Representing and intervening

Introductory topics in the philosophy of natural science

Ian Hacking

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For Rachel

'Reality . . . what a concept' – S.V.

Acknowledgements

What follows was written while Nancy Cartwright, of the Stanford University Philosophy Department, was working out the ideas for her book, *How the Laws of Physics Lie.* There are several parallels between her book and mine. Both play down the truthfulness of theories but favour some theoretical entities. She urges that only phenomenological laws of physics get at the truth, while in Part B, below, I emphasize that experimental science has a life more independent of theorizing than is usually allowed. I owe a good deal to her discussion of these topics. We have different anti-theoretical starting points, for she considers models and approximations while I emphasize experiment, but we converge on similar philosophies.

My interest in experiment was engaged in conversation with Francis Everitt of the Hanson Physical Laboratory, Stanford. We jointly wrote a very long paper, 'Which comes first, theory or experiment?' In the course of that collaboration I learned an immense amount from a gifted experimenter with wide historical interests. (Everitt directs the gyro project which will soon test the general theory of relativity by studying a gyroscope in a satellite. He is also the author of *James Clerk Maxwell*, and numerous essays in the *Dictionary of Scientific Biography*.) Debts to Everitt are especially evident in Chapter 9. Sections which are primarily due to Everitt are marked (E). I also thank him for reading the finished text with much deliberation.

Richard Skaer, of Peterhouse, Cambridge, introduced me to microscopes while he was doing research in the Haematological Laboratory, Cambridge University, and hence paved the way to Chapter 11. Melissa Franklin of the Stanford Linear Accelerator taught me about PEGGY II and so provided the core material for Chapter 16. Finally I thank the publisher's reader, Mary Hesse, for many thoughtful suggestions.

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(1982). Parts of Chapters 10, 12 and 13 are adapted from *Versuchungen: Aufsätze zur Philosophie Paul Feyerabends* (ed. Peter Duerr), Suhrkamp: Frankfurt, 1981, Bd. 2, pp. 126–58. Chapter 9 draws on my joint paper with Everitt, and Chapter 8 develops my review of Lakatos, *British Journal for the Philosophy of Science* 30 (1979), pp. 381–410. The book began in the middle, which I have called a 'break'. That was a talk with which I was asked to open the April, 1979, Stanford–Berkeley Student Philosophy conference. It still shows signs of having been written in Delphi a couple of weeks earlier.

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Analytical table of contents

Introduction: Rationality 1

Rationality and realism are the two main topics of today's philosophers of science. That is, there are questions about reason, evidence and method, and there are questions about what the world is, what is in it, and what is true of it. This book is about reality, not reason. The introduction is about what this book is *not* about. For background it surveys some problems about reasons that arose from Thomas Kuhn's classic, *The Structure of Scientific Revolutions*.

PART A: REPRESENTING

1 What is scientific realism? 21

Realism about theories says they aim at the truth, and sometimes get close to it. Realism about entities says that the objects mentioned in theories should really exist. Anti-realism about theories says that our theories are not to be believed literally, and are at best useful, applicable, and good at predicting. Anti-realism about entities says that the entities postulated by theories are at best useful intellectual fictions.

2 Building and causing 32

J.J.C. Smart and other materialists say that theoretical entities exist if they are among the building blocks of the universe. N. Cartwright asserts the existence of those entities whose causal properties are well known. Neither of these realists about entities need be a realist about theories.

3 Positivism 41

Positivists such as A. Comte, E. Mach and B. van Fraassen are anti-realists about both theories and entities. Only propositions whose truth can be established by observation are to be believed. Positivists are dubious about such concepts as causation and explanation. They hold that theories are instruments for predicting phenomena, and for organizing our thoughts. A criticism of 'inference to the best explanation' is developed.

4 Pragmatism 58

C.S. Peirce said that something is real if a community of inquirers will end up agreeing that it exists. He thought that truth is what scientific method finally settles upon, if only investigation continues long enough. W. James and J. Dewey place less emphasis on the long run, and more on what it feels comfortable to believe and talk about now. Of recent philosophers, H. Putnam goes along with Peirce while R. Rorty favours James and Dewey. These are two different kinds of anti-realism.

