

Note to the 1992 Edition

For this second edition, the main text has been left intact except for correction of the more serious typographical errors and spelling errors; some overlooked entries in the bibliography have been added, and others corrected. There is, in addition, a new Afterword.

The first edition of the book was widely discussed, and I am grateful for the serious and lengthy treatment it received at the hands of reviewers. Because the criticisms were so heterodox, a comprehensive response would have turned out to be a long catalogue of disconnected arguments. Furthermore, some of the detailed criticisms, such as James Brown's suggestion that deliberate secrecy could account for the difficulties of communication encountered by laser scientists, were already answered in the text, and some of the philosophical criticism, such as Barry Barnes's discussion of the treatment of induction in Chapter One, do not affect the main thesis. One consistent problem for reviewers was methodological relativism, but this has been, and is being, continually contested in other places. While, then, I have responded directly to a few reviews, the larger part of the Afterword is designed to place the book in the context of some of the more obstinate debates that have been taking place over the last few years within sociology of scientific knowledge.

The Afterword is self-contained; it comes with its own bibliographical notes and is not referenced in the index.

University of Bath
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Introduction

[In Tlon there are] objects composed of two terms, one of visual and another of auditory character: the colour of the rising sun and the far-away cry of a bird. There are objects of many terms: the sun and the water on a swimmer's chest, the vague tremulous rose colour we see with our eyes closed, the sensation of being carried along by a river and also by sleep . . . (Jorge Luis Borges, 'Tlon, Uqbar, Orbis Tertius')

During the last decade sociologists, historians and philosophers have begun to examine science as a cultural activity rather than as the locus of certain knowledge. The ideas that come out of this research have significance for more than a few specialist academics, for when it is regarded in this way the study of science can tell us things about culture as a whole — while at the same time this new perspective demystifies the role of scientific expertise. The importance of these ideas both for professional academics and for all those whose lives are touched by science has led me to try to provide a readable account while still making a technical contribution. Thus I have attempted to write the main text in a way which makes it accessible to anyone with an interest in and some knowledge of either the natural sciences, social sciences, history of science or philosophy. I have also added a 'Postscript' which explicitly draws out the book's wider relevance.

To keep the text within bounds of length and comprehensibility I have made much use of discursive notes in some chapters, most of which would have been included in a longer text. I hope the interested but non-specialist reader will turn to at least some of them. A few of the notes are intended purely for the specialist and I have indicated this by putting the superscript in parentheses — thus.⁽⁵⁶⁾ I have also gathered some technical material that is not likely to be of interest to the general reader into a short methodological appendix.

The effort that the book does require is an initial derailment of the mind from the tracks of common sense. Our cultural environment — the everyday world — has to be turned into a strange place if we are to see that its perceived orderliness is a remarkable and mysterious human accomplishment. I have tried to engender this shift of consciousness in the reader by introducing the book with problems arising out of philosophical scepticism. The outcome is a form of 'relativism' — a term and a philosophy that frightens many. But this form of relativism is a pleasant glade not far from the perceptual railway tracks we normally ride on. Indeed, the relativist glade has paths in it that lead to most of the destinations of the metal road. However, they do not lead in quite the same predetermined way; a woodland track invites exploration, has alternate routes and offers a wider choice of scenery than a railway.

2 *Changing Order*

The first chapter has been designed to open the mind for an exploration of the fundamental problems of order in conceptual and social life. It shows that our concepts and our social conventions reinforce each other — as in a network — and this explains the maintenance of order. Concepts and conventions are ‘jointly entrenched’ within ‘forms of life’. The problem of change is left for empirical investigation in later chapters. Chapter One concludes by showing how the fundamental problems of conceptual order and change give rise to the well known difficulties encountered in building intelligent machines.

The second chapter examines science’s ordering principle — the replicability of observations and experiments. I adopt a metaphor from *The Hitch-Hikers Guide to the Galaxy*: the Earth as a computer constructed by ‘philosopher-mice’. We are able to see the problems of intelligent machines, discussed in Chapter One, re-emerge when science as a whole is thought of as a giant computer. In particular, it does not seem possible to construct a computer-type ‘algorithm’ for ensuring that experimental replication always provides a definitive test for the existence of new and disputed natural phenomena.

