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CRITIQUE VC 541 VC 542	24

Source (Journal, Vol., Number, Pages, Date)

1 2	61 62
	31
	32

Brief Summary

1 2 10	61 62
SUMMARY:	61
	62
	63
	64

Explosive reaction

SIR — Lovins'[1] review "Nuclear weapons and power-reactor plutonium" lacks mention of prior rebuttals to his position[2,3]. Moreover, in following the transition of this article with a widely circulated draft (personal communication, 20 March 1978), one senses a *post hoc* justification for his testimony at the Windscale nuclear-fuel reprocessing hearings.

The conclusions drawn by Lovins are unsupported by the analysis. He implies that "power reactors are not an implausible but are rather potentially a peculiarly convenient type of large-scale Pu production reactor". If power reactors were such practical sources, nuclear-weapons states would not be operating special-purpose military plutonium production reactors — they would be buying up reactor-grade plutonium for their weapons programmes, instead of storing it in pools. Reactor-grade plutonium is a dangerous substance, able to sustain a nuclear explosion. But Lovins fails to quantify its military value.

For governments that contemplate becoming nuclear-weapons states, the accessibility of plutonium is only one factor: others are the quality of material, the availability of facilities, and the possibility of nuclear-explosive testing. There are numerous disincentives too.

For "amateurs", there are hurdles not mentioned by Lovins. In general, plutonium generated in power reactors is not suited for military applications, and unauthorized access has been severely restricted by physical safeguards. His superficial treatment does not help our common goal of minimizing the propagation of nuclear weapons.

There are some specific technical objections: His introductory paragraphs are not justified by the subsequent analysis. He applies such terms as "unsuitable", "usable", "inferior", and "less than optimal", to power-reactor Pu. He generates a list of "assumptions" that represent selected extremes, later to render them "false". Although he says "We must know exactly what 'less than optimal' [Pu for bomb-making] means", he does not define it. Lovins eventually admits that there is "somewhat greater technical difficulty in using power-reactor Pu for effective military bombs."

His "*conservative assumption*" that arbitrarily high even-isotope content will not affect his arguments has been challenged, although Lovins does not mention the contradiction. On that assumption, Lovins proposes that "Pu discharged from power reactors will. . . have major military potential". His Table 2 does not reach high *concentrations* of the even-plutonium isotopes, thereby preventing the reader from making independent judgements on the efficacy of isotopic denaturing at higher even-isotope concentrations. Independent estimates of dramatic reductions in explosive yield that accompany isotopic dilution[3,4] are not included in Table 2.

He categorizes his choice of parameters as "realistic", thereby dismissing the deleterious effects of other forms of denaturing. Lovins claims that my "analysis of weapons physics seems deeply flawed". That is the entire support he gives to the charge. Yet, his own "weapons physics" is sparse, lacking any equations, and sustained mainly with citations from sources which I consider discreditable.

There are many misunderstandings, oversimplifications and omissions in Lovins' treatment of near-critical fissile masses: the difference between reactivity and excess reactivity; the role of synergistic effects; and his inability to distinguish between laboratory and field developments. I have identified and studied eight phenomena that can adversely affect the yield of an explosive configuration: critical mass dilution, metallurgical phase change, radial compression, subcritical multiplication, predetonation, generation time, reactivity limit and surface leakage. Lovins ignores most of them.

The interpretation that "the least favourable moment cannot get any less favourable" for an explosive yield is contradicted by Poisson statistics — a predetonation contour that Lovins notes but fails to apply. His repeated conception that the "worst case, minimum, 'fizzle' yield *is still*. . . 'militarily useful' ", is inconsistent with probability theory.

Examination of the citations taken by Lovins from Wohlstetter and Gilinsky indicates the origin of his circular arguments regarding plutonium denaturing. Of the limited official information generally available, most current reviews depend heavily on R.W. Selden's unpublished conclusions, which are self-contradictory.