5 Incommensurability 65

T.S. Kuhn and P. Feyerabend once said that competing theories cannot be well compared to see which fits the facts best. This idea strongly reinforces one kind of anti-realism. There are at least three ideas here. Topic-incommensurability: rival theories may only partially overlap, so one cannot well compare their successes overall. Dissociation: after sufficient time and theory change, one world view may be almost unintelligible to a later epoch. Meaning-incommensurability: some ideas about language imply that rival theories are always mutually incomprehensible and never inter-translatable, so that reasonable comparison of theories is in principle impossible.

6 Reference 75

H. Putnam has an account of the meaning of 'meaning' which avoids meaning-incommensurability. Successes and failures of this idea are illustrated by short histories of the reference of terms such as: glyptodon, electron, acid, caloric, muon, meson.

7 Internal realism 92

Putnam's account of meaning started from a kind of realism but has become increasingly pragmatic and anti-realist. These shifts are described and compared to Kant's philosophy. Both Putnam and Kuhn come close to what is best called transcendental nominalism.

8 A surrogate for truth 112

I. Lakatos had a methodology of scientific research programmes intended as an antidote to Kuhn. It looks like an account of rationality, but is rather an explanation of how scientific objectivity need not depend on a correspondence theory of truth.

BREAK: Reals and representations 130

This chapter is an anthropological fantasy about ideas of reality and representation from cave-dwellers to H. Hertz. It is a parable to show why the realism/anti-realism debates at the level of representation are always inconclusive. Hence we turn from truth and representation to experimentation and manipulation.

PART B: INTERVENING

9 Experiment 149

Theory and experiment have different relationships in different sciences at different stages of development. There is no right answer to the question: Which comes first, experiment, theory, invention, technology, . . .? Illustrations are drawn from optics, thermodynamics, solid state physics, and radioastronomy.

10 Observation 167

N.R. Hanson suggested that all observation statements are theory-loaded. In fact observation is not a matter of language, and it is a skill. Some observations are entirely pre-theoretical. Work by C. Herschel in astronomy and by W. Herschel in radiant heat is used to illustrate platitudes about observation. Far from being unaided vision, we often speak of observing when we do not literally 'see' but use information transmitted by theoretically postulated objects.

11 Microscopes 186

Do we see with a microscope? There are many kinds of light microscope, relying on different properties of light. We believe what we see largely because quite different physical systems provide the same picture. We even 'see' with an acoustic microscope that uses sound rather than light.

12 Speculation, calculation, models, approximations 210
There is not one activity, theorizing. There are many kinds and levels of theory, which bear different relationships to experiment. The history of experiment and theory of the magneto-optical effect illustrates this fact. N. Cartwright's ideas about models and approximations further illustrate the varieties of theory.

13 The creation of phenomena 220
Many experiments create phenomena that did not hitherto exist in a pure state in the universe. Talk of repeating experiments is misleading. Experiments are not repeated but improved until phenomena can be elicited regularly. Some electromagnetic effects illustrate this creation of phenomena.

14 Measurement 233
Measurement has many different roles in sciences. There are measurements to test theories, but there are also pure determinations of the constants of nature. T.S. Kuhn also has an important account of an unexpected functional role of measurement in the growth of knowledge.

15 Baconian topics 246
F. Bacon wrote the first taxonomy of kinds of experiments. He predicted that science would be the collaboration of two different skills – rational and experimental. He thereby answered P. Feyerabend's question, 'What's so great about science?' Bacon has a good account of crucial experiments, in which it is plain that they are not decisive. An example from chemistry shows that in practice we cannot in general go on introducing auxiliary hypotheses to save theories refuted by crucial experiments. I. Lakatos's misreports of the Michelson–Morley experiment are used to illustrate the way theory can warp the philosophy of experiment.

