MANAGERIAL SKETCHES OF THE STEPS OF MODELING

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ABSTRACT

Observations of modeling efforts suggest that many models fail for managerial reasons. This paper is based on the hypothesis that 1) managerial failures occur because various facets of the modeling process are inherently hard to manage, and 2) that deliberate management can reduce or eliminate many common problems. The hypothesis is pursued by breaking the modeling procedure into a series of steps, sketching what typically does but should not happen at each of them, and putting forth some thoughts about what can be done to avoid the normal pitfalls. Particular attention is paid to mundane variables such as time allocations and finances and to attitudes and emotional considerations. In general, when a modeling study is not deliberately managed, the construction phase preempts the bulk of time and resources to the detriment of planning, conceptualization, testing, documentation, and client-modeler interaction. This phenomenon appears to be caused, in part, by an over-emphasis on the "harder", more technical work of construction; by difficulty justifying work that produces no direct, tangible product; and by mental resistance to testing.

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> The roads by which men arrive at their insights into celestial matters seem to me almost as worthy of wonder as those matters in themselves.
> Johannes Kepler, 1609

I. INTRODUCTION

The modeling literature tends to refer to models as abstract entities. Theory, method, data, and behavior are discussed at length; reference to the modeler, the client, and the institutional context in which they interact is almost religiously avoided. The minds that analyze social and economic processes are seldom found reflecting on the social processes and economic forces which affect them personally as members of a profession.

While understandable in the context of scientific research, such avoidance of the human and social side of modeling is misleading. Models are not brought by storks. They don't grow up in the realm of pure reason, and they aren't nurtured by air. Like another product that storks don't bring, models are conceived in human interaction; are shaped by the environment in which they are formed, and require time, money, and patience to grow into anything useful to society. The practical outcome of a modeling study is as likely to be influenced by the conditions of its conception, the environment in which it is formed, and the amount of love bestowed on the project as by the data the model contains or the theory on which it is built. A well-managed model that is mediocre by academic standards may have a far greater and more productive impact than a technically superlative model that no one wanted.

This paper grew out of field observations of ten public-sector models. It rests on the following assumptions: 1) Certain managerial difficulties are inherent in the modeling process. These difficulties beset all modelers and render many modeling studies ineffectual. 2) Greater awareness of the modeling process can improve management of these inherent difficulties. 3) Process awareness may be developed through detailed observation of actual modeling projects. 4) A self-critical mode is encouraged by presenting, blamelessly (or better yet, humorously), material with which the reader can identify.

Working from these premises, I have grouped the actions that occur during the life cycle of a model into seven managerial steps: preconception, establishing contact, conceptualization, construction, testing, documentation, and implementation. In the course of the paper I shall caricature what typically does, but should not, happen at each of these steps, and suggest ways of avoiding the normal snags and bogs. For simplicity, I shall discuss the steps in straightforward, one-thing-happens-at-a-time fashion. The illusion that modeling proceeds so tidily has been dispelled elsewhere (Randers 1972; Hammond 1974). Here we can make do with a reminder that some steps (particularly conceptualization) tend to be iterative, while others, such as testing and implementation, tend to be ongoing activities.

Most model management problems originate in the early steps of modeling, while ill effects are concentrated in later stages. In this paper I will discuss problems where they manifest themselves, not where they begin. Thus recommendations, particularly for testing and implementation, will frequently refer to what should have been done in earlier steps.

Because it is sometimes possible to correct your own mistakes and rarely possible to correct the mistakes of others, the reader should look at the following comedy of errors more as a description of I-and-thou than as an account of what "they" do wrong.

II. PRECONCEPTIONS

> Thus we economists and other social scientists are now studying intensively how people behave, and how they are motivated and then conditioned both by their inherited constitution and by their environment....Only about the peculiar behavior of our own profession do we choose to remain naive. ...The point is that we could better avoid biases, and could therefore expect more rapid progress in the social sciences, if we were a little less naive about ourselves and our motivations. A minimal desideratum is that we be always aware of the problem and attain some degree of sophistication about the operation of the personal and social conditioning of our research activity.
>
> Myrdal 1968

A. Modeler Preconceptions

Preconceptions are the often implicit and unrecognized forces that shape the model before it becomes explicit, and that will operate behind the model throughout its development. To an extent, preconceptions are adaptive and necessary. Without them, modeling would begin at an unworkable ground-zero of a completely unordered situation with no ordering principles. Yet, sometimes preconceptions are maladaptive. That is, they may order the perceptions of modeler and client

in a manner that poorly explain the real-world situation and poorly answers the client's needs.

Preconceptions take many forms. Some are firmly entrenched, such as the formalized preconceptions of a modeling paradigm, a strong belief in the free market system, or a general skepticism toward, or overenthusiasm for, modeling in general. Others are ephemeral and may fade or change during the course of building the model. The modeler may have just read a report on changing global weather patterns and be convinced for a while that they must be included in the model. The client may have recently received a Congressional mandate to pay more attention to income distribution, but over the course of model construction his focus of attention may shift back toward balance of payment problems. Be they deeply entrenched beliefs or thoughts of the hour, modeler and client preconceptions strongly influence the modeling process.

A. Modeler Preconceptions

On the modeler's side, professional preconceptions (particularly the modeling paradigm) will shape his methodology and structure his thinking. They will focus his attention on certain problems and may make him unwilling to consider others. The system dynamicist's perspective, for example, biases him toward aggregate, closed-loop structures. These biases are apt to blind him to: 1) dispersion problems such as geographical variations and income distribution; 2) hierarchical structures such as differences in degrees and forms of centralization; and 3) parameter-determined short-term behavior (see D.H. Meadows 1976).

