LETTERS

Kovalev's Health

In late May and early June 1979, the 20th International Symposium on Electrocardiology will be held in Yalta and a meeting devoted to comparative electrocardiology will be held in Syktyvkar. Electrocardiographers and cardiac electrophysiologists have been invited to attend these meetings at a time when the famous cardiac electrophysiologist Sergei A. Kovalev may be near death in a Soviet prison.

Kovalev was arrested in 1974 in connection with the publication of the *Lithuanian Catholic Chronicle* and the *Chronicle of Current Events* (News and Comment, 5 Nov. 1976, p. 585). In mid-1975, soon after the imprisonment of Kovalev, 48 cardiac electrophysiologists throughout the world appealed to the government of the U.S.S.R. on his behalf. In 1976 an appeal appeared in *Science* (Letters, 8 Oct. 1976, p. 133) which pointed out not only that Kovalev was gravely ill but that if he were free and allowed to leave Russia, a post awaited him at Cornell University.

Not long ago, 55 European cardiac electrophysiologists issued a renewed appeal on behalf of Kovalev. Since then word has arrived from Russia that Kovalev is in very poor health and is regarded by his fellow prisoners as a "doomed man."

Under these circumstances, those who attend these meetings may wish to make their concern about Kovalev known to their hosts if an opportunity for them to do so presents itself. Others may wish to join us in declining to attend either of the meetings and in making their reasons known to their colleagues in Russia, to their own scientific organizations, and to the governments of their own nations.

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Medical School Facilities in Cincinnati

William J. Broad (News and Comment, 23 Feb., p. 724) writes about Caribbean medical schools. I view his article as a balanced presentation of a very difficult problem in which there is a definite possibility that the public, the students, or others may be exploited. Further attention from the regulatory bodies is important and necessary.

One quote in this article suggests that the University of Cincinnati supports and aids the development of the American University of the Caribbean. Our faculty, administration, and students are deeply concerned about this entity and its quality. We neither oppose nor support it because we believe other bodies or mechanisms must reach decisions about its acceptability. In the interim, we shall not act to exclude its students or faculty from resources that are open to the public. We will, however, insist that they do not use facilities and resources intended for our own students.

This very serious and dangerous situation should be given widespread professional and public attention.

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Energy: Bechtel Cost Data

J. Michael Gallagher of Bechtel National Inc. (Letters, 22 Dec. 1978, p. 1242) protests that, because my computation (Letters, 22 Sept. 1978, p. 1077) of the high cost of delivered electricity produced by nuclear plants uses Bechtel capital-cost data, my conclusion (*1*, *2*) that nuclear power is not competitive with soft technologies is being "implicitly" ascribed to Bechtel.

Not by me. My allusions to the data base of the 1975 Bechtel Energy Supply Planning Model (ESPM) (*3*, *4*) refer unambiguously and exclusively to the capital costs specified (*5*)—as readers will find by restoring Gallagher's twice-quoted "In fact, they are Bechtel's data" to its restrictive original context. Wherever I use additional, non-Bechtel data (capacity factors, deflators to 1976 dollars, fuel and operating costs, soft-technology capital costs, and so forth), that fact and the data sources are explicitly stated. Gallagher considers this "extremely misleading." I do not see how it can be plainer.

Gallagher writes: "In my judgment his [Lovins's] data and conclusions bear little relation to the ESPM data base with which he purports to have started." This implies that those cost data which I cited as drawn from the ESPM were not in it, or were copied or used incorrectly. Fortunately, Gallagher does not actually say that, for it is untrue. The ESPM data base is good for exactly what I cite it for. It should be, since I was at pains more than 2 years ago to confirm with him (and with the ESPM's then director, Meir Carasso) that I was interpreting their data correctly (*1*). In October 1976, I queried and resolved with Gallagher several unclear or inconsistent details of the unpublished internal documents underlying the published ESPM data base. He and Carasso knew the type of whole-system cost calculation I was doing. Neither expressed any reservation about such a use of their data. Both were helpful (*1*).