16 Experimentation and scientific realism 262
Experimentation has a life of its own, interacting with speculation, calculation, model building, invention and technology in numerous ways. But whereas the speculator, the calculator, and the model-builder can be anti-realist, the experimenter must be a realist. This

thesis is illustrated by a detailed account of a device that produces concentrated beams of polarized electrons, used to demonstrate violations of parity in weak neutral current interactions. Electrons become tools whose reality is taken for granted. It is not thinking about the world but changing it that in the end must make us scientific realists.

Preface

This book is in two parts. You might like to start with the second half, *Intervening*. It is about experiments. They have been neglected for too long by philosophers of science, so writing about them has to be novel. Philosophers usually think about theories. *Representing* is about theories, and hence it is a partial account of work already in the field. The later chapters of Part A may mostly interest philosophers while some of Part B will be more to a scientific taste. Pick and choose: the analytical table of contents tells what is in each chapter. The arrangement of the chapters is deliberate, but you need not begin by reading them in my order.

I call them introductory topics. They are, for me, literally that. They were the topics of my annual introductory course in the philosophy of science at Stanford University. By 'introductory' I do not mean simplified. Introductory topics should be clear enough and serious enough to engage a mind to whom they are new, and also abrasive enough to strike sparks off those who have been thinking about these things for years.

Introduction: rationality

> You ask me, which of the philosophers' traits are idiosyncrasies?
> For example: their lack of historical sense, their hatred of becoming, their Egypticism.
> They think that they show their *respect* for a subject when they dehistoricize it – when they turn it into a mummy.
>
> (F. Nietzsche, *The Twilight of the Idols*, 'Reason in Philosophy', Chapter 1)

Philosophers long made a mummy of science. When they finally unwrapped the cadaver and saw the remnants of an historical process of becoming and discovering, they created for themselves a crisis of rationality. That happened around 1960.

It was a crisis because it upset our old tradition of thinking that scientific knowledge is the crowning achievement of human reason. Sceptics have always challenged the complacent panorama of cumulative and accumulating human knowledge, but now they took ammunition from the details of history. After looking at many of the sordid incidents in past scientific research, some philosophers began to worry whether reason has much of a role in intellectual confrontation. Is it reason that settles which theory is getting at the truth, or what research to pursue? It became less than clear that reason *ought* to determine such decisions. A few people, perhaps those who already held that morality is culture-bound and relative, suggested that 'scientific truth' is a social product with no claim to absolute validity or even relevance.

Ever since this crisis of confidence, rationality has been one of the two issues to obsess philosophers of science. We ask: What do we really know? What should we believe? What is evidence? What are good reasons? Is science as rational as people used to think? Is all this talk of reason only a smokescreen for technocrats? Such questions about ratiocination and belief are traditionally called logic and epistemology. They are *not* what this book is about.

Scientific realism is the other major issue. We ask: What is the world? What kinds of things are in it? What is true of them? What is truth? Are the entities postulated by theoretical physics real, or only constructs of the human mind for organizing our experiments? These are questions about reality. They are metaphysical. In this book I choose them to organize my introductory topics in the philosophy of science.

Disputes about both reason and reality have long polarized philosophers of science. The arguments are up-to-the-minute, for most philosophical debate about natural science now swirls around one or the other or both. But neither is novel. You will find them in Ancient Greece where philosophizing about science began. I've chosen realism, but rationality would have done as well. The two are intertwined. To fix on one is not to exclude the other.

Is either kind of question important? I doubt it. We do want to know what is really real and what is truly rational. Yet you will find that I dismiss most questions about rationality and am a realist on only the most pragmatic of grounds. This attitude does not diminish my respect for the depths of our need for reason and reality, nor the value of either idea as a place from which to start.

I shall be talking about what's real, but before going on, we should try to see how a 'crisis of rationality' arose in recent philosophy of science. This could be 'the history of an error'. It is the story of how slightly off-key inferences were drawn from work of the first rank.

Qualms about reason affect many currents in contemporary life, but so far as concerns the philosophy of science, they began in earnest with a famous sentence published twenty years ago:

> History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed.