Chapters Three, Four and Five report the main field studies. They are all close examinations of passages of science in which scientists tried to repeat each other’s work: laser building — a relatively straightforward piece of science; the detection of gravitational radiation — an area at the very frontiers of research; and ‘the secret life of plants’ and ‘mind-over-matter’ — areas of parapsychology. Chapter Four concludes with a technical appendix on the detection of gravity waves.

The main argument of these three chapters turns on a comparison of the process of replication of scientific findings among these areas of science. This comparison will reveal the existence of what I have called the *experimenters’ regress*. This is a paradox which arises for those who want to use replication as a test of the truth of scientific knowledge claims. The problem is that, since experimentation is a matter of skilful practice, it can never be clear whether a second experiment has been done sufficiently well to count as a check on the results of a first. Some further test is needed to test the quality of the experiment — and so forth.

Both Chapters Four and Five conclude with a discussion of ways in which scientists try to test the quality of experiments directly in order to circumvent the experimenters’ regress. In Chapter Four the process of calibrating the apparatus is discussed; in Chapter Five it is the use of surrogate phenomena. The failure of these ‘tests of tests’ to resolve the difficulty demonstrates the need for further ‘tests of tests of tests’ and so on — a true regress.

The sixth chapter pulls together the themes of Chapters Two, Three, Four and Five and develops their implications for the problems of perceptual order set out in Chapter One. I show how the individual scientist is tied into the network of institutions in the wider society and try to demonstrate how these constrain research choices and the outcome of work at the laboratory bench. The sources of stability in the conceptual universe are explored and the problems and means of 'changing order' are discussed. During the discussion a number of propositions about experiment are developed. The first ten of these are available for easy reference on the first page of Chapter Six.

A Postscript discusses the wider implications of the book for science education, science policy questions, forensic science, public inquiries and the role of scientific expertise in the institutions of democratic society. It concludes with some examples of the way that an understanding of scientific change sheds light on political processes. A methodological appendix follows.

It might be thought that the passages of science compared in Chapters Three, Four and Five are selected according to a strange principle. After all, there is one piece of near-technology done without any intent to 'test' a finding — the TEA-laser; there is one piece of science drawn from a central theoretical tradition of physics, albeit one using frontier technology and coming up with some unexpected findings — the detection of gravity waves; and there are two pieces of parapsychology, one of which was done by what one might call the 'marginal man's marginal man', on the psychic life of plants. From a viewpoint which makes clear distinctions between 'real' science and 'pseudo' science, these would be odd things to *compare*. From the viewpoint of this book they are not such strange bedfellows; the relativist attitude demands that the analysis of the way that knowledge is established is not shackled at the outset by common sense judgements about what is and what is not true. The question is, rather, how things come to be seen as true or false; and this requires the self-conscious innocence which goes along with the suspension of everyday certainties.

These three examples of scientific practice are chosen for comparison because they represent two out of what I shall call the 'three phases' of science. These comprise the 'revolutionary' phase, the 'extraordinary' phase and the 'normal' phase. In the revolutionary phase large scale and widespread changes in the whole conceptual structure of disciplines take place. This idea, due to Kuhn (1962), has been the subject of heated philosophical debate; it is not discussed in this book (but see Collins and Pinch, 1982). The extraordinary phase, on the other hand, is easy to recognize. It is the site of smaller scale controversy. Such controversy arises when claims

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are made that do not sit comfortably with the prevailing orthodoxy. When the stormy waters settle, what is left is the normal phase (another, much less contentious, Kuhnian term) in which nearly all science is actually done. The case studies reported here are representative of the normal phase (TEA-lasers) and the extraordinary phase (gravity waves and parapsychology). The parapsychological study has, perhaps, proto-revolutionary qualities, and it is certainly a little further away from the centre of orthodoxy than even the gravity wave story.

