cago area are about 5°C lower than those in the New Orleans area during the climatically defined summer (June through August). For this 3-month period, the listed means at three Chicago stations (converted from their original Fahrenheit units) range from 21.8°C at O'Hare Airport to 22.8°C at Midway Airport. In comparison, the means at the two listed New Orleans stations, Moisant Airport and Audubon Park, are 27.4°C and 28.0°C, respectively. For the normally warmest month, July, the Chicago means range from 22.8°C to 23.9°C; New Orleans, 27.8°C to 28.3°C.

With the above conditions, it would take about 170 years, rather than 20 years, for Chicago's average summer temperatures to equal those now experienced in New Orleans. This comment is not at all meant to deny the urgency of averting the greenhouse warming; a warming of even 1°C would be too much.

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Benveniste on *Nature* Investigation

In Robert Pool's article of 5 August (Research News, p. 658), the conditions of the publication and dismissal of our recent paper in *Nature*, "Human basophil degranulation triggered by very dilute antiserum against IgE" (*1*), are discussed instead of what is crucial to this debate, the validity of the subsequent *Nature* investigation (*2*) of our results. Three facts suffice to show that the *Nature* report is neither sound nor fair.

1) Figures 2 and 6 of the *Nature* report actually confirm our work. These figures show two remarkable results that were obtained—one after blind counting of cells in front of the "investigation squad" and the other in Milan. Both show 70% degranulation (achromasia) after 18 and 22 dilutions and after 34 and 35 dilutions, respectively. Yet these clear-cut data are not seriously challenged in the *Nature* report. For figure 2, I am supposed to have "not seen one like this before," but identical data are in table 1 and figure 1b of our original paper (*1*). Such data must be either real or "synthetic." Yet we are declared honest by the antifraud squad. The two curves shown in figure 6 of the *Nature* report are criticized for being "discordant" by two dilutions, which is exactly what we stated in our original paper (*1*).

2) The sentence "plainly this [the statistical noise] does not apply to all the data, for example, the fourth experiment" (shown in figure 2 of the report), appeared in the "final version" of the *Nature* report shown to me and was referred to in my reply (*3*), but it did not appear in the printed version of the report (*2*). Why? Most likely because it plainly means that a lot of our data are statistically sound.

3) The report complains (*2*, p. 290) that a legal official "is said not to have had time to decode" the data, and twice that data obtained in Israel "are not available"; the official report we have in hand (*4*) testifies that these data were decoded on 11 June 1987, and they are printed in our *Nature* paper (*1*). *Nature* thus calls "not available" data they published 1 month ago.

Our results are honest and true; the only problem is that they are unexplainable. If challenging results are condemned by means of specially designed laws (*5*), this constitutes a death penalty to science.

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HHS Policy on in Vitro Fertilization

Colin Norman's article (News & Comment, 22 July, p. 405) about the Department of Health and Human Services' announcement that an Ethics Advisory Board (EAB) would be restored was welcome news about a possible remedy to one of the most serious injustices in U.S. science. There was one error in his history. The recommendations of the original EAB that federal funds support research on clinical in vitro fertilization and untransferred human embryos were not "adopted as department policy" in 1979. Secretary Joseph Califano received but did not approve the recommendations, preferring first to publish them in the *Federal Register* for comment. The next HHS secretary, Patricia Harris, decided that the federal government should not be involved at all in such research and also decided not to recharter the EAB. The recommendations of the EAB have never been approved by a secretary of HHS and are not "department policy." Infertile persons and families at high genetic risk have been the great losers from the lack of research support and involvement of the National Institutes of Health in peer review of science in this area.

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Hazardous Waste Clean-Up

The Superfund project chosen for discussion by Mark Crawford (News & Comment, 24 June, p. 1725) is a clear example not just of management deficiencies in the national toxic waste program but of the persistant failure of the Environmental Protection Agency (EPA) to consider secondary consequences of its actions. Whether or not in situ vitrification, electrical heating of soil to a glassy state for encapsulating waste, is a suitable technology at the Ohio River Valley site, it is obvious that attaining a vitrifying soil temperature requires a very large electrical input. Most electricity in the Ohio Valley is generated by coal-fired power stations linked in a regional network that has an output governed by aggregate demand. Emissions from these stations are widely implicated as major contributors to the acid rain and ozone that are straining relations between the United States and Canada and are believed to be damaging forests in the northeastern United States and crops in the Ohio Valley. Coal burning also adds to the atmospheric carbon dioxide that most climatologists believe is leading to a worldwide greenhouse warming. EPA is active in all these areas, yet the agency's compartmentalization virtually precludes its Superfund program from considering such issues in choosing decontamination technologies.

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Erratum: In the report "Iron photoreduction and oxidation in an acidic mountain stream" by D. M. McKnight *et al.* (29 Apr., p. 637), reference 17 [E. L. Madsen, M. D. Morgan, R. E. Good, *Limnol. Oceanogr.* **31**, 382 (1986)] was cited (p. 638) as indicating "that biological processes are not responsible for light-induced Fe(II) production." This possibility was not addressed by Madsen *et al.*, nor did Madsen *et al.* use both poisoned and natural streamwater-sediment mixtures to distinguish between biotic and abiotic contributions to iron reduction. In table 1 of the same report, the values in columns two and three were incorrect. They should have been, for sodium, calcium and magnesium, respectively, 2.7 ± 0.34 mg liter^{-1}, 13.0 ± 0.73 mg liter^{-1}, and 4.3 ± 0.24 mg liter^{-1}.