Decisive transformation – anecdote or chronology – image of science – possessed – those are the opening words of the famous book by Thomas Kuhn, *The Structure of Scientific Revolutions*. The book itself produced a decisive transformation and unintentionally inspired a crisis of rationality.

A divided image

How could history produce a crisis? In part because of the previous image of mummified science. At first it looks as if there was not exactly one image. Let us take a couple of leading philosophers for illustration. Rudolf Carnap and Karl Popper both began their careers in Vienna and fled in the 1930s. Carnap, in Chicago and Los Angeles, and Popper, in London, set the stage for many later debates.

They disagreed about much, but only because they agreed on basics. They thought that the natural sciences are terrific and that physics is the best. It exemplifies human rationality. It would be nice to have a criterion to distinguish such good science from bad nonsense or ill-formed speculation.

Here comes the first disagreement: Carnap thought it is important to make the distinction in terms of language, while Popper thought that the study of meanings is irrelevant to the understanding of science. Carnap said scientific discourse is meaningful; metaphysical talk is not. Meaningful propositions must be *verifiable* in principle, or else they tell nothing about the world. Popper thought that verification was wrong-headed, because powerful scientific theories can never be verified. Their scope is too broad for that. They can, however, be tested, and possibly shown to be false. A proposition is scientific if it is *falsifiable*. In Popper's opinion it is not all that bad to be pre-scientifically metaphysical, for unfalsifiable metaphysics is often the speculative parent of falsifiable science.

The difference here betrays a deeper one. Carnap's verification is from the bottom up: make observations and see how they add up to confirm or verify a more general statement. Popper's falsification is from the top down. First form a theoretical conjecture, and then deduce consequences and test to see if they are true.

Carnap writes in a tradition that has been common since the seventeenth century, a tradition that speaks of the 'inductive sciences'. Originally that meant that the investigator should make precise observations, conduct experiments with care, and honestly record results; then make generalizations and draw analogies and gradually work up to hypotheses and theories, all the time developing new concepts to make sense of and organize the facts. If the theories stand up to subsequent testing, then we know something about the world. We may even be led to the underlying laws of nature. Carnap's philosophy is a twentieth-century version of this attitude. He thought of our observations as the foundations for our knowledge, and he spent his later years trying to invent an inductive logic that would explain how observational evidence could support hypotheses of wide application.

There is an earlier tradition. The old rationalist Plato admired geometry and thought less well of the high quality metallurgy, medicine or astronomy of his day. This respect for deduction became enshrined in Aristotle's teaching that real knowledge – science – is a matter of deriving consequences from first principles by means of demonstrations. Popper properly abhors the idea of first principles but he is often called a deductivist. This is because he thinks there is only one logic – deductive logic. Popper agreed with David Hume, who, in 1739, urged that we have at most a psychological propensity to generalize from experience. That gives no reason or basis for our inductive generalizations, no more than a young man's propensity to disbelieve his father is a reason for trusting the youngster rather than the old man. According to Popper, the rationality of science has nothing to do with how well our evidence 'supports' our hypotheses. Rationality is a matter of method; that method is conjecture and refutation. Form far-reaching guesses about the world, deduce some observable consequences from them. Test to see if these are true. If so, conduct other tests. If not, revise the conjecture or better, invent a new one.

According to Popper, we may say that an hypothesis that has passed many tests is 'corroborated'. But this does not mean that it is well supported by the evidence we have acquired. It means only that this hypothesis has stayed afloat in the choppy seas of critical testing. Carnap, on the other hand, tried to produce a theory of confirmation, analysing the way in which evidence makes hypotheses more probable. Popperians jeer at Carnapians because they have provided no viable theory of confirmation. Carnapians in revenge say that Popper's talk of corroboration is either empty or is a concealed way of discussing confirmation.

