On the Very Idea of a System Dynamics Model of Kuhnian Science

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Introduction

The appearance of Thomas Kuhn's Structure of Scientific Revolutions engendered considerable discussion about the nature of scientific change. Kuhn challenges the prevailing view of science as a continuous, logical enterprise by attempting to debunk science's myth of rationalism. As an historian as well as philosopher of science[1], he attempts to explain science's extraordinary success not by developing methodological canons divorced from scientific practice, but by looking at how scientists actually work. (Lakatos and Musgrave 1970, 236-237)

Acknowledging the philosophical importance of actual scientific practice is controversial. Kuhn's critics question both his characterization of science as mostly "puzzle-solving", as well as his claim that such practice is necessary for scientific development.[2] It will not be the task of this essay to rehearse these still unresolved debates. That is better left to historians and philosophers. Rather, I would like to recognize another important contribution to the discussion, one that is orthogonal to any other that I know of. In "The Growth of Knowledge: Testing a Theory of Scientific Revolutions with a Formal Model,"[3] John Sterman has built a model of Kuhn's account of scientific change. He asks not whether science does, could or should correspond to Kuhn's view, but whether Kuhn's theory is dynamically consistent. He is interested in whether the behavior Kuhn describes (i.e., paradigm emergence, normal science, crisis and revolution) actually follows logically from the assumptions Kuhn makes. To do so he constructs a system dynamics computer model.

Modeling

Modeling is a worthwhile enterprise. Unlike traditional attempts to resolve disagreement, in which scholars attempt to intuitively describe the behavior implied by a complex and often unstated set of assumptions, the modeling process requires that assumptions and logic be made explicit, and thus open to debate and discussion. Simulation infallibly yields the behavior implied by the assumptions. (Forrester 1971, 54; 1987, 147; Meadows 1980, 27) A successful model of a theory can lend extra confidence that the theory being modeled is a sensible one.

But what constitutes a successful model? Good system dynamics models are designed with well-defined boundaries, so that changes

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[1] Some of his views are foreshadowed in Thomas S. Kuhn, The Copernican Revolution. (Cambridge, Mass.: Harvard University Press, 1957).

[2] The best collection of critical views remains Lakatos and Musgrave (1970). For a very brief review of the issues in these critiques, see Barnes 1982, 58-63.

[3] See Technological Forecasting and Social Change 28, 1985, 93-122.

in system behavior may be understood as arising endogenously. Undesirable behavior is assumed to result from the system's loop structure rather than from uncontrollable exogenous factors.[4] Establishing the system boundary requires simplification and reinterpretation. A model wouldn't be useful if it replicated the complexity of the system being modeled. Indeed, "[t]he art of model building is knowing what to leave out." (Sterman 1988, 137) Within system dynamics the exclusion criterion is dictated by the model purpose. A good model contains only those features necessary to accomplish the model's purpose. (ibid)

While simplification is universally recognized as necessary, it is always controversial when put into practice. Sterman is well aware of potential criticism: "In the course of formalizing Kuhn's theory, certain changes in emphasis and interpretation are necessarily introduced..." (Sterman 1985, fn 4, 94) "The model should be viewed more as a rough translation of the theory into formal terms than as a definitive rendering." (ibid, 100) I do not question the need for reinterpretation and simplification; that is part of the modeling process. I also recognize that particular reinterpretations and simplifications will be value judgments of the modeler. But there comes a point when these activities go beyond the bounds of reasonableness, when they begin to do violence to the crucial ideas underlying the system being modeled.

