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PREDICTION AND PRESCRIPTION IN SYSTEMS MODELING

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Modeling is a principal tool for studying complex systems. Since models may be used for predictions, for analysis, or for prescription, we must ask what our goals are before we build our models. Historically, predictive numerical models have dominated our practice. Since the world we are modeling is orders of magnitude more complex than even the largest models our computers can handle, we must conserve computational power, first, by asking how much temporal detail we need and how much can be supported by available data and theories, second, by asking whether knowledge of steady states may not be more important than knowledge of temporal paths, third, by using the hierarchical properties of systems to aggregate and thereby simplify them, and, fourth, by substituting symbolic modeling, where appropriate, for numerical modeling.

M odeling is a principal—perhaps the primary— tool for studying the behavior of large complex systems. Forty years of experience in modeling systems on computers, which every year have grown larger and faster, have taught us that brute force does not carry us along a royal road to understanding such systems. Nature is capable of building, on the scale of microcosms or macrocosms or any scale between, systems whose complexity lies far beyond the reach of our computers and supercomputers, present or prospective. Even in environments as artificial and constricted as the game of chess we are faced with numbers on the order of 10 raised to the 120th power. The combinatorics of such numbers are almost beyond our imagining, and certainly beyond our capabilities for computation.

Modeling, then, calls for some basic principles to manage this complexity. We must separate what is essential from what is dispensable in order to capture in our models a simplified picture of reality which, nevertheless, will allow us to make the inferences that are important to our goals. It is optimistic to suppose that principles can be laid down that are so general as to apply to the whole range of modeling tasks. In numerical analysis, we have already developed many classes of algorithms that are specific to particular computational domains and fields of application. Nonetheless, it may be worthwhile to look at the broader aspects of modeling, and to see whether there

are some useful things that can be said at a more general level.

I have organized my comments around the notions of prediction and prescription. When we model systems, we are usually (not always) interested in their dynamic behavior. Typically, we place our model at some initial point in phase space and watch it mark out a path through the future. We may do this with a number of different ends in view: we may wish to see if the model will settle down to a position of stable equilibrium; we may wish simply to predict future events (the weather); or we may wish to examine the prospective effects of policy decisions. I will not have much to say about the first of these goals; the second and third are what, in the title of this paper, I call prediction and prescription, respectively.

CHAOS AND ITS PROBLEMS

From the beginning of human efforts to model systems—long before the computer era—linearity has dominated the computational scene, not because anyone believed that the systems of interest were truly linear, but simply for reasons of computational tractability. In a few cases, (which, fortunately, covered many of the simpler situations required by field theories in physics) nonlinear systems could be handled computationally, at least for idealized boundary conditions. But the centrality of computational

tractability even in physics is marked by the large number of *theories*—for example, theories of the atomic nucleus—that are really proposals for retaining certain terms in approximations to more complete, but uncomputable, models and omitting others.

For the first three-quarters of this century, theories of nonlinear dynamics focused on two and three variable systems, which could be explored graphically, at least for their qualitative properties. If we look at a standard textbook like Andronov and Chaikin (1949), we see that the main interest was in characterizing the positions of stable and unstable equilibria of such systems and their limit cycles.

Although we knew full well that, at certain boundaries in phase space, paths could diverge that originated at points very close to each other, we chose usually to ignore these phenomena as *rare*. As a consequence, such topics as turbulence had to be handled wholly pragmatically, with empirical *Reynolds Numbers* and other parameters, and without much aid from mathematical modeling. Physical modeling, of course, was possible.

The rapid rise, in the last decade, of chaos theory, with its associated ideas of bifurcating systems, solitrons, strange attractors, and fractals, has changed all of this.[1] It has not, of course, taught us how to predict the paths of divergent systems. Rather, it has shown the fundamental reasons why such prediction may be impossible, now and forever. Furthermore, it has led us to consider that some of the important systems we should like to understand, including the weather and the economy, may be essentially chaotic.

