RELIABLE KNOWLEDGE

An Exploration of the Grounds for Belief in Science

JOHN ZIMAN

Canto

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An exploration of the grounds for belief in science

JOHN ZIMAN, FRS

Emeritus Professor of Physics, University of Bristol

CAMBRIDGE
UNIVERSITY PRESS

Published by the Press Syndicate of the University of Cambridge
The Pitt Building, Trumpington Street, Cambridge CB2 1RP
40 West 20th Street, New York, NY 10011–4211, USA
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

First published 1978
Reprinted 1979
Canto edition 1991
Reprinted 1996

British Library cataloguing in publication data

Ziman, John
Reliable knowledge: an exploration of the grounds for belief in science.
1. Science – Philosophical perspectives
I. Title
501

Library of Congress cataloguing in publication data

Ziman, John M 1925–
Reliable knowledge.
1. Science-Philosophy. I. Title.
Q175.Z55 501 78–3792

ISBN 0 521 40670 6 paperback

Transferred to digital printing 2003

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Preface

This book has grown out of a lecture given in various places to a wide variety of audiences under the title 'Is Science to be Believed?' A generous invitation from the Van Leer Jerusalem Foundation encouraged me to reformulate, deepen and expand this into notes for four long seminars, which were presented in April 1975 to a helpfully critical group of philosophers, humanists and natural scientists. It seemed easy then to promise to 'write these notes up for publication'; but they had acquired a life force of their own, and it took another two years to break and tame them into the present text. Having no academic pretensions or professional affiliations outside physics, I have tended to read my own way into the diverse literature that is relevant to this enquiry, and to come to my own, perhaps idiosyncratic conclusions on many vexed issues of fact or principle. But I am grateful to Richard Gregory for some perceptive comments on the original notes, and to many others who have clarified tricky points by asking difficult questions in the lecture hall or in private conversation. Rosemary Fitzgerald gave valuable assistance in collecting the illustrations. And it is a pleasure once more, to express to Lilian Murphy my appreciation of her swift, accurate typing.

Bristol, June 1977

Note to the Canto edition

The 15 years since I began serious work on this subject have not changed significantly what I would want to say to the general reader. In spite of its informal style, this book was also intended to be, and remains, a serious challenge to other scholars in this field.

J.Z.

I
Grounds for an enquiry

'Science repudiates philosophy. In other words, it has never cared to justify its truth or explain its meaning.'
Alfred North Whitehead

1.1 *The challenge*

This work arises from two sources: a *challenge* and a *theory*. The challenge is to the beneficience of science as an agent of social change: the theory concerns the nature of scientific knowledge.

The attack on science comes from many quarters, but is not well concerted. The medley of opposition includes many strange companions-in-arms, following contradictory causes. The conservative fears that science will destroy the only world that he knows; the progressive imagines that it will poison the paradise to come; the democrat is cautious of the tyrannous capabilities of technique; the aristocrat fears the levelling tendency of the machine. The pleas of defence are equally inconsistent: some say that scientific progress is automatic and inevitable; others that the future must be determined by rational scientific planning; technocrats delight in telling us that science will make life more comfortable; space addicts proclaim that man must go forth and conquer the universe.

Science is such a complex human activity, so much a part of our civilization, so rapidly changing in form and content, that it cannot be judged in a few simple sentences.[1] We observe, nevertheless, that some of the products of scientific technology have been damaging to human welfare. In such cases, one can usually blame factors outside the realm of science: too hasty innovation, subordination to unworthy causes, distortion of social needs, or displacement of genuine human goals. But the feeling has arisen that the evil factor is knowledge itself; science is characterized as a materialistic, antihuman force, a Frankenstein monster out of control.

More subtle critics[2] do not minimise the instrumental power of

[1] This statement needs no documentation. *The Force of Knowledge* (1976: Cambridge University Press) is my personal view of the sociological and historical background to the present work.

[2] Exemplified admirably by Theodore Roszak (1972) in *Where the Wasteland Ends* (London: Faber).

science in its material, technical mode. The reliability of scientific knowledge in engineering, manufacturing, or medicine is not really in doubt. But they resist the attempt to extend science to the niceties of biological behaviour, human emotion and social organization. Any claim to scientific authority in such matters is regarded by such critics as pretentious, and inherently unsound. Other sources of insight and other guides to action must be treasured or sought beyond the reach of the scientific method.

This is the challenge – and it must be treated very seriously. A century ago, we might have described it as the conflict between Science and Religion. Nowadays, most people no longer base morality and aesthetics on divine revelation or rational theology; but no mature person with experience of life can seriously suppose that the issues of love and death, of justice and charity, could possibly be resolved by consulting the *Handbuch der Physik* or some latter-day edition of an *Encyclopaedia of the Behavioural Sciences*. On such matters, science clearly has little to say.

On the other hand it prejudges the issue to presume that a 'method' that has proved its worth in the realm of material technique can tell us nothing of value concerning man in society. We humans are part of the natural order of things, and subject to its necessities. The response to the challenge can be neither outright defiance nor abject surrender; the field of conflict is the middle ground, where the claims of science can be seen to be neither fanciful nor beyond reasonable doubt.

