short-run income determination, with different types of expectations likely to be useful for different time periods. Second, there is no presumption that the same forecasting model will perform best for various time periods ahead (e.g., the annual rate of activity one quarter, two quarters, or four quarters in the future) or for varying time intervals (one quarter, a half-year, or a year). This is true because of the heterogeneity in both the predictive time span of different variables (e.g., housing starts versus plant and equipment expenditure plans or *ex post* versus *ex ante* variables) and in the relative importance of changes in different components of the gross national product for shorter and longer periods of time. Furthermore, it should be pointed out that if the time interval covered by the forecast is short enough, purely random or nonpredictable elements may predominate. Third, no attention has been paid in this study to the comparative predictive performance of forecasting tools other than quantitative income models.

Some Recent Models

The most recent short-term model which has been made publicly available is a large-scale quarterly model by Lawrence R. Klein, which consists of twenty-nine structural relations plus some accounting identities and tax-transfer payments relations and is fitted to a sample of postwar observations terminating in 1958. This system makes use of some *ex ante* as well as *ex post* variables. The mimeographed announcement of the model itself is dated April 13, 1961; and the initial forecasts—covering the first three quarters of 1961—were publicly released on April 24. A more complete description and discussion of the model has been presented at this conference. It is not possible to tell without additional information and a great deal of work how much influence the different types of structural equations have on the estimated gross national product and whether they better or worsen the forecasting results. However, the first six structural demand equations—i.e., for consumer durables, consumer nondurables, consumer services, investment in plant and equipment, investment in housing, and investment in inventories—are likely to be particularly important in explaining fluctuations in GNP; and it is interesting to examine the respects in which they differ from simple models. First, consumption has been disaggregated into durables, nondurables, and services. There would seem to be good reason for the disaggregation of at least consumer durables from the rest of consumption.[2] In explaining each of these types of consumption, a nonlabor-to-labor income ratio has been added to the usual personal disposable income and lagged consumption variables. In addition, an index of consumer durable goods buying plans has been added to the durable goods equation; a lagged cash balance variable, to the other two consumption equations; and a population variable, to the services equation. Instead of a single lagged consumption variable, there are now three, one each for consumer durables, nondurables, and services. It is difficult to tell what has been gained by these complications either from a theoretical or empirical viewpoint. The coefficients of the nonlabor-to-labor income ratio would appear to be opposite in sign to those obtained by Klein in his more aggregative annual model and incorrect in sign if the cross-section results which he cites there are taken as the basis for justification of inclusion of such a variable into time series equations.[3] The cash balance variable is significant only in the services regression and is omitted completely from the durables regression, presumably because of an incorrect

[2] See Irwin Friend and Robert Jones, "The Concept of Saving," *Consumption and Saving*, Philadelphia, 1960, Vol. II.

[3] See Lawrence R. Klein and A. S. Goldberger, *An Econometric Model of the United States, 1929–1952*, Amsterdam, 1955.

sign, even though this seems to be the one place where a-priori considerations might suggest an important positive influence. The reason for the peculiar cash balance effects may be that the variable used includes corporate as well as personal balances (and corporate cash balances may be inversely correlated with cyclical conditions, while durables consumption is directly correlated); but the question of the justification for such a variable in the other two consumption equations remains. Nor do the regression coefficients of the lagged consumption variable in the three separate consumption equations seem to be as reasonable as the regression coefficient of lagged consumption in a composite consumption function. The separate equations imply surprisingly little difference between short-run and long-run income effects on services expenditure, a more pronounced (and more plausible) difference between these effects on nondurable expenditure, and an apparently strong tendency for the long-run income effect on durables expenditures to be considerably smaller than the short-run income effect. The size of the constant term in the services regression is also troublesome. Thus, there does not seem to be much theoretical justification for the complications introduced in the usual consumption function, and the statistics presented do not permit an evaluation of either the improvement in the goodness of fit obtained or in the reliability of forecasts made.

So far as the three investment equations are concerned, the plant and equipment regression is quite simple and reasonable, though as will be indicated later we feel it can be improved through the substitution of a form which might be considered even simpler. But we are more dubious about some of the complications introduced in the housing and inventory equations, particularly the use of number of marriages and interest rates as explanatory variables in the former and of change in prices in the latter. Again, however, we have no basis for evaluating the statistical improvement, if any, effected.

The record achieved in the second- and third-quarter forecasts for 1961 from the Klein model is presented, in Table 1, by comparing them with the actual values reported subsequently. The first-quarter projections, which are considerably closer to the actual reported values, are not considered here as forecasts in view of their timing and, therefore, are not shown.

It should be noted that the comparative-level values above are not so interesting as the respective changes, since the relevant official

TABLE 1

COMPARISON OF FORECAST AND ACTUAL VALUES, KLEIN QUARTERLY MODEL, 1961 (billions of 1954 dollars except GNP)[a]

	Second Quarter				Third Quarter			
	Forecast		Actual		Forecast		Actual	
	Level	Change[b]	Level	Change	Level	Change	Level	Change
Durable consumption	43.4	2.0	39.8	2.2	47.9	4.5	39.9	0.1
Nondurable consumption	145.4	1.5	142.6	1.0	148.2	2.8	144.5	1.9
Services consumption	116.9	1.4	119.2	1.4	118.7	1.8	120.6	1.4
Residential construction	18.2	0.5	17.6	1.1	19.9	1.7	18.7	1.1
Private plant and equipment	35.4	0.0	36.9	0.6	36.3	0.9	37.8	0.9
Inventory investment	−2.7	1.7	2.9	6.1	0.7	3.4	3.9	1.0
GNP (current prices)	508.8	10.0	516.1	15.3	528.2	19.4	525.8	9.7

[a] Seasonally adjusted at annual rates.

[b] These are changes from preceding-quarter forecasts, which in I-1961 are fairly close to actual values.

national accounts statistics were revised slightly in July 1961,[4] subsequent to these forecasts. Even the earlier national income statistics do not seem to correspond precisely to the base figures used in this model. A comparison of actual and forecast changes in GNP points to a significant understatement of the recovery in the second quarter and an even larger overstatement of the recovery in the third quarter. As a result, the two quarterly forecasts of GNP are not too impressive (though such a statement implies that other methods are available which are at least as reliable or otherwise more appealing); but the change for the two quarters combined is reasonably close to the reported values. It is interesting to observe that simply taking the average increase in GNP in the first and second recovery quarters of all earlier postwar cycles ($5.6 billion and $9.7 billion, respectively, in 1961 dollars) results in a less reliable forecast for the change in the second quarter and a more reliable forecast in the third quarter of 1961. (If these "naïve" forecasts are adjusted for the difference between the average value of government purchases and exports in past recovery quarters and in the relevant 1961 periods, the "predicted" increases in GNP in the second and third quarters of 1961 would

[4] The official statistics were further revised in July 1962. In terms of changes, the new data indicate that Klein's second-quarter forecast was somewhat better than shown here; his third-quarter forecast was slightly poorer; and for the two quarters combined, the forecasted change shows greater deviation from the actual values.

amount to $6.5 billion and $11.4 billion, respectively, without any allowance for further multiplier effects.)

A question which immediately arises, of course, is whether the assumptions made in the essentially conditional Klein forecasts—viz., a steady growth in the government sector, exports steady, and prices almost steady, etc.—account for a major share of the discrepancies between the forecast and actual values. By comparison of the assumed and actual values (available after the forecast was made) of the exogenous variables, and by consideration of the relevant structural coefficients, it is possible to estimate roughly the impact on the forecasts of errors in the assumptions regarding the exogenous variables.[5] The changes from the first to the second quarter in the exogenous variables (the ratio of wage to other personal income, consumer buying plans index, cash balances, population, anticipated plant and equipment outlays, marriages, long-term interest rate, and lagged housing starts) appearing in the consumption and fixed investment equations were rather consistently overestimated; i.e., the second-quarter levels were placed too high. Thus, in view of the generally positive structural coefficients involved, the forecasted changes (in constant dollars) in consumption and fixed investment were too large. Of the other elements affecting the estimation of the gross national product, the major exogenous variables were government expenditures, exports, and the government wage bill (all in current dollars) as well as the set of appropriate price indexes for consumption, investment, and private GNP. Errors in the assumed changes from the first to the second quarter in government expenditures, exports, and the government wage bill were largely offsetting. On the other hand, the assumed constancy of prices (in all but one case) generally represented an understatement of price changes and thus served as a mild offset to the overstatement in real consumption and investment noted above. On balance, it would appear that a "corrected" forecast of change in GNP from the first to the second quarter would result, if anything, in an even greater error than appears in Klein's original estimate. From the second to the third quarter, in contrast, the errors in the assumed change in exogenous variables ran mainly to understatement. The changes in the exoge-

[5] One could, of course, solve the system of equations, substituting the actual values of exogenous variables. Apart from obvious problems associated with the size of the model and data revision, the problems of duplicating Klein's procedures, involving adjustments and selections of predetermined variables, precluded any attempt at "complete" solution.

nous variables appearing in the consumption and fixed investment equations were for the most part understated; the changes in government expenditures plus exports were understated by about $2 billion (current dollars, annual rates); and the changes in prices were again understated. There was little error in the estimate of the change in the government wage bill. Had the actual changes in the exogenous variables been used in the third quarter, therefore, it appears that the forecasted change in GNP would have shown an even larger overstatement.

Thus, the substitution of actual for assumed values of the exogenous variables in the Klein model apparently would have resulted in worse rather than better forecasts for the second and third quarters of 1961 (though not necessarily for the two quarters combined). If the major components of GNP which were forecast separately are examined, it appears that the chief deficiency of the model was in the estimate of inventory investment (and, to a lesser extent, other investment) in the second quarter and in the estimate of durables consumption and inventory investment (and, to a lesser extent, every major component of GNP other than plant and equipment) in the third quarter.

Clearly it is much too early to appraise this model adequately. An appraisal by Arthur Okun of the earlier Klein-Goldberger annual econometric model (see note 6) concludes that the model performed well for 1953 and 1954 but not for 1955, 1956, and 1957 as compared to forecasts by business economists generally.[6] The years of relatively poor performance, not surprisingly, are those appreciably beyond the period on which the model was based.

Another recent, moderately large-scale quarterly model was published by James S. Duesenberry, Otto Eckstein, and Gary Fromm in *Econometrica*,[7] October 1960, though completed considerably earlier. The model contains fourteen equations depicting the interrelationships of the items involved in going from gross national product to disposable income. This system, which is fitted to a sample of observations terminating in 1957, with some observations for 1958 available before the article was completed, is somewhat less complex than the Klein model—with fixed investment as well as government pur-

[6] Arthur M. Okun, "A Review of Some Economic Forecasts for 1955–57," *Journal of Business*, July 1959. Okun points out, however, that Daniel B. Suits used this model to predict 1956 with comparative reliability by adjusting the initial 1956 forecast of the model by the amount of the rather substantial 1955 error.

[7] "A Simulation of the United States Economy in Recession."

chases and net exports determined outside the system—has a more specialized nonforecasting purpose, and makes no use of expectational data. For our purposes, it would seem proper to compare the quarterly forecasts implicit in the model and the actual values of changes in GNP for at least 1960 and 1961, except for one complication. These equations were fitted to the official national accounts data prior to their substantial revision in late 1958;[8] so a significant part of the discrepancies between the forecast and actual data may reflect the revision of the basic data rather than pure forecasting deficiencies of the model. As a result, the comparative data will not be presented; but it may be noted that the discrepancies between forecast and actual changes in GNP seem quite substantial, largely reflecting the inventory function used but also, at times, reflecting the consumption function. Thus, of the seven 1960–61 quarters tested, the change in consumption seemed to be overstated by close to $7 billion in the third quarter of 1960 and understated by a somewhat larger amount in the second quarter of 1961, with these two aberrant quarters immediately following cyclical turning points.

The last quarterly model to which we shall refer was presented by Lowell E. Gallaway and Paul E. Smith in the *Journal of the American Statistical Association* in June 1961.[9] Like the Duesenberry-Eckstein-Fromm relationships, the Gallaway and Smith model is fitted to observations terminating in 1957, with observations for 1958 also available before the article was completed; so again the implicit quarterly forecasts and actual values for 1960 and 1961 may be compared. This model is extremely simple, involving essentially only a consumption function, an aggregative gross private domestic investment function, and a composite government expenditure and net foreign investment relation (there are, therefore, no autonomous expenditures), and is based entirely on *ex post* lagged variables. Though only five explanatory variables—all in change form—are involved in the prediction of change in gross national product, viz., disposable income, money supply, prior change in gross national product, property income before taxes, and government expenditures on goods and services plus net foreign investment, the model is surprising in the amount of importance it apparently gives to the money supply and property income. A comparison of the quarterly actual and predicted changes for 1960 and 1961 is presented in Table 2, but it

[8] See *U.S. Income and Output*, Department of Commerce.
[9] "A Quarterly Econometric Model of the United States."

