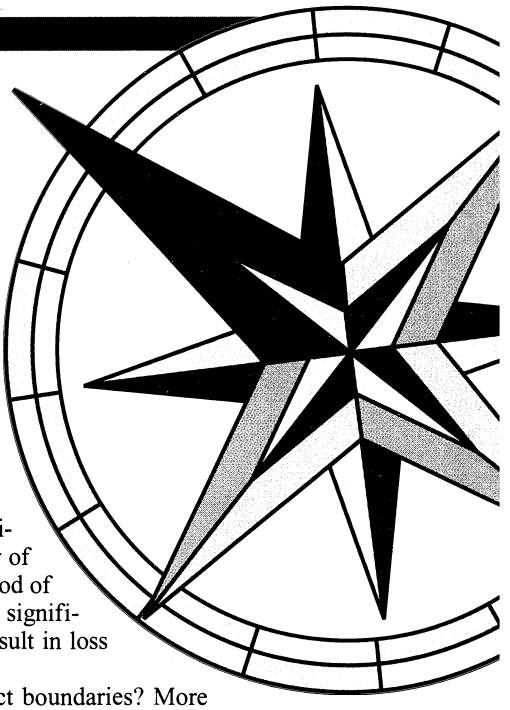

Unseemly Competition

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In mid-October 1999, the U.S. Office of Science and Technology Policy published in the *Federal Register* a new government-wide policy to define scientific misconduct in order to protect the integrity of the research record (see www.whitehouse.gov/WH/EOP/OSTP/html/9910_20_3.html). As currently proposed, scientific misconduct for U.S. federally supported research will consist of "fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results." Falsification includes data obtained by instrumental manipulation, and plagiarism explicitly includes information encountered during peer review. The new definition does not deal with authorship disputes (which are deferred to the authority of collegial arbitration). A final set of definitions will be implemented after a period of open public commentary. A finding of intentional misconduct, representing "a significant departure from accepted practices of the scientific community," could result in loss of research funding and disqualification from future funding.

Can the scientific community rest easier with these newly drawn conduct boundaries? More important, can the funders of scientific research now relax in the belief that with these uniform federal definitions, misconduct will be detected and prevented? *Science* believes not. The defined acts, acknowledged by all to be egregiously detrimental to the research record, occur extremely rarely. Far more common, yet still happily infrequent, are acts that are far more insidious and disruptive to the scientific process and to the careers of young scientists.

For some scientists and some journals, it would appear that proper conduct is what you can get away with in the never-ending competition for papers, precedence, and prominence. Consider a few examples. (i) A presents preliminary work in progress at a departmental meeting and then moves to a new post. Months later, B recalls the idea of A's presentation but not the source while challenging a new postdoctoral fellow to test this implausible idea. Compelling evidence for A's concept is obtained, which B and his postdoc publish. When their paper appears, A feels betrayed and stops studying that particular problem.

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(ii) C and two colleagues make an important chemical discovery. While their results are under review at one journal, they refine their process, derive broader implications, and send the new improved results to a different journal. Both papers are accepted, but neither paper mentions the other and the reviewers of each paper are unaware of the other data. The editors of the two journals feel duped.

(iii) A team isolates and clones an important regulatory enzyme. In searching for reviewers, the editors invite E, a senior scientist formerly active in that area, who stipulates no conflict with ongoing activities. The review arrives long after the requested date and demands extensive revisions and rereview of the revised paper. Weeks later, the paper is published. One week later, a paper from E's laboratory appears in a competing journal with the same finding.

In each of these cases, some justifying basis for what appears to be unjust behavior could be invoked: A never publishes his good ideas; B hears many presentations of provocative ideas; C and colleagues meant no harm in independently publishing their refined findings; E's lab is so large that the work of a postdoc on maternity leave was momentarily forgotten. The boundary is far from black and white.

Peer review plays a major role in protecting the final phase of the research record but cannot solve all problems. Authorship and collaboration problems are a serious threat to the research enterprise and to the motivation of young scientists, especially when they involve misappropriation of ideas and data. Choosing which prior work to cite and intentionally overreferencing one's own work or disparaging that of others are among the most frequent criticisms of manuscripts during review. An important but inadequately applied principle of collaboration is to set up a plan, best written down at the outset, as to who will do what and how credit will be attributed. The discussion forums in *Science's* NextWave continue to alert young scientists to these issues.

No nation's scientific community is immune to the various obvious and not so obvious forms of misconduct; thus, there needs to be ongoing international discussion of these issues. Strengthening the integrity of the research enterprise requires recognition that ethical hooliganism, be it intended or not, is intolerable.