I relied on the ESPM data base for hard-technology capital costs (including ancillary facilities) because at the time of my analysis in 1976 it was the most comprehensive, detailed, consistent, and officially credible source available. It was therefore being widely used in 1975 and 1976 in federal agencies, not only for the aggregate resource calculations for which it was mainly intended—and for which I also used it (*1*)—but also for broad technology cost calculations analogous to mine. Whatever uses the data base might be put to, Bechtel surely sought to ensure its accuracy: the ESPM report estimates that its capital costs for the electric facilities I considered are accurate to −10 percent, +20 percent.

Gallagher quotes a part of the report which, read hastily, might seem to warn against using the data base to compare different technologies. It actually said that optimizing the detailed choice of particular technologies was not its main purpose; Gallagher's quotation should have continued (*3*, vol. 1, p. 6-5).

> . . . e.g. no distinction is made [in the data base] between a coal fired power plant using supercritical steam conditions and others; nor between BWR's [boiling-water reactors] and PWR's [pressurized-water reactors], etc. . . .

My analysis, like the ESPM, relies on nominal facility characteristics rather than making fine distinctions of type, size, or design. The ESPM report nowhere suggests that its data are unsuitable for my kind of rough cost comparison between broad categories of technologies. Further, the end-use cost advantage I calculate for soft technologies over nuclear power (*2*) is robust—conservatively a factor of 2 to 3—rather than sensitive to subtle refinements as Gallagher implies.

Having said his data base is unsuitable for comparing technologies, he next criticizes me for *not* having so used it: he claims that for consistency I should have used the ESPM's solar heating and cooling costs. I didn't—an omission irrelevant to the validity of my hard-technology cost calculations—because cooling should be done more cheaply by good architecture and because the ESPM solar heating system (*6*) was

grossly suboptimal. It used a packaged rather than a field-erected collector—roughly doubling the cost per square meter (*7*); was about seven times oversized (*8*); and required backup even though 100 percent solar heating is cheaper for a house to which cost-effective heat-conserving measures have been applied first (*9*). Seeking the best buy for each technology, I relied on Bechtel data in Bechtel's area of expertise—hard technologies—but, outside it, preferred other data sources (*2*) that were more detailed and technically sound and included all the other soft technologies I wished to analyze. (The ESPM included no others.) Contrary to Gallagher's implication, I normalized all cost data to the same accounting conventions for all technologies.

The ESPM was intended, as Gallagher states, to calculate only total capital costs, not fuel or operating costs (*10*). For a complete accounting I therefore had to add non-Bechtel auxiliary assumptions (*1*, *2*). Seeking to show that my results "are often dominated more" by those than by the ESPM capital costs, Gallagher compares my first published capital-cost calculation (*11*)—"$3179 to $5000" (*12*) per kilowatt electric (kWe) delivered for nuclear and $2476/kWe delivered for coal—with the ESPM's total capital cost of "approximately $1100/kW installed capacity for both coal and nuclear systems (in 1974 dollars, including transmission and distribution)." Of course they differ—like comparing apples with horned toads. Gallagher's costs are per kilowatt electric installed, not delivered; using the capacity factors from (*13*) and the ESPM's transmission and distribution (T & D) losses [16.4 percent (*1*)] and capital costs ($489/kWe installed), the ESPM's actual 1974-dollar capital costs of $1074 and $964/kWe *installed* for nuclear and coal, respectively, would become $1976 and $2306/kWe *delivered*. Gallagher has also omitted (*1*, *2*, *11*) the ESPM's marginal fuel-cycle investments and my initial core cost and compared his 1974 dollars with my 1976 dollars. The discrepancies he cites arise solely from these sources, which result from his arbitrary omissions, not my "arbitrary assumptions"; and my non-ESPM assumptions are not "arbitrary" but based on the best available statistical fits to the historic data. Further, substituting Bechtel's capacity factor and T & D losses for mine as above does not, as Gallagher implies, "strongly influence" the nuclear capital cost per delivered kilowatt electric that he cites (*11*) but reduces it by a mere 0.6 percent to

$3158—and *increases* the coal-system cost 32 percent to $3278 (all in 1976 dollars).

Gallagher objects to my use of the ESPM data base as it stood at October 1976 (*4*), saying it is outdated. Of course, I could not have used post-1976 data in a 1976 analysis, and in defending that analysis subsequently it would be confusing to substitute new data rather than reexplain the old. But I emphasized in my letter that newer data are available, such as the Mooz (*14*) and Komanoff-Taylor (*15*) regressions for light-water reactor capital cost, and that using them results in *higher* nuclear costs—hardly congenial to Gallagher's case. I retained the 1975 ESPM data as a deliberate conservatism, and said so (*16*).