Purpose of this Essay

This essay has two purposes. First, I want to show that Sterman has indeed exceeded a modeler's prerogative of reinterpretation. The model excludes components of the theory which for Kuhn play an important role in paradigm change. I will illustrate this by comparing Sterman's and Kuhn's accounts of the causes of scientific revolution, and then judging how faithful the model is to Kuhn's theory. Given that some reinterpretation is inevitable, this is not an easy task. It first requires that we be able to point to some well-bounded body of discourse and call it "Kuhn's interpretation." It is well known in political science that Hobbes was no "Hobbesian" and that Machiavelli was no "Machiavellian." It think it a safe bet that neither is Kuhn "Kuhnian." Thus, applications and interpretations considered somehow Kuhnian must, to the extent possible, be distinguished from views Kuhn himself has expressed.[5] Indeed, it will be difficult enough to establish a stable account of Kuhn's own thoughts, evolving as they have over more than two decades. The second requirement is that we be able to distinguish fundamental from peripheral arguments. This can be done by examining criticisms of

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[4] For an excellent and brief discussion of system dynamics, see Jay W. Forrester (1987), "Lessons from System Dynamics Modeling," System Dynamics Review, Vol. 3, No. 2, Summer 1987, Pp. 142-146.

[5] Gutting (1980) is a nice collection of interpretations and applications. The bibliography lists 250 works about Kuhn, as well as 48 books and articles by Kuhn himself.

Kuhn and his subsequent refinements. Those aspects of his formulation which have received the most attention are deemed to be the most important. My second purpose is to highlight a few methodological problems which must be faced before a truly valid model of Kuhnian science can be constructed. These will emerge in the process of model evaluation.

If my purpose is to illustrate invalidity, it is not enough merely to know that such a project is possible. There are degrees of validity, and while an appropriate model boundary is necessary, it is by no means sufficient. A more fully specified definition of validity will permit a finer-grained evaluation.

Validity

The best discussion of validity criteria within system dynamics remains Forrester (1961, 115-129), who maintains that validity only has a useful meaning with respect to a model's purpose. (ibid, 115) The purpose of most models is to understand the behavior of complex systems, so that undesirable outcomes may be minimized or avoided. Forrester identifies two requirements a valid model must satisfy. I will call these reality conditions.[6] First, the model must generate behavior that doesn't significantly differ from that of the real system. (ibid, 119) Second, the relationships in the model must represent the true causes of action. (ibid, 122) The second criterion is added in recognition that any number of models can be constructed to reproduce a given set of behaviors, but a model can only be said to "explain" this behavior if its equations reflect the real causal relationships in the system.

While these criteria are meant to have general applicability, they presuppose that a well-defined distinction can be made between the model and reality. I admit that for most models such a distinction can readily be made. In these, the behavior to be explained is easily identifiable empirically. Inventory, profits, sales, fear, pleasure and so forth, are data that are "given" in the sense that they can be understood independently of any comprehension of the forces which determine their behavior.[7] Thus, it isn't necessary to understand the myriad of forces causing sales to fluctuate in order to recognize that sales do fluctuate. The knowledge base used in constructing the model will normally be quite heterogenous. Many different explanations and observations will be taken into account.

There are other models, and Sterman's is one, that do not purport to represent real-world systems. These models are of theories, and their purpose is not to solve problems, but to probe an argument's internal consistency. Thus, the purpose of Sterman's

[6] Forrester has much more to say about validity, but for the present purposes these two criteria suffice.

[7] Note that I list both quantitative and nonquantitative variables. The distinction between the two, and any validity problems associated with the latter, are not of direct interest here.

model is "to test the dynamic consistency of Kuhn's theory...by formalizing it and then testing the formalized theory with a computer simulation model." (1985, op. cit., 94) I understand "testing" in this context to mean being able to account for the behavior Kuhn postulates. That is, Sterman's model will "explain" some body of data by reproducing its qualitative behavior. The problem is that the model has no data in the same sense as the real-world models discussed above. Here the behavior to be explained must necessarily come from the same database from which the model was built. They are both interpretations of the same authoritative texts. This considerably blurs the distinction between the model and the system being modeled, so that to maintain that the model reflects "reality" is to border on the tautological.[8] This would not be a problem if Sterman had chosen to model an actual scientific revolution rather than a generic one. In this case behavior would consist of historical data, culled from sources separable from Kuhn and others' interpretations of him. The model would become that of a real-world system.

I do not want to make too much of the distinction between these two model types. The differences are a matter of degree, not of kind. It is not so much that models of theories are easier to validate, but that they are easier to <u>in</u>validate. The first reality condition presents a difficult hurdle for modelers of real-world systems because of the sheer complexity of exhibited behavior. This behavior is the result of both stochastic and structural factors. Reproducing the behavior predicted by a theory is less difficult for two reasons. One has already been discussed, the very close dependence of model and data on the same database. Another is that this behavior is simply less complex than real-world behavior. Theories, after all, simplify.

