Occasional Review

Prometheus the impostor

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Abstract

The problem of scientific fraud has been used to indict the whole system of science. The response of the scientific community has been understandably heated but insufficient. The discussion seems to have reached an impasse as both parties in the dispute share mistaken views. A switch is needed to a framework in which the democratic foundation of the scientific society and the free spirit of scientific inquiry can be preserved.

Introduction

The scientific community has been challenged in the past five years or so by sensational reports on major cases of fraud in science. These were not marginal cases but happened in leading scientific institutions and implicated central figures in the American scientific and medical community: leading scientists, editors of journals, and university administrators. The cases were recently listed and discussed in a book that indicts the whole social system of science.¹ Responses to the problem are not confined to books about it: they include congressional hearings,² the establishment of special professional committees,^{3,4} the recommendation of ethical professional guidelines,⁴ workshops at professional annual meetings (for example, at that of the American Association for the Advancement of Science in 1983), editorials,⁵ and critical reviews.^{6,9} The major target of the current attack is the value of referees and peer review, which is how the wheat is separated from the chaff, the chief mechanism used to render science credible.

As the indictment of the scientific community has been far reaching the official response has been understandably heated. A recent review of the dispute concluded that the criticism that has been

launched against the scientific community is important not only because it is accompanied by positive alternative proposals but also because it elicits hostile responses from the scientific community.¹⁰ It is time to pause and take a careful look at the problem.

This paper presents the problematical state of science in general and of fraud in science in particular in a different light. I hope to diagnose certain mistaken views endorsed by all parties, views that bog the discussion down. By radically altering the framework within which the discussion proceeds it is possible to propose some practical ideas to lift the burden from editors, who have a fear of unwittingly becoming party to fraud.

Prometheus as a child

Jaroslav Hašek, the lamentably neglected Czech writer, has written a short story about the origin of the worship of fraud.¹¹ Baluska was sent by his father to purchase a few links of sausage. On his way back he engaged in a street brawl. The package dropped from his hands and two links of the sausage disappeared into the mouth of a hungry dog. Baluska's father could not believe this story and suspected that his son had eaten the sausages and would not admit to his sin. The father refused to punish Baluska before the truth was disclosed. Baluska, however, stuck to his version, so his father made him kneel on dry peas to hurt his knees and denied him access to food. Hours later, tortured by the smell of dinner, hungry Baluska confessed, without hesitation and without blushing. His father thereupon graciously whipped him. Baluska humbly joined his family at the dinner table. On retiring to sleep he thanked God for the revelation of the art of lying.

William Broad and Nicholas Wade, two established science reporters, tell a long story about the origin of the worship of scientific fraud.¹ It constitutes the centre of the current debate on the matter. Briefly, it relates to laboratory "masters" and their "apprentices": the masters, the political scientific elite, play Baluska's father; the apprentices play Baluska.

Broad and Wade claim not only that the lesser lights prove false but that the great men of science such as Galileo and Newton are also corrupt. "The roots of fraud lie in the barrel," they conclude, "not in the bad apples." That is to say, the conventional ideology of science, which denies the existence or importance of fraud, makes its adherents extremely susceptible to communicable deceit. In the opinion of Broad and Wade the very denial of the existence or importance of fraud and, more generally, of irrationality—that is, the pathological aspect of rationality—in the process of scientific inquiry

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infects the scientific community as a whole by the pathological aspect of morality. Is that so? Are we all Baluskas?

Broad and Wade claim that much. They also observe that one cannot deny their charge without thereby denying, however unwittingly, the creative aspect of science. To admit creativity, they say, is to give unqualified acceptance to fraud as inherent to the process, because deceit is initially indistinguishable from innovation. In other words, we cannot engage the creative muse without also contracting the deceptive demon. But this is a risk: the muse may depart, leaving us with the demon alone. Is there any guarantee that the muse may have the last word?

