# PROGRESS and Its PROBLEMS

*Toward a Theory of Scientific Growth*

LARRY LAUDAN



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# *Prologue*

*We must explain why science—*
*our surest example of sound knowledge—*
*progresses as it does, and we first must find out how,*
*in fact, it does progress.* T. S. KUHN (1970), p. 20

Epistemology is an old subject; until about 1920, it was also a great one. What produced the change was a confluence of three quite separate developments, each of which effected a profound transformation in the study of knowledge. There was, first of all, the crisis produced by the realization that knowledge was neither as certain nor as incorrigible as thinkers since Plato and Aristotle had presumed it to be. There was, secondly, the increasing professional insularity of academic philosophers, and their related conviction that disciplines such as psychology and sociology, which had played a major role in earlier epistemological theories, had no interesting insights to offer. (This insularity was further promoted by the guileless duplicity of scholars in other fields, who were all too prepared to bequeath "the problem of knowledge" to the professional philosophers.) There was, finally and catastrophically, a growing tendency (especially in the English-speaking world) to imagine that one could grapple with the nature of knowledge while remaining blissfully ignorant of its best extant example—the natural sciences.

Despite the attempted appropriation of epistemological issues by the professional philosophers, many of the classical questions

about the nature of scientific knowledge still remain of broad, general interest: Does science progress? Are our ideas about nature really worthy of credence? Are some beliefs about the world more rational than others? Issues such as these go well beyond the bounds of specialized disciplinary monopolies. They do so in large part because most people in the West draw the bulk of their beliefs about nature, and even about themselves, from the corpus of science. Without Newton, Darwin, Freud, and Marx (to mention only the more obvious), our picture of the world would be vastly different from what it is. If science is a rationally well-founded system of inquiry, then it is only right and proper that we should emulate its methods, accept its conclusions, and adopt its presuppositions. If, however, science is predominately irrational, then there is no reason to take its knowledge claims any more (or less) seriously than we take those of the seer, the religious prophet, the guru, or the local fortuneteller.

For a long time, many have taken the rationality and progressiveness of science as an obvious fact or a foregone conclusion, and some readers will probably still think it bizarre to believe that there is any important problem to be solved here. Although this confident attitude has been almost inescapable given the cultural biases in favor of science in modern culture, there have been a number of recent developments which bring it into serious question:

1. Philosophers of science, whose primary aim is to define what rationality is, have generally found that their models of rationality find few, if any, exemplifications in the actual process of scientific activity.[1] If we accept the claim made on behalf of these models to the effect that they define rationality itself, then we seem forced to view virtually all science as irrational.

2. Attempts to show that the methods of science guarantee it is true, probable, progressive, or highly confirmed knowledge—attempts which have an almost continuous ancestry from Aristotle to our own time—have generally failed,[2] raising a distinct presumption that scientific theories are neither true, nor probable, nor progressive, nor highly confirmed.



3. Sociologists of science have been able to point to several episodes in the recent (and distant) past of science which *seem* to reveal many nonrational, or irrational, factors decisively involved in scientific decision making.[3]

4. Some historians and philosophers of science (e.g., Kuhn and Feyerabend) have argued, not merely that certain decisions between theories in science *have been irrational,* but that choices between competing scientific theories, in the nature of the case, *must be irrational.*[4] They (especially Kuhn) have also suggested that every gain in our knowledge is accompanied by attendant losses, so that it is impossible to ascertain when, or even whether, we are progressing.[5]

The skepticism to which such conclusions point has been reinforced by the general arguments of cultural relativism to the effect that science is just one set of beliefs among many possible ones, and that we in the West venerate science, not because it is more rational than its alternatives, but simply because we are a product of a culture that has traditionally set great store by science. All systems of belief, including science, are seen as dogmas and ideologies, between which objective, rational preference is impossible.

Confronted by the acknowledged failure of the traditional analysis to shed much light on the rationality of knowledge, three alternatives seem to be open to us:

1. We might continue to hope that some as yet undiscovered minor variation in the traditional analysis will eventually clarify and justify our intuitions about the cognitive well-foundedness of science and thus prove to be a worthy model of rationality.