The differences in the complexity of the behavior these models generate reflect the relative transparency of the corresponding systems being modeled. Discerning the real causes of behavior is considerably more difficult for a real-world system than it is for a theory-constructed system. Theories simplify behavior because the causal connections they postulate simplify real-world causes. Ceteris paribus, the simpler the causes, the easier they are to distinguish.

It follows that the easier the causes are to identify, the easier particular claims to have recognized these causes are to critique. There can be no appeal to particular authoritative texts in evaluating models of real-world systems, while such appeals necessarily occur in the case of theoretical systems. Thus, an assessment of a model of why scientific revolutions actually occur would be legitimized by reference to a wide variety of historical, sociological, psychological and philosophical texts. No particular text would, prima facie, be privileged over any other. On the other hand, a model of Kuhnian science may only be challenged through

[8] I do not mean to imply that Sterman intentionally "cooked" his reference mode to match his base run. In all models of this type there is necessarily an iterative process of model development and reference mode refinement.

appeal to Kuhn and perhaps a few of his interpreters. While the existence of authoritative texts by no means guarantees an authoritative interpretation of those texts[9], the space of interpretive possibilities must nonetheless be smaller.

Assessing the Model

Of the many points of contact between Sterman and Kuhn, the most important issue is why scientists reject paradigms. The core of Sterman's model is scientists' confidence in the paradigm (CP). (1985, op. cit., 104) A confidence level of one represents total commitment, while a level of zero indicates total rejection. Confidence is a function of the relative number of accumulated anomalies (RA) and the rate of progress of the paradigm (RSP, defined roughly as the ratio of the number of puzzles solved in a year to the total number solved). If the number of anomalies increases above some acceptable number, or the rate of progress falls below some expected level, then confidence will decline.

Practitioners join or leave a paradigm based on the confidence of those in the paradigm relative to the confidence of outsiders in other paradigms. (ibid, 105) The higher the practitioners' confidence, relative to other paradigms, the higher the recruitment rate. The lower the relative confidence, the higher the defection rate. Recruitment and defection are modeled as the same process, though arithmetically inverse. Membership changes are determined through the difference between recruitment and defection.

The model's validity problems center on the role of alternative paradigms. For Kuhn the distinguishing feature of normal science is that few scientists engage in inventing novel theories; they are busily solving puzzles the dominant paradigm supplies. (1970, 24) Novel theories emerge only when the old paradigm is in crisis. These issues are not peripheral. His critics have focused precisely on the distinction between normal and revolutionary science. Note some of the chapter titles in Lakatos and Musgrave (1970): "Against Normal Science", "Does the Distinction Between Normal and Revolutionary Science Hold Water?", and "Normal Science and its Dangers." The postulation of normal science is one of Kuhn's most controversial claims.

Sterman is aware of Kuhn's position, but chooses not to model it, due to a combination of a system dynamicist's need to preserve endogeneity and any modeler's natural wish to create as parsimonious a model as possible. Thus, he correctly notes that competing paradigms "tend to be born in the crisis phase of an existing paradigm," and are "part and parcel of the dynamic process," but then avers that credible models must also generate the predicted behavior "without relying on external driving forces such as the emergence, as if by magic, of a new and better theory." (Sterman, 105; 96) If having an alternative paradigm is desirable, and it can not be introduced exogenously, then the only remaining choices are either to model the emergence of the new paradigm, or

[9] The voluminous literature concerned with interpreting Karl Marx is proof of this.

posit a continuously existing alternative. Preferring parsimony to accuracy, Sterman chooses the latter strategy.

In principle such a tactic is not impermissible. The effect of this alternative paradigm can be neutralized during normal science and then switched on right before a crisis by adjusting the values of confidence in alternative paradigms (CAP) and effect of confidence on recruitment and defection (ECR and ECD). But note that unless the switch conditions are determined within the model, the net result would be to introduce the switch exogenously, something Sterman explicitly declares to be unsatisfactory.

