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"Squishy" Problems and Quantitative Methods*

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ABSTRACT

The edited text of a talk on potential hazards in the application of quantitative methods to "squishy" problems without well-defined structure, of the type frequently encountered in government policy and decisionmaking. Squishy problems are defined, and a three-level conceptual model of analysis which displays the relative roles of logical inference and qualitative human judgment is described. Two ways in which people use models of all types, as a ***surrogate*** for the substantive problem (e.g., Newtonian mechanics as a surrogate for "real" mechanics), and as a ***perspective*** on the problem (e.g., two-dimensional perspective drawing) are described and contrasted, and some of the implications of the difference for the analysis of squishy problems are discussed.

It is clear that in recent years there has been an increasing trend toward use of quantitative methods in government policy analysis. This started, perhaps, with the use of operations research in World War II. The McNamara Defense Department pushed it further, and then under the Johnson administration, quantitative methods of various types came into widespread use throughout government. Some of the supporters and advocates of this trend see it as a wave of the future-the application of rationality, scientific method, etc., to governmental problems. Others, and I include myself, are less sanguine. What I want to do in this paper is discuss the nature of quantitative methodology and some of the problems encountered in its use.

I am going to use the term "policy analysis" as a broad brush term for analysis done in support of the government policy- and decisionmaking process-the kind of thing Rand does, the kind of thing you do-as distinguished, say, from scientific research, where the objective is one of finding knowledge. One important distinction here, I think, is that the scientist can pick and choose his problems and ignore or put aside those that he does not want to look at yet. This allows him to impose very stringent external validity criteria on his work and his results. The policy analyst has

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less flexibility on what problems he has to deal with. They come to him. Therefore he must adopt a somewhat more problem-dependent stance in the methodology he uses and the validity checks he imposes.

By "quantitative methodology" I mean both the general bag of computational techniques, tools, etc., available to the policy analyst-game theory, statistics, simulation, computer techniques of all kinds-and the mathematical theories that support those techniques. I want to look briefly at the nature of the supporting theory and the techniques that derive from it, and then at different kinds of applications of the techniques to different problems.

The supporting theory-and here I am thinking of statistical decision theory, linear programming, and so on-is essentially mathematics. It deals with mathematical models defined as entities in themselves, with their structure determined by a set of defining premises, and with results being derived from the structure of the model as logical consequences of that structure.

Schematically, pure mathematical analysis looks something like the depiction in Fig. 1. The model is defined by a set of assumptions and premises. Analysis consists of obtaining results as logical consequences of that model. This may be done by computer calculation, by logical inference, or by a combination of both. Once derived, the results are tautologies within logical structure of the model.

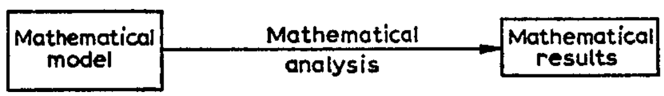

Fig. 1. Mathematical analysis

In application, we depend on similarity between the model and the problem that the analyst is interested in. The simplest, most straightforward kinds of application are those that arise, say, when we deal with well-understood physical phenomena. Analysis of this type is depicted in Fig. 2. We have a substantive problem which can be made to look much like the model used to analyze it. The link between problem and model is thus a straightforward one, as is the link connecting model to results. The directness of those two links allows the results to be interpreted as conclusions about the substantive problem fairly directly. As I said, this type of situation arises quite frequently in the physical sciences. It also arises in statistical experimentation, where the analyst uses a probability sampling procedure in order to make his problem one of analyzing a mathematical model he knows and understands.

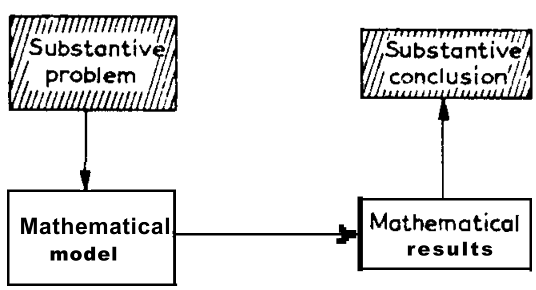

Fig. 2. A common perception of analysis

Many of the problems a policy analyst encounters, however, tend to be a lot ***squishier*** than this, in the sense that they have no well-defined formulation. Or, if they look like they do, it remains well-defined only as long as we do not lean on it too hard or question the assumptions too strongly. Questions like "What forces do we need to deter the Soviet Union?' or "What are Soviet intentions in the Middle East ?" tend to be squishy.

