

Research misconduct: learning the lessons

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It is only with perverse nostalgia that I now hold the September, 2005, letter from Jon Sudbø. His letter accompanied a paper *The Lancet* published on October 15, 2005. It is the letter that prompted an expression of concern published on January 21, 2006. It is the letter that eventually led to a retraction notice being issued on February 4, 2006.

Sudbø's first communication with us refers to the "double-edged sword" of cancer therapy. There is also a double-edged sword to research and publication. On January 13, 2006, news of Sudbø's fraud broke in the Norwegian media. It was too late for the American *Journal of the National Cancer Institute*. In their January 18 issue, they reported the start of a clinical trial based on Sudbø's work. The headline ran: "Years of research come to fruition with launch of oral cancer prevention trial." Eight days later I received a message from Anders Ekbom confirming that one key element of this long-term research programme had been fabricated.

Does the Sudbø affair represent a series of extraordinary acts by one man, indicative of a single individual's aberrant behaviour? Or does it reveal a catastrophic failure of an entire multidisciplinary, polyinstitutional, and international system of science? Fortunately, we have the investigation of Anders Ekbom to guide us. Here, the facts of the case are lucidly laid out. The Ekbom Commission thoughtfully reflects on the nature of justice in cases of research misconduct; the difficulty of finding the right judgment between error, incompetence, and outright dishonesty; and the task of defining a correct standard of proof against which to measure individuals and institutions.

In Britain, we have adopted a less intellectual attitude. We ask only whether a person has been a jerk or a crook. But the blunt simplicity of this question is no joke. For fraud leaves a shadow of desolation and betrayal be-

hind it: in Sudbø's case, and most acutely, for one research fellow whom he had both supervised and deceived.

The Ekbom Commission's conclusions were devastating. "Several people should have reacted", they wrote. Ekbom meant co-authors, supervisors, superiors, opponents, colleagues, and perhaps even editors (although, politely, he does not say so). Why? Because there were warning signs.

It is important that we do not overreact. Bad cases make bad law. We do not need more regulation of research. We need intelligent regulation. A light touch. Regulation better coordinated and better enforced. Indeed, one could make the case that the discovery of Sudbø's fraud was a stunning success. A lie detected quickly, investigated appropriately, and corrected immediately.

Still, it is right to ask: why was the Sudbø fraud not detected earlier? What arrangements might be put in place to make sure such a fraud would be detected in the future? In answer to the first question, the Commission alludes to several possibilities. First, the presentation of Sudbø's data was so elegant that it possessed some kind of bewitching quality on all those who saw it. Second, the possibility of fraud seemed beyond the limits of rational belief. Third, there was "boundless trust" in Sudbø, a man who had become a "favourite son" of the research community. Fourth, his co-authors were cleverly manipulated, disabling their critical faculties. And finally, this was, after all, "sensational research" – who was going to swim against such a strong tide of success? None of these explanations is especially satisfactory.

In truth, few procedures were in place for the quality assurance of Sudbø's research. Insufficient care was taken over the preparation of his work for publication. There were inadequate institutional arrangements with respect to the training of scientists and the management of research. And there was "a disturbing lack of awareness" among scientists "of the prevailing rules for good research practice." There is also an astonishing paragraph in the Ekbom report, a paragraph that should be elaborated on if we are to understand this case fully. Ekbom mentions one person, an individual with suspicions, who retained documents and who knew that something was wrong. Out of fear, this person stayed silent.

The medical journal is also a neglected source of scrutiny. A journal is the final common path for acts of scientific dishonesty. It bears a great responsibility for protecting not only the record of research, but also the conscience of the research community. Sudbø's fraud reveals the strength but also the fragility of the research community. There are parallels here with the cloning scandal, perpetrated by Dr WS Hwang from South Korea. The

journal *Science* commissioned an independent review of this monstrous episode of misconduct. The *Science* panel concluded that:

- the journal had been intentionally deceived
- no peer review procedure is fool proof
- but procedures can reasonably be strengthened
- after all, existing procedures clearly failed
- incomplete answers to reviewers' questions should have triggered concerns
- editors were sometimes too easily persuaded by the beguiling rhetoric of authors
- the nature of the collaboration should have been explored more deeply and not accepted at face value
- worse, "the cachet of publishing in *Science* can be an incentive not to follow the rules"
- editors should start from a position of "a healthy level of concern", not blind trust
- there should be a "formal risk-assessment" of papers by editors to calculate the probability of deception and the consequences (if misconduct was discovered) for the reputation of the journal, science, and policy-making
- high-visibility papers should receive greater scrutiny
- journals should tighten their rules on co-authorship
- more primary data should be made publicly available

Each of these conclusions has a direct corollary in the Sudbø case. The Ek-bom Commission, for example, had some sharp remarks about the wider inclusion of co-authors in the review and publication process. And about the risks of fast-tracking papers. I can think of five hypothetical reforms that would have prevented the frauds of Sudbø, Hwang, and many others. They are extreme. But I know that they would have worked.