Transcending individual preconceptions are the societal preconceptions belonging to the aura of the times. It is impossible to say how, exactly, modeling has been influenced by the social climate of the nineteen-sixties and seventies. However, I am quite certain that the profession would have evolved quite differently had its formative years come during the McCarthy era, the Great Depression, the Spanish Inquisition, or the Ming Dynasty. (For an excellent treatment of the effects of cultural bias on scholarly analysis, see Myrdal 1968, p.5-35.)

Unless we can devise some demon to sit atop preconceptions and sort out the maladaptive, we are going to have to make do with primitive common sense. Here common sense would say "be aware and act with awareness". Preconceptions are always there. To the extent that the modeler is aware of them, he can make them explicit and deal with them directly. To the extent that he does not see them, preconceptions will lurk behind his work, shaping it in fashions unrelated to his intended purpose.

More specifically, be self-critical, examine your motives and your a priori assumptions. Look for the biases inherent in your techniques. Continually ask what you are trying to do and why. Introspection and self-correction are the negative feedback which keep the modeling study on the course of its goals. Without introspection, nothing can prevent preconceptions from taking the reins and steering the modeling study into the land of happy irrelevance.

The usual training of modelers does little to encourage

self-critical awareness. Moreover, the technical nature of the field may actively select against the "soft" introspective attributes that counteract the bullying of preconceptions. The full antidote to preconception problems requires restructuring of values in the modeling profession. Practical moves in the direction of open-minded modeling might include: 1) greater emphasis on field work in modeler training, 2) exercises requiring the student to defend viewpoints he normally opposes, 3) role-playing exercises in which student take the client's role, and 4) encouraging students to look for flaws in their own procedures and rewarding those who discover their own mistakes.

B. Client Preconceptions

In contrast to the modeler's paradigm-centered preconceptions, client preconceptions often lack theoretical structure and are highly responsive to the institutional environment. Rightfully so, for the client can seldom affect change without cooperation from other members of his institution and its supporters. A client from the U.S. Agency for International Development may go out of his way to maintain the good will of Congress; the World Bank may not publish reports that would embarrass member nations. Any client may go out of his way to respect the opinion of a person or group he considers important. Most clients must account for the money they spend on a modeling effort.

Changing institutional considerations can be very disruptive to the modeling process. As an extreme example, one agricultural model that was originally meant to focus on Nigeria was moved to Korea when a revolution blocked the Nigeria study.* A more common occurrence would be that of a client commissioning a population study when the public is concerned about population, and then ignoring it as agency interest shifts from population to resource allocation.

Be conscious of client preconceptions. Imagine yourself in the client's place. Sense the institutional pressures the client is under. Those pressures will be transmitted to bear on you. Distinguish clearly between the client's personal biases and the institutional pressures he is under. Ally yourself with the client in counteracting institutional pressures that are detrimental to effective modeling. Don't accept client biases that will block model usefulness (e.g., short-term focus, point-predictive expectations), but don't take them all on at once either. You cannot affect a person's preconceptions unless he respects and trusts you. Work on gaining confidence. Once confidence is gained, begin on key biases slowly. Most of the biases that repeatedly confront system dynamicists stem from non-systems logic. These biases are best overcome by supplanting other logics with systems logic. Your main task, thus, is an affirmative one--teaching systems logic. Don't get so embattled in the defense of details that you forget to convey the main point.

*The Korean Agricultural Sector Simulation Model constructed under George Rossmiller at Michigan State University.

III. ESTABLISHING CONTACT

This step begins when the specific modeling project is first discussed by the modeler and client, and continues until a contract is agreed upon. The more mundane decisions of model building are heavily concentrated in this early step. Financial arrangements are established, as well as deadlines and reporting schedules, physical locations for research, travel allowances, and research group size. If the client is in the public sector, vehicles to make the modeling effort responsible to the public will be specified or neglected in this step. How high in the bureaucracy the modelers will be heard and on what terms they will be heard will also be determined.

More subtle trends are also established in this period. Do modelers and clients like each other? Is there to be mutual enthusiasm about the modeling project or mutual distrust? Will the modeler listen to the client? Will the client listen to the modeler?

Between the mundane and the subtle matters determined in establishing contact, the operational future of a modeling study is fairly well defined. Where there is a client who knows the system well, the extent to which his knowledge will be incorporated into the model will be constrained by the communication channels established between modeler and client.

The size and complexity of the model will tend to be proportional to the funds spent on modelers' salaries. (There may be cases of modelers making a model simpler by investing more time in it; and even cases of larger staffs creating smaller models. The overwhelming tendency, however, is for complexity to increase in proportion to the time spent modeling.)

The quality of documentation will be heavily influenced by the time and resources allotted to secretarial, editorial, and printing costs and the emphasis placed on documentation in early stages. Where there is both a user-client and a sponsor-client (for example, UN-sponsored models built for member nations), the relationship between the user-client and the modeler is bounded by the arrangements made to convey the model to its intended users.

Finally, the attitudes formed in early stages are likely to color all subsequent work. The budgeting, timing, institutional format, and attitudes developed in the contractual phase of modeling can make the difference between a model that gathers dust and one that becomes an effective planning tool.

Planning a model is no simpler than setting up a business enterprise, planning a battle, or designing a house--and should be approached with no less care. Planning a modeling effort involves coordination of limited resources toward an end. The resources are diverse and their functions complex; humans, machines, institutions, and money are all involved. A modeling study is more likely to succeed if its organizational and material underpinnings are carefully thought through. Investment in the wrong technology, forgetting to account for "human factors", lack of attention to user (consumer) needs, overly timid or overly ambitious plans--all can easily undermine

a modeling effort.