For example, I convert the ESPM reactor cost, $585/kWe installed in 1974 dollars, to $929 in 1976 dollars using the Bupp & Treitel index (*1, 2, 11*), which shows r^2 of 0.71 on a 35-plant sample. The newer Mooz and Komanoff-Taylor regressions, with respective r^2's of 0.76 on 39 plants and 0.83 on 42 plants, would have yielded corresponding costs (in 1976 steam-plant dollars) of $1474 and $1330 (*17*). Other recent studies (*18*), including two by Bechtel (*19, 20*), are also consistent with or higher than my $929/kWe. Using all Gallagher's latest 1977 costs *and* his other assumptions (*21*), then deflating by his 7 percent to 1976 dollars, yields $1037/kWe installed for the reactor and $2905/kWe delivered for the whole-system nuclear capital cost, compared to my $929 and $3495. Thus his latest data, far from confuting or "substantial[ly] reworking" my findings, broadly confirm them.

His final quotation, about the relevance of economic calculations, referred in its original context (*1, 11*) to the importance of sensitivities, externalities, and the two-orders-of-magnitude lower price of the costliest industrial energy relative to the cheapest human labor. But I agree that economic calculations, which are worth doing for an audience that considers them important, should be "based on consistent and current information," conservative, scrutable, thoroughly documented, and widely published for protracted peer review. That is just what my cost calculations are. May I therefore hope this will be the last time I ask in these columns that we stop inventing tedious new misreadings and start getting on with better energy policies?

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References and Notes

1. A. B. Lovins, *Soft Energy Paths: Toward a Durable Peace* (Harper Colophon, New York, 1979; originally Friends of the Earth and Ballinger, Cambridge, Mass. 1977, and Pelican, 1977), especially pp. 105-115.
2. ———, *Annu. Rev. Energy* **3**, 477 (1978).
3. M. Carasso, J. M. Gallagher, K. J. Sharma, J. R. Gayle, R. Barany, *The Energy Supply Planning Model* (final report to the National Science Foundation, Bechtel Corp., San Francisco, August 1975; available from the National Technical Information Service, Springfield, Va.).
4. As updated to October 1976. I stated (*1*) that Gallagher and Carasso had kindly given me the results of Bechtel's data-base update to October 1976; Gallagher states in his letter that this update had not changed the original 1974–1975 cost estimates for the systems I considered; both statements are correct. Our autumn 1976 communications did, however, confirm earlier cost figures, and resolved several errors and ambiguities in other relevant data (such as the definitions of transmission-line modal splits) used in the original ESPM report (*3*). The 1975 base costs are still not updated today (*16*).
5. In this letter, capital costs mean direct construction costs plus owner's costs (mainly for interest during construction)—given, respectively, in the ESPM's tables D-3 and D-2 (*3*, vol. 2)—and all coal-electric plants (called simply "coal" for short) include scrubbers.
6. The ESPM nominal solar heating system, about which I could obtain no further details, was described only as a 500-square-foot, single-house, active system with 1000-gallon storage and an auxiliary boiler, supplying 70 million Btu's per heating season. In 1974 dollars, it cost $10,350 (−20 percent, +40 percent) per house, or $20.7 per square foot [(*3*, table D-3) gives the specific cost correctly as $129,000 per 10^9 Btu's per year and incorrectly as $129 per 10^6 Btu's per day; the former figure is 13 percent *higher* than that shown for solar space cooling. The 1977 update (*20*) reduces the heating-system cost to $20.4 per square foot in 1977 dollars]. After normalizing to my accounting conventions (*2*), the capital cost alone for the ESPM system is 4.8 times the maximum total cost of my better-optimized solar system of the costliest kind.
7. A. B. Lovins, *Energy Policy* **6**, 171 (1978), p. 175; *Proceedings of the 1st New England Site-Built Solar Collector Conference* (Mechanical Engineering Department, Worchester Polytechnic Institute, Worchester, Mass., 1978).
8. A. B. Lovins, in U.S. House of Representatives, Committee on Government Operations, Subcommittee on Environment, Energy and Natural Resources, *Nuclear Power Costs* (Government Printing Office, Washington, D.C., 1978), part 2, p. 1106. See also H. Nash, Ed., *The Energy Controversy: Soft Path Questions and Answers* (Friends of the Earth, San Francisco, in press).
9. A. B. Lovins, *Soft Energy Notes*, in press.
10. The ESPM does, however, assess many approximate operating factor inputs by two-digit Standard Industrial Classification category for use in interfacing with input-output models.
11. A. B. Lovins, in *Future Strategies for Energy Development* (Oak Ridge Associated Universities, Oak Ridge, Tenn., 1977), pp. 109-113. This calculation is the precursor of chapter 6.1 of (*1*).
12. This misrepresents (*11*), in which I use the deliberately conservative figure $3179/kWe delivered. The figure $3496 [later adopted in (*2*)] is mentioned (*11*, p. 112n) as better fitting the historical cost data, and ~ $5000 is mentioned (*11*, p. 114) as a more realistic estimate, but neither is used in the analysis in (*11*), and the latter figure does not underlie any of my conclusions.
13. J. M. Gallagher, M. Carasso, R. Barany, R. G. J. Zimmerman, "Direct requirements of capital, manpower, materials, and equipment for selected energy futures" (Bechtel Corp., San Francisco, April 1976; available from the National Technical Information Service, Springfield, Va.). These capacity factors, from the Energy Research and Development Administration and Brookhaven National Laboratory, were 0.65 for nuclear and 0.50 for coal, compared with my 0.55 for nuclear and 0.62 for coal (*1*). The ESPM leaves capacity factors to be chosen by its user.
14. W. E. Mooz, "Cost analysis of light water reactor power plants" (Report R-2304-DOE, Rand Corp., Santa Monica, Calif., 1978).
15. C. Komanoff, "A comparison of nuclear and coal costs" (testimony to the New Jersey Board of Public Utilities, Docket 762-194, Phase III, 9 October 1978).
16. See (*11*), pp. 109n and 112n; (*1*), p. 106, note 4;

