

# Para fraud in Biology

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**Keywords:** para fraud, fraud, scientific integrity

**ABSTRACT:** *The concept of para fraud is described as “illogical or improper behaviour towards other peoples’ views or publications,” and 19 different kinds of common practices coming under this heading are listed. Ways of combating it are suggested.*

## INTRODUCTION

The concept of fraud is well understood, widely documented and generally deprecated.<sup>1-5</sup> ‘Para fraud’ is different. It may be defined as “illogical or improper behaviour towards other peoples’ views, or publications, which distort attempts to elucidate truths.” Para fraud is rarely admitted, but potentially very influential. There has been little literature and no research on para fraud, or its total influence on current beliefs. Therefore, it is difficult to document, and one cannot mention names for legal reasons. This paper is intended to give substance to the concept of para fraud in order to explore its extent, and to list some measures which may be taken against it. A preliminary list of these practices has been published.<sup>6</sup>

### Assumptions of this paper

These are:

- A. Research workers seek the truth as they see it, and they have a right to try to persuade their colleagues of their opinions.
- B. Knowledge advances by questioning accepted paradigms.
- C. Research workers and academics, in general, have a duty to enter into dialogue with all serious interested parties about: any research or views which they have published and are central to their disciplines; any material about which they have taught students or spoken publicly; evidence from other authors which they quote in support of their views.

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### The different practices

The following is a list of practices which constitute parafraud; each practice is followed by suggestions about how it may be diminished:

1. *Not publishing results which contradict one's own hypotheses or beliefs, or those of one's superiors or providers of support.*

These practices are so widespread that they are accepted as normal and proper. Obviously, they are totally unscientific because they represent publication of only a proportion of data—that is, those data selected to support the research worker's own thesis. Over the years, I have observed these practices by researchers in many laboratories. Selected use of results is statistically unacceptable and lacking in integrity, but rarely reported.<sup>7,8</sup>

*Prevention* is essentially an educational process.

The scientific community must first admit and condemn the practices, and then try to eliminate them by active measures.

2. *Not distinguishing between hypotheses and findings.*

Many hypotheses have been accepted for so long that they are regarded as 'truths', and are elevated to the status of findings. A few examples are: the Davson-Danielli model of the cell membrane; the Singer-Nicolson fluid mosaic model of the cell membrane; the concept of 'active transport' across a cell membrane; the Dale hypothesis that one kind of synapse contains one particular transmitter; the receptor hypothesis by which transmitters, hormones, drugs, toxins, and antibodies are believed to act; the subcellular localisations of biochemical activities; the chemical theory of transmission across motor end plates and synapses.<sup>9</sup>

A large volume of evidence has been collected which is compatible with each of these hypotheses, but much of it is either compatible with other hypotheses as well, or is interpreted by reference to the original hypothesis—and therefore cannot be considered evidence for or against it. Valuable evidence must critically favour one of two or more opposing hypotheses.

Furthermore, the validity of a hypothesis is measured by the accuracy with which it forecasts correctly the results of future experiments. Popper pointed out that a finding was of little value, unless it could be disproved, and that it is the duty of scientists to try to disprove their own hypotheses.<sup>10</sup>

*Prevention* of this practice is also essentially through education. Research workers should be trained to recognise immediately the differences between the relative epistemological values of hypotheses and findings.

3. *Not carrying out control experiments and appropriate calibrations.*

There are many areas of biological sciences in which experiments involving complex and energetic procedures have been carried out for decades without any attempts being made to examine the effects of the procedures themselves on the preparations. These include study of the effects on *final measurements* of: killing the

animal; adding unnatural reagents or natural reagents in unphysiological concentrations; homogenising tissue; centrifuging it; post mortem changes until the tissue is processed; complete recovery of the compounds being measured; effects of histological reagents on the immunological reaction of antibodies with antigens in immunocytochemistry; calibration of ion-selective electrodes, photometers, fluorimeters; protein measurements in the presence of all the substances originally present in the tissue.<sup>9,11,12</sup>