TABLE 2

Comparison of Forecast and Actual Changes in GNP, Gallaway-Smith Quarterly Model, 1960–61
(billions of current dollars)

	1960				1961		
	I	II	III	IV	I	II	III
Forecast	7.0	7.6	1.6	.2	2.3	0.7	16.7
Actual	13.2	4.9	−1.3	−.6	−3.7	15.3	9.7

should be noted again that the changes in the official national income statistics subsequent to the completion of this model may affect somewhat the original regression coefficients which have been used for projection purposes (though not to a serious degree, since the basic national accounts data used were apparently obtained from *U.S. Income and Output*). The record of performance for this period seems rather poor, especially for the second quarter of 1961 (and is not particularly improved if the actual values of government expenditures and net exports are substituted for the forecast values). The performance was somewhat better on a semiannual than on a quarterly basis, which is consistent with the results obtained from the Klein and the Duesenberry-Eckstein-Fromm models.

Some Preliminary Short-Term Relationships

That inventory investment plays a key role in short-run cyclical movements in the gross national product or generally in the type of relatively minor cycles experienced since the end of World War II will come as a surprise to no one. However, it is of interest to investigate just how important fluctuations in inventory investment have been as compared to fluctuations in other major types of investment expenditure. A rough simple way of doing this is to relate the quarterly changes in gross national product to changes in total investment plus government expenditures, ΔZ, and then separately to changes in this total less changes in residential construction, $\Delta(Z - H)$; less changes in plant and equipment expenditure, $\Delta(Z - PE)$; and, finally, less changes in inventory investment, $\Delta(Z - I)$, all seasonally adjusted at annual rates in billions of 1954 dollars. These results are then supplemented by adding in appropriate form what we consider to be the most relevant additional variables for forecasting plant and equipment expenditures, viz., such expenditures anticipated a quarter earlier (PE^e), and residential construction, viz., housing starts in hundreds of thousands of units, a quarter earlier (HS_{-1}), to those relationships in which the current value of these variables is no longer assumed known. The computed regression equations for the period from the third quarter of 1951 to the fourth quarter of 1960 (thirty-seven observations) are shown below, together with the adjusted coefficients of determination, standard errors of the regression line, and standard errors of the regression coefficients. This period is selected in view of the timing of the significant rise in the personal saving–disposable income ratio (i.e., decline in the consumption ratio as measured in the national accounts) from the earlier, relatively depressed, post-World War II level to a new and, comparatively stable, higher level, which suggests that for short-term projection it may be safer to restrict the usual assumptions relating to stability of structural relationships (or at least of the short-run consumption function) to the period starting with mid-1951.

The following four regressions point to the particularly important role played by inventory investment in the quarterly fluctuations of gross national product.

(1) $$\Delta Y = 2.11 + \underset{(0.08)}{1.39}\Delta Z \qquad \bar{R}^2 = 0.90 \quad \bar{S} = 1.8$$

(2) $$\Delta Y = 2.31 + \underset{(0.10)}{1.45}\Delta(Z - H) \qquad \bar{R}^2 = 0.86 \quad \bar{S} = 2.2$$

(3) $$\Delta Y = 2.22 + \underset{(0.14)}{1.37}\Delta(Z - PE) \qquad \bar{R}^2 = 0.71 \quad \bar{S} = 3.1$$

(4) $$\Delta Y = 1.87 + \underset{(0.34)}{1.13}\Delta(Z - I) \qquad \bar{R}^2 = 0.22 \quad \bar{S} = 5.1$$

Without positing any direction of causation, it is interesting to observe that changes in total consumption statistically contribute much less to changes in gross national product than changes in investment, of which inventory investment is by far the most important. Thus,

(5) $$\Delta Y = -2.19 + \underset{(0.22)}{2.11}\Delta C \qquad \bar{R}^2 = 0.72 \quad \bar{S} = 3.0$$

Incidentally, there is virtually no observable simple relationship between short-term changes in gross national product and those in government expenditures plus net exports.

The above relationships are only slightly modified if the regressions are computed for the period from the second quarter of 1953 to the fourth quarter of 1960 (thirty-one observations). This shorter span, in addition to reflecting a period of comparatively stable savings-income ratios, is selected to minimize the possible economic irregularities associated with the Korean War. For the shorter period the regressions are:

$$(6) \qquad \Delta Y = 2.07 + \underset{(0.08)}{1.40}\Delta Z \qquad \bar{R}^2 = 0.92, \quad \bar{S} = 1.7$$

$$(7) \qquad \Delta Y = 2.24 + \underset{(0.10)}{1.47}\Delta(Z - H) \qquad \bar{R}^2 = 0.88, \quad \bar{S} = 2.2$$

$$(8) \qquad \Delta Y = 2.15 + \underset{(0.14)}{1.46}\Delta(Z - PE) \qquad \bar{R}^2 = 0.77, \quad \bar{S} = 2.9$$

$$(9) \qquad \Delta Y = 1.81 + \underset{(0.40)}{1.59}\Delta(Z - I) \qquad \bar{R}^2 = 0.33, \quad \bar{S} = 4.9$$

$$(10) \qquad \Delta Y = -2.47 + \underset{(0.23)}{2.23}\Delta C \qquad \bar{R}^2 = 0.75, \quad \bar{S} = 3.0$$

It can be seen that with one possible exception the regression and correlation coefficients show no appreciable change from those computed for the longer period. The coefficient of $\Delta(Z - I)$ is higher, but not significantly so, for the shorter period; and the $\bar{R}^2$ for this relationship is somewhat higher, as it is also for the other relationships. Again, the results suggest the predominant importance of a satisfactory inventory investment relationship in predicting quarterly changes in gross national product.

Since quarterly data may be subject to large random errors (including estimating errors in the national accounts) which are essentially unpredictable at the present stage of our knowledge, it is desirable to extend all our analyses to semiannual and annual intervals. These results, which cover eighteen observations for the semiannual data and nine observations for the annual data, follow. The semiannual regressions are:

$$(11) \qquad \Delta Y = 4.12 + \underset{(0.08)}{1.52}\Delta Z \qquad \bar{R}^2 = 0.96, \quad \bar{S} = 2.0$$

$$(12) \qquad \Delta Y = 4.56 + \underset{(0.13)}{1.60}\Delta(Z - H) \qquad \bar{R}^2 = 0.90, \quad \bar{S} = 2.9$$

$$(13) \qquad \Delta Y = 4.30 + \underset{(0.20)}{1.65}\Delta(Z - PE) \qquad \bar{R}^2 = 0.80, \quad \bar{S} = 4.2$$

$$(14) \qquad \Delta Y = 3.59 + \underset{(0.49)}{1.55}\Delta(Z - I) \qquad \bar{R}^2 = 0.35 \quad \bar{S} = 7.6$$

$$(15) \qquad \Delta Y = -5.58 + \underset{(0.21)}{2.40}\Delta C \qquad \bar{R}^2 = 0.88 \quad \bar{S} = 3.3$$

These semiannual relationships have higher correlations and lower relative standard errors ($\bar{S}$ divided by the mean absolute value of ΔY) with not too much change in the comparative importance of fluctuations in the different components of gross national product. The corresponding annual regressions are:

$$(16) \qquad \Delta Y = 7.48 + \underset{(0.12)}{1.65}\Delta Z \qquad \bar{R}^2 = 0.96 \quad \bar{S} = 2.6$$

$$(17) \qquad \Delta Y = 8.45 + \underset{(0.25)}{1.62}\Delta(Z - H) \qquad \bar{R}^2 = 0.84 \quad \bar{S} = 5.0$$

$$(18) \qquad \Delta Y = 7.47 + \underset{(0.39)}{2.02}\Delta(Z - PE) \qquad \bar{R}^2 = 0.77 \quad \bar{S} = 6.1$$

$$(19) \qquad \Delta Y = 7.52 + \underset{(0.65)}{1.28}\Delta(Z - I) \qquad \bar{R}^2 = 0.27 \quad \bar{S} = 10.8$$

$$(20) \qquad \Delta Y = -7.37 + \underset{(0.31)}{2.07}\Delta C \qquad \bar{R}^2 = 0.84 \quad \bar{S} = 5.0$$

These annual relationships have correlation coefficients about the same as, or even lower than, the semiannual, though the relative standard errors are somewhat further reduced. Again, there is not much change in the comparative importance of fluctuations of the major categories of investment in explaining fluctuations in gross national product, pointing to the critical role played by inventory investment even in annual changes in over-all economic activity in the postwar period. Also, even on an annual basis, knowledge of the change in total investment plus government expenditures, viz., ΔZ, permits a comparatively reliable estimate of the change in gross national product. This is significantly more reliable than the corresponding estimate associated with knowledge of the change in the much larger figure for total consumption.

It may be noted that adding housing starts (HS_{-1}) and anticipated plant and equipment expenditures (PE^e) to the corresponding quarterly regressions of gross national product on total investment less housing (equations 2 and 7) and on total investment less plant and equipment expenditures (equations 3 and 8) contributes significantly to the explanation of changes in gross national product. For the longer period (thirty-seven observations) the new equations are:

$$(21)\qquad \Delta Y = 2.30 + \underset{(0.09)}{1.41}\Delta(Z - H) + \underset{(0.31)}{1.41}\Delta HS_{-1} \qquad \bar{R}^2 = 0.89 \quad \bar{S} = 1.9$$

and

$$(22)\qquad \Delta Y = 1.71 + \underset{(0.13)}{1.31}\Delta(Z - PE) + \underset{(0.46)}{1.44}\Delta PE^e \qquad \bar{R}^2 = 0.77 \quad \bar{S} = 2.7$$

and for the shorter period (thirty-one observations):

$$(23)\qquad \Delta Y = 2.28 + \underset{(0.09)}{1.43}\Delta(Z - H) + \underset{(0.43)}{1.28}\Delta HS_{-1} \qquad \bar{R}^2 = 0.90 \quad \bar{S} = 1.9$$

and

$$(24)\qquad \Delta Y = 1.49 + \underset{(0.11)}{1.42}\Delta(Z - PE) + \underset{(0.47)}{2.12}\Delta PE^e \qquad \bar{R}^2 = 0.86 \quad \bar{S} = 2.2$$

where ΔPE^e is anticipated plant and equipment expenditures in quarter t (anticipated a quarter earlier) less actual expenditures in quarter $t - 1$; and the subscript -1 is used in the equations as a shorthand expression for $t - 1$.

Some Short-Term Predictive Models

The maximum amount of information about prospective economic activity which is normally assumed known in forecasting models is government expenditures and exports. The first of these variables (or at least the federal government share) may be regarded as a control variable giving rise to conditional forecasts, and no one to our knowledge has yet had much success in projecting the second (viz., exports). We have, therefore, tested a number of very simple quarterly, semiannual, and annual models in which only government expenditures and, for convenience, net exports are assumed known. Since certain promising bodies of anticipatory data are of recent vintage, and do not cover the entire period for which the basic models have been computed, we shall whenever possible relate residuals from the basic relationships to these anticipatory data in an attempt to determine whether they add to predictive ability.