Start from a vision of how you want the modeling effort to proceed through each step of modeling. Imagine the real-world details of each step, how much time will be required to do a good job, how modeler and client should communicate, and what things are likely to go wrong. Make provisions for things that should happen. Take precautions against likely difficulties.

When envisioning how the study should proceed, review the histories of your previous studies and examine the procedures followed by others. Try to avoid repeating mistakes. Pick up on things that appeared productive.

Non-congruent expectations are a rich source of tension and misunderstanding between modeler and client. It is worth checking for them during the planning stage and routing out any that are found. The formula for this precaution is simple: frank discussion, arbitration, and formalized consensus. Have the client relate in detail what he expects you to deliver and what he hopes the model will do. Tell him point by point all the ways you will require his cooperation to do your job, what data you count on him to supply, what sorts of support and cooperation you expect when it comes time to implement the model. Quite likely one or both of you will find the other's notions naive. Continue discussion and arbitration until your pictures for who, what, when, and how are identical--and pragmatic. Finally, to prevent false expectations from re-establishing themselves, put the agreement in writing.

In this and the next step (conceptualization), both client and modeler are apt to become impatient to "stop fooling around and get to work." Resist that valuation. These steps are the most critical and the most difficult parts of the modeling process. Do not be misguided by the absence of immediate tangible evidence of production. Haste is folly when it is unclear where to go. As a minimum, allow a month planning per year of work. To keep financial pressure from rushing this step, you may want to adopt the trial contract strategy that has proven useful at Pugh-Roberts (see Weil, 1976). Start out with a contract for a few months' work. Build a prototype demonstration model. See how things go in your relationship with the client. After the trail period you and the client will be in a much better position to ascertain whether or not you want to work with each other. Presuming you do, you will be in a better position to design the terms for further work.

IV. CONCEPTUALIZATION

On the basis of field observations of conceptualizers, I divide the genus Conceptulus into three races: C. methodica, C. effusa, and C. frutescens. The distinguishing traits of the three races are summarized in Figure 2.

C. methodica (common name: the drudge) works carefully assembling data, collecting accepted theories, and drawing diagrams. If he has whims, he doesn't follow them, nor does he venture too far from the obvious when drawing boundaries. His work is typically orderly but not inspired. That is, he does not create new theory. C. methodica thrives on well-cultivated

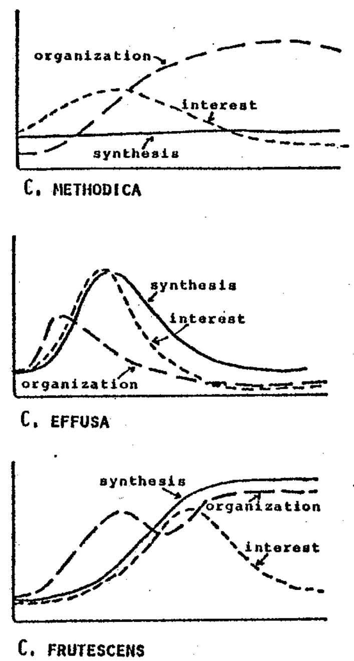


Figure 1 Characteristic development of organization of material, interest in the problem, and synthesis of theory for three races of conceptualizers.

theories. He is most comfortable readapting a generic structure, and is apt to become quite lost starting from scratch.

C. effusa (common name: the dreamer) is C. methodica's opposite. Judging from the frequency with which he uses the word "interesting", we might hypothesize that avoidance of boredom is his mainspring. He avoids the ordinary as assuduously as C. methodica clings to it. Typically, he favors wide boundaries. Chasing whims and ethereal syntheses are his specialties--often to the exclusion of order. C. effusa does best in areas where there is no established theory. However, he is more likely to simply spin off new theories than to develop any solid theoretical structure.

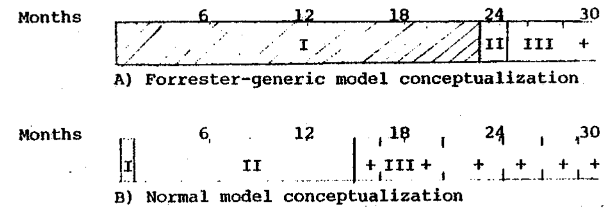
C. frutescens is distinct from the above two species in that his conceptualizations typically bear large, fertile fruit. The generic models C. methodica uses and C. effusa avoids are the products of C. frutescens work. Perhaps the feature that most distinguishes the C. frutescens from other races of conceptualizers is the ability to select the essential and reject the dross.

C. frutescens neither follows superfluous whims, as *C. effusa* is prone to, nor works in excessive detail as *C. methodica* tends to. Rather he moves toward a clear, clean theory.

In the short term the modeler has little choice but to live with his innate conceptualization traits. Methodical people should concentrate on problems that are conceptually straightforward, such as adaptions of generic models and engineering-type structures that do not require generation of original social theory. Effusive conceptualizers should seek situations in need of theoretical ground-breaking.

In the long term it would be more useful to be able to move both methodical and effusive styles toward the more desirable reference mode presented in the description of *C. frutescens*. Here, quite plainly, is a system we don't understand. Strategies for improvement of conceptualization are as laden with the "shooting blind"-type intuitive reasoning as are contemporary strategies for reducing inflation. Many people assert that conceptualization, like creativity, cannot be taught--and there don't seem to be any cases on record to dispute that assertion. In short, to develop a strategy for improving conceptualization we need better understanding of the conceptualization process.

