Letters

World Modeling

The scathing review by Martin Shubik (3 Dec., p. 1014) of Jay W. Forrester's book *World Dynamics* is, unfortunately, more than the personal opinion of an angry man. It is the expression of the views of very influential groups of people, which threaten to discredit and suppress a method of forecasting which I consider as of the greatest importance and promise.

There can be no doubt that Forrester's world model is only a first approximation. It lumps together highly industrialized and developing countries. It makes quantitative assumptions about interrelations, and Shubik is of course right in his skeptical remark about their validation by "the acceptance of top decision makers." Such questions are not decided by authority. Forrester's assumptions are validated by their plausibility, and greatly reinforced by the *insensibility* of his results to the details of the assumptions.

It is somewhat hard to understand the violence of the reactions to Forrester's work, of which Shubik's review is only an example. The rational way of rejecting it would be to show that it is so critically dependent on the model, and on the policies which are fed into it, that its predictive value is nil or less than that of intuitive forecasts. To my knowledge this has never been attempted, by any of the critics. Instead they reject it out of hand, intuitively, with angry aspersions at the author and the Club of Rome.

The problem in question is nothing less than the future of mankind. Shall we treat it, as until now, "intuitively," which means, as Forrester rightly remarks, using primitive mental models, and contemptuously reject the help which the computer can give to our thinking? Surely, the rational and responsible way would be to improve the inputs by the collaboration of the widest possible circle of disciplined minds, not only econometricians but also politicians, psychologists, and creative technologists who can make guesses at new inventions which might radically alter the prospects. The costs would be negligible, not only in relation to the importance of the problem, but also in relation to what the United States spends every year on data acquisition.

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Shubik's review fails to enlighten the reader concerning Forrester's objectives. Forrester has chosen five quantities, namely, population, natural resources, capital investment, fraction of capital investment in agriculture, and pollution, as quantities which are of most significance in the world system. Mathematically describing interrelations between these quantities, he then programs a digital computer to operate the resulting mathematical model. The trends in our world system indicated by this computer simulation are ominous indeed. Natural resource depletion turns out to be the limiting factor on growth, and population ultimately falls, to the accompaniment of a rise in pollution and a decline in the quality of life. Furthermore, none of the conventional solutions helps matters; for example, removing the natural resource constraint leads to an even more catastrophic pollution crisis and rapid die-off of population. These conclusions parallel the thinking of others both here (*1*) and abroad (*2*) attempting to view the world environmental situation holistically.

The author anticipates criticism of his work, and has ably answered the reviewer's primary objections by pointing out that governments readily concoct legislation which is presumed to correct ills in the social systems but which is in fact based merely on mental images of the real systems, images which are far less accurate than those he has constructed for computer analysis. The reviewer has wandered further onto shaky ground by attempting to liken the author's work to the analysis of RLC circuits. Forrester correctly describes the world system as a complex, nonlinear, multi-loop feedback system, and his programming reflects this. RLC circuits, on the other hand, are not by themselves feedback systems but are simply passive, essentially linear networks of electrical quantities and are of doubtful relevance to the discussion.

Forrester's dynamic model is by his own admission crude; but even crude models often give insight into a problem and serve as stepping-off points for more sophisticated models. That Forrester's model seems to be inherently unstable is a matter for thought, concern, and further research, not ridicule or head burying.

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References

1. "Man in the living environment," report of the Workshop on Global Ecological Problems (Institute of Ecology, Madison, Wis., 1971).
2. A. Lewis, *New York Times*, 14 January 1972, pp. 1 and 8.

