Science and ethics

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In thinking about science ethics, we should bear in mind that the most important thing is to do science – and do good science. That is the root of everything. We should learn about ethics and make it a part of our thinking, but not let it get in the way. We don't want to be endlessly saying no, you can't do that and you must worry about this – rather we should integrate ethics smoothly into routine practice. We can think about scientific integrity on three levels: personal integrity, collective integrity and institutional integrity.

Personal integrity

The first level is personal integrity. Clearly it is absolutely basic to science to have trust between individuals. This is fairly easily achieved within a group, but is much more difficult over distance and between strangers. In order to establish that level of trust, essential for scientific communication, we need to practise personal integrity.

But life sometimes gets in the way. In order to get anything done, one has to have a measure of personal ambition. One has to be determined to solve problems and be excited by the challenge. And science is a joyful activity: it's incredibly exciting to be the only person in the world – maybe in the universe – to have seen something, to have realized how something works. That is the excitement of discovery, of understanding. And the natural response is first to clutch it to oneself. Then, for most of us, it's natural for that response to be followed by sharing, because we want to let other people know about it. Both those stages are important.

Now in order for science as a whole to work, our ambitions have to be realized. We not only have to share, but also have to have our own contribution recognized. So, in the end, we get jobs, promotions, prizes, and all the rest of it. That gives the material rewards that complement the excitement of the science itself. The key at the personal level is not to get so bound up with that ambition that you reach a point of desperation where you simply can't let anything go, where you're terrified that someone will steal from you. There has to be some slack.

Admittedly, unless you're extraordinarily lucky, during your working years, you will encounter plagiarism. You will find that, at some point, somebody will take something from you and claim it for themselves. A recent account by the editor of this magazine describes the hurt of such an event (The Biochemist, June 2008). It happens, but so long as you have enough projects going on and you're excited about the whole process, that's a minor matter compared with the much more rewarding aspects of discovering and sharing and gaining peer approval – because, unless we're very unusual (humans being social animals), we are very much motivated by the encouragement of our colleagues. The warmth of the group and of the larger community in science helps to keep us going.

In addition to plagiarism, there is a steady trickle of cases of straightforward falsification. My first direct experience of such a case was a long time ago, during my work on nematode cell lineages. I was told that a research student had discovered part of the lineage that I hadn't yet looked at. It was in the egg, and he had drawn out a nice diagram showing how the cells divided. The cells in the egg are small and hard to see, and people were quite surprised that he'd come up with this result. Recognizing that I was practised in following other parts of the lineage, they regarded me as an expert and confidentially asked me to look into it. If I found it correct, no more would be said. So I started looking. I couldn't see much at first, but then began to decipher the cells of concern and soon had my version of the division pattern. It was quite different from what he had drawn. Either he had made it up, or he had convinced himself that he could see something down the microscope. It was quite a critical thing for our little community of nematode research, but, once I sent in my report, the paper was withdrawn, and nobody knew about it apart from the few involved. It was quietly stopped, which was an ideal outcome.

This sort of thing is going on all the time. It is a concern for the International Council for Science (ICSU)'s Committee for Freedom and Responsibility in the conduct of Science (CFRS), to which I belong. The committee used to deal primarily with the rights of scientists – for example, intervening with governments to provide visas. But the committee has now been charged with taking equal interest in the responsibilities of scientists. Of course this includes the possibilities of falsification and the need for integrity. One of our members, David Vaux, has made it his business as a reviewer to look for falsification in the literature. He finds every week, in the cell biology journals, examples of figures that have been altered outright or plagiarized, of statistics that are totally wrong – all things that have come through the reviewers. It's hard to spot faked figures, but he's getting good at it – noticing where pixels have been manipulated

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and portions of photos rearranged (see article p.20).

Personally, I find it hard to understand why anyone is so greedy, or so desperate, that they try to extend beyond what they can really do by cutting corners in these crude ways. We shall return to this question in a moment, but, whatever the reasons, it is essential that we reduce the incidence of such malpractice by all means possible.

Collective integrity

That brings us to the second level, collective integrity. My lineage checking, and David's spotting errors in journals so that they can be corrected, are examples of the self-policing of science. One doesn't want this to be nasty – it's not a question of staring over people's shoulders the whole time. It's really a matter of noticing what's going on. In a laboratory, or in a group, you become aware if someone is cheating. If someone is painting the mice, or changing cell lines, or producing figures that have no basis, it normally becomes very obvious to labmates. The best way is to nip it in the bud, by being willing to question.