2. We might, alternatively, abandon the search for an adequate model of rationality as a lost cause, thereby accepting the thesis that science is, so far as we know, blatantly irrational.

3. Finally, we might begin afresh to analyze the rationality of science, deliberately trying to avoid some of the key presuppositions which have produced the breakdown of the traditional analysis.

Enormous efforts have been devoted, particularly in the last decade, to the pursuit of strategies (1) and (2). Philosophers of

science, by and large, have taken the first option. Thus, Lakatos asks, "What are the *minimum* changes needed in the Popperian analysis of science to enable it to solve the problem of rationality?"[6] Salmon asks, "What are the *minimum* adjustments needed in Reichenbach's theory to square it with scientific practice?" Hintikka poses the question, "What kind of *tinkering* with Carnap's inductive logic will make it relevant to scientific testing?" While one admires the tenacity and ingenuity illustrated by proponents of this approach, the results are not, on the whole, very encouraging. Most of the difficulties which stood in the way of a Popper, a Carnap, or a Reichenbach still remain obstacles for their latter-day disciples.[7]

The second option has proved more popular with historically oriented thinkers. Thus, both Kuhn and Feyerabend conclude that scientific decision making is basically a political and propagandistic affair, in which prestige, power, age, and polemic decisively determine the outcome of the struggle between competing theories and theorists. Their mistake seems to be one of jumping to a premature conclusion. They start from the premise that rationality is exhaustively defined by a certain model of rationality (each of them takes Popper's model of falsifiability as the archetype). Having observed, quite correctly, that the Popperian model of rationality will do scant justice to actual science, they precipitately conclude that science must have large irrational elements, without stopping to consider whether some richer and more subtle model of rationality might do the job.

Because the one option seems unpromising and the other premature, I am inclined to think that we should consider pursuing the third strategy. Let us drop some of the traditional language and concepts (degree of confirmation, explanatory content, corroboration and the like), and see if a potentially more adequate model of scientific rationality begins to emerge. Let us see whether, by asking anew some of the elementary questions about science, we cannot get a slightly different perspective on scientific knowledge.

In what follows, I shall attempt to trace out the consequences of the view that science fundamentally aims at the solution of



problems. Although the view itself is commonplace, very little attention has been given to exploring it in detail. What the different types of problems are, what makes one problem more important than another, the criteria for counting something as an adequate solution, the relation of nonscientific problems to scientific ones; none of these issues have been addressed in the detail they demand. To anticipate some of my conclusions, I propose that the rationality and progressiveness of a theory are most closely linked—not with its confirmation or its falsification—but rather with its *problem solving effectiveness.* I shall be arguing that there are important *nonempirical,* even *"nonscientific"* (in the usual sense), factors which have—and which should have—played a role in the *rational* development of science. I shall suggest, further, that most philosophers of science have mistakenly identified the nature of scientific appraisal, and thereby the primary unit of rational analysis, by focussing on the individual theory, rather than on what I call the *research tradition.* This study will show, moreover, that we need to distinguish between the *rationality of acceptance* and *the rationality of pursuit* if we are to make any progress at reconstructing the cognitive dimensions of scientific activity.

My basic strategy in what follows will involve the blurring, and perhaps the obliteration, of the classical distinction between scientific *progress* and scientific *rationality.* These two notions, both central to any discussion of science, have often seemed at cross purposes. Progress is an unavoidably *temporal* concept; to speak about scientific progress necessarily involves the idea of a process occurring through time. Rationality, on the other hand, has tended to be viewed as an atemporal concept; it has been claimed that we can determine whether a statement or theory is rationally credible independently of any knowledge of its historical career. Insofar as rationality and progressiveness have been linked at all, the former has taken priority over the latter—to such a degree that most writers see progress as *nothing more than* the temporal projection of a series of individual rational choices. To be progressive, on the usual view, is to adhere to a series of increasingly rational beliefs. I am deeply troubled by the unanimity with which philosophers

have made progress *parasitic* upon rationality. In part, my worry arises from a concern that it involves explaining something which can be readily understood (progress) in terms of something else (rationality) which may be far more obscure. More serious, however, is the absence of any convincing argument as to why we should explicate our concept of progress in terms of rationality. The two concepts are doubtless related, but not necessarily in the manner usually supposed.

