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Call for openness about farm-animal experiments

SIR — The recent call by the Nuffield Council on Bioethics ("UK panel urges animal researchers to go public" *Nature* 435, 392; 2005) for scientists to discuss more openly the use of animals in experiments should start with farm animals. During their lives, most commercial livestock are subject to experimentation at several levels: the individual farm; the national flock or herd; and the global livestock industry. Indeed, at a more basic level, a farm's day-to-day and year-to-year refinement of animal-management practices and business activities is itself scientific, experimental and involves animals.

"Commercial livestock are subject to experimentation at several levels: the farm, the national flock or herd and the global livestock industry."
— Ian G. Colditz

At the individual farm level, genetic improvement programmes provide one example of animal experimentation through collection of data on pedigree and on individual performance such as body weight and milk production. Another example is collection of blood for serology, on the farm or at the point of slaughter, to test hypotheses on disease epidemiology.

What benefits might flow from a broader appreciation of commercial farms' dependence on farm-based animal experimentation within their enterprise and more broadly across their industry?

First, experimental practices, especially genetic improvement programmes, can have welfare consequences that deserve our attention (see W. M. Rauw *et al. Livest. Prod. Sci.* 56, 15–33; 1998). These can be beneficial, for example when animals are selected for calving ease. They can also be harmful, for example in broiler chickens whose legs are weakened by selection for fast growth rate.

Second, the discussion may help illuminate the nature of human–animal relationships and potentially reduce the stigma associated with use of animals in universities and research institutes.

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Plagiarism criteria ignore the way research evolves

SIR — Your Special Report on plagiarism, "Taking on the cheats" (*Nature* 435, 258–259; 2005), does not, in my opinion, appreciate the way in which scientific research evolves.

In my experience, incremental progress is often reported at one or more scientific conferences, until a comprehensive manuscript can be submitted to a reputable journal for publication. The work may be further published in research monographs, in review articles and, on occasion, in textbooks.

It seems to me a misunderstanding to insist that a piece of work must be published only once.

A series of progress reports would naturally have extended sections in common — if this were not allowed, no scientist would consider presenting work at a conference with published proceedings. Increasingly, conference papers are being published as regular books and sometimes even as special issues of standard journals. Is it not defeating the scientific purpose of conferences if final papers are expected, rather than discussion papers on work in progress?

Similar conflicts can arise between journal and book publishers. I submitted a paper to a scientific journal and later incorporated a description of it in a research monograph. However, the efficient book publisher got the monograph on the street two months after receiving the manuscript, whereas the journal turned out to have a backlog resulting in accepted papers waiting more than a year to appear in print. In this instance, could I be accused of committing 'self-plagiarism' on the basis of overlaps, when I have no control over the schedules of the publishers?

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Six-word rule could turn description into plagiarism

SIR — Your Special Report on the software that journal editors are considering to help them catch academic cheats (*Nature* 435, 258–259; 2005) suggests that six words used contiguously in more than one published paper now constitutes plagiarism. I design novel oligonucleotides to inhibit STAT3 activity in hormone refractory prostate cancer. I use this phrase of more than six words to describe what I do in the introduction to my manuscripts. Am I committing self-plagiarism?

Plagiarism must absolutely be defined not by words used but by data shown. That is the serious offence, not that someone reuses a key descriptive phrase in several papers.

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Penalties plus high-quality review to fight plagiarism

SIR — Your Special Report (*Nature* 435, 258–259; 2005) on plagiarism in scientific texts overlooks an important related problem, namely multiple publications of the same data (graphs or pictures).

The computer programs for identifying duplicate text in manuscripts do not seem capable of handling this particular kind of dishonesty. In one case I have identified, the authors had published the same electron micrographs repeatedly in different journals, and attempted to do so with a manuscript that was sent to me for review. In trying to hide self-plagiarism, the copied figures were presented upside down, rotated by 90°, or slightly cut or expanded. This is reminiscent of Jan Hendrik Schön's scientific misconduct (*Nature* 417, 367–368; 2002).

