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SPECIAL REPORT

# LIFE IN A MULTIDIMENSIONAL WORLD

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The methodology of the science of science is claimed to be plagued by one-dimensional thinking, and it is urged that a multi-dimensional view be adopted instead. In a one-dimensional model "cause" is a meaningful word, superlatives can be used, dichotomous thinking is realistic, with a resultant "zero-sum" mentality, and the "make a hypothesis – find a correlation" method makes sense. In the multidimensional framework these four characteristics are unsuitable, and instead a quite different set of questions arise as appropriate. This is illustrated on five examples taken from among currently interesting questions in the science of science. Following some remarks about simplicity and about the role and limitations of multiple regression analyses, it is concluded that, among other things, more purely phenomenological studies are needed to make progress in the science of science.

## I. Introduction

This discussion is about methodology, about ways of looking at problems, in the area of the science of science, or perhaps in a much larger field of inquiry. The concerns expressed here are broad, and refer to very fundamental patterns of investigation. As one would expect in such a situation, the conclusions, statements, and proposals will by necessity have to be tentative, provisional, perhaps even somewhat diffuse, in the expectation that time, discussions, and experimentation will eventually precipitate a form of these considerations which is both clear and functional.

In a nutshell, my point is that an overwhelming fraction of work in the science of science, and in fact in many other areas of inquiry, has been carried out in an implicitly or explicitly one-dimensional framework and therefore with a correspondingly one-dimensional methodology. It is my contention that this is a fundamentally incorrect way of looking at problems which, from the very outset, distorts reality and hence is unable to arrive at truly insightful conclusions. Instead, I claim, one must adopt a multidimensional model of reality and use a methodology befitting this model in order to achieve meaningful and functional understanding which then also have some predictive power.

While the terminology in the above paragraph needs explanation and hence the meaning of this paragraph may not yet be clear to the reader, it is not difficult to find statements from the past that appear to point in the same direction. To quote one from a personage of high visibility, *Feyerabend*[1] wrote: "This plurality of theories must not be regarded as a preliminary stage of knowledge which will at some time in the future be replaced by the One True Theory. Theoretical pluralism is assumed to be an *essential feature* of all knowledge that claim to be objective." Without determining what *Feyerabend* means by "theory" and "plurality" one cannot be sure exactly how his statement relates to the present discussion, but it is evident that both express dissatisfaction with the present one-dimensional methodology.

The discussion will be fitted into four sections. The first (Section II) will describe some of the relevant features of a one-dimensional methodology and of the way it poses problems. This is followed (in Section III) by a description of various aspects of analysis in many dimensions. The last section (Section V) offers some concluding observations.

In order to avoid working entirely in an abstract domain, some problems in the science of science will be used as examples in Section IV to illustrate the statements of the discussion. These problems are as follows:

(1) The relationship between science and technology
(2) The motivation of scientists
(3) Lotka's law
(4) The influence of "Zeitgeist" on scientific theories
(5) Is there a different science for the Third World?

## II. The one-dimensional fantasy and its problem posing

A one-dimensional model is one in which events, factors, causes, and effects (called henceforth "elements") can be arranged in a linear chain, ordered either chronologically or in a logical sequence. One might imagine a picture of such a model as a series of circles connected with lines in a chain-like fashion. The lines also carry arrows, generally in one direction, though occasionally in both directions.

Such a model has a number of characteristic properties.

(1) "The cause of something" is a meaningful concept.

On the whole, in such a model, when arranged in a logical sequence, each element is connected to one and only one other circle with the arrow going from that other circle to it. The other circle can then be considered, unambiguously, *the* cause of our element.

(2) Superlatives are meaningful.

Words like "best", "largest", "fastest", have unambiguous meanings in a one-dimensional world. Along a single scale, the extreme can be well defined.

(3) Happenings can be thought of in dichotomous and hence often polarized terms, in terms of gains and losses. Indeed, along a single dimension, once the positive direction is agreed on, any movement is definitely a gain or a loss. Furthermore, in such a one-dimensional world a gain for one of two competing parties automatically becomes the loss for the other. The world in this model is a "zero-sum" game.

(4) The model is well suited to the "make a hypothesis – test it by correlation" method so predominantly used in the social sciences. A hypothesis is made about A being the "cause" of B, and then data are gathered to see if there is, indeed, a correlation between A and B. If so, the hypothesis is said to have been verified. Although this description of the hypothesis-correlation method is somewhat oversimplified, it suffices for the present purposes.