It is most difficult, of course, if the person involved is senior. It has not happened to me, but it does happen to people; it must have happened in Hwang Woo-Suk's laboratory, for example, that junior people in the laboratory knew that something was wrong. But let's not just talk about Asia, it's happening in the US, it's happening in the UK. Bosses are found out every now and again in terms of doing something false, either large or small.

We must recognize that when somebody junior does have to criticize, they are stepping out of line in a heroic fashion. It's a terrible career decision to confront the boss and to say that results have been falsified. Not everybody is willing to do that, and it's very important therefore that we support whistleblowers. There always needs to be an investigation to uncover malicious complaints, but if someone is genuinely revealing malpractice in a laboratory, then they should be protected from victimization.

More commonly, though, falsification occurs at junior level, but is not recognized by the senior scientist. When that happens, it clearly is a failure of the supervisory process. This may be nobody's fault, but there is one way in which the structure of science contributes to the problem – namely the trend towards ever more publications per year. A practical proposal, which we have been discussing in CFRS, is that scientists should be discouraged from publishing so many papers. At every university, there are examples of people who have their name on more papers per year than they can possibly comprehend fully. The ideal maximum number varies with the field and exactly what you're doing, but the bottom line is that if someone simply has their name on a paper without really knowing the contents, and what went into it, it doesn't matter that they're head of the laboratory and wrote the grant proposals, they should not be an author, but should be acknowledged for the administrative role that they fulfilled in the production of that paper. Full authorship should be reserved for papers where they really have made a contribution.

If we can begin to reduce this pressure for more and more publications, by convincing search, promotion, funding and award committees that an overly long publication list should attract a negative mark, then we shall have done science a service. At the collective integrity level, we must all be able to vouch for what we're doing. A further benefit of such a change would be reduction of pressure on reviewers, so that they would have time to investigate manuscripts more fully and would have a better chance of detecting malpractice.

Institutional integrity

The third level, which is very important yet not much talked about, is institutional integrity. At this level, there is plenty of room for conflict. Most scientists work in companies, large numbers for governments, and others in universities. A company owes a duty to its shareholders: a promising drug in the pipeline can sometimes lead to pressure not to disclose adverse effects, or to set up trials in such a way as to provide a favourable outcome. Much overt malpractice has been exposed and reduced, but more subtle forms continue unabated – for example, misrepresentation in marketing. Universities are not immune from these pressures, either because of corporate links or in their own right as competitive businesses. They also could play a key role in reducing the pressure for publication as discussed above. Government scientists can experience very substantial conflict. We know, for example, that people in NASA have been prevented from speaking out about climate change. We know that in the UK, one scientist was actually hounded to his death because he blew the whistle on fake information in the dossier justifying the Iraq war. That is an extreme case, but, at a less dramatic level, it is important to be aware of the potential for deceit and critical that senior organizers in all sectors try to eliminate malpractice wherever it occurs.

Martin Rees's book *Our Final Century?* explores the prospect of advances in technology leading to the extinction of the human race during the coming century. We have been worrying about this outcome, in terms of nuclear destruction, for 50 years. But we are now beginning to worry about biotechnology – the possibilities of producing maliciously some seriously pathogenic and pandemic organism and releasing it. Already people have distributed anthrax, and the trouble is that such misguided individuals with more and more power at their disposal become very dangerous. But it's not just a matter of individuals – nations or corporations are liable to pervert developments in science for harm.

How can we detect and avoid malicious use? The answer is not straightforward because potentially harmful technology is mostly dual use – that is to say we need it to make discoveries, inventions and useful products. Probably the best possible safeguards lie in personal and collective integrity – the eyes and ears of the scientific community. It is up to us all as scientists to be guardians of institutional integrity. For this to work, there must be strong policies that help people not only to detect malpractice, but also to act effectively when they find it.

Conclusion

So we have three levels – personal, collective and institutional. CFRS is working with all the scientific unions to help formulate policy and provide guidance. Crucially, we must not be content simply to blame the individual for unethical behaviour. Of course, if somebody does something wrong, they should be castigated. But we should ask equally about the pressures that led the individual to behave in that way. Better management of competition in science, in order to limit the crude race for ever more (rather than better) output, is urgently needed. This task lies at the door of the institutions. ■

This article is based on a lecture given to research students in the Faculty of Life Sciences at the University of Manchester on 12 June 2008.

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