As will be seen, the parapsychology and the gravity wave debates reported in this book look very like each other in terms of the structure of argument which surrounds the claims of replicability; and they both look quite unlike the TEA-laser case. Thus, if there is anything odd about comparing such a heterogeneous collection of passages of scientific activity, it is the TEA-laser that is the odd one out! This marks a difference between the perspective of this work and more orthodox ways of looking at science. In earlier perspectives it would be parapsychology that would look odd because of its marginality with respect to mainstream science, and because the other two cases are drawn from physics whereas it is a study of living subjects. But, as Chapters Three, Four and Five will reveal, the important dimension turns out not to be the scientific subject matter but the phase of science that is represented.

Chapter One

The Mystery of Perception and Order

A few years ago a sketch on television's *Monty Python's Flying Circus* featured a misleading Hungarian phrasebook. 'Can I have a box of matches?' was mistranslated into English along the lines of 'I would like to feel your beautiful thighs.' The appropriate rebuff in English was mistranslated into Hungarian as 'Your eyes are like liquid pools.' The phrasebook turned what should have been a routine exchange between a large Hungarian and a meek tobacconist into a violent brawl. The phrasebook introduced disorder into what the participants expected to be routine orderly interaction.

Without order there can be no society. Communication, and therefore the whole of culture in its broadest sense, rests on the ability of human beings to see the same things and respond to them in the same ways. There may be variation in perception and meaning between different groups, but the very existence of 'groups' depends on the uniformities within them. The fact is that there are groups, societies and cultures; therefore, there must be large scale uniformities of perception and meaning.

Though these uniformities are fundamental, the way that they come about and the way that they are maintained are profound mysteries. These mysteries underlie the major problems of philosophy, linguistics, sociology, artificial intelligence, and the philosophy of science. Concerted perception and understanding in an open environment seem to be something that humans simply do, without consciously thinking about it. This book is about the way such concerted perception and action come about. It explores the problem by focusing on the particular way scientists come to perceive, describe and understand new natural phenomena in a uniform way. It examines some instances of this sort of agreement and offers these instances as exemplary cases of the formation and maintenance of more general patterns of action. The field studies reported, narrowly focused as they are, are intended to reflect light on to the deeper problem of culture.

The difficulty is that, because we manage concerted perception and action with such unthinking ease, it hardly seems like an accomplishment of any note. Our common perceptions, as I have suggested elsewhere (Collins, 1975), are like ships in bottles. The ships, our pieces of knowledge about the world, seem so firmly lodged

in their bottles of validity that it is hard to conceive that they could ever get out, or that an artful trick was required to get them in. Our world is full of ships already within their bottles and it is only the rare individual who gets a brief glimpse of the ship-in-bottle maker's art. Science, more than any other cultural activity, is in the business of putting new ships into new bottles — that is, it is in the business of building new bits of knowledge. Even in science, however, the art is so routinized that the tricks are only visible when some self-conscious attention is given to them, for instance, in the case of a scientific controversy. The first task, then, is to pull the mind free of the taken-for-granted ways of seeing and, instead, to let it see the sticks and strings and glue from which the ships of knowledge are built. I use philosophical scepticism, which is safe, legal and inexpensive, to loosen the trammels of commonsense perception.

Scepticism and the problem of inductive inference

Scepticism begins with the problem of why we should expect the future to be like the past. Why do we expect regular sequences of events to continue and how is it that we seem to obtain evidence about the future by extrapolating from past regularities? Inferring general rules from repeated past regular instances is called induction. Thus scepticism engenders what is known as 'the problem of induction'.¹ This is a philosophical problem having to do with the way that our 'inductive inferences' — generalizations from past experience — can ever be certain or even probable. At root, however, scepticism concerns the perception of any sort of regularity at all. My concern is not with how we could be certain *in principle* about induced regularities but about how we actually come to be certain about regularities *in practice*. This is a shift of focus which allows what I will call a 'sociological resolution' of the problem of induction. Many fundamental questions to do with the discovery of the rules of scientific and everyday activity are really locally specified versions of this practical puzzle.