Broad and Wade think that the guarantee exists. Though fraud and creativity are initially indistinguishable, time tells the one from the other. There is a free market of ideas, and the "invisible hand" of Adam Smith will guarantee the success of creative innovations. But how will deceit fare? Can it survive in the imperfection of the market? Does not the exclusiveness of science guarantee the survival of fraud? They quite high handedly deny this, maintaining that the "invisible hand" hypothesis is supplemented by the ad hoc hypothesis that the market is assisted by an "invisible boot." The humble will live to see the demon graciously kicked out. They will then receive purely objective truths. Meanwhile, however, kneeling patiently on dry peas and collecting what they consider to be "facts," scientists should faithfully mumble Sir Francis Bacon's immortal aphorism: "Truth is the daughter not of authority, but time."

It may be worthy of notice that Broad and Wade are not the only reporters sensitising us to the existence of fraud in the community of science, nor are they alone in their criticism that the procedures of science contain inherently subjective and irrational components. They represent, however, an extreme case of popular scientific journalism, which presents science as a whole as inherently containing a fraudulent strain throughout. They claim that their crusade is justified: their thesis is anchored both in empirical evidence and in a conceptual analysis of the real context of science. As their contribution has merit their argument and proposed solutions are taken here as a challenge to the canons broadly accepted in the scientific community.

The argument at times may strike us as valid, especially because the reports of Broad and Wade on current cases of fraud are known to be true and their analysis reinforces the prevailing portrait of the scientist as a collector of facts. They claim that as pure facts are non-existent the collector of facts undertakes a Sisyphean labour.

I have no quarrel with the contemporary case studies of Broad and Wade, and their history is neither here nor there. Most scientists will share their philosophical premises about science as the collection of "facts." Their conclusion, however, seems erroneous. Scientists cannot agree with them that scientific inquiry is such a vague process that nobody knows sheer fraud from genuine scientific innovation and that truth is obtainable not by some rational process but merely and solely by the forces of history. Can their premises be accepted yet their conclusions rejected? If not, how should we modify the premises?

Prometheus as an impostor

The history of the philosophy of science is the history of the question, Can we avoid error, fantasy, and deceit?¹² Sir Francis Bacon, the early seventeenth century prophet of modern science, believed that error is completely and systematically avoidable. He viewed error as rooted in wilful, irrational ideas that are avoidable. He implied that the erring scientist is insincere about giving up his preconceived ideas; he is thus no better than a common liar. Hence Bacon prescribed for the scientist a strict moral training aimed at epistemological purity.

From the origins of the scientific procedures let us move to the present. Paul K Feyerabend and Thomas S Kuhn are leading contemporary philosophers, in whose light Broad and Wade present their case. These philosophers acknowledge that Bacon's view was the cornerstone of modern empirical science, yet they dismiss it as false. Feyerabend replaces it with the pragmatist's belief that "anything goes," including black magic and bent knees, and he prescribes moral training as a cure for epistemological anarchy.¹³ Deceit is justified if and when it works. Kuhn replaces Bacon's view with his own faith in the authority of leading scientists as the touchstone for the rational order within science, and he prescribes moral training as a cure for epistemological heteronomy.¹⁴ Deceit is justified if and when it is ordered by the scientific elite—except that then, by definition, it is deceit no longer.

Broad and Wade follow Feyerabend and Kuhn when they say that error is necessarily unavoidable. They also note that authority, no less than anarchy, leads to fraud. They question whether we could avoid both these extremes as well as the deceit they bring about. Baconism, which equates mistakes with lies, has pushed Broad and Wade to epistemological and moral despair, so they look for a political way out: they put their trust in the free market place. Their pessimism is excessive, and so their remedy is redundant.

If we assume that Broad and Wade are right in viewing Feyerabend's cynicism as well as Kuhn's fiduciary authoritarianism as counsels of despair, and if we draw no comfort from their faith in the free market's "invisible boot" and do not share their faith in the judgment of history, what then are we to do?

Before answering this question I wish to review Broad and Wade's positive proposals. Broadly, they endorse Adam Smith's utopian vision of society. They suggest an ever fiercer competition than that currently in evidence. They want the competition to be enhanced by making peer reviewers use standards higher than those currently in use—we should judge scientists by the quality not the quantity of their publications. This, however, may endanger the complete freedom that Broad and Wade wish them to enjoy. They add that freedom must also be given for the boots to kick out of the market the pathological aspect of rationality. As they claim that in science the irrational, the immoral, and the innovative are initially indistinguishable their prescription seems to me to be plainly cynical; it is a medicine worse than the disease.

