



## Requiem for Large-Scale Models

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To cite this article: Douglass B. Lee Jr. (1973) Requiem for Large-Scale Models, Journal of the American Institute of Planners, 39:3, 163-178, DOI: [10.1080/01944367308977851](https://doi.org/10.1080/01944367308977851)

To link to this article: <https://doi.org/10.1080/01944367308977851>



Published online: 26 Nov 2007.



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"That is not said right," said the Caterpillar.  
"Not *quite* right, I'm afraid," said Alice, timidly: "some of the words have got altered."  
"It is wrong from beginning to end," said the Caterpillar, decidedly; and there was silence for some minutes.

*Alice in Wonderland*  
Ch. V, Advice from a Caterpillar  
Lewis Carroll

## Requiem for Large-Scale Models

*Douglass B. Lee, Jr.*

The task in this paper is to evaluate, in some detail, the fundamental flaws in attempts to construct and use large models and to examine the planning context in which the models, like dinosaurs, collapsed rather than evolved. The conclusions can be summarized in three points: 1. In general, none of the goals held out for large-scale models have been achieved, and there is little reason to expect anything different in the future. 2. For each objective offered as a reason for building a model, there is either a better way of achieving the objective (more information at less cost) or a better objective (a more socially useful question to ask). 3. Methods for long-range planning—whether they are called comprehensive planning, large-scale systems simulation, or something else—need to change drastically if planners expect to have any influence on the long run.

Almost a decade ago, John Reps presented a paper to planners in which he attacked traditional modes of land-use control and offered alternatives; his paper was titled "Requiem for Zoning." This attack, directed at physical planners from one of their own, came at a time when many thought that mathematical models and computer data banks would overrun the field. His effort deserves a symmetrical gesture. (1964)

This paper is about large-scale urban models. The characteristics exhibited by these models are (1) they are large in the sense that the only practical way to operate them is on a computer; (2) commonly they are spatially disaggregated, and allocate activities to geographic zones; and (3) they pertain to a single specific metropolitan area, as opposed to being generalized abstract or hypothetical models. The epitome

of the genre is the comprehensive land-use model of the type constructed in the middle of the last decade.

These models were begun in the early 1960s and largely abandoned by the end of the 1960s. Considerable effort was expended on them, and a good deal was learned. Contrary to what has often been claimed, what was learned had almost nothing to do with urban spatial structure; the knowledge that was increased was our understanding of model building and its relationship to policy analysis. For that alone it was a valuable experience, but not if the lessons are ignored. For many in planning and many in a number of related fields that have recently become interested in planning, the lessons are being ignored.

Some planners never accepted models as legitimate activity of the field, and they will claim this paper vindicates their position.<sup>1</sup> This is incorrect; there was a need at that time for better analytic and quantitative procedures, and there was also a need for the development of better theory. Now, the need for both theory and method is even greater. It is not our intent to discourage those who would apply quantitative methods to urban problems, but, rather, to redirect their talents into more valuable pursuits than repeating the mistakes of the last decade.

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A prototypical land-use model is broken down into subareas (called zones or districts) generally larger than census tracts in size, for which various activities (population, basic employment, commercial employment) are recorded. Categories are disaggregated (e.g., age, income, industry), along with basic exogenous data and structural parameters. The model begins operating at some recent point in time, and allocates new activities to zones, in discrete lumps that represent periods of two or five years. Descriptions or reviews of these models are found in ASPO, 1970; Harris, 1965; Lee, 1968a; Lee, 1968b; Goldner, 1968; Goldner, 1971; Brown, et al., 1972; and Kilbridge, et al., 1972.

As will be discussed below, a multiplicity of goals has surrounded these models, and the failure to separate ends and their associated means from each other contributed to the failure of the models. There are two themes, however, that run consistently through the modeling efforts, dominate or subsume other purposes, and allow for an evaluation of model performance without considering each possible goal separately. First, it has been consistently thought that these models would help planners in their professional roles as advisors to public decisionmakers, with emphasis on objective plan evaluation, in reaction to the often mystical methods by which architect-planners and urban designers prepared land-use plans. Second, a wide range of learning benefits have been cited for the models, such as contributions to the development of theory, the education of modelers, and communication with lay decisionmakers. Using these general goals as evaluation criteria, the performance of the models will be considered; later, the goals themselves will be reevaluated.



## SEVEN SINS OF LARGE-SCALE MODELS

### *Hypercomprehensiveness*

Excessive comprehensiveness has been partly the result of historical accident. The dominant pattern in city planning emphasized the need for comprehensive thinking and a comprehensive master plan in order to guide metropolitan growth, prevent large-scale inefficiencies and negative neighborhood effects,

and preserve open space. Comprehensive models were an inadvertent way to continue this school of thought while at the same time making planning less the province of architects. Another historical accident was the expansion in scope of the field. Everything seemed to be an urban problem, and everything seemed interrelated; the whole world was a jumble of secondary and iterative side effects. Some way of integrating it all was needed without giving up anything (having just discovered the interrelationships existed, we did not want then to ignore them intentionally), and computers and models held out this promise.

The overly comprehensive structure of existing large-scale models has two aspects: (1) the models were designed to replicate too complex a system in a single shot, and (2) they were expected to serve too many purposes at the same time. These aspects are also mutually reinforcing, in that a broader scope increases the potential market for model output and the marginal cost of one more purpose always seems small.

Too broad a scope usually means too many variables and too much detail are included in the model structure. Allocating growth in residential and commercial land uses by several categories each, for an entire metropolitan area, disaggregated into census tracts or slightly larger zones, is a task presently beyond our capabilities, without even including redevelopment and spatial redistribution effects. The state of our knowledge of urban structure and process is far too weak to support such an effort and have the result make much sense. Including more components in a model generates the illusion that refinements are being added and uncertainty eliminated, but, in practice, every additional component introduces less that is known than is not known. The sum of knowledge of urban structure is large, but the density is thin. Moreover, the nature of our understanding is such that the total (in the sense of a comprehensive model) is less than the sum of the parts.

A multiplicity of purposes is a natural consequence of too broad a scope, and should be suspect. The land-use models were expected to predict future growth, locate clusters of new activities, trace out the impact of slum clearance, demonstrate the effect of a zoning policy, and evaluate changes in the transportation system, to mention only a few. In retrospect, it is clear that nothing is gained from combining so many purposes into one vehicle, and failure to separate ends (and hence means) can be regarded only as intellectual laziness. Trying to do everything at once simply means that all are more likely to be done poorly.

