

ment, since the difference can be dramatic. A deviation from the expectation greater than 6.1 times the standard deviation will, according to the Chebychev condition, have a probability smaller than 0.027. If normality is assumed the probability will be below 10^{-9} .

The portfolio model, based on mean-variance analysis, really assumes that returns are normally distributed. The author is well aware of this, and carries out a standard test, which indicates that it is reasonably safe to assume normality and apply the model. This does not necessarily mean that the normality assumption holds at the tail of the distribution, and can be used to compute the probability of bankruptcy. The Chebychev condition, however, holds for any distribution, and it should be possible to make some reasonable assumptions about the shape of the distribution of returns, which would give much lower limits for probability of bankruptcy.

The author's conclusion that regulation can only increase the bankruptcy risk is certainly correct under his assumptions, but the increase may well be infinitesimal.

The purpose of regulation is presumably to force the intermediary to behave as a prudent risk averse investor. Koehn assumes that the intermediaries use the portfolio model, and this implies in a sense that most of the need for regulation is assumed away. For instance, restrictions that limit investments in securities issued by a single corporation will in general be unnecessary, if the investor seeks an optimal diversified portfolio. Only if the intermediary shows too little risk aversion will it be necessary to lay down some restrictions to prevent all the eggs from being put in one basket. If the intermediary wants to diversify in any case, as Koehn assumes, additional restrictions, if they have any effect at all, can only increase the risk of bankruptcy.

The book is written in a clear and engaging style, and the computations are easy to follow, through a series of well-designed tables. The results are illustrated by a number of graphs, which also serve as a basis for the author's arguments. There is some deficiencies in the generality of these geometric consideration, and some readers may have preferred analytic proofs. This is, however, a minor objection. The book should be useful to anybody interested in applied portfolio models, and it will be read with pleasure by those who suspect that much of the current regulatory activity is counterproductive.

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An Unformalized Forecasting Model, by Aleksander Markowski. Stockholm; National Institute of Economic Research, 1979. 119 pp.

There is a considerable tension in macroeconomics between the use of formal mathematical models of the economy and the reliance upon subjective informed opinion. This conflict surfaces in debates over theory, policy, and forecasting. It has been called the battle of the models versus the mystics.

Model builders argue that the explicit construction of a model compels the revelation of one's behavioral assumptions and thought processes, and makes economics

scientific in that forecasts can then be mechanically replicated by others. Thus, Samuelson wrote that

science is public knowledge, reproducible knowledge. When Robert Adams wrote an MIT thesis on the accuracy of different forecasting methods, he found that 'being Sumner Slichter' was apparently one of the best methods known at that time. This was a scientific fact, but a *sad* scientific fact. For Slichter could not and did not pass on his art to an assistant or to a new generation of economists. It died with him, if indeed it did not slightly predecease him. What we hope to get by scientific breakthrough is a way of substituting for men of genius men of talent and even just run-of-the-mill men. That is the sense in which science is public, reproducible knowledge. [2, p. 8]

However, in practice most modelers are also subject to this indictment in that their parameter estimation involves subjective nonreplicable model alterations. In addition, when actually using their models for forecasting, the parameters or models are adjusted to obtain forecasts that are in accord with their own informed judgments.¹ This is true, for example, of Lawrence Klein's very successful Wharton Model, which Samuelson has compared to the chess playing automaton that baffled the crowned heads of the world in the late eighteenth and early nineteenth centuries [2]. Hidden inside the machine was an expert chess player who only lost when he wasn't quite sober. Similarly, inside the Wharton Model one can find Lawrence Klein making subjective forecasts. And when he retires, the performance of the Wharton Model will surely suffer.

Model builders argue that the economy is too complicated to be successfully analyzed by mental gymnastics alone. A complex detailed model and a computer are needed to organize one's thoughts and to keep track of all the linkages, indirect effects, and feedbacks. Judgmentalists respond that the models are mathematically sophisticated but theoretically crude. Too much attention is paid to convenience and too little to realism. Too many equations are assumed to be linear. Too many variables are dropped because their parameters cannot be accurately estimated. Too many factors are ignored because they have not or cannot be measured. Too much time is spent with computers and too little time with people. Economic decisions are made by humans in very nonmechanical ways. These decisions are critically dependent upon individual perceptions and interpretations ("animal spirits") that we do not understand and cannot quantify. Similarly, the judgments of informed observers will not be easily described by mechanical equations. Economics is indeed an art and not a science.

Since the early 1960s the National Institute of Economic Research in Stockholm has been regularly publishing forecasts for the Swedish economy based upon a blend of data, models, and subjective judgment. The book reviewed here describes some of their procedures. Although rather sparse, it does serve to illustrate the strengths and weaknesses of both models and mysticism.

The institute's informal "KI model" is aimed at short-term projections. Annual forecasts for a given year are published the preceding October and in January, May, and October of that year. Formal econometric relations are estimated for many sectors of the economy, and their implied predictions serve as a base for the final

¹The Fair Model [1] is one of the few that makes wholly mechanical forecasts.

forecasts. There is a heavy reliance on survey data and subjective interpretations of recent historical experience.