I have urged the necessity of doing the control experiments and calibrations over a period of 25 years; and over this period, no one has claimed that they *have*<sup>11-16</sup> been done. I surmise that research workers in biology have either assumed that: (a) their predecessors, supervisors or seniors have already done them; (b) they can publish their experiments without the controls, because such controls have never been carried out hitherto, so that the referees of journals cannot insist that acceptable manuscripts must include them; (c) teachers have not taught students that the validity of experiments depends upon carrying all relevant control experiments and calibrations; (d) an unfortunate possibility is that biological research workers, who theoretically accept the crucial importance of control experiments, do not practice the consequences of that acceptance. It is difficult for educated laymen, or naive tyros to believe that the results of such uncontrolled and uncalibrated experiments have been accepted and interpreted as providing scientific insights by research workers all over the world. In making this assertion, I hereby invite any research worker in the world to send me references to control experiments and calibration systems which have been carried out comprehensively for any of the procedures mentioned in the publications listed above. In my opinion, failure of interested parties to either find such experiments in the literature, or carry them out themselves, is a measure of the parlous state of biological research at the end of the twentieth century.

*Prevention.* Paradoxically, persuading the biological research community to do all the important control experiments and calibrations has been totally unsuccessful,<sup>11-17</sup> but one must keep on trying.

#### 4. *Failure to identify assumptions inherent in experimental procedures used.*

Virtually every experimental procedure implies important assumptions, often unrecognised by practicing research workers. Unfortunately, an experimental procedure – like a chain – depends for its strength upon every single assumption, including the weakest.<sup>9,11,17</sup> Of course, failure to recognise an assumption does not mean that it is not necessarily implied. It is useful to give a few examples. The use of subcellular fractionation to find out the localisation of a particular biochemical activity in a cell depends upon the assumption that the procedure, including homogenisation and centrifugation, does not alter the enzyme activity or location. This overall assumption encompasses 24 separate assumptions, which I have listed elsewhere.<sup>17</sup> Some of them are contrary to physico-chemical laws. The accuracy with which a histological section demonstrates the structure of a tissue depends upon the assumptions that dehydration twice, embedding, cutting and staining tissues, do not change their appearance or dimensions significantly, whereas experiments over a

century have shown that they do. Several assumptions are inherent in the use of the electron microscope on biological tissues; here one must highlight the assumption that the heavy metals used for staining have an affinity for every structure in the tissue. A fourth example is the widespread assumption that a tissue constituent after extraction with powerful physical agents and chemical reagents behaves the same when it is purified than when it was present in the parent tissue.

This practice can be *prevented* by editors and supervisors requiring research workers to list all the assumptions inherent in the procedures they use, and to examine their warrantability, theoretically or experimentally.

5. *Unwillingness to preserve raw data and permit them to be inspected.*

In very few laboratories, raw data are preserved and are open to anyone who wishes to examine them, whether or not the person examining them is sympathetic or hostile to the views of the experimenters. Traditionally, laboratories, universities and institutes, own the data of their research workers. The desire to publish results implies that the authors wish to give the information to the scientific world. Therefore, the authors should be prepared to allow other serious workers to examine the raw data upon which the publications are based. The idea that the data upon which a paper is to be published are confidential seems to me to be illogical, except possibly in a commercial or industrial context.

In the past, doctoral students, junior research workers and technicians, used to present their raw data to their seniors, say, once a week, and the seniors looked at them critically.<sup>18-20</sup> However, the increasing size of research groups since the Second World War, has often made this impossible. Furthermore, whereas until the computer revolution, research workers wrote down their own raw data, nowadays many instruments, such as spectrophotometers, fluorimeters and even microscopes have computer outputs, which may be 'edited'. Of course, changes in the output for arbitrary reasons is straightforward fraud,<sup>21</sup> not parafrad.

In the interests of the reproducibility of their research work and scientific integrity, I would suggest that all raw data should be preserved for at least 20 years in the research unit or in the archives for examination by any interested persons.