For this reason, the question of the *reliability* of scientific knowledge has become a serious intellectual issue. Once we have cast off the naive doctrine that all *science* is necessarily *true* and that all *true* knowledge is necessarily *scientific*, we realize that *epistemology* – the theory of 'the grounds of knowledge' – is not just an academic philosophical discipline. Very practically, in matters of life and death, our grounds for decision and action may eventually depend on understanding what science can tell us, and how far it is to be believed.

1.2 *The theory*

But what *is* science? How is it to be distinguished from other bodies of organized, rational discourse, such as religion, politics, law, or 'the humanities'? In an earlier work,[3] I have tried to show that scientific

[3] *Public Knowledge* (1967: Cambridge University Press).

knowledge is the product of a collective human enterprise to which scientists make individual contributions which are purified and extended by mutual criticism and intellectual cooperation. According to this theory *the goal of science is a consensus of rational opinion over the widest possible field.*

From this point of view, much can be understood about the ways that scientists are educated, choose research topics, communicate with one another, criticize and refine their findings, and relate to one another as members of a specialized social group. The 'consensus principle' thus leads directly into what is now called the *internal sociology* of the scientific community. From there we naturally proceed to investigate the place of science in society at large, trying to throw light on such important practical questions as the economics of research and development, the organisation of scientific institutions, priorities and planning of research, and the agonising ethical dilemmas facing the socially responsible scientist.

It is undoubtedly of great value to understand *how* science is done, and to appreciate the social role of the scientist and his institutions. But the epistemological challenge strikes deeper. What are the characteristic features of the body of knowledge acquired by this means? How does the consensus principle determine the *content* of science? What sorts of statement, about what aspects of the totality of things, are legitimate candidates for validation as 'public knowledge'? And to what extent does the striving for consensus eventually provide adequate grounds for belief and action?

In this book, therefore, I have deliberately turned away from the sociological aspects of science, to reconsider these fundamental intellectual issues. I am fully aware, of course, of the immense literature on the philosophy of science, where these very questions are asked again and again, and given a whole rainbow of answers. The writings of Plato and Aristotle, Bacon and Descartes, Kant and Wittgenstein, on this subject are the common heritage of our culture. But not being a trained philosopher, I could not pretend to be acquainted with all past and present opinions, all insights and all objections, on so large a topic.

Instead of attempting a general assessment of the epistemological problem, I propose to adopt the intellectual strategy of a typical paper in theoretical physics. A model is set up, its theoretical properties are deduced, and experimental phenomena are thereby explained, without detailed reference to, or criticism of, alternative hypotheses. Serious objections must be fairly answered; but the aim is to demon-

Although scientists often promise immeasurable future delights of understanding and truth, the epistemological challenge is always uttered at a particular moment: 'What can we believe *now*?' In assessing the credibility of scientific knowledge, we naturally look back over the past, but can put little weight on prognostications of an uncertain future. Our model, therefore, must be historically accurate, but need not be self-propelling; it will seldom be necessary to refer in detail to the psycho-dynamic forces that continually transform the contents of science (6.7).[6]

This is fortunate, since discussion of intellectual 'creativity' always tends towards a logical impasse – to cerebrate by other means than those of a particular science the unknown concepts that will eventually arise in that science. We shall see, indeed, that much more down-to-earth intellectual phenomena of belief and doubt, where the subject matter and context are known in advance to the psychological investigator, are also connected with 'creative' powers of imagination and intuition (5.4).

On the other hand, we cannot adopt a 'freeze-dried' model, where, on the appointed date, dispassionate, unprejudiced recording angels fly down to examine the scientific archives, and make an absolute assessment of the validity of each scrap of knowledge. As we saw in § 1.1, the epistemological challenge is not just an academic question; it arises in a human situation, and the answer is often required to deal with a human predicament. Those who ask the question, 'is this a matter on which science is to be believed?' must be given an answer that takes into account their own biographical experience and capabilities of comprehension. It would have been misleading, for example, to tell a railway engineer in 1920 that he should no longer believe in Newtonian mechanics because it had just been superseded by Einstein's general theory of relativity; for all his purposes, Newton's laws of motion remain as true as ever. From the very beginning I reject any system of *metascience* that purports to have such angels at its beck and call.[7]

[6] The numbers in parentheses are cross-references to other sections.

[7] This applies, in particular, to 'logical empiricism', in the various forms criticized by G. Radnitzky (1968) in *Anglo-Saxon Schools of Metascience* (Goteborg: Akademiforlaget). But I also, most emphatically, reject his hubristic view (p. xiv) that 'the metascientist will, one day, function like the business *consultant* – he will have to advise, warn, etc. in connection with the knowledge-producing enterprise, be it for the purpose of the planned production of some specific piece of knowledge or know-how, or be it for the regulation of the available "scientific capital" of a nation, a firm, etc. by means of foreign trade in scientific knowledge'.