A bigger problem with positing a continual alternative paradigm, but negating its effects, is that it reifies structure. There is a big difference between novel theories emerging in crisis, and novel theories always existing but only gaining salience during crisis. The first view is of the emergence of a new structure, the second of an ever-existing structure that suddenly gains importance. The difference is ontological, and may not matter in terms of model results, but it surely matters if one is concerned with how accurately the model represents the theory.

This relatively minor problem of system boundary is made much worse by the way in which alternative paradigms are actually implemented in the code. Disregarding the differential effects of alternative paradigms in normal and revolutionary science, he sets the confidence in these paradigms to a constant.[10] This represents a fundamental error in the model. Because the confidence in alternative paradigms never changes, it can be removed without changing the qualitative or quantitative behavior of the model. Alternative paradigms in Sterman's model are superfluous.[11]

For Kuhn alternative paradigms are not only necessary but crucial to the dynamics of the fall of a paradigm. He is quite explicit on this:

> [T]he act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature <u>and</u> with each other. (1970, op. cit., 77)

Once again, the validity of the model depends on how critical this aspect of Kuhn's theory is. I take it to be decisive. The "<u>and</u>" in the last sentence quoted is italicized in the original, indicating that Kuhn knew that the most provocative part of the thesis was

[10] The value of CAP, 0.5, corresponds to maximum uncertainty in the competing paradigm.

[11] To remove the effect of other paradigms, I set the confidence in alternative paradigms, CAP, equal to one, and then rescaled the effect of confidence on recruitment and defection, ECR and ECD, to range from zero to one.

contained in the last clause. Indeed, Kuhn's elaborate and very interesting comparisons of Lavoisier's and Priestley's views of chemistry, and Copernican and Ptolemaic astronomy, testify to the importance of alternative paradigms in the revolutionary process. It is here that Sterman has sacrificed too much in the name of parsimony. One can't reject the role of competing paradigms and still claim to be providing a valid model of Kuhnian scientific revolutions.

It is illuminating-- and intriguing, that despite the injudicious choice of model boundary, the model has satisfied the first reality condition. Sterman does manage to reproduce behavior characteristic of Kuhnian paradigm change. This can mean one of two things. Either Kuhn's theory contains propositions (i.e., the whole business of alternative paradigms) that are not necessary to produce the forces of change he postulates, or the model has failed the second reality criterion. That is, the forces modeled are not the same forces Kuhn postulates. Postponing discussion of the first possibility for the time being, let me consider the second. Does the model validly portray even those aspects of Kuhn's theory that it claims to represent?

Consider again the question of why scientists reject paradigms. In Sterman's formalism this question is equivalent to asking why recruitment falls and defection rises. Falling confidence plays the key role. Confidence rises or falls due to the combination of two factors: relative number of anomalies and progress in puzzle solving. Note the character of the equations that determine the effects of anomalies and progress on confidence:

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CC.KL=NCC*ICC.K*RCC.K
ICC.K=EAC.K + EPC.K

EAC.K=TABHL(TEAC,RA.K,0,6,.5)
TEAC=5/2.15/0/-1.2/-2.15/-2.9/-3.4/-3.9/-4.4 ...

EPC.K=TABLE(TEPC,RSP.K,0,5,.5)
TEPC=-5/-2.15/0/1.2/2.15/2.9/3.4/3.9/4.2 ...[12]
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CC is total change in confidence. ICC is indicated change in confidence, and does most of the work. NCC is normal change in confidence and RCC is receptiveness to change in confidence. They are multipliers which reduce or magnify the effect of ICC. EAC is the effect of anomalies on confidence, and EPC is the effect of progress on confidence. For reasons of space not all the values of TEAC (or TEPC) are listed. RA is relative anomalies and measures how many anomalies there are relative to the acceptable number. RSP stands for relative solved puzzles, and compares the current rate of puzzle solving with the total number of puzzles the paradigm has solved. The coefficients for anomalies and solved puzzles are left out of the ICC equation because in the base run they have a value of one.