Models can be comprehensive in scope without be-

ing comprehensively detailed; in fact, it is the attempt to be both that is self-defeating. An example of a good aggregate model is Keynesian fiscal policy, which prescribes a tax increase to counteract demand-pull inflation. In conjunction with suitable monetary policy, the model worked well once it was accepted in the highest councils of government. It explained the inflation set in motion by the Vietnam War, and proposed adequate remedies, leaving the details of how much to tax, which taxes to increase, and what adjustments to make in spending to more specialized models. Unfortunately, the prescription was not followed, and now different models are called for (see Eisner, 1971).

#### *Grossness*

Ironically, while the models often sank under the weight of excessive data that were required to provide microscopic detail, the actual level of detail was much too coarse to be of use to most policymakers. In generating forecasts for the city or metropolitan area as a whole, in several dimensions of its attributes, the models could not provide adequate richness of detail for a less-than-comprehensive view. A lump of population, probably located within half-a-dozen census tracts, is helpful as a forecast to a very limited number of people. Planners for a utilities or school district might be interested, but they probably would not need a land-use model in order to get the information. On the other hand, most of those who would be interested in land-use forecasts will also want something else that the model does not provide.

The notion persists that combining different purposes generates sufficient economies of scale to be able to reach a significantly higher plateau, i.e., a general-purpose model. Even if the notion were correct, building a model without specific purposes is about as helpful as collecting data without knowing who the users are.

#### *Hungriness*

Data requirements of any model that purports to realistically replicate a specific city are enormous. San Francisco's housing market model needed 15,000 items of data for a single run, excluding all the data used in calibrating and testing the model and its submodels; this for a city with a stable population of slightly more than 700,000 people. Collecting twenty variables for census tracts in the county that contains Pittsburgh, for each of three years (e.g., 1950, 1960, and 1970) produces about 30,000 numbers. Yet this

information could hardly be considered a complete description.

Data constitute the window through which the model views a city. Strong theory can extract much from these data, but it cannot add new information. It is sometimes helpful to consider what a model ought to know in order to produce a given output, before asking for the output.

#### *Wrongheadedness*

Perhaps the least discussed problem in modeling is the deviation between claimed model behavior and the equations or statements that actually govern model behavior. The impression is often given that the structure used in a given model is adaptable to any kind of system performance that might be encountered, but, in fact, a specific model may absolutely preclude some alternatives and combinations, and it may substantially distort many others. The deeper problem is that relationships between variables other than the specified ones are implicit in the model and often difficult to perceive. In the case of a highly structured model, this task is much easier and the problem correspondingly less important; in highly descriptive models, the implications of model structure are practically inscrutable. A few examples may provide some insight into the difficulty:

a. Trip distribution functions (the heart of any gravity model, which includes most of the land-use models) are fitted to observed trip frequencies for different classes of workers and (sometimes) different classes of households. While valid at the scale of a metropolitan area, the gravity model has no statistical explanatory power at the neighborhood level. This is a near-classic case of imputing individual (or census tract or neighborhood) behavior from aggregate relationships—the ecological fallacy. In addition, the purely descriptive nature of the gravity model implicitly assumes that the existing land-use patterns served by existing transportation, with existing prices, qualities, and service levels, will be maintained in the future. To then ask the model what will be the impact of a change in the *type* of transportation service provided is pointless; the model lacks the structural information to be able to trace out the consequences of such a change. Models (such as Empiric) which are even more descriptive than the gravity models are correspondingly more committed to the status quo.

b. The Herbert-Stevens model is probably the most highly structured of the land-use models intended to be operational, and thus the most transparent. It was formulated as a linear program, and one of the

important features was the interpretation of the dual variables as land rents. This consequence of the model structure was clearly pointed out, but a number of others were not:

- If any amount of a particular land use is allocated to a particular zone, no other land use will be allocated to that zone unless the supply of that land use is exhausted.
- If the supply of a particular land use is insufficient to fill a particular zone, the rent for that zone is zero unless there is another land-use category bidding in the zone.
- In general, no more than two land-use types will ever occupy the same zone, and there will be no more zones with mixed land use than there are categories of land use.
- Since the rent is a variable attached to a zone, all land uses in the same zone pay the same rent. In mixed zones, the program grants a "subsidy" to the lower-paying land use in order to allow it to live there.
- The "subsidies" are variables attached to land-use categories, and hence a land use which receives a subsidy in one zone must receive it in all zones. The consequence of these two effects is that all land uses save one receive a subsidy, and the subsidies are cumulative from one land use to the next.

These results are obtained by straightforward application of the standard duality theorems available in any linear programming text and do not in the least depend upon the data (or, of course, the functioning of the real estate market).

Other examples could be listed. For most large-scale models, limitations or unintended constraints resulting from the model structure are almost impossible to perceive, and so remain unknown.

#### *Complicatedness*

As the number of components (e.g., variables) increases in a model, the number of potential interactions between them increases as the square of the number of components. A model with 20 endogenous variables has 180 possible interactions if they are symmetric and simultaneous, or 380 interactions if causation each way is allowed. Since most models have several dependent variables for each tract or zone, the number of variables commonly encountered is on the order of 1000 or 2000 as a beginning. Adding lagged variables increases potential relationships by another exponent. Obviously many of these relations can be eliminated a priori and others can be combined into a

single equation, but the sheer magnitude of the choice available places a strain on conceptualization. Once inside the model, of course, the interactions occur whether they are indirect or direct, or significant or not. One of the rationales for large models is to allow for these interactions so as to include secondary and tertiary effects, but permitting the interaction does not necessarily mean that the modeler either has any control over it or learns anything from it.

A central idea in modeling is that if components, whose behavior is known, are hooked together in a known set of relationships, the behavior of the resulting system can be explored. As applied to urban modeling, the technique is called simulation. Many problems emerge from this fairly simple and plausible sounding idea, and they are due to error and complexity. Either one can easily get out of hand, and the combination is volatile.

Whatever the components of a city or a model city are, their microscopic behavior is largely unknown. The best information we have has to do with aggregate relationships that include the effects of an unknown but large number of other variables. To assume that these relations hold true in the same form when all other variables are allowed to vary independently has no basis in theory or experience. Multicollinearity (correlation of independent variables) makes the statistical evaluation criteria meaningless as well as making the coefficient estimates unreliable, and misspecification (omission of important variables) also makes the coefficients unreliable. Alonso (1968) has provided examples of how a model constructed of such relationships needs to progress only a few steps before it is producing pure noise. In practice, the models make literally millions of such steps.