The best simple set of "structural" equations which we have been able to derive so far for explaining quarterly changes in the total and major components of gross national product over the period from the beginning of 1953 through the end of 1960 is the following:

$$(25)\qquad \Delta C = \underset{(.41)}{1.04} + \underset{(.07)}{.30}\Delta\tilde{Y} + \underset{(.15)}{.16}\Delta C_{-1} \qquad \bar{R}^2 = 0.50 \quad \bar{S} = 1.67$$

$$(26)\qquad \Delta H = \underset{(.09)}{.14} + \underset{(.12)}{.55}\Delta HS_{-1} + \underset{(.12)}{.28}\Delta H_{-1} \qquad \bar{R}^2 = 0.60 \quad \bar{S} = 0.47$$

$$(27)\qquad \Delta PE = \underset{(.13)}{-.39} + \underset{(.17)}{.94}\Delta PE^e + \underset{(.015)}{.033}(\Delta\tilde{Y} + \Delta Y_{-1}) \qquad \bar{R}^2 = 0.71 \quad \bar{S} = 0.65$$

$$(28)\qquad \Delta I = \underset{(.58)}{1.96} + \underset{(.053)}{.075}(\Delta Y_{-1} + \Delta Y_{-2}) - \underset{(.16)}{1.02}I_{-1} + \underset{(.19)}{.69}\Delta O_{-1} + \underset{(.72)}{1.77}\Delta PE^e \qquad \bar{R}^2 = 0.59 \quad \bar{S} = 2.08$$

The identity below completes the model:

$$(29)\qquad \Delta Y = \Delta C + \Delta H + \Delta PE + \Delta I + \Delta G'$$

where the new symbols are G', government expenditures plus net exports, and O, unfilled orders. Changes with the subscript -1 (i.e., $t-1$) are measured from $t-2$; and all variables are again seasonally adjusted at annual rates in billions of 1954 dollars[10] except for HS, which is in hundreds of thousands of units, as previously noted. The regression coefficients in equations 25 and 27 have been estimated by a two-stage least-squares procedure. A $\Delta\tilde{Y}$ indicates that computed values (from a reduced form discussed subsequently) have been used instead of actual values. No ΔY term has been used in equation 26, since it adds nothing statistically and in a quarterly form may not be needed on a priori grounds once HS_{-1} is included.[11]

The $\Delta\tilde{Y} + \Delta Y_{-1}$ term in equation 27 gives significantly better results than either alone and has the theoretical justification that it assumes that income over a longer time span than a single quarter is relevant to plant and equipment expenditure decisions. It seems to have an informational content not contained in ΔPE^e; and it may be noted that attempts to use accelerator terms (such as $\Delta^2 Y$, $\Delta^2 Y_{-1}$, and more complex accelerators involving average in-

[10] Unlike the other variables deflated by the appropriate price indexes drawn from the national accounts, O (and in later regressions the level of sales S and the level of inventories L) were deflated by the wholesale price index, omitting farm products and food, converted to a 1954 base. Deflation by the wholesale price index for all commodities produced no significant differences in the estimated coefficients.

[11] It may be worth noting that the $\bar{R}^2$ presented for change forms would be expected to be substantially lower than for the corresponding level forms; so in comparisons of change and level forms, attention should be directed to the standard errors.

comes) did not help nearly so much as the income term used, even though some of these accelerator terms may also serve as a proxy for unanticipated sales. Attempts to substitute or add other explanatory variables, such as beginning-of-period capacity utilization, lagged profits, and ΔPE_{-1}; or to substitute $PE^e - PE^e_{-1}$ for $\Delta PE^e = PE^e - PE_{-1}$, did not give as satisfactory results.[12] Nor did a direct measure of unanticipated manufacturing sales help to explain residuals from equation 27 for the short period (starting with the third quarter of 1959) for which such data were available.

The $\Delta Y_{-1} + \Delta Y_{-2}$ term in equation 28 again gives better results than either income alone and relates inventory investment to an average of two quarters' income. The substitution of business sales for income gave about the same results; so income was utilized to simplify the model. Lagged change in inventory investment did not help at all. The lags used were obtained by empirical investigation.[13] The explanatory variables I_{-1} and O_{-1} are obvious candidates for inclusion both on theoretical grounds (particularly I_{-1}) and on the basis of earlier studies. However, PE^e is included as a proxy for business expectations (which to some extent of course is already reflected in O_{-1}) and for direct evidence on a type of expenditure which may be associated with inventory investment.

All of the signs of the regression coefficients in these structural equations are in accordance with theoretical expectations; and given the standard errors, even the magnitudes of the coefficients are not too unreasonable. The housing, plant and equipment, and inventory equations give surprisingly good fits for such quarterly relations, though they provide comparatively small lead times for forecasting purposes. The housing equation is further deficient as a structural relation in view of its omission of income and housing stock variables, neither of which was useful. The consumption function, however, is more disappointing; but a number of attempts to improve it did not help, including the substitution of the more theoretically correct disposable income or of more complicated averages of current and past incomes for Y and the addition of cyclical variables.

The residuals from the consumption and inventory equations (equations 25 and 28) were related to two other major bodies of an-

[12] Further work is now in process, substituting a $PE^e - PE^e_{-1}$ for the ΔPE^e variable and using first anticipations data in lieu of second anticipations.

[13] A $\Delta^2 O_{t-1}$ term was also tested in lieu of ΔO_{t-1}, but did not seem to give as good results. Cf. Paul G. Darling, "Manufacturers' Inventory Investment, 1947–1958," *American Economic Review*, December 1959, pp. 950–963.

ticipatory data which have recently been made publicly available—the Federal Reserve Board–Census Bureau quarterly series on anticipated expenditures on consumer durables and the Department of Commerce quarterly series on anticipated inventory investment. In view of the presumed greater importance of explaining inventory fluctuations, for reasons which have been discussed, the comparative absence of previous work in integrating inventory anticipations into models of income determination, and our own time limitations, we have spent more time on testing the utility of inventory than of consumer anticipations.[14] However, the results in both areas are generally negative so far as the explanation of residuals from equations 25 and 28 (or from similar equations tested) is concerned. The residuals from the inventory equation were related to changes in reported and corrected first and second inventory anticipations[15] for each quarter from the third quarter of 1959, when the series started, to the fourth quarter of 1960. The resulting regression was then used to determine whether the information on inventory anticipations helped in the "prediction" of inventory investment (or, more precisely, in the reduction of the residuals between actual change in inventory investment and that estimated from equation 28) in each of the four quarters of 1961 and the first quarter of 1962. Unfortunately, while the correlation between inventory residuals and changes in inventory anticipations is fairly impressive[16] ($\bar{R}^2 = 0.51$, 0.38, 0.84, and 0.54 for the reported and corrected first anticipations and the corresponding second anticipations, respectively), the inventory anticipations data do not seem to help at all in the "prediction" of the quarters noted for 1961 and 1962. On the other hand, the prospects for future utility of inventory anticipations data[17] are enhanced by these posi-

[14] We have not tested at all the utility of the consumer anticipations data compiled by the University of Michigan Survey Research Center, since they are collected only about twice a year, are not conveniently available, and have been and are being analyzed elsewhere.

[15] See Murray F. Foss, "Manufacturers' Inventory and Sales Expectations: A Progress Report on a New Survey," *Survey of Current Business*, August 1961, and later issues of the *Survey* for the basic data used. The data are seasonally adjusted and were further price deflated.

[16] The changes in inventory anticipations are the $\Delta I^{2e} = I^{2e} - I_{-1}$, and $\Delta I^{1e} = I^{1e} - I^{2e}_{-1}$ data reported by Commerce, where the superscript 2*e* refers to the second anticipation and 1*e* to the first anticipation. The attempted deflation of the reported data made the results worse. Further work is now in process, substituting the corresponding $I^e - I^c_{-1}$ and also I^e alone for ΔI^e.

[17] The Department of Commerce finds that inventory anticipations explain short-run fluctuations in inventory investment somewhat better than a regression equation not unlike the one used here, except for the expectational term (see "Factors Influencing

tive correlations between changes in the inventory residuals and in the anticipations series and, also, by the positive simple correlations between changes in inventory investment and in at least the second corrected anticipations series for the entire period from the third quarter of 1959 through the first quarter of 1962 ($\bar{R}^2 = 0.01$ and 0.71 for the corrected first and second anticipations). Surprisingly, there is no indication in this analysis, admittedly covering a very short period of time, that the corrected inventory anticipations data perform any better than the reported anticipations data in explaining deviations from the regressions which relate inventory behavior to *ex post* variables. On the other hand, without *ex post* data, the predictive value of the second anticipations seems to be definitely superior to the first anticipations.

A similar analysis was made of the relationship between the residuals from the computed consumption relation (equation 25) and the Federal Reserve–Census series, starting with the second quarter of 1959, on plans to buy automobiles within twelve months (as reported two months before the quarter in question).[18] The results here were less promising than for the inventory anticipations (with an $\bar{R}^2$ of zero and no help in the "prediction" of consumption in the four quarters of 1961 and the first quarter of 1962).

The reduced form for ΔY which was used to obtain the structural relations presented above is:[19]

Manufacturers' Inventories," *Inventory Fluctuations and Economic Stabilization*, Joint Economic Committee, 87th Cong., 1st sess., December 1961, Part I). However, the Commerce analysis is confined to manufacturing inventories alone (since trade and other inventory anticipations are not yet available) and relates to change in book value rather than to inventory investment as measured in the national accounts. We have attempted to construct a composite inventory structural equation utilizing anticipations for the manufacturing sector and the *ex post* regression for the trade (and other) sectors, so far without much success.

Another *ex post* inventory form, recently derived by Paul G. Darling (*Inventory Fluctuations and Economic Stabilization*, Part III), does not appear to offer any significant improvement in fit or in predictive ability over equation 28. This form, recomputed for the period from the third quarter of 1951 through the fourth quarter of 1960, yields the following result:

$$I_i = 19.41 + .26S_{t-1} + .04O_{t-1} + .51\Delta O_{t-1} - .74L_{t-1} + .13I_{\substack{i \\ t-1}} - .00009t \qquad \bar{S} = 1.95$$

where L is the level of inventories and t is a linear time trend variable with 1951-III = 0. It is quite possible that the deflators we used, as well as the different time period, account for the somewhat poorer results we obtained with this form as compared with Darling's findings.

[18] See "Quarterly Survey of Consumer Buying Anticipations," *Federal Reserve Bulletin*, May 1961, and later issues of the *Bulletin* for the basic data used. The twelve-month anticipation data were used in an attempt to avoid problems of seasonality.

[19] The *ex post* reduced form for ΔY obtained by solving the structural equations is:

$$\Delta Y = 4.03 + 1.50\Delta G' + 4.06\Delta PE^e + .16\Delta Y_{-1} + .11\Delta Y_{-2} - 1.53I_{-1} + 1.04\Delta O_{-1} + .24\Delta C_{-1} + .83\Delta HS_{-1} + .41\Delta H_{-1}$$

$$(30)\quad \Delta Y = \underset{(1.36)}{4.91} + \underset{(.47)}{.75\Delta G'} + \underset{(1.42)}{4.11\Delta PE^e} + \underset{(.26)}{.53\Delta Y_{-1}} - \underset{(.36)}{1.55 I_{-1}} + \underset{(.37)}{.94\Delta O_{-1}} - \underset{(.63)}{.36\Delta C_{-1}} - \underset{(1.29)}{.25\Delta HS_{-1}} + \underset{(1.16)}{1.07\Delta H_{-1}} \qquad \bar{R}^2 = 0.71 \quad \bar{S} = 3.69$$

A ΔY_{-2} term was tested but not used, since the results were somewhat less satisfactory, probably reflecting a relatively high intercorrelation with ΔY_{-1}. Again, attempts to add other explanatory variables, which we discussed in connection with the structural equations, and also to improve the fit by breaking down the entire period into recession and nonrecession periods separately, were not particularly successful.[20] The inventory anticipations help very little and the consumer plans moderately well in explaining deviations between actual GNP and that implied by equation 30 in the "base" period, thus reversing their roles in the relevant structural equations; but, again, neither helps at all in the "forecast" period.

The signs of the coefficients of all the variables in equation 30 are in accordance with theoretical expectations with the exception of ΔC_{-1} and ΔHS_{-1}, which are not statistically significant and are retained because they help considerably and have the correct signs in the structural equations. The unsatisfactory results yielded by ΔC_{-1} and ΔHS_{-1} in the reduced form equation for ΔY are probably explained by the relatively high intercorrelations of ΔC_{-1} with ΔY_{-1} and of ΔHS_{-1} with ΔH_{-1} and of both with the other variables included in the regression. Nevertheless, the standard errors of most of these coefficients are quite high and the magnitudes of the coefficients vary considerably in the different forms tested, with that of ΔC_{-1} generally positive if ΔY_{-1} is omitted and that of ΔHS_{-1} generally positive if ΔH_{-1} is omitted. All the explanatory variables combined are able to explain about seven-tenths of the variance in the quarterly changes in gross national product. Of the six cyclical turning points in the fitted period, the model using computed rather than actual values for the lagged endogenous variables correctly "predicted" five and led one by a quarter (the 1960 downturn).[21] The

[20] However, the adjusted standard error of the regression for the recession period was appreciably smaller than for the period as a whole.

[21] The use of computed rather than actual values for the endogenous variables improved both the "predictions" in the base period and the actual predictions discussed below, a result consistent with expectations if the model correctly represents the true structure and if the serial correlation of the endogenous variables is less troublesome than the random errors. The use of such computed values also is a first step in increasing the forecasting lead time of the model.

