



Allaying the Threat of Biological Weapons

SIR BRIAN HEAP'S EDITORIAL "SCIENTISTS against biological weapons" (16 Nov., p. 1417) reminds me of a mischievous and skeptical aphorism attributed to Amrom Katz, a shrewd arms control analyst at Rand Corporation many years ago. Katz said, "We have never found anything that the Soviets have successfully hidden" (1, p. 212).

It is one thing to say you are "against biological weapons," but it is another to recognize how difficult is the process of inspection and verification at reasonably high levels of reliability. We now know, of course, that the former Soviet Union manufactured tons of biological agents after pledging formally and publicly that they would not. We have good reason to believe that Iraq is in the same category of using talent to cheat on commitments. We suspect that North Korea is in that category as well.

So what is the answer? At least four efforts need to be sustained. First, international "norms" are a baseline. Although words do not deter everyone, it is extremely important to underscore over and over again the abhorrence all peace-loving people have about biological weapons. Second, occasionally it might be necessary to use force, such as in the present war in Afghanistan. Only force can work in the extreme cases. Third, we need a surge of effort by the National Institutes of Health, academic health centers, and industry on vaccines and drugs against biological weapons. And fourth, more research and development (R&D) should be devoted to improving ways of defending against biological weapons and verifying, if feasible, the terms of any treaty. The U.S. Department of Defense, including the Defense Advanced Research Projects Agency, merits our help.

Generalities about making treaties and warnings about the biological weapons threat are not enough. Hard work on the bully pulpit, military action when essential, biomedical research on therapy, and R&D on inspec-

tion, verification, and defense—these are the four keys to eliminating denial of the threat and shoring up the foundations of freedom.

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References and Notes

1. A. Katz, in *Verification and SALT: The Challenge of Strategic Deception*, W. C. Potter, Ed. (Westview, Boulder, CO, 1980), pp. 193–220.

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The reality of dealing with biological weapons.

Response

I WAS GLAD TO SEE THAT Nichols agrees that it is extremely important for everyone to show their

abhorrence to the use of biological weapons. The suggestion that attention should be devoted to developing effective therapies and vaccines is a further example of the valuable role that scientists can play in tackling the threat from biological weapons.

I welcome the creation of the liaison role between the U.S. National Academy of Sciences (NAS) and the White House Office of Science and Technology Policy, as this will enhance the impact of the extensive program being undertaken by the NAS on bioterrorism. However, the suspension this past November of the Fifth Review Conference of the Biological Weapons Convention until November

2002 was very disappointing. It suggested that some nations, particularly the influential United States, would need to enter into serious discussions about monitoring and verification in sensitive areas, such as the biotechnology and pharmaceutical industries (with emphasis on protecting intellectual property). Such discussions would be difficult, but the example of the Chemical Weapons Convention has shown that it is possible to include industrial interests and produce a verifiable international protocol.

Research on therapy vaccines and other defensive measures can only ever be part of the picture; we also need international commitment to reinforce the existing prohibitions on the development, production, and use of biological weapons. Bioterrorism and biological warfare is an international issue, and no individual nation will benefit by focusing on its own industries and defense to the detriment of a global effort to reduce the threat of biological weapons.

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Studies of Dietary Fat and Heart Disease

IN HIS LETTER ABOUT THE ARTICLE "THE soft science of dietary fat" (News Focus, G. Taubes, 30 Mar. 2001, p. 2536), Scott M. Grundy says that saturated fatty acids (SFA) are the main dietary cause of coronary heart disease (CHD) ("Dietary fat: at the heart of the matter," 3 Aug., p. 801), and he cites two reviews in support (1, 2).

In one of the reviews, there are no references (1); in the other, of which Grundy is a co-author, most of the references do not appear to be supportive of his statement (2). For instance, the authors say that "populations consuming diets high in saturated fats have relatively high levels of serum cholesterol and carry a high prevalence of coronary heart disease" (2, p. 34), referring to 12 studies (3–14). In the eight cohort studies

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(3–10), only one had examined the association between SFA and serum cholesterol (10), five found no increased SFA consumption among CHD patients (3, 4, 6, 9, 10), and one found a smaller consumption (7). In addition, three of the 12 studies were reports from a project comparing the incidence of CHD in native Japanese living in Japan with Japanese-Americans living in the United States (12–14). Although it is correct that the Japanese-Americans, on average, had higher cholesterol, ate more saturated fat, and had a higher incidence of CHD, the determining factor for heart disease was not their cholesterol levels or their diets, but how acculturated they were to Western culture (13).

Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted by e-mail (science_letters@aaas.org), the Web (www.letter2science.org), or regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.

Grundy also writes in his letter that lowering serum LDL cholesterol by dietary means reduces CHD risk. But the study he cites did not specifically address this question (15), and more to the point, meta-analyses of all controlled and randomized trials that have used modification of dietary fat as the only type of intervention have shown that neither the incidence of nonfatal CHD, nor coronary or total mortality, was lowered significantly (16, 17).

Grundy's way of presenting scientific data is not unique. An analysis of three influential reviews in this field showed that insignificant findings in favor of the diet-heart connection were systematically inflated, and unsupportive studies were either not included or they were quoted as if they were supportive (18).

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2. *Report of the Dietary Guidelines Committee on the Dietary Guidelines for Americans, 2000* (U.S. Department of Agriculture, Agricultural Research Service, Washington, DC, 2000).
3. R. B. Shekelle *et al.*, *N. Engl. J. Med.* **304**, 65 (1981).
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12. A. Kagan *et al.*, *J. Chronic Dis.* **27**, 345 (1974).
13. M. G. Marmot *et al.*, *Am. J. Epidemiol.* **102**, 514 (1975).
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15. M. R. Law *et al.*, *Br. Med. J.* **308**, 363 (1994).
16. U. Ravnskov, *J. Clin. Epidemiol.* **51**, 443 (1998).
17. L. Hooper *et al.*, *Br. Med. J.* **322**, 757 (2001).
18. U. Ravnskov, *J. Clin. Epidemiol.* **48**, 713 (1995).

A Case of Misinterpretation

LEWIS WOLPERT, IN HIS REVIEW OF MY BOOK *Science, Truth, and Democracy* (Books *et al.*, 25 Jan., p. 633), attributes to me positions that I do not hold and then sometimes criticizes those positions using points I actually make. I address a few of the examples here.

Wolpert says I seem “too sympathetic to the concept [of] the underdetermination of theory by evidence,” and that I should have provided examples of underdetermination as it might affect our views about DNA, for example (*I*). I actually said that we should be “wary of the global underdetermination thesis,” and I offered the example of the Watson-Crick hypothesis as a case in which

the thesis is implausible precisely because it is hard to think of serious rivals (2, p. 36).

Later in his review, Wolpert supposes that, on my view, “motives determine scientific understanding.” I claim no such thing, and indeed in chapter 2, I articulate his point that scientific “validity” is independent of “motives.”

Wolpert also asserts, apparently contrary to me, that “science by popular appeal would be a disaster.” I begin chapter 10 of my book by recognizing the perils of vulgar democracy and go on to consider, at some length, how scientific research might respond to public needs while recognizing the expertise of scientists (which Wolpert sees me as neglecting).

Wolpert ends by complaining that he has learned nothing from my book (and, by implication, from others in the philosophy of science). Like some of my fellow philosophers, I take pains to write for a nonphilosophical audience, but no amount of clarity is proof against a hasty reading.

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References and Notes

1. The underdetermination of theory by evidence is the thesis that there are always alternative theories that are equally well supported by the available evidence.
2. P. Kitcher, *Science, Truth, and Democracy* (Oxford Univ. Press, New York, 2001).

CORRECTIONS AND CLARIFICATIONS

REPORTS: “Dynamic brain sources of visual evoked responses” by S. Makeig *et al.* (25 Jan., p. 690). In reference 36, the name of the first author of the conference proceeding should be R. Goldman, not R. Chapman.

LETTERS: “Amplifying importance of new research in Peru” by D. H. Sandweiss, M. E. Moseley (23 Nov., p. 1651). An erroneous affiliation was given for M. E. Moseley. He is at the Department of Anthropology, University of Florida, Gainesville, FL 32611–7304, USA, and his e-mail is moseley@ufl.edu

REPORTS: “The sequence of the human genome” by J. C. Venter *et al.* (16 Feb. 2001, p. 1304). There were errors in two references. First, the author list was incomplete for reference 176. It should have read, “A. Krogh, M. Brown, I. S. Mian, K. Sjolander, D. Haussler, *J. Mol. Biol.* **235**, 1501 (1994).” Second, the title of the proceedings in reference 177 was incorrect. The reference should have read, “K. Sjolander, *Proc. Int. Conf. Intell. Syst. Mol. Biol.* **6**, 165 (1998).”