On the other hand, the new mathematics of chaos has had some important positive messages for us. It has taught us to ask a set of different questions about dynamic systems. What are the *magic numbers* that govern bifurcation and hence, the transition, for example, from the smooth flow of liquids to turbulence? How can we renormalize fractal-like systems so that we can understand their behavior on different temporal or spatial scales? What are the shapes of the strange attractors that will bound the final, if chaotic, motions of such systems? How can we explain the curious *anti-entropic* structures, solitrons among them, that are created by the dynamics of many simple nonlinear systems? These questions, and some of their answers, are the valuable gifts that the theory of chaos has brought us, to console us for the loss of our powers of prediction.

The implications of these developments for the modeling of large socio-economic systems, environmental-energy systems, or ecological systems have hardly begun to be digested. One implication, for systems that we have reason to suspect are divergent or chaotic, is that we must give up prediction as the primary goal of modeling. Of course, chaos is essentially a statistical condition. It does not imply that *anything goes.* We may, for example, despair of tracing the future course of business cycles without renouncing the goal of making statements about the long-run development of an economy. For instance, we might make perfectly sound statements about the upper limits on per capita GNP without being able to say how closely, or when, these limits will be approached.

With these preliminary remarks about our new glimpses of chaos, and more generally, our new awareness of the wealth of possibilities that can emerge from nonlinearity, I will turn to the use of models for predictions.

PREDICTION

It is obvious why we are so fascinated by prediction of the future—whether achieved through horoscopes or otherwise. The future is *our* future, or at least the future of our children and their children. It bodes well or ill for us. Moreover, if we could forecast it, perhaps there are some actions we could take to alleviate its ill effects and enhance its favorable ones. If rain is predicted, we can carry an umbrella, and if sunshine, we can plan a picnic or an excursion to the beach. Notice, as in the example of the weather, that we may predict without being able to influence the predicted event. In fact, this is the simplest and commonest kind of prediction.

Much of what is called *analysis* in engineering design also comes under the heading of prediction. When we analyze the stresses in an airplane wing, we look for unstable roots that predict that the wing would sooner or later break off. Here, we are not interested in the time path, but in the possibility or likelihood of an event. A great deal of the power of engineering analyses, and their computational feasibility, resides in the fact that we do not have to track dynamic paths in detail, but only discover the stable and unstable equilibria of the system.

Modeling that undertakes to predict the time paths of large systems does not have an unmitigated record of success. Demography is a discipline that has had sobering experiences in the prediction of future populations. I need hardly mention dynamic economic models, which are regarded as admirable if they can do a little better than chance for a year or two into the future. And no one is making claims for the predictive accuracy of the Club of Rome report (Meadows et al. 1972), now a decade and a half old.

The Club of Rome report is an interesting case, which deserves a little more examination. Nowadays, it is often used as evidence that prophecies of gloom and doom are wholly misleading—which is surely the wrong conclusion to draw from it. Economists have complained that the Club of Rome model had the wrong equations in it, not derived from assumptions of the maximization of subjective expected utility. That also is surely a wrong-headed reaction because those assumptions are known to bear little relation to the real world.

The core of the Club of Rome model was a set of nonlinear dynamic mechanisms, including mechanisms that cause population and capital to increase as functions of present population and capital, and which, insufficiently damped by other constraints, cause exponential growth not only of these variables but of other system variables. One does not have to run such a model very many hours on a large computer—in fact, one does not need to run it at all—in order to conclude that, for many ranges of values of its parameters, such a system will sooner or later explode, at least in the sense of producing large limit cycles of booms and busts of population and other variables (such as per capita income). The basic idea was demonstrated many years ago with simple two-equation models of prey/predator relations (for example, the rise and fall of lynx and rabbit populations).

Nor is the exact time of the explosion a very interesting datum. The fundamental conclusion drawn from the model, that exponential growth cannot be sustained indefinitely, is entirely true, has enormous import for public policy, and could have been inferred from textbook treatments of dynamic systems without any computation.

I am not saying that the Club of Rome model should not have been run. Perhaps it required the actual plots of the time paths of the variables, however speculative, to attract and briefly focus public attention upon the dangers of a world having unconstrained growth of population and energy use. If so, then every dollar spent on computing these time paths was well spent, and the inaccuracy of the predictions was irrelevant to the significance of the message conveyed.

