Letters

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.

JOHN A. HOYT
President,
The Humane Society of the United States,
2100 L Street, NW,
Washington, DC 20037
MARTIN L. STEPHENS
Director, Laboratory Animals,
The Humane Society of the United States

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.

JOHN ABELSON
Department of Biology,
California Institute of Technology,
Pasadena, CA 91125
RICHARD C. ATKINSON
Chancellor,
University of California, San Diego
RICHARD L. DAVIDSON
Editor-in-Chief,
Somatic Cell and Molecular Genetics,
University of Illinois,
Chicago, IL 60612
RENATO DULBECCO
President, Salk Institute,
San Diego, CA 92138
MORRIS E. FRIEDKIN
Department of Biology,
University of California, San Diego,
La Jolla, CA 92093
GEORGE E. PALADE
Dean for Scientific Affairs,
University of California, San Diego
J. EDWIN SEEGMILLER
Institute for Research on Aging,
University of California, San Diego
MELVIN I. SIMON
Department of Biology,
California Institute of Technology
DANIEL STEINBERG
Department of Medicine,
University of California, San Diego

"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-

bers of the organizing committee, who seem to have been left in the dark concerning the criteria to be applied to the selection of scientists to be invited to this "international" meeting. Prospective participants caring about the integrity of the world's scientific enterprise, however, may wish to be aware of the exclusionary policy governing attendance at that meeting. In view of the unsettled conditions in that part of the world, prospects for this meeting seem doubtful in any event, but that is an altogether different issue.

EMANUEL EPSTEIN
Department of Land, Air and Water Resources, University of California, Davis, CA 95616–8627

Human Genome Initiative

Bernard D. Davis and colleagues (Perspective, 27 July, p. 342) are plainly wrong. They simply cannot "see the forest for the trees."

The crisis in funding for younger scientists is very real. But this is not a consequence of the Human Genome Initiative. Rather, it is largely an unplanned result of well-intentioned but poorly analyzed policies of the National Institutes of Health.

Yes, we should sequence the human genome because "it is there." Just as we explore the solar system because "it is there" or the world of quarks because "they are there."

In this case, "it" is the complete biological inheritance of our species—that marvelous message, evolved for 3 billion years or more, that gives rise to each one of us. For *Homo sapiens* there can be no more important script in the universe. That some (Davis not included) call 95% of the message "junk" is only the fault of their perspective.

We should seek to know and comprehend every byte of this text. Our generation of biologists should be grateful that we are the ones to have this opportunity.

ROBERT L. SINSHEIMER
Department of Biological Sciences, University of California, Santa Barbara, CA 93106

Females at Work

Worker ants are females, not "he's" (P. W. Sherwood, Letters, 10 Aug., p. 612). An individual ant is far from resembling "a ganglion on legs"; she has a wealth of complicated, inherited behavior patterns (*1*). The juxtaposition of the letters by Sherwood and Cheryl K. Olson, "Shoehorning men into studies," is an interesting coincidence. Let's not shoehorn ants into computers. Remember, any worker can be a queen if she gets enough of the right food when she's young.

THOMAS H. JUKES
Department of Biophysics, University of California, Berkeley, CA 94720

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Energy from Solar Cells

As I was quoted in Jeremy Cherfas' article "Greenpeace and science" (News & Comment, 16 Mar., p. 1288) and have received some criticism, I would like to clarify my point. This was that the amount of solar energy falling per year on (just over) 2 square meters of collector could provide all the domestic, electrical energy needs (excluding space heating), of the average U.K. household per year if 50% efficient solar cells were available. This is, I believe, a correct statement. The average solar energy flux in the United Kingdom is about 900 kilowatt-hours per square meter per year (*1*), and domestic electrical equipment, excluding space heaters, in the average British home consumes 1000 kilowatt-hours per year (*2*).

The first conclusion I draw from this is that research into higher efficiency solar cells is important even for use in the United Kingdom, and it makes even more sense for lower latitudes. As Cherfas' article reports, I am starting such research with the help of a Greenpeace grant. I should make clear that the Greenpeace Fellow receives less than one-third of the salary quoted in Cherfas' article.

My second conclusion, not reported in the article, is that research into cheaper and more efficient energy storage is also important, as much of this energy arrives at the wrong time. There should be a major research effort into more efficient and cheaper batteries for short-term, local storage and into larger systems (for example, pumped water, compressed air, superconductors, and hydrogen generation) for long-term, central storage. These larger systems could be exploited by developing the grid to run backward. The prospects for all the renewable energy sources would then be considerably enhanced.

Finally, another figure can be given to stimulate thinking about funding priorities. There was great excitement a year ago when it was thought that a source of cold fusion had been found that produced *heat* at a power density of about 10 watts per cubic centimeter. There was already available at that time a renewable technology that produces *electricity* at a power density greater than that. A 20%-efficient gallium-arsenide solar cell with a 5-micrometer active region generates electricity with a power density of 34 watts per cubic centimeter when in a solar flux [the solar power per square meter at sea level on a clear day when the sun is at an angle of 48° with the vertical (*3*)] of 844 watts per square meter. The search for cheaper and more efficient solar cells and batteries might not be helped by the kind of media hype that surrounded cold fusion, but it would certainly benefit from the sort of funding both cold and hot fusion have attracted.

KEITH W. J. BARNHAM
The Blackett Laboratory, Imperial College of Science, Technology, and Medicine, London SW7 2BZ United Kingdom

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U.K. Physics Proposal

For the record, the statement in the 6 July Briefings that the content of English undergraduate physics courses should be cut "by at least two-thirds" (p. 21) goes a little too far. The proposal of our working party is that the content should be reduced by *one*-third only—still quite a substantial reduction.

R. G. CHAMBERS*
H. H. Wills Physics Laboratory, University of Bristol, Royal Fort, Tyndall Avenue, Bristol B58 1TL, United Kingdom

*Chairman, Physics Higher Education Working Party

Erratum: The report "Protein crystal growth in microgravity" by L. J. DeLucas *et al.* (3 Nov. 1989, p. 651) contained a typographical error regarding the *B* values of space- and Earth-growth crystals. The last sentence of the first paragraph on page 652 should have read, "The slopes of these plots are directly related to the difference in overall *B* values for two different crystals, b (Earth-grown) and a (space-grown)."

Erratum: In the Research Article "Three-dimensional structure of cellobiohydrolase II from *Trichoderma reesei*" by J. Rouvinen *et al.* (27 July, p. 380), the strand tilt in TIM barrels was misstated. The last sentence of the first column on page 382 (continuing in the second column) should have read, "Neighboring strands are commonly tilted to each other by about −26°, and have a shear of 8 in encircling the barrel (*37, 38*)."