TABLE 3

COMPARISON OF FORECAST AND ACTUAL VALUES, QUARTERLY, 1961 AND 1962
(billions of 1954 dollars)

	First Quarter 1961		*Second Quarter 1961*		*Third Quarter 1961*		*Fourth Quarter 1961*		*First Quarter 1962*		*Second Quarter 1962*	
	Predicted	Actual	Predicted	Actual	Predicted	Actual	Predicted	Actual	Predicted	Actual	Predicted	Actual
ΔC	1.0	−0.6	3.2	4.3	3.8	3.5	5.7	4.6	3.1	3.3	2.8	3.0
ΔH	−0.2	−1.3	0.3	0.9	0.7	1.4	0.7	0.9	0.3	−1.5	−0.2	1.7
ΔPE	−1.5	−2.1	0.2	0.5	1.0	0.9	1.3	1.1	0.5	0.1	0.8	1.4
ΔI	−2.8	−2.3	3.3	5.0	2.2	1.5	1.5	1.9	−1.3	0.5	1.1	−2.2
ΔY[a]	−0.6	−3.8	6.6	10.0	7.5	6.5	13.2	13.0	3.9	4.0	4.3	3.4

NOTE: All actual figures are as reported in *Survey of Current Business*, July 1962, and in preliminary release OBE 62–69 of GNP data for the second quarter of 1962.

[a] Sum of individual components including actual figures for $\Delta G'$.

model did not clearly signal any false turn, though on one occasion it showed an insignificant decline when the actual change was an insignificant increase.

The "predictions" from equations 25–29 for the four quarters of 1961 and the first two quarters of 1962 are presented in Table 3. They were obtained by substituting computed rather than actual values for the endogenous (lagged as well as current) explanatory variables in these relationships.[22]

The forecasts as a whole seem better than those obtained from the more complex models, though this conclusion is highly tentative, particularly because the lead times involved in this comparison are comparatively small and because these forecasts must be considered largely *ex post* predictions. For GNP as a whole a significant overstatement in the first quarter of 1961 is associated with a corresponding understatement in the following quarter, and the directions of movement are correct in both quarters. The GNP forecasts for the next four quarters are remarkably good. The most significant prediction errors in the components of GNP consist of the errors for consumption in the first quarter of 1961 and for inventory investment and home construction in the first and second quarters of 1962. The underestimation of inventory investment in early 1962 probably reflects the anticipated steel strike, and this is more than offset by the overestimate in the second quarter. The first- and second-quarter errors for home construction are also offsetting, which may partly reflect deficient seasonal adjustments in either the actual or predicted

[22] It may be noted that the computed values used included I_{-1}.

values.[23] We have not examined the possibility of other "abnormal" developments which might help to explain the remaining sizable errors. Of the major components, the least satisfactory forecasts are those for housing, largely reflecting the unsatisfactory nature of recent housing starts data; and further work on this relationship is under way, including the incorporation of data on new housing permits and other anticipatory housing series.

Since at least part of our lack of success in explaining quarterly fluctuations in the gross national product and its components may be attributable to erratic changes (including errors of observation) which are to some extent offsetting over longer periods of time, it is quite conceivable that significantly better explanatory and predictive results might be obtained by lengthening the time unit of analysis from a quarter to half a year. Semiannual relationships based on calendar half-years were fitted for the period from the second half of 1951 through 1960 (we went all the way back to 1951 so as to increase the number of observations—to eighteen). The semiannual relationships for explaining changes in the total and major components of gross national product generally turn out to be an improvement over the corresponding quarterly relationships, which is particularly noteworthy in view of the substantially longer lead times involved in semiannual "forecasts."[24] The best structural relationships obtained on a semiannual basis are given by equations 31–34:

$$\Delta C = \underset{(.71)}{1.95} + \underset{(.05)}{.38}\Delta\tilde{Y} + \underset{(.12)}{.14}\Delta C_{-1} \qquad \bar{R}^2 = 0.77,\ \bar{S} = 1.82 \tag{31}$$

$$\Delta H = \underset{(.15)}{.28} + \underset{(.22)}{1.06}\Delta HS_{-1/2} + \underset{(.012)}{.037}(\Delta\tilde{Y} - \Delta Y_{-1}) - \underset{(.09)}{.16}\Delta PE^{e} \qquad \bar{R}^2 = 0.76,\ \bar{S} = 0.58 \tag{32}$$

$$\Delta PE = \underset{(.39)}{-.81} + \underset{(.26)}{.57}\Delta PE^{e} + \underset{(.032)}{.072}(\Delta Y + \Delta Y_{-1}) \qquad \bar{R}^2 = 0.64,\ \bar{S} = 1.21 \tag{33}$$

$$\Delta I = \underset{(1.18)}{.77} + \underset{(.09)}{.12}(\Delta\tilde{Y} + \Delta Y_{-1}) - \underset{(.27)}{.94}I_{-1} + \underset{(.83)}{1.21}\Delta PE^{e} \qquad \bar{R}^2 = 0.44,\ \bar{S} = 3.24 \tag{34}$$

[23] Offsetting first- and second-quarter errors for 1961 as well as 1962 are found for both home construction and plant and equipment expenditures.

[24] In view of this result, further work is being done on a semiannual forecasting model based on overlapping half-years in a study by Irwin Friend and Paul Taubman.

where the time interval for the subscripts is one-half year: $\Delta HS_{-1/2}$ represents the change from the first to the second quarter of the preceding half year. PE^e was derived by averaging the second anticipation of plant and equipment expenditures for the first quarter of each half year and the first anticipation for the second quarter at seasonally adjusted annual rates. The regression coefficients in these equations have again been estimated by two-stage least squares (from a reduced form presented below). All the coefficients have signs which conform to expectations, and their magnitudes seem generally reasonable.

A comparison of these equations with the quarterly results indicates that the consumption and housing equations are considerably improved on a semiannual basis, since the correlation coefficients are significantly higher, and the standard errors are not too much larger than the corresponding quarterly estimates, though the semiannual errors might have been expected to be nearly twice as large. The consumption function has the same form as the quarterly relation, though, as would be expected, the short-term marginal propensity to consume (i.e., the $\Delta \tilde{Y}$ coefficient) is higher in the semiannual form.[25] The semiannual housing function is different in two respects from the quarterly form: current income is introduced in the form of an accelerator term; and a plant and equipment expectation variable is introduced to reflect credit conditions in the housing market; it is, of course, inversely correlated with accessibility of credit. This ΔPE^e variable has a similar influence in the corresponding equation in the annual model. A different form of the accelerator in the housing equation, where an average of $\Delta \tilde{Y}$ and ΔY_{-1} was used in conjunction with H_{-1} as explanatory variables instead of $\Delta \tilde{Y} - \Delta Y_{-1}$ gave about equally good results.

The semiannual plant and equipment and inventory equations are not quite as good statistically as the quarterly results, though they do have the advantage of a considerably longer lead time for forecasting purposes. The form of equation 33 for plant and equipment is identical with that used in the quarterly relation, but is less satisfactory in view of the relatively low coefficient of ΔPE^e, which would be expected to be close to one. Equation 33 is significantly improved if the first two observations, which were distorted by the Korean War, are eliminated, with the new relation:

[25] It is higher still in the annual form.

$$(35) \quad \Delta PE = -.71 + .64\Delta PE^e + .074(\Delta \tilde{Y} + \Delta Y_{-1}) \qquad \bar{R}^2 = 0.80$$
$$(.30) \quad (.19) \qquad (.024) \qquad \bar{S} = 0.90$$

The semiannual inventory equation is the same as the quarterly form except that no unfilled orders term is included, since neither a ΔO_{-1} nor $\Delta^2 O_{-1}$ term (nor both together) helped.

The reduced form which was employed in deriving the computed values for ΔY used in the above structural equation is:[26]

$$(36) \quad \Delta Y = 6.53 + 1.96\Delta G' + 5.27\Delta PE^e + .50\Delta Y_{-1} \qquad \bar{R}^2 = 0.80$$
$$(1.36) \quad (.45) \qquad (.83) \qquad (.21) \qquad \bar{S} = 4.16$$
$$+ 2.00\Delta HS_{-1/2} - 3.35 I_{-1}$$
$$(1.90) \qquad (.67)$$

A ΔC_{-1} term was tested but not used, since the results were somewhat less satisfactory, probably because of its intercorrelation with ΔY_{-1}.

The cyclical turning points are again reasonably well duplicated by this semiannual model. The "predicted" changes in gross national product (again in billions of 1954 dollars) calculated from equations 31–34 also appear to compare favorably with the actual figures. This comparison is presented in Table 4 (with the data as usual at seasonally adjusted annual rates):

TABLE 4

Comparison of Predicted and Actual Changes in Gross National Product, Semiannual, 1961–62
(billions of 1954 dollars)

	1961				*1962*	
	First Half-Year		*Second Half-Year*		*First Half-Year*	
	Predicted	Actual	Predicted	Actual	Predicted	Actual
ΔC	2.5	1.4	8.9	8.0	10.3	7.1
ΔH	1.0	−1.0	1.7	2.3	0.2	−0.2
ΔPE	−1.6	−1.8	1.0	1.7	2.3	1.4
ΔI	−4.2	−1.0	4.8	5.0	2.3	0.3
ΔY	0.3	0.2	17.5	18.0	18.8	12.9

Note: See Table 3 for source of actual data.

The GNP forecast for the first half of 1962 is somewhat too high, with most of this overestimate reflecting consumption and inven-

[26] The *ex post* reduced form for ΔY, obtained by solving the structural equations, is:
$$\Delta Y = 5.55 + 2.53\Delta G' + 4.12\Delta PE^e + .39\Delta Y_{-1} + .35\Delta C_{-1} + 2.67\Delta HS_{-1/2} - 2.39 I_{-1}$$

tories. The result for inventories may be due to the threat of the steel strike and its aftermath. In this semiannual model, the use of computed values for the lagged endogenous variables did not improve the predictions as much as in the quarterly model. As a result, the most significant prediction error in the first half of 1961, that for change in inventory investment, would have been considerably reduced and the overestimate of change in gross national product in the first half of 1962 somewhat reduced, by the substitution of actual for computed values of the lagged endogenous variables in obtaining predictions from the structural equations.

In view of the few observations available no test of the inventory and consumer anticipations data in explaining residuals from these semiannual equations was attempted.

On an annual basis, the time period covered by our analysis (viz., 1951–60) is too brief to permit any definitive conclusions. However, it may be useful to present the most interesting relationships obtained for explaining annual fluctuations in gross national product. The best statistical fits were associated with the following system of structural equations:

$$(37)\qquad \Delta C = 3.61 + \underset{(.05)}{.43}\Delta\tilde{Y} + \underset{(.11)}{.07}\Delta C_{-1} \qquad \bar{R}^2 = 0.90 \quad \bar{S} = 1.69$$

$$(38)\qquad \Delta H = .46 + \underset{(0.25)}{1.67}\Delta HS_{-1/4} - \underset{(.08)}{.10}\Delta PE^e \qquad \bar{R}^2 = 0.91 \quad \bar{S} = 0.56$$

$$(39)\qquad \Delta PE = .10 + \underset{(.03)}{.99}\Delta PE^e + \underset{(.01)}{.10}(\Delta\tilde{Y} - \Delta Y_{-1}) \qquad \bar{R}^2 = 0.95 \quad \bar{S} = 0.75$$

$$(40)\qquad \Delta I = .11 - \underset{(0.17)}{1.04}I_{-1} + \underset{(.05)}{.18}\Delta\tilde{Y} + \underset{(.21)}{.42}\Delta PE^e \qquad \bar{R}^2 = 0.92 \quad \bar{S} = 1.43$$

where all coefficients except those in equation 38 have been estimated by the method of two-stage least squares.

Before discussing the implications of the above system, we may briefly consider the estimated reduced form for ΔY used in deriving the two-stage least-squares estimates:[27]

[27] The *ex post* reduced form for ΔY, obtained by solving the structural equations, is:
$\Delta Y = 14.76 + 3.45\Delta G' + 4.52\Delta PE^e - .34\Delta Y_{-1} + .24\Delta C_{-1} + 5.76\Delta HS_{-1/4} - 3.59I_{-1}$

$$(41)\quad \Delta Y = \underset{}{33.02} + \underset{(0.90)}{3.92\Delta G'} + \underset{(1.19)}{6.80\Delta PE^e} \qquad \bar{R}^2 = 0.97 \quad \bar{S} = 2.28$$

$$+ \underset{(.26)}{.30\Delta Y_{-1}} - \underset{(1.45)}{2.35\Delta C_{-1}} - \underset{(7.52)}{3.43\Delta HS_{-1/4}}$$

$$- \underset{(1.22)}{5.37 I_{i-1}} - \underset{(.31)}{.99\Delta O_{-1}}$$

In view of the number of observations available, of course, the number of variables barely allows estimate of this relationship. Alternative reduced forms were obtained with lower standard errors (viz., the substitution of ΔHS_{-1} for $\Delta HS_{-1/4}$), but the corresponding structural equations were considerably poorer from both economic and statistical points of view.