Leaving aside for the moment questions of political impact, one could argue that the Club of Rome model represents a misapplication of computational resources. The lesson the report aimed to teach could have been taught with a three-equation model computed (if we insist on looking at time series) on a pocket calculator. The computation thus saved could have been used for other purposes, which I will outline later.

There may even be positive harm, other than use of expensive computers, in carrying out such a modeling exercise when the data and equations of the model are only approximately accurate at best. It may give skeptics entirely too much ammunition for questioning even the conclusions that can be drawn validly from the model. Something of this sort happened to the Club of Rome report, but the modeling of nuclear winter provides an even better example.[2]

The basic data that are available for modeling the atmospheric and climatological effects of a nuclear exchange are very crude. The available theories of the dynamics of the atmosphere are equally inadequate to the problem at hand. Given these limitations, it was a heroic effort to construct a possible scenario of the aftermath of nuclear warfare. In the course of building the models, all sorts of assumptions had to be made about the probable values of the parameters concerned. The behavior of the models was a sensitive function of the superposition of these estimated values. In the modeling, some effort was exerted, quite creditably, to examine the robustness of the results under variations in the values of the parameters.

The result, as we know, was rather indeterminate. For some ranges of assumptions (read *for some scenarios of nuclear exchange*) the consequences for the weather, and hence, for human survival, were serious. Within other ranges of assumptions, the consequences were much milder—it appeared that the species might well survive! But the lesson that was drawn from these results by a large segment of the public, and especially by those who were previously unconvinced of the need for nuclear disarmament, was that Doomsday was not guaranteed. And if not guaranteed, Doomsday might not come. The nuclear winter issue rapidly lost political force and disappeared from the public view.

Was this the correct conclusion to draw from the analyses, and did the form of the analyses influence the form of the conclusion? My own answer to the first question is "no," and to the second one, "yes." Kahneman, Slovic and Tversky (1982) have taught us that the response of a patient to the statement, "You have a twenty percent chance of dying during this operation," is very different from the response to the statement, "You have an eighty percent chance of surviving the operation." (The latter statement is interpreted much more optimistically than the former.) From this we can conclude that the response to the statement, "Humanity is more likely than not to survive the climatological effects of a nuclear exchange," will be more optimistic than the response to, "There is a distinct chance that humanity will

suffer enormous casualties from the climatological effects of a nuclear exchange."

Let me describe one other example of *iffy* modeling where the response to the findings surprised the modelers. A few years ago, a couple of experiments were run on seeding the clouds of a hurricane (far from the coast) to see if this treatment would reduce the strength of the winds. A decision tree analysis was performed on the data, gathered from two hurricanes over a period of a day or two for each. The decision tree analysis of these very noisy data showed that, even for rather conservative values of the priors, seeding the hurricane would probably produce decidedly beneficial consequences. When these data were presented to a knowledgeable and responsible group of scientists (mostly physicists: it was the U.S. President's Science Advisory Committee), they voted, I think unanimously, that seeding should not be used *for real* until additional data were obtained.

Expected values cut no ice with this group. I think you can guess some of the thoughts running through their minds. If we seed a hurricane just off New Orleans, and as a result it is redirected to Mobile, who will sue whom? There is a strong belief in our culture and in our legal system that nonfeasance is less heinous than misfeasance or malfeasance. Even apart from the courts of law, we feel greater responsibility for our action than for our inaction.

It appears, then, that there is a good chance that the findings derived from *iffy* predictive modeling efforts will be ignored, or if not ignored, misinterpreted. One could take this as a reason for not modeling under circumstances where the results are not likely to be definitive. I would draw a slightly different conclusion: that we should redesign our modeling efforts, as far as possible, to focus them on the questions that we *can* answer more or less definitively.

In the case of the nuclear winter, perhaps the question to be asked should have been: Is it at all possible that a nuclear exchange will cause widespread death and suffering through its climatological effects? In the case of the cloud-seeding study, perhaps the question to be asked should have been, How much additional experimental data, and what kinds of studies on legal liability do we need before we can make a serious decision about seeding hurricanes?