Battlefields

Carnap thought that *meanings* and a theory of *language* matter to the philosophy of science. Popper despised them as scholastic. Carnap favoured *verification* to distinguish science from non-science. Popper urged *falsification*. Carnap tried to explicate good reason in terms of a theory of *confirmation*; Popper held that rationality consists in *method*. Carnap thought that knowledge has *foundations*; Popper urged that there are no foundations and that all our knowledge is *fallible*. Carnap believed in *induction*; Popper held that there is no logic except *deduction*.

All this makes it look as if there were no standard 'image' of science in the decade before Kuhn wrote. On the contrary: whenever we find two philosophers who line up exactly opposite on a series of half a dozen points, we know that in fact they agree about almost everything. They share an image of science, an image rejected by Kuhn. If two people genuinely disagreed about great issues, they would not find enough common ground to dispute specifics one by one.

Common ground

Popper and Carnap assume that natural science is our best example of rational thought. Now let us add some more shared beliefs. What they do with these beliefs differs; the point is that they are shared.

Both think there is a pretty sharp distinction between *observation* and *theory*. Both think that the growth of knowledge is by and large *cumulative*. Popper may be on the lookout for refutations, but he thinks of science as evolutionary and as tending towards the one true theory of the universe. Both think that science has a pretty tight *deductive structure*. Both held that scientific terminology is or ought to be rather *precise*. Both believed in the *unity of science*. That means several things. All the sciences should employ the same methods, so that the human sciences have the same methodology as physics. Moreover, at least the natural sciences are part of one science, and we expect that biology reduces to chemistry, as chemistry reduces to physics. Popper came to think that at least part of psychology and the social world did not strictly reduce to the physical world, but Carnap had no such qualms. He was a founder of a series of volumes under the general title, *The Encyclopedia of Unified Science*.

Both agreed that there is a fundamental difference between the *context of justification* and the *context of discovery*. The terms are due to Hans Reichenbach, a third distinguished philosophical emigré of that generation. In the case of a discovery, historians, economists, sociologists, or psychologists will ask a battery of questions: Who made the discovery? When? Was it a lucky guess, an idea filched from a rival, or the pay-off for 20 years of ceaseless toil? Who paid for the research? What religious or social milieu helped or hindered this development? Those are all questions about the context of *discovery*.

Now consider the intellectual end-product: an hypothesis, theory, or belief. Is it reasonable, supported by the evidence, confirmed by experiment, corroborated by stringent testing? These are questions about *justification* or soundness. Philosophers care about justification, logic, reason, soundness, methodology. The historical circumstances of discovery, the psychological quirks, the social interactions, the economic milieux are no professional concern of Popper or Carnap. They use history only for purposes of chronology or anecdotal illustration, just as Kuhn said. Since Popper's account of science is more dynamic and dialectical, it is more congenial to the historicist Kuhn than the flat formalities of Carnap's work on confirmation, but in an essential way, the philosophies of Carnap and Popper are timeless: outside time, outside history.

Blurring an image

Before explaining why Kuhn dissents from his predecessors, we can easily generate a list of contrasts simply by running across the Popper/Carnap common ground and denying everything. Kuhn holds:

There is no sharp distinction between observation and theory.

Science is not cumulative.

A live science does not have a tight deductive structure.

Living scientific concepts are not particularly precise.

Methodological unity of science is false: there are lots of disconnected tools used for various kinds of inquiry.

The sciences themselves are disunified. They are composed of a large number of only loosely overlapping little disciplines many of which in the course of time cannot even comprehend each other. (Ironically Kuhn's best-seller appeared in the moribund series, *The Encyclopedia of Unified Science*.)

The context of justification cannot be separated from the context of discovery.

Science is in time, and is essentially historical.

Is reason in question?

I have so far ignored the first point on which Popper and Carnap agree, namely that natural science is the paragon of rationality, the gemstone of human reason. Did Kuhn think that science is irrational? Not exactly. That is not to say he took it to be 'rational' either. I doubt that he had much interest in the question.

We now must run through some main Kuhnian themes, both to understand the above list of denials, and to see how it all bears on rationality. Do not expect him to be quite as alien to his predecessors as might be suggested. Point-by-point opposition between philosophers indicates underlying agreement on basics, and in some respects Kuhn is point-by-point opposed to Carnap-Popper.