[12] Sterman, 1985, op. cit., 120.

Consider the first equation. Since NCC and RCC are positive and constant, any change in the sign of CC must be due to the change in sign of ICC in the second equation. This will occur when the sum of EAC and EPC is less than zero. Thus, confidence declines when the effects of anomalies and progress on confidence are negative. This will only happen when the number of anomalies (RA) rises too high, and not enough puzzles (RSP) are being solved. From our perspective the important point is that confidence falls because there are too many anomalies and too little progress.

Kuhn would not agree that paradigms are abandoned, at least in the beginning, because there are too many anomalies, or too little progress. As he says, "paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms." (1970, 157) Neither Lavoisier's oxygen theory nor the phlogiston theory could account for all the facts that the other could account for. Each could explain phenomena the other couldn't account for. Similarly, Copernican astronomy did not surpass Ptolemaic in accuracy until over a half century after Copernicus had died, yet Copernican theory prevailed. (Kuhn 1977, 323) Lavoisier's oxygen theory and Copernican astronomy were initially accepted not <u>because</u> of problem-solving ability, but despite it.

The point is extremely important, because it is precisely Kuhn's unwillingness to privilege problem-solving ability that prompts his detractors to accuse him of abandoning science to "mob rule" and "irrationality." Bell and Bell (1980), for example, identify two views of Kuhn, one of which places him fairly close to Popperian refutationism, and another "dogmatic" interpretation, which insists there is no intellectual criterion for paradigm comparison. (ibid, 18-20)

Kuhn recoils from such accusations. (See 1970b.) It is not that paradigm choice is made irrationally, but that accuracy is not the only criterion of choice. Paradigms are compared not only with reality, but with each other. Evaluation will be based as well on consistency, scope, simplicity and prospects for future progress. Together with accuracy, these criteria form the shared basis for choice. (1977, 321-322)

In any given historical situation these criteria may conflict with one another. While both Copernican and Ptolemaic astronomy were internally consistent, only Ptolemaic was also consistent with other physical theories. Thus, consistency spoke in favor of Ptolemy. Simplicity, on the other hand, favored Copernicus. At least in terms of mathematical apparatus, Copernican astronomy required only one circle per planet, while Ptolemaic required two. (1977, 323-324) Resolution of these conflicts requires ranking these criteria in order of importance.

Note, however, that "importance" is a value judgement. There is no a priori reasoning for favoring simplicity over consistency, or any one criterion over any other. Science is silent on the issue, so that scientists will differ over which is the more important. Copernican astronomy triumphed because Copernicus had faith that his simpler, more elegant theory, once fully articulated, would surpass the Ptolemaic system in accuracy. But during Copernicus' life such success was but a dream. Ultimate triumph was not achieved by Kepler until long after Copernicus had died.

Yet even if scientists could agree on how to rank the criteria, they would still disagree on how to apply them in particular situations. (1977, op. cit., 331) Thus, while Copernican astronomy was simpler, it was so only in terms of mathematical apparatus. In was not simpler in terms of the computational work required to make predictions. Simplicity here has two different meanings. Science, once again, does not recommend one or the other. (ibid, 324) Similarly, scientists weighing the relative accuracy of the oxygen and phlogiston theories would almost certainly disagree on which was the more accurate, since the theories did not account for the same phenomena. Thus, even if accuracy were the agreed upon value, choosing between the two would require deciding which phenomena were more important, surely a decision that would vary with the individual. (ibid, 323)

Kuhn is reserving a role in the paradigm debate for the scientist as a unique individual. Personality and education will influence choice. (1970b, 241) Thus, one can not explain what animated the early Copernicans without recourse to the "ear for mathematical harmonies" provided by the rise of neoplatonism in Renaissance Europe. (1957, 181) It is because of these individually varying factors that Kuhn insists that an algorithm able to dictate rational, unanimous choice is unattainable. (1977, op. cit., 326)

Although the model fails to represent these criteria of decision, it is not necessarily completely invalid. Kuhn makes a distinction, though not very explicitly, between the initial acceptance and the ultimate acceptance of a paradigm. (1970, 156) He argues that only a relatively few scientists need be converted through these individual criteria. They will then develop the new paradigm to a point where other scientists can adopt it purely for reasons of predictive accuracy and puzzle-solving ability. Ultimately, a proposed paradigm does not become the new paradigm of normal science unless it surpasses the old one in its ability to solve puzzles.