Before we leave the topic of prediction, I must mention one final malady that plagues predictive models of social systems. Such systems have numerous feedback loops, but that, in itself, is not the problem. We constantly model with success engineering systems that are full of passive feedback loops. But the feedback loops in social systems are not passive but predictive. Each of the participants may be trying to forecast the behavior of other actors and of the system in order to adapt his or her own behavior advantageously.

The von Neumann and Morgenstern (1944) theory of games was the first large-scale systematic attempt to deal with this problem, although as early as Cournot economists have been aware of it. The most recent attempt at a solution is the Muth–Lucas theory of rational expectations (Lucas and Sargent 1981). What these impressive theories have provided us with is not so much a solution of the problem as a demonstration of its deep intractability.

More than forty years of intensive research leaves us with the rather firmly established conclusion that there is no unequivocal definition of rationality under conditions of mutual outguessing. Lacking such definition, we have no way, a priori, of postulating expectational or reactional equations for models of social systems, and precious little relevant empirical data for postulating such equations a posteriori. The theorems of game theory and rational expectations have added a new hazard to be faced by designers of social and economic models aimed at prediction.

The difficulties loom especially large for models of competitive, confrontational situations, that either are zero-sum, or are regarded by the participants as being so. For example, models of mutual deterrence, and other models of international relations are exquisitely sensitive to assumptions about the formation of expectations and reactions to the behavior of other parties. Here again, it may be impossible to obtain answers to our predictive questions, and we may be well advised to ask a different set of questions instead.

If we change the questions that we seek to answer in our modeling, then it is also probable that we will want to change the models and our methods of analyzing them. It is to this question that I would like to turn next.

PRESCRIPTION

Generally, modeling serves policy. We construct and run models because we want to understand the consequences of taking one decision or another. Predictive models are only a special case where we seek to predict events we cannot control in order to adapt to them better. We do not expect to change the weather, but we can take steps to moderate its effects. We predict populations so that we can plan to meet their needs for food and education. We predict business

cycles in order to plan our investments and production levels.

Of course, our models may, and frequently do, contain policy variables. Our demographic models may contain variables that represent birth control policies or the effects of public health measures. (Usually, we simply incorporate these in the estimates of birth and death rates.) Our economic models may contain variables that represent government fiscal and monetary policies. Our model of the eutrophication of a lake may include the quantities of phosphates we drain into it. When we include such variables in models, our intent in modeling changes from simple prediction to prescription.

When our goal is prescription rather than prediction, then we can no longer take it for granted that what we want to compute are time series. We do not care what the water quality will be in the lake next year or the year after that. What we care about is how much the phosphate input needs to be reduced to eliminate the unwanted eutrophication. Moreover, given the crudity of our data and the approximate nature of our model of the eutrophication process, we are interested mainly in orders of magnitude. This means that we can greatly simplify our calculations, possibly with the result that the back of an envelope will suffice.

Given my fondness for computers, I always find it a bit regrettable when I reach that conclusion: that I don't need a computer, but only an envelope and a pencil. But facts must be faced. Intelligent approximation, not brute force computation, is still the key to effective modeling.

Let me return to the Club of Rome problem, viewing it now, not as a problem of prediction but as one of prescription. What are the important questions to be answered by the modeling? The important questions, I submit, are: What is the steady-state population that can be sustained on Earth at a reasonable standard of living, and the steady-state rate of energy production that can be sustained without serious damage to the environment? These questions, of course, engender others: What is a reasonable standard of living? What are the alternative sustainable energy sources, and what are their relative advantages and disadvantages? What are the feasible ways of disposing of the wastes produced by people, by production, and by energy generation?

If these are the right questions, or a reasonable approximation thereto, then we probably do not care about a predictive model that tells us, for each point in time, just where we stand. What we want is a model of the steady state, a model of an acceptable world where all constraints on resources and environment are met, more or less permanently. If we can define such an acceptable world, then it will be time to consider the paths we must follow to reach it. Nor will it be important, even at this second stage, to map these paths in detail. Good attention to feedback and correction will get us to our goal even without specific temporal plans.