To let the disease go unnoticed, however, is no less cynical. This, of course, is why the criticism raised against the conceptual and social frameworks of science must be taken as seriously as possible. The intense emotions raised by the current debate attest to the entrenchment of the problems. They are rooted in the ethic of science, which was prescribed by Bacon on the basis of the old and defunct methodology that assumes the existence of pure facts for science to collect. This methodology has gained much popularity since its introduction by Bacon and is still shared by many, including Broad and Wade as well as their critics, who admit that facts are not so pure. The immediate deleterious effect on science of this inconsistency is that the debate becomes bogged down.

It is worth noting that because Bacon could not allow science any mistake he had to view the erring scientist as a common liar. Similarly, because Broad and Wade cannot allow Newton his mistaken view of science they deem him to be an irrational thinker, a liar no less, akin to present day deceitful masters and apprentices.

Back to these deceitful laboratory masters and apprentices; should we or should we not consider them to be mistaken rather than criminal liars? This must be decided in civil court. Whether they should be exempt from responsibility as severe cases of pathological rationality should also be decided in court. Ethical committees and journalists should not serve as legal bodies; they may be called on, as psychiatrists often are, to serve as witnesses but never as judges.

Prometheus as a young man

Actual cases of scientific fraud are used by Broad and Wade as evidence for their argument; they criticise the process of review by referees and demand tighter eliminatory control and monitoring mechanisms. Likewise, they use the cases as examples of the unhelpful authoritarian and competitive working relationships in prestigious medical laboratories and demand free marketing of high quality research. They also notice, however, the irrational psychological factors that come into play—for example, Oedipal fears and wishes—and claim that these factors cannot be eliminated from the living context of science. The scientific establishment used the same evidence as grounds for the establishment of ethical committees within the scientific community. Will this do?

I do not think so. The current referee and peer review systems are in need of improvement. We all know that perfectionism itself causes many problems. It is a defensive manoeuvre against the fear of making mistakes; the mistaken view of mistakes as lies makes this defensiveness imperative. Yet error, says Sir Karl Popper, is unavoidable¹⁵; it can be rational and, when responsibly made and honestly reported, is not even culpable. The exposure of error to empirical tests, he says, is the practice of scientific inquiry, which leads to scientific progress. This is precisely why Einstein could view some of his revolutionary theories as mistaken yet not hesitate to publish them. He viewed them as the best existing approximation for the truth.¹⁶

The first error to correct, then, is Bacon's identification of all error with fraud.

It follows that we need not guard science against error, only against fraud. The guardian of science against mistaken hypothesis is not the referee system or ethical committees but empirical experiment and the demand that each experiment be independently repeated before it be publicly and officially acknowledged.¹⁵ The guardian of society against fraud, however, is different. Usually fraud is a matter for the law. Ethical committees cannot replace laboratories that perform repetitions of scientific experiments and normally should not replace the system enforcing the law. Their function would be better restricted to the detection and elimination of suspected dishonesty with intent to defraud. As scientific deceit is often perpetrated with public money some such cases, concerning senior as well as junior researchers, ought to be referred to the court. This seems to me to be in line with Broad

and Wade's trust in the open market, yet more in line with the part played by the democratic legal institutions in the market of a democratic society.

The system of modern medicine is open to public monitoring and criticism. Such mechanisms are served by both ethical committees and normal court procedures. The system of science may follow the example of medicine. Academic freedom may well be maintained by keeping thieves, policemen, and judges where they belong—outside science.

The second error to correct is the identification by Feyerabend and Kuhn (as well as by Broad and Wade) of the freedom of the traditional scientific community with that enjoyed by members of closed clubs.

It follows that we need to guard science against the abuses of professional freedom by undemocratic procedures. Social procedures, however, are a matter for social institutions. In so far as science is of interest to society as a whole its procedures and operation must come under lawful public scrutiny. Economic freedom is not sacrificed or abused because of tax evaders, so academic freedom need not be sacrificed or abused because of truth evaders.