The easiest way to begin to see this problem, and the standard starting point, is with the work of the philosopher David Hume. Hume posed the problem in terms of our ideas about cause: take an event — call it 'a' — such as the striking of a stationary billiard ball by another, and a second event — 'b' — the billiard ball moves across the table. We are inclined to say that the the billiard ball 'is propelled' across the table since we take it that its movement is 'caused' by the impact. We see sequences like a – b happen frequently and indeed we see and know *from experience* that the first billiard ball is what causes the second to move. But, suppose the regularity of the a – b sequence were just an extended coincidence, not a causal relationship — how

would we see the difference? In other words, what is it that we see in the impact of the billiard balls that makes us view it as a causal relationship, which we are confident will continue, rather than an extended coincidence which we would not expect to continue? The answer is 'nothing'. Why then do we treat the relationship as one of necessity? Why do we think such relationships contain causal certainties?

One can sharpen the impact of the question by thinking of repeated sequences of events that we do not treat in this way. For example, we do not think that accurate forecasting of, say, the weather carries an implication of cause, even though in this case an event 'a', such as the vocalization of the words 'it will rain' is regularly followed by event 'b': precipitation. Of course, the weather forecast is not always right, but if it were to become more and more accurate we would not become more and more tempted to impute a causal relationship between the weather and the forecast. In the last resort, were the forecast to become wholly accurate, we would sooner be inclined to impute clairvoyance to the forecaster than causal efficacy to the forecast. Again, there are regular sequences which seem to incline us to think that the longer they carry on the more likely they are to break down. For example, naive roulette players will be more and more inclined to bet on black the longer an unbroken sequence of reds continues. Here they see Event A — the spin of the wheel — followed regularly by Event B — the falling of the ball into the red slot — and they become more and more certain that next time B will not follow A. These two examples show us that regularity of events in itself does not compel us to see causal relationships.

Hume thought that, although causes were invisible, we had a psychological propensity to impute necessity, and therefore cause, to regularly repeated sequences. No doubt there is truth in this; man is fundamentally a regularity-perceiving creature. We do, as a matter of fact, induce from the particular to the general all the time.² As Hume saw, however, this sort of 'solution' to the problem does not help because it would only be useful were it obvious beforehand which sequences of events could be generalized; as we have remarked, there are many sorts of regular sequences, such as the forecast and the weather, that we might generalize but don't, and many irregular ones that we do.³ For these reasons, citing a generalizing propensity as an explanation of our inductive (regularizing) tendencies is simply truistic. Since a generalizing tendency would allow us to see anything and everything as regular, which would amount to seeing nothing, this sort of 'solution' is vacuous. In fact, the problem of perceiving regularity is a sub-division of the broader question about the very possibility of perception.

Afterword

Science Acts

Changing the Order of Science¹

Once a scientific fact has been established by, for example, repeated experiment, it seems fixed. One of the aims of *Changing Order* is to explain how facts can change in spite of this. *Changing Order* shows that change is possible because the establishment of, say, the replicability of a phenomenon depends upon communally defined skills and rests on agreement within social collectivities. The book shows that the social determination of scientific knowledge is possible in spite of scientific method—indeed, it argues that scientific method is social through and through. *Changing Order* is meant to encourage a change in the relationship of science to other cultural endeavours by altering our appreciation of scientific method. It is *not* meant to change scientific method.

Though the general mechanisms of the closure of scientific controversy are discussed in the book, there is no detailed sociological theory of change, nor does *Changing Order* try to explain the establishment of any particular consensus. It does not explain why we no longer believe in high fluxes of gravity waves or the emotional life of plants.² The book makes space for the possibility that if social institutions had been different then our beliefs about gravity waves or vegetable consciousness might now be different. Critics who have suggested that *Changing Order* falls short in not explaining why one fact emerged rather than another have mistaken the thrust of the book.³ *Changing Order*, along with its precursor papers published in the mid-1970s, is meant to bring about a change in a well-established order of ideas concerning science as a whole. It helps to make the history of scientific knowledge possible; it is not itself history.

Increments and Revolutions

Changing Order offers a general description of the transformation of scientific forms-of-life, not a causal theory of change. It is wrong to suggest, as Pickering does, that the book posits contingency as the cause of change in science.⁴ The book's model of change can encompass a variety of sociological theories, including Pickering's 'dynamics of practice', in which alliances are formed among those who have ready-made tools to do new scientific jobs. But *Changing Order* does not restrict itself to incremental transitions; it contains one case study of 'normal' science and two studies of potential radical transition. Thus, the book does not, as Pickering suggests, compete with his theory, firstly because it operates at a different level of generality, and secondly because his theory is too limited to deal with revolution.

