

R. J. BERRY**Scientific Fraud and Scientific Method:
A Comment on J. N. Hawthorne's Paper**

A recent paper by Tim Hawthorne in this Journal appeals for Christian standards to be applied in scientific practice. One of the examples of apparent scientific fraud quoted by Hawthorne is the work of Gregor Mendel on the inheritance of variation in peas. This may be too harsh: clearly Mendel's results have stood the test of time, and it may have been that Mendel's pea experiments were merely intended as a demonstration of concepts previously established by Mendel, but unreportable for reasons irrelevant to the science. Although we must be ruthlessly honest in our research, we must also recognise that scientific hypotheses do not arise (inductively) from simply collecting and organizing data. The conventional methods of publishing scientific results do not give scope for describing the reason for carrying out the investigation described.

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In *Science & Christian Belief* 5 (1):37–45 (April 1993), Tim Hawthorne describes a number of scientific frauds which have been exposed and draws the moral that fraud is an endemic temptation for anyone seeking wordly success (and none of us are excluded from the temptation). I do not want to disagree or detract from Hawthorne's analysis; my purpose is to dissect the sources of scientific fraud further, and point out that 'massaging results' may have more complex roots than a simple desire for glory.

The problem about the practice of science is that it differs considerably from the idealised version propounded by convention or by philosophers of science. It is the problem which led Peter Medawar to give a BBC talk on 'Is the Scientific Paper a Fraud?'¹ He did not mean 'that the interpretations you find in a scientific paper are wrong or deliberately mistaken, (but) the scientific paper may be a fraud because it misrepresents the process of thought that accompanied or gave rise to the work that is described in the paper'. His argument was that science does not proceed by induction, ordering an array of facts to produce generalisation, but rather (following William Whewell and Karl Popper) by a 'hypothetico-deductive approach' in which the consequences of hypotheses are tested by experiment and then modified or rejected as appropriate. The key question is where the hypotheses come from. Medawar believed that 'scientists should not be ashamed to admit, as many of them apparently are ashamed to admit, that

¹ Listener for 12 September 1963; reprinted in Edge, DO (ed.) (1964). *Experiment, a Series of Scientific Case Histories*, London: BBC, and in Medawar, P. B. (1990). *The Threat and the Glory*, Oxford University Press.

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'.

Medawar followed up his original broadcast talk by inviting a number of distinguished scientists to describe how they had carried out a favourite piece of research (Their contributions are collected in *Experiment* (Edge, DO ed), London: BBC, 1964). It turned out that many of them were like Archimedes, inspired by taking a bath. The conventional idea of science as a painstaking accumulation of evidence is misleading; the collection and analysis of data is a secondary consequence of the primary scientific enterprise, which is the propounding and refining of hypotheses.

It is at this secondary level that fraud usually enters science, but the fraud can be unintentional (and unpremeditated) because the scientist concerned is so convinced of the correctness of his or her hypothesis that the selection of data (or 'results') is biased towards supporting the hypothesis.² A possible example of this is Mendel's work on peas, quoted by Hawthorne as 'not objectively discovered, but approximated to a segregation ratio already known to him from theoretical considerations'. The basis for this judgement was an analysis by R. A. Fisher of Mendel's published results, from which Fisher concluded that the data were too close to expectation to have been obtained from breeding experiments as described.³ Fisher suggested that Mendel may have been deceived by an assistant 'who knew too well what was expected' and/or that the experimental programme was probably a carefully planned demonstration of the factorial scheme which Mendel had discovered earlier.

If Mendel was provided with his data by too faithful a follower, he would not be the first. The most famous example is Paul Kammerer's suicide following the discovery that his confirmation of Lamarckian inheritance had been obtained by the injection of indian ink into his specimens⁴ (Arthur Koestler argued somewhat implausibly that this must have been done by an enemy and not, as is generally accepted, by Kammerer's technician⁵). However, Fisher in later life (when I was a student of his in 1956) inclined to his other possibility—that Mendel had already worked out his so-called laws of inheritance before his pea-breeding work, and that the latter was merely carried out as a demonstration experiment when he already knew the ratios expected from the crosses. The basis for this belief was the knowledge that Mendel kept mice in the monastery at Brunn⁶ and, as Grüneberg⁷ notes, 'it is quite likely that he

2 Isaac Newton was fond of telling how he was led to propose gravity by an apple falling on his head, but he was still convinced of Descartes' vortices at the time of his life when it was supposed to have happened. Moreover his crucial experiment about the true nature of light and colour was, according to Rupert Hall (*Issac Newton: Adventurer in Thought*, Oxford: Blackwell, 1992) 'largely fictitious, newly invented for its present purpose'.

