

CIRJE-F-156

On Paradigms, Theories and Models

Haider A. Khan

University of Denver / CIRJE, Univeristy of Tokyo

June 2002

Discussion Papers are a series of manuscripts in their draft form. They are not intended for circulation or distribution except as indicated by the author. For that reason Discussion Papers may not be reproduced or distributed without the written consent of the author.

On Paradigms, Theories and Models

Haider A. Khan
University of Denver
Denver
Co. 80208 USA
Tel. 303-871-4461/2324
Fax 303-871-2456
e-mail:hkhan@du.edu
and
Visiting Professor,
CIRJE,
Faculty of Economics
University of Tokyo,
Tokyo, Japan
Tel.03-5841-5642
Fax 03-5841-8294
Revised, March 2002
April, 2002

Abstract:

The purpose of this brief note is to alert the reader to the existing confusing state of affairs in the social sciences regarding the terms paradigm, theories and models, trace a few of the causes, and offer some tentative distinctions that may make our discourses a bit clearer. Since the word paradigm is used in so many different ways, it is suggested that we avoid using this term unless necessary in a particular context. For most ordinary scientific discourse and debate, the terms theories and models are sufficient. As shown in this paper, they are terms that can be defined clearly, and used to raise relevant questions about choice among different theories and models. From this perspective, paradigm seems to be an example of the traps that beset a careless user of ordinary language. Wittgenstein was the most important modern philosopher to point this out in general. To use a somewhat Wittgensteinian language, paradigm is an example of a language game that has somewhere gone awry. But we still have the language games of models and theories that are eminently serviceable for the social science discourses.

On Paradigms, Theories and Models

Haider A. Khan

Conflation of the distinct terms--- paradigm, theories and models--- is an all too frequent cause of confusion in the social sciences. The purpose of this brief note is to alert the reader to this confusing state of affairs in the social sciences, trace a few of the causes, and offer some tentative distinctions that may make our discourses a bit clearer. It should be acknowledged at the outset that there are genuine philosophical difficulties that are subjects of debate in the frontiers of contemporary philosophy of science; however, most social science discussions do not involve these debates--- the confusions are fairly elementary, but remain unacknowledged. A bit of classic Wittgensteinian 'linguistic therapy' is all that is needed for most purposes. And that is exactly what I intend to offer here. After clearing up an elementary difficulty regarding what counts as almost a 'natural and intuitive' notion of 'theory' and 'model' in some circles, I will proceed to compare and contrast the concepts of theory and model in a rigorous way, turning to the concept of paradigm later. The reason for this are the multiple meanings and implications of the latter. In addition, the work done earlier on theories and models will help making sense of paradigms later. Thus, the expository order will in fact follow a certain logical order.

However, an elementary point regarding formal vs. ordinary languages needs to be made first. There is a dogmatic tendency among some economists in particular that asserts that only formal 'theories' should count as bona fide theories. This, of course, conflates theories with formal models, and begs the interesting question: under what conditions is a theory identical with a model? The answer that is implicit in this tendency is: under all conditions. This assertion needs to be reexamined.

If we depart from this type of dogmatism--- and there are good reasons for this departure--- then we have to admit at least some ordinary language formulations to the status of theories. For instance, we could include under the term theory, the original ordinary language formulations of the market economy system by Smith, of the price-specie flow mechanism by Hume, the trade theory of Ricardo etc. To rule these out as non-theories on the ground that these are not formal, does not seem sensible since they all yield testable implications which is a commonly accepted criterion of whether we allow the term 'scientific theory' to be applied to a string of propositions in a substantive area of research and discourse.

Finally, the argument with respect to rigor which is offered sometime in defense of the proposition that 'without formalization there are no theories' can not withstand logical and historical scrutiny either. Historically, many fields that are not formal (e.g., geology) gained the status of a theory-based science when interesting causal structures that were not necessarily formal were advanced (e.g., the theory of plate tectonics). Logically, both formal and ordinary languages can be used rigorously or non-rigorously. Of course, an unintentional lack of rigor always exposes the proposed theories to the possibility of outright rejection. But this can happen in the case of

formal as well as ordinary language formulations. In what follows I assume that the scientists who propose new theories can formulate their propositional syntaxes in the most rigorous manner without logical inconsistencies, regardless of the nature of language used.

Having cleared up this initial difficulty, I proceed now as I promised earlier to try to define the concepts theory and model more rigorously than I have done so far. The final step will be to introduce the idea of a paradigm and show the difficulties that are involved in treating this slippery concept.

Theories and Models: Explanatory schema

Both theories and models are said to be explanatory in nature. They could also be predictive; but as many natural science theories are not predictive, at least this is not a requirement that all theories can fulfill in the social sciences.¹ Therefore, my proposal is to drop this requirement which is a legacy of logical empiricism from its Vienna Circle days. The key task then is to begin with a coherent logical sketch of explanation.

The logical form of an explanation is that of a *Modus Ponens*. So, the schema can be expressed as:

If **P** then **Q**
P

Therefore, **Q**

The main point is that P may involve both theoretical, unobservable terms and auxiliary hypotheses as well as some observables. Q is the set of observation statements to be explained.