The symbols in the above equations are self-explanatory (note that a lag of one period refers to a year) with the exception that ΔPE^e refers to the first differences in anticipated plant and equipment expenditures for the year t (as reported at the beginning of that year), $\Delta HS_{-1/4}$ is the change in housing starts between the last two quarters of the previous year, and ΔO_{-1} represents the difference between unfilled orders at the beginning of years t and $t - 1$.

While the fit is apparently quite satisfactory in the reduced form, there are only nine observations (and only one degree of freedom), the signs of several of the regression coefficients seem incorrect, and the constant term appears unusually large.[28] In the structural equations, on the other hand, the theoretically expected signs are obtained for all regression coefficients, the coefficients seem reasonable and are consistent with the quarterly and semiannual results, and the standard errors are quite low. Although it appears in the reduced form, a ΔO_{-1} term is not shown in the inventory equation, since its coefficient was close to zero and completely insignificant in this structural form.

The housing function again employs ΔPE^e as a proxy variable for credit availability, but neither change in current income nor an accel-

[28] In view of these results in the reduced form, which are probably due, at least in part, to intercorrelations among the exogenous variables, an alternative form with several of the suspect variables removed (and thus with more degrees of freedom) is given below:

$$\Delta Y = 21.86 - \underset{(.37)}{.39\Delta Y_{-1}} + \underset{(0.69)}{2.64\Delta G'} + \underset{(0.74)}{3.83\Delta PE^e} - \underset{(1.79)}{4.31 I_{-1}} \qquad \bar{R}^2 = 0.85 \quad \bar{S} = 4.85$$

Here, the signs are correct except for the ΔY_{-1} term, which is insignificant.

erator term helped. In the annual data an accelerator term in the plant and equipment equation, unlike the case of quarterly and semiannual data, is of considerable help, probably because of the longer time periods involved. This accelerator term may also serve as a proxy for unanticipated sales. The use of ΔPE_t^e in the inventory equation as an expectational or cyclical indicator also proved to be quite helpful, and the resultant standard error is fairly satisfactory.

It is interesting to note that anticipated plant and equipment expenditure is consistently a well-behaved and important explanatory variable in this annual model. In the housing equation, lagged housing starts proved a much stronger variable than lagged housing investment (ΔH_{-1}), although the appropriate lag for housing starts is, not surprisingly, less than a year.

Equations 37 to 40 were used to "predict" change in gross national product for 1961 and 1962. Actual values of the lagged endogenous variables were employed in the predictions because they seem to give fully as good predictions as computed values and the potential time advantage of computed values is not so important in the annual data. The predicted changes in gross national product and components for 1961 compared with the actual values for 1961 as well as the predicted changes for 1962 (all in billions of 1954 dollars) are presented in Table 5.

TABLE 5

COMPARISON OF PREDICTED AND ACTUAL CHANGES IN GROSS NATIONAL PRODUCT, ANNUAL, 1961–62 (billions of 1954 dollars)

	1961		*1962*	
	Predicted	Actual	Predicted	Actual
ΔC	5.9	6.0	18.0	n.a.
ΔH	−0.3	−0.0	0.2	n.a.
ΔPE	−2.6	−1.1	4.8	n.a.
ΔI	−3.6	−1.6	5.0	n.a.
ΔY	3.9	7.7	32.6	n.a.

NOTE: See Table 3 for source of actual data. $\Delta G'$ is estimated at $4.5 billion (in 1954 dollars) for 1962.

The predicted change in ΔY for 1961 proved to be somewhat too low, owing to consistent underestimates of investment components; but the sign of the change is correct in each case. With data available through the second quarter of 1962, the "predicted" gross national product for 1962 seems reasonably satisfactory, although in all likelihood it will prove too high. Based upon the assumption of modest increases in government expenditures (mainly state and local government) and no change in net exports for the remaining quarters of the year, the predicted change implies a 1962 GNP of $480.5 billion in 1954 dollars, or about $562 billion in current dollars (assuming a 1 per cent increase in prices over 1961).

It may be noted that revised national income estimates as of July 1962 were used in the predictions, although no adjustments were made in the equations. Predictions for 1961 based upon previously released values for the lagged endogenous values were somewhat better. The use of computed values for the lagged endogenous variables also improved the prediction for 1961 but resulted in an increase in the apparent overestimate of GNP for 1962.

While the annual results must be used with particular caution, they suggest that it is no more difficult to explain or "predict" annual than semiannual fluctuations in gross national product. The annual ΔY multiplier for ΔPE^e of over 4 implied by the model, i.e., the change in GNP associated with a unit change in plant and equipment expectations, seems rather high, as do the semiannual and quarterly multipliers for ΔPE^e, and to a lesser extent, the corresponding multipliers for $\Delta G'$. However, ΔPE^e reflects the effects of changing business expectations generally. Finally, it might be noted that limited-information estimates of the coefficients of the annual model were not nearly so satisfactory as the estimates presented.

Supplementary Comments and Tentative Conclusions

For short-run prediction of the gross national product, the most obvious deficiency in the simple models which have been presented is the assumption that government expenditures plus net exports is known. Consequently, these models permit only conditional forecasts. We shall not discuss in this paper the attempts that have been made by others to fill in this gap, of which probably the most important work for short-term forecasting relates to the use of budgetary and related data (as well as the recent trend in outlays) to predict federal expenditures on goods and services.[29] However, it should be noted that the substitution of government expenditures plus gross

[29] See Murray Brown and Paul Taubman, "A Forecasting Model of Government Purchases," *Journal of the American Statistical Association*, September 1962.

exports for the government expenditures plus net exports variable, on the grounds that it is more reasonable to treat the former than the latter as exogenous, does not change appreciably the results of the quarterly analysis.

Another potentially significant deficiency of the models presented here for short-run prediction of the gross national product is the absence of tax variables and relationships. Clearly, the impact of major changes in tax rates may be substantial and should be allowed for if they occur. We have constructed simple quarterly models with consumption related to disposable income and the latter to the gross national product but are not presenting them here, since we plan to do much more with them and so far they offer no improvement over the results presented (in a period, of course, of relatively small changes in tax rates). We also plan to do more with monetary variables.

Turning to the substantive (albeit highly tentative and admittedly inconclusive) results treated in this paper, the simple quarterly model we have tested seems to do at least as well as the more complex models, though much more exploration and testing of this hypothesis is required and planned. Semiannual and apparently also annual models are somewhat more successful than quarterly models, even though they entail forecasts for substantially longer periods ahead. It should be noted that in all of these predictions, at least one and one-half months of the period being projected would normally have elapsed before the forecasts could be made, which is a significant limitation of the quarterly model presented.

Limited tests of the predictive value of key series on business and consumer anticipations within the context of complete short-run models of income determination point to plant and equipment anticipations as the one stellar performer in all of the quarterly, semiannual, and annual analyses carried out. Business inventory anticipations and consumer automobile purchase plans do not seem to contribute significantly to the explanation of fluctuations in gross national product in these simple models, though the former appear to offer somewhat more promise. It should be stressed that apparently significant results can be obtained from these two series, but that these results are not at all stable from one set of regressions to another; equally good results can be obtained by the use of objective *ex post* variables alone. If housing starts and unfilled orders data are also included as anticipatory (though not anticipations) data, it may be noted that they also add to the forecasting ability of the short-run models but, particularly for unfilled orders, not so strongly nor so consistently as the plant and equipment anticipations series.

Obviously, this paper has only scratched the surface in exploring the subject of optimal short-run forecasting models. We are in the process of testing the utility of many other bodies of anticipations data (and of other anticipatory series such as stock prices) and eventually hope to experiment with appropriate models for different time periods when certain types of data may not be available. Even with the anticipatory series tested in this paper, much more work is necessary before reasonably definitive conclusions can be drawn. In the meantime, without too strong preconceptions, we invite a comparison of the predictive ability of these simple models for the period ahead with the alternatives currently available.

LAWRENCE R. KLEIN, University of Pennsylvania

Straightforward forecasting is one among many possible applications of econometric models. It is to be expected that Friend and Jones might find a model of smaller dimension than most major models now being used in this country and throughout the world that forecasts well. Indeed, an extensive empirical effort might uncover one with higher sample correlations and better *ex post* extrapolations than can be found in the models whose usefulness they are questioning.

The objectives of econometric model construction are manifold: (1) to *explain* the structure of the economy, (2) to give empirical content to theory, (3) to try to solve the mystery of the business cycle, and (4) to guide alternative economic policy decisions. A model that has a fine empirical record in sample correlations and *ex post* forecasting may serve these broader ends poorly.

One of the reasons for making models complicated is to try to explain economic events that have no place in the Friend-Jones models. Forecasting *is* one of the objectives, and the forecasting of prices, interest rates, employment, unemployment, factor shares, wage rates, etc., is a problem of great interest. Success has not been uniformly good in these areas, but they certainly warrant a major effort. To do the things that we want to do, it is inevitable that systems will have to be large and complicated, whether we are interested in forecasting or in a wider range of problems. It seems to me that being against the inclusion in models of such things as production functions, demand-for-labor equations, labor market adjustment equations, and interest rate and price equations is like being against motherhood, the family, and all the widely accepted social customs of our world.

Econometric research will actually trend toward just the opposite of simplicity. Bigger, more detailed, and more complex models are being constructed. They will dominate the field because they will give much more information and allow us to tackle a larger number of problems than their predecessors. The econometric model project of the Social Science Research Council's Committee on Economic Stability, drawing upon the combined research efforts of many different econometricians with varying backgrounds, veered immediately in the direction of building a big model with much sector detail. It will dwarf the models considered large in the present discussion. The consensus of professional opinion is clearly the opposite of Friend and Jones's.

Apart from the desire to display or analyze variables that can only be studied in the context of a large model, experience has shown that there is a positive advantage in having a detailed model. An economy as complex as ours shows heterogeneous dynamic movements. Sometimes one sector is strong; sometimes another. In a large, detailed system it is possible to have compensating errors. In fact, our forecasting experience has frequently shown this. It is not accidental that strengths in some sectors are offset by weaknesses elsewhere. Insofar as GNP forecasting is concerned, a detailed model frequently comes out better for these reasons.

Is the Friend-Jones model really so simple? Their model consists of four equations and an identity. These four are the most powerful among those contributing to the explanation of GNP, on the demand side, in my larger model; therefore, I am not at all surprised that the model appears to work fairly well. Surely they will not object to an endogenous treatment of foreign trade, even if exports have to be assigned a predetermined value in forecasting since they depend on overseas variables. Incidentally, the export equation might add much to our understanding of the functioning of the economy without contributing a great deal, as a relationship, to individual forecasts. Surely they will not object to explicit treatment of taxes. Given a few more innocent complications like these, and their model will not be simple any more. These changes ought not to impair forecasting ability and might help. Any number of differences like this between their model and more complex models have this same property; i.e., they should not impair the forecasting ability; they might improve it; they promote economic understanding.

In recent periods the American economy has fallen into the Keynesian pattern in the sense that the short-run "supply of output accommodates itself to demand for output." The American situation has not always been like this since the end of World War II; it was often not like this before the war; and it will not always be this way. A more universal model is needed. The larger United States models parallel similar large-scale efforts in Canada, the United Kingdom, Holland, Japan, and a number of other countries where econometric methods are being introduced for model-building purposes. We are searching for more universal schemes than one like the Friend-Jones model, which has a very limited scope of applicability.

The same anticipatory variables that Friend and Jones find powerful in their forecasting model were already explicitly introduced in models they criticize—orders, housing starts, investment anticipations, and others. These variables are powerful in short-run forecasts but have brief lead times; therefore, Friend and Jones cannot look far ahead in the future; but this is of critical importance in useful forecasting. In at least one respect, my own model is enlarged because of an attempt to make order variables endogenous and to generate them within the system. The work of Robert Eisner (reported at this conference) and Dale Jorgensen hold much promise for the generation of investment intentions. Similarly, Sherman Maisel, in his work on the Social Science Research Council project, has developed equations for housing starts. If Friend and Jones build a more useful model capable of looking further ahead, they will soon find that their imagined simplicity has vanished.