## III. Analysis in many dimensions

A many-dimensional model is one in which elements are located not along a single chain but all over space, with a complicated network of connecting lines extending among these elements[2]. Another way of visualizing this situation is to imagine a multidimensional Cartesian space, each axis of which is labeled by a different aspect or property of the element and the element is then located in this multidimensional space so that its coordinates along the various coordinate axes describe a different aspect of the element.

It is easy to see that the four characteristics discussed in the previous section in connection with one-dimensional models lose their meanings in multidimensional situations. When there is a complex network of connections and arrows between elements, the meaning of "cause" becomes at best very tenuous, and in any case the statement "A is *the* cause of B" is void of any meaning. In a multidimensional space superlatives have no meaning until one establishes a correspondance scale between the units along the various coordinate axes, a process that is complicated, usually highly subjective, and is seldom actually performed.

Movements in a multidimensional space no longer follow a "seesaw" pattern, and hence "gain" and "loss" are not defined unambiguously. In any case, it is mostly untrue that a gain of one party in a competition entails a loss for the other. Finally, making a hypothesis in a one-dimensional form is likely to be ambiguous, and in any case cannot be "verified" by establishing a correlation.

Indeed, an empirically established correlation between A and B in a multidimensional model can have a large variety of interpretations, of which I list twelve:

(1) A is a necessary condition of B

(2) A is a (non-necessary) condition of B

(3) B is a necessary condition of A
(4) B is a condition of A
(5) A and B are in a feed-back loop
(6) C is a necessary condition of A and B
(7) C is a condition of A and B
(8) C is a necessary condition of A and a condition of B
(9) C is a condition of A and a necessary condition of B
(10) C is a necessary condition of A and is in a feedback loop with B
(11) C is a condition of A and is in a feedback loop with B
(12) C is in a feedback loop with A and B.

In such a multidimensional situation there are many important questions to be asked, most of which do not exist in the one-dimensional fantasy:

(a) Is A necessary condition of B? One knows with certainty, because of the multidimensional nature of the model, that A is practically never a sufficient condition of B.

(b) How many necessary conditions does B have?

(c) How many conditions does B have which are not necessary but only contributory?

(d) Is there a feedback between A and B? Is this a simple feedback loop, or does it pass through other elements? Are A and B connected by several feedback loops, passing through different sets of other elements?

(e) What is the relative weight of the various influences on a given element A? Are the influences independent of each other, or are there pairs (or larger sets) for which the joint weight is not the sum of the individual weights?

Indeed, the meaning of "influence", "connection" or "contribution to" are themselves multidimensional concepts. When we say "A influences B", we may mean any of the following, or more:

(a) A (co-) determines the outcome of B
(b) A (co-) determines the time it takes to reach B
(c) A (co-) determines the path by which B is reached
(d) A (co-) determines the way B is formulated

Keeping with the multidimensional nature of this discussion, let me try to illuminate the subject by yet another image, depicted in Figure 1. In it the large circle represents observations to be "explained", while the rectangles stand for various assumptions made for the purpose of such explanation. If we work in the context of a one-dimensional model, we make one (simple or composite) assumption and then expect it to explain or fail to explain the set of observations. In a multidimensional model we can afford to be much more subtle about the matter, as shown in the figure. Each assumption may "explain' a certain subset of the observations, and these subsets can be partially overlapping (in part because of the connection between what we mean by "explain" and what we mean by "influencing"). If we then want to arrive at a complete explanation, we must ask a number of questions:

(1) What other assumptions are needed to cover the whole set of phenomena that were observed and that are to be explained.

(2) Once we have a complete set in the above sense, is our set free of selfcontradictions?

(3) What can we learn from the nature of the subdomain that is covered multiply by our assumptions?

(4) How does our success in explaining depend on the relative weights given to the various assumptions? If two assumptions are in conflict in their respective predictions for a given observation, can weights be attached consistently to cover all observations?

Multidimensionality also applies to the semantics underlying an analysis. Our language, partly intentionally and partly by negligence, is quite ambiguous, and thus the formulation of a question in ordinary language represents in reality a set of different questions, due to the multiple meaning of each word to the question. An example of this was just given in the analysis of the word "influences".