When I informed the editor, she rejected the paper with a letter stating that it was unacceptable, as a reviewer had detected the plagiarism. But there are good arguments for journals to go further and take punitive measures, such as notifying the authors' institution and, if applicable, their funding agency. Papers identified as fraudulent after publication should be removed from the web version of the journal and be replaced by a note disclosing the scientific misconduct. The authors' consent should not be required.

Even more serious sanctions may need to be discussed publicly and included in the guidelines for manuscript submission. For example, the authors could be banned from publishing, permanently or temporarily, at least in the cheated journal. Funding agencies could add pressure by stating that scientists practising scientific misconduct will be denied the right to submit proposals.

Last, but not least, many cases of scientific misconduct could be avoided if the work of good reviewers were appreciated more explicitly. I tell myself that I'm doing it for the benefit of science. But, lacking appreciation, some reviewers might do their job in a sloppy manner. Why not encourage them? The only journal I know that makes an award for excellence of review is *Environmental Science and Technology*. Publishing a list of the top 10 or 20 reviewers each year may encourage more in-depth evaluations of manuscripts.

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***Nature's* policy on duplicate publications is outlined at www.nature.com/nature/authors/policy/index.html#a4. *Nature* has on occasion notified employers concerning misconduct detected during peer review, and reserves the right to bring it to readers' attention — Editor, *Nature*.**

BOOKS & ARTS

Literary device? Walther Gerlach (below) may have tried to develop an atomic bomb, but claims that Germany tested one on the island of Rügen in 1944 are unsubstantiated.

The race for the bomb

How close was Nazi Germany to developing atomic weapons?

Hitlers Bombe: Die Geheime Geschichte der Deutschen Kernwaffenversuche
by Rainer Karlsch
Deutsche Verlags-Anstalt: 2005. 432 pp. €24.90, SFr 43.50

Dieter Hoffmann

Berlin historian Rainer Karlsch's book deals with one of the most controversial questions in the modern history of science in Germany: did the nation makes serious progress towards developing an atomic bomb? The sensational title of his book, *Hitlers Bombe*, could suggest that German scientists built and tested an atomic bomb, but this implication is not borne out by the book's content. Germany certainly did not have an atomic bomb the size of those that the United States dropped on Hiroshima and Nagasaki in August 1945, which is today's standard meaning of the term 'atomic bomb'.

So what does Karlsch mean when he uses the word 'bomb'? He uses no unique nomenclature, speaking of tactical atomic weapons, of nuclear bombs, of thermonuclear hollow explosive bombs, and of uranium-235 bombs. In fact he tells us a story of how Germany developed a small nuclear weapon, which he says was tested at least twice.

The physical principle of the bomb is never described precisely or comprehensively. For example, Karlsch describes a design consisting of two spheres, nested one inside the other. The smaller one in the centre contained heavy water, surrounded by a sphere of nuclear fuel. The outer spherical surface was covered in conventional hollow explosive charges, which, when ignited, compressed the heavy water so intensely that it started a fusion reaction. The neutrons generated in this way would have triggered fission reactions in the nuclear fuel. It is plausible that the Germans based their work on advanced research into hollow explosive charges, but even so, by ordinary physics, the reaction speeds, pressure and available temperature are at least two orders of magnitude too small to initiate a fusion reaction. Furthermore, it is unclear how the Germans obtained their plutonium or their enriched uranium. Karlsch's explanation, both here and elsewhere, is vague, and he generally gives only qualitative descriptions without concrete figures. So it is not clear how well this thermonuclear hollow explosive bomb, as well as other bomb designs, would have worked, if it worked at all.

Nevertheless, Karlsch believes that a nuclear weapon could first have been tested in October 1944 in northern Germany, on the island of Rügen. A second test reportedly killed several hundred prisoners at a concentration camp in Thuringia in March 1945. But these tests bring us to another problem of the book: the historical reliability of Karlsch's sources. There is doubt over the evidence for the first test in Rügen, as the first report about the event, shortly after the war, is controversial and unreliable. The second event is better supported by documentary evidence. It seems certain that some kind of new and powerful weapon was tested in Thuringia in March 1945 — but what sort of weapon is not clear.

Karlsch attempts to prove his hypothesis that it was a nuclear weapon with data from

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