Letters

Energy: Calculating the Risks (II)

Herbert Inhaber, in his article "Risk with energy from conventional and nonconventional sources" (23 Feb., p. 718), concludes that the health hazards of deriving energy from wood, wind, and sunlight are comparable to those of using coal or oil and much greater than those of using nuclear power. The article, however, displays none of the calculations on which this surprising conclusion supposedly rests, but simply describes the author's approach and summarizes the results. For all the details, readers are referred to Inhaber's report for Canada's Atomic Energy Control Board [(*1*), hereinafter referred to as AECB 1119]. We have examined AECB 1119 in some detail, having been motivated to do so in part because one of us was named in the article and in the report as the "well-known nuclear critic" whose data Inhaber says he has used to preclude accusations of pronuclear bias. A report we coauthored (*2*) is indeed the source of a number of AECB 1119's citations, but Inhaber has both misrepresented and misused our results.

We are not the only ones thus abused. Comparison of AECB 1119 with its references reveals instance after instance where Inhaber misread his sources or propagated errors. As we shall show here, in fact, Inhaber's report is a morass of mistakes, including double counting, highly selective use (and misuse) of data, untenable assumptions, inconsistencies in the treatment of different technologies, and conceptual confusions. Several statements in the *Science* article about how the numbers in the underlying report were derived, moreover, are misleading or wrong. When the effect of the major errors and inconsistencies in AECB 1119 are removed, the *Science* article's conclusions change drastically: the difference between coal's health hazards and those of nuclear power shrinks, and the calculated hazards of the renewables fall to near or below those of nuclear.

These are serious charges. We document them here at such length as *Science*'s space limitations permit, and in greater detail elsewhere (*3*). We begin by comparing some of the article's assertions with what one finds in the underlying report.

Inhaber makes many statements in the article conveying the impression that he has treated conventional and nonconventional energy technologies on the same footing. But examination of AECB 1119 shows that the implied systematic approach and consistency are absent. Indeed, with all Inhaber's emphasis on the occupational risks of constructing energy facilities, he clearly has not included the occupational risks of building coal, oil, or nuclear power plants in the risk figures for these technologies. The numbers tabulated in AECB 1119 for occupational deaths and injuries in the coal, oil, and nuclear fuel cycles and summarized in figure 5 of the *Science* article come for the most part directly from Smith *et al.* (*2*) and are for operation and maintenance only; they include no contribution from materials acquisition, component manufacture, or plant construction. Inhaber lists materials requirements and partial labor requirements (onsite construction but not materials acquisition or component manufacture) for the conventional technologies in the article and in AECB 1119, but he does not apply the methodology he has described to translate this information into occupational risks for inclusion in his totals. If he had used this methodology for nuclear power in the same fashion he did for the renewables, the lower bound of nuclear's occupational risk as presented in the article's figure 5 would have been about 1.7 times higher and the upper bound about 1.15 times higher.

Inhaber claims at several points in the article that he has bent over backward to avoid any bias toward nuclear power. Concerning public risk from reactor accidents, for example, he writes: "To avoid any bias in favor of nuclear power, I used the highest values of public risk from reactors taken from a wide number of sources (in some of these, Rasmussen's values were used)." Indeed, the *Science* article and AECB 1119 lead the unwary reader to believe that the references from which "the highest values of public risk from reactors" were taken include not only the Rasmussen report but also the Ford/MITRE study (*4*) and Smith *et al.* (*2*). In fact, however, Inhaber's "upper limit" figure for reactor risks is about three times smaller than the upper limit given in the Rasmussen report, more than 40 times smaller than the upper limit given in Smith *et al.* (notwithstanding Inhaber's taking credit, based on his citation of this reference, for using the values of a "well-known nuclear critic"), and more than 200 times smaller than the upper limit implied in the Ford/MITRE study. Had Inhaber actually used, say, the Smith *et al.* upper limit, the upper limit on public man-days lost per megawatt-year of nuclear-generated electricity would have been about 60 rather than the 1.5 shown in figure 6 of the *Science* article, and the upper limit on nuclear's total man-days lost per megawatt-year in figure 7 would be about 70 rather than the 10 shown.