One revealing example of model behavior comes from experience with the San Francisco housing model. The model was completed in 1965, and a few test runs made on it before work was stopped. The estimates of housing construction produced by these runs fit actual data in an intuitively satisfactory way. One entry was substantially off, but five of the six marginal totals differed from the actual by 10 percent or less, and the two-period total was only 3 percent off. Later, additional data became available, including new data and more detail. Testing the model against the new data showed that previous conformance on totals had been achieved at the expense of large compensating errors in subcategories and that even a careful adjustment of the parameters left some major categories at well over 100 percent error (San Francisco Department of City Planning, 1968). One of the extreme cases was

in luxury apartments, in which the model predicted 1,092 new units in a two-year period while the market produced only 64.

Another example concerns the use of the Pittsburgh TOMM for estimating the spatial impact of housing clearance for a large expressway. An early run (after the model had been constructed and then shelved) produced results that were obviously gibberish. No comment was made. Subsequently, a research group at Harvard tried the model and managed to relocate households in areas adjacent to the clearance area and in a separated donut around it (Kilbridge et al., 1970). The reason given for this latter effect was that the residents worked in the space between and had been symmetrically relocated. No data were available for testing the model's accuracy.

Error and complexity combined make a model hard to evaluate, and attempts to do so may be misleading, as in the case of the San Francisco model. The task of evaluation is also not helped by inappropriate measures of performance. For example, the Empiric model for the Boston area reported correlation coefficients between estimated and actual land use that were insignificantly different from perfection (Harris, 1965). Inputs used for estimating test-year (1960) values included, however, base-year values (1950) for all the endogenous variables plus the actual values of exogenous variables "forecast" from 1950 to 1960. The correlation used for evaluation, in this case, is completely spurious, since it measures nothing but autocorrelation. A simple correlation between the 1960 variables and either the 1950 endogenous variables or the exogenous variables would have produced a coefficient of at least .9 in value.

A more useful evaluation criterion could be based on actual versus predicted *changes* in the endogenous variables, using several forecasts of the exogenous variables. Even this measure is not clearly neutral, due to something which might be called the size effect; namely, larger zones are likely to manifest larger changes, whatever other characteristics they may exhibit. Thus many observed statistical relationships are nothing more than the result of various quirks, despite an apparent conformance to standard tests of significance (although not to the assumptions upon which the tests are premised).

Suppose, hypothetically, that the statistical problems of error and uncertainty could be completely eliminated, and a "perfect" model were developed. The model would, of course, be very large and very complex. Imagine a user going through the process of collecting inputs, starting the model up, observing the

results, changing one or more of the inputs, and again observing the results. For any given vector of inputs  $X$ , a vector of outputs  $Y_t$  is observed for each point in time. This is classically known as a "black box." What goes in and what comes out are known exactly, but the process by which one is transformed into the other is a mystery. The linkage that results in an increase in variable  $y_i(t)$  as a consequence of a decrease in policy variable  $x_j$  cannot be determined, hence there is no way to find out whether the relationship holds in the real world. How can such a model be validated for use as a policy analysis tool? What politician will believe it if the model goes against his intuition or his self-interest?

Finally, large models are not simply constructed and operated; they must be "massaged" into being, first to make them operate at all and then to get sensible output. Inevitably this requires numerous special features in the computer program that keep the model from going out of bounds, so that output described as "reasonable" is a self-fulfilling tautology. The model produces reasonable results because its builders imposed constraints on the model's operation that prevent it from producing anything else. Because the models contain large but unknown amounts of error and they are too complex, and there are no evaluation measures, modelers have little choice except to fudge the models into shape.

#### *Mechanicalness*

All large-scale models must be implemented on a digital computer, and the machine may be simply a fast device for carrying out the required calculations and decision trees or it may introduce additional problems. Aside from the difficulties of preparing and debugging a program which will represent the model correctly (no small feat), the computer has two characteristics that must be explicitly dealt with: (1) a solution which is "exact" is effectively impossible, because there is always some amount of rounding error; and (2) all solutions are achieved iteratively, i.e., one step at a time.

Numerical error may be small if the model structure is essentially an accounting framework (such as the San Francisco housing model), the computer uses long words and high-level hardware (such as Control Data 6400), and numerical procedures are robust. Numerical error may be large if a large matrix must be inverted (as in a system of simultaneous equations), if the model equilibrium is obtained through successive adjustments (as in the TOMM), and the computer uses short words (such as the 6 or 7 significant figures nor-

mally used in the IBM 360). Design of any computer matrix inversion routine requires careful attention, but implementation on a short-word machine necessitates a battery of highly specialized techniques. Yet many routines currently available for this standard problem cannot produce one-digit accuracy for a mildly pathological case (Longley, 1967; Wampler, 1970). Since the large models rarely fit the standard algorithms, their numerical accuracy is not only unknown but untreatable.

Iterative procedures are the staff of life of a digital computer. If a model fits a standard format, such as a system of linear equations, then algorithms have been prepared that make the result numerically no different from a simultaneous solution. The more common model is a complex set of simultaneous equations, constraints, tests, and branches that are adjusted iteratively until the solution stabilizes. The result then depends in some part on the *order* in which the model is solved (an interesting test but one surprisingly difficult to carry out would be to shuffle the input data). No modeler knows the extent to which his model is sensitive to this criticism, and hence the actual computer model is, at best, a hopeful approximation of the documented model.

There is a popular illusion that confronting a computer with one's ideas enforces rigor and discipline, thereby encouraging the researcher to reject or clarify fuzzy ideas. In the very narrow sense that the human must behave exactly like a machine in order to communicate with it, this is true. But in a more useful sense, the effect is the opposite; it is all too easy to become immersed in the trivial details of working with a problem on the computer, rather than think it through rationally. The effort of making the computer understand is then mistaken for intellectual activity and creative problem solving.

#### *Expensiveness*

The cost of most of the modeling efforts ran into the millions of dollars. While it is difficult to identify the specific costs of any particular model, a rule-of-thumb estimate for a full-scale land-use model is probably at least \$500,000. This includes the design and development costs, hence the marginal cost of additional usage goes down. Yet the City of San Francisco estimates that it would take another \$250,000 to get its already working housing market model into shape for potential usage. Like almost all such efforts, there has never been any question of continuing development of a model utilizing the local resources of the city in question. As long as the models were heavily supported by federal agencies, there was some willing-

ness to tolerate them, but even this willingness was exceeded in Pittsburgh. The City Fathers decided to not even apply for an extension of their modeling grant.

An appealing possibility often appears of using an existing, already programmed model, developed for another city, at a cost substantially less than the original cost. The possibility is illusory. Getting data and learning how to use the model require more effort than the output is worth, for even small uses. Models that actually work produce results that are applicable to very little, and fully calibrating a model for local conditions is usually almost as big an effort as the original.



