3 Is the scientific paper a fraud?

I have chosen for my title a question: Is the scientific paper a fraud? I ought to explain that a scientific 'paper' is a printed communication to a learned journal, and scientists make their work known almost wholly through papers and not through books, so papers are very important in scientific communication. As to what I mean by asking 'is the scientific paper a fraud?'—I do not of course mean 'does the scientific paper misrepresent facts', and I do not mean that the interpretations you find in a scientific paper are wrong or deliberately mistaken. I mean the scientific paper may be a fraud because it misrepresents the processes of thought that accompanied or gave rise to the work that is described in the paper. That is the question, and I will say right away that my answer to it is 'yes'. The scientific paper in its orthodox form does embody a totally mistaken conception, even a travesty, of the nature of scientific thought.

Just consider for a moment the traditional form of a scientific paper (incidentally, it is a form which editors themselves often insist upon). The structure of a scientific paper in the biological sciences is something like this. First, there is a section called the 'introduction' in which you merely describe the general field in which your scientific talents are going to be exercised, followed by a section called 'previous work' in which you concede, more or less graciously, that others have dimly groped towards the fundamental truths that you are now about to expound. Then a section on 'methods'—that is OK. Then comes the section called 'results'. The section called 'results' consists of a stream of factual information in which it is considered extremely bad form to discuss the significance of the results you are getting. You have to pretend that your mind is, so to speak, a virgin receptacle, an empty vessel, for information which floods into it from the external world for no reason which you yourself have revealed. You reserve all appraisal of the scientific evidence until the 'discussion' section, and in the discussion you adopt the ludicrous pretence of asking yourself if the information you have collected actually means anything; of asking yourself if any general truths are going to emerge from the contemplation of all the evidence you brandished in the section called 'results'.

Of course, what I am saying is rather an exaggeration, but there is more than a mere element of truth in it. The conception underlying this style of scientific writing is that scientific discovery is an inductive process. What induction implies in its cruder form is roughly speaking this: scientific discovery, or the formulation of scientific theory, starts with the unvarnished and unembroidered evidence of the senses. It starts with simple observation—simple, unbiased, unprejudiced, naïve, or innocent observation—and out of this sensory evidence, embodied in the form of simple propositions or declarations of fact, generalizations will grow up and take shape, almost as if some process of crystallization or condensation were taking place. Out of a disorderly array of facts, an orderly theory, an orderly general statement, will somehow emerge. This conception of scientific discovery in which the initiative comes from the unembroidered evidence of the senses was mainly the work of a great and wise, but in this context, I think, very mistaken man—John Stuart Mill.

John Stuart Mill saw, as of course a great many others had seen before him, including Bacon, that deduction in itself is quite powerless as a method of scientific discovery—and for this simple reason: that the process of deduction as such only uncovers, brings out into the open, makes explicit, information that is already present in the axioms or premises from which the process of deduction started. The process of deduction reveals nothing to us except what the infirmity of our own minds has so far concealed from us. It was Mill's belief that induction was the method of science—'that great mental operation', he called it, 'the operation of discovering and proving general propositions'. And round this conception there grew up an inductive logic, of which the business was 'to provide rules to which, if inductive arguments conform, those arguments are conclusive'. Now John Stuart Mill's deeper motive in working out what he conceived to be the essential method of science was to apply that method to the solution of sociological problems: he wanted to apply to sociology the methods which the practice of science had shown to be immensely powerful and exact.

It is ironical that the application to sociology of the inductive method, more or less in the form in which Mill himself conceived it, should have been an almost entirely fruitless one. The simplest application of the Millsian process of induction to sociology came in a rather strange movement called Mass Observation. The belief underlying Mass Observation was apparently this: that if one could only record and set down the actual raw facts about what people do and what people say in pubs, in trains, when they make love to each other, when they are playing games, and so on, then somehow, from this wealth of information, a great generalization would inevitably emerge. Well, in point of fact, nothing important emerged from this approach, unless somebody has been holding out on me. I believe the pioneers of Mass Observation were ornithologists. Certainly they were man-watching—were applying to sociology the very methods which had done so much to bring ornithology into disrepute.

The theory underlying the inductive method cannot be sustained. Let me give three good reasons why not. In the first place, the starting point of induction, naïve observation, innocent observation, is a mere philosophic fiction. There is no such thing as unprejudiced observation. Every act of observation we make is biased. What we see or otherwise sense is a function of what we have seen or sensed in the past.

The second point is this. Scientific discovery or the formulation of the scientific idea on the one hand, and demonstration or proof on the other hand, are two entirely different notions, and Mill confused them. Mill said that induction was the 'operation of discovering and proving general propositions', as if one act of mind would do for both. Now discovery and proof could depend on the same act of mind, and in deduction they do. When we indulge in the process of deduction—as in deducing a theorem from Euclidian axioms or postulates—the theorem contains the discovery (or, more exactly, the uncovery of something which was there in the axioms and postulates, though it was not actually evident) and the process of deduction itself, if it has been carried out correctly, is also the proof that the 'discovery' is valid, is logically correct. So in the process of deduction, discovery and proof can depend on the same process. But in scientific activity they are not the same thing—they are, in fact, totally separate acts of mind.

But the most fundamental objection is this. It simply is not logically possible to arrive with certainty at any generalization containing more information that the sum of the particular statements upon which that generalization was founded, out of which it was woven. How could a mere act of mind lead to the discovery of new information? It would violate a law as fundamental as the law of conservation of matter: it would violate the law of conservation of information.