Let me provide just one more example of the utility of steady-state analysis. A national decision has just been reached that nuclear wastes will be buried in a depository in Yucca Mountain in Nevada. The wastes must be retained for some 10,000 years, and there are some large uncertainties as to what will happen to them over that enormous period, but that is not my concern here. Rough figures are available on the rate at which plutonium wastes are accumulating in the United States at present levels of nuclear power generation. We know also the size of the proposed depository.

Using these three numbers (storage time, rate of production, and size of depository) we compute on the back of the envelope the number of such depositories that are necessary in the steady state. The approximate number is 50 to 70 for the United States alone. I do not propose to draw any policy implications here from this number. I simply present it as an interesting, and possibly significant, fact and as an illustration of the utility of steady-state analysis.

Our practical concern in planning for the future is what we must do *now* to bring that future about. We use our future goals to detect what may be irreversible present actions that we must avoid, and to disclose gaps in our knowledge (for example, the feasibility of fusion energy) that must be closed soon so that choices may be made later. Our decisions today require us to know our goals, but not the exact path along which we will reach them.

Analyzing the conditions for a steady state may require a rather detailed model of the interactions of the variables. One could think of linear programming as a possible framework for such a model, but perhaps that formulation is too restrictive. But however we formulate it, we can devote our computational resources to studying the steady-state interactions rather than plotting time paths in which we have no real interest, and which certainly will have no reality. Redeploying our computational resources in this way, we will be in a position to explore many alternative scenarios and to examine the robustness of our results

and their sensitivities to changes in our assumptions or in policy.

I should not like to argue that we are *never* interested in making predictions of time paths. But if we take the goal of our modeling to be prescriptive rather than predictive, we will at least examine that issue at the outset, rather than taking for granted that temporal prediction is the name of the game. We will also consider carefully what aspects of the situation can be modeled at a level of certainty and accuracy that is commensurate with the decisions we are trying to make. We will not run the computer simply because it is there.

AGGREGATION

I have proposed one way in which the computational demands of modeling can be brought within acceptable bounds. Are there other ways in which we can cope with complexity? I am sure that there are a number, but I would like to focus on a particular one that is familiar to me. The complexity of our models of a system can often be reduced greatly if we are willing and able to limit the frequency range of the dynamics that interest us.

Elsewhere, I have made the argument that the systems, both natural and manmade, in which we are interested are almost always hierarchical (Simon 1981). That is to say, they have a boxes-within-boxes architecture. Quarks combine to form protons, protons and neutrons form nuclei, nuclei and electrons form atoms, small groups of atoms form radicals and similar components of molecules, and these components form molecules, and so on, through another half dozen levels of biological structure, and social structure above that.

The important property, for our purposes, of such hierarchical systems is that the behavior of the units at any specific level can be described and explained without the need for a *detailed* picture of the structures and behavior at the levels below. That is why we have been able to suspend, from skyhooks, the entire structure of biology, chemistry, and physics while awaiting the appropriation of 12 billion dollars for a supercollider that will tell us what the fundamental particles, those that quarks are made of, are really like.

The hierarchical structure of the world enables us to understand it from the top down (or from the middle out), and in fact to study it one layer at a time with only moderate concern for the layers immediately below and above. Only gross, aggregate features

of the layer below show up in the next layer above. Thus, for many purposes of celestial mechanics, we can treat the planets and their satellites as massive points, without extension or structure. For many purposes of genetics, we can treat DNA as a string of nucleotides, without further internal chemical or geometric structure.

From a mathematical viewpoint, hierarchical structures can be represented as nearly-completely-decomposable dynamic systems (Simon and Ando 1961). That is to say, when we examine the matrix of coefficients that represents the dynamics of such a structure, we find that the variables can be grouped in clusters, with high average rates of interaction among variables belonging to a single cluster, but much lower rates of interaction between clusters. Taking into account different orders of magnitude in the rates of interaction, we may find this pattern repeated at many levels—the boxes-within-boxes structure mentioned earlier.