The available range of explanations of scientific knowledge reiterates deeply argued positions in the philosophy of the social sciences which can be set out with the aid of a lighthearted metaphor.⁵ One might say that theories of science and of society must have the right *consistency*. There are theories—ethnomethodology is a good example—which seem to allow society to be too thin and gaseous, as though everything that happens is a local achievement. What these approaches do not explain is why some things are harder to change than others. Why is it harder to establish the existence of, say, paranormal phenomena than, say, optical pulsars? These theories fail to explain how it is that it is easier to agree or believe some things today because certain things happened yesterday.⁶

At the other extreme there are theories of science, such as those favoured by the rationalist philosophers, where scientific life is like a preformed shape of solid stone; it cannot be changed except by violent and damaging distortion.

There is an in-between kind of theory where the community gives form to scientific beliefs but they are unchangeable once set. The analogous consistency is concrete rather than stone—an easily flowing liquid that solidifies when left. But all beliefs require work to maintain them—they do not stay set by themselves. Few ‘constructivists’ would nowadays admit to holding concrete-type theories, though, as we will see, some of Latour’s formulations have too much rigidity; objects that are ‘black-boxed’ are hard to un-black-box—they are not much different from stone.

Better theories give cultural life the consistency of a more or less thick liquid. A liquid never sets, can be transformed into any shape, but resists rapid transformations. One may make progress through mud, but only by slow movement, not by running. Such theories as Pickering’s suggest a slowly flowing liquid whose shape at time ‘t’ is always closely related to its shape at time ‘t-1’.

Though liquid is a more appropriate consistency for representing science than gas, stone, or concrete, we can improve the metaphor further. There are two aspects of science still unrepresented: the continual input of energy that is needed to maintain the shape of our beliefs—they will deteriorate without maintenance—and the potential for more rapid change during periods of revolutionary or extraordinary science. Ice is a good model, for ice needs energy to keep it set, but *ice cream* is better still because it is a little less rigid.

Ice cream, if left to itself, will slowly lose all form. Heat or pressure—representing the revolutionary or extraordinary periods of science—will turn it rapidly to liquid. In *Changing Order* the world of the TEA-laser is thoroughly frozen, whereas the other two studies show the speed with which local hot spots can develop. Ice cream is a good aide-mémoire lest we lose sight of this aspect of science.

Unfreezing Science

Turn now from *analyses* of science to the *impact* of sociology of scientific knowledge on science's place in the world. How much does the sociology of scientific knowledge 'melt' the traditional authority of science? *Changing Order* tries to bring science to the same epistemological level as other knowledge-making activities. The way it does this, however, is less straightforward than it seems.

Sociology of scientific knowledge offers strong empirical evidence that if our beliefs about controversial features of the world are a consequence of the way the world is, this is not evident during passages of discovery and proof; an account which rests on orderly interaction with the world can be provided only after retrospective reconstruction. As I argued in an earlier paper, these findings are still compatible with a 'hidden hand' model of science.⁷ Thus, the ideas and findings set out in *Changing Order* need relativism only at the level of methodology. The only thing that has to be agreed is that the prescription 'treat the world *as if* it had no effect on what people believe about it' is a reasonable starting point for the study of scientific action. This 'methodological relativism' is not in need of further defence.⁸ Methodological relativism, however, is, as one might say, an *input* to the sociology of scientific knowledge. The question remains whether relativism is also an *output* from the sociology of scientific knowledge.

The answer is that while sociology of scientific knowledge does not *prove* relativism it does lead inexorably in that direction. This is because the more successful an analysis based on certain presuppositions the more those presuppositions look right—the more they take on the characteristics of an output, not just an input. Just as the empirical success of descriptions of the world based on Euclidean geometry encourages us to think that parallel lines never meet, it is the fruitfulness of the sociological case studies that leads us to reevaluate the nature of science. If truth, rationality, success, and progress (see the article on TRASP cited in note 7) are not found to be the driving forces of science when discovery and justification are described in as much detail as possible, then, it seems, science does not need them to explain its development.