6. *Unwillingness to discuss experiments.*

Many junior research workers are unwilling to discuss their experiments or the theories upon which they are based, probably because they assume that their supervisors know the subjects well enough and feel that their studentships might be at risk if they asked penetrating or awkward questions. Sometimes, the research work is paid for by industrial bodies on condition that the work is not discussed with outside persons. One can understand the student's reticence under these circumstances, but any unwillingness of their supervisors to allow them to discuss their projects freely, or of pharmaceutical companies to permit free exchange, is to be deprecated. The former is counter-productive in the sense that it prevents free thought. The latter represents the person who is paying the piper calling the tune. This may also cause distortion of truth.

Much more serious is the unwillingness of senior research scientists to discuss

their published experiments, theories and views. Often speakers at learned society meetings evade proper questions. Frequently, there is no reply to a letter asking an author about data, or taking issue with any conclusions. The brief answer without references, or the briefer reply making uninformative generalisations is only slightly better. In general, unwillingness to provide data or discuss published work should be totally unacceptable, and not tolerated by the scientific community.

It would seem to me that the only way of assuring this form of scientific integrity is to require academics and research workers to sign a code including the willingness to discuss their publications and research work.<sup>22</sup>

7. *Omitting details of experiments in publications.*

This is sometimes done so that competitors cannot catch up with a piece of research before the research worker has exploited it maximally. It is certainly true that if one is a single research worker or working in a small group, one could easily show one's technique or results to the head of a large laboratory who could instruct assistants to repeat and submit them for publication within a few days. Leaving out details is a way of avoiding this. Unfortunately, achieving priority for the discovery of new findings, whether by an individual or a company, is one of the strongest motives of the modern research worker.

Since these omissions arise from ambition or commercial secrecy, it is difficult to know how to combat them. One way of diminishing the necessity for secrecy is to carry out research in fields which are not popular.

8. *Ignoring findings which predate or are inimical to one's own views.*

This practice is fairly common. It may be because: (a) one does not trust the quality of the research as reported; (b) it shows that one's own experiments are of bad quality; (c) one's supervisor does not want one to quote the said research; (d) one has missed the reference in the literature before one had written the paper. I know several research workers who have measured the concentrations in blood or brain of constituents; these had been reported previously to go up, remain the same, or fall, under particular conditions. These research workers found that particular constituents went down. They then carried out data searches only for those previous papers in the literature which had shown the constituent to fall. The discussion sections in their papers were then reviews of how their results fitted in with the latter publications, not with all those showing the complete range of results. The particular research workers did not consider that they were doing anything wrong.

This is also an educational problem. Please see 1.

9. *Admitting mistakes.*

Anyone can make mistakes in good faith in respect of controls, calibrations, calculations, errors, statistics, presentation of results, and interpretations of results. These can easily be missed by those referees who do not read the manuscripts with sufficient attention. After a paper has been published, the author's reputation may be badly damaged by admitting mistakes. Even if the author submits a correction for

publication some months later, it is unlikely that a reader would be able to connect it with the original publication, and thus modify the memory of its conclusions:<sup>23,24</sup> It is also rather difficult to publish corrections. Many research workers feel that their honour is threatened when someone else draws attention to mistakes in their papers. How often do they delete from their list of publications in their curricula vitae a paper in which they have made a significant mistake?

This requires more intellectual honesty and also a willingness of prestigious journals to publish corrections from authors and others.

10. *Refusing to accept intellectual responsibility for papers used to support one's own findings.*

It is quite common for one research worker to cite as evidence in support of their theses findings from another specialised research worker; the citing authors may never have done the procedures quoted themselves, may not understand how to do them, and may not know the theory behind the procedures. I believe that whenever one quotes another research worker, one also takes on the responsibility of the worker cited for the validity of the findings. It is one's duty to examine the theory and technique for any evidence one cites in support of one's own views.