In view of all these objections, it is hardly surprising that Bertrand Russell in a famous footnote that occurs in his *Principles of Mathematics* of 1903 should have said that, so far as he could see, induction was a mere method of making plausible guesses. And our greatest modern authority on the nature of scientific method, Professor Karl Popper, has no use for induction at all: he regards the inductive process of thought as a myth. 'There is no need even to mention induction,' he says in his great treatise on *The Logic of Scientific Discovery*—though of course he does.

Now let me go back to the scientific papers. What is wrong with the traditional form of scientific paper is simply this: that all scientific work of an experimental or exploratory character starts with some expectation about the outcome of the enquiry. This expectation one starts with, this hypothesis one formulates, provides the initiative and incentive for the enquiry and governs its actual form. It is in the light of this expectation that some observations are held relevant and others not; that some methods are chosen, others discarded; that some experiments are done rather than others. It is only in the light of this prior expectation that the activities the scientist reports in his scientific papers really have any meaning at all.

Hypotheses arise by guesswork. That is to put it in its crudest form. I should say rather that they arise by inspiration; but in any event they arise by processes that form part of the subject-matter of psychology and certainly not of logic, for there is no logically rigorous method for devising hypotheses. It is a vulgar error, often committed, to speak of 'deducing' hypotheses. Indeed one does not deduce hypotheses: hypotheses are what one deduces things from. So the actual formulation of a hypothesis is—let us say a guess; is inspirational in character. But hypotheses can be tested rigorously—they are tested by experiment, using the word 'experiment' in a rather general sense to mean an act performed to test a hypothesis, that is, to test the deductive consequences of a hypothesis. If one formulates a hypothesis, one can deduce from it certain consequences which are predictions or declarations about what will, or will not, be the case. If these predictions and declarations are mistaken, then the hypothesis must be discarded, or at least modified. If, on the other hand, the predictions turn out correct, then the hypothesis has stood up to trial, and remains on probation as before. This formulation illustrates very well, I think, the distinction between on the one hand the discovery or formulation of a scientific idea or generalization, which is to a greater or lesser degree an imaginative or inspirational act, and on the other hand the proof, or rather the testing of a hypothesis, which is indeed a strictly logical and rigorous process, based upon deductive arguments.

This alternative interpretation of the nature of the scientific process, of the nature of scientific method, is sometimes called the hypothetico-deductive interpretation and this is the view which Professor Karl Popper in *The Logic of Scientific Discovery* has persuaded us is the correct one. To give credit where credit is surely due, it is proper to say that the first professional scientist to express a fully reasoned opinion upon the way scientists actually think when they come upon their scientific discoveries—namely

William Whewell, a geologist, and incidentally the Master of Trinity College, Cambridge—was also the first person to formulate this hypothetico-deductive interpretation of scientific activity. Whewell, like his contemporary Mill, wrote at great length—unnecessarily great length, one is nowadays inclined to think—and I cannot recapitulate his argument, but one or two quotations will make the gist of his thought clear. He said: 'An art of discovery is not possible. We can give no rules for the pursuit of truth which should be universally and peremptorily applicable.' And of hypotheses, he said, with great daring—why it was daring I will explain in just a second—'a facility in devising hypotheses, so far from being a fault in the intellectual character of a discoverer, is a faculty indispensable to his task'. I said this was daring because the word 'hypothesis' and the conception it stood for was still in Whewell's day a rather discreditable one. Hypotheses had a flavour about them of what was wanton and irresponsible. The great Newton, you remember, had frowned upon hypotheses. 'Hypotheses non fingo', he said, and there is another version in which he says 'hypotheses non sequor'—I do not pursue hypotheses.

So to go back once again to the scientific paper: the scientific paper is a fraud in the sense that it does give a totally misleading narrative of the processes of thought that go into the making of scientific discoveries. The inductive format of the scientific paper should be discarded. The discussion which in the traditional scientific paper goes last should surely come at the beginning. The scientific facts and scientific acts should follow the discussion, and scientists should not be ashamed to admit, as many of them apparently *are* ashamed to admit, that hypotheses appear in their minds along uncharted byways of thought; that they are imaginative and inspirational in character; that they are indeed adventures of the mind. What, after all, is the good of scientists reproaching others for their neglect of, or indifference to, the scientific style of thinking they set such great store by, if their own writings show that they themselves have no clear understanding of it?

Anyhow, I am practising what I preach. What I have said about the nature of scientific discovery you can regard as being itself a hypothesis, and the hypothesis comes where I think it should be, namely, it comes at the beginning of the series. Later speakers will provide the facts which will enable you to test and appraise this hypothesis, and I think you will find—I hope you will find—that the evidence they will produce about the nature of scientific discovery will bear me out.

The Strange Case of the Spotted Mice

and other classic essays on science

Peter Medawar

Foreword by

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Oxford New York
OXFORD UNIVERSITY PRESS
1996

Oxford University Press, Walton Street, Oxford OX2 6DP

Oxford New York
Athens Auckland Bangkok Bombay
Calcutta Cape Town Dar es Salaam Delhi
Florence Hong Kong Istanbul Karachi
Kuala Lumpur Madras Madrid Melbourne
Mexico City Nairobi Paris Singapore
Taipei Tokyo Toronto

and associated companies in
Berlin Ibadan

Oxford is a trade mark of Oxford University Press

First published as an Oxford University Press paperback 1996

British Library Cataloguing in Publication Data
Data available

Library of Congress Cataloging in Publication Data
Data available
ISBN 0-19-286193-X

10 9 8 7 6 5 4 3 2

Typeset by Best-set Typesetter Ltd., Hong Kong
Printed in Great Britain by Mackays
Chatham, Kent