#### **CONSTRAINTS THAT ARE NOT BINDING**

Urban modeling has attracted some investigators who believe they have perceived a breakthrough in some particular area, and think the models are now feasible. Whether the models are desirable is of more interest here than whether they are possible, but it is instructive to consider what some of these breakthroughs are.

##### *The Monocentric Breakthrough*

A basic model in rent theory makes the simplifying assumption that a single market exists at a dimensionless point in the middle of an infinite undifferentiated plain. This point may be thought of as a city or possibly as the CBD of a city. In reality, of course, markets are not dimensionless or perfectly concentrated, and an operational model must take account of this. Employment locations are taken as the market for labor, and each location generates attraction for work trips; shopping operates in an analogous fashion. These are the conceptual underpinnings of the gravity model. Despite the fact that no large-scale model from Lowry forward has restricted any trip attraction measure to a single point, there are occasionally those who feel that it is the monocentric assumption that is holding that field back (Stegman, 1969; NBER, 1971).

##### *The Systems Breakthrough*

Another group has thought that the missing ingredient is the ability to deal conceptually or numerically

with large-scale systems (Cooper et al., 1971). It should be fairly clear from the previous discussion that new computer algorithms will not touch the important problems in modeling urban systems (these algorithms are not directed at the purely numerical difficulties). Operations research has been usefully applied to certain well-defined highly structured problems with simple goals, but not yet to urban modeling (see Hoos, 1969).

#### *The Computer Capacity Breakthrough*

Despite the many-fold increases in computer speed and storage capacity occurring over the decade, there are some researchers who are convinced that it has been the hardware limitations that have obstructed progress and that advances in modeling are now possible because of larger computer capacity (NBER, 1971). There is no basis for this belief; bigger computers simply permit bigger mistakes.

#### *The Dynamic Breakthrough*

The majority of models currently in use are incremental comparative-static in structure. Two static equilibriums that differ in some limited way (different policies, different time) are compared, and impacts or predictions inferred. It is always possible to say that these models are not dynamic enough, but this does not necessarily mean that they should be more dynamic and less something else, or even more dynamic at greater cost (Batty, 1971; Forrester, 1968). Dynamic models, can be used to complement cross-sectional models, but making the land-use models more dynamic would be no more productive than using smaller zones or more household types.

### **THEORY IN MODELS**

Two aspects of the relationship of theory to the land-use models will be considered. One is the question of the extent to which theory was used in the models, and the other is the extent to which the models contributed to theory. The conclusion in both cases is that the models had almost nothing to do with theory.

The proper relationship of theory and models is a subject of the philosophy of science, and cannot receive adequate treatment here. When the models are expressly for purposes of policy analysis, the pragmatic criterion must be the primary basis for evaluation, and this test we have already applied. In fact, some social scientists (Scriven, 1971) suggest that *all* social science research ought to be measured against a social policy problem. Nonetheless, it is possible to

provide some information about the use of theory in the large models without attempting to exhaust the subject.

Two points need to be emphasized: the amount of theory available is nowhere near sufficient to support a large-scale urban model, and the choice of theory necessarily limits the uses to which the model can be put. Location theory and rent theory were natural places to begin searching, but the various streams of thought whose origins lie in von Thunen, Losch, and Weber are either very abstract (hence they ignore too much), very partial, or observable only at a highly aggregated level. From the other side, the literature on the real estate development process is so microscopic as to be ungeneralizable. Economic theory has largely shunned these two areas, at least partly because they are so unmanageable.

Lacking a coherent body of theory that could explain land-use dynamics, the modelers turned to analogies and descriptive regularities. The untested hypotheses of a variety of social science fields were accepted uncritically and merged without ever establishing the validity of either the individual relationships or the combined structure. One example is the gravity model, which was fitted to aggregate data based on existing land-use patterns and trip behavior, and then employed as a *behavioral* explanation of future patterns. In between, a formal theory was developed to rationalize the transformation, on the postulate that households located so as to minimize some type of trip purpose. Other examples take such forms as "like households tend to attract each other," "development patterns in the same area tend to remain the same," "only some households are free to relocate in a given time period," "commercial activities are usually found in clusters," "the capital stock adjusts slowly over time," and "prices increase more readily than they decrease." While the assertions may be true, in the qualitative form stated above and when all other things are equal or unchanging, they are not the structural relations upon which a model can be built.

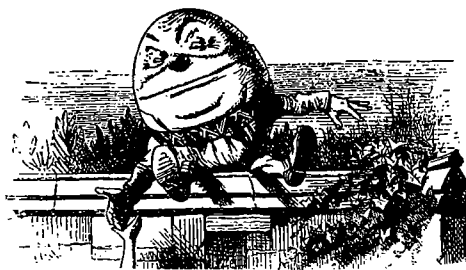
The other point—that usage of the model is dependent upon which theories are selected—is analogous to using a regression to estimate the dependent variable for values of the independent variables much larger than any used to fit the equation. A model constructed of empirical regularities might be very useful for interpolation between data points or for minor marginal changes, but it is dangerous to use it for prediction under as-yet-unobserved conditions. Models of the style exemplified by the land-use models are inherently bad for long-range forecasting, even



when they can accurately replicate the present. Econometric models, which are similar in many ways to the land-use models, are not used for forecasting beyond a few quarters.

On the other hand, if the simulation format is used, then behavioral relationships are mandatory and the behavior of each component must be fully known under *all* possible conditions, not just the specific set used for fitting. Such a model allows the user to investigate the limits of the system and test its performance under different values of the parameters, but such a model of a city is impossible to build. Partially described relationships are then, at best, only useful for answering questions about those particular relations. A model that does not explicitly understand the impacts of transportation on land use cannot be used to predict those impacts, even indirectly. A model that is predicated on moderate-income families that live in suburbs and drive automobiles cannot be asked how to change that situation.

If the models did not have much theory to apply to the task, is it still possible that they helped in constructing new theory or could help in the future? Unfortunately, instead of testing the hypotheses upon which they were built, the models revealed nothing about the correctness of the structural relationships. The processes of fitting and calibration both assume that the equations are accurately specified and true, so the end product was an unknown mixture of explained variation and force fitting. Had the modelers taken the time and effort to validate all the components of their models, none of them would have been completed. At best, large models might verify or reject some hypotheses as by-products of their central mission, but there would seem to be better ways if theory-building is the objective. Thus neither did the models apply theory nor did they add to it.



#### RISE AND FALL OF THE MODELING MOVEMENT

The decade of the sixties provides some closure with respect to modeling; a cycle began, ended, and is now ready to begin again. Nothing ever really gets completed or ends with a neat tying of strings, but we seem to have just passed a lull in the level of model-

ing activity (in the large-scale models) and are starting again on the uphill side. The last section was essentially a cross-sectional slice across the models, and this section will represent a longitudinal view. A central question is how well we learned from experience with the modeling efforts.