1.4 *Consensibility and consensuality*

In its simplest form, therefore, our model consists of a number of independent scientists, linked by various means of *communication*. Each scientist makes observations, performs experiments, proposes hypotheses, carries out calculations, etc., whose results he communicates to his colleagues. As an individual, the scientist, like any other conscious being, acquires a great deal of personal knowledge about the world he inhabits, not only through his own experience but also through the information flowing to him from others. But when we talk of scientific knowledge, we refer to the content of the messages that accumulate and are available in the public domain, rather than to the memories and thoughts of each person.[8]

Going beyond this truism, we shall assume that scientific knowledge is distinguished from other intellectual artefacts of human society by the fact that its contents are *consensible*. By this I mean that each message should not be so obscure or ambiguous that the recipient is unable either to give it whole-hearted assent or to offer well-founded objections. The goal of science, moreover, is to achieve the maximum degree of *consensuality*. Ideally the general body of scientific knowledge should consist of facts and principles that are firmly established and accepted without serious doubt, by an overwhelming majority of competent, well-informed scientists. As we shall see, it is convenient to distinguish between a *consensible* message with the *potentiality* for eventually contributing to a consensus, and a *consensual* statement that has been fully tested and is universally agreed. We may say, indeed, that consensibility is a necessary condition for any scientific communication, whereas only a small proportion of the whole body of science is undeniably consensual at a given moment.

This model imposes constraints upon the *contents* of science. In the first place, fully consensible communication requires an unambiguous *language*, of which the ideal form is *mathematics* (2.2). But the exchange of logically consistent messages is fruitless unless they refer to recognizable and reproducible events within the experience of individual scientists; this explains the fundamental role of controlled observation and *experiment* (3.3) in the conventional 'method' of science.

But human cognition and communication are not restricted to pointer readings and algebraic formulae. Through our natural facility

[8] This is evidently 'World 3' of Karl Popper's *Objective Knowledge* (1972: Oxford University Press) – the logical contents of books, libraries, computer memories, etc. (5.5).

digm is not necessarily close to 'absolute truth'. As has been emphasized, our model of science does not contain any independent source of 'objective' knowledge, and is therefore vulnerable to error in two significant ways.

In the first place almost every scientist is raised up, by formal education and research experience (6.2), within the world picture of his day, and cannot happily consent to statements that are obviously at variance with what he has learnt and come to love. The achievement of intersubjective agreement is seldom logically rigorous; there is a natural psychological tendency for each individual to go along with the crowd, and to cling to a previously successful paradigm in the face of contrary evidence. Scientific knowledge thus contains many fallacies (4.5) – mistaken beliefs that are held and maintained collectively, and which can only be dislodged by strongly persuasive events, such as unexpected discoveries or completely falsified predictions. In other words, our model must take into account the effects of its collective intellectual products on the cognitive powers of each of its individual members.

Secondly, and more significantly, is there any defence against the charge that the whole scientific paradigm is a self-sustained delusion (5.10)? The scientists in our model are almost always deliberately trained to a particular attitude to natural phenomena. How are their intellectual constructs to be distinguished from those of any other self-accrediting social group, such as a religious sect? What reason have we for preferring the scientific paradigm as the ideal, unique world picture?

We may assert that the social system of science is always open to the outsider (6.3), and that contributions of fact or opinion are not solely restricted to registered True Believers. It is well known, for example, that major scientific progress often comes from scientists who have crossed conventional disciplinary boundaries, and have no more authority than a layman in an unfamiliar field. According to the ethics of 'the scientific attitude', science is valid in principle for Everyman, because *any* man could, if he wished, take up the study of science for himself, and would eventually be freely persuaded of its truth.

In practice, however, this is almost impossible; and when we look at the brainwashing implicit in the long process of becoming technically expert in any given branch of science, we see that it scarcely answers the objection – he who emerges from this process is no longer the unbiassed independent inspector who entered it ten years before.

More to the point, it must be emphasized that no scientist is a disembodied observing and conceptualizing instrument; he is a conscious human being, born and reared in the common life of his era. Long before he is taught about electrons, and genes, and exogamous fratries, he has acquired practical experience of pots and pans, cats and dogs, uncles and aunts. Although such mundane objects are seldom discussed as such in high science, they are not excluded from its realm. However fantastic it may appear on its wilder shores, the scientific consensus includes, by definition, the matter-of-fact, and must be coherent with everyday reality (5.10). Failure to accord with reliable 'commonsense' evidence is quite as discreditable as falsification of a theory by a contrived, abstruse experiment. Of course, commonsense evidence may often turn out to be irrelevant or ambiguous, but it cannot be trampled underfoot.[9]

The epistemological challenge to science thus leads to such profound questions as how each person acquires his view of the world, how far all men see the same world, and whether there can be any conceivable alternative to the 'reality' in which most men believe. The answers to these questions must not be anticipated here, for they determine the whole outcome of this book.