The naïve Popperian position is to recognize from the above that no theory can ever be completely confirmed, and offer the (naïve) falsificationism as the way science can maintain its integrity and progress. For this, we have to use the logical form of *Modus Tollens*, i.e.:

If **P** then **Q**

Not **Q**

Therefore, not **P**

Clearly, this ignores the problem of joint hypothesis testing, or more deeply and generally, the Duhem-Quine problem. If P is a set of hypotheses (some of which may be auxiliary hypotheses, but others--- the plural is important here--- are part of the theory itself), then the observation sentence, ‘not Q’ does not by itself automatically lead us to a valid inference regarding which of the hypotheses (one or several) are to be rejected.

¹ It will become clear from the discussion of explanation that given the explanation scheme “retrodiction” is possible. Once an event has taken place, with careful work, in principle, we can always uncover the causal mechanism that brought it about.

The key point is that the *modus ponens* and *modus tollens* forms are accessible to both ordinary languages and formal languages. Thus the theoretical status of an ordinary language theory is not threatened simply because it is formulated in ordinary language. We can now turn to the question of models derived from such theories in a rigorous manner.

Suppose we have a theory T in an ordinary language, *O*. In order to form a (partial or total) model M of T in a formal language we have to translate the terms of T to the terms of the formal language *L*. If the translation from *O* to *L* is complete, T and M are logically and empirically equivalent. If the translation from *O* to *L* is incomplete then the model necessarily captures less content--- logically and empirically--- than T.

To recapitulate briefly in the language introduced above, we explain an observable phenomenon (e.g., inflation, unemployment, legal institutions) with the help of either T or M, if we can deduce the observation statement from some lawlike statements and a set of auxiliary hypotheses. The latter are particularly important. Hence the best way to explain the concept of explanation I have in mind is to discuss their role as well as the role of the lawlike statements in a logical schema by giving a concrete example below:

An example of changing an auxiliary hypothesis in the Asian Crisis Explanations in the World Bank Study

The role of the auxiliary hypotheses have been discussed in the philosophy of science literature by Hempel, Putnam, Boyd and others. Briefly, in a deductive-nomological (D-N) “model” of explanation first advanced by C.G.Hempel the scientist uses the following logical scheme.

L: L_1, L_2, \dots, L_n
(Laws or lawlike statements)

A: A_1, A_2, \dots, A_n
(auxiliary hypotheses)

E: Explanandum (i.e. that which is to be explained)

The line above the explanandum indicates that statements under L and A together lead to the logical deduction of E.²In the above explanation(D—N) model the set of laws

² Although Logical Positivists like Hempel and Scientific Realists such as Putnam and Boyd use the same logical scheme, a crucial difference is that the latter group accords real ontological status to theoretical entities such as quarks, genes, human ‘rationality’ or ‘institutions’. See Putnam(1975) and Boyd et.al.(1980; 1991)

or law-like statements L is usually accepted without reservations unless there is a scientific revolution underway which provides a new “paradigm” with a new set L^* of laws etc. The set of auxiliary hypotheses A are accepted on a more provisional basis. These range from matters such as instrumental reliability in the physical sciences to things such as institutional structures/stylized facts in the social sciences. An example of such an explanation in economics and the crucial role of switching auxiliary hypotheses with respect to explaining the East Asian miracle and debacle is given below.

In the East Asian Miracle and related studies the government-business relationship is given the status of serving informationally-efficient role in explaining allocation of investment funds among other things. How does this work? In reality, the argument can be made quite complicated; but the following somewhat simplified version maintains the basic structure and premises of the explanation and can be used for illustrative purposes.³

One possible way to formulate the argument is as follows:

- L_1 : Efficient markets operate by price-flexibility leading to market clearing
 - L_2 : When prices do not carry all the information, “other institutional mechanisms” in addition to the fairly well-functioning markets are necessary to make markets function efficiently so that investment can be allocated properly.
 - A_1 : In (miracle) East Asian economies some (a relatively small number—mostly in the financial markets) prices did not carry all the relevant information.
 - A_2 : However, government-business exchange of information substituted (to a large extent) for the missing information in A_1 .
-

E : Investment was allocated efficiently (for the most part) in the miracle economies

However, in the post-crisis period the auxiliary hypothesis A_2 has been changed to something like the following:

- a_2 : The (crony-capitalist) government-business relations lead to distorted signals and create serious moral hazard and adverse selection problems.

³ See Khan and Yanagihara (1999) for an extended discussion of explanations of the Asian miracle and crisis.

Notice how easily this change leads to the seemingly correct explanation of:

e: There has been serious misallocation of investment in East Asia.

Not only does this switch seem to explain in a logical manner the misallocation of investment—claimed to be an integral part of the Asian crisis---it also gives “crony-capitalism” an explanatory salience that is most unexpected given the explanation of the success of Asian economies in the past. This example illustrates both the ease with which auxiliary hypotheses can be changed under most (perhaps all) theories and models and the difficult issues involved in testing whether the switch is legitimate as a scientific exercise. Only extensive historical and institutional research complemented perhaps by careful econometric work that identifies clearly the role of each and every auxiliary hypothesis and their logical relations to one another and the Laws or Law-like statements within a complex scientific explanation can begin to address the task of proper testing in this instance.