. . . A substantial error in interpreting the book results from the reviewer's not taking note of the book's history as given in the preface. His remark "the Club of Rome, which sponsored this work . . . should provide some guidance" implies that the book is a consequence of the sponsored project. The preface clearly states: "On June 29, 1970, I attended a meeting of the Club of Rome in Bern, Switzerland . . . the group was invited to Cambridge . . . a meeting convened on July 20 for ten days of study, presentations, and discussion. The dynamic model of world interactions described in this book was devised in the early part of July to form a basis for discussion at the conference. It must be considered a preliminary effort. . . . As a result of the July meeting, the executive committee of the Club of Rome decided to establish a one-year research program at M.I.T. An international team under the leadership of Professor Dennis L. Meadows is going beyond the model described here to explore more deeply the underlying assumptions and the several major subsystems that form the sectors of the total world system." The content of the book led to the establishment of

the project at M.I.T. and is not a report on the output of the project. The book was an unsponsored personal effort. The project sponsored by the Club of Rome and the Volkswagen Foundation has been for the purpose of extending the model, modifying and documenting the assumptions, and discussing the inputs as well as the consequences by working with scientific research groups in other institutions who have relevant information and opinions. The published results of the project will begin to appear soon.

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Unlike many of my colleagues who are extremely skeptical about large-scale simulation, data banks, and the organized use of politico-socioeconomic models, I believe (and am on record as believing) that an effort of the magnitude of the United States space program should be mounted to do this work. Where Forrester and I differ is that, although we may both see the limitations of econometrics and the importance of model building in areas where measures are hard to obtain, we have different views of what is feasible and useful. In my reading of his *Industrial Dynamics* and *World Dynamics* and glancing at *Urban Dynamics*, I have not seen anything to suggest that the models presented are other than extremely simplistic, given the current state of knowledge in the social sciences. I fear that if Forrester, with his drive, energy, and intelligence, were to obtain large funding for his work at the national level, he might easily be the source of a fad for producing grossly oversimplified models, which after a few years would bring about a reaction that would seriously set back progress in simulation, data bank creation, and large-scale social-scientific investigation. . . .

In reply to the specific points in Gabor's letter, I should say first that it appears to me the burden of proof of the relative worth of Forrester's work lies somewhat with its proponents. I have not seen this proof forthcoming. No evidence is given in Forrester's books that his results are insensible to the details of the assumptions. Furthermore, contrary to Gabor's assumption, such insensibility is not always a merit. Those who have worked with industrial, economic, social, or psychological simulations (Orcutt, Simon, Coleman, Abelson, Adelman, Crecine, Clarkson, to name a few) are well aware of the difficulties of obtaining a good first-approximation model. If it is too insensitive to parametric sensitivity analysis, the model is probably concentrating on the wrong variables. If it is highly sensitive, then data sufficiently accurate for the purposes at hand are probably impossible to obtain.

The application of careful dimensional analysis and the specification of good measures in the right dimensions are all difficult and critical. For instance, what is a measure of "welfare"? Even given a measure, what is its operational significance? Answers to questions such as these call for an intimate knowledge of subjects such as economics, sociology, psychology, and political science. Why are so few social scientists referred to by Forrester? Are there none whose knowledge is worth considering when building models of social, political, economic processes? In fact, there are many who have both deep substantive knowledge and the ability to use computers and who are convinced that the computer and computer models are critical for good social scientific exploration. Consider, for example, the Brookings SSRC model, Tinbergen's work on planning, Orcutt's simulation, or Klein's econometric models. Forrester in his writings does not tell us why such other workers' applications, their sensitivity analysis methods and model building, are not as good as his.

I believe that a proper scientific evaluation of the work on planning and forecasting models in the social sciences is of great importance. This, of course, should include Forrester's work. I propose to Gabor, to the Club of Rome, to the AAAS, to NSF, and others that a project be sponsored or at least a conference be held to initiate such an evaluation. It may turn out that Forrester's work is better than, as good as, or worse than that of Balderston and Hoggatt, Clarkson, Kalman Cohen, Coleman, Crecine, Cyert and March, Gordon, Klein, Leontief, Manne, Markowitz, Meyer, Naylor, or Orcutt, to name a few. An examination of Forrester's contribution to this large body of work in such a context could be of benefit to all. It might provide him with the opportunity to join with members of the growing body of social scientists working on social systems. I should be happy to supply him with a list of eminently qualified judges of work of this nature, in the fields of artificial intelligence, computer science, communications, eco-

nomics, future studies, operations research, political science, philosophy of science, psychology, statistics, and other pertinent subjects.