In so far as science is of interest to the scientific community its procedures and operation must come under professional scrutiny. This calls for professional standards for quality. Do high standards and freedom go together? After all, high standards must often be enforced. If so, could science afford less than high standards as a lesser evil between anarchy and authoritarianism?

Joseph Agassi says that standards within science or anywhere else can be democratically endorsed; they can be rational when publicly discussed, and then their enforcement serves a regulative function and cannot be condemned as authoritarian. He says that the exposure of scientific standards to public critical discussions, and even to social tests, can be part of the practice of scientific inquiry and may lead to scientific progress.¹⁷ This is precisely why Arnold S Relman, the editor of the *New England Journal of Medicine*, could insist on high standards for scientific quality yet admit failure when the journal failed to meet them, and then unhesitatingly publish retractions of the fraudulent papers and open a discussion on the matter—viewing his editorial critical monitoring as the best approximation to control that is still congruent with scientific freedom.⁵

We know, however, how difficult it is for an individual scientist to carry his criticism through the barrier of members of the busy scientific elite who help monitor and improve scientific reports. The elite know all too well how to make the price of complaint high.¹ If Kuhn is right and science is a social order then ombudsmen could be appointed, as they are in other orders, such as armies. They had better be elected, however, and they should help individuals initiate the necessary processes. Such elected ombudsmen could serve a social function as moral referees, for individual scientists as well as for editors, in line with the critical free spirit of science.

The third error to correct is Feyerabend's and Kuhn's identification of equality with anarchy.^{13,14}

It follows that we need not guard science against anarchy, only against inequality. The guardian of science against anarchy is not the referee system, fierce competition, ethical committees, the law, or the scientific elite but the empirical scientific method when implemented in a democratic, open, scientific community. The members of the scientific elite cannot be held accountable for the scientific community as a whole, nor should they control it. Their social function therefore, would be best restricted to the monitoring of excellence, the education of young scientists, and the monitoring of scientific reports and proposals in the light of democratically accepted standards. To their last task first.

Many reviewers do their job voluntarily. It is a highly esteemed job, standing in the gate of science. The publication of data, however, does not mean that they are scientific; data cannot count as scientific until they are independently repeated. This methodological rule was endorsed by the Royal Society of London when it was founded in the mid-seventeenth century and has been accepted by the scientific community ever since. Reviewers do not and should not use laboratories in which to repeat the reported experiments, nor do editors. They pass judgment on the papers and determine whether they stand up to accepted standards of scientific methodology—whether they count as potentially scientific. The scientific community, including the reviewers, is called on to challenge the data after the report is published. Responsible editors as well as scientists should readily publish well established refutations as well as retractions of previously reported data.

Scientific methodology strengthens here the demand for scientific freedom and prescribes that error and fraud can not be completely eliminated by the process of review. Indeed, when Popper's criticism of Bacon is endorsed then standards for what counts as scientific are not necessarily identical with standards for what should count as the truth.¹⁸

The fourth error to correct is the prevailing identification of standards for publication with standards for acceptance as scientific.

It follows that we need guard the process of scientific publication not against unscientific methodology, but against the imposition by reviewers of their own "best" approximation for the truth. That is to say, problems often arise when a reviewer uses his own laboratory and fuses its boundaries with

the process of review; he may attempt to repeat the experiment and claim the proposed data to be either refuted or unrepeatable. If he does so he oversteps the limits of his commission.

The guardian of the process of scientific publication against unscientific methodology is not fierce competition, ethical committees, or the law but the empirical methodology of science when critically reviewed by highly respected peers, not in their position as members of an unquestionable elite but as knowledgeable and responsible scientists. The guardian of the process of publication against abuse by the elite is different. In democratic societies abuse of control is a matter for ethical committees and for the law.

We have come full circle. Let me recapitulate. Bacon's implicit identification of error with wilful irrationality, which led to the demand of moral training in epistemological purity, is mistaken and led to the erroneous identification of standards of scientific publication with standards of science. Feyerabend's and Kuhn's recommendation on moral training in epistemological anarchy or heteronomy, respectively, is rooted in their mistaken identification of equality with anarchy—a mistake that unwittingly justifies fraud. The analysis by Broad and Wade and their proposed solutions to the problem of scientific fraud are distorted because they are rooted in the inconsistent concoction of the ideas of Bacon and Smith, Feyerabend and Kuhn. It seems that their errors can be corrected and the spirit of scientific freedom still be preserved when Popper's view of scientific method and Agassi's view on a scientific democratic society are endorsed and understood to view science as operating within the larger, lawful context of a democratic society.