Normal science

Kuhn's most famous word was *paradigm*, of which more anon. First we should think about Kuhn's tidy structure of revolution: *normal science, crisis, revolution, new normal science.*

The normal science thesis says that an established branch of science is mostly engaged in relatively minor tinkering with current theory. Normal science is *puzzle-solving*. Almost any well-worked-out theory about anything will somewhere fail to mesh with facts about the world – 'Every theory is born refuted'. Such failures in an otherwise attractive and useful theory are *anomalies*. One hopes that by rather minor modifications the theory may be mended so as to explain and remove these small counterexamples. Some normal science occupies itself with mathematical articulation of theory, so that the theory becomes more intelligible, its consequences more apparent, and its mesh with natural phenomena more intricate. Much normal science is technological application. Some normal science is the experimental elaboration and clarification of facts implied in the theory. Some normal science is refined measurement of quantities that the theory says are important. Often the aim is simply to get a precise number by ingenious means. This is done neither to test nor confirm the theory. Normal science, sad to say, is not in the confirmation, verification, falsification or conjecture-and-refutation business at all. It does, on the other hand, constructively accumulate a body of knowledge and concepts in some domain.

Crisis and revolution

Sometimes anomalies do not go away. They pile up. A few may come to seem especially pressing. They focus the energies of the livelier members of the research community. Yet the more people work on the failures of the theory, the worse things get. Counter-examples accumulate. An entire theoretical perspective becomes clouded. The discipline is in *crisis*. One possible outcome is an entirely new approach, employing novel concepts. The problematic phenomena are all of a sudden intelligible in the light of these new ideas. Many workers, perhaps most often the younger ones, are converted to the new hypotheses, even though there may be a few hold-outs who may not even understand the radical changes going on in their field. As the new theory makes rapid progress, the older ideas are put aside. A *revolution* has occurred.

The new theory, like any other, is born refuted. A new generation of workers gets down to the anomalies. There is a new normal science. Off we go again, puzzle-solving, making applications, articulating mathematics, elaborating experimental phenomena, measuring.

The new normal science may have interests quite different from the body of knowledge that it displaced. Take the least contentious example, namely measurement. The new normal science may single out different things to measure, and be indifferent to the precise measurements of its predecessor. In the nineteenth century analytical chemists worked hard to determine atomic weights. Every element was measured to at least three places of decimals. Then around 1920 new physics made it clear that naturally occurring elements are mixtures of isotopes. In many practical affairs it is still useful to know that earthly chlorine has atomic weight 35.453. But this is a largely fortuitous fact about our planet. The deep fact is that chlorine has two stable isotopes, 35 and 37. (Those are not the exact numbers, because of a further factor called binding energy.) These isotopes are mixed here on earth in the ratios 75.53% and 24.47%.

'Revolution' is not novel

The thought of a scientific revolution is not Kuhn's. We have long had with us the idea of the Copernican revolution, or of the 'scientific revolution' that transformed intellectual life in the seventeenth century. In the second edition of his *Critique of Pure Reason* (1787), Kant speaks of the 'intellectual revolution' by which Thales or some other ancient transformed empirical mathematics into demonstrative proof. Indeed the idea of revolution in the scientific sphere is almost coeval with that of political revolution. Both became entrenched with the French Revolution (1789) and the revolution in chemistry (1785, say). That was not the beginning, of course. The English had had their 'glorious revolution' (a bloodless one) in 1688 just as it became realized that a scientific revolution was also occurring in the minds of men and women.[1]

