Tritium from Russia

Harold M. Agnew (Editorial, 13 Sept., p. 1475) has proposed that the United States buy annually from Russia the supply of tritium expected to be needed for U.S. nuclear weapons in about 10 to 12 years and argues that this approach would be substantially cheaper than either the accelerator approach to producing tritium or its production in commercial-type light-water power reactors in the United States.

I agree and, as a result of discussions with responsible Russian officials, believe that one can satisfy U.S. need for tritium in this way at about one-tenth the cost of domestic production.

Furthermore, by carrying one production design through to detailed planning and obtaining requisite permits, one answers possible concerns about a sudden cutoff of the supply of Russian tritium, in view of the buffer provided by the 12.3-year half-life of existing tritium stocks. I have studied tritium production options for the Department of Energy, and these are my personal conclusions.

Gustave K. Kohn (Letters, 25 Oct., p. 481) gives no indication of the cost of tritium production by low-voltage glow discharge, if indeed tritium is produced by such an approach. The world price of tritium is some $2 per curie, and Kohn tells us that 5 kilowatts for "hundreds of hours" produces "10's of nanocuries." If we assume the duration is 400 hours and that the "tens" are 30, then we see that 2000 kilowatt-hours ($100 at $0.05 per kilowatt-hour) produces 30 nanocuries, for a cost of $3 billion per curie.

Although the reactor approach and the accelerator may be 10 times more costly than buying tritium from Russia, a factor of a billion is out of sight.

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Misconduct Annotations

An 18 October ScienceScope item (p. 331) "Societies back Fisher" discusses the civil litigation between Bernard Fisher and the U.S. Department of Health and Human Services. The piece suggests incorrectly that annotations on listings in the electronic databases *Medline*, *Cancerlit*, and *Physicians' Data Query* implied that Fisher had committed misconduct. There was no such implication. These annotations derived from the Office of Research Integrity's (ORI's) final finding that scientific misconduct had been committed by Roger Poisson, a researcher who contributed data to the National Surgical Adjuvant Breast and Bowel Project (NSABP). The purpose of the annotations (which said, "scientific misconduct—data to be reanalyzed" and similar language) was to flag for the attention of the research community those articles that were based on NSABP data and that could have been affected by Poisson's contaminated data. As Judge Ricardo M. Urbina concluded (*1*), the annotated database entries were not about Fisher, but about the research underlying the articles. The agencies wanted the scientific community to be on notice that Poisson's misconduct, which had been confirmed, might have affected the validity of some of the articles.

The ScienceScope item also suggests that Judge Urbina ordered the removal of the annotations. This is only part of the story. The annotations were placed during the summer of 1994. However, when Fisher's attorneys complained about them in February 1995, the National Cancer Institute and ORI agreed to remove the "scientific misconduct" portion of the annotations. Without waiting for implementation

of that agreement, Fisher filed suit, and the parties then agreed to the portion of Judge Urbina's order (*2*) requiring removal of the annotations. Thus, the Court simply signed off on the agreement already reached by the parties. The piece also implies that the order was violated. The order required NIH to correct the databases that it controlled directly and to notify its licensees of the correction. NIH did precisely this. On 27 November 1995, Judge Urbina denied Fisher's suggestion that the court's earlier order (*3*) had been violated.

Scientists should not be concerned that annotations have been in the past or will be in the future placed in databases before a misconduct investigation is completed. They have not and will not be.

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References

1. R. M. Urbina, *Memorandum Order Granting Defendant's Motion for Summary Judgment and Denying Plaintiff's Motion for Partial Summary Judgment, Bernard Fisher, M.D.* v. *National Institutes of Health et al., C.A. No. 95* [District of the District of Columbia (DDC), Washington, DC, 13 August 1996)].
2. _____, *Bernard Fisher, M.D.* v. *National Institutes of Health, Order and Preliminary Injunction* (DDC, Washington, DC, 16 March 1995).
3. _____, *Bernard Fisher, M.D.* v. *National Institutes of Health, Bench Order* (not written) (DDC, Washington, DC, 27 November 1995).

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Red Alga Terminology

In the report "Nuclear encoding of a chloroplast RNA polymerase sigma subunit in a red alga" by K. Tanaka *et al.*, (28 June, p. 1932), the last sentence beginning on page 1932 read, "The chloroplast RNA polymerase and σ subunit of a related red alga, *Galdieria sulphuraria*, have recently been analyzed (*6*)." Reference 6 in our report referred to two papers by R. F. Troxler and his colleagues (*1*, *2*). Although the alga studied by Liu and Troxler in 1996 (*2*) has been called "*Cyanidium caldarium*" and has been used for many years in the title of their papers [for example, (*2*, *3*)], we found this "*C. caldarium*" strain (Allen strain) to be phylogenetically and systematically different from the *C. caldarium* RK-1 strain studied in our report. In 1991, J. Seckbach (*4*) proposed that the strain being studied by Troxler and his colleagues should be renamed *G. sulphuraria*. Confusion has resulted from two apparently different algae having the same name. Attempting to avoid further confusion in our report, we used the algal name *G. sulphuraria* instead of *C. galdarium* when referring to Liu and Troxler's work. We should have explained this choice in our report.

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References

1. R. F. Troxler, F. Zhang, J. Hu, L. Bogorad, *Plant Physiol.* **104**, 753 (1994).
2. B. Liu and R. T. Troxler, *Proc. Natl. Acad. Sci. U.S.A.* **93**, 3313 (1996).
3. R. T. Troxler, Y. Yan, L-W. Jiang, B. Liu, *Plant Physiol.* **107**, 985 (1995).
4. J. Seckbach, *J. Phycol.* **27**, 794 (1991).

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HIV-1 in Oropharyngeal Lymphoid Tissues

In the report "Replication of HIV-1 in dendritic cell–derived syncytia at the mucosal surface of the adenoid" (5 Apr., p. 115), Sarah S. Frankel *et al.* found that (i) "[m]any