When I have taken issue with some colleagues, the answer has been that one should *trust* other research workers. It has been alleged that I have doubts about their *abilities*. This is missing the point. I am merely asserting that quoting another paper should be a sign that one has examined its findings critically, and that in one's own opinion, it possesses sufficient quality, to be used as evidence for one's own thesis. I am *not* questioning the personal honour or ability of the authors cited.

It is possible that in large research units employing a range of experts, a particular research worker may not understand all the technicalities of the work of a collaborator, but this should not be acceptable in small groups with members of similar backgrounds. In the large units, each research worker should consider it his or her duty to educate each other, perhaps at seminars, on the theory, practice, advantage and limitations of the techniques in which he or she specialises, in order to share the intellectual responsibility for the application of their own techniques to the whole projects.

11. *Being a co-author of publications to which one has not contributed.*

In my opinion, all co-authors share the responsibility for a publication. This includes the glory, the mistakes, the fraud, the results, and the interpretations. Under no circumstances should heads of laboratories put their names on manuscripts, unless they have contributed to the thinking, the carrying out, or the interpretation of the experiments.<sup>18,25</sup> Being the head of the laboratory or only writing the publication is not sufficient to justify authorship. In recent years, there have been several examples of distinguished co-authors of papers subsequently withdrawing authorship from published papers.<sup>26-27</sup> Would they have done so, if the fraud had not been detected by others?

This is another educational problem.

12. *Misquotation of publications and findings.*

In the heat of discussion at meetings of learned societies, speakers sometimes quote papers in support of their views, believing in good faith, that their quotations are accurate. When they look up the references later on, they sometimes find that they were mistaken. However, a small minority of those who answer questions use references they know to be inaccurate or non-existent. A further group of research workers genuinely believes that they themselves have data showing what was quoted, but fail to find the data, when looking for it. For example, at meetings of the Physiological Society, colleagues have alleged that they have micrographs showing: 'unit membranes' in all orientations in one picture; the membrane around the cytoplasm of neuroglial cells in brain sections; the synaptic knobs in a full range of orientations in electron micrographs. I have requested anyone to send me micrographs of any of these appearances,<sup>28</sup> (p.47) or citations of publications showing them,<sup>29</sup> but over a period of ten years, no one has done so. Yet microscopists continue to tell meetings and Usenet, that they have such pictures. This is a further invitation to any reader to send me such micrographs, or to contemplate the consequences of the possibility that they do not exist.

One can write to the authors of misquotations and ask them to correct them, in print, if possible, but they rarely reply.

13. *Partisan refereeing of applications for grants.*

A research worker applies for a grant of several thousand pounds to pursue a particular project. The application goes to a committee of peers. Ideally, the whole committee judges the quality of the application as fairly and objectively as possible, and compares it with all the competing applications for the same funds.

In Britain today, firstly, referees are anonymous; secondly, they may be competing at the time of application or in the future with the applicants for the same funds; thirdly, they do not have to give precise reasons to applicants for turning down applications; fourthly, they may be members of committees considering their own applications. Some committees do not require those members who are applicants to withdraw when their own applications are being considered. Even when they do, it is obvious that the committee members feel very uncomfortable when they turn down an application from their colleagues; fifthly, while some committees have statutory members, others are chosen by the current committee members without election, which must tend to perpetuate conservative opinions. Therefore, the committees are likely to give grants to those with whose approaches they agree. In these circumstances, it is extremely unlikely that applications for grants by Galileo, Semmelweis, Darwin or Einstein, would have been approved by committees of their peers at the time.<sup>30</sup>

In general, research committees judge which *one* of a number of approaches to the solution of a problem is likely to be fruitful, and funds that one. Since advance has often come from opposing groups doing experiments to test the validity of the research of their opponents, and a committee may well choose wrongly, there is a good case for

support to be given to research workers or groups with differing or opposing approaches to the same problem.

I propose that applicants for grants be allowed to appear at committees considering them, and that they be entitled to ask and answer questions from members of the committees. Also, no member of a group applying for a grant or involved in any way with applicants should be allowed to serve on a committee considering a grant. Furthermore, applicants should be entitled to present cases of unfair refereeing to an ombudsman.