Under the guidance of Lavoisier the phlogiston theory of combustion was replaced by the theory of oxidation. Around this time there was, as Kuhn has emphasized, a total transformation in many chemical concepts, such as mixture, compound, element, substance and the like. To understand Kuhn properly we should not fixate on grand revolutions like that. It is better to think of smaller revolutions in chemistry. Lavoisier taught that oxygen is the principle of acidity, that is, that every acid is a compound of oxygen. One of the most powerful of acids (then or now) was called muriatic acid. In 1774 it was shown how to liberate a gas from this. The gas was called dephlogisticated muriatic acid. After 1785 this very gas was inevitably renamed oxygenized muriatic acid. By 1811 Humphry Davy showed this gas is an element, namely chlorine. Muriatic acid is our hydrochloric acid, HCl. It contains no oxygen. The Lavoisier conception of acidity was thereby overthrown. This event was, in its day, quite rightly called a revolution. It even had the Kuhnian feature that there were hold-outs from the old school. The greatest analytical chemist of Europe, J.J. Berzelius (1779–1848), never publicly acknowledged that chlorine was an element, and not a compound of oxygen.

The idea of scientific revolution does not in itself call in question scientific rationality. We have had the idea of revolution for a long time, yet still been good rationalists. But Kuhn invites the idea that every normal science has the seeds of its own destruction. Here is an idea of perpetual revolution. Even that need not be irrational. Could Kuhn's idea of a revolution as switching 'paradigms' be the challenge to rationality?

1 I.B. Cohen, 'The eighteenth century origins of the concept of scientific revolution', *Journal for the History of Ideas* 37 (1976), pp. 257–88.

Paradigm-as-achievement

'Paradigm' has been a vogue word of the past twenty years, all thanks to Kuhn. It is a perfectly good old word, imported directly from Greek into English 500 years ago. It means a pattern, exemplar, or model. The word had a technical usage. When you learn a foreign language by rote you learn for example how to conjugate *amare* (to love) as *amo*, *amas*, *amat* . . ., and then conjugate verbs of this class following this model, called the paradigm. A saint, on whom we might pattern our lives, was also called a paradigm. This is the word that Kuhn rescued from obscurity.

It has been said that in *Structure* Kuhn used the word 'paradigm' in 22 different ways. He later focussed on two meanings. One is the paradigm-as-achievement. At the time of a revolution there is usually some exemplary success in solving an old problem in a completely new way, using new concepts. This success serves as a model for the next generation of workers, who try to tackle other problems in the same way. There is an element of rote here, as in the conjugation of Latin verbs ending in *-are*. There is also a more liberal element of modelling, as when one takes one's favourite saint for one's paradigm, or role-model. The paradigm-as-achievement is the role-model of a normal science.

Nothing in the idea of paradigm-as-achievement speaks against scientific rationality – quite the contrary.

Paradigm-as-set-of-shared-values

When Kuhn writes of science he does not usually mean the vast engine of modern science but rather small groups of research workers who carry forward one line of inquiry. He has called this a disciplinary matrix, composed of interacting research groups with common problems and goals. It might number a hundred or so people in the forefront, plus students and assistants. Such a group can often be identified by an ignoramus, or a sociologist, knowing nothing of the science. The know-nothing simply notes who corresponds with whom, who telephones, who is on the preprint lists, who is invited to the innumerable specialist disciplinary gatherings where front-line information is exchanged years before it is published. Shared clumps of citations at the ends of published papers are a good clue. Requests for money are refereed by 'peer reviewers'. Those peers are a rough guide to the disciplinary matrix within one country, but such matrixes are often international.

Within such a group there is a shared set of methods, standards, and basic assumptions. These are passed on to students, inculcated in textbooks, used in deciding what research is supported, what problems matter, what solutions are admissible, who is promoted, who referees papers, who publishes, who perishes. This is a paradigm-as-set-of-shared-values.

The paradigm-as-set-of-shared-values is so intimately linked to paradigm-as-achievement that the single word 'paradigm' remains a natural one to use. One of the shared values is the achievement. The achievement sets a standard of excellence, a model of research, and a class of anomalies about which it is rewarding to puzzle. Here 'rewarding' is ambiguous. It means that within the conceptual constraints set by the original achievement, this kind of work is intellectually rewarding. It also means that this is the kind of work that the discipline rewards with promotion, finance, research students and so forth.