Three components of the dynamic process can be distinguished: (1) changing methodological or technical capacity and changing perceptions of it; (2) changing goals of the modeling efforts; and (3) changing views of planning and its social context.

#### *The Science of Modeling*

The large modeling efforts were begun during a period when a high degree of optimism existed about the ability of science and the scientific method to solve our problems. Sputnik was a major stimulus, and support for science was big, both governmentally and popularly. Formal models, computers, data banks, systems analysis, empirical testing, and operations research were the means for achieving a breakthrough in social problem-solving. As the middle of the decade approached, optimism within the ranks was still high but expectations were severely reduced. Results seemed to recede miragelike. Operations research and economic theory concerned themselves more and more with the development of theory, mostly mathematical, and gave up most pretense of application. Systems analysis got us into space, but at a cost far greater than anyone was yet prepared to spend on city problems. Storage of data turned out to be a much more difficult problem than anticipated, yet almost trivial compared to the problems of using the data. Computers gave the impression of consuming time rather than saving it. By the end of the decade, it was clear to most planners that the techniques were not going to come close to delivering on their promise, that the formal models fit urban systems only very superficially, and that even applying very simple techniques to very immediate and partial problems was not all that easy.

Actual analytic capacity increased during the period. Computers got larger and faster by at least an order of magnitude, algorithms for solving many standard problems and their variations were developed and computerized, numerical analysis became an important applied branch of mathematics, such fields as information storage and retrieval and management information systems developed almost from scratch, and the quality and quantity of data probably improved. But initial conceptions were so far beyond actual capability that the increase in technical capacity was much more than offset by the awareness of real limitations.

### *The Goals of Modeling*

In the early stages, statements about objectives were largely in response to the prevailing conventional wisdom in the field, concerned with physical master planning. Gradually, the modelers created a goal-set of their own, which became almost impervious to external sources of feedback. Finally, goals were altered in the light of obvious performance deficiencies, and emphasis shifted to goals which had been by-products in the early stages.

#### *Early Goals<sup>2</sup>:*

- To understand factors that influence land developments and provide a better factual basis for plans. As planners implicitly accepted the instrument of the comprehensive long-range plans, models were expected to provide a better rationale for making the plans. By understanding the market forces underlying urban structure, planners would create more realistically achievable plans, and simple efficiency would supplant the aesthetic criteria then in use for plan making.
- Accepting the need for a plan, to reveal the inconsistencies and high costs to be found in plans drawn up by planners without models. This conflict led to the plan-versus-projection debate, and the labeling of models as "deterministic."
- To evaluate interdependencies and potential conflicts between separate planning programs. While comprehensive planning ostensibly already did this, it could not deal with the interactions of secondary effects because they were too numerous and too complex. This objective was one of the first to be abandoned because it required not only several large-scale models but numerous smaller interconnecting models; in short, a complete management system.
- To predict. While seldom stated quite so simply, there definitely was a strong idea in the minds of modelers that cities could be constructed synthetically—that would, in effect, operate themselves. While obviously there was some limit on the time horizon of the prediction, twenty or thirty years did not seem out of the question.
- To control and direct urban growth. The implication here is that sufficiently strong causal relationships could be identified and quantified between various control or policy variables and the objective or state variables.

#### *Middle Goals<sup>3</sup>:*

- To contribute to urban theory, although there is a subtle shift in that the understanding might not be

complete, only a contribution, and that some of the comprehensive planning goals were being played down.

- To make conditional predictions, shifting toward forecasting the values of attributes for future points in time as a consequence of conditions such as possible states of nature and exogenous variables including policy variables as well. This, of course, reduced the burden on the model to know everything.
- To evaluate the sensitivity of urban growth to alternative planning policy. Doubt began to creep in as to whether planners and policymakers could seriously affect the course of urban growth, or at least whether the models would be able to identify any effect.
- To educate planners in urban structure. Another hedge that appeared more strongly as time went on was the one that even if the models were not fully operational, everybody learned a lot, and this alone justified the effort. The modelers were supposed to have learned by working with their ideas in a formalized framework, and the nonmodelers by interacting with the modelers and having their fuzzy concepts subjected to the rigorous discipline of the computer format.

#### *Late Goals<sup>4</sup>:*

- Impact analysis. The third stage in the quest for prediction, impact analysis, was simply the difference between two controlled conditional predictions. One forecast is usually the "null" (no public programs) or "most likely" state, and the difference between that set of states and those for the alternative program is the impact of the program. It is thus not necessary to be able to accurately replicate the real world at any specific point in time but to replicate relative differences accurately. Modelers regard this as an easier task.
- To forecast small-area population, employment, and land use. Using a metropolitan framework, zones much larger than census tracts, and considering growth allocation only, statistical extrapolation for short increments of time is considered do-able. In effect, these are spatially disaggregated multiequation econometric models.
- To educate lay decisionmakers. This is perhaps the ultimate comedown, as it means using the models as heuristic aids in the context of operational gaming. Players make decisions in the synthetic city, observe the consequences, and make new decisions.

## MODELING IN A CHANGED PLANNING CONTEXT

At the same time that modelers were learning something about the applicability of science to models and modifying their goals to some extent to reflect what they learned, the context of planning itself was changing.

### *The Comprehensive-Incremental Revision*

Up until at least 1960, the most commonly held view of planning by members of the field was the three-stage process of goal formulation, plan-making, and implementation (see Altshuler, 1965). Even the first stage was weak at the beginning because most planners felt that the goals were obvious. Effort was directed primarily at creating a plan according to a set of principles that incorporated the supposedly universally held goals. Implementation amounted to selling the plan to the community (like missionaries to the heathens) and administering zoning regulations.

A different school of thought emerged from the fields of political science, management science, and organizational behavior, as represented by Lindblom, 1959; Banfield, 1959; Simon, 1957; Wildavsky, 1966; and Clavel, 1968. Actual decisions, it was pointed out, were not made according to broad goals and grand plans but, rather, in response to incremental problems which were within the immediate sphere of influence of the decisionmaker. While the administrative model is not necessarily ideal from a normative standpoint, it introduced a note of reality into planning thought. These ideas were easily available in the early part of the decade, but they did not have much impact on planning practice until the second half, and the modelers still remained apparently unaffected. Perhaps this is not surprising when it is realized that the large models symbolized the last offensive of the technocratic, hypercomprehensive mode of planning.