There are thus two general methods of paradigm change. The first utilizes the idiosyncratic value systems of certain key individuals. They adhere to the new paradigm despite the fact that it may not be as accurate as the old paradigm. The new paradigm may simply have aesthetic appeal. The second method involves collective behavior, and is based on the new paradigm's ability to solve puzzles. Practitioners convert because the new paradigm solves more puzzles. Given the way paradigm change is portrayed in Sterman's model, it appears to be applicable only to the second group. The individual behavior characteristic of the early stages of revolution remains unmodeled.

Toward a Valid Model of Kuhnian Science

There are two areas in which the model is invalid. The first is its neglect of alternative paradigms. In principle such paradigms are not difficult to incorporate. Since they arise endogenously, they are well-suited to being modeled within system dynamics. It was an injudicious choice of system boundary to exclude them, but an understandable one. Done properly, the model would have at least doubled in size.[13]

The second aspect of invalidity concerns the incomplete representation of paradigm change. I submit that if we take Kuhn literally about the lack of an algorithm dictating rational, unanimous choice, no valid system dynamics model of Kuhnian science is possible. To see this, let us begin by imagining the very best possible model, a valid and accurate representation of Kuhn's theory. Belief in this model implies belief in the dynamic consistency of Kuhn's argument, since the purpose of the model is to illustrate this consistency. But belief in the model is more than just belief in dynamic consistency. Since model behavior is just the logical consequence of simulating the model's assumptions, belief in the model is equivalent to belief in the model's assumptions. These assumptions are the interrelationships of the variables as represented in the code. The American Heritage Dictionary (2nd College Edition, 1982) defines "algorithm" as "a mechanical or recursive computational procedure." (93) A system dynamics model is precisely such a procedure. The code of our imaginary model is nothing more than a mechanical method for comparing paradigms. Thus, if we continue to believe in the validity of this model, then we have accepted what Kuhn describes as unattainable, an algorithm dictating rational, unanimous choice.

There is no prima facie contradiction here. I can grant Kuhn consistency without having to agree with him if I only disagree with his assumptions. This is so because by disagreeing with an argument I am disagreeing either with the behavior predicted by that argument, or with that argument's assumptions. By granting dynamic consistency I am assenting to the behavior, given the assumptions. Thus, any disagreement must be over the assumptions. Now, if our imagined model is the best possible, then disagreeing with Kuhn's assumptions is equivalent to disagreeing with the model's assumptions, since the model's equations embody Kuhn's assumptions, by assumption. But questioning a model's assumptions means questioning its validity. Since by construction the model is the best possible, this is the same as asserting the invalidity of the most valid model. If the most valid model possible is invalid, then no such model is possible.

Let me repeat, this is true only if Kuhn is taken literally. As discussed previously, what Kuhn means is that in the crisis stage of a paradigm, an individual's historical and cultural context plays a key role. For the modeler of Kuhnian science the problem then becomes incorporating these exogenous and contingent elements into what is supposed to be a model of generic processes.

If Sterman's model were of an historic revolution rather than a generic one, then the latter problem disappears. Thus, if the model were of the Copernican revolution, then Copernicus' neoplatonism, the necessity of calendar reform, and all the other reasons that animated Copernicus could be validly incorporated into

[13] All the structures of the model-- recruitment, defection, confidence, etc., would have to be duplicated for the competitor. Extra code would also have to be added to compare the paradigms.

the model. They become, if you will, the facts of the matter. But generic models must identify forces common to the behavior of all the revolutions to which Kuhn's theory applies. Causal factors unique to any particular revolution may not be included.