This book describes many of the specific factors taken into account. There is an imperfectly related Keynesian demand side and input-output supply side. Interest rates and financial markets play a very minor role. The hourly wage rate depends upon vacancies and an exogenous negotiated wage increase. Prices depend upon input costs and world prices.

The construction of consumption predictions is discussed in some detail. The institute's predictions of private consumption (C) are influenced by forecasts of personal disposable income (Y), consumer prices (P), lagged consumption, and lagged retail sales. The consumption forecasts are "made on a purely judgmental basis, though several formal equations are consulted" (p. 28). A chapter is devoted to investigating the extent to which the actual forecasts can *ex post* be formally explained by an explicit model. This exercise is useful and the results are interesting.

The following regressions are reported:

$$\dot{C} = a_1 + b_1 \dot{Y} + c_1 \dot{P} \quad (1)$$

$$C = a_2 + b_2 Y + c_2 \dot{P} Y_{-1} + d_2 Y_{-1} + e_2 C_{-1}, \quad (2)$$

where \dot{X} signifies the percentage change in X , and various combinations of the parameters other than a_i and b_i are set equal to zero. In one set of regressions all of the data are forecast values; in the second set actual values are used.

It turns out that the institute's forecasts can be fairly well, albeit imperfectly, explained by these equations. These specific relations also indicate the value of explicitly revealing the factors that influence judgmental forecasts. First, such a revelation discloses the different assumptions that cause dissimilar forecasts. A choice between competing forecasts can then be based upon assumption preferences. Second, there can be criticism and possibly improvement in one's judgment. Here many would question the presence of an intercept in the rate of change equation (1). In both the forecast and actual data, the estimates of a_1 range between one and a half and three. Apparently there are nonlinearities or undisclosed factors that have exerted a continuing upward pressure on both forecast and actual consumption. Many observers would also inquire about the simultaneous presence of \dot{P} in both the C and \dot{C} relations. Does this reflect Pigou or Wicksell effects, or seemingly the former in (1) and the latter in (2)? Besides the confusion and the possible need for both price level and rate of change effects, the equations immediately suggest interest rates and the market value of wealth as possible additional influences. It may well be that the institute's researchers have a plausible rationale for their forecasts. But it is surely useful to expose that rationale to critical scrutiny.

The other side of that coin is that their forecasts are undoubtedly quite sophisticated and complex, both in functional form and in the number of influences taken into account, and consequently cannot be perfectly described by simple mechanical formulas. It is interesting though that there are smaller standard errors here for the equations using forecast rather than actual data. This suggests the existence of important additional influences on consumption that were not in fact taken into

account by the institute's forecasters. In addition, their forecasts are found to be more sensitive to income and much less influenced by inflation (and often with the opposite sign) than are actual consumption data. These kinds of exercises may be helpful in evaluating and improving forecasts. Reportedly, the efforts by Lars Jacobsson in the late 1960s to construct a formal econometric version of the institute's judgmental methods has in fact "affected the methods employed for the judgmental forecast" (p. 66).

The institute's forecasts of consumption, as well as other selected variables, are recorded in an addendum.² In the annual January forecasts³ of \dot{C} for 1964–77, the root mean squared error (RMSE) is 1.85. For comparison the benchmark naive model using average \dot{C} up until the year preceding the forecast has a RMSE of 2.01. In 1971 there is a revision break in the data such that actual \dot{C} is not really comparable with the forecasts. With 1971 excluded the RMSEs are 1.68 for the institute and 1.67 for the naive model. It would be very interesting to also compare the institute's judgmental forecasts with those from a formal model such as (1).

There is a chapter comparing the cyclical patterns in the forecasts with actual data for various variables. With the forecasts made in October of the preceding year and in January, the degree of correspondence with the actual data is disappointing. As might be expected, the May and October same year forecasts are closer to actual experience.

A major inadequacy of an informal judgmental model is that one cannot examine its implied predictions in hypothetical (perhaps policy) simulations. This is particularly true when as here there is a heavy reliance on survey data. This book is consequently of interest mainly to other model builders who want to compare notes.

LITERATURE CITED

1. Fair, Ray C. *A Model of Macroeconomic Activity*, Vols. I and II. Cambridge, Mass.: Ballinger, 1976.
2. Samuelson, Paul A. "The Art and Science of Macro Models Over 50 Years." In *The Brookings Model: Perspective and Recent Developments*, edited by Gary Fromm and Lawrence Klein, pp. 2–10. Amsterdam: North-Holland, 1975.

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Substitution Effects, Speculation and Exchange Rate Stability, by Patrick Minford. Amsterdam: North-Holland, 1978. x + 222 pp. \$36.75

Throughout the 1950s and 1960s substitution effects were of minor importance in theories of the balance of payments. Analysis of the trade account focused on income effects although a devaluation was recognized as an expenditure switching

²I examined the consumption forecasts because of their emphasis in the text. A reasonable comprehensive evaluation of all the institute's predictions is difficult, since the addendum contains more explanatory footnotes than forecasts.

³In the four years where multiple forecasts are given, a simple average was used. In three of the years it was necessary to ignore the qualification "just over" or "just under."