14. *Partisan refereeing of manuscripts submitted for publication.*

A single research worker or a group may devote one to ten years to producing results for submission for publication. The research workers have invested their time, their self respect and money from public funds or private charities. This entitles the research worker or group to serious consideration of the resultant manuscript. However, referees, who are usually anonymous, may give only a few minutes to considering quite complex research work. The authors' philosophy, findings or interpretations, may be different or hostile to those of the referees. Manuscripts originating from institutes not considered prestigious by referees are more likely to be rejected.<sup>31</sup> A referee or editor may be a competitor of the author. Either may dislike the author personally. Any of these biases may well result in the referee recommending that the paper be rejected for publication. An author's rebuttal of a referee's reasons for refusal to publish is sometimes not given fair consideration.

This brings one to the question of what is the function of an editor. Either the editor is an honest broker between the author and the referee, so that the latter may be over-ruled, if he or she is unfair to the author. Or, the editor regards it as his or her duty to defend referees from angry authors, even when the referees have been unfair.<sup>32,33</sup> Editors of journals should act as umpires between authors and referees. If the referee does not spend enough time considering a manuscript, or makes unfair, improper or unreferenced assertions about a manuscript, the editor should choose another referee. The aim of the editors should always be to assist publication, or indicate improvements which would make a manuscript acceptable for publication.

More and more journals are asking the referees whether they are prepared to allow their names to be given to authors, so that they may be able to enter into dialogue with each other. This is an encouraging trend, although it is not yet very widespread.

I would like to suggest that there also ought to be an ombudsman for authors of manuscripts. They should be entitled to ask the ombudsman *not* to consider the intellectual content of manuscripts, but to decide whether the referees have given the authors fair and serious consideration.

15. *Multiple publication.*

There are so many learned journals nowadays, that it is unlikely that a referee for one journal will know that substantially the same research has been published elsewhere. Multiple publications can be achieved by (a) cutting papers into the smallest publishable units;<sup>24,34</sup> (b) publishing the same results in several languages; (c)



publishing the same results in a journal specialising in a particular chemical, as well as another journal, specialising in the tissue upon which it acts; (d) not telling the organisers of international meetings that one has already published the material; (e) presenting already published material at meetings to which one has been invited and whose proceedings are to be published. Of course, this cannot be regarded as improper, if one has been asked to present a lecture reviewing one's own work; (f) submitting a proceeding to two or more learned societies with overlapping interests, then an international meeting, then a specialised meeting, then submitting a complete manuscript for publication. By this means, one can inflate one's curriculum vitae.

Multiple publication may be acceptable if submitted to different audiences, and if the author(s) give reasons for it.

16. *Not allowing research workers with whom one disagrees to present their findings at national and international meetings of learned societies.*

Such meetings are extremely popular nowadays, partly because many meetings publish abstracts and proceedings. There would not be time for all authors to present their work at length, so speakers are generally allotted sufficient time for major presentation (say, 30-50 minutes), a short time for summarising a few findings (10-15 minutes), or no time, but a square metre of board to be manned (say for 2-3 hours). The latter means that the abstract is published; this merits a grant for travel to, and attendance at, the meeting, and a feeling that one has participated. A committee of the learned society usually decides on the plenary speakers, and those who give shorter talks, but the committee meets in secret, receives no representations and cannot be argued with. Those whom the committee does not allow to speak are permitted to put up posters.

A research worker showing a poster is usually competing for attention with several hundred others. The audience is moving. Someone who stops to discuss the poster with its author(s) blocks the exchange with anyone else interested. Therefore, discussion must be brief, and is best used only to arrange a subsequent rendezvous. The audience of peers cannot hear the speaker answering questions, which, in my opinion, is an essential element of normal educational exchange. I regard poster 'presenters' as the research proletariat, in whom the history of science will never evince a great interest. Nevertheless, many research workers are prepared to tolerate the practice, and a substantial proportion approve of it. It amounts to censorship when it is used to deny access of 'controversial' or 'heretical' views to the mass of fellow research workers at international meetings.