It is easy to show that under these conditions the dynamic behavior of the system can be factored into different frequency ranges, each corresponding to a particular level of interaction rates. When we are studying high frequency interactions, hence short-term behavior, we need consider only the interactions within the smallest clusters, treating each cluster as independent of the others. We can then aggregate, replacing the detail of each cluster by a few aggregate properties (for example, in the astronomical example, its total mass, its center of mass, and the location and velocity of that center). Using these aggregates, we can then examine the behavior of the clusters at the next higher level over a somewhat longer span of time. The process can be repeated indefinitely, as long as the property of near decomposability is present at the appropriate level. Physicists will recognize this as the trick that underlies renormalization.

SYMBOLIC MODELS

Thus far, I have been writing as if numbers were the natural stuff of which models are created. If we were to take a census of all the computer models that have been created in the past forty years, that assumption would surely appear to be correct. Following up our success of the past three hundred years in using real numbers (and occasionally imaginaries) to represent the world, we have mostly programmed our computers to make numerical calculations.

In fact, until quite recently, many computer users, even quite sophisticated ones, supposed that numbers

were the only symbols that computers could process— that somewhere in the bowels of computer memory were great globs of binary symbols that could only be interpreted as numbers. We are all now aware that that is false. Computers can process symbols of all kinds, including symbols that represent natural language or diagrams (Simon 1981).

Human beings are great users of natural language, and much less frequent users of numbers. It is not at all obvious that the best way to model human behavior is to describe it numerically. Of course, if we are modeling an economic system, prices and quantities of goods and services are quite natural symbols to use. But what about a political system? What are the units of power? Of persuasion? Of patriotism and nationalistic fervor?

For some years now, researchers in artificial and cognitive science have been exploring the capabilities of nonnumerical models. The best developed, perhaps, are models of human problem solving (Newell and Simon 1972). In many of these programs, not a single real number appears. I cannot, in this paper, describe the ways in which situations are described nonnumerically, but I can give one example of this kind of modeling.

Jaime Carbonell (1979) constructed a program, often referred to as "The Goldwater Machine," that undertakes to predict how a specific political figure will respond to a specific public event. The program is given information about the political views and values of a public figure (Goldwater was used for debugging purposes), and is also provided with some inference processes that can reason about these views and values and apply them to particular states of affairs. The program is then given information about a particular event (a development in Middle East politics, say), and asked to predict what the public figure who is being modeled will say about the event.

I will leave the matter with this single tempting example. So far there are not a great many others. Here is an exceptionally promising direction for modeling social behavior, not only of individuals, but of groups and societies as well.

CONCLUSION

There is no single moral that I should like to draw from my discussion of modeling unless it be the commonplace moral that we will do a better job if, before we begin, we ask what our goals are—what questions we are trying to answer. In modeling, as in any other human activity, there is a certain amount of historical inertia. Predictive models bulked large in the early history of modeling, and especially in our enthusiastic efforts to explore the potential of computers when they first became available to us. Moreover, again for historical reasons, we tended to conceptualize our computer as a number cruncher, and we did not immediately see its potential for qualitative and symbolic modeling that did not use numbers.

Before we begin a modeling task, we need to ask whether we need temporal detail, and if so, what amount of it can be supported by the kinds of data and theories that we have available. We need to ask whether a good understanding of steady states may be more important to us than a tracing of time paths. We need to ask whether we can simplify the systems we are modeling by making use of their hierarchical properties to aggregate, or in other ways. And we need to ask whether there are aspects of the situation of interest that are better modeled symbolically, in words or pictures, rather than numerically.

We need to ask these questions because, at best, the situations we wish to model are orders of magnitude more complex than the most elaborate models that supercomputers of the present and future will sustain. We need to apply keen intelligence, whether of people or computers, to make sure that we capture in our models the aspects of the world's systems that are important to us.

NOTES

1. Cvitanovic (1984) gives an excellent introduction to chaos theory and reprints many of the original sources.

2. On nuclear winter, see National Research Council, Committee on the Atmospheric Effects of Nuclear Explosions (1985).

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