In some respects, however, this outcome cannot really be in doubt. Science does, after all, have its triumphs. It would be absurd to deny the validity of a theoretical system such as quantum mechanics, to which we owe our stocks of nuclear weapons. Who would doubt the credibility of Mendelian genetics, now completely confirmed at the molecular level by the deciphering of the genetic code? At least *some* of the knowledge that has been acquired 'scientifically' is as reliable as it could possibly be.

The basic strategy of this book is, therefore, to illustrate the workings of the social model of science by reference, initially, to the 'natural sciences', where the power of the 'scientific method' has been demonstrated beyond reasonable doubt. The most astonishing achievements of science, intellectually and practically, have been in *physics*, which many people take to be the ideal type of scientific knowledge. In fact, physics is a very special type of science, in which the subject matter is deliberately chosen so as to be amenable to quantitative analysis (2.7). But it is only when we have fully understood how

[9] In other words I accept the viewpoint summarized by G. Santayana (1962) in *Reason in Science* (New York: Collier Books) 'Science...is common knowledge extended and refined. Its validity is of the same order as that of ordinary perception, memory, and understanding. Its test is found like theirs, in actual imitation, which sometimes consists in perception and sometimes in intent. The flight of science is merely longer from perception to perception, and its deduction more accurate from meaning to meaning and from purpose to purpose.'

science really works even under the most favourable conditions that we can appreciate its limitations. For that reason, I felt it necessary to discuss the 'philosophy' of physics at some length, especially in Chapters 2 and 3. Of course, this is difficult, because physics is a very sophisticated intellectual discipline, whose techniques and attitudes are not easily explained to the uninitiated; I hope that I have managed at least to hint at some of this, by reference to various historical and contemporary examples, without losing the reader on the way. No doubt quite similar case histories could be found in chemistry, geology, or biology, but they would not necessarily be any easier to grasp out of context.

This investigation of the epistemology of the natural sciences takes up the greater part of the book. It is only in the final chapter that we arrive at a position from which we can begin to consider the fundamental question of the book as a whole – how much ought we to believe of what science might tell us about man as a conscious social being, subject to unreasonable emotions and irrational institutions? I do not pretend that such a question *can* be 'answered', but it seems appropriate to subject it to a scrutiny based upon all that we have learnt about the credibility of the natural sciences, where the subject matter is so much easier to control. The results of this scrutiny are not, to tell the truth, very favourable to the 'behavioural sciences' as we know them today; perhaps, after all, the epistemological challenge is not unjustified in that respect.

Needless to say, this inquiry is entirely concerned with the *cognitive* aspects of science and not at all with any instrumental applications of scientific knowledge to technology or other human activities. A successful application of knowledge is, of course, a pragmatic demonstration of its validity, and much of what is referred to as 'observation' or 'experiment' in fact derives from carefully recorded practice. Similarly, a confirmed or falsified prediction may have been derived from a very practical event, such as the failure of a carefully designed bridge. The main themes of this book may seem academic and aloof; but in a society dazzled by silver-tongued technocrats and other self-accrediting experts these questions are only a few breaths away from harsh realities and bitter home truths.

not avoided by the irresistible trend towards a single world-wide language for scientific communication. To be sure that his work will be read and understood, the Italian or Egyptian or Argentinian scientist translates it for himself from his native tongue into the international scientific language. This language is no longer Latin, but is, of course, English; or, rather, it is ***Broken English***, for even those who speak and write it accurately and fluently seldom command it in all the depth and subtlety of a mother tongue. The consensible contents of such publications are thus no broader in scope than what can be accurately translated from one language to another by a competent scientific author.[2]

Science could not, in any case, use all the resources of a natural language such as English. Consensible communication demands the deliberate style or mood appropriate for the transmission of unambiguous knowledge – what Gilbert Ryle calls *didactic* discourse.[3] But consensibility is not enough; every scientist is pressing towards consensuality. His communications are intended not only to tell things as he saw them, or as he thinks they are; he is also desperately keen to *persuade* his readers or audience. A scientific message often has the purpose of changing a preconceived notion, demonstrating an unsuspected contradiction, or announcing an unexpected observation. It is addressed to an actual sceptic, a potential critic; it must be convincing, it must be watertight.

By a psychological inversion this rhetorical motive is best served by a very plain and modest style.[4] But in normal, natural language it is easy to slip out of the noose of a line of reasoning. Ordinary everyday verbal controversies are always loose and inconclusive; one side or the other finds loopholes, such as ill-defined terms or ambiguities of expression, that allow him to avoid an unpalatable conclusion. That is why legal documents have to be written in a complex, formalized (and ultimately repellent) language. Scientific communications are

[2] President de Gaulle once insisted that French scientists should speak French at international conferences. This was countered by the English participants who said that they would speak French too!

The very real difficulties of translating advanced scientific and technical information into rigidly traditional languages such as Arabic is discussed perceptively by C. F. Gallagher, 'Language rationalization and scientific progress' in K. H. Silvert (ed), *The Social Reality of Scientific Myth* (New York: American Universities Field Staff Inc.) pp. 58–90.