The real corruption of the system is the closed way in which conference organisers choose who shall speak for how long, and who should be able to exhibit themselves in a space 1 metre square, and be proud of it. This problem does not seem to have been addressed by any international learned society.

I would propose that committees which select major speakers at international conferences be elected and answerable to the members. Furthermore, they should put aside time for those holding viewpoints challenging to, or different from, their own.

17. *Denying those with minority, challenging, or unpopular views, access to popular scientific and educational media.*<sup>22,35</sup>

Students considering becoming research workers and interested lay persons listen avidly to science programmes on the radio and television. They read such popular scientific journals, as *Scientific American*, *New Scientist* and *La Recherche*, at a time when they are very receptive to received wisdom. Information absorbed during such a learning spurt acquires a biblical authority, which professional research workers find difficult to question or to abandon later on. People with challenging views are generally excluded from access to the popular media. A real consequence of this is that the prospective students and the lay public believe that in science there is a great deal more unquestionable truth than there is in fact, so that wrong hypotheses, interpretations and ideas, survive much longer than they should. The total effect of the media is that the establishment educates or conditions the public to accept its views, and inoculates the public against those views which threaten its own.

One can only request the editors of popular scientific journals to allow more space for the critical, challenging and alternative views on a topic.

18. *Personal rudeness directed against those with whom one disagrees.*

This is a common reaction to those with whom scientists disagree. They describe them as 'old-fashioned', 'out of date', 'mad', or 'ignorant'. In many cases, they have not read their publications or been prepared to enter into proper scientific dialogue with their intellectual antagonists. Another device is to describe them as 'controversial' which is a long established trick to warn audiences not to listen to them. The term 'controversial' is totally meaningless, in that, firstly, all new theories must challenge the accepted canon, and are therefore, ipso facto, controversial; secondly, it does not matter a whit whether an idea is controversial or not. What matters is the evidence presented in support of, or against, it; thirdly, there is an implication, that there is strong opposition to the 'controversial' view, in contrast to the established view, which is not regarded as carrying the same burden.

The term 'heretical' should be even more objectionable to scientists than for academics in general. It implies that there is a body of unchangeable and unchallengeable truth; doubting of the fundamentals of a corpus of knowledge is called heresy. The word originally meant 'choice' in Greek, and, later, the theologians gave it the pejorative sense it still has today.

Rudeness in scientific exchange may take many forms, from refusing to reply to proper questions, to answering them in a hostile fashion, to not replying to correspondence, to being abusive in the presence or absence of the person with whom one disagrees, to writing hostile or misleading things about the person in confidential correspondence, to being personal about their appearance, gender, or alleged ignorance. In my opinion, editors of journals are far too tolerant of rudeness about authors of papers; they should *never* permit derogatory remarks, although they should allow negative statements, if they are immediately followed by detailed justification for them, preferably with references, which may be discussed.<sup>22,36</sup>

Although there may be many reasons for rudeness, —such as the rude people

feeling sure that they are right, to not being sure of themselves, or to realising that they are wrong—there is really no excuse for it. One should always be addressing and talking about the quality of the evidence, or lack of it, rather than the character of those who discuss it.

Sometimes reviewers publish hostile reviews about books in learned journals, in which the person attacked has little or no opportunity to defend himself. Even when the editor gives the injured party the space to reply, it appears several weeks after the original comments, when the readers no longer remember the main issue. The same situation occurs, when one tries to take to task a newspaper article, or a radio or television broadcast, for wrong or misleading information.

*Prevention* of this kind of behaviour is also an educational problem, but chairpersons of meetings and editors of journals have an important role in discouraging rudeness.

19. *Resisting the promotion of people with whose views one disagrees.*

This is an almost universal phenomenon in public and private institutions. In science, the practitioner would say that she or he believes that the candidate is incompetent, ignorant, incapable of taking responsibility, not a good teacher, etc., rather than admit personal disagreement or dislike.<sup>1</sup>

This can be inhibited by having independent academic or trade union representatives at selection meetings, and short lists of candidates drawn up by the whole selection committees, but it is very difficult to eradicate completely.

