The Fraudulent Laboratory

By Theodore Dalrymple

When I was young and naïve, the thought never occurred to me that what appeared in medical journals might be fraudulent. I knew that there had been scientific hoaxes, such as the Piltdown Man, and I knew that, man being fallible, mistakes were made. Papers in medical journals were often followed in the correspondence columns by lively debate over the interpretation of findings, which were seldom indisputable, especially when they involved complex statistics. Statisticians, after all, are like economists: they seldom agree about anything.

I was too optimistic. Scientific dishonesty poses a real threat to the credibility of scientific research. It is unfortunately far from easy to solve this problem without throwing the baby out with the bathwater.

Most doctors, being busy, read scientific papers only superficially. They read—or skim—the summary and conclusions, on the assumption that the editors have done their job properly and not permitted anything too egregious to escape them. Readers retain the conclusions in their minds and sometimes even alter their practice accordingly.

A few years ago, I decided to read one of the most respected

of all medical publications, the *New England Journal of Medicine*, more closely, line by line. I was surprised by how many elementary errors of analysis there were in it, such as the taking of correlation for causation. There were also the most obvious omissions, and I suspected, though I could not prove, that quite a lot of data-trawling went on: the belief that if one has enough data, something by chance will emerge as if *not* by chance. And many of the papers were inherently unreproducible, almost by design, and certainly very unlikely ever to be reproduced. The reader simply had to take or leave their findings.

But still, I never suspected outright fraud. It is true that the *Lancet*, another of the most respected medical journals, had published, with harmful effects in practice, a now infamous paper supposedly linking the measles, mumps, and rubella (German measles) vaccine with the development of childhood autism, but such gross cases of editorial negligence and scientific dishonesty were rare—or so I thought.

Relatively recently, however, it has been discovered that a very high percentage of scientific studies are unreproducible, and a smaller but still significant number are outright fraudulent. There are now scientists dedicated to searching out deficient or dishonest scientific papers, and there is an excellent website, *Retraction Watch*, similarly dedicated. Its investigations often lead to retraction, the signaling that a paper is so seriously flawed that its results or conclusions can no longer be relied upon and should, for preference, not be quoted.

The grounds for retraction are various, and sometimes a little troubling, in that they are not on occasion only intellectual or scientific, but moral. I have, for example, seen retractions because the authors of a medical, physiological, or psychological experiment did not comply with current ethical standards, for example by obtaining proper consent from the subjects of the experiment. This objection can apply,

of course, only to very recent research, because the vast majority of research in the past, upon which our current knowledge rests, was unethical according to those standards and would have to be retracted, plunging us back into a state of comparative ignorance—and impotence.

Should results recently obtained by unethical means (according to current ethical standards) be retracted? Would this not add insult to injury? If the results so obtained were of genuine scientific value, should they be ignored? The harm of the experiments (if any) has already been done, and it is better to obtain some benefit from improper conduct than to eschew such benefit altogether.

On the other hand, researchers need to be restrained by scruples, even if it means that certain questions cannot be answered, either in the easiest possible way or perhaps at all. Perhaps some procedure other than outright retraction—an ignominious posting in red, for example, pointing out that the results were obtained unethically—might be better. Retraction on moral grounds, with the implicit direction that the results should not be quoted in future literature on the subject, could come dangerously near to censorship.

Plagiarism is another ground for retraction, but plagiarism may be partial rather than complete. A plagiarised paragraph or illustration does not mean that everything else in the paper is false or valueless. Retraction is a blunt instrument, a single punishment for a multitude of sins of varying degrees of heinousness. Perhaps it should be confined to examples of outright falsification or the most outrageous violations of scientific method.

The process of retraction is, like many charities, subject to what is known as mission creep. *Amnesty International*, for example, started as a charity to support and draw attention to political prisoners who had committed or advocated no acts of violence, a laudable aim, but now holds forth on all manner of

subjects, such as maternal mortality rates. Such moral grandiosity leads to a loss, not a gain, of moral authority, and something similar might happen with the process of retraction if the grounds of retraction are too wide or numerous.

In any case, does retraction work? There is no simple answer to this question. A recent post on *Retraction Watch* suggests that it does sometimes work. For example, the now-notorious paper by Professor Didier Raoult, claiming that chloroquine and azithromycin might be effective against Covid-19, which was of such poor quality that it should never have been published in the first place, has not been cited at all since its retraction, though it was cited 3,162 times beforehand. One has to beware of arguing *post hoc ergo propter hoc*, but the causative relationship in this case seems likely.

The effect of retraction is more difficult to assess in other cases, however. One paper that tried to determine the effect of the Mediterranean diet on mortality, published in 2013, was cited 1,734 times before retraction in 2018 and 902 times since. There is a natural tendency for papers to be cited less as time passes, for research always moves on, besides which the intrinsic quality of papers must affect the number and duration of citations, so it is impossible here to say whether retraction had any effect at all.

Perhaps more alarmingly, a paper on Wakefield's research on the alleged connection between MMR vaccination and autism was cited 643 times before retraction and 1,047 times afterwards: though citation does not by itself necessarily mean endorsement or acceptance. At best, Wakefield's work was so scientifically flawed as to have been useless, but in the end proved grossly fraudulent. An assessment of the effect of retraction is impossible, and depends on a counterfactual: would the paper have been cited more (or possibly fewer) times if it had not been retracted?

Retraction Watch does not claim that retracted papers should never be cited, but where they are cited the retraction and the reasons for it should be mentioned. Editors should be especially vigilant and ensure that the retraction of a retracted paper should be mentioned, giving good reasons why the retracted paper is cited despite the retraction.

The incentives to scientific fraud, and the ease with which it is committed, have never been greater. There are orders of magnitude more scientists now than ever before, each struggling for recognition. In a world of metrics, where people are measured and judged by them, for example by how much they publish, there is a strong incentive to publish rubbish with pretensions to novelty; and the very metrics themselves become the object of fraud—meta-fraud, as it were.

In the fight against dishonesty in scientific research, as in the fight against bad ideas, there is no final victory. An interesting question is why some, but not all, fraudulent ideas persist, despite exposure.

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