What is interesting about this example is that it shows that theories and models may coincide if and only if the theory can be formalized completely without loss of content. Otherwise they need not coincide, and the model will always be simpler than the (at least partially) ordinary language theory. At the same time the model will in general be clearer than the (more complex) ordinary language theory. Hence, under the circumstances where theory is more complex than a model there seems to be a trade off between comprehensiveness and clarity. Perhaps this is what Nelson had in mind in part when he tried to make a distinction between “appreciative” and formal theories. However, appreciative theories could be sharpened, and this need not be always in the direction of making them more formal. After all, if the formal language is limited in certain ways, going from the ordinary language theory to a formal model will always involve a loss of content. This violates the condition given earlier for the coincidence of a theory and its model.

In order to avoid confusion, it should also be mentioned here that often many people mean by a model an analogical device. For example, to use the globe as a model of the earth, we are using an analogical device. In the sense I have used the term model, an economic (or physical , biological, sociological, political etc.) model is not just an analogical device. It is a simplification of the empirical reality(what Marx called ‘the

chaotic concrete' in the Grundrisse), but it attempts to describe, explain and, sometimes predict, in a formal way. A theory also does the same, but it need not be always formal.

I now turn to the problem of paradigms.

Paradigms: what are they?

Since Thomas Kuhn published his book *The Structure of Scientific Revolutions*, use of the word paradigm has proliferated. So much so that even politicians do not shrink from using it. And of course, one always hears of "a new paradigm". Kuhn himself became very dismayed by this development, and as anyone familiar with his work knows, new paradigms do not come in such large numbers and frequencies as the current ordinary and even academic usage would seem to imply.

However, Kuhn is not completely blameless either. The density of his prose and the ambiguities in his original version are at least partially responsible. In 1965, Margaret Masterman pointed out twenty two different senses in which paradigm was used by Kuhn in his short monograph. At the end, however, these can be reduced to three main categories:⁴

1. Paradigm as a sociological construction: this is the version that is popular among the relativists and sociologists of knowledge. Paradigms come into being when enough people in a scientific community accept them as frames of reference and as these are then used for the 'puzzle-solving' activities in normal science. Clearly, in this formulation, truth-claims are relative to the paradigms. It is not clear if people can communicate across paradigms. Paradigms may include theories and models in the sense developed above, but it is not necessary to have full-blown theories and model to carry on the social activity called 'normal science'.
2. Paradigm as an exemplar or an artifact: This is what Kuhn later claimed he had in mind all along. In graduate school, students are trained using "paradigm" cases and examples. In experimental sciences methods of research are taught by example. This usage is close to the idea of scientific training as an apprenticeship. Again, theories and models are neither necessary nor sufficient for this to happen, although all mature sciences have them.
3. Paradigm as theory: This is the most restrictive sense. However, this has textual support also. Kuhn's critique of logical positivism involved pointing out the "theory-ladenness" of observations. Therefore, 'normal science' would seem to involve theoretical terms. Furthermore, scientific revolutions, when they occur, seem to change our theoretical vocabulary. Thus, quarks entered the scientific lexicon with the development of the quantum mechanics revolution in physics. In economics within the neoclassical school, the idea of

⁴ See Kuhn(1970;1977) and Lakatos and Musgrave(1970).

cardinal utility was replaced by the idea of ordinal utility and indifference curves arising from the (partial) ordering of preferences.

Since the word paradigm is used in so many different ways, my inclination is to avoid using this term unless necessary in a particular context. For most ordinary scientific discourse and debate, the terms theories and models are sufficient. As shown above, they are terms that can be defined clearly, and used to raise relevant questions about choice among different theories and models. From this perspective, paradigm seems to be an example of the traps that beset a careless user of ordinary language.

Wittgenstein was the most important modern philosopher to point this out in general. To use a somewhat Wittgensteinian language, paradigm is an example of a language game that has somewhere gone awry. But we still have the language games of models and theories that are eminently serviceable for the social science discourses.

References:

- Boyd, Richard, 1980, *Lecture Notes in Philosophy of Science*, Ithaca: Cornell University.
- Boyd, Richard, P. Gasper and J. D. Trout eds., 1991, *The Philosophy of Science*, Cambridge, Ma: The MIT Press.
- Hempel, C.G., *Philosophy of Natural Science*, Englewood Cliffs, NJ: Pacific Hall
- Khan, Haider A. and Toru Yanagihara, 1999, "Asian Development Before and After the Crisis: Some Stylized Facts and Paradigmatic Features with an Agenda for Future Research", manuscript ADBI, February 1999.
- Kuhn, Thomas, 1970, *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press.
- , 1977, *The Essential Tension*, Chicago: University of Chicago Press.
- Lakatos, Imre and Alan Musgrave, 1970, *Criticism and Growth of Knowledge*, Cambridge: Cambridge University Press.
- Putnam, Hilary, 1975, *Mathematics, Matter, Method*, London and New York: Cambridge University Press.