Another limitation of the Friend-Jones model is that it does not lend itself well to simulation studies. The Klein-Goldberger model achieved some very respectable successes in the strict field of forecasting, but I would regard the most significant application of that model to be the Adelman-Adelman simulation.[1] To estimate a model, complicated as it may be, and to *propagate* random shocks through it in a pattern that faithfully duplicates America's 100-year business cycle history gives, in my opinion, great insight into the cyclical process. It deals with important matters of business cycle theory and

[1] Irma Adelman and Frank L. Adelman, "The Dynamic Properties of the Klein-Goldberger Model," *Econometrica*, October 1959, pp. 596–625.

suggests possible interpretations. Simple models are not suitable here, especially if they rely heavily on anticipatory variables that cannot be generated over the course of the simulation run.

For my tastes, the Friend-Jones approach has too much of pure empiricism and not enough theory or other a-priori information. Rigid following of the size of correlation coefficients and of *t*-ratios of individual estimates, as well as *ex post* forecasting, are seriously overdone in their paper. Even on the interpretation of signs, I cannot agree. Who is to say what is the correct sign of the factorial income distribution coefficient in the equations of demand for particular components of consumption? There is very good a-priori reason to include interest and demographic variables in equations of housing demand. There will be *genuine* forecasting occasions when they might wish they had such variables in their relationship.

As *ex post* forecasts, the Friend-Jones results are good. As for the standards that they will have to meet in real tests of predictions, the forecasting success of the Klein-Goldberger model and its successor is much better presented in Daniel B. Suits' recent article than in the piece to which they refer.[2] In such references it is hard to bring out the dramatic pressures put on the genuine forecaster who makes a public statement, based on a complicated model result, such as we found in the recession of 1953–54 or the recovery of 1961.

As I mention in my reply to Franco Modigliani at this conference, the marginal income coefficient in the equation for consumer durables demand now appears to be too high. In a re-estimation of the model with revisions of this coefficient, the forecasts of 1961 would be better. The 1962 forecasts, which are only just being checked for the first time, were too high for GNP, but the degree of overestimation (in genuine forecasting) was considerably reduced by changing this coefficient.

These comments have been largely centered around my own models, which come in for criticism by Friend and Jones. I would say that the criticism of the Duesenberry-Eckstein-Fromm model is largely misplaced. That is not a complicated alternative. It has hardly more behavioral relationships than does the Friend-Jones model. Besides the inventory and consumption equations, it has only relationships between personal income and GNP and between personal income and disposable income. In addition to a number of technical

[2] "Forecasting with an Econometric Model," *American Economic Review*, March 1962, pp. 104–132.

fiscal relations, the main relations are those dealing with corporate saving, capital consumption, profits, and inventory revaluation. I would classify this model, in terms of complexity, with the Friend-Jones model. The two will be more similar in this respect if the latter introduce taxes and transfers explicitly. As to the Gallaway-Smith model, even Friend and Jones describe it as "extremely simple." How can the main point of the latter authors be demonstrated or proved by reference to this system?

COMMENT

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The paper presented by Friend and Jones is a report on work still in the formative stage. Their preliminary research is based on the premise that highly multivariable models are not likely, at least at present, to predict more accurately than much simpler ones, and that simpler ones have the further advantage that an optimal combination of variables is easier to find and test. This premise implies that a high level of aggregation is desirable in a forecasting model; consumption, for example, is treated as a single homogeneous category in all of their tests. Like most people I would prefer to deal with simple rather than complex model systems, perhaps because it is easier to trace the basic cause of a poor forecast in the simpler models. But I would have thought that the weight of recent empirical and analytical investigations pointed in the other direction—that more, not less, disaggregation is necessary, and that more rather than less complex forms represented a move in the direction of realistic specification of behavior relationships.[1] However, as the authors point out, the proof of the pudding is in the eating.

The appropriate criteria for determining what constitutes a "good" forecasting model are discussed in a brief review of some current models. It seems to me that Friend and Jones have ambiguous feelings

[1] I have added to my comments on the original Friend-Jones paper. Footnotes to my original comments represent additions, and are based mainly on the Friend-Jones Reply.

The evidence I had in mind here is the vast array of cross-section studies, relating both to households and business firms, which suggest rather strongly that disaggregation would improve the explanation of both saving and investment decisions by economic units. It does not follow, of course, that a time series prediction model incorporating these complexities will do better than one that ignores them—certainly not at the present time, and perhaps not in the foreseeable future. My own judgment would be, however, that prediction models will continue to be seriously in error (on occasion) until the models begin to describe decisions with realistic behavioral variables rather than with proxy variables that perform reasonably well most of the time.

about this matter. They start by saying quite plainly that ability to predict is the only really satisfactory test of a forecasting model, making appropriate adjustments for conditional forecasts if assumed and actual conditions diverge. In other parts of the paper, I get the impression that improvements in the fit for the base period are of some importance in determining a proper model; and some attention —far too little in my view—is given to the presence or absence of economically satisfactory (i.e., logically tenable) signs for regression coefficients.

At one level, this problem is not particularly interesting. Suppose that forecasting model "A" provides a near perfect fit for the past and that all its terms are consistent with firmly established theoretical preconceptions. On the other hand, "B" shows a worse fit for the past, but its theoretical basis is equally sound. If B persistently forecasts more accurately than A, it seems obvious that B is a better model. The difficulty lies with the word "persistently." Does a better record for the only two available forecast periods constitute a reasonable test? Does it make any difference whether the forecast period involves sharp changes in the level of GNP or no change in GNP? On the whole, I have the feeling that Friend and Jones (in this paper, at any rate) have given far too much attention to the improvement in prediction that results from adding this or that variable, and far too little to testing alternative models that involve competitive but internally consistent hypotheses about behavior.[2] This impression may be incorrect, since their paper is a progress report and, hence, is incomplete. But the tone of the paper is that prediction is what counts; and this position, while correct in the sense of being a necessary condition for virtue in a forecasting model, seems to me essentially misleading. Let me illustrate more specifically what I have in mind.

Friend and Jones contrast two models, identical except that one incorporates the recent Commerce series on businessmen's (subjective) inventory anticipations and the other does not. The inventory

[2] In their Reply, Friend and Jones say that "the simple point, which we do discuss at some length (and which, as Juster illustrates, is frequently overlooked), is that it is relatively easy to get good fits and much more difficult to get good forecasts." (Parentheses added.) Agreed, except for the clause in parentheses, but why is this the case? I would be inclined to argue that the basic reason is an unwillingness on the part of model-builders to impose theory on their model, in that variables of obvious relevance and importance are left out if a structural regression with the appropriate sign for them cannot be had. But if the variable is clearly relevant, the real difficulty must be that the structural equation is improperly specified. Having expressed all this in somewhat ex-cathedra form, I freely confess that I have no practical solution to offer.

anticipations variable greatly improves the fit for 1959 and 1960 (a hindcast period) but does not improve 1961 predictions. Their procedure involves regressing inventory anticipations against the GNP residuals from a model (equation 16) that *already* includes actual inventory investment in period $t - 2$ and the change in both unfilled orders and business sales between $t - 1$ and $t - 2$.

Perhaps inventory anticipations really do not improve predictions of the quarterly change in GNP, but I do not see how it can be known by this test. To begin with, the first three quarters of 1961 may not constitute a reasonable test period. I have no real objection to its length, although everyone, including the authors, would doubtless prefer a longer one. But whatever is causing the model to generate numbers different from actual GNP during this period may have nothing to do with inventory change. If the inventory anticipations improve predictions of inventory change (I_i) but knowing I_i does not help much (for this period) in predicting the change in GNP, one can hardly say that the model is not improved.

In the second place, and more important in my judgment, the authors seem to me to have given too much weight to the numbers and too little weight to theorizing in deciding whether the model is improved and which variables are necessary. Inventory anticipations are presumably based on something. To have real content they must be based on implicit forecasts of sales, on production schedules based on these forecasts, and on the current inventory level. If this is the case, why use both inventory anticipations and the combination of actual inventory change, lagged change in orders, and lagged change in sales to explain I_i? The latter are the objective variables most closely associated with I_i, while the former is an alternative (subjective) forecast of I_i. If the anticipations constitute a conditional forecast of inventory change based on assumptions about production and sales, the appropriate model would have the form

$$I_i = f[\hat{I}_i, (S - \hat{S}), (P - \hat{P})],$$

where $\hat{I}_i$ is anticipated change in inventory; $\hat{S}$ is anticipated sales; S, actual sales; $\hat{P}$, production scheduled in the light of current inventory and anticipated sales; and P, actual production. I do not see the point in simply inserting $\hat{I}_i$ into a model that already purports to explain actual $\hat{I}_i$, and, hence, I_i itself.[3]

[3] In their Reply, Friend and Jones argue that the rationale for including both inventory anticipations themselves and the (*ex post*) data on which the anticipations must

Somewhat similar treatment is accorded the other recent body of subjective anticipatory data—the FRB-Census survey of automobile buying intentions. Here Friend and Jones find that the intentions data do not help to explain residuals from their GNP hindcast during 1959 and 1960, nor do they improve 1961 predictions. But the only substantial quarterly changes in sales of durables during the 1959–61 period—to which the intentions series ought to be related if it is of any use—occurred in the first and second quarters of 1961. The only noticeable changes in the intentions series during the 1959–61 period are increases between the third and fourth quarters of both 1959 and 1960, followed by declines in the first quarter of the respective years. These are almost certainly seasonal movements; hence, the series shows little actual variation to date. Since it is clearly not possible to explain residuals or anything else with a series which consists (as it should) of essentially random numbers during the period when it has been available, it seems to me that the test must be inconclusive.

The analysis of these subjective anticipations or intentions variables in the Friend-Jones paper is exclusively concerned with examination of time series relationships, presumably on the grounds that a time series model is obviously needed for prediction. My own judgment is that any substantial improvement in forecasting models obtained by including these kinds of variables depends on achieving a much better understanding of the way in which anticipations are related to behavior. And it seems to me unlikely that much will be learned from time series about the interrelationships between actual investment, on the one hand, and business investment intentions, sales expectations, and actual sales on the other—or between purchases of consumer durables and consumer buying intentions, associated expectations, and outcome; or between inventory changes and inventory anticipations, unfilled orders, expected sales, and actual sales. Much more analysis of cross-section data seems essential if any of

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have been based is that "the *ex post* variables may be necessary to take account of firms that do not do serious jobs of budgeting" while "anticipated inventory investment . . . may very well have an informational content that *ex post* variables do not have." It still seems to me that this procedure serves to introduce a sort of statistical haze over what may be a very useful set of relations. If inventory anticipations mean one thing for firms that undertake serious budgeting and another for firms that do not, fine. But then the appropriate model has a disaggregated inventory function consisting of one structural equation for type A firms and a quite different one for type B firms. What has been gained by getting coefficients that represent averages for both sets of firms combined?

Incidentally, I knew that one of the authors had previously made use of the kind of analysis suggested by my comments. But I had supposed that the comments were to be directed to the paper presented at this conference.

these kinds of relationships are ever to be usefully incorporated into forecasting models. The alternative is to let enough time elapse so that sufficient variance is generated to permit reliable use of time series tests. Many of these variables, especially those relating expectations and outcomes to the fulfillment of plans or intentions, are likely to be of little use most of the time. Their values are typically fairly close to the mean, and it is possible that only substantial departures from the mean are of any consequence for behavior. But if such variables occasionally exhibit extreme values and if they do in fact have an association with behavior, models that fail to incorporate them will perform badly during some periods—even though most of the time such variables will fail to improve predictions. In sum, I would argue that (subjective) anticipatory data cannot be effectively incorporated into forecasting models without extensive use of cross sections to decide whether and how these variables relate to spending decisions. Once we know whether and how, e.g., investment plans are altered when sales diverge from expectations, this relationship can be incorporated into a forecasting model. But the whether and how, as well as adequate tests of alternative hypotheses, are simply not obtainable from time series.

Let me turn now to some specific points in the empirical results. Early in the paper the authors examine the question of which expenditure sectors are most closely associated with changes in GNP. Appropriate classification of GNP into sectors that, in old-fashioned terminology, are mainly induced or mainly autonomous (with respect to GNP) is obviously useful if forecasting is the goal, since the induced sectors do not need to be independently explained. Friend and Jones run regressions that relate ΔGNP to (ΔZ), the latter being the combined change in private investment, government spending, and net foreign investment, and then to

$$\Delta(Z - I_{pe}), \Delta(Z - I_h), \Delta(Z - I_i),$$

and so forth. ΔZ explains some 90 per cent of the variance in ΔGNP for the quarterly data, somewhat more for semiannual or annual series. Removing particular investment sectors from ΔZ always reduces the association with GNP, the biggest drop occurring when $\Delta(Z - I_i)$ is the independent variable; the same pattern is shown by all the series—quarterly, semiannual, and annual.