A simple example of what I mean by a squishy question is the question of what the middle character in Fig. 3 is. If you look at it one way, it is a B, while if you look at it another way, it is a 13. It is not clear that the question has a well-defined answer. Formulating it in such a way that it does, by saying, "I am only going to read across" or "I am only going to read up and down," does not solve that basic problem.

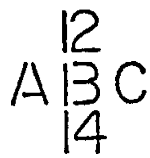

Fig. 3. Example of a squishy problem

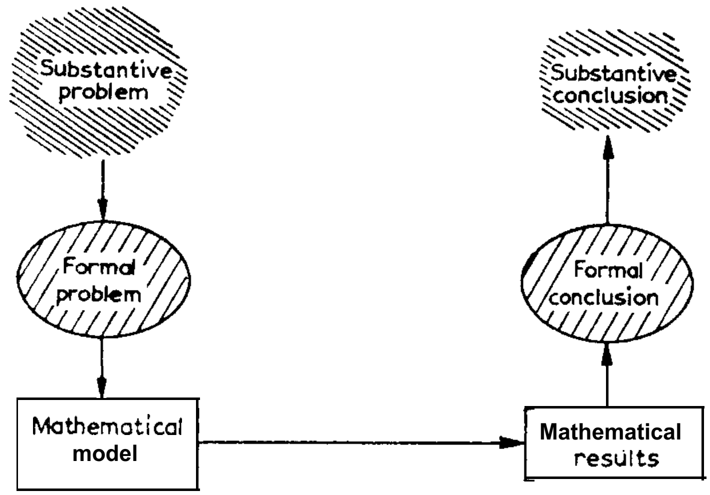

Fig. 4. Application of quantitative methodology: the general case

What happens when we try to use quantitative methods or mathematical models in situations like this? What we end up with, I think, is a three-tiered kind of situation like that shown in Fig. 4. At the top level we have whatever substantive problem it is we are interested in. The substantive problem may be very squishy and frequently is. At the bottom we have a mathematical model logically analyzed to produce mathematical results, which we somehow have to get back up to form a conclusion about the substantive problem. In between, and not always very explicitly laid out, is what I have chosen to call the "formal problem" and the "formal conclusion." These are the links that the analyst constructs to join his substantive problem and mathematical model on one side, and his results and his substantive conclusion on the other side.

Consider a computer simulation, for example, of air-to-air combat, used to investigate the suitability of alternative aircraft for NATO. In some sense, the question of what aircraft designs we want for the next generation NATO aircraft, perhaps, is the substantive problem. The mathematical model is a representation of a very specific aerial combat process that somehow is going to help answer that question.

There is no single formal problem linking these. Rather, the formal problem can be thought of as ranging widely between the two. It may be one of how well the different aircraft designs do in real aerial combat very much like the kind described by the model. In that case the link between formal problem and model is a fairly straightforward one, but the links between the formal and substantive levels, between NATO requirements and that particular type of air-to-air combat, that particular dogfight situation, will be ill-defined and judgmental.

On the other hand, the formal problem might be thought of as one of how the different airplanes do in air-to-air combat generally. In that case, the links between the formal and substantive levels are better defined, but the link to the model becomes fuzzier, because the same model is now being used as a representation of a more general combat process. In any case, formulation-moving from substantive problem to model-and interpretation-of the results back to the substantive problem-are highly judgmental in nature. But most of our conventions for using, discussing, and dealing with quantitative techniques, tend to focus on the bottom link from model to results. They tend to look at how good or bad the ***technical*** parts of the analysis (along this link) are, and to divert attention from the links of formulation and interpretation. But in the analysis of squishy problems those are the links that are critical to the final conclusion. The link from model to results, while important, is minor by comparison. Unfortunately, I think there is a widespread tendency among the quantitative analytic community, and the advocates of increasing use of quantitative methodology, to tend to ignore these issues-to worry about technical competence of analysis done using quantitative methodology and to concentrate on the methodology rather than on how it fits to the problems.

An example of the kind of problem that this focus on methodology can cause can be seen in some applications of regression analysis and similar techniques, which in recent years have become very popular policy analysis techniques. If the independent variable or variables are policy variables, in the sense that they can be manipulated by whatever organization the analyst is working for, and the dependent variable is one that the organization would like to manipulate, then it is very tempting for the analyst to run his regression, and if the results are statistically significant, to argue on those grounds that by manipulating the independent variables the organization can manipulate the dependent variable.