It is incorrect to state "*categorically* that bombs from reactor-grade or deliberately 'denatured' Pu are less effective, less powerful or less reliable than those made from weapons-grade Pu". On the other hand, using a term from Lovins, it is "disingenuous" to deny that those attributes may be applied to the average properties of denatured plutonium. I estimate that plutonium can be isotopically denatured as effectively as ^{235}U or ^{233}U at a given fissile fraction. All reactor fuels and reactor types pose comparable levels of difficulty regarding weapons application, given corresponding safeguards and fissile fractions. Weaknesses of one or the other should be identified and remedied. However, the relative reactor usability and supply of isotopically denatured plutonium is another matter.

The point that the "marginal time and money required to use civil reactors for *military production* are orders of magnitude less than those needed for dedicated military facilities," has an element of truth mixed in with carefree terminology. One must differentiate between base power reactors and small research reactors. Research reactors do indeed provide a hedge, a "civilian cover" for some weapons-grade plutonium production; however, power reactors are self-protected and inaccessible under an appropriate safeguards regime.

Institutional and technological safeguards can minimize diversion and proliferation. As our respective nations expand their arsenals of destructive weapons, such misplaced concern only adds to irresolution. One can transcend some of the dispute by noting that international safeguards and a ban on all testing of nuclear weapons are the most prominent and effective steps that should be taken.

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1. Lovins, A.B. *Nature* 283, 817–823 [illegible] *Nature* 284, 190 [illegible]
2. *Power Engineering* [illegible] *Bull. at. Scient.* 34, 62, 59 [illegible]; 35, 50, 57 [illegible]; *Phys. Today* p 2 [illegible] (December 1978), p 33 (July 1979).
3. De Volpi, A. *Proliferation, Plutonium, and Policy: Institutional and Technological Impediments to Nuclear Weapons Propagation* (Pergamon, Oxford, 1979).
4. Sahin, S. *Nature* 287, 578 (1980).

Role for ESF?

SIR — One aspect of its work that the European Science Foundation (ESF) could substantially expand (*Nature* 20 November 1980, pages 202, 204) is the sponsoring of scientific conferences and schools, especially in interdisciplinary areas.

The association of NATO Advanced Study Institutes and NATO Summer Schools with the NATO military pact is a matter of continuing shame to the academic community. Scientific organizations should be pressing the national governments to agree to the ESF taking over NATO's role in conference sponsorship and funding in Europe. The US side could presumably be accommodated via bilateral NSF–ESF arrangements.

Let's end the shameful military link with scholarship forthwith!

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Theories of cancer

SIR — In reviewing my book *The Politics of Cancer*[1], Peto[2] charged that one table (Table 6.4) selectively and deliberately omitted data that would otherwise have questioned the conclusion that low dietary doses of saccharin (0.01 per cent) are carcinogenic in rats. This statement is incorrectly based on comparison of Table 6.4 with data in an appendix to the report of the Office of Technology Assessment[3]. However, the caption of Table 6.4 clearly states that it is based on another table, prepared by Melvin Reuber for use in the congressional testimony on saccharin by the Health Research Group on 21 March 1977[4]. Peto persisted in publishing this serious allegation in spite of two warnings.

Peto also appears unfamiliar with the content of the Office of Technology Assessment's report, which he cites as the basis for his charges on saccharin. This report explicitly discusses Reuber's low-dose data and his conclusion that "the increased incidence of lymphosarcoma of the thorax in rats at the 0.01 per cent . . . are highly significant in the saccharin study"[5]. Reuber also emphasizes that the carcinogenic effects of saccharin in various studies were not always dose-related.

Contrary to Peto's impressions, inversions in dose–response data are not uncommon in both experimental and epidemiological carcinogenicity studies. Reasons for such inversions include competing risks, heterogeneity in tested populations, and statistical fluctuation, particularly when dose–response curves are shallow. Peto also fails to recognize that saccharin has produced tumours in experimental animals at low as well as at relatively high doses.