None of this comprises an attack on science. The arguments can be construed as a critique only if science is held up against the canonical rational-philosophical model. Sociology of scientific knowledge does not show that science has failed to attain the standards associated with the canonical model, but that the canonical model is unattainable. Science does all that science can be expected to do.⁹ Science is changed in its relationship to other institutions however. Science is no longer epistemologically preeminent; the driving force, as with other elements of culture, is the community.

Actors and Actants

Not all approaches to scientific knowledge have the implications sketched out above. The network theory as developed recently by Callon and Latour is an example. Callon and Latour do not accept that sociology of scientific knowledge alters the balance of power between science and culture. They think the debate is misplaced, preferring to set their own programme on an orthogonal dimension. This leads to what one might call 'radical symmetrism'.

David Bloor first expressed the principle of 'symmetry': for the purposes of historical and sociological analysis, one must treat what comes to be seen as true in science in the same way as what comes to be seen as false; never must analysts allow themselves to explain what comes to be believed at the time by reference to what is discovered to be true *later*. What counts as true is the *outcome* of social processes; the truth is not the cause of that outcome.¹⁰ Callon and Latour have extended this principle to all dichotomies, including the social and the natural. Thus, we must not say that the social and the natural are *socially* constructed, because that would be to use an element of the dichotomy which is to be explained as the starting point of the explanation.¹¹ Rather than see human actors and nonhumans competing for power within our theories, Callon and Latour try to treat both equally, as *actants*. But when we set radical symmetrism into the context of the existing debate about science we discover that it recapitulates much of the traditional view.¹²

Now, it is true that most of the scientific objects that populate even a sociologist's world have become so well established that for analytic and practical purposes it no longer makes sense to talk of them in terms other than those used by scientists. For example, neither I, the analyst, nor the principal actors in two episodes described in *Changing Order*, Joe Weber and Cleve Backster, treated the output of voltmeters as 'socially constructed'; their, in principle, challengeability can be ignored in these case studies. Latour would refer to such a state of affairs by saying that voltmeters are 'black-boxed'—their insides (and the 'insides' of the corresponding concepts) are no longer of concern to anybody. Once something has been black-boxed—in my terminology, closure has been accomplished—Latour treats it as an actant which can affect the balance of power in the network. If you have voltmeters on your side then you can determine voltages and no one can challenge your measurements without voltmeters of their own.

Crucially, however, this kind of argument would be wrong if the debate were about, say, psychokinesis. 'Psi-mediated experimenter effects' are continually on the mind (forgive the pun) of experimenters in the paranormal, and these make the readings of voltmeters difficult to separate from the intentions of the scientists. Therefore, within the science of psychokinesis a voltmeter can be challenged without recourse to a com-

This work in the sociology of science explores the way scientists conduct, and draw conclusions from, their experiments. The book is organized around three case studies: replication of the TEA-laser, detecting gravitational radiation, and experiments in the paranormal. Through detailed descriptions of these projects, Collins shows what it is like to try to reproduce results in a laboratory.

In his new Afterword, Collins places *Changing Order* in the context of some of the more obstinate debates of the last few years within the sociology of scientific knowledge.

"It is central to the canon of the highly influential 'sociology of scientific knowledge' tradition that grew up in Britain during the 1970s. . . . Collins' studies are accessible, enjoyable, gripping and challenging."

—Andrew Pickering, University of Illinois, Urbana-Champaign

"The writing is clear, spirited, witty. . . . Collins has written, in sum, a masterful, amusing book, showing on the ground how the society of science argues, showing that replication works, but only the way other human institutions work, by agreement. Scientific knowledge, he shows, is social."

—Donald N. McCloskey, *Journal of Economic Psychology*

"Collins is one of the genuine innovators of the sociology of scientific knowledge. . . . *Changing Order* is a rich and entertaining book." —*Isis*

"The book gives a vivid sense of the contingent nature of research and is generally a good read." —Augustine Brannigan, *Nature*

"This provocative book is a review of [Collins's] work, and an attempt to explain how scientists fit experimental results into pictures of the world. . . . A promising start for new explorations of our image of science, too often presented as infallibly authoritative." —Jon Turney, *New Scientist*

H. M. COLLINS is the director of the Science Studies Unit at the University of Bath.

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