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Test Bias

The letter (24 Dec. 1971, p. 1278) by Kenneth Clark and Lawrence Plotkin was meant to correct what they describe as "three egregious misstatements" in Stanley's article "Predicting college success of the educationally disadvantaged" (19 Feb. 1971, p. 640) where he discussed, among many other studies, one by Cleary (*1*). Clark and Plotkin took out of context a single sentence in the 7½-page article: "Cleary tried to replicate the findings of Clark and Plotkin [*2*] with a better controlled design, but failed." This conclusion referred to Stanley's prior quotation from the Clark and Plotkin study: ". . . Clark and Plotkin . . . had reported results of a study based on 'alumni' classes of the National Scholarship Service and Fund for Negro Students in which they concluded that:

> . . . scholastic aptitude test scores are not clearly associated with college grades. It is suggested that college admissions officers weigh test scores less, since they do not predict the college success of Negro students in the same way they do for whites. This study indicates that motivational factors are probably more important than test scores in the demonstrated superiority of Negro students in completing college."

Stanley was not questioning their conclusion that an able, highly motivated group of black students persisted well to graduation in a variety of interracial colleges during the 1950's. (For example, see note 19 in Stanley's article.) He did, however, cite much evidence—including Cleary's study—that Scholastic Aptitude Test scores and high school records tend to predict the college grades of blacks at least as well as they do those of nonblacks. In their letter Clark and Plotkin disregard this other evidence and thereby imply that Stanley's whole case rests on the Cleary study alone, which they seem to perceive as part of an Educational Testing Service plot against them ("Accustomed as we have become for our study to be the launching pad for ETS papers. . ."). Although one need not defend the professional integrity of researchers at ETS (*3*), we note that neither Stanley's article nor most of the reports he cites were done at ETS. Some of the strongest evidence came from investigations by black non-ETS researchers such as S. O. Roberts, Joseph P. McKelpin, and Charles Leo Thomas and from studies in Illinois, Maryland, Georgia, and the Seven Sisters colleges. Even a glance at the bibliography in the Stanley article would indicate the broad data on which he based his conclusions. (Indeed, only three lines of his paper were devoted to the Cleary article.)

Since Clark and Plotkin have chosen to criticize the Cleary article, let us consider the points they make. Clark and Plotkin say that their data were better than Cleary's because they "had data on over 1200 Negro subjects (with questionnaire response from over 500) drawn from all sections of the country and distributed in hundreds of diverse colleges." The quality of data is, of course, always relative to the purpose they are to serve. For the conclusion of Clark and Plotkin that an admissions officer (who necessarily works within a single institution) should weigh test scores less, distribution of the sample among "hundreds of diverse colleges" is a disadvantage rather than an advantage. For the admissions officer, Cleary's within-college analysis is clearly more relevant. Since Cleary was able to analyze data in only three colleges, she limited her conclusions: "The schools used in this study do not represent the full spectrum of colleges in the United States, so general conclusions cannot be reached." But there are many studies other than Cleary's in which the within-college analysis has been performed, and most of these were reviewed in the Stanley article. With each new analysis, the Clark and Plotkin conclusion becomes less credible.

In their criticism of the Cleary article, Clark and Plotkin persist in their emphasis of correlation coefficients to the exclusion of regression lines. It is well known that the size of a correlation coefficient is a function of the variability of the group: when the range of scores is restricted, the correlations are attenuated. For this reason and others, the comparison of regression lines is a more appropriate analysis. Clark and Plotkin computed no correlation coefficients or regression equations, nor did they even work within colleges. Instead, they pooled college grades from 187 different colleges and