[3] 'It is talk in which, unlike most of the others, what we tell is intended to be kept in mind...to be remembered, imitated and rehearsed by the recipient...(It) can be accumulated, assembled, compared, sifted and criticized'. *The Concept of Mind* (1949: London: Hutchinson).

[4] See Ziman, *Public Knowledge*, p. 96.

forced along the same path. In the search for perfect precision and overwhelming certitude, they become formalized statements in which technical terms that have been previously defined with the maximum rigour are bound together in unambiguous syntactical arrangements implying complete logical necessity.[5] No wonder scientific writing lacks literary grace and is denounced for its barbarity!

2.2 *Mathematics as the ideal language*

The ultimate step in formalizing a language is to transform it into *mathematics*. As each word in the language becomes more and more precisely defined, its specific meaning comes to reside in its relations to other words; these relations acquire the force of axioms, akin to those defining the essential relationships between say, the 'points' and 'lines' in Euclidean geometry. Two scientists who are familiar with such a system of definitions and axioms can thus send each other unambiguous messages. There is no danger of misunderstanding the statement, 'the carbon atoms in benzene form a regular *hexagon*', because a regular hexagon is a well-defined figure. The mathematical concept of *number* is very precise. The statement that 'a neutral carbon atom contains *six* electrons' is completely consensible, being comprehensible and capable of verification in principle, by any observer.

The ideal language for scientific communication is thus to be found in mathematics. Of its essence, mathematics is unambiguous and universally valid. Not only do modern Chinese and Indian mathematicians use the standard symbolism of European algebra; ancient Chinese mathematicians discovered the 'theorem of Pythagoras' independently of their Greek contemporaries, and ancient Hindu mathematicians juggled with enormous numbers long before these were needed in astronomical computations. The urge to express all scientific knowledge in mathematical terms is an elementary consequence of our model of science. In the pursuit of a consensus, we are bound to hit upon this device for constructing messages with the maximum degree of clarity and precision. Whatever we may eventually suspect concerning the limitations of a mathematical description of human experience, the central place of mathematics in the natural sciences is well-deserved and appropriate.[6]

[5] E.g. 'The ultimate reason for formalization [of scientific theory] is that it provides the best objective way we know to convince an opponent of a conceptual claim.' P. Suppes (1968) *Journal of Philosophy*, **65**, 651.

[6] This implies that *mathematics is a social institution*, as pointed out by D. Bloor (1973) *Studies in the History and Philosophy of Science*, **4**, 173.

be formulated, but whose truth or falsity cannot be decided, within a given axiomatic system. In other words, a scientific message communicated in the mathematical language generated by these axioms could contain fatally uncertain statements relying on such propositions.

In the analysis of such problems, we inevitably encounter a special branch of modern mathematics – formal *logic*. The fundamental importance of logic in science needs no emphasis.[9] At this point we see logical consistency or *logicality* as a necessary condition for meaningful intersubjective communication. A scientific message that was logically contradictory would be totally ambiguous and hence void. A patently illogical language would be quite useless as a medium for science. One of the advantages of a good mathematical symbolism is that it can avoid logical errors by intellectual automation.

But researches in the foundations of mathematics and mathematical logic have shown that the hope of finding a unique and *perfectly* logical language of this kind is vain. In the drive towards an absolute consensus, we eventually arrive at a point where differences of opinion concerning the status of the laws of logic itself could not be resolved by reference to a higher authority.

Logicality is not a *sufficient* condition for scientific discourse. It applies only to the *grammar* of the scientific language, and says nothing about the contents of the messages whose form it constrains. Consensual theorems of formal logic are an important branch of pure mathematics but are practically empty scientifically. Almost all science is based upon a variety of other principles that are shared by the community of scholars but are not deducible from logic alone. It is practically impossible, for example, to make a scientific statement that does not depend on such Kantian 'categories' as 'space' and 'substance'.[10] Much of the subject matter of pure mathematics has similar non-logical foundations.

Here we touch on one of the central issues of the epistemology of

[9] We might refer to a quotation from Max Weber 'In Greece, for the first time, appeared a handy means by which we could put the logical screws upon somebody, so that he could not come out without admitting either that he knew nothing, or that this and nothing else was the truth, the *eternal* truth that never would vanish as the doings of the blind men vanish.'

[10] P. F. Strawson (1966) in *The Bounds of Sense* (London: Methuen) p. 150 remarks (5.6): 'we should remember that all Kant's treatment of objectivity is managed under a considerable limitation, almost, it might be said a handicap. He nowhere depends upon, or even refers to, the factor on which Wittgenstein, for example, insists so strongly; the *social* character of our concepts, the links between thought and speech, speech and communication, communication and social communities...another name for the *objective* is the *public*.'