An example of a pair of variables, that meets the conditions I have just outlined, but not necessarily the conclusions, would be annual rainfall in a given area and mean annual reservoir height in the same area. The variables are going to be correlated. The reservoir height is a variable that local government, say, has some policy control over. It can change the amount of water in the reservoir by changing the reservoir management policies-how much water is used for irrigation, for power generation and so forth. Rainfall might well be a variable that it would like to have some control over. The ludicrousness of the conclusion, however, that reservoir heights determine rainfall, is fairly clear in this case. Unfortunately if the variables happen to be such that the cause and effect relationships involved are a bit more obscure, that there are more variables involved, that the question is one that has a great deal of bureaucratic self-interest attached to it, etc., that same type of conclusion may well be reached and may look (superficially at least) quite a bit more reasonable than it does with rainfall and reservoir height.

There are examples of this type of methodology used during the late 1960s in analyses of the Vietnam war which essentially came to reservoir-height-causes-rainfall type conclusions. This occurred partly because the analysts involved got too tied up in the methodology and in what it said, without thinking carefully enough about just how the model related back to the substantive problems that they were trying to solve.

One way of looking at the use of quantitative models in dealing with squishy problems is as a special case of a general human cognitive process of using models (or simplified representations) of complex problems to deal with them. There are essentially two ways, I think, in which we do that. One way is to use a model as a ***surrogate*** for a problem, as a replacement for it. To do this, we assume that the model captures enough of the substantive problem so that we are willing to take the answer that the model provides, and accept that answer as a conclusion about the substantive problem. Newtonian mechanics is a model of real mechanics, for example, whatever that is. It is also an abstract mathematical construct. It is not real mechanics, but in some sense it captures enough of real mechanics that we accept it willingly as a surrogate for real mechanics. At least, we do so in the sense that if we have a problem involving trajectories, we will take the Newtonian answer as an answer for the real problem.

This way of using models-taking the model as a surrogate for the problem, finding the solution of the model and then accepting that-is one that lies pretty much, I think, at the heart of the physical sciences and most other well-developed applications of mathematics. It is thus the one that people with scientific and technical training tend to internalize as they get that training, as the way that models ought to be used. It is a good way of using a model when the problem is sufficiently well-defined. It can be a less than adequate way when the problem is more squishy.

There is another way, however, in which we use models. Most of us do not even think about it as a way because it is so common. That is what might be called using the model as a ***perspective*** on a problem. The drawing in Fig. 5 is a model of a cube, a two-dimensional model of a three-dimensional object. As such it has a two-dimensional structure of its own, Now consider the question of the shortest distance between the two points ***a*** and ***b*** on the surface of the cube. Within the structure of the model itself, the shortest distance is the line ***ab.*** Most people, however, would pick the lines ***acb*** as the shortest distance. They do this because they do not restrict themselves to the structure of the model, but make use of a richer and more detailed understanding of three-dimensional space and how three-dimensional space gets represented in two-dimensional drawings than is represented in the model. They use the model to help trigger this internal understanding rather than to supplant it, as happens when the model becomes a surrogate.

In analyzing most squishy problems we cannot build adequate surrogates, so that if mathematical models are to be useful at all they are useful as perspectives. This requires, however, that the analyst using them carry along-and not throw out, not forget-the additional richness he understands internally about the problem he is dealing with. And that he do this, perhaps, not in the mathematical analysis per se, but in his formulation, in deciding what model he wants to use and how he wants to use it, and in interpreting his results. Let's go back to the example of regression analysis. The conclusion that the statistical significance of the relationship between rainfall and reservoir height shows that there is a ***relationship*** there is a valid conclusion. The jump to causality, to reservoir height causes rainfall, is invalid. The analyst can see it as invalid and not make the jump using the knowledge of reservoir height and rainfall and how they relate that he carries along in his head. He can, and should, do the same thing in less obvious cases.

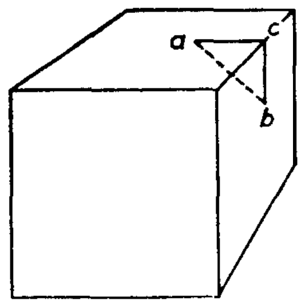

Fig. 5. A model as a perspective

Another example of a mathematical model that I, at least, think is an excellent perspective on a lot of problems, but poor surrogate for any, is Prisoner's Dilemma. This is a well-known two-by-two game. Say two of us rob a bank and get caught. The local police have insufficient evidence to get a felony conviction, but enough evidence to hold both of us, say, six months for possession of concealed weapons. The police interrogator tells me that the DA really wants a felony conviction, so if I turn state's evidence and let him convict my partner, he will let me off. At the same time another interrogator is making the same offer to my partner. If we both turn state's evidence, however, they cannot let us both off but will get both of us on a lesser felony conviction. We each get, say, five years as opposed to the ten if one confesses and the other does not.