There is, however, some evidence that conceptualization may be influenced by variables less nebulous than muses and modeler genius. At the conference that produced this volume, Jay Forrester was asked to describe how he conceptualizes. The time phasing of the process he described is approximated in Figure 2.



I: identification of trends and state variables
II: construction and reformulation
III: testing, interpretation, and later stages

Figure 2: Time allocation to phases

For a new generic model, roughly two years is invested in sorting through information about the system, identifying recurrent behavioral trends associated with a problem, and isolating the state variables controlling the trends of interest. This done, the formulation of rate variables and construction of the initial model takes a number of weeks. Thereafter conceptualization phases into a period of extensive testing and observation of model behavior, which in turn leads to refinement of both the model and of the analyst's conceptual understanding of the system.

By contrast, the time phasing of a more normal modeling study (my observation) is approximated in Figure 2b. The modeler more or less rushes into the study. Often he begins with a rough idea of what his main state variables are to be. Within a month he is constructing the initial model. Construction is a relatively lengthy--and messy--process. Frequently it involves bouts of problem redefinition, expansion of model boundaries, and incorporation of new state variables.

Correlating the Forrester process with Forrester results and normal process with normal results does not imply that a long gestation period is the key to <u>fructosa</u> conceptualization. It does, however, offer the whisper of a suggestion that production of model concepts, like other forms of production, is not random: that there may be systematic relationships between what goes into the process, how it is organized, and what is produced. If such systematic relationships do exist--and we can develop an understanding of the system--it is likely that we can learn to systematically control the conceptualization process. If the development of such managerial know-how does not revolutionize conceptualization--if modelers admit to the conceptualization control scheme's validity and ignore it in practice--we will at least have developed a grounds for perfect empathy with clients who balk when it comes to implementing structural change.

<u>Warning</u>! Once attained, conceptualizations tend to erode. Modelers become mesmerized by their models. They get to know each equation personally, and lose the ability to visualize the real-world situation the equations represent. Symptoms of conceptual erosion are often striking in model documentation. In extreme cases, conceptual understanding degenerates into a nit-picking detailed analysis without an overview. Documentation moves through the model line by line, but loses sense of the whole structure. Mathematics predominates over social awareness. Form outvoices content. Great care is taken to justify everything, but nothing earthshaking is said.

A clear mental model must be maintained throughout the

process of building a formal model. If the modeler ever ceases to sound like he has a clear understanding of the real world, something is disastrously awry. You simply cannot build a good model without a feel for the part of the real world you are simulating. In the long run, a clear understanding of the real world is of far more value than the model itself, particularly to the client. A clear understanding must always be kept in the foreground. The greatest gift the modeler can give is a conceptual grip on the wheels of the world.

Modelers must discipline themselves to step back. They must look up from their models and take the real world more seriously than they take their representations of it--regularly throughout the modeling process. Along with book research they should observe the real world. They should talk with the people whose decisions are information streams in the model. Wherever possible, they should observe the plant and physical operations the model simulates. When they read, they should not confine their readings to economics and statistics, but should read history, philosophy, anthropology, and novels as well. And they should feel that those readings are as pertinent to their work as are economic tracts.

Conceptualization must begin from realism. Nothing should be incorporated into a model until the modeler can imagine, in a tangible way, how it happens in the real world. Before introducing a money flow, the modeler should imagine the people who handle the money--they ways they are feeling, what they look like, the circumstances in which they live. Production functions should not be written until the modeler can visualize the buildings and the machines, the workers and the material with which they work. Population-growth functions should not be written without asking "What do children mean to these people?"

Once achieved, a deliberate attempt must be made to maintain realism. After translation into mathematical form, concrete understandings easily drift off into abstractions and technical mumbo-jumbo. It is all too easy for numbers to pose for reality. Once numbers become "the real", it is only a matter of time before mathematical logic co-opts the logic of the initial conceptualization.

Second Warning! Ongoing conceptualization frequently gives rise to deep, fundamental questions such as "What does this model say about the way the real world operates? Is that how the world behaves? Does the model address important questions? Are the solutions this modeling study poses within the client's power to implement? Am I working for the right client? Am I building the right model?"

When sufficiently mild, such questions are highly beneficial: they furnish the jolt needed to initiate rounds of model refinement and improvement. However, the effect of questioning on a deeper level can be quite troublesome. Like the third-year medical student asking "Do I really want to be a doctor?", rethinking one's conceptual premises can lead to ulcers. There will almost always be some material about which the modeler can develop grave doubts. Yet pragmatic and institutional constraints commonly make it unreasonable to go back and rework the areas of

uncertainty. If the model is a group effort, each member of the group may develop his own set of qualms about the model's conceptuali base and deep questioning may fragment the modeling effort. Moreover, modelers who have signed a contract and have deadlines to meet cannot easily backtrack because they have become unsure of their direction.

In Modeling Utopia, such questions would emerge during conceptualization to be openly discussed among modelers and model clients. The discussion would lead to a new, more useful problem definition. The modelers would then proceed without nagging doubts about what they were doing. The client would end up with a model that filled his needs and modelers who were sure enough of the merits of their work that they would be glad to take the pains to record it well and deliver it to the world with enthusiasm. Implementation would be assured.

In practice, few models fit the scheme of Modeling Utopia. Too often the problem becomes redefined in the modeler's mind half way through the modeling process. By this time, the modelers are already too committed to their original problem definition to change the model. Thus they finish off the model according to the original problem definition and put their enthusiasm into looking for a new contract that will allow them to work on a new model with the knowledge gained from the last model. The old model, with which the modelers have become disenchanted, is completed and documented only sufficiently to meet contractual obligations. The commitment needed to communicate and implement a model is lost.