It is not at all clear to me what these data show. Friend and Jones make two points: (1) these results underline the important role of inventory accumulation in the fluctuations of GNP; (2) knowledge of ΔZ would permit very reliable estimates of ΔGNP, and these estimates would be significantly more reliable than those obtained from knowledge of the (much larger, in absolute terms) change in consumption, ΔC. One is left with the impression—perhaps wrongly—that consumption need not be of much concern in the design of a satisfactory forecasting model. Instead, effort should be concentrated on trying to explain movements in the sectors that traditionally were thought to be the cause of changes in GNP, especially government expenditures, inventory investment, and plant and equipment investment.

Two comments are in order. First, the only clear evidence that changes in a given spending sector may be the cause, rather than the consequence, of a change in income consists of an observed low correlation with income change. If inventory change is uncorrelated with income change it obviously cannot be a consequence of income change; if it is highly correlated no analytical conclusion is possible. By this criterion changes in I_i and G are clearly causes of GNP change, while PE and consumer durables could be either. Secondly, it seems to me that this problem can be investigated with sharper analytical and statistical tools. The procedure, which is described below, grew out of discussions with Gary Becker and Jacob Mincer.

Suppose we divide GNP into a number of component sectors—A, B, and R. Assume that we know on a priori grounds that A is wholly autonomous with respect to GNP; A might be government spending, for example. Also on a-priori grounds, we know or are willing to assume that R is wholly induced by GNP; for example, R might be consumer expenditures on nondurable goods. We are interested in whether the B sector is induced, autonomous, or partly both. The simplest procedure is to estimate coefficients for the following regressions:

$$R = b_0 + b_1A + b_2B + u_1 \quad (1.0)$$

$$A = a_0 + a_1R + a_2B + u_2 \quad (1.1)$$

If B is wholly induced by GNP it is completely substitutable for R, which is known to be wholly induced; in that case a dollar increase in B will be associated with a dollar decrease in R, holding A constant, and b_2 will be -1. If B is wholly autonomous, on the other hand, a dollar increase in B will be associated with an increase in R—the amount depending on the size of the multiplier—and b_2 will be positive and equal to $k - 1$, where k is the multiplier. Further, the coefficients of A and B will be the same, because both will be associated with the same amount of change in R and hence in GNP. I am assuming that a dollar change in expenditures that are wholly autonomous with respect to GNP will have the same multiplier regardless of the character of the change. This assumption may not be entirely realistic, although its validity does not depend on the presence or absence of complex interrelationships among the several expenditure sectors. In effect, A and B are perfect substitutes if B is wholly autonomous, while B and R are perfect substitutes if B is wholly induced. If B is partly induced but has some autonomous element, b_2 will be somewhere between b_1 and -1.

Equation 1.1 serves as a check on the b_2 coefficient in equation 1.0, since b_2 will be a biased estimate if u_1 is correlated with any of the other variables. In equation 1.1, a_1 and a_2 will be both positive and of equal size if B is wholly induced, while a_2 will have a value of -1 if B is wholly autonomous. Estimates of a_1 and a_2 supplement the information provided by b_1 and b_2, and facilitate a better judgment as to the relative importance of induced and autonomous elements.[4]

[4] Friend and Jones correctly point out that my criterion for isolating autonomous (my terminology, not theirs) expenditure sectors depends on the proposition that "sectors" have been defined in a meaningful rather than an arbitrary way. If, for example, PE is in fact wholly induced by GNP, "PE in the petroleum industry" may well appear to be autonomous. The problem they raise is a real one, although as a practical matter it seems manageable. In their illustration, to take a case in point, if ΔD really appeared to be autonomous vis-à-vis ΔGNP solely because of the negative correlation between ΔD and ΔN (in which case the appropriate consumption sector is $\Delta D + \Delta N$) this fact would show up quite clearly in a multiple regression of ΔN on ΔD and ΔZ (where ΔZ is $\Delta GNP - \Delta D - \Delta N$), as noted in my comments. In such a situation the coefficient of ΔD would presumably be -1.

I have no particular quarrel with the reservations expressed by Friend and Jones about the bias in estimates obtained by my suggested procedures. Whether my procedure or theirs is more useful, I leave to the judgment of the reader. Incidentally, my reference to I_i as a cause of income change, given my criteria, may be incorrect, but it certainly was not an oversight! For quarterly data over the period III–1950 to III–1960, the correlation between $(\Delta GNP - \Delta Z)$ and various expenditure sectors is as follows (where ΔZ is defined as in the Friend-Jones paper except that consumer durables have been included):

$X_1 = \Delta GNP - \Delta Z$
$X_2 = \Delta D$ = consumer durables change
$X_3 = \Delta I_f$ = change in gross private investment less change in inventory investment
$X_4 = \Delta I_i$ = change in inventory investment
$X_5 = \Delta G'$ = change in government spending on goods and services plus change in net foreign investment

$b_{12} = .29$, $r_{12}^2 = .16$
$b_{13} = .32$, $r_{13}^2 = .17$
$b_{14} = .11$, $r_{14}^2 = .11$
$b_{15} = .06$, $r_{15}^2 = .01$

The best candidates for autonomous (vis-à-vis GNP) sectors are clearly X_4 and X_5, that is, change in inventory investment and change in G'.

This procedure essentially constitutes a test of whether a particular component of GNP can be better substituted for that GNP component known to be wholly autonomous or for that component known to be wholly induced. If no a-priori knowledge is available, the test cannot tell us anything except that components A and B are substitutes, C and D are substitutes, and A and B are complementary to C and D. We cannot determine whether A and B are autonomous or induced, and similarly for C and D.

For quarterly data covering the same period as the paper, one such test shows the following results:

$$\Delta R = b_0 + .096\Delta D + .134\Delta I + .153\Delta G' + u_1 \tag{1.0}$$

$$\Delta G' = a_0 + .308\Delta D - .362\Delta I + .644\Delta R + u_2 \tag{1.1}$$

where $\Delta G'$ = change in government spending on goods and services plus net foreign investment
ΔI = change in gross private domestic investment
ΔD = change in consumer durable goods expenditures
ΔR = change in $[GNP - (G' + I + D)]$

all in current prices.

I would interpret these results as indicating that there are autonomous and induced elements in both D and I, with the latter having relatively more of an autonomous and less of an induced element. In equation 1.0, the coefficients of D, I, and G' are all positive, indicating that expenditure changes in these sectors, net of each other, are all associated with changes in the same direction for the R sector; hence, all these sectors contain a strong autonomous element. The coefficients of D and I are less than G'; hence, these sectors are apparently not quite so autonomous as G'. In equation 1.1, I and G' are partial substitutes; since G' is known to be autonomous, I must have a strong autonomous component. The D coefficient is positive but is considerably below the coefficient for the (wholly induced) R sector; hence, D appears to be partly autonomous, although evidently not so much so as I. Both equations rank the sectors G', I, and D in that order with respect to the relative importance of autonomous components. In connection with a current National Bureau project, I plan to do additional empirical work on this question.

My final comment relates to the form in which the anticipatory variable ΔPE^e is introduced in the change equations. This variable appears in a long list of equations (3, 16, 19, 24, 27, 28, 29, 32, 34) and represents the only real anticipatory variable—as distinct from lagged forms—that the authors found of much use. Unfortunately, even this variable may not really help, since their regression 3,

$$\Delta Y = 2.20 + \underset{(.14)}{1.37}\Delta(Z - PE) + \underset{(.39)}{.80}\Delta PE^e,$$

contains a spurious positive correlation between ΔY and ΔPE^e. The latter is defined as $(PE_t^e - PE_{t-1})$, that is, ΔPE^e is anticipated investment in plant and equipment for period t less actual investment in the preceding period. But, adding and subtracting PE_t, we get,

$$\Delta PE^e = PE_t^e - PE_{t-1} + PE_t - PE_t; \tag{2.0}$$

combining the two terms in the middle,

$$\Delta PE^e = (PE_t - PE_{t-1}) + (PE_t^e - PE_t), \tag{2.1}$$

or

$$\Delta PE^e = \Delta PE_t + (PE_t^e - PE_t) \tag{2.2}$$

That is, the change in anticipated spending on plant and equipment during t is equal to the actual change in plant and equipment expenditures (ΔPE_t) plus the difference between anticipated and actual expenditures in t. Even if the second term in equation 2.2 has a covariance of zero (anticipated and actual being entirely unrelated), ΔPE^e might still be correlated with ΔY if the first term (ΔPE_t) has a strong enough correlation with ΔY.[5] It is hard to tell whether the spurious element in ΔPE^e is strong enough to force a statistically significant relationship with ΔGNP where none really exists—it depends on the relative size of the variances of ΔPE_t and $PE_t^e - PE_t$. I would originally have thought that the spurious element would not be strong enough because PE_t^e is a fairly good predictor of PE_t, but the coefficient of ΔPE^e is barely twice its standard error even with the aid of a spurious element.[6]

[5] I gather from the Reply that Friend and Jones do not agree that their ΔPE^e variable contains a spurious correlation with the dependent variable ΔY. First, the availability of "completely reliable anticipatory data for every sector . . . so that $\Delta Y^e = \Delta Y$" leads to the conclusion, not that the correlation between ΔY^e and ΔY is "entirely spurious," but that $Y_t^e - Y_t = 0$, hence, that there is in fact no spurious element. Second, my objection to the Friend-Jones procedure is that one simply cannot tell whether ΔPE^e is really helping to predict ΔY, because of the way they have chosen to define ΔPE^e. If one explains ΔY by the Friend-Jones variable ΔPE^e, both sides of the equation turn out to contain the common element ΔPE; hence, the observed correlation has a spurious element of unknown strength. If PE_t^e and PE_t are perfectly correlated, the spurious element is nil; if the two are wholly uncorrelated, the spurious element accounts for all of the observed correlation. And if the truth is somewhere between, as it doubtless is, one cannot say anything.

[6] In the revised version of the paper the coefficient of ΔPE^e (equation 22) has grown to where it is now about three times its standard error.

REPLY by Irwin Friend and Robert Jones

A closer reading of the first two pages of our paper should reveal to Klein that we are not "against the inclusion in models of such things as production functions . . . interest rate and price equations" but that we do question their usefulness for short-run prediction of the gross national product and its major components. Nor are we particularly impressed by appeals either to "motherhood" or the "concensus of professional opinion."

We of course do not object to any complications, whether "innocent" or otherwise, which improve the forecasting ability of our model. We simply require that they do in fact rather than as an ex-cathedra proposition.

We must confess that we are confused by two statements appearing in the same paragraph of Klein's comments; viz., our "approach has too much of pure empiricism and not enough theory or other a-priori information"; and in referring to our criticism of his model "Who is to say what is the correct sign of the factorial income distribution coefficient in the equations of demand for particular components of consumption?" Klein has every right to have different theoretical preconceptions from our own but considerably less right to overlook the other relevant and conflicting "a-priori information" from cross-section data which he himself has cited on other occasions. We consider this another example of the weaknesses of large-scale models, which in this case would seem to be based on "too much of pure empiricism."

We agree with Klein and emphasize in our paper that our "good" results must be considered largely as *ex post* predictions and still have to stand up in the crucible of public *ex ante* forecasting. However, it is only fair to point out also that any literate outsider can at any time in the future not only test the usefulness of our model in a few minutes of not too arduous labor but also use the model himself for forecasting—which, of course, is not true of Klein's model. As a matter of fact, a comprehensive and objective test of the predictive ability of a large-scale model by any outside analyst is extremely difficult to perform for a variety of reasons, including the problems raised by frequent adaptations made in the model.

Finally, we do not understand the point of Klein's concluding comments on our brief discussion of the Duesenberry-Eckstein-Fromm model, which we characterize as "moderately large-scale," and the Gallaway-Smith model, which we characterize as "extremely simple." Klein feels that the Duesenberry-Eckstein-Fromm model is not moderately large scale. We disagree, but think it hardly worth arguing about. More important, he apparently also feels that the Gallaway-Smith model is either not relevant to what he characterizes as our "main point" or that if it is, it would be inconsistent with ours in view of the relatively poor results obtained. We of course do not take the position that any model is good because it is simple and assume that Klein does not take the position that any model is good because it is complicated. However, in discussing relevant past models, we believe in presenting them even if they are not corroborative of our "main point."