Inhaber's declaration that "present-day technology, models, and systems with their corresponding risk, are used" is also deceptive. His occupational and public risks from the coal fuel cycle, for example, are based in part on practices that are either illegal in present U.S. operations (coal-dust levels in mines) or in new plants (SO_2 emissions). Present dust standards imply occupational deaths from black-lung disease as much as 60 times lower than the figure used by Inhaber. Correction of the black-lung figures would lower Inhaber's upper limit of the occupational man-days lost per megawatt-year of coal-generated electricity by a factor of 1.4.

The sulfur dioxide emissions Inhaber says he considered (*1*, ed. 3, p. A-1) fall within the New Source Performance Standards in force at the time the report was written; but the upper limit of the number of public deaths from sulfur dioxide-related disease (table A-2, all editions), which is used in the risk calculations, corresponds not to these emissions but to emissions five times higher, exceeding the New Source Performance Standards by a factor of 3.3 (*5*). The net inflation of public man-days lost from the coal fuel cycle is a multiplicative factor of 1.3 compared to what would be obtained by consistent use of the New Source Performance Standards.

Inhaber's exaggerations of the risks of

coal use also inflate substantially the apparent risks of the renewables, since most of the upper-limit risk of the latter comes from coal "backup" in all cases where backup is assumed to be required. (The *Science* article's figure 4 indicates that 62 percent of the upper-limit risk of wind, 74 percent of the upper-limit risk of photovoltaics, and 85 percent of the upper-limit risk of solar-thermal-electric systems come from the assumed coal backup.) Since Inhaber's entire treatment of storage and backup for renewables is intricately fallacious, however, one cannot get reasonable figures for this part of the risk simply by removing the inflation from coal's effects. Inhaber has used the same (wrong) ratio of backup to renewable energy and the same quantity and type of storage, for example, for all three kinds of systems, notwithstanding their entirely different characteristics and the different roles they would play in utility grids.

Inhaber's common ratio of backup to renewable energy is derived for the case of the solar-thermal-electric system (*1*, p. E-7, all editions), but he has it wrong. He assumes (and tallies up the materials requirements for) an energy-storage capability of 16.5 hours of operation at 70 percent of rated capacity, which his references indicate would permit an annual load factor of about 85 percent. Yet to the risk computed for each 1000 megawatt-electric years delivered by this system, he adds the risk for 19 percent as much energy—190 megawatt-electric years—from coal as "backup." Here Inhaber appears to have misunderstood his source on backup requirements (*6*). The solar plant described needs no *net* backup energy at all to be the energy-producing equivalent of a conventional baseload plant *with the same annual load factor*, although it needs some backup *capacity* if the reliability characteristics of the grid are not to be altered by the solar plants. Removing the risk of the superfluous backup energy would reduce Inhaber's upper limit estimate of the risk of solar-thermal-electric systems more than sixfold.

The manner in which Inhaber has sampled the literature for the data he uses in his report is also remarkable. In AECB 1119's treatment of photovoltaics, for example, he starts with materials requirements from an unpublished Jet Propulsion Laboratory (JPL) interoffice memorandum dated May 1976, ignoring in so doing the somewhat lower numbers published in the final report of the same project (*7*), which report he also cites. Then Inhaber asserts (*1*, p. F-1, all editions), ostensibly on the basis of a report in a 1971 conference proceedings, that the land requirement for photovoltaics is 34,500 square meters per megawatt-year of electrical output rather than the 3800 square meters per megawatt-year used by JPL, and that it follows that the JPL materials requirements per megawatt-year must be multiplied by a correction factor of 2.27. Inhaber's basis for making the inflation factor 2.27 instead of 34,500/3800 = 9.08 is his supposition that the 34,500 square meters per megawatt-year "refers to peak power" and should therefore be divided by 4 to correspond to the average output. This is an astonishing bit of reasoning. First, peak power is measured in megawatts, not megawatt-years. Second, it takes about four times *less* area to make a peak megawatt than to make an average megawatt, not four times more. Third, it is an elementary calculation to verify that the JPL land requirement was correct in the first place (*8*), so no "correction" to the JPL materials requirements on this basis is warranted at all. Removal of this error alone reduces the nonbackup part of Inhaber's upper-limit estimate of risk from photovoltaics by a factor of about 1.7.