Both the need for and the destructiveness of hindsight questioning are apparent. Whether modeling can be managed to avoid the destructiveness remains to be seen. More time for the initial conceptualization--closer modeler-client relationships or institutionalizing one or more periods to rethink the original problem definition, as advocated by experienced modelers at Pugh-Roberts (Weil 1976; Roberts 1972), might help to bring models into a form so sound that grand questions will not prevent their satisfactory completion.

V. CONSTRUCTION

> Models are designed to solve problems and are not an end in themselves. The kinds of models constructed are determined by the needs of the problem to be solved. (Anyone involved in model building can testify to the difficulty involved in being objective about this. It is very easy to let models become an end in themselves.)
>
> T.J. Manetsch, 1974

Model construction consists of translating the conceptualized structure into a form that the computer can digest. To the modeler, model construction is home ground. He may never have studied how to conceptualize or how to relate to a client, but he has had years of training model construction. Generally he is fond of the trade and enjoys using its tools.

Having a model to construct is having an elaborate puzzle to solve. The modeler proceeds with enthusiasm. He pulls out all his shoptalk and begins discussing with his fellow modelers the relative merits of one design feature or another. Talk turns to reference modes, slopes of table functions, dominant loops, oscillatory behavior, and modelers begin teasing each other about keeping the model simple while they wander off into bogs of complexity.

Conceptualization provides the intellectual blueprint. In the construction phase, a series of tools, including diagrammatic techniques, mathematical algorithms, and computer software packages (canned programs), are used to transform the intellectual blueprint and the data into a technical structure. The tools are far from passive in this process. DYNAMO, with an internal discipline more intense than the coerciveness of 1984 Newspeak, simultaneously makes simulation in approved system dynamics form a technically simple task and makes it quite messy to go outside the paradigm. Aggregate, nonlinear state-determined feedback systems can be simulated with no more than a bit of algebraic reasoning and a knowledge of DYNAMO. Hybridization with other techniques, such as input-output, optimization, or stochastic modeling, requires considerable computational skill and generally creates horrendous conceptual difficulties.

A. Client and Construction

Of all the steps of modeling, there is none more alien to the client than construction and none where he is more likely to be left out. The buzz of shoptalk and jargon which arises as the modelers descend on their puzzle is nonsense to all but the initiated. Even if the client were educated in modeling, he would at this stage be hard put to keep abreast of what is going on. (Indeed, the modelers themselves may be lost a good part of the time.)

In short, while the model is translated into something the computer can understand, it is being translated into something the client cannot understand. True, if the client understood the problem definition and if the computer program were true to the problem definition, then the client would understand the computer program, in theory if not in letter. However, some time has passed since the problem was defined. The client has had an opportunity to become vague and the modelers have had an opportunity to lace the original vocabularly of the problem definition with technical terminology. Under the circumstances, the client easily ceases to believe he understands the model, even if he does understand its conceptual base.

Re-establishing contact is a human, not a mechanical, matter. Empathy, sympathy, and skill in communication are required to locate and clarify, in a non-condescending way, the factors that block others from understanding a model. Above all, the process requires time and patience. If physical distance separates client and modeler, trips, phone calls, and mountains of correspondence will be necessary. If the client is not an individual but an agency, the problem of re-establishing contact is all the more difficult. If the modelers do not possess the requisite human skills or the client is not sufficiently committed to the model, the stress on client understanding during model construction may depress client involvement beyond all recovery.

Some loss of client understanding during model construction is unavoidable. However, that loss should and can be minimized if:

1) contact does not cease entirely during model construction,
2) modelers introduce the client to their jargon slowly and keep it to a minimum,

3) clients insist that modelers make what they are doing clear and inform the modeler when they cease to understand what is going on,
4) the model construction phase does not take so long that the client forgets that the model exists, and
5) the model becomes no more complicated than its assigned purpose requires.

The above 'ifs' will not be met spontaneously. Their fulfillment is best assured if the difficulties during model construction are anticipated in the contact-establishment phase. A modeling study which starts out with a well-timed reporting schedule, a consensus to maintain communication, and a realistic attitude toward the difficulties of communication is far less likely to run aground during model construction. To the extent that communication does fail, despite advance preparation, the damage will be much less serious and much easier to repair.

VI. TESTING

Testing is the intellectual highpoint of the modeling process. In a sense, formal models are built to allow testing. Were mathematical models not amenable to a diverse spectrum of testing procedures, they would have little advantage over verbal models. If tests could not indicate a model's validity and if a model, once accepted as being valid for a purpose, could not be used to test the impacts of potential policies, models would be no more than super-precise descriptions--mathematical paintings.

Procedurally, too, testing is a climactic activity. Suspense tends to build from the time the model is conceptualized to the time it is ready to be tested, as the modeler wonders how it will work.

Professionally, testing is a bramble patch. Validity testing is the subject of pitched battle between system dynamics and other paradigms. The consensus among system dynamicists seems to be that the statistical tests used by other paradigms are not useful in system dynamics (Forrester 1961; Senge 1975; Mass and Senge 1976), although some advocate formal measurement of predictive error as a means of gaining client confidence (Weil 1976).

Sensitivity and policy testing are recognized as important (Forrester 1961). How sensitivity and policy tests should be conducted is taught largely through the use of examples and through apprenticeship. The implicit generalization is "that the tests to be conducted can only be prescribed on a case-specific basis according to the purpose of the study and the nature of the system being studied". In other words, testing form is a matter of judgement. Judgement is gained by experience.