One possible solution is to make the contingent forces generic. The process is rather simple. One takes all the reasons that these key individuals were committed to their paradigms, and one constructs a category that includes all and only these reasons. Thus, "simplicity", "accuracy", "scope", etc., all become lumped into a category called, say, "aesthetic advantages." Then one constructs a function representing the relationship between the number of aesthetic advantages and confidence in the paradigm. Such a function might indicate that as the degree to which the old paradigm is aesthetically pleasing goes down, so does confidence. The uniqueness of a Copernicus or an Einstein becomes only one instantiation of a generic process of paradigm disillusionment. The difficulty with this formulation is not that it is reductionist, but that it implies that the practitioner somehow "chooses" the new paradigm when the aesthetic problems of the old one become too great. Yet Kuhn suggests that "choice" may not be the best way to describe what is going on.

The most radical of Kuhn's theses, and one I have not touched on in this paper, is the concept of paradigm incommensurability. Incommensurability means that there can never be full translation from one theory to another. (See Kuhn 1970, 148-159, 198-204; 1970b, 266-278). The idea here is that a theory's concepts change their meanings and applicability in moving to the successor theory. "Planet" did not mean the same thing to Copernicus as it did to Ptolemy. Einsteinian "mass" is radically different from the Newtonian version. Full translation would require conversion of both theories into some neutral sense-datum language, and for Kuhn no such language exists. This expresses his belief that "facts," which would normally be appealed to in paradigm debate, are not independent of theory. People in different paradigms speak different languages, so that point by point comparison of two different paradigms is no more possible than such a comparison of two languages. Yet it is just such a comparison process that one must perform if one "chooses." The process of paradigm change is better understood as a gestalt switch or conversion. The scientist simply begins to practice in the new paradigm. (Kuhn 1977, 338-339; 1970, 204)

Sylvan and Glassner (1985, 103) suggest that a model ought to be judged based on the fit between the assumptions of the theory being modeled and the mathematics used to model the theory. Kuhn's turn toward semantics to illustrate incommensurability suggests that more interpretive modeling methodologies, such as artificial intelligence, might be more suitable for simulating the deep structure of Kuhnian paradigm change.[14]

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[14] Kuhn's role as a thesis advisor for Kenneth Haase, who is doing a dissertation on automated discovery systems, indicates that he is aware of such possibilities.

Conclusion

Sterman concludes his article by noting that "[i]t is not necessary to invoke either competition between theories or 'great men' hypotheses to account for scientific revolutions." (1985, op. cit, 118) He is correct. His reproduction of the behavior characteristic of paradigm change indicates as much. But this quotation is marked by both a presence and an absence. The presence is of the word "necessary," the absence of the word "Kuhnian." If Sterman's theory doesn't require great men and competitor theories, can the same be said for Kuhn's? There is no clear answer to this. While Kuhn never explicitly declares these forces to be necessary, neither does he say they are unnecessary. They are simply there.[15] In some ways this is what makes him an historian rather then an philosopher. But whether these forces are necessary or not is a question of the validity of Kuhn's theory, not of Sterman's model. Regardless of any necessity, great individuals and competitor theories are indispensable, indeed constitutive of Kuhnian science. Sterman most assuredly has a theory of scientific revolutions, but not a model of Kuhnian paradigm change.

I am not suggesting that the model has no value; it does. Indeed, it is a bold experiment, and as it now stands it is splendidly representative of the feedback processes at work in normal science as well as the dynamics of collective behavior during paradigm change. It thus lends us extra confidence that these parts of Kuhn's theory are sensible. Furthermore, its very faults raise some fascinating questions about model construction in general. The need to account for the role of individuals raises the question of how it is possible to incorporate individual behavior into a model representing collective behavior. There are composition problems associated with having two very different foci of analysis in the same model.[16] And if the behavior can not be understood in feedback terms, there is the further question of how to create more "intelligent" system dynamics models. Merten (1988) suggests the use of "intelligent logical loops" to model the structure-transforming behavior characteristic of social evolution. Insofar as this can generate qualitative behavioral change, it is a step in the right direction.

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[15] He does assert that the functions performed by the behavior he postulates are necessary if science is to flourish, but leaves open the possibility that other behaviors might serve similar functions. See Kuhn, 1970b, 237.

[16] I am indebted to John M. Richardson, Jr. for pointing out this implication of my argument.

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