Pathological science

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The Undergrowth of Science: Delusion, Self-Deception and Human Frailty
by Walter Gratzer

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There are few experimental scientists who have not found themselves occasionally pursuing red herrings in the form of illusory results hatched from laboratory artefacts, or from wishful thinking based on an attractive but incorrect hypothesis. It is the nature of scientific endeavour that research is prone to go astray, especially when entering uncharted territory. It is the norm for individual scientists to err for short periods of time before finding the true path. The danger, however, arises when scientists lose their objectivity, when they insist their ideas are correct despite copious evidence to the contrary and the warnings of other scientists. This danger turns tragic when this kind of pathological thinking, often spurred by political ideology, infects entire scientific communities.

Of course, we all like to believe that we are wise enough to avoid the errors of our less fortunate peers. However, the message of Walter Gratzer's *The Undergrowth of*

Science: Delusion, Self-Deception and Human Frailty is that such recognition becomes surprisingly difficult once scientists begin to stray. Gratzer, a professor of biophysical chemistry at King's College London, helps us to distinguish the good from the bad in scientific thinking, the healthy from the pathological, in his analysis of both individual and collective scientific delusions.

Gratzer's book makes fascinating reading. His examples are categorised by their disciplines and by the way in which the scientific method was transgressed. I found myself more than occasionally stunned by the thin line between science and pseudoscience and the ease with which experienced scientists crossed it. Before reading Gratzer, I had—an apparently misplaced—confidence in modern medicine, although I am aware that the recommendations for a healthy diet, for instance, swing with the prevailing fashions of the time. Now I realise that surgical procedures can be determined just as much by fashion as by scientific evidence. All it takes, explains Gratzer, is one respected surgeon with a sincere but errant belief in the power of a particular procedure and thousands of patients can undergo life-threatening operations that leave them needlessly crippled. Gratzer points to the example of the well-known British surgeon Sir William Arbuthnot Lane who truly believed, and successfully convinced a generation of surgeons, that the human colon is an evolutionary relic we would be better off without. Lane recommended removal of the colon as a remedy for constipation and, by 1935, had performed more than a thousand such operations himself. Despite the death rate from the operation alone being close to 20 percent, Lane's anti-constipation surgery became widely accepted, and he was even knighted in honour of his heroic contribution to medicine. Today, no one, or at least no one reputable, believes that the removal of a healthy colon could possibly be beneficial. But similarly, even as late as the 1960s, removal of the tonsils was the recommended remedy for throat inflammation, and lobotomy was still

widely performed at mental hospitals. Reading these examples prompts the obvious question: how many of today's medical practices are based on equally erroneous individual or collective beliefs?

Many readers of *EMBO reports* will assuredly remember the cold fusion story of 1989 and think, as I did, that such a colossal blunder could never happen to them. I personally have changed my mind since reading Gratzer's description of the chain of events at the University of Utah that swept along Stan Pons and Martin Fleischmann and their competitor Steve Jones. Imagine that you are working on a subject of extreme importance, if not salvation—a cure for cancer, for example—and your preliminary results suggest your idea really works. To secure funding for further experiments to rigorously test your hypothesis you submit a grant proposal, and later learn that it has been evaluated by a competitor who has subsequently tried and confirmed *your* idea in preliminary experiments. Moreover, your competitor is now saying that he is ready to publish, although he would be willing to collaborate. Would you now continue with the time-consuming controls and repetitions, knowing that you could lose the race? Or would you rather consider your competitor's results as objective confirmation of *your* idea, and work as quickly as possible to submit your preliminary data to a refereed journal? Before reading Gratzer's book, I would have taken the latter course and hurried my submission. Now I hope to remember that shortcuts on the way to fame are also shortcuts on the way to infamy.

Gratzer's book is not only a fascinating read, it is a lesson about doing good science that may prove more valuable than a shelf of advanced text books. I strongly recommend this book to every scientist. By helping scientists to do good science, it will surely help us all.

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