### *The Goals of Planning*

At the beginning of the decade, planning concerns were a combination of a rather simplistic efficiency (not constructing a building that would later have to be torn down to make way for a highway) and a desire for amenities, including parks and playgrounds, open space, and good architectural design. Issues of poverty, powerlessness, racism, and the redistribution of wealth and income gradually commanded attention within the field, and the large-scale land-use models could not adapt to the new issues. In a few cases, such as the relocation of households displaced by an expressway in Pittsburgh, the models attempted to respond

to some of the new needs, but it was more as an incidental by-product than a redirection of effort.

### *Confidence in the Social Order*

When the models were first proposed and developed, the general tenor was one of belief that the existing social order, its elected and appointed representatives, and its government could and would solve whatever urban problems as might exist. We were all working together, and men needed only to be shown the right way for them to follow it. Planning was oriented toward the top of the pile rather than the bottom, and large-scale models as a technique fit into the hierarchical view of social administration (critics who argue that methods are not value-free have a valid point here). We accepted, more than now, the rule of experts. Confidence also encourages one to look further into the future, on the assumption that the little things will iron themselves out.

Probably the most radical shift in public thought that has occurred in some time was the general realization that government (or U.S. governments) did not always know best and more often than not responded to special interests rather than the general welfare. An important additional component was the belief that this situation could be changed, and primarily by collective citizen action working outside the established order. Needless to say, the big models were not suited to citizen participation and played no part in the new style of democratic government.

## ORGANIZATIONAL LEARNING WITHIN THE MOVEMENT

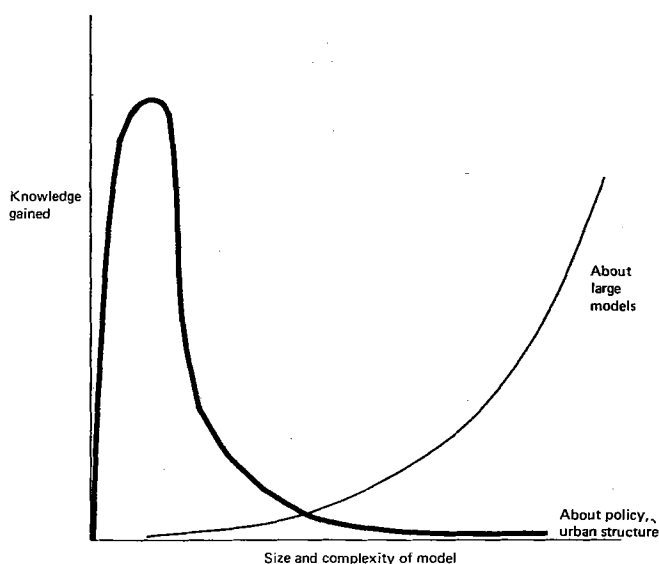
An important question with disturbing implications is why did none of the various modeling efforts reorient their designs as experience became available? A few did—the San Francisco and Pittsburgh Community Renewal Program modeling projects eventually scrapped most of their overly ambitious plans—but, for the most part, the modelers were especially insensitive to outside information, even when it came from other members of the fraternity.

Two events mark the major period of activity within the planning profession. First, the *Journal* devoted an entire issue to the topic in 1959, and an awareness of difficulties yet optimism in a new direction were expressed at that time. The second event was a similar issue of the *Journal* that appeared in 1965. While the tone was still optimistic, several sharp critics could be found, and the movement subsequently went underground.<sup>5</sup> A conference sponsored by the Highway Research Board was interpreted by planners as

a lagging indicator, but this may have been the point at which the engineers became interested.<sup>6</sup>

Within most of the agencies where modeling took place there was a dichotomy between the modelers and the antimodelers which had the effect of minimizing the amount of learning that occurred.<sup>7</sup> Modelers tended to be self-confident, optimistic, and self-conscious of their role as innovators in the vanguard of the profession; antimodelers resented and feared the modelers to a degree that verged on paranoia, were overwhelmed by the jargon and paraphernalia that came along with the modelers, and hesitated to ask the questions that might have at least produced a coherent team out of the participants. The modelers failed to see the importance of explaining to their fellow professionals and their clients the substance of their work and were too impatient to take the time to do so anyway. The effort required probably would at least have equaled that of constructing the model itself.

Besides failing in the face-to-face communication of their efforts, the modelers also failed in their evaluation and documentation of the experience. Partly this was because they were also behind schedule, the projects required more effort than had been anticipated, and the results produced did not create a compulsion to get the word spread far and wide. As a consequence, each study learned independently that the plan-making and evaluation process was much more difficult than had been anticipated and much less productive also.



As work on the models began winding up, a few reached the state of being declared operational. After two runs with the San Francisco model, the consultant pronounced it successful and ready for policy analysis of the Community Renewal Program and a tool for

understanding the complex real world system it simulated (Arthur D. Little, 1966: 40). Two years later, with the benefit of additional data, a local review of the model found that it could not be made to fit the new data and behaved perversely under stress. No public evaluation is available for most of the other models, and in most cases none took place. The few attempts to develop practical methods by the modelers outside the format of the big models were unsuccessful and unreported. Many of the modelers were highly motivated with the best of intentions, and many worried about the lack of success. Others, even as late as 1966, apparently assumed that the benefits of large-scale modeling were self-evident and were advocating the construction of big simulations even at the expense of short-range planning.<sup>8</sup>

Pragmatic failure of the models shows most clearly in the recent turn of events in transportation planning. Here, the models were used most extensively and performed as they were expected to; yet the area transportation studies were still proposing more highways while the inefficiencies and the inequities of the highway system were becoming apparent to large numbers of people, many of whom had no particular expertise in transportation planning. Because the models assumed—either explicitly or more often implicitly—that the important variables were fixed, the resulting plans simply amplified the existing distortions in the transportation system. Lupo (1971) describes one case in which citizen opposition in Boston eventually corrected the modeler's assumptions.<sup>9</sup>

What should a model be able to provide in order to improve transportation planning? Two approaches would be useful. One is to construct a number of accounting models that simply keep track of the state of a number of variables of interest under alternative corridor plans. The amount of land consumed, the cost of the land consumed including demolition, the number of residents displaced and their income circumstances, the spatial extent of such external costs as noise and atmospheric lead, the changes in volumes on feeder networks resulting from the proposed system, and a number of other variables would be worth tabulating. The second approach is to build explicitly partial models aimed at specific policy questions. How would traffic change if a parking tax were instituted or a bridge toll increased? What is the best kind of development to have around a transit station? What are the true social costs of each mode, and what would be the consequences of using these costs as a basis for user charges? How should demand be forecast so as to provide the right investment in the right mode at the

right time? So far, transportation planning has ignored these questions, yet quantitative models of some kind are clearly called for.