Contrary to Peto's impression that "it is not surprising that so many chemicals (such as saccharin) at such (high) doses can cause cancer in animals", there is an overwhelming consensus in the qualified, independent

NATURE 289 115-116 (1981)

scientific community that high-dose testing does not produce false positive results, and that this is necessary to reduce the insensitivity of carcinogenicity tests, reflecting the small number of animals tested compared with large human populations at presumptive risk[6]. It is also well recognized that carcinogenicity testing in excess of maximally tolerated doses (MTD) can produce false negatives due to competing toxicity.

Peto's charge of selective omission of the saccharin data is not his only misrepresentation. After his circulation of drafts of his review to various US experts in the autumn of 1979, Schneiderman, then Associate Director for Science Policy of the National Cancer Institute (NCI) and co-author of the government report on the importance of occupational carcinogens[7], explained in a letter to Peto that breast cancer correlates as well with Gross National Product as fat, that occupation had been ignored in studies exclusively associating lung cancer with smoking, that there have been major recent increases in lung cancer among non-smokers and that there have been "big and frightening" recent increases in cancer incidence which cannot be accounted for by smoking or other lifestyle factors.

Similarly, Upton, then Director of NCI and a co-author of the report[7], challenged the erroneous charge that a "4–5" multiplication factor was used to inflate the 1978 government estimates of cancer anticipated from occupational exposures. ". . . This simply is not true . . ."

Repeatedly, Peto dismisses as personal views statements in *The Politics of Cancer* to which he takes exception, rather than recognizing that they are based on fully referenced primary sources and without attempting to challenge these sources directly. For instance, he disparages the conclusion "that the cure rates for major cancers have not been improving much over recent years" without noting that this reflects cited NCI data. Peto refers to the "claims that cancer costs the US economy over $25 billion per year" without recognizing that such figures are derived from NCI sources. Similarly, Peto criticizes as "misleading or unbalanced" references to the term "medical-industrial complex" without attribution to its source, referenced and identified in the text (p.77), as the caption of an editorial in *The Lancet*[8].

Peto charges that the book denigrates the value of short-term carcinogenicity tests, which he asserts is a "most inexplicable error of scientific judgement". While problems of such tests, particularly the limited associations between carcinogenicity and Ames test data for compounds from a wide range of structural classes[9,10] are recognized, the book concludes (p.68) that there is a "range of useful applications" for these tests, particularly when incorporated into battery protocols.

Peto accepts that industries "delay or obstruct any hygienic measures which will cost money, . . . (and) that the scientific literature is not immune from distortion by financial interests". Peto apparently also accepts the wide range of case studies in *The Politics of Cancer*, which document a common pattern of constraints, including manipulation, distortion and destruction, in health, safety and economic data generated or interpreted by industry and its consultants. Yet he seems willing to accept studies sponsored or endorsed by industry as authority for the nearly exclusive lifestyle theory of cancer causation. Moreover, he is opposed to further regulatory controls on grounds of costs, professed "inactive conservatism", and because "for most toxic chemicals, we now have both qualitative and quantitative uncertainty about the health benefits of restriction". This seems a questionable basis for prudent public health policy.

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1. Epstein, S.S *The Politics of Cancer* (Sierra Club Books, San Francisco 1978, revised and expanded in Anchor/Doubleday, New York, 1979, to which quotations refer).
2. Peto, R. *Nature* 284, 297-300 (1980).
3. *Cancer Testing Technology and Saccharin* U.S. Congress, Washington, D.C., June 7, 1977.
4. Wolfe, S.M. Johnson, A. *Public Citizens Health Research Group, Testimony Before Subcommittee on Health, House Commerce Committee Hearings on Saccharin*, March 21, 1977.
5. Reuber, M.D. *Envir. Hlth Perspect.* 25, 173 (1978).
6. Report to the Surgeon General, USPHS. *Evaluation of Environmental Carcinogens* (Ad Hoc Committee on the Evaluation of Low Levels of Environmental Chemical Carcinogens, April 22, 1970).
7. Bridbord, K. *et al. Estimates of the Fraction of Cancer in the United States Related to Occupational Factors* (NCI, National Institute of Environmental Health Sciences, and National Institute for Occupational Safety and Health, 15 September, 1978).
8. *Lancet* ii, 1380 (1973).
9. Rinkus, S.J. & Legator, M.S. *Cancer Res.* 39, 3289 (1979).
10. *International Program for the Evaluation of Short Term Tests for Carcinogenicity* National Toxicology Program, Dept of Health and Human Services, April, 1980.