science. Philosophers have long been concerned with the investigation and characterization of the categorial frameworks[11] that must be shared if consensibility and ultimate consensuality are to be achieved. Scientists must already agree on a great many things if they are to come to agreement on something more. For the present, however, we are in no position to specify in advance, or to delimit hypothetically, the range of the 'supreme principles' in the cognitive sphere. We shall discover, indeed, that the practice of science, within the reality of human life, individually and collectively, develops and refines such principles (6.7). We cannot even be sure that they can be regarded as objective or *a priori*. A great deal of excellent scientific knowledge depends upon a widely shared human perceptual faculty – the mysterious skill that we call *pattern recognition* (Chapter 3). Yet this faculty does not seem amenable to complete logical analysis (5.3) and it is not perfectly uniform amongst all men. Going back to logic itself, we have no guarantee that the elementary forms to which we are so attached are absolutely universal; they may depend on the 'world-wide' characteristics of the only sentient beings with which we happen to be familiar – mankind with its vocalized languages and other cultural devices.[12]

Nevertheless, having been warned not to take formal mathematical reasoning entirely at its own estimation, we know that a scientific communication is almost valueless unless it is expressed in precise language and has a sound logical structure. These desirable qualities are most readily achieved by using mathematical concepts and symbolism. Quantitative measurement and mathematical theorizing do not automatically generate reliable scientific knowledge, nor are they essential for reputable research in every field of science; but in appropriate circumstances they contribute enormously both to consensibility and to consensuality. The remainder of this chapter will be concerned with the intellectual role of mathematics in science, and with its influence on the strategies of research and on the contents of our knowledge of nature.

[11] E.g. *Categorial Frameworks* by S. Körner (1970: Oxford: Blackwell).
[12] At least we can agree with D. Bloor in *Knowledge and Social Imagery* (1976: London: Routledge & Kegan Paul) p. 97: 'mathematics like morality is designed to meet the requirements of men, who hold a great deal in common in their physiology and in their physical environment'. The 'requirements' of course are those of unambiguous communication.

2.4 *The mathematical machine*

Mathematics is invaluable to science as a strong grammar for didactic discourse; it is the ideal vehicle for precise intersubjective communication. The clarity and universality of mathematical language is of the greatest practical importance. Scientific messages are not normally addressed directly from one particular scientist to another. An essential feature of our model of the 'scientific method' is the library or *archive* to which messages are communicated, and where they are stored for subsequent consultation (6.5). Science is *public* knowledge; it is the contents of this archive, and should not be extended indiscriminately to all that may be known or suspected about the world by all would-be scientists. A mathematically phrased statement in the archive is in the best form for consultation, comprehension, or critical assessment.

A further special advantage of mathematical messages is that they may be symbolized, manipulated and *transformed* according to precise rules, without loss of meaning. Suppose, for example, that we have observed 750 black swans and 250 white ones flying overhead. Our message could equally well have recorded that, 'of 1000 swans, 75% are black and 25% are white'. Or we could have said that 'the ratio of black to white swans is as three to one'. Or we could point out that 'there were 500 more black ones than white ones'. Or it might be noted that 'the probability of any one swan being black is 0.75'. Or we insist that 'if w is the number of white swans and b is the number of black swans, then $b+w=1000$ and $b=3w$'. Or we might even, for some esoteric theoretical reason, be pleased to report the remarkable fact that

$$\sin^{-1}\left(\frac{w}{b-w}\right)=\frac{\pi}{6}$$

– and so on, and so on. The rules of arithmetic, algebra, trigonometry, calculus, group theory, analytic function theory, etc. etc. permit us to generate an infinite variety of unambiguous statements, of varying degrees of complexity, all of which are logically equivalent to the original message.[13]

By academic tradition, mathematics straddles the boundary between the 'Arts' and the 'Sciences'. This ambivalence is justified. There is no doubt that a genuine mathematical conclusion must be completely consensual; a theorem, once satisfactorily proved, must be true every-

[13] H. Putnam (1975) in *Mathematics, Matter and Method* (Cambridge University Press) p. 43, makes this point.

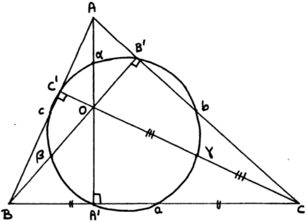

Fig. 1.

building upon precisely formulated axioms, and taking immense care in the proof of each theorem, the mathematician can construct a remarkable edifice of logical consequences. Anyone who has studied elementary plane geometry, exploring, step by step, the properties of straight lines, triangles and circles, must have been delighted to come upon the 'theorem of the nine points circle'.[15] Yet who would believe such an astonishing proposition (Fig. 1) on the basis of empirical verification of a few particular instances or rather vague 'hand-waving' of the kind that is familiar in popular scientific lectures?[16]

The analytical or cartesian method exploits algebraic symbolism as an instrument for automating speech; formal logical operations can thus be followed through in much greater depths than can be managed by the unaided human brain (5.8). Thus, for example, the meaning of a perfectly grammatical sentence, such as, 'The book the man the gardener I employed yesterday saw left is on the table'[17] is almost impossible to grasp verbally. Yet it is child's play to evaluate an algebraic expression of similar structure, such as

$$[B+\{M+(G+I\cdot E)S\}L]I'+T$$

[15] 'In any triangle ABC, the midpoints of the sides (a, b, c), the feet of the perpendiculars from the vertices (A', B', C') and the bisectors of the lines from the vertices to the orthocentre (α, β, γ) all lie on the same circle.'