(2), p. 501, note e; (14); and notes 7 and 8 of my letter. Further, with the apparent exception of electric distribution costs (21), the newer studies Gallagher cites update only escalation and indirect costs; the 1975 base costs and schedules will not be updated until the spring of 1979.

17. I assume a 1.1-gigawatt dual unit with a cooling tower, built outside the Northeast, as the architect-engineer's 25th unit and the country's 124th commercial construction-permit issuance (134, including 16 under turnkey contracts, had been issued by 31 August 1976). The *empirical* cost [smoothed as in (15)] of plant 58, commissioned in December 1977, was $920/kWe in 1976 steam-plant dollars, confirming the conservatism of my $929 for 1976 *ordering* and zero real escalation.
18. See, for example, note 8 of my letter and, in terms of total cost per kilowatt-hour sent out, C. L. Rudasill, "Coal and nuclear generating costs." [Report No. PS-455-SR, Electric Power Research Institute (EPRI), Palo Alto, Calif., April 1977)].
19. EPRI's average coal cost (18), derived from a special Bechtel study, is $595 to $721 per kilowatt electric, comparing well with my $607. Komanoff has shown [(15), "Responses to PSE & G Requests 31 & 35," 27 December 1978] that the average U.S. historical ratio of nuclear-to-coal capital costs per kilowatt electric installed is 1.51 (1.72 without an industry-derived 16 percent addition for coal plants without scrubbers). My own nuclear-to-coal ratio, 1.53, is consistent with this historical 1.51 and exceeds the ESPM's unrealistically low 1.23 because of 2 years' differential escalation at 13 percent per year in the Bupp & Treitel conversion from 1974 to 1976 dollars (1, 2). If we assume zero differential escalation after 1976, the EPRI-Bechtel 1977 coal cost of $595 to $721/kWe and the historical nuclear-to-coal ratio of 1.51 together imply a nuclear cost of $898 to $1089/kWe, averaging 7 percent above my $929/kWe. Thus in order to achieve a nuclear cost of only $929/kWe, coal would have to escalate *faster* than nuclear from 1977 to 1985 in order to reduce their ratio below historical levels. This implausible requirement indirectly confirms the conservatism of my reactor cost figure.
20. J. M. Gallagher, R. Barany, P. F. Paskert, R. G. J. Zimmerman, "Resource requirements, impacts, and potential constraints associated with various energy futures" (annual report to the Department of Energy, Bechtel National, Inc., San Francisco, August 1978; available from the National Technical Information Service, Springfield, Va.). The nuclear cost given, using the 7 percent and 9 percent annual escalation and interest rates that the authors assume, is $1110/kWe installed in March 1977 dollars. The ratio of this cost to their average coal cost (weighting high- and low-Btu-coal plants according to the ESPM's table 7-7) is 1.51, precisely the historical average and consistent with my argument (19).
21. This assumes costs (including escalation and interest) as given in (20) for all facilities; Bechtel's 0.65 capacity factor (13); the ESPM's 16.4 percent T & D losses (1); my fuel-cycle parameters (1) and initial core costs (1) ($100/kWe installed, inflated 7 percent to 1977 dollars); and the T & D modal splits (1) supplied by Gallagher on 4 October 1976. Per kilowatt electric of installed generating capacity, (20) then yields 1977 dollar costs for the reactor, marginal fuel-cycle facilities, transmission, and distribution of, respectively, $1110 (12 percent up from my value), $79 (3 percent down), $97 (5 percent up), and $290 (48 percent