One need not look very far to find poor judgement used in model testing. Many modelers suffer from the inclination, when faced with a large model and an astronomical number of potential subjects for testing, to concentrate their efforts on "tuning" the model, rather than testing it. Parameters are adjusted to attain a better historical fit more frequently than they are subjected to changes that might challenge the robustness of system behavior. Although real-world decision functions and information flows are frequently full of noise (Forrester 1961), it is rare for modelers to test the sensitivity of their models to different amplitudes and types of noise. Extreme parameter

combinations are seldom investigated and structural changes are scarce. Thus model behavior under extreme conditions--the very realms in which nonlinearities become important and interesting results frequently occur--often goes unobserved. Even worse, little serious thinking seems to go into model testing. In almost two years of weekly seminars and daily lunchtime discussions within a modeling group, I have yet to hear anyone seriously discuss how to structure testing of his own model. Most people, myself included, appear to begin testing informally, as part of the debugging and refinement process. From informal tests they develop an intuitive grasp of how the model functions. From this intuitive grasp--which could be quite inaccurate (as we all know, intuition has a rough time with higher-order systems)--they go on to structure sensitivity tests. From the results of sensitivity tests and a notion of what policy tests will produce the desired effect on the client, they conduct policy tests. Whereupon testing is considered complete unless outside criticism intervenes.

Why does testing so frequently become superficial? For one thing, modelers often become so absorbed in revision and elaboration of structure that they leave no time for careful testing. This use of time is somewhat like preparing an elaborate meal and throwing it out uneaten. Simulation's main advantage over other forms of analysis is that it allows one to see what happens when all one's assumptions operate simultaneously. If one quits after assembling one's assumptions and doesn't take the time to observe the results of their interaction, in detail and under carefully devised experiments, one might as well be writing essays or drawing diagrams. For another thing, rigorous testing runs counter to the grain of intuition and habit. The verbal theories with which we were raised cannot be tested as simulation models can; they are too inexplicit to allow detailed examination of structure and behavior and too inflexible to allow experimentation. Unless a deliberate attempt is made to establish and maintain rigorous testing procedures, modelers easily revert to testing formal models with little more care than they would employ in evaluating a verbal model.

Moreover, there may be active subconscious resistence to testing. Few of us relish manipulating our models in ways that could invalidate either our conceptualized structure or our intuitive understanding of system behavior. It is intellectually uncomfortable to test in ways that might destabilize our grasp of the model. Thus we easily fall into substituting superficial diddling for comprehensive and meaningful tests.

Such anti-testing forces should be ruthlessly opposed. Validity-testing must not be allowed to degenerate into an attempt to demonstrate that the model is valid. It should be a serious attempt to locate places where the model, or the modeler's understanding of it, is not valid, leading to improvement and refinement of model structure and the modeler's structural understanding. The exercise should be undertaken with the same vicious skepticism one would use in test-driving a used car--there is almost always something wrong under the hood. The point is to locate and correct the problems, not to paint over the rust spots.

Specifically, testing should involve careful observation of model variables under a variety of experimental conditions. The modeler should identify the structural reasons for variable behavior and for differences in variable behavior under different exper-

imental conditions. Then he should question whether the model's structural causes are plausible in the real system. (If they aren't, it is time to think about revising the model structure.) To derive full benefit from tests, the modeler should be explicit about how he expects the model to perform in each test and watch carefully for ways in which model output deviates from expected behavior. Anomalies between expected and observed results are a signal that either the model or the modeler's understanding of model behavior is unrealistic. In either case, there is a lesson to be learned.

Along with its other tribulations, testing must frequently survive a predatory social environment. Given a peerage of knowledgable critics pressuring him toward tests appropriate to the model's structure and purpose, the modeler would have social incentive to test well. Given, instead, a set of hostile critics pressuring for tests that are largely irrelevant to the model's purpose and structure, the modeler will be driven toward defensive, rather than insight-seeking tests. By and large, simulation modeling seems to have generated hostile critics faster than it has developed a self-policing professional peerage. Thus the heat is on for tests that "prove" rather than "improve" models.

The modeler may avoid this destructive situation in a number of fashions. He can hire an outside critic whose judgement he respects. He can structure inside criticism into the modeling process by deliberately assigning members of the modeling team to criticize the work of other team members. Or, if the client is well tuned into the modeling study, he can supply criteria by which the model's performance can be evaluated. None of the above types of criticism is likely to appear sponteneously. If the modeler wants them, he must actively solicit them and reward them with rewards appropriate to the situation (money, appreciation, and respect, coupled to an improved product for outside and inside critics, and the client, respectively).

VII. DOCUMENTATION

> ...The writer either has a meaning and cannot express it, or he inadvertently says something else, or he is almost indifferent as to whether his words mean anything or not. This mixture of vagueness and sheer incompetence is the most marked characteristic of modern English prose ....As soon as certain topics are raised, the concrete melts into the abstract and no one seems able to think of turns of speech that are not hackneyed: prose consists less and less of words chosen for the sake of their meaning and more and more of phrases tacked together like the sections of a prefabricated henhouse.
>
> George Orwell

Behind the question of documentation responsibility lies the larger question of the modeler's identity. Is the modeler a scientist? Is he a consultant? Is he an agent for change? Is he a servant of the quest for truth? Is he working for the client's interest? Or is he working for "the good"? All of the above?