The article "Fallacies of lifestyle cancer theories" by S.S. Epstein and J.B. Swartz begins on page 127 of this issue.

Spanish science

SIR — The publication (*Nature* 23 October 1980, p.674) of the *Manifesto* on the status of science in Spain issued by leading academics and scientists of universities and research institutes is a major event. I would like to make a few comments from the viewpoint of a scientist who has been working closely for over ten years with colleagues at Spanish universities.

This is a time of great expectations both nationally and internationally for Spanish science and engineering. Democratic changes and heightened national expectations have opened the door for a major strengthening of scientific, engineering and technological work. The largely dormant past in universities and the undeveloped governmental infrastructure for science are giving way to a healthy stirring throughout the whole system. A recent visit to Madrid confirms a vigorous self-examination of the structure of research and teaching with significant organizational changes proposed.

There is, of course, no unique set of circumstances that will guarantee motivation of faculty and students, but there generally appear to be three essential elements. Scientists must be assured of government respect for scientific work without political interference; there must be reasonable financial support and job opportunities for researchers and graduate students; and there must be adequate and respected links between government, universities and public and private research institutions. Only the first element is now firmly in place. Difficulty in providing the second element is not unique to Spain, but occurs in many industrial countries of moderate size where the number of research positions is limited. At the moment, for this reason, some of the best Spanish science and engineering graduates see their future abroad.

Academics might themselves develop research institutes within the universities directed to national needs such as high technology, engineering services, mitigation of earthquake and other natural hazards, and agricultural improvements. Although such efforts are exhausting and time-consuming, entrepreneurship to draw in industry, provincial governments and private funds is an essential part of scientific vigour. The *Manifesto* appears to address largely the central government. No doubt, realistically at this stage, the major funding for research must come from Madrid, but, in the long run, a diversity of support would appear to be the most fruitful.

The third element should be easier to achieve since it does not in itself entail much expenditure. The need to examine critically scientific goals and infrastructure related to national needs has in other countries led to the establishment of something like a Royal Commission with independent powers to probe, report and recommend. Strengthening and linking government institutes undertaking research, similar to the highly effective scientific and industrial research organizations of Australia, New Zealand and elsewhere might be considered. There appears to be a need to integrate government scientific institutes more closely with universities, given the new social climate in Spain in which protection from debilitating political agitation is not required.

From my own experience, it is crucial for scientists and engineers in Spain to organize themselves systematically so that their professional societies and academies are representative and respected. The work of such associations can clearly demonstrate to the body politic (of all parties) that among the many competing interests in a modern state, science should have a high priority on both economic and cultural grounds. Rather than an emphasis on "rights", continual demonstration is needed of the way that scientific enquiry really maintains itself in a free society. Again, from my own experience, it is important to cultivate those members of the *Cortes* who are scientists, engineers, technologists and economists. If they are to accept the responsibility of speaking for the Spanish scientific community they must have the necessary contacts and realistic proposals that can be argued effectively.

Finally, as a sympathetic outsider, I am struck by the role that Spanish science and technology might play in the developing world. Much of Central and South America lies open, one would suppose, to the establishment of close ties with Spanish scientists and institutions. The value of common language and other heritages cannot be overemphasized. Although some cultural and scientific ties have been forged, there are exciting possibilities for an expansion of teaching, scientific exchanges, bilateral scientific and technological agreements.

Through such channels, the idealism and abilities of many Spanish science graduates could find a natural place that serves not only national self-interest but also the global responsibility of all scientists. Might we not look forward to a vigorous Spanish science that belongs not only to Europe but also to the Americas? Part of the answer lies in the force of the *Manifesto* itself.

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