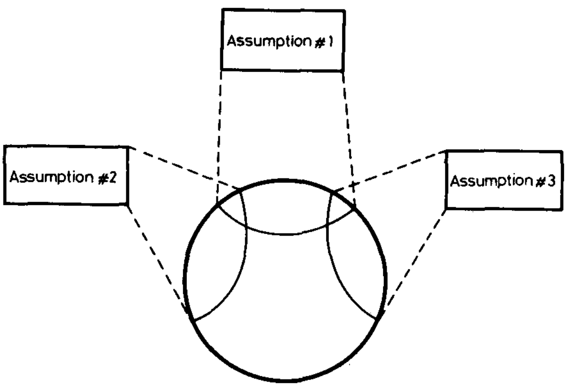


Fig. 1.

## IV. Illustrations

As indicated earlier, the discussion in the previous sections will now be illustrated on some examples taken from issues in the science of science. One way to illustrate multidimensionality is to demonstrate that the terminology used in the one-dimensional description of the problem is ambiguous, with various meanings corresponding to the various dimensions in the multidimensional formulation. This is what I want to do in the following examples.

1. "The relationship between science and technology is determined by the effect of technologically created instrumentation on new scientific investigations."[3]

In a one-dimensional framework this statement has a definite meaning, and to verify its validity it is sufficient to quote a few examples in which instrumentation played an obviously crucial role in bringing about scientific discoveries.

In a multidimensional framework the situation is very much more complicated and nebulous.

To begin with, by "effect on" do we mean determination of the content of the scientific discovery, or of the time duration it takes to arrive at a scientific discovery, or of the way in which such a scientific discovery is reached, or of the way the new scientific law is formulated? In the present context, for example, one might argue that whereas the content of a scientific discovery would have been the same even in the absence of a given kind of instrumentation, the result would have taken longer to reach.

Similarly, we might inquire whether the statement refers to the presence of instrumentation at all, or to a particular kind of instrumentation. Often, one might argue, a certain scientific discovery can be made in several ways, requiring different types of instrumentation, so that the existence of one particular kind is not crucial.

We then might inquire as to what the other ingredients to scientific discovery are. In a multidimensional framework we know that even if instrumentation does influence scientific discovery in one of the many meanings outlined above, it is at best only a necessary but certainly not a sufficient condition of it.

Once other factors are also found, the relationship of these various factors, their relative strengths and weights, and the topology of their connections need to be established.

The most likely outcome of all this investigation is the conclusion that in one or several of the possible meanings of the words, the availability of a certain kind of instrument assists in the creation of a scientific discovery, and that such discoveries, in turn, in some way assist the building of further instrumentation. Such a qualitative statement would certainly be less than surprizing. Quantifying it, that is, to assign a weight to this particular connection within the maze of connections is a more interesting but infinitely more difficult task.

(2) "The motivation of scientists for pursuing scientific research is in their desire for priority of discovery."[4]

In the multidimensional framework the following questions need to be explored before even the meaning of the statement is fully clarified:

(a) Is it meant that if a desire for priority were absent, Man would not do science?
(b) Is it meant that *some* scientists are attracted by the desire for priority?
(c) What are the other motivations scientists have for pursuing research?
(d) Is the desire for priority really a secondary sympton of a more basic motivation?
(e) How can we establish the presence or absence of motivations which are practically

never challanged and hence cannot serve as parameters in a survey comparing situations in the absence and in the presence of such a parameter?

(f) Is it meant that the type of research scientists engage in is influenced by the desire for priority?

(g) Is it meant that the method scientists use to pursue research is influenced by a desire for priority?

(h) Is it meant that scientists have stronger motivations in the presence of a desire for priority than in the absence of it?

(i) Is it meant that scientists work faster if a desire of priority is present?

(j) If there are many other motivations in scientists for pursuing research, what are the relative strengths of these others in relationship to the strength of the desire for priority.

(3) "Lotka's law for the distribution of scientific productivity is a consequence of stochastic laws governed by the reward system inherent in and internal to science itself and hence is independent of the context of science determined by external factors."[5]

This is an interesting example since the law itself is a scientometric one and hence is meant to be unambiguous, even if the reasons for the existence of this law are not necessarily scientometric.