If the modeler is a scientist, his documentation responsibilities are straightforward. By the general practice of the sciences he should: 1) review the literature to establish the position of his study within the body of established knowledge, 2) state his problem or hypothesis, 3) describe his method in enough detail that an independent scientist could use the same procedures and attain the same results, 4) describe his results, 5) interpret his results and draw conclusions. As adapted to computer models, he should also state his assumptions and describe data sources.

If the modeler is a consultant, his documentation responsibility is entirely pragmatic and highly circumstantial. He

should document as best serves the client's interest. The client's interests depend on what he wants to do with the model and how well he grasps the model without documentation. If the cient has been actively involved in the modeling process and understands the model, his needs may well be satisfied by a set of outlines and a few charts detailing the features of the model he most needs to know and is most likely to forget. If the model was completely <u>ad hoc</u>, if it answers a question and is of no further use once that question is answered, careful model documentation may be a luxury. (For example, it would be hard to justify putting a man-year of work into formally documenting an IBRD model constructed to shed light on one specific decision and dropped thereafter.) On the other hand, if the model was intended as a tool for ongoing use in planning, client interest may require extensive documentation. User manuals will be needed for maintenance, updating and operation, as will numerous explanatory works to assist in interpreting the model's output and communicating its results. If the client uses models frequently, he may benefit from such documentation as will allow knowledge gained from one modeling study to be transferred to the next.

The modeler may have a message he wants to get across and the model may be a device to help him make his point. In this case his documentation is essentially propaganda and should be written to be convincing. It should be short, undetailed, clearly-written and hard-hitting. Attention should be paid to appearance. Methodological discussion should be minimal, and nontechnical. Main points only should be stressed.

Finally, the modeler may be an academic. Many modelers are professors living in a publish-or-perish world or graduate students writing dissertations. If the modeler is in one of these positions, he will want to document to meet the standards of the academic, but not necessarily scientific, community. His documentation form will be directed by the format of one or another academic journals or by a thesis committee.

One documentation job cannot serve all of the above functions. Scientific documentation is seldom what the client most needs for his own use. Activist documentation cannot easily meet scientific standards or the standards of the academic community. It is difficult to write an academic journal article to have propaganda value or to fit the scientific format. Should the modeler then document in five different ways to answer all responsibilities? I leave that question to you, but with a recommendation that the decision should be made deliberately in the step of establishing contact. If documentation responsibilities are not clearly established and the needs for documentation are not allowed for, documentation tends to become an afterthought. Afterthought documentation is seldom of good quality.

When a documentation style is selected, the following should be considered:

1. Documentation is a time-consuming process. The process of preparing a public report plus a scientific or academic report of a modeling study may double the time and expense spent

on a modeling effort. If good documentation is desired, time and money should be allotted for it in the contract.

2. Documentation may improve modeling quality. Modelers are apt to be sloppy in one way or another. A lot of poor modeling is done and never detected. If required to present their work and make it accessible to criticism, modelers are more likely to catch their mistakes and less likely to use technically unjustifiable procedures.

3. Many modelers don't write very well. Preparation of documentation for the general public, be it explanatory or persuasive, will probably require editorial assistance and may require that a writer be hired. If there is to be a report to the public, provision should be made to assure adequate writing skills in the contract agreement.

4. Modeling has great potential as a tool to mystify and confuse the nontechnical world. If models are used as tools in public decision making and are not documented in such a way that the public can understand them, they are, in spirit, technocratic and undemocratic.

5. Documentation of all sorts is an important mechanism for allowing model-generated knowledge to be disassociated from the modeler. An undocumented model contributes more to the modeler's understanding of the system than it does to the world's understanding. If a model is well documented, the model-generated knowledge can be passed on to the technical community through scientific and/or academic documentation, to the public through plain language documents.

6. Computer modeling is a young discipline and its field

record is poor. If modeling is to improve and grow, knowledge gained in one study must be passed on to other studies. It is also important that models be subjected to criticism, both from the technical world and from the nontechnical world (particularly that portion of the nontechnical world who know well the real world situation which the modeler is simulating). Such criticism requires that models be given both technical and nontechnical documentation.

7. If a model isn't important or if its documentation is particularly obtuse, no one is likely to pay much attention to the documentation. Generic models and models with controversial subjects therefore have greater need of documentation than do case-specific models designed to answer questions no one worries about. Nontransparent documentation may not be worth printing.

8. Not much is known about model management or about the process of modeling. Documentation of managerial aspects might be fruitful for the development of management techniques.

VIII. IMPLEMENTATION

In Modeling Utopia, the client, deeply distressed with a Problem, comes to the modeler crying "Oh what is going to happen?

What can I do to avoid this ruinous fate?" The modeler steps forth, performs an analysis, and offers a Solution. The Solution is accepted and used by the client. (Of course, the Solution works beautifully and the client is eternally grateful.)

By contrast, a cynic might describe the real-life situation as tending toward the following. Modeler and some funding source come together and agree to build a model. The model purpose is designed as "furthering the understanding of XYZ" and "exploring the usefulness of the PQ methodology". The modeler receives funds and goes off to build his model. Two years later he brings back his masterpiece. The funding source can't use it, but they know the chief of the BS division of the CDE agency which formulates XYZ policy. An appointment is secured. The Division Chief listens politely and looks over the model output, but does not call the modeler back or ask for further information. The modeler's interests move on; the model fades out of the scene entirely.

What happens in Modeling Utopia that fails to happen in real life? Essentially, in Utopia the transfer of model-generated ideas is smooth and automatic, while in real life the gap between the modeler's and the client's ideas is so great that little or no transfer takes place.

To approach this question of transfer-nontransfer, I shall construct a simple verbal model of information transfer. In this model, the word "conclusion" is used broadly to signify any piece of information built into, or generated by, the model.