[16] 'I would like to make a distinction between a proof and a demonstration. A demonstration is a way to convince a reasonable man, and a proof is a way to convince a stubborn man': M. Kac (1973) in *The Physicist's Conception of Nature* edited by J. Mehra (Dordrecht: Reidel) p. 560. All scientists are, of course 'stubborn men'!

[17] Quoted from *Chomsky* by J. Lyons (1970: London: Collins).

by merely following the rules of algebra. The fascinating power of the digital computer rests upon its ability to perform an immense number of elementary manipulations of this kind, in pursuit of the logical consequences of some symbolic formula (6.10).

The essence of mathematical reasoning is that it is perfectly transparent in thin sections, yet intellectually opaque in bulk. A sequence of geometrical theorems or algebraic manipulations cannot be comprehended as a single mental argument; for a long calculation, a computer becomes a 'black box', whose inner workings must be trusted even though they cannot, in practice, be followed through from beginning to end. This lends to the results of a complicated mathematical calculation a peculiar novelty that can give immense prestige and rhetorical force to a successful prediction (2.8).

Suppose, for example, that we have observed one man, and another man, and another man going into an empty room; there will be little surprise at the subsequent confirmation of a prediction that the room contains three men. On the other hand, suppose that we refer to the 'mathematical theory of continuous groups' and point out that the 'octet of irreducible representations of the SU(3) symmetry group is incomplete', and hence that there should be an eighth elementary particle of a particular type;[18] it savours of magic, and is extraordinarily compelling towards acceptance of the theory, when just such a particle is discovered. Logically speaking, these two cases of scientific prediction are almost equivalent; rhetorically, they are poles apart.[19]

It is interesting to note, however, that experience with the application of the theory of continuous groups has taught theoretical physicists to 'see through' such calculations, so that nowadays the logic of such a transformation of the observational data has become almost as obvious as counting sheep (5.4). As science evolves, new theoretical models, newly discovered phenomena, new experimental techniques and new mathematical methods become so familiar that they are incorporated into the 'world map' that every scientist carries in his head (4.4). Deductions whose confirmation would once have seemed quite astonishing become entirely routine, as if no more than elementary exercises in mental arithmetic or 'physical thinking'.

The *creative* role of mathematics in science is balanced by its use as

[18] This refers to the famous prediction in 1961 by M. Gell-Mann and Y. Neeman of the existence of the 'Omega-minus' particle, which was discovered in 1964.

[19] But if the first prediction had been 'falsified' by there being found only two men in the room, we might say that we had evidence of a cannibalistic murder; whereas failure in the second case would just be a sad blow to another elegant but over-optimistic theory!

2.5 *Metaphors and models*

Axiomatization is the final, decadent stage of *theorizing*. A body of quantitative scientific knowledge that has been assimilated to an abstract structure of mathematical relations is no longer fit for human consumption; it provides fodder only for the computer (6.7). Nothing more can be done with it; it has scientific interest only as an instrument for the advancement of learning in other fields. This stage of maturity is exemplified by classical mechanics and thermodynamics, which are no longer questionable in themselves, but which are the foundations for such extraordinary feats of computational virtuosity as the guidance of space vehicles or the prediction of the weather.

Mathematical theorizing is equally unprofitable in the primitive, exploratory phase of a new branch of science (7.3). An elaborate, sharply defined theory conjured up out of fragmentary and uncertain evidence may have its intellectual charms, but can prove a misleading guide to research. This is probably the situation in the field of brain research, where the cerebration of mathematical theories of memory, perception, cognition, etc. has had little impact on the experimental study of the subtle and complex facts. At this stage, the quantitative representation and mathematical transformation of the data is essentially *phenomenological*; correlations and regularities are noted, for themselves, without reference to more general systems of explanation.

Eventually, however, the innumerable messages that pass to and fro between the observers and into the archive must be categorized according to some general ordering principle. Scientific knowledge is incomprehensible – i.e. cannot be grasped by the human mind – unless it can indeed be represented by a few relatively simple and coherent theories. In constructing and manipulating mathematical theories we rely very heavily upon models – so much so, that this word has become a (slightly trendy) synonym for a 'theory' in all branches of the natural and behavioural sciences.[20]

The mathematical models of theoretical physics have been widely studied by historians and philosophers of science. As products of the human imagination, and dominant symbols of successive revolutions of thought, they are of immense intellectual significance. What is the source of their rhetorical power and authority?

[20] The defect of this fashionable usage is that it often lends to a vague hypothesis an unwarranted air of concreteness and logical consistency. The verb 'to theorize' is now conjugated as follows: 'I built a model; you formulated a hypothesis; he made a conjecture.'