Do we finally scent a whiff of irrationality? Are these values merely social constructs? Are the rites of initiation and passage just the kind studied by social anthropologists in parts of our own and other cultures that make no grand claims to reason? Perhaps, but so what? The pursuit of truth and reason will doubtless be organized according to the same social formulae as other pursuits such as happiness or genocide. The fact that scientists are people, and that scientific societies are societies, does not cast doubt, yet, upon scientific rationality.

Conversion

The threat to rationality comes chiefly from Kuhn's conception of revolutionary shift in paradigms. He compares it to religious conversion, and to the phenomenon of a gestalt-switch. If you draw a perspective figure of a cube on a piece of paper, you can see it as now facing one way, now as facing another way. Wittgenstein used a figure that can be seen now as a rabbit, now as a duck. Religious conversion is said to be a momentous version of a similar phenomenon, bringing with it a radical change in the way in which one feels about life.

Gestalt-switches involve no reasoning. There can be reasoned religious conversion – a fact perhaps more emphasized in a catholic tradition than a protestant one. Kuhn seems to have the 'born-again' view instead. He could also have recalled Pascal, who thought that a good way to become a believer was to live among believers, mindlessly engaging in ritual until it is true.

Such reflections do not show that a non-rational change of belief might not also be a switch from the less reasonable to the more reasonable doctrine. Kuhn is himself inciting us to make a gestalt-switch, to stop looking at development in science as subject solely to the old canons of rationality and logic. Most importantly he suggests a new picture: after a paradigm shift, members of the new disciplinary matrix 'live in a different world' from their predecessors.

Incommensurability

Living in a different world seems to imply an important consequence. We might like to compare the merits of an old paradigm with those of a successor. The revolution was reasonable only if the new theory fits the known facts better than the old one. Kuhn suggests instead that you may not even be able to express the ideas of the old theory in the language of the new one. A new theory is a new language. There is literally no way of finding a theory-neutral language in which to express, and then compare the two.

Complacently, we used to assume that a successor theory would take under its wing the discoveries of its predecessor. In Kuhn's view it may not even be able to express those discoveries. Our old picture of the growth of knowledge was one of accumulation of knowledge, despite the occasional setback. Kuhn says that although any one normal science may be cumulative, science is not in general that way. Typically after a revolution a big chunk of some chemistry or biology or whatever will be forgotten, accessible only to the historian who painfully acquires a discarded world-view. Critics will of course disagree about how 'typical' this is. They will hold – with some justice – that the more typical case is the one where, for example, quantum theory of relativity takes classical relativity under its wing.

Objectivity

Kuhn was taken aback by the way in which his work (and that of others) produced a crisis of rationality. He subsequently wrote that he never intended to deny the customary virtues of scientific theories. Theories should be accurate, that is, by and large fit existing experimental data. They should be both internally consistent and consistent with other accepted theories. They should be broad in scope and rich in consequences. They should be simple in structure, organizing facts in an intelligible way. They should be fruitful, disclosing new events, new techniques, new relationships. Within a normal science, crucial experiments deciding between rival hypotheses using the same concepts may be rare, but they are not impossible.

Such remarks seem a long way from the popularized Kuhn of *Structure*. But he goes on to make two fundamental points. First, his five values and others of the same sort are never sufficient to make a decisive choice among competing theories. Other qualities of judgement come into play, qualities for which there could, in principle, be no formal algorithm. Secondly:

> Proponents of different theories are, I have claimed, native speakers of different languages. . . . I simply assert the existence of significant limits to what the proponents of different theories can communicate to each other . . . .Nevertheless, despite the incompleteness of their communication, proponents of different theories can exhibit to each other, not always easily, the concrete technical results available by those who practice within each theory.[2]

When you do buy into a theory, Kuhn continues, you 'begin to speak the language like a native. No process quite like choice has occurred', but you end up speaking the language like a native nonetheless. You don't have two theories in mind and compare them point by point – they are too different for that. You gradually convert, and that shows itself by moving into a new language community.

2 'Objectivity, value judgment, and theory choice', in T.S. Kuhn, *The Essential Tension*, Chicago, 1977, pp. 320–39.