Does research misconduct extend beyond biomedicine?

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Research misconduct has been with us since Galileo Galilei, founder of the scientific method. Colleagues had difficulty reproducing his results. The boy genius Isaac Newton introduced the "fudge factor" to magnify the predictive power of his results. And the geneticist Greg Mendel's results were deemed too good to be true.

Misconduct in the biological sciences can start with the "tidying up" of experimental data, through to the fudging of statistics, and the invention of entire experiments. There are some striking examples, such as the Gupta Files in 1989.

Gupta recycled "Himalayan" geological fossil specimens by assaying fictitious locations with foreign materials once housed in museums and other people's laboratories. He managed to work with 60 co-authors for more than 25 years, and he was not found out until a fellow palaeontologist questioned the striking similarity of the so called Himalyan fauna with those found in Wales.

In 1997 Brach and Hermann produced work on multi-drug resistance in cancer treatment. They had mixed and matched computer images to produce new data. They were eventually rumbled when colleagues, who suspected that they had fabricated their results, consulted the university dean.

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In 2000 a new species of Chinese bird fossil, the archaeoraptor, was discovered, which explained the link between dinosaurs and bird evolution. But the tail came from a different species and had been glued on to the body. The findings had been published in a journal that did not use peer review, but the error became obvious once exposed to public scrutiny.

In 2002 Jan Hendrik Schön claimed to be able to create transistors from single molecules using nano-electronics. He published 80 papers in two years—one paper every eight days. Seven of them were published in *Nature*. Fabricated data were found in 16 of the 24 cases examined.

The fraud came to light only when researchers failed to replicate the results and found that the graphs in three separate papers were identical. But it's easy to see how he eluded detection because the same technique was applied to many different modalities and the graphs were always going to look similar.

So why does scientific misconduct exist?

There is an enormous pressure to publish, largely because of its impact on career prospects. This is one of the few disciplines in which scientists are graded, not on personal merit or how good they are at their job, but by the number of papers they publish—hence "publish or perish."

Added to which, we are all fighting for the few grants available, and the numbers of top jobs are limited, with a huge bottleneck at postdoctoral level. To get these jobs, a fantastic publication record is required. The competition to publish quickly is enormous, with authors who have taken three or four years to complete a piece of research petrified of being scooped within days.

Publishing large volumes of work can also achieve fame and recognition. In some areas authors are encouraged to publish regardless of the quality. It's the volume that counts.

Another reason is money. In China fossils regarded as national treasures cannot be sold legally. This has led to a thriving black market, in which the more different the species, the higher the price is likely to be. A US curator bought the archaeoraptor for US$150,000.

In the biomedical sciences the profits to be gained by pharmaceutical companies for developing drugs and vaccines sometimes drive the creation of positive results in basic research.

Misconduct is also easy to do. There is a fine line between manipulating digitised images to clean up data and creating completely new data, as in the cases of Brach and Hermann and Schön.

But one of the most compelling factors is trust. We trust co-workers to do what they say they are going to do, which is why Gupta went undiscovered for so long by his 60 colleagues. People are deemed innocent until proven guilty, and despite the gossip, it's often a long time before a formal inquiry is instigated. And in Europe there is no unified approach to this.

What is the punishment? Embarrassment, withdrawal of funding, blacklisting by journals and loss of scientific integrity are all likely. But formal inquiries resulting in job loss or severe punishment are rare.

Who is responsible?

First and foremost, co-authors must take responsibility. They contribute to, and read, the paper. At *Nature*, all authors must give consent before the paper can be published. But once again, it is difficult for co-authors to cross the line of trust and question each other's integrity. It is deemed insulting not to trust a data source. We need to change the culture before this becomes acceptable.

Peer review has a major role. Editors peer review work to ensure that it is technically sound. But do they pursue glamour, and as such, undertake short cuts, over-rule hostile referees, and select sympathetic ones? Ultimately, no editor wants to publish something that is wrong and which they will have to retract.

Do the referees responsible for the technical review need to be more critical, spend more time, and take the initiative to look beyond the paper?

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Funding agencies, universities and institutes also have a role. Fraud doesn't just happen at the stage editors see it. These agencies see it at various stages before publication. Should they carry out spot checks on unpublished work? Should they follow up on any gossip? Should they insist on internal peer review of work that is about to be published, and do more to encourage the teaching of good laboratory practice?

What next?

In Europe we have nothing equivalent to the US Office of Research Integrity, set up in 1989 to monitor allegations of misconduct in biomedicine. In 2001 127 were reported to the Office of Research Integrity (ORI). But even this system relies on scientists reviewing scientists, and this takes time, for which there is no pay.

The American Chemical Society talked about setting up a committee to develop policy in 2000, but nothing has happened so far. In physics there is no such committee as yet, because no one feels the need for it. But the example of Schön shows that perhaps there is.

Discussion

A delegate pointed out that if he wrote a paper suggesting that the Golgi apparatus was an artefact and sent it to a world expert, it would be rejected on the grounds that to accept it would invalidate all previous work. "There's an inbuilt system whereby people who question established thought don't get a fair referee."

Dr Dhand agreed that getting a balanced review on papers that question literature spanning decades was indeed very difficult. For that reason, she said, such a paper would not be sent to one referee who was unlikely to agree. "Our job is to find the people who would agree, and we go out of our way to do that." She added that *Nature*'s policy was to ask authors to suggest reviewers for and against their work.

Richard Smith commented: "It's a human problem. Beethoven's music was accused of being just noise and Van Gogh's paintings just daubs. If you come up with something truly original, the world is not going to be able to cope with it."

One delegate pointed out that any co-author shares an equal intellectual responsibility, but authors are also responsible for the integrity of any papers quoted in support of their work. But most people don't accept this, he said.

Did the peer reviewers in the Schön case have the responsibility to review not just the papers in question, but all the papers the author had ever written, as suggested by the *New York Times*, suggested another?

Dr Dhand said that from the referee's point of view, the technique was already established. Papers on it had been published widely throughout the physical sciences, and it was the application of the technique that was critical. "With hindsight, it's easy to look at numbers and say how could this have been missed? But in reality the raw data have become so large they can't be reviewed. You have to look at data that has been worked on and analysed."

Discussion ensued about whether catching a fraudster in two years was a success story, considering the thousands of papers out there, or whether some alarm should have been raised at the sheer volume being written.

The problem, said Dr Dhand, was that it was one method applied to different systems. "If it had been a biological principle you could ask how could seven papers on one principle go unnoticed? But this was a technique."

Various comments were made about how easy it is to commit fraud when there is no licensed degree to throw away and no prospect of losing your job. Richard Smith pointed out that in biomedical science people often had their license to practice removed.

Did Dr Dhand think biomedicine should adopt the "casino" approach? "Trust is a factor that allows misconduct to go undetected. But I don't think most scientists are fudging data. And in science you could argue that you would be found out because as soon as you publish, people will try and replicate your data."