Hypothetical Reform 1: Slow down the peer-review process. Ignore the calls to speed up peer review by scientists aggrieved at its snail-like pace. Let us take time to document warning signs. Let us raise the bar for publication of high-risk papers. Let us have a higher index of suspicion for fraud. Plainly, trust does not work.

Hypothetical Reform 2: Follow the example of clinical trials: insist on an independent data and safety monitoring board for all research studies. Create

in-built checks and balances: an oversight mechanism that does not exist today.

Hypothetical Reform 3: Change the culture of our research institutions. Have fewer rules but stronger values. A research career should be seen as a privilege that demands a set of very specific duties. Scientists should be rewarded for the total life of what it means to be a scientist – mentorship, education, training – and not merely the products of that life, whether measured in grants or published papers.

Hypothetical Reform 4: In both the Sudbø and Hwang cases, at least one reviewer dissented from the majority. Yet by a simple democratic vote, both papers won through to publication. The authors' responses to queries from reviewers appeared plausible. This approach to peer review is clearly flawed. Instead, we should demand absolute concordance between reviewers if publication is to proceed.

Hypothetical Reform 5: Until we take the responsibilities of authorship more seriously, we will not be taking research misconduct anywhere seriously enough. Wringing our hands over fraud without being clear that one should take credit only when a contribution has been serious and substantial, and without recognising that as an author one has a responsibility to check the integrity of one's colleagues, is like complaining about climate change as we drive our SUVs to the Vinmonopol. Curbing fraud means smartening up our own behaviour.

(Note: The Ekbohm Commission is surprisingly soft on authorship. For cost-benefit reasons Ekbohm and his team did not fully investigate the roles of Sudbø's co-authors. The Commission viewed gift authorship, the absence of data checks, and a lack of internal review of authorial roles as "not uncommon". Deviation from the norms of authorship were "of less importance in relation to the main issue in this case." Indeed, Ekbohm and his associates saw authorship transgressions as "less gross and serious." I disagree.)

These five measures seem draconian. Yet how bad does the next case of research misconduct have to be, how damaged does public trust in science have to become, before we do what we know in our hearts and our heads is necessary to strengthen the integrity of research? The lessons of Sudbø seem agreed. We must all adhere to existing guidelines. We must not introduce more regulations. We must not disable research. Fraud will happen again.

As an editor, who feels sincere responsibility for the accuracy and honesty of the scientific record, I do not believe this response is sufficient. In an era when deference and trust are under challenge, we have to recalibrate our procedures to match public and societal expectations of transparency, accountability, and willingness to strive continuously to improve the quality of what we do in a demonstrable way. Put simply, journals have to raise their game. Editors must supplement trust with vigilance. Each peer-reviewing editor should see himself or herself as a critical guardian of research integrity. Editors must work to strengthen the collective responsibility of co-authors. Journals should revise their pre and post acceptance processes to reflect these changes in attitude.

The Ekbom Commission raised some particularly troubling questions as it closed its inquiries. Would the Sudbø case diminish the interest of scientists outside Norway from collaborating with Norwegian researchers? Would the institutions caught up in this latest fraud have their names forever and “inevitably” linked to Sudbø?

I do not believe so on either count. Norwegian science is not defined by one man or one incident. The institutions affected are complex and diverse organisations. They support excellent research of high national and international standard. The wound created by Sudbø will heal. But the speed with which it will heal and the risk of its recurrence will depend on the conclusions of conferences such as that which we are reporting in this issue of *Michael*. Our greatest enemy is silence.

The Dream Life of Sukhanov by Olga Grushin is the story of a depressed, beleaguered, middle-aged editor who is reviewing the sad course of his life and the mistakes he has made. He is arriving at the end of his career. The novel is about the way he tries to edit his experience. This is hard because his life seems to consist of a series of failures and betrayals. Yet his conclusion is clear: “true wisdom could be distilled only in the retort of suffering”. Not a bad epitaph for an editor. Not a bad lesson to be drawn from the extraordinary case of Jon Sudbø.

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