### CURRENT ACTIVITY

There would be little need for a review of the past experience were it not for the resurgence of interest in models of the large-scale variety and the apparent disregard for the sources of previous failure that is evident in the current movement. Most of the pressure comes from outside the field of city and regional planning, but planners should be concerned because models are diverting talent and funding from what could be more productive uses in terms of solving urban problems. At least two forces seem to be present in the modeling renaissance:

- Innovation travels amazingly slowly in this day of instant communication, and some people are just getting excited about models for the first time. Some of these people are in highly technical fields that are looking for a social purpose and some are in foreign countries that want nothing short of the most modern methodology.

- Emphasis in public programs has shifted somewhat from the peak of interest in technology during the Sputnik era toward broader social problems such as education, housing, and the environment. Successes in aerospace have encouraged the application of systems techniques and technology to social problems, and funding shifts have set physical scientists and aerospace engineers to work in the federal bureaucracy and private industry on the new problems. Thus among people who were doing other things in the early sixties there is still a strong attraction to engineering systems analysis as a means of solving urban problems.

### *The Forrester Model*

The splashiest entry in the new race is Forrester's (1968) differential equations model of a city. There are several good reasons for not considering this

model in the present context: it is not spatially disaggregated, and it is of an abstract city, in that it uses no data. It is also a poor example of the class, and bushels should not be rejected on the basis of one bad apple. But because the approach has received a lot of attention (e.g., Kadanoff and Weinblatt, 1972), has attracted a substantial following, and exhibits most of the major flaws of large-scale models, it must be evaluated.

The model claims to show that public programs that benefit the poor only make the situation worse, and this result is called "counter-intuitive" by Forrester. A quick glance at the model structure reveals that the problem households (who cost more in services than they contribute in revenues) will multiply in response to poverty programs and low-cost housing, but will disappear if these programs are not provided. Forrester learned what he knows about cities from John Collins, a former mayor of Boston, and it is not too surprising that a major public official in a large central city should hold these views. But it is more than misleading to offer this conception as a generalized model of a city; it is incorrect.

If Forrester's point is that public programs have often produced results that were contrary to those intended, many people—Milton Friedman being a notable example—have been saying this for a long time. The rational response is to try and find out what went wrong, and one cause may have been a failure to understand the structure of the problem. Rather than illuminating this structure, Forrester's model buries what is a simplistic conception of the housing market in a somewhat obtuse model, along with some other irrelevant components.<sup>10</sup> He then claims that the problem cannot be understood without the irrelevant complexity.

What is most disturbing about this model is the gulf between what it is and what Forrester claims it is. The model is unsuitable for policy analysis because it has only a single response, which is built in; other responses could be and have been built in, but they are different formulations of the same or different problems from different viewpoints.<sup>11</sup> There is no "correct" model. The uses of such a model might be two-fold: (1) trace out the consequences of different assumptions about the nature of the problem, making the assumptions very explicit; or (2) employ the model in an operational gaming context to teach students and lay decisionmakers about the structure of cities and possible sources of secondary effects. Particularly interesting would be to model the role behavior of other decisionmakers besides central city mayors,

and let citizens and administrators deal with them in model form.

### *The NBER Simulation*

A group of modelers at the National Bureau of Economic Research is in the process of constructing a large-scale simulation that is both more microscopic and more ambitious in scope than any model actually realized up to this point (there have been others that were more ambitious in design). Descriptions can be found in NBER, 1971; and Ingram et al., 1972; previous work out of which the current effort emerged include Kain and Meyer, 1961; Niedercorn and Kain, 1963; and Kresge and Roberts, 1971. A similar but unrelated project at MIT is described in Engle et al., 1972.

This model probably is representative of the best of the current designs, but it is, nonetheless, simply larger doses of all the things that have failed to work in the past. Primary emphasis has been placed on the housing model, which is very similar to the one Arthur D. Little built for San Francisco. Household location decisions are based on access to employment (as in the Lowry model), disaggregated equations for different household types and different housing types (as was done in the ADL model, although the latter used a larger number of types), and spatially located zones. A portion of households are designed as "movers" (as in the Pittsburgh TOMM) and the housing stock is "deteriorated" (as in the ADL model), giving the NBER model approximately the same kind of pseudo-dynamic qualities as the models that preceded it. A final "unique" feature of this model is that it is being built by economists on the expressly stated belief that none of the others were. Policy applications are not expected for several years.

### **PLUM**

The Projective Land-Use Model has been patiently nurtured by William Goldner for the better part of a decade, and is one of the most operational—in the sense that it can be plugged in, turned on, and made to sit up and bark—of the conventional land-use models. It has been used in the Bay Area Simulation Study, the Bay Area Transportation Study, for some airport location analyses, by the Metropolitan Transportation Commission, and for the City of San Diego, among others. Documentation is thorough and can be found in Goldner, 1968, and Goldner, et al., 1972.

An evolutionary form of the Lowry model, PLUM contains enough adjustments, plausible fudge factors, constraints, and empirical descriptive relationships so that it produces "reasonable" results. It also is almost totally opaque, as far as connecting inputs with

outputs, and can thus be properly regarded as a "black box." Unless it tells him what he already wants to believe, it is hard to imagine what a policymaker might do with this model. Support for PLUM has come from the Federal Highway Administration, whose interest may be characterized by a desire to keep patterns of transportation planning about the same as they have been in the past.

Other models of the PLUM type exist, but most of them are in the hands of private consultants, who have a proprietary interest in seeing that the models are used as much as possible without being discussed too openly.



### **GUIDELINES FOR MODEL BUILDING**

Probably the most important attribute any model should have is transparency. It should be readily understandable to any potential user with a reasonable investment of effort. "Black-box" models will never have an impact on policy other than possibly through mystique, and this will be short lived and self-defeating. A transparent model is still about as likely to be wrong, but at least concerned persons can investigate the points at which they disagree. By achieving a consensus on assumptions, opposing parties may find they actually agree on conclusions. A good deal of effort will be required to get people to think this way, because it is a trait of modelers and nonmodelers alike to throw in everything at once when they do not understand a problem; odds are that somewhere in the jumble are the parts to the explanation. The trouble is, nothing is learned from such an exercise, and future problems receive the same treatment. Patience and good will are necessary in this communication and education process; big "black-box" models are not.

From the inspection and evaluation of previous large-scale models and their flaws, a few rough guidelines can be derived for designing future modeling efforts:

1. A balance should be obtained between theory, objectivity, and intuition. Excessive concern for theory results in a loss of contact with the policy problem, but policy cannot be formulated well without a strong theoretical foundation. Overemphasis on objectivity

is one of the major mistakes of the large models, and results in an empty-headed empiricism; on the other hand, most social questions have a quantitative component and require quantitative information to resolve. Some kind of wisdom or judgment is also essential, but intuition in the absence of theory and methodology is useless for dealing with urban problems. Both traditional comprehensive planning and large-scale modeling have been significantly lacking in theory.