In this case, the first set of ambiguities are with respect to Lotka's law itself. Does it pertain to the lifetime production of a group of contemporaneous scientists? Or does it pertain to a sample taken out of a scientific community at some given time, and measured over a finite time period? Much discussion on this and other aspects of the formulation of Lotka's law has appeared in the literature.[6]

But even beyond the formulation of the law itself, there are ambiguities in the formulation of the above statement. Are the stochastic laws referred to above simply an averaging over the influence of factors external to science? Is it meant that the law is to hold for any scientific community which obeys the above mentioned rules of the reward system, regardless of what the external conditions are? How much of a quantitative deviation is allowed from Lotka's law before the law "fails"? If other, external factors also play a role, what are they, and what are their strengths? Is the reward system itself influenced by external factors? Is a stochastic explanation an explanation at all, or is it a phenomenological tool to make partial progress even in the presence of sizeable gaps in our basic understanding of a problem?

(4) "Scientific theories are shaped by the contemporary social and political aspirations, perspectives, outlooks, points of view, and trends, collectively referred to as 'Zeitgeist'."[7]

Do we refer here to the content of scientific theories, to the method of reaching them, to the promptness with which they are formulated, or the form in which their content is expressed? For example, somebody might argue that while the content of the scientific theory is independent of the Zeitgeist because it pertains to the physical reality of nature, the way the new insight is expressed may depend on the "Zeitgeist", and so may the speed by which this insight was acquired, and the way in which it was arrived at.

Do we mean that unless the "right Zeitgeist" prevails, the discovery would not have been made at all? What are the other factors that contribute to the formation of a theory? Are perhaps both the Zeitgeist" and the new theory consequences of other factors? If there are indeed many contributing factors, what are they and what are their relative weights? Is it possible that new scientific insights affect the "Zeitgeist" rather than the reverse?

(5) "Science as we know it today is a product of 'Western' culture and thus countries in the Third World must strive to develop a new science which is their own and which is different."[8]

This statement is somewhat akin to the previous one, but is in many ways even more ambiguous. "Different science" is an ambiguous concept. Does it mean different results, a different method, a different sociology of scientists, a different problem choice, a different philosophical base? What is meant by "a product of Western culture", beside the obvious fact that historically modern science evolved in Western Europe? Had Western culture not existed, would we have no science at all, or less of it, or in a different form, or in different areas of knowledge? How can we tell when we see this "new" science that the Third World is to develop? If the science in the Third World shows some differences compared to science in the West, are the differences to be ascribed to the different cultural backgrounds, or are the differences only superficial and show the different stages of development of the same science? What other factors might contribute to a "new" science, and are these factors related to or connected with the differences between Western and other cultures?

The unifying element in all these examples is the emergence of ambiguities, ramifications, and novel types of questions in a multidimensional framework, while the analysis in a one-dimensional framework of the same examples appears unambiguous, straightforward, and simple. Indeed, the problem in question assumes an entirely different shape once projected into a multidimensional space. Viewing the same in one dimension may buy tractability and formal success, but only at the cost of giving up resemblance to reality.

(6) Scientific beliefs. An interesting specific illustration of this contrast between one-dimensional and many-dimensional frameworks was offered recently in a study of scientific beliefs.[9] The situation studied in this paper is a yet open research problem in science, when pieces of experimental information, yet unvertified by independent duplication, abound, and when various theoretical models are advanced without yet having the benefit of a substantial set of authenticated experimental data to separate the false guesses from those which turn out to be correct. Understandably enough, in

such a situation arguing for a particular point of view takes place in a framework of several dimensions, including reference to some data, esthetic considerations, the utilization of analogies with previous theories, etc. What is intriguing from our present point of view is that the authors very well exhibit the multidimensionality of the situation on a purely phenomenological level, but seem to be greatly hindered in their interpretation and analysis of the data because of their implicit one-dimensional thinking, something that affects their semantics as well as their categorization and concept forming.

## V. Conclusion

The foregoing discussion seems to suggest that I advocate a more complex, less simple type of explanation of phenomena in the science of science than what is often offered. Considering the traditional emphasis on simplicity, at least in the natural sciences, and the related stress on economy of thought, is this not a step in the wrong direction?

I do not think so, and for two reasons. First, we are not dealing here with traditional studies in the natural sciences, where it is a good approximation to constrain oneself to laboratory phenomena which were carefully reduced to the effect of one or two factors only. Social sciences always seem to deal with a system, a complex set of interacting elements, and, for such a system, the simple, linear, one-dimensional model is not a realistic approximation.