On reading the comments by F. Thomas Juster, we must confess that we have a vision of Don Quixote tilting at windmills. We are certain there must be some valid criticism contained in his comments and are embarrassed to admit that we have not been able to find it. With the exception of his concluding comment, which we shall consider first because it appears to be the only instance in which he takes issue with one of our substantive results, we shall discuss all of his major strictures in the order in which they appear.

Juster states that change in plant and equipment expectations (ΔPE^e) represents "the only real anticipatory variable—as distinct from lagged forms—that the authors found of much use. Unfortunately, even this variable may not really help . . ." This concluding comment is certainly one of the strangest in his paper. We had to read the reasoning twice and still did not believe what we read. The reasoning goes like this: ΔPE^e is certainly correlated with ΔPE, which is obviously correlated with the change in GNP (ΔY), thus introducing a "spurious" correlation between ΔPE^e and ΔY. Therefore, at the extreme, ΔPE^e might unfortunately be perfectly correlated with ΔPE and thus introduce extremely high spurious correlation with ΔY. Apparently, Juster is confused by the relationship between ΔPE^e and, in turn, ΔPE and ΔY and that between ΔPE^e or ΔPE and $\Delta(Y - PE^e)$ or $\Delta(Y - PE)$. Following his line of reasoning, clearly if we had completely reliable anticipatory data for every sector of ΔY, so that $\Delta YE^e = \Delta Y$, he would arrive at the novel conclusion that this was entirely spurious. This is a surprising dereliction for someone who counsels more theory and less arithmetic.[1]

[1] If we may be permitted to indulge in a little more arithmetic, the same point can be made in more precise statistical terms. Assume that Z (which now may be regarded

Let us consider the rest of his comments in order. First, he states that "the weight of recent empirical and analytical investigations" has pointed in the direction of "more, not less, disaggregation" and "more rather than less complex forms." Now, since we indicate in our paper that we are not aware of any such "weight of . . . investigations" and, in fact, are attempting to obtain some relevant empirical evidence, we would have preferred a little less ex-cathedra statement and a little more empirical evidence—as a matter of fact, any at all. In our revised paper, which incorporates other results presented at this conference as well as new results we have obtained, we note further evidence of the unsatisfactory basis of the common implicit assumption that disaggregation and complexity improve forecasting results.

A second criticism seems to be that we place too much stress on the ability to predict as the most satisfactory test of a forecasting model and pay too little attention to improvements in fit for the base period and "far too little" attention to "the presence or absence of economically satisfactory . . . signs for regression coefficients." Juster seems to have completely missed the reason for our stress on ability to predict. The simple point, which we do discuss at some length and which, as Juster illustrates, is frequently overlooked, is that it is relatively easy to get good fits and much more difficult to get good forecasts. Concerning our neglect in failing to point out economically unsatisfactory signs, which seems to be implied in Juster's comments, we wish he would give one example. It is true, as we point out, that an adequate test of predictive performance requires more than the extremely small number (typically two) of forecasting periods we used, and we stress the need for continuing to test in the light of subsequent performance whatever models are adduced. However, Juster does not seem to realize that while a couple of good forecasts may not give strong support to a forecasting model a couple of bad forecasts create grave doubt about its usefulness. This does not mean, of course, that the model which fails in

as a generalization of PE^e) is an autonomous variable highly correlated with PE and that $Y = PE + C$, with C, or consumption, largely induced. Then we may write $PE = a_1 + b_1Z + u_1$ or $b_1Z = PE - a_1 - u_1$, and fit the regression $Y = a_2 + b_2Z + u_2$ which may also be written

$$Y = a_2 + \frac{b_2}{b_1}PE - \frac{a_1b_2}{b_1} - \frac{b_2}{b_1}u_1 + u_2.$$

Clearly it makes little sense to talk of spurious correlation between Z and Y simply because PE is part of Y.

this critical sense may not or should not be resurrected in some modified form but that at the very least some such modification is called for.

Juster's third comment is that our test of predictive utility of inventory anticipations in a GNP model does not permit us to tell whether the predictions have been improved. He states, "If the inventory anticipations improve predictions of inventory change (I_i) but knowing I_i does not help much (for this period) in predicting the change in GNP, one can hardly say that the model is not improved." If he had looked at the discussion several paragraphs beyond the ones he is apparently commenting on, he would have noted that the inventory anticipations data are (as yet) not helpful in explaining deviations between estimated and actual inventory investment.

Next, Juster raises the banner of measurement without theory in connection with our use of anticipations data in forecasting relationships. He raises the question of the use of a large number of *ex post* variables in conjunction with inventory anticipations to explain the level of inventory investment as against the use of inventory anticipations in conjunction with deviations between the actual and anticipated levels of operational variables which entered into the inventory anticipation. We thought we made it clear that we had to rely on the *ex post* variables for the period as a whole because of the small number of observations for which inventory anticipations data were available and that we simply related residuals from the *ex post* regressions to the anticipatory data over the period for which this was possible. Clearly, we would have introduced the anticipatory data in another manner had more data been available. However, in any case, there is another point which Juster seems to miss completely. Anticipated inventory investment can be introduced in conjunction with *ex post* variables (even without, say, a sales minus anticipated sales type of term), since it may very well have an informational content that *ex post* variables do not have. On the other hand, the *ex post* variables may be necessary to take account of firms that do not do serious jobs of budgeting. We plan, of course, to use deviations of other relevant operational variables (such as sales) from expected values in conjunction with anticipated inventories when enough data become available. Not too surprisingly our limited experiment to introduce such a term in this paper was not successful. Probably this was due, at least in part, to the small number of relevant observations but also to the lack of anticipatory inventory and sales data for the trade sector.[2] Juster evidently was not aware that one of the authors, in collaboration with another colleague, was perhaps the first to use the type of analysis which he is recommending here, in a situation, of course, where the data were available.[3]

We have somewhat weaker reservations about his discussion of the inadequacies of the consumer intentions data that we attempted to use for improving predictability. However, when he says that the tests "might be inconclusive," this is really all we said, though we have no objection to its repetition.

Again, we have no strong reservations to Juster's comment that, "much more analysis of cross-section data seems essential if any of these kinds of relationships are ever to be usefully incorporated into forecasting models." However, we might point out the rather obvious point that we relied on other work, cross-section as well as time series, done by ourselves and others in this area. We feel Juster is raising a straw man when he states that he would argue that "anticipatory data cannot be effectively incorporated into forecasting models without extensive use of cross sections to decide whether and how these variables relate to spending decisions." Obviously, we do not disagree, but the ultimate utility of such information must be in the insights it gives us into time series behavior, and it is this that we are testing.

In the rest of Juster's Comment he takes issue with our examination of the relationships between changes in various expenditure categories and changes in GNP, and then proceeds to set up an alternative scheme which he feels is superior. We are afraid that again he misses the rather obvious point of what we were trying to do. We were not attempting a breakdown of GNP components into "autonomous" and "induced" classifications in the section to which he refers, since we had not yet set up the subsequent simple forecasting models which do indicate the assumed flow of causation. Our initial correlations were simply directed to determining which components of GNP had been primarily associated with the short-run changes in total GNP; but, as we were careful to point out, no

[2] However, it might be pointed out that the Department of Commerce did not find sales deviations useful in explaining quarterly unrealized inventory investment even for the manufacturing sector, for which both sets of data are available over the limited period covered (see Murray F. Foss, "Manufacturers' Inventory and Sales Expectations: A Progress Report on a New Survey," *Survey of Current Business*, August 1961).

[3] Irwin Friend and Jean Bronfenbrenner, "Business Investment Programs and Their Realization," *Survey of Current Business*, December 1950.

direction of causation was being assumed and the question of which variables were autonomous was not being considered. It is only in our forecasting models that we indicate the nature of our economic assumptions regarding autonomous and induced variables; and, as a matter of fact, we are inclined to believe the stipulation of a complete forecasting model is the only satisfactory way of doing this. Inventory investment may be largely induced but still may be the most important variable whose fluctuations have to be explained if we are interested in accounting for fluctuations in GNP. We are rather hesitant to expatiate on so obvious a point, but we shall do so anyhow. If ΔGNP is not highly correlated with ΔX_i, a component of GNP, then there is a large variance in the residuals of the regression of either of these variables on the other. On the other hand, if ΔGNP and ΔX_i are highly correlated, ΔX_i may be induced with small residuals or may be autonomous and free of negative correlation with ΔX_j. As a matter of research strategy, components of the second type are clearly worth more attention than the first, even though the results cast no light on how "autonomous" or "induced" the different ΔX_i are. In this connection, while it is not greatly relevant to our argument, Juster's statement that "the only clear evidence that changes in a given spending sector may be the cause, rather than the consequence, of a change in income consists of an observed low correlation with income change" seems to be incorrect. A little reflection, for example, would indicate that the correlation between ΔY and ΔD, which is durable consumption, might be close to zero. Juster would, therefore, apparently conclude that ΔD is autonomous, even though in fact ΔD and ΔN, which is nondurable consumption, were highly negatively correlated and even though the relation of ΔD per se with ΔY was not meaningful, whereas that of ΔC and ΔY was. Incidentally, we assume his reference to I_i as a clear cause of GNP change, in accordance with the criteria he sets up, is simply an oversight.

Turning finally to his suggested prescription for determining whether a component of GNP is induced or autonomous, we might point out that we have used a related mechanism for helping to determine the appropriate nature of structural relations.[4] We really do not see its utility, in the context in which Juster is trying to use it, for two reasons. First, no rigorous criteria are used to take care of

[4] Irwin Friend and Robert Jones, "The Concept of Saving," and "Rejoinder," *Consumption and Saving*, Philadelphia, 1960, Vol. II.

the only realistic case, that of partial substitutability (unlike the treatment in the Friend-Jones paper previously referred to); and secondly, in the case of partial substitutability, both his equations would lead to biased estimates of parameters.[5] In any case, we consider vastly preferable the approach we follow in erecting a set of structural relationships.

FURTHER REPLY to Juster

We feel that Juster's supplementary comments (see footnotes to his Comment) do not require any modification of our reply, with two minor exceptions. First, we are happy to correct our erroneous impression that his reference to inventory investment as a clear cause of GNP change was an oversight. Second, and more important, we still consider his treatment of spurious correlation between ΔY and ΔPE^e as mystical, in spite of the modification he has made in his original comments from ΔPE^e *would* still be correlated with ΔY *because* this first term (ΔPE_t) is obviously correlated with ΔY, even if anticipated and actual investment were entirely unrelated, to ΔPE^e *might* still be correlated with ΔY *if* the first term (ΔPE_t) has a *strong enough correlation* with ΔY. We thought we had indicated in fairly precise statistical terms that in writing, as Juster does, $\Delta PE^e = \Delta PE_t + (PE^e - PE_t)$, there is no basis for talking of spurious correlation between ΔPE^e and ΔY simply because ΔPE_t is part of ΔY. We might point out, as a last attempt at clarification, that while ΔPE_t is clearly part of ΔY, it is not part of ΔPE^e, because of the negative correlation between the two terms into which Juster divides ΔPE^e.

[5] It might be useful to point out that in Juster's equations 1.0 and 1.1, even if A is completely autonomous, R and B, holding A constant, may still be positively correlated, the assumption Juster makes. To indicate the theoretical rationale, there presumably is implicit in Juster's equations something equivalent to the following model, where t is time, ϵ_1, ϵ_2, and ϵ_3 are random residuals, and $f_1(t)$ and $f_2(t)$ may, of course, be highly complicated functions:

$$A = f_1(t) + \epsilon_1 \quad (1)$$
$$R = \alpha_2 + \beta_2 Y + \epsilon_2 \quad (2)$$
$$B = \alpha_3 + \beta_3 Y + f_2(t) + \epsilon_3 \quad (3)$$
$$Y = A + R + B \quad (4)$$

Given A, a negative correlation between R and B is certain only if the residuals are negatively correlated and B has no autonomous part (i.e., $f_2(t) = 0$). But if A is given without t being held constant, then variation in $f_2(t)$ and, hence, in Y may still occur; and the positive correlation of the Y terms in equations 2 and 3 may still outweigh the negative correlation of ϵ_2 and ϵ_3, giving positive partial correlation of R and B, even holding A constant. There is, of course, no obvious reason to assume the negative correlation of residuals which Juster appears to have in mind.