down). The updated costs thus agree quite well with those I obtained by escalating the ESPM's costs from 1974 to 1976 dollars with appropriate indices (1, 2)—except for distribution, whose base cost the update has inexplicably halved (16, 20) from a value Bechtel described in May 1976 as "based on quite detailed information, with both quantities and prices listed, [so] we are confident based on a thorough review . . . that the estimate is reasonable, given the assumptions used." ["Review of electric distribution costs" (memorandum to Brookhaven National Laboratory, Bechtel Corp.)]. Because the other capital costs agree so well, combining Gallagher's latest costs (20) with *my* 0.55 capacity factor and 10.7 percent T & D losses changes the whole-system nuclear cost from $2905/kWe delivered to $3204, only 8 percent below my $3495 (all in 1976 dollars, deflating the Bechtel values 7 percent); this difference arises from the changed distribution base cost. Thus *neither* substituting Bechtel's latest costs *nor* their ancillary assumptions for mine significantly changes my results, as Gallagher suggests.

Carcinogenicity of Phenacetin

The article (1) that Pedro Cuatrecasas quotes in his letter to *Science* (5 Jan., p. 6) is a summary of the activities carried out from 1971 to 1977 under the Programme on the Evaluation of the Carcinogenic Risk of Chemicals to Humans of the IARC (International Agency for Research on Cancer). The program is focused on the preparation of monographs in which all available experimental and epidemiological data, as well as data on use, production, and occurrence of individual chemicals are critically analyzed and summarized. The monographs end with an evaluation of the carcinogenicity of the chemical in animals and humans. Faced with a very large number of chemicals in our environment, we used certain criteria in our selection of those to be considered in the monograph program. It seemed reasonable to give precedence to chemicals for which (i) there is evidence of human exposure and (ii) there is some evidence of carcinogenicity in experimental animals or some evidence or suspicion of human risk.

It is clearly stated in a note to the reader at the beginning of each of the IARC monographs that "inclusion of a chemical in the monographs does not imply that it is a carcinogen, only that the published data have been examined. Equally, the fact that a chemical has not yet been evaluated in a monograph does not mean that it is not carcinogenic."

If the reader consults volume 13 (2) of the IARC monographs, which has the subtitle "Some miscellaneous pharmaceutical substances," a few misunderstandings could perhaps be avoided with regard to the evaluation of phenacetin as being associated with the occurrence of cancer in humans. At the time phenacetin was evaluated, that is, 18 to 25 October 1976, the results of only one experimental carcinogenicity study on phenacetin (3) were available. No evidence of treatment-related tumors was found in this study, in which phenacetin was mixed in the diet of Berlin-Druckrey rats at a dose of 40 milligrams per animal per day. The results of another study indicated *N*-hydroxyphenacetin, a putative metabolite of phenacetin, is carcinogenic in rats, producing hepatocellular carcinomas (4). The evaluation of the carcinogenicity of phenacetin in experimental animals states: "In one limited study in which phenacetin was administered orally to rats, no carcinogenic effects were observed. One putative metabolite of phenacetin, *N*-hydroxyphenacetin, is carcinogenic in rats after