In fact, recent events suggest that it is not even any longer a matter of distinction between the natural and the social sciences. In the former itself, we are now entering a chapter in history when studies of tightly interacting systems become more prominent, and therefore a methodology of investigation suitable for such studies is in the process of development. In the area of nuclear and elementary particle physics, dominated by short range but very strong forces, the old type of approximation (mathematically expressed in terms of perturbation theory) clearly showed itself inadequate, although a functional replacement for it has yet to be invented. The same kind of movement toward the study of systems is evident in many areas of biology, as well as in economics. Whether the result of these new developments will eventually be "simple" is not clear, especially in view of the multidimensional meaning of "simple".

Another thought that might arise in the wake of the above discussion pertains to the increasingly popular multiple regression studies in scientometrics. Are they not a manifestation of multidimensional thinking?

Yes and no. Multiple regression analysis can further multidimensional thinking if, (a) the problem is amenable to the type of quantitative treatment multiple regression analysis represents, (b) if the various categories used in the analysis are indeed different dimensions of the problem, that is, they represent different circles in the non-linear set of elements pictured earlier, and not only various divisions along the same dimension. It is not an accident that in none of the five examples, chosen from among prominent problems in the science of science, can a multiple regression procedure be of much use, at least in the present stage of our knowledge and technique.

So what can we conclude from this discussion with respect to the future? One consequence I would like to stress is that we need more purely phenomenological studies of problems in the science of science. We have seen that the simple "make a hypothesis-find a correlation" method is hopelessly naive in a multi-dimensional situation. The type of simple hypothesis that is usually demanded in current research proposals will, at best, illuminate only a tiny fragment of a forest of crossing and interwoven lines of connections, and even if some correlations are established which appear to relate to this simplistic hypothesis, little is learned by the process.

Instead, we need purely phenomenological studies, in which a set of elements are investigated and their interconnections mapped without a preconceived expectation of what the causal connections may be, and what the "explanations" of the phenomena are. Some guess concerning interrelationships between elements is of course necessary even for the choice of the set of elements one wants to include in a study, but such *a priori* cerebration should be kept to a minimum. Only when a large body of such phenomenological information has been accumulated will we be in a position to begin to theorize about the topology of the interconnecting lines and their strengths.

As a last remark, I would like to add that eventually the outcome of methodological debates like the present one is decided by success or failure, measured mainly in terms of the ability to predict. At the present, the science of science, inspite of the many intriguing intimations it has produced, is, on the whole, unable to make any predictions and can supply, at best, only *a posteriori* "explanations" of observed phenomena. It is for this reason that people in the policy areas are at the moment less than enthusiastic about the science of science, and why there remains a very visible chasm between academic scholars of the science of science and the policy makers and managers of science. Whereas there are many other reasons also for this division, to me the inherent deficiency of the one-dimensional methodology predominantly used in the science of science is a significant cause of it. It seems that if we want to achieve predictive success in this field, we have to revamp our thinking in a basic way by turning to multidimensionality.

## References

[1] P. K. FEYERABEND, How to be a Good Empiricist. A Plea for Tolerance in Epistemalogical Matters, in: P. N. NIDDITCH (Ed.), *The Philosophy of Science,* University Press, Ely House, London, Oxford 1968, pp. 12–40.

[2] The relationship that may possibly exist between the ideas suggested in this paper and those in the writings of B. *Latour* and collaborators has not been investigated so far.

[3] For an example of this point of view, see D. de SOLLA PRICE, The Science/Technology Relationship, the Craft of Experimental Science, and Policy for the Improvement of High Technology Innovation, Final Report for the Division for Policy Research Analysis, National Science Foundation, March 1982.

[4] This point of view is quite rampant in the community of the sociologists of science. See for example R. K. MERTON, *The Sociology of Science,* The Univ. of Chicago Press, Chicago, Press, Chicago, 1972, p. 286.

[5] The main proponent of this view has been D. de SOLLA PRICE. See for example D. de SOLLA PRICE *Jasis* 27 (1976) 292.

[6] For a fairly recent bibliography, see J. VLACHY, *Scientometrics* 1 (1976) 109.

[7] A good example of this point of view can be found in P. FORMAN, *Weimar Culture, Causality, and Quantum Theory, 1918–1937: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment,* Historical Studies in the Physical Sciences III, University of Pennsylvania, Philadelphia, 1971, pp. 1–115.

[8] An interesting series of exchanges, some of which reflects this point of view, appeared in *Interciencia* 6 (1981), 3 167.

[9] G. N. GILBERT, M. MULKAY, *Soc. Stud. of Science,* 12 (1982) 383.