So, what's the situation I am in now, and what should I do? Suppose first that I do not know what my partner is going to do. If he holds fast, if he does not confess, then by confessing I can reduce my own sentence, from six months for carrying a concealed weapon to getting off Scot-free for turning state's evidence. So, I am much better off to confess, assuming he does not. Assuming he does confess, on the other hand, I reduce my own sentence from the ten years I will get if I remain silent and he confesses to the five years I will get if we both confess. Thus, whatever he does it is rational for me to turn state's evidence. I improve my situation either way. Now the situation is symmetric, so that the same considerations hold for him. Thus rationality would suggest that we are both better off if we turn state's evidence. However, we then both end up with five years in prison, rather than the six months we would have had if we both held fast.

The dilemma captured in that game is one that tends to recur throughout human affairs. It is present in many of the environmental problems that are making a lot of news now. It is always cheaper for a riverfront town to dump raw sewage into the river than to treat it, for example, but if all the towns do this, the river will become polluted for all. The dilemma is present in our daily lives. Regardless of what others do, it would be to my advantage, if I could get away with it, not to pay taxes. If no one paid them, however, organized society would collapse. It is also present in the affairs of nations. Each nation in a defense alliance may find it cheaper to let others carry the defensive load. If all try this, however, the alliance may not serve its purpose.

The usefulness of Prisoner's Dilemma as a model of these situations lies not in the fact that it tells us how to "solve" them. It clearly does not. The solution depends on more context than the model captures. Depending on the context, it may contain elements of criminal sanction, coercion, mutual trust, and living with degraded outcomes. This last we find particularly in the environmental area. The value of Prisoner's Dilemma as a model of these situations lies in the fact that it provides a context-free integrating perspective on the core elements common to all of them. The value of this perspective is not diminished by the fact that it does not prescribe a "solution," since these core elements do not provide enough structure for that.

The characteristic of being a surrogate or a perspective is not one which resides in the model, or even in the combination of model and problem. Rather, it resides in the head of the analyst-in the way he thinks about the model and its relationship to the problem, in whether he accepts the model as the problem and forgets what else he knows "for purposes of analysis," or carries along that additional knowledge and uses it to guide his use of the model and his interpretation of the results it produces.

The distinction between a surrogate and a perspective is an important one in policy analysis. Many problems are too squishy to allow the construction of an adequate surrogate, in the sense that models used in the hard sciences are surrogates. Most models, then, are best viewed as perspectives, as incomplete and non-unique ways of looking at the problem they represent.

In technical model building terms, building a perspective on a problem is less demanding than building a surrogate. The model need not include everything that "counts" in some abstract sense, but need only reflect the parts of the problem which the analyst wishes to investigate and the relationships between them. A simple model can be highly useful, because the analyst remains conscious of the fact that it represents only part of his problem, and not the whole thing.

This same factor, however, makes analysis of a perspective more difficult and demanding in some important ways than analysis of a surrogate. Because the model as a perspective does not fully capture the structure of the problem, the analyst cannot simply work within the model. He cannot depend on rules of procedure and the validity of his logic within the model to ensure the validity of his substantive conclusions. The shortest path between the two points on the cube is not the same as the shortest path on the drawing of the cube. The analyst working with perspective needs a greater substantive understanding of his problem than does the analyst working with a surrogate, and he must use that understanding throughout all phases of his analysis.

A case in point is the argument that "if you agree with the assumptions, you must agree with the conclusions." If the model is an adequate surrogate, this is a valid argument. If the individual assumptions go together to build a structure that we accept as the structure of the problem, then the results from the model should be acceptable as conclusions. If, though, as is frequently the case in policy analysis, the individual assumptions are all slightly questionable, approximations made for purposes of convenience, good perspectives but not adequate surrogates, then the whole model and any conclusions have to be reevaluated holistically to see whether or not the structure of the model really does reproduce the structure of the problem well enough to support the proffered conclusions.

One of the things that can happen working in perspective is that you get something that looks reasonable in pieces, something that reflects reality in sections, but when you put the whole thing together somehow it does not come out that way. Figure 6 shows this happening in the simple case of a two-dimensional model of a three-dimensional object, but it also shows up in more complex situations. It shows up, I think, when highly simplistic assumptions are made about complex bureaucratic decisionmaking behavior, the rationality of national political systems and so forth, and conclusions are projected back to reality without sufficient holistic evaluation of the model as a whole.