2. Start with a particular policy problem that needs solving, not a methodology that needs applying (master planning is a methodology, in this sense). Work backward from the problem, matching specific methods with specific purposes, and obtaining just enough information to be able to provide adequate policy guidance. Overkill is not only wasteful, it is almost always too late. Long-range planning means evaluating immediate decisions with regard to long-run consequences, rather than constructing grand plans or big models.

3. Build only very simple models. Complicated models do not work very well if at all, they do not fit reality very well, and they should not be used in any case because they will not be understood. The skill and discipline of the modeler is in figuring out what to disregard in building his model.

Planning is in the unique position of being oriented around urban problems rather than around any discipline or science. The field can draw from others, selecting only those theories and methods that will be most useful. At the same time, practitioners in other fields are seeking to apply their knowledge to our urgent social problems, from environment to urban services. If planners fail to adopt and adapt theory and methodology as these become available, they will find themselves working less and less on the problems; on the other hand, if planners pick up ideas naively and uncritically, the field will simply jump from fad to fad. Somewhere between lies the optimum path.

\* Helpful comments or ideas from Robert Goldman, Martin Krieger, Eduardo Leira, Ira Lowry, and the *Journal's* reviewers are gratefully acknowledged.

1 An example of this viewpoint is Raymond (in Erber, 1970). A *Journal* reviewer offered the Raymond article as a possible supporting reference; in fact, my position is diametrically opposed to Raymond's. Especially conscientious readers might care to compare the two articles.

2 Some early goal statements are Voorhees, 1959: 57; Harris, 1960: 268; Worrall, 1971: 4. For example:

The use of models in the city planning field will help to:

1. Understand factors that influence land development and traffic problems.
2. Provide a better factual basis for plans.
3. Evaluate and test alternative plans.
4. Develop more realistic plans.

A mathematical model provides a systematic statement of relationships, and hence improves our conceptions of the forces associated with community, growth and related urban transportation requirements. (Voorhees, 1958: 58)

3 Middle goals are typified by Arthur D. Little, 1963: 20, 21; Goldner, 1971a: 100. An example is:

Three principal roles for mathematical models of cities can be distinguished. First, such models have been developed to help in refining and experimenting with hypotheses about the structure of cities; they form an essential part of theory development in urban research. Second, models have been used to provide methods for educating planners in urban theory. Third, and perhaps most important, the models can be used in practical planning studies to help predict the likely consequences of planning or not planning the future of cities. (Batty, 1971: 425)

4 Late goal statements include Batty, 1971: 428; and Crecine, 1969.

5 Few of the modelers in the 1965 publication indicated any reservations about what they were doing; if anything, the forecasts were more ambitious than in 1959. Yet most of the modeling efforts had been cut drastically in scope and expected output. One nettle was Lowry, 1965: 165.

6 Much criticism is contained in the papers and discussions at the Dartmouth HRB conference, but the general attitude still was that the efforts would continue in the same way as before. A careful reader can easily be trapped into regarding this publication as a progress report on the state of the art. The revealing statements are lost in the fringes, such as the following by William B. Ross, then a Deputy Undersecretary at HUD:

We are quite proud to have funded the San Francisco Housing Market Study, but it is still a disappointment to find that it had no relation either to the community renewal program or to the process of renewal in the city of San Francisco. We would like to change that situation. (Hemmens, 1968: 22)

Stronger criticisms have appeared subsequently, but generally received limited exposure (Lee, 1968a; Lee, 1968b; San Francisco Department of City Planning, 1968; Boyce, et al. 1970; Roberts, 1970; Orski, 1970; Worrall, 1971; Lee, 1971; Hemmons, 1970; Bolan, 1970). Most of these focus on a few issues or models, without attempting to evaluate the land-use model as a type; in several recent works (Catanese, 1972; Robinson, 1972) no criticisms appear.

7 Boyce, et al., 1970, contains a good description of what happened inside the organizations.

8 System Development Corporation, in reporting to the city of Los Angeles, recommended that it develop a comprehensive modeling program *at the explicit expense* of short-range planning (System Development Corporation, 1966: 15).

9 The Lupo book contains some specific criticisms of the Empiric model used by the state to forecast land use; while not completely accurate, the criticisms are essentially correct. They were apparently supplied by a planner's advocacy group called Urban Planning Aid.

10 An interesting experiment was performed on the Forrester model by one researcher (Stonebraker, 1972). He removed about two-thirds of the model without altering the remaining parts, which were left intact, and the model performance was not significantly altered. While this certainly bears out Forrester's assumption that system behavior is only sensitive to

certain key variables, it is an odd logic that says they need to be represented in the model.

- 11 Another example of the Forrester style is described in a news article in *Science* (Gillette, 1972). *World Dynamics* is reviewed in Shubik, 1971, which produced an exchange of letters (Gabor, 1972; Hemond, Goodwin and Niering, 1972; Forrester, 1972; Shubik, 1972). Ingram, 1970; Fleisher, 1971; Averch and Levine, 1971; and Kadanoff, 1971, are evaluations of *Urban Dynamics*. Planners would generally rather ignore Forrester, but many academics are strongly positive towards him. For example, see Hemond, Goodwin, and Niering, 1972, p. 109; and

Forrester's assumptions are validated by their plausibility, and greatly reinforced by the insensibility of his results to the details of the assumptions. (emphasis in original)

(Gabor, 1972, p. 109)

Gabor means insensitivity, but what he fails to realize is that it is no particular feat to construct a model that is insensitive to its parameters, or even to much of its structure. Unfortunately, many persons accept uncritically the method because they agree with the conclusions.

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## PLANNERS ROLLCALL

All members of AIP concerned about current trends in national urban policy are urged to participate in AIP's Planners Rollcall for the 93rd Congress. About 600 members participated during the 92nd Congress.

Planners Rollcall is AIP's national legislative monitoring function. Participants work with Al Massoni, AIP's director of national affairs, and contact

senators, congressmen, and others knowledgeable about the effects of national policy on local areas. Activities include testifying at hearings, informing committee staffs, etc.

In return for this participation when it is needed on Capital Hill, those in Planners Rollcall receive a bi-weekly summary of actions on the Hill and new legislation of interest to planning.

### Sign Up Anew

Please note that those who participated in the Rollcall during the last Congress must also sign up anew to continue in Planners Rollcall. This is due to the need for current information in light of the new Members of Congress who will be taking office.

Write to: Al Massoni, AIP national office, 1776 Massachusetts Ave., NW, Washington, DC 20036