

## Animal Rights and Violence

In response to a recent car bombing in Britain, J. G. Collins calls upon animal advocates to repudiate such terrorist acts (Letters, 27 July, p. 345). The Humane Society of the United States (HSUS), the nation's largest animal advocacy organization, has been on record against such violent acts for many years. Earlier this year we, along with the American Society for the Prevention of Cruelty to Animals and the Massachusetts Society for the Prevention of Cruelty to Animals, drafted and endorsed the following resolution on nonviolence.

*WHEREAS the foundation of the animal protection movement is that it is wrong to harm others; and*

*WHEREAS threats and acts of violence against people and willful destruction and theft of property have been associated with the animal protection movement; therefore be it*

*RESOLVED that we oppose threats and acts of violence against people and willful destruction and theft of property.*

*RESOLVED that we shall energetically work to reduce, as rapidly as possible, the massive pain and suffering of billions of animals through non-violent means.*

This resolution is currently being circulated to thousands of animal advocacy organizations in the United States for their endorsement. As the HSUS recently informed a congressional committee, we not only oppose violence on philosophical grounds, but also believe it does not advance the cause of animal protection.

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## PNAS Paper: Rules and Standard Practice

We would like to comment on the matter of Theodore Friedmann (Briefings, 10 Aug., p. 622). We believe that what happened cannot be construed to represent "scientific misconduct." It is not even altogether clear that Friedmann literally "violated the publication . . . rules" of the *Proceedings of the National Academy of Sciences* (PNAS).

What happened is that Friedmann and his five colleagues submitted a manuscript

through Morris E. Friedkin for publication in PNAS; it was accepted on 20 January 1987 and published in May 1987. Friedmann had already presented the findings (a new method for retroviral infection of cultured hepatocytes) at a November 1986 symposium in Denver sponsored by the Eleanor Roosevelt Institute ("The Next 25 Years in Human Genetics"). The proceedings of that symposium were to be published subsequently in *Somatic Cell and Molecular Genetics*. Friedmann duly sent a manuscript summarizing what he had presented at the Denver meeting, but *only after his paper had been accepted by PNAS*. The symposium paper was published in July 1987 (in a separate section of the journal clearly labeled as "Conference proceedings"), *after* the PNAS article had appeared in May 1987. This chronology, accepted by PNAS and by all concerned, does not violate the PNAS stipulation that findings reported there "are not being submitted for publication or have not been published elsewhere." Thus, Friedmann may have violated the spirit of the PNAS rules, but not the letter. Richard L. Davidson, editor-in-chief of *Somatic Cell and Molecular Genetics*, has written to Friedmann as follows:

The inclusion of such material in published conference proceedings is in no way incompatible with its publication also in refereed scientific journals . . . I categorically reject any implication of impropriety concerning the inclusion of your material in the Conference Proceedings and its publication also in the *Proceedings of the National Academy of Sciences* or other journal.

However, Friedmann has recognized that he did err in two important respects: he failed to cross-reference the PNAS paper in the symposium paper, and he did not obtain formal clearance from his coauthors (the same five coauthors) before submitting the symposium report, presuming that since the data were essentially the same data there could be no dissent. The essential point is that these were errors of omission, probably the result of haste and carelessness, but surely not intended to deceive or mislead anyone. Bad judgment, yes; scientific misconduct, no.

There is a broader question underlying this unfortunate episode. It is how we are to define "duplicate publication." Are papers published as proceedings of scientific symposia to be considered "publications"? The organizers of conferences want to make the proceedings of their conferences available to a wider audience of investigators. In this instance there was a long series of closely related papers on gene therapy, and their availability in one place was probably helpful by presenting the "state of the art" to all interested scientists. The issue of how valuable such conference reports are can be

argued. Suffice it to say that they represent standard practice, and yet we have not sufficiently regularized our rules and regulations regarding them. We think that PNAS and other primary publications, as well as the scientific societies involved, need to deal with this issue. Meanwhile, we feel it is unfortunate that the report in the 10 August Briefing overstates the case against Friedmann and does him a serious disservice.

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## "International" Meeting: Criteria for Invitation

It has been announced that a meeting will be held at Abu Dhabi, United Arab Emirates, from 8 through 15 December 1990, under the appellation "International Conference on High Salinity-Tolerant Plants in Arid Regions." Israel has a number of illustrious scientists in this field, yet these scientists received no announcements of or invitations to this meeting. The impression is unavoidable that, in the selection of scientists to be invited to this conference, considerations other than the legitimate ones of interest and competence in the matter at hand played a role.

In no way do I mean to cast aspersions on Western and other scientists listed as mem-