By definition, a model is not a complete and faithful rendering of reality. It is no more than an analogy or *metaphor*. It implies a structure of logical and mathematical relations that has many similarities with what it purports to explain, but cannot be fully identified with it. The wise theorist does not assert or attempt to prove the necessary validity or verisimillitude of his model; this is to be discovered by further experience. He says (often in just these words) 'Suppose we think of it this way: what follows?' Even Kelvin's mechanical model of the ether as a medium packed with levers and pulleys, for all its apparent concreteness, cannot have been meant as a serious *description* of empty space. At its conception, a model is no more than a guide to thought, or a framework for a mathematical interpretation of inexplicable phenomena.

What is not always recognized is that a model, being drawn from another field of knowledge than that to which it is to be applied, carries a certain amount of pre-existing understanding of its own properties. The Rutherford–Bohr picture of the atom as a planetary system of electrons in orbit around a nucleus owes its strength not only to the basic principles of classical physics, but also to our familiarity with just such systems in astronomy. The knowledge conveyed in such a metaphor goes much further than the technique of solving a few differential equations; it contains a large element of experiential and intuitive understanding (5.4). To exclude such elements by axiomatization of the model in advance of its application would be sterile, for it would put off the occasion to discover the insights about the *explicandum* for which the hypothesis was originally proposed. It would be difficult, even now, to give a precise logical definition of Darwin's model of interspecific competition as the motive force of organic evolution. This model derived its explanatory power from the fact that its audience were familiar with industrial and social competition, of which many characteristic features could be grasped and compared with biological phenomena without formal demonstration or proof.[21]

These tacit features of a well-conceived model both restrict and enlarge its capabilities. The restriction is advantageous, for it spares us from self-contradictory conjectures. The fact that the model system

[21] This property of a model exemplifies a more general principle suggested by Michael Polanyi (1958) in *Personal Knowledge* (London: Routledge & Kegan Paul) p. 169: 'These powers enable us to evoke our conception of a complex ineffable subject matter with which we are familiar by even the roughest sketch of any of its specifiable features. A scientist can accept, therefore, the most inadequate and misleading formulation of his own scientific principles without ever realizing what is being said, because he automatically supplements it by his tacit knowledge of what science really is, and thus makes the formulation ring true.'

is actually realized in its original sphere implies that its defining properties are coherent and mutually consistent. When, for example, Niels Bohr set up a mathematical theory of nuclear fission, using as his model the break up of a drop of liquid, he did not need to explore his equations in detail to prove that their solutions were unique, mathematically stable, etc.; these properties could be taken for granted from his familiarity with the physics of real liquids. A theory constructed thus, by mathematical analogy, has a 'realizable coherence' which can be grasped intuitively long before it can be proved. The defect of many a theory built around a hypothetical mathematical relationship – for example, the non-linear field equations to which Heisenberg gave the later years of his life – is that one simply does not know in advance whether it will 'work' at all. All too often, the 'interesting' properties for which it was conceived are accompanied by pathological mathematical and physical features which make it meaningless in practice. By confining his imagination to realizable models, the theorist avoids speculative schemes that fail through sheer inconsistency and automatically keeps within the bounds of consensibility.

On the other hand, the imagination is enlarged by a model that is known by experience to exhibit a rich variety of phenomena. When, for example, the distribution of stars in a galaxy is likened to the distribution of molecules in a gas, the analogy may be intended to apply to some very smooth and simple processes of uniform expansion or steady flow. But as the model acquires scientific status and authority, it suggests the possibility of astronomical phenomena akin to turbulence, or wave propagation, which are familiar properties of ordinary gases. Such 'physical' properties, known so well from experience, would suggest themselves long before the corresponding mathematical solutions would have been found in the equations of motion of the galactic system.[22]

In the search for consensus, a 'realistic' model also has major advantages over more abstract theoretical schemes. Precisely because of its internal consistency and tacit limitations, it is more dramatically falsifiable in some vital particular. The eighteenth-century model of heat as an 'imponderable fluid' was effectively falsified by the dynamical production of heat in apparently endless quantities out of the

[22] Once again, Polanyi (*Personal Knowledge*, p. 104) states the general principle '[These] major intellectual feats demonstrate on a large scale the powers which I have claimed for all our conceptions, namely of making sense beyond any specifiable expectations in respect to unprecedented situations'.

Reliable Knowledge

An Exploration of the Grounds for Belief in Science

JOHN ZIMAN

Why believe in the findings of science? John Ziman argues that scientific knowledge is not uniformly reliable, but rather like a map representing a country we cannot visit. He shows how science has many elements, including alongside its experiments and formulae the language and logic, patterns and preconceptions, facts and fantasies used to illustrate and express its findings. These elements are variously combined by scientists in their explanations of the material world as it lies outside our everyday experience. John Ziman's book offers at once a valuably clear account and a radically challenging investigation of the credibility of scientific knowledge, searching widely across a range of disciplines for evidence about the perceptions, paradigms and analogies on which all our understanding depends.

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Cover illustration: *A Philosopher Lecturing on the Orrery*.
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Cambridge Paperbacks:
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ISBN 0-521-40670-6

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