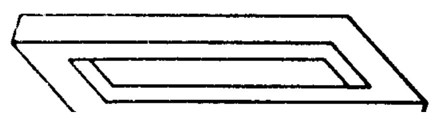

Fig. 6. A risk of working in perspective

An example here would be the rational gain calculating Soviet national decision process that is commonly used in strategic force posture planning. The primary Soviet interest is assumed to be one of attacking the United States as soon as the Soviet fatality count drops below whatever this year's magic number is. This can be a very useful perspective for force structure calculations. When that decisionmaking mechanism is used, as it sometimes is, as representative of a more complex process, used to draw inferences about Soviet political behavior, perceptions and so forth, then I think we are getting into a situation very much like that shown in the figure.

One of the arguments frequently heard for quantitative methods is that they are "objective," in the sense that the physical sciences are objective. "Objectivity" here refers to a collection of things, like being unprejudiced, being free-standing in the sense that the knowledge conveyed by the analysis rests on the model itself, not on the judgment of the analyst producing it, etc.

There are at least two distinct meanings of objectivity here which are worth distinguishing. One refers to knowledge as being "free-standing," being grounded in the model and in the answer produced by the model in the sense that a trajectory calculation is. You can accept the model as a surrogate for the problem, and can believe the trajectory calculation without understanding the model. You need not know anything about the competence of the man who produced the calculation, except that he used the model properly. This free-standing kind of objectivity is a characteristic of the conclusions produced, rather than of the analyst producing them.

Another meaning of objectivity in the physical sciences is "unprejudiced." Here it is a characteristic of the analyst or the scientist rather than the knowledge that he produces. He goes to his task concerned with seeing things as they are, rather than proving a particular point or coming up with a particular answer.

There is a widespread image, and I think a useful one, although Thomas Kuhn has somewhat undercut it, of the physical sciences as unprejudiced objective inquiry producing free-standing objective knowledge. Now, what about softer areas like policy analysis? Can we get both forms of objectivity there?

We usually contrast objectivity with subjectivity, so to each form of objectivity there corresponds a form of subjectivity. Subjectivity thus means both the use of judgment, and it means a prejudiced approach to a problem, trying to come to a particular answer. Extreme cases of advocacy are everybody's favorite example of prejudiced analysis.

The logical compatibilities or incompatibilities between these meanings are shown in Fig. 7. In some sense, free-standing and judgmental knowledge have to be different. One is grounded in the judgment of the analyst, the other in the method alone. At the same time, a prejudiced and an unprejudiced approach to the problem are different ways for the analyst to come to the problem. They are logically incompatible.

Objectivity	Subjectivity	
	Judgemental	Prejudiced
Free-standing	No	Yes
Unprejudiced	Yes	No

Fig. 7. Logical compatibilities between forms of objectivity and subjectivity

The other two combinations are not logically incompatible. Depending on the nature of the problem, it may be that the only way an unprejudiced analyst can deal with it is judgmentally, that the problem is not amenable to free-standing solution. This is frequently the case with squishy problems. On the other hand, if the analyst is in that kind of situation and he insists on something that looks free-standing and ***looks*** as though his judgment is not there, as one often finds in instances of abuse of quantitative methodology then that in itself is a severe prejudice. It is prejudice not perhaps in the direction of which answer he wants, but in the direction of what does that answer have to appear to be based on. It has to appear to be based on calculations rather than judgment. That in itself, I think, can be as severe a form of prejudice as any that says which direction the answer should come out.

What this all boils down to I guess, is that squishy problems generally do not have unambiguous logically defined answers. Quantitative methods and models, however, produce such answers. There are no real grounds on which we can rigorously justify interpreting the answer produced by a quantitative model or method as the logical answer to the squishy substantive problem. Rather, we should think of such an answer as the ***opinion*** (albeit the informed, considered opinion, if he's done a good job) of the analyst involved. It's his opinion because it rests on his judgments and his choices about what model to use, how to use it and how far to trust it, how to interpret the results in light of any inadequacies in the model, etc.

This is not to denigrate the value of quantitative methodology. It has great potential value, but as an aid to careful and considered human judgment, and not as a replacement for such judgment. When we forget that and focus our attention too strongly on our methods and our computations as the source of our answers, we not only fail to realize that potential but we run the risk of being seriously misled. The source of knowledge and understanding about squishy problems has always been, and will continue to be, wise people more than sophisticated methods. This is a subtle distinction, perhaps, but an important one. And if we fail to make it, there is no way we can realize the real potential of the people or of the methodology we do have available to us. I am not suggesting that we give up quantitative methods, but rather that we use them with a goodly grain of salt, and that we grant "the computer" no more automatic and unquestioned authority than we would grant a shaman or a fortune teller.