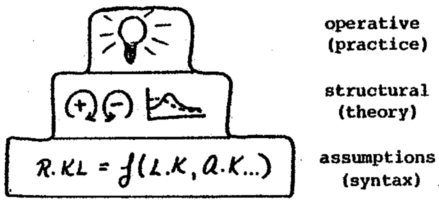


FIGURE 3: Conclusion Heirarchy

Picture conclusions as two parallel hierarchies--the modeler's conclusions and the client's conclusions. At the ground level of each lie fragmentary assumptions that relate one variable to another. The second level is composed of structural conclusions--conclusions that relate system behavior to feedback structures and their time characteristics.* The highest level is composed of operative conclusions--conclusions that relate model structure to real-world structure and become the structural archetypes that guide our intuition of systems.

The first problem of modeling, with which we have flirted previously, is how to build the right model and interpret it accurately. Assuming this has been accomplished and potentially valuable conclusions have been drawn from the model, a new problem arises--how to put the conclusions to use. Modeling is not functional unless it leads to an improvement of operational conclusions <u>on the client's side</u>. The modeler's conclusions are absolutely useless unless transmitted to the client as operational conclusions.

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* The secondary conclusions drawn by other modeling paradigms are very different from those drawn by system dynamicists. Linear programmers are likely to relate behavior to the topography of the constraint set or the way the objective function interacts with the activity set. Econometricians are more likely to explain behavior by looking at the natures of key relationships in economic theory.

Operationally, each level of conclusions rests on the level one below it and gains cohesion from the level one above it. Structural conclusions are senseless without an understanding of the assumed relationships on which they are based, and empty unless they mature into operative conclusions. A causal loop is without substance unless the person looking at it understands the real-world significance of each link in the loop. If structural understanding fails to become operative, it cannot gain the cohesion that comes from merging theory and practice. Likewise, operative conclusions without structural understanding are pure black-box intuition.

The modeler typically begins a new model with a residual of operative and structural conclusions left from previous modeling experiences. These leftovers assist him in assembling the model's syntax of assumptions. The syntax of assumptions becomes formalized to generate new structural and operative conclusions. Difficulties typically arise for the modeler when a lower hierarchical level outpaces the level above it. Most commonly, the assumptions become a huge mass of inter-related variables before structural conclusions develop to give the syntax cohesion--hence, the chronic "big model" problem.

The difficulties that beset the modeler in moving up the conclusion hierarchy are small compared to the difficulties that beset the client, or anyone else. The modeler gradually builds up a framework of conclusions around his core knowledge of modeling. He deliberates each of his syntax assumptions as he conceptualizes and constructs a model. His structural conclusions are reached slowly and are built upon direct knowledge of model assumptions.

The client, by contrast, comes in cold. If he has a core of modeling knowledge, it probably doesn't coincide with the modeler's. Thus, many things that the modeler takes for granted are invisible to the client. He is seldom given ample time to assimilate syntax-level assumptions before the modeler assails him with a detailed causal loop diagram and begins explaining structure-behavior relationships. If he tries to pull back to look at model assumptions--the natural thing for him to do at this point--the modeler is likely to cut off his inquiries with the observation that structure determines behavior. Eventually the client gives up.

Recommendations on how to avoid such problems have been made by Ed Roberts and others (Roberts 1972). Most of the strategies suggested focus on: 1) involving the client in conceptualization, 2) closer more frequent modeler-client communication throughout the modeling process, 3) starting simple, and 4) including client assumptions in model structure. In general, such procedures offer a means for keeping the modeler's and the client's respective conclusion structures growing in synchrony throughout the steps of modeling.

An alternate strategy more appropriate to situations where close client contact is not possible (especially the situation where the client is the general public) would be to translate model-derived structural and operational conclusions completely into verbal form, and then let the translation compete with other

mental models on mental-model terms. (See, for example, Randers 1976; Budzik 1975. Also, D.H. Meadows considered employing this strategy in The Limits to Growth.) At its best, the end product of such a strategy would be an essay, such as Garret Hardin's "Tragedy of the Commons" or Mathus' essay on population: a straightforward explanation of how the hypothetical system works, how it resembles the real world, and what factors influence the system's behavior.

We have thus far pretended that the hierarchy of conclusions is a parade of neutral, colorless bits of information to which people react unemotionally. Were this the case, model management would be a purely technical matter. In fact, however, system dynamics studies seldom lead to neutral conclusions. As discussed in an earlier paper (D.H. Meadows 1976), our paradigm gives us a propensity toward boatrocking, iconoclastic, radical conclusions. We tend to tell people that their previous actions have either had no impact at all or else aggravated the problem. We frequently insist that nothing short of drastic actions (that is, structural change) will make the system behave in the desired fashion. These traits tend to provoke emotional reactions.

That our conclusions are seldom neutral places an additional burden on the conclusion-transfer process. This burden would be eased if, before a modeling study is begun, the modeler and client would have a frank discussion about boatrocking. The modeler should make sure the client understands the propensities of system dynamics and is willing to expose his conceptual framework to major conceptual upheavals. If there is any doubt about client readiness for the modeler's type of conclusions, the modeler should seek a new client and/or the client should seek a new modeler. Thereafter, the modeler should stay off his high horse and avoid posing as the Problem-Solver of Modeling Utopia. He must not presume his model will be accepted. If he aspires to see his model implemented, he must deal with real people, real institutions, and real inertia. He must remember that it is easy and safe for him to advocate change, but that it will be difficult and risky for clients to implement it--in short, that there are usually well-based reasons for client resistance to model conclusions.

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