3 Fisher, R. A. (1936). Has Mendel's work been rediscovered? *Annals of Science* 1 115-137.

4 q.v. Broad, W. & Wade, N. (1982). *Betrayers of the Truth: Fraud and Deceit in the Halls of Science* New York: Simon & Schuster.

5 Koestler, A. (1972) *The Case of the Midwife Toad*. New York: Random House.

made some of his fundamental discoveries in the mouse first and that he then proceeded to do things properly and on a large scale in the garden pea by way of repetition and confirmation'. If this is so, Mendel would have been unable to publish his results because there was a monastic edict against the keeping of mice (and other mammals) because of the habit of medieval celibates of breeding them so as to observe their 'voluptuous and libidinous habits'. We shall probably never know the truth because the Brunn Monastery burned most of Mendel's papers after his death.

But as with so much scientific discovery the story is even more complicated. In 1829, more than 35 years before the publication of Mendel's work, Louis Coladon, a Genevan pharmacist, produced 'Mendelian' ratios from controlled crosses of mice differing in coat colour, and was explicit about the 'non-blending' nature of the inheritance of these variants.⁶ It is possible that Mendel knew about Coladon's work, that he confirmed it to his own satisfaction on his own mice, and then carried out a demonstration experiment for publication.

Was Mendel fraudulent? If the above scenario is correct, he may have been economical with the truth in describing the origins of his ideas, but he cannot really be accused of cheating.

The aim of the influential collection of science fraud stories published by Broad & Wade⁴ was primarily to show that research is not a wholly rational and explicitly logical procedure, but one subject to the same confinement and constraints that afflict other professional men and women trying to make their way in the world.⁹ Commenting on it, Medawar asks:

'What lesson should the scientific profession learn? Should we henceforth go around on our guard, doubting and questioning, looking for fraud and misrepresentation with the air of men expecting to find evidence of it? No, indeed not. Listening for a second time to Sir Kenneth Clark's splendid series of television broadcasts on 'Civilisation', I was again struck by the importance that Clark attached to confidence as a bonding agent in the advance of civilization, as it is indeed throughout professional life. Do not lawyers, bankers, clergymen, librarians, and editors tend to believe their fellow professionals unless they have a very good reason to do otherwise? Scientists are the same. The critical scrutiny of all scientific findings—perhaps especially one's own—is an unqualified desideratum of scientific progress. Without it science would surely founder—though not more rapidly, perhaps, than it would

6 Iltis, H. (1924, English translation 1932) *Life of Mendel*. London: George Allen & Unwin. Although Fisher refers to the German edition in his 1936 paper, J. H. Bennet (Preface to Mendel, G., *Experiments in Plant Hybridisation* Edinburgh & London: Oliver & Boyd, 1965) gives reasons for believing that Fisher had not read Iltis's book.

7 Grüneberg H. (1957). *Genes in Mammalian Development*. Inaugural Lecture at University College London. London: HK Lewis.

8 Edward W. F. (1829) *Des Caractères Physiologiques des Races Humaines Considérés dans leurs Rapports avec l'Histoire*. Paris (edited by Grüneberg, *loc. cit.*).

9 See also Kohn, A. (1986) *False Prophets. Fraud and Error in Science and Medicine*. Oxford: Basil Blackwell.

if the great collaborative expertise of science were to be subjected to an atmosphere of wary and suspicious disbelief¹⁰.

This conclusion is very similar to that of Hawthorne's in his article, where he quotes Paul exhorting believers to put away falsehood and speak the truth with their neighbours. The main difference between Hawthorne and Medawar is that the former appeals to Christian virtues whilst the latter depends on what Christians would call creation ethics. In fact there is no difference in the short-term working out of our behaviour; only in the longer term will the consequences become apparent (Mat. 13:30).

Professor Tim Hawthorne responds

I quite agree with all that Sam Berry has to say and was fascinated by his additional information about Mendel. My only reservation concerns Berry's implication that I accused Mendel of fraud. In fact I was particularly careful not to do this, writing as follows: 'Was this fraud on the part of one who was to become abbot of his monastery? Not really, in my view, but only an acceleration towards the truth'.

Sam Berry is Professor of Genetics in the Department of Biology, University College London, London WC1E 6BT. He is President of Christians in Science.

10 *London Review of Books*, 17–30 November 1983; reprinted in *The Threat and the Glory*. Oxford: Oxford University Press, 1990.