### **Real problems**

It is notoriously difficult to distinguish between proper scientific challenges and unscientific nonsense. The history of science is replete with the unacceptable ideas of yesterday, which have become the accepted dogmas of today. The scientific community is not slow to label new or challenging ideas as ridiculous. However, one must beware of certain categories of authors: firstly, some try to defy the fundamental laws of geometry, physics or thermodynamics. Such people are guilty of bad science. The pseudo-scientists constitute a second category; these include the astrologists, the palmists, the aromatherapists and the creationists, who are treated with a respect which they do not deserve. The scientific community is also remarkably tolerant of these non-scientific disciplines. I believe that this tolerance may be due to the fact that the establishment is not threatened by them.

A second category of authors is not good at presenting evidence, and this damages their ability to put their cases. Their manuscripts may be badly written, too long, too repetitive, obviously emotional, ungrammatical, or written in an unliterary style. Often the authors feel that their cases are very strong, and that their failure to adhere to the conventions of a journal, or the editing or shortening of their manuscripts, is used as an excuse for rejecting their manuscripts for publication. Untrained writers sometimes fail to distinguish between *evidence* based on long term observations by trained professionals, and the *opinion* of journalists who have spent a few hours interpreting

complex stories for popular newspapers. Those people, who put forward serious challenging views over a long period of time, are at risk of becoming paranoid if they cannot publish them, while those whom they challenge ignore them. If the challengers answer all criticisms assiduously, the people who feel themselves threatened sometimes accuse them of being 'obsessive', 'paranoid', 'boring', 'obstinate', 'unwilling to listen to criticism' – all epithets describing conditions of which challengers have to be aware.

Challengers have three ways of tackling this problem. Firstly, they can declare their willingness to discuss, debate, correspond or lecture about their views in private or in public with anyone who does not agree with them. Secondly, they should be prepared to change their views, and admit publicly that they have done so, if they are shown to be wrong; they should also be prepared to admit ignorance, if that is the case. Thirdly, they should retain their sense of humour, especially in the face of the hostility which they will meet.

Despite all these strategies, challengers to the establishment may, of course, be wrong. When one believes that one has made an important advance in thinking—whether one is right or not—and has become associated with it in the public mind, it is virtually impossible to admit that one is wrong. As a Russian proverb says, "It is easier to climb on to the back of a tiger than it is to get off it."

Very often, strong protagonists of any views combat evidence against their beliefs by ignoring it. Although I have put the case that any real scientist should regard it as a duty to react to any relevant evidence in a proper manner, there is no mechanism available to induce intellectual antagonists to answer questions or admit contradictions in their views. One can only try to shame them by asking pointed questions at public meetings, or raising them in the columns of *Nature*, *Science*, *Experimentia*, etc.<sup>6,13,16,37,38</sup>

A third category of challengers is genuinely mentally ill and has fixed ideas, partly based on reality. They are often unable or unwilling to discuss their views in a rational manner. Some of their evidence may be true, some may be taken out of context, some is based on unanswerable questions. Their views are accompanied by a great deal of emotion, and sometimes of hostility. Consideration of this attitude for a short time makes one realise that this reaction of mentally ill people is not very different from the reaction of healthy people whose views one questions.

### **Reactions of the biological research community**

There have been a number of private responses of colleagues to my assertions about parafrad, although none have yet been published.

- A. Several have asserted that they do not agree with my basic belief that scientists have a moral duty arising out of their calling to enter into unlimited dialogue with those who disagree with them. They feel that this expectation is a sort of invasion of their privacy, like a travelling salesman who refuses to go away. They also point out that busy well known academics do not have time to deal adequately with all the correspondence, which such openness would generate.