It will be the assumption here that we may be able to learn something by inverting the presumed dependency of progress on rationality. I shall try to show that we have a clearer model for scientific progress than we do for scientific rationality; that, moreover, we can define rational acceptance in terms of scientific progress. In a phrase, my proposal will be that *rationality consists in making the most progressive theory choices,* not that progress consists in accepting successively the most rational theories. This inversion of the usual hierarchy offers some insights into the nature of science which tend to elude us if we preserve the traditional relation between progress and rationality.

Another of the chief obstacles to the development of a theory of scientific progress has been the universal assumption that progress can occur only if it is *cumulative,* that is, if knowledge grows entirely by accretion. Because there are grave difficulties, both historically and conceptually, with the progress-by-accretion view, I propose a definition of scientific progress which does not demand cumulative development.

In order for the ambitions of this enterprise to be brought to fruition, and to prevent its being misconstrued, two key points must be stressed. First, the term "progress" has many *emotive* overtones deeply rooted in the subjective intuitions of both friends and critics of science. The object of this work is not to exploit that emotiveness, but rather to offer objective criteria for determining when progress has occurred. In too many discussions of progress, insufficient attention has been given to separating out the question of what progress is from the question of its moral and cognitive desirability. Any adequate theory of progress must make such a distinction as sharply as possible. There is a second crucial ambiguity in normal usages



of "progress" which must also be noted. Specifically, it is commonplace to speak of progress, meaning an improvement in the material or the "spiritual" conditions of life. Although that sense of progress is unquestionably important, I shall say virtually nothing about it in this essay. My exclusive preoccupation will be with what I call *"cognitive progress,"* which is nothing more nor less than *progress with respect to the intellectual aspirations of science.* Cognitive progress neither entails, nor is it entailed by, material, social, or spiritual progress. These notions are surely not altogether disconnected, but they do refer to very different processes, and, at least for purposes of the present discussion, should be sharply distinguished.

One final point is in order. Previously, too many discussions of scientific rationality and progress have been both uninformed by, and inapplicable to, the actual course of the evolution of science. The various well-known philosophical models of rationality have been shown to be inapplicable to most of those cases in the history of science where, at least intuitively, we are convinced that sensible, rational choices were being made. Without assuming that whatever science does is, by definition, rational, we should nonetheless be able to demand of any model of science that it substantially "fit" the actual course of scientific change. Accordingly, historical cases and episodes will be used extensively in this essay; these are intended not merely to *illustrate* my philosophical claims, but also to *test* them. If the model under discussion here fails to illustrate the manner in which scientific desicion making has actually worked (at least some of the time), then it will have failed entirely in its ambitions.

Because of the unusually heavy weight attached in this approach to historical material—material which some philosophers deem to be absolutely irrelevant to epistemology—I shall also briefly discuss the general question of the bearing of descriptive data (such as history) on a normative theory (such as a model of scientific rationality).

Part One of the following study articulates a model of scientific progress and rationality, and exhibits how that model, for all its evident incompleteness, avoids many of the paradoxes

which previous models have generated, and makes some sense of the historical data. Part Two examines the ramifications of that model for a variety of intellectual inquiries, ranging from the history of ideas to the history and philosophy of science and the sociology of knowledge.

It has not been possible for me to explore all the issues concerned with scientific progress in the detail which they deserve. For that failure, I can only ask the reader's mercy. This is not, nor is it intended to be, a finished piece of work. At many points, argument sketches pass for arguments and plausible intuitions are invoked where, ideally, explicit doctrines are called for. A great deal remains to be said on all the matters I address. But the study of rational knowledge and its growth, like knowledge itself, is a cooperative venture of a community of minds. My purpose is merely to offer a fresh perspective on some problems which have preoccupied reflective people for a very long time.

*Part One*

# A Model of Scientific Progress

*The activity of understanding*
*is, essentially, the same as*
*that of all problem solving.* K. POPPER (1972), p. 166