As our final detailed example, we consider Inhaber's treatment of methanol from biomass. He assumes that the methanol is made from wood obtained in conventional logging operations (in the treatment of which he makes many errors we will not detail here) and that the product is used to drive automobiles at 12 percent efficiency (mechanical work at the wheels divided by chemical energy in the fuel). Inhaber contends it is fair to consider a megawatt-year of electricity produced at a power plant to be equivalent to a megawatt-year of mechanical energy delivered to the wheels of automobiles because the electricity "could have been used to drive autos and buses." The absurdities in this contention are too many to explore thoroughly here. We note only that (i) losses between the power plant and the wheels of electric autos (transmission and distribution, battery charging and discharging, and losses in the controller and in the electric motors themselves), completely ignored in Inhaber's comparison, are typically around 50 percent; (ii) if electric vehicles really made more sense, one could easily burn the wood directly to make electricity without suffering the significant conversion loss in going from wood to methanol.

Inhaber's actual numerical procedure to calculate occupational risks of building methanol plants is to take numbers for oil refineries from Comar and Sagan (*9*) and multiply them by "correction factors" of $3.0 \times 2.0 \times 1.5 = 9.0$. The 3.0 is the ratio of the efficiency of electricity generation with oil (0.36) to the assumed fuel-to-work efficiency of methanol in automobiles (0.12); multiplication by this factor is completely incorrect, as noted above. The factor of 2.0 Inhaber explains as being due to the fact that methanol contains only half as much energy per gallon as does gasoline; hence, he contends, it requires twice as much materials and labor to build a methanol plant as to build an oil refinery. This, too, is wrong. If volumetric energy density of the product governed the size and complexity of the facility, coal-gasification plants would be impossible. The fact is that methanol-from-biomass plants require fewer and less complicated operations than oil refineries and would probably require less construction material and labor, not more. The factor of 1.5 comes from Inhaber's assumption that methanol plants last only 20 years, while oil refineries last 30. His reference on this point gives 20 years as the owner's depreciation period for accounting purposes, having nothing to do with physical lifetime. Thus the whole factor of 9 is an arbitrary and unwarranted inflation of the materials and labor requirements of methanol; what crowns the performance is that the values from Comar and Sagan that Inhaber multiplies by the factor of 9 are not for construction at all, but for operation and maintenance. Removal of the first unwarranted "correction" factor would reduce the total risk due to methanol as shown in the article's figure 7 by a factor of 3.0 since this factor pervades every methanol calculation in AECB 1119. Removal of the other inflation factors and errors in the methanol calculations [see (*3*)] would reduce the various components of Inhaber's methanol risk by additional factors of 1.5 to 10.

We could go on and on, but we believe we have presented enough detail to give the reader the flavor of what is in and behind Inhaber's article in *Science*. Correcting just his largest errors completely transforms his results, raising the upper limit of nuclear risks to public and occupational health into the lower part of the uncertainty range for coal and oil, and dropping the health risks of the nonconventional sources into the middle of the uncertainty range for nuclear. Even correction of *all* of Inhaber's errors would not produce the "right" answers about relative risks of conventional and nonconventional energy technologies, of course, because many needed data are as yet nonexistent, and because important categories of harm are left out of his approach altogether. But by propagating an analysis riddled with distortions, errors, and inconsistencies, Inhaber has muddied rather than illuminated even the circumscribed part of the risk problem he tackled.