- B. Knowledge is power. When one shows that someone with a considerable reputation is mistaken, one diminishes their power and self respect. It is very rare for human beings to give up power voluntarily for purely intellectual reasons.
- C. Many research workers have alleged that the pressure on them to publish makes them indulge in some of these unscientific practices.<sup>39-41</sup> I would maintain that 'pressure to publish' means that their desire to advance their careers and increase their salaries, makes them act in an intellectually dishonest fashion. 'Pressure to publish' is a phrase which allows it to appear as if the research workers are not responsible for their actions—but that the organisation of research is. What this really means, is that the particular research workers are giving their personal advancement priority over their seeking for the truth.
- However, it is true that many honest research workers, who would not compromise intellectually, have failed to complete their doctorates, many research assistants have not been appointed to lectureships, many applicants have been turned down for grants, and many scientists have left the profession. Even as recently as the 1930s, many Soviet biologists died in labour camps for not accepting Lysenko's wrong ideas.<sup>42</sup> At this time, the outlook for a research worker who does not complete a doctorate, obtain grants for research, or find a lectureship, is very poor.
- D. An extremely common reaction was that the world is a wicked place. Research workers are no more or less honest than salesmen, politicians and spies. They seek to advance themselves by whatever mechanism society allows them. Supporters of this attitude maintain that one should expect no more of scientists than of the population at large. I would strongly dispute this cynical attitude. Since scientists, like clergy, proclaim that their careers are devoted to seeking out truths, one should not accept that their every day practice should ignore their ideals.
- E. They believed that the practices described as para fraud were rare enough that their total impact on the corpus of knowledge was insignificant. They pointed out, correctly, that there was hardly any literature on these practices—as distinct from fraud, where there was a plethora. Without evidence, the list of such misdemeanours must remain purely anecdotal. Para fraud is difficult to quantitate, and therefore one can not assess how influential it is. Yet every research worker, to whom one speaks, knows many examples of such practices. Of course, that is one reason why it is so dangerous.
- F. One of the most common reactions to assertions about para fraud was not wanting to talk about it.
- G. Another common assertion was that neither single research workers, nor groups, would have sufficient time to do meaningful experiments, if they carried out the comprehensive control series. This is tantamount to saying that there is not time to

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do good experiments whose results would endure. Even if this were true for the research worker embarking on a career, which I would dispute, it could not be true for the heads of large research groups.

### **What can be done?**

In addition to the specific measures, which have been mentioned under each category, I would like to list actions which might improve the quality of more general scientific and academic knowledge.

Firstly, the climate of academia has to be changed, so that integrity becomes more important to a scientist than power or self-advancement, because it is a reasonable assumption that more honesty in research will greatly improve its usefulness. Scientists should be much more prepared to enter into dialogue, without rancour, with those with whom they disagree. They should also be prepared to admit and publicise their important mistakes.

Secondly, membership of committees dealing with, applications for grants for research, or with manuscripts submitted for publication, or with selection of speakers for international conferences, should be answerable to the membership for the decisions they make.

Thirdly, all undergraduate, postgraduate and research students should be taught, as formal parts of their courses, logic, semantics, statistics, philosophy of science and intellectual integrity. They should be actively encouraged to ask fundamental questions about received knowledge, their own research, and their supervisors' work. Research workers should be permitted to abandon projects in which they have lost faith. There should be no shame in a grant holder returning funds to the grant-giving body if he or she loses belief in the value of the project.

Fourthly, research workers should be encouraged to admit mistakes in theory, experiment, calculations, calibrations and conclusions, and publish them.

Fifthly, the attitude that the validity of an opinion is measured by the number or authority of those who hold it, should be replaced by more careful consideration of *evidence*.

Sixthly, the idea for an ombudsman for research has been previously suggested.<sup>43</sup> This person was meant to be employed by a university or institute to help and monitor the quality of research. The idea suggested here under categories 13 and 14 is for an ombudsman who would be an outside independent person, who would ensure fair consideration of applications for grants, and manuscripts submitted for publication.

### **Final point**

Parafrad is one aspect of scientific deception.<sup>44</sup> Although parafrad has been described in biological research, it is reasonable to ask how widespread it is in other areas of research, in academia in general, in politics and in theology.

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