Yet problems still remain. Can fallible science, practised by members regarded as equals, guarantee progress? We can only hope so, if progress is not viewed as rooted in mutiny. Progress is often rooted in challenge to tradition. Scientific progress often constitutes the exposure of error and the experimental challenge of rival hypotheses. Social progress relies on the exposure of social ills, such as mistake and deceit, by the challenge of critical discussion, rival views, or rival standards or procedures. Challenge of views, however, does not necessarily challenge the social or scientific standing of their holders. Yet very often the elite is defined by the paradigm—the current view—and when the paradigm is criticised the elite may well feel threatened.

This, I think, is why Agassi insists that only democratic science is compatible with smooth progress.¹⁷ The reports on scientific fraud and the rather quick introduction of institutional monitoring measures bear witness to the democratic aspect of the present scientific system.

The fifth error to correct is the identification by Kuhn and Feyerabend of progress with revolt.^{13,14}

It follows that we need not guard science against stagnation, only against revolt. For the guardian of science against stagnation is the empirical nature of scientific experiment; when implemented democratically it allows for peaceful changes of paradigm. The guardians of society against revolt, however, are moral education, open social institutions, and the law. In the case of science the law should intervene only in extreme cases of law breaking. Otherwise, as far as interpersonal relationships within scientific institutions are concerned, the field is left open for institutional monitoring and control and for scientific moral education.

We are back to our initial problems: can we avoid revolt and stagnation in science, and also error and deceit? That is to say, can we avoid both moral evil and psychological—intellectual as well as emotional—impoverishment? Can we avoid the demon and still engage the muse? Can we educate young scientists to be brave and fair expositors of mistakes as well as autonomous? Can there be hope for Baluska?

The history of the philosophy of moral education is the history of the vicissitudes of the myth of paradise, of Adam and Eve and the serpent. Temptation led to the abuse of free will, yet temptation furnished the search for knowledge. Since then science and morality have been viewed as conceived in sin, rooted in the temptations of sex and aggression that inevitably lead to revolt.

The modern respectable transformation of this myth is Freud's theory (or myth?) of the Oedipus complex. According to Freud, the wish to search, at the root of scientific activity, might be viewed as the wish to expose, rooted in sexual and aggressive instincts. On the Oedipal level, the wish to expose evokes guilt. The guilt thus aroused is the cornerstone of human morality.

If we apply this to scientific education it follows that the scientist, the creative expositor of truth, can either be honest or cover up and mangle about his temptation and sin. Guilty he always is and remains, of course. This, I think, is why Bacon prescribes that temptation must be maximised along with self restraint. If the scientist does not force his whims on mother nature and put her in chains then he will not have any reason to make mistakes or lie. Mother nature will then disclose her secrets to her well behaved suitor.

When the views of both Freud and Bacon are endorsed the impatient scientist is viewed as nothing but a clear case of mere malingering.

The sixth error to correct is Bacon's (and Freud's) identification of uninhibited and exciting search with temptation and sin, which in science leads

to the identification (criticism) of scientific ideas with (an attack on) the scientists who hold them.

It follows that we need not protect science against any tempting idea, only against immoral scientific practices. For the guardian of science against wrong ideas is, I repeat, its empirical method democratically implemented. The guardians against immorality are democratic institutional monitors and moral education. If scientists are autonomous, as Popper says they are,¹⁶ then democratic scientific institutions open to legal scrutiny should suffice for them. If not, the whole scientific enterprise is in jeopardy no matter how hard we try to correct its erroneous framework by tightening its standards.

Leading scientists claim that students, especially doctoral and post-doctoral fellows, must be closely supervised and morally educated.¹⁹ They thereby unwittingly imply that the young starters are the rotten apples in the barrel. They ignore the simple biological fact that fresh starters, like apples, get older and rot only when they mature well inside the barrel. So the question is, Can we expect the young scientist to be creatively rational and morally to improve under the influence of his older teachers? Can Baluska trust his father to guide him on the road to autonomy?