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References and Notes

1. H. Inhaber, *Risk of Energy Production* (Report AECB 1119, Atomic Energy Control Board, Ottawa, Ontario, March 1978); *ibid.*, ed. 2, May 1978; *ibid.*, ed. 3, November 1978. The *Science* article does not specify to which of the three editions it refers, and some of its numbers differ from those in all three. Our comments on AECB 1119 here refer to the third edition unless otherwise specified.
2. K. R. Smith, J. Weyant, J. P. Holdren, *Evaluation of Conventional Power Systems* (Report ERG 75-5, Energy and Resources Group, University of California, Berkeley, July 1975). Inhaber's first reference in his AECB report contains 30 citations to this report, 13 direct ones, plus 17 more where Inhaber took the data from our report but mentioned also the original source we had cited.
3. J. P. Holdren, K. Anderson, P. Gleick, I. Mintzer, G. Morris, K. R. Smith, *Risk of Renewable Energy Sources: A Critique of the Inhaber Report* (Report ERG 79-3, Energy and Resources Group, University of California, Berkeley, April 1979).
4. Nuclear Energy Policy Study Group, *Nuclear Power: Issues and Choices* (Ballinger, Cambridge, Mass, 1977). The authors state on p. 179 that "the expected number of cancers could be several times higher, depending on the assumed dose-response model used in deriving the risk estimates," than the values given in the Rasmussen report. On the same page, they note that "the WASH-1400 probability estimate could be low, under extremely pessimistic assumptions, by a factor of as much as 500." The implied upper limit on the product of probability and consequences is a factor of 1500 to 2500 larger than the WASH-1400 "best estimate." Inhaber's "upper limit" is only 6.7 times the WASH-1400 "best estimate."
5. To derive this result we used the upper limit of the National Academy of Sciences' dose-response relation referenced by Inhaber, for the most unfavorable location that the Academy considered (a plant sited 60 kilometers upwind from New York City) and worked backward from Inhaber's figure for public deaths to determine the emissions needed to produce these. See National Academy of Sciences, *Air Quality and Stationary Source Emission Control* (Government Printing Office, Washington, D.C., 1975), chap. 13.
6. R. Manvi, *Performace and Economics of Terrestrial Solar Electric Central Power Plants* (JPL Internal Report 900-781, Jet Propulsion Laboratory, Pasadena, Calif., October 1976). We have consulted the head of the JPL solar project of which this work was a part, and he confirms our analysis of the point and of Inhaber's error (R. Caputo, private communication, March 1979).
7. R. Caputo, *An Initial Comparative Assessment of Orbital and Terrestrial Central Power Systems* (Final Report, Report 900-780, Jet Propulsion Laboratory, Pasadena, Calif., March 1977). Inhaber propagated a number of errors from the 1976 JPL internal memorandum, despite early warnings from Caputo that this material was unreliable (R. Caputo, personal communication); in fact the memorandum appears to have been Inhaber's main source for his methodology and for much of his data relating materials requirements to occupational injuries and diseases.
8. Average insolation on a horizontal surface in the United States is about 180 watts per square meter (averaged over seasons and night and day). Assuming the collectors cover half the land area charged to the plant and that the efficiency of the cells in converting sunlight to electricity is 10 percent, and using the same 30-year lifetime assumed by Inhaber, yields 180 W/m² × 0.10 × 0.50 × 30 years = 270 watt-year/m², which gives 3700 square meters per megawatt-year.
9. C. L. Comar and L. A. Sagan, *Annu. Rev. Energy* 1, 581 (1976).

Paper Studies

We tabulate and ponder many aspects of our research and development (R & D) process in this country [see, for example, Senator Bayh's concerns with bringing developments to application (Letters, 12 Jan., p. 120)]. Scholars have devised thoughtful models of the process of technological innovation. For instance, Kelly *et al.* (*1*) call attention to its nonlinearity, and Wenk and Kuehn emphasize the multifaceted governmental roles (*2*). However, to the best of my knowledge, neither the conceptualizers nor the empiricists—see (*3*)—have focused on the dimension of "physical" R & D versus paper studies.

It is difficult to specify what fits into the paper study category. I have in mind such things as forecasts, technological feasibility and market studies, cost-benefit analyses, environmental impact statements and technology assessments, systems and policy analyses, and program evaluation. I speculate that such endeavors represent a substantial fraction of the federal R & D budget, and that they play a crucial role in directing the technological innovation process. But I don't know and wonder if anyone does now know.

I suggest that compilation and dissemination of some basic information on the dimension could usefully address a number of issues. For instance, as a faculty member in a department that trains operations researchers and systems analysts, I would like to know the scale of efforts supported in such areas. From a national perspective, one could ask what sort of people perform various paper studies and whether they are suitably trained? For example, the growing commitment to program evaluation requires many professionals. Are we educating such people in the most sensible manner for this task or just relabeling willing contract researchers? It would also seem worthwhile to inquire broadly into who