Conclusion

This is why I cannot endorse Broad and Wade's analysis of the problem of scientific fraud and their proposed solutions. Their report on such matters as deceit in science is important: it presents a piece of journalistic adventure and sensitises us to the matter. They start, however, by giving up on scientific rationality and end by losing sight of morality. Neither can I accept the current response of the scientific community to the critique of the social system of science. It is insufficient. Every analysis of the problematical state of science and every proposed solution to the matter of scientific fraud ought to take human fallibility as unavoidable and inquire under what methodological, moral, social, and legal conditions error can be conceived of as rational or responsible, or both. Moreover, such analysis ought to exhibit respect for and sensitivity to both the democratic foundation of the scientific society and the autonomy of the individual scientist.

TIMES PAST

Junior doctors were sometimes awkward long before they invoked the modern concept of units of medical time. There is an example of a stand taken by some young doctors many years ago that moved precariously close to industrial action. This was on the first day of March 1911 at the Royal Infirmary, Sheffield, a teaching hospital of one of England's most senior provincial medical schools. Six of the resident house staff wrote in protest to the chairman of their board as follows: "Having been informed that the honorary staff of this institution have proposed that the senior resident officer be granted eight or 10 weeks' leave of absence in order to read for an examination and that during that period, we, the other officers, shall perform the duties of the posts which are immediately senior to these we respectively hold, only to return to our present positions at the end of the time; and having been further informed that this proposal has been passed by the board of management; we the undersigned, beg most respectfully to decline to perform the said duties."

Clearly the board had recognised that study leave for the senior resident officer was a desirable thing, but had offered nothing to placate the remaining house staff who would have to act up during the absence—or so the house staff believed. What happened then? The solicitor to the board wrote on 22 March that he could not advise the board to consider legal proceedings against the doctors as their refusal had not contravened the wording of their contracts. Behind the scenes, however, a policy of retribution seems to have been pursued. The evidence for this comes from separate letters that have survived, written shortly afterwards, expressing "sincere apology" from three of the six house staff who had sent the original threatening letter.

A study of *Who's Who* over the subsequent 50 years gives us no information about the further careers of the six doctors who protested at the rumour of extra work. The senior resident officer who asked for the unusual privilege of study leave and was consequently the cause of all the commotion had a distinguished career. He features with honour in *Who's Who*; he became a jewel in the local medical scene; he became vice president of the Royal College of Surgeons of England and was knighted. He was Ernest Finch.

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Although no statistical significance can be drawn from this story, there may be a useful moral to be learnt from it today. But is this moral that we should accept extra duties with equanimity, and even joy, or is it that we should hammer our employers to provide yet more study leave?—H T SWAN, honorary lecturer in the history of medicine, Sheffield.

A woman in her 60s had three episodes of bronchitis and stopped smoking after 50 years of addiction to the habit. Previously she had a normal sleep pattern but she has developed distressing insomnia. There are no other symptoms of withdrawal and her weight remains the same. As tobacco is classified as a narcotic alkaloid should supportive sedation at night be considered and if so for how long?

Minor disturbance of sleep is one of the many symptoms that characterise the tobacco withdrawal syndrome, which the American College of Psychiatrists has now classified as a psychiatric disorder,¹ along with the syndromes of withdrawal from other drugs of dependence. It is unusual, however, for the insomnia of tobacco withdrawal to persist for more than a week or two, or to continue after more prominent symptoms such as craving and irritability have subsided. Because it is only transient, insomnia due to tobacco withdrawal does not require treatment with sedative drugs. Possibly this patient is suffering from depression, which may be unrelated to her giving up smoking. This would be especially likely if the insomnia is characterised chiefly by early waking. If there is evidence of depression I suggest that her insomnia be treated with amitriptyline 25-50 mg at night in the first instance rather than with a benzodiazepine, which could give rise to dependence and would not help depression.—M A H RUSSELL, senior lecturer and honorary consultant physician, London.

- 1 American Psychiatric Association. *Diagnostic and statistical manual of mental disorders*. 3rd ed. (DSM-III). Washington: APA, 1980.