II

DISPUTES BETWEEN EXPERTS

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EXPERTS frequently disagree on scientific and technological questions which are relevant to political issues. Some of these questions have been labelled "trans-scientific" by Dr. Alvin Weinberg [1] because they are in principle beyond the capacity of science to answer. For example, it would require so many mice, so much time, so many scientists and so much equipment to obtain significant results on the biological effect of very low level radiation that the experiments would probably never be undertaken. Other questions can be answered but for one reason or another have not been. In either case, such disagreements between scientists who testify as experts is a major source of confusion to policy-makers and to the public. One example is the recent ABM dispute.[2]

Another example is the disagreement over the harmful effect of low level radiation. This dispute originated in early concern over the fallout from nuclear tests and has been renewed in the current controversy over nuclear power plants. Major recent critics are Dr. John Gofman and Dr. Arthur Tamplin, research associates at the Lawrence Radiation Laboratory. Dr. Gofman is also professor of medical physics at the University of California, Berkeley, and a past associate director of Lawrence. In late 1969, Drs. Gofman and Tamplin claimed that if the population of the United States were exposed to the maximum level of radiation permitted by federal standards, there would be an additional 16,000 to 32,000 cases of cancer and leukaemia each year. They recommended a ten-fold reduction in the federal standards. The scientific reception of their work has been well described in *Science*: "... the Atomic Energy Commission (AEC) challenged their assumptions, disputed their estimates, and disagreed with their recommendations." [3] Still, while many scientists who are experts in the subject disagreed with them, the two are generally considered to be reputable scientists whose arguments are clearly not wrong. Another *Science* article noted that: "Most scientists who have worked on setting [radiation] standards believe that many of the assumptions made by Gofman and Tamplin are unjustifiable but find it difficult to disprove specific points." [4]

[1] Weinberg, Alvin M., "Science and Trans-Science", *Minerva*, X, 2 (April, 1972), pp. 209–222.

[2] "Guidelines for the Practice of Operations Research", *Operations Research*, XIX, 5 (September, 1971), pp. 1123–1158. Reproduced in abridged form as "The Obligations of Scientists as Counsellors: Guidelines for the Practice of Operations Research", *Minerva*, X, 1 (January, 1972), pp. 107–157. Also see Doty, Paul, "Can Investigations Improve Scientific Advice? The Case of the ABM", *Minerva*, X, 2 (April, 1972), pp. 280–294.

[3] Boffey, Philip, "Gofman and Tamplin: Harassment Charges against AEC, Livermore", *Science*, CLXIX (28 August, 1970), p. 838.

[4] Holcomb, Robert, "Radiation Risk: A Scientific Problem?", *Science*, CLXVII (6 February, 1970), p. 854.

Nuclear Power and the Fluoridation of Water

In order to clarify my own confusion on this sort of technical disagreement, I was led to study the conduct of opposed experts, comparing the nuclear power controversy with the water fluoridation controversy of the 1950s. Both controversies focused in large part on similar technical questions: what are the harmful effects, if any, of long-term exposure to low level doses of fluorine or radiation. The two questions have similar patterns: fluorine and radiation are known to be lethal in large doses and although there is no clear evidence of their lethal effects in very low doses, neither—so some experts argue—is there compelling evidence to the contrary.

There is a popular stereotype of the anti-fluoridationist as a " kook ", bigot, and extreme " right-winger ". While some opponents of fluoridation could have been described in these terms, it is necessary to recognise that some respectable scientists and physicians have also opposed fluoridation, fearing possible toxic effects. Yet few " neutral " commentators have given serious consideration to their arguments. Two psychologists have called opposition to fluoridation an " anti-scientific attitude ".[5] Social scientists have by and large been inclined to assume that an informed voter could not rationally oppose fluoridation, and they studied its frequent defeat in referenda as examples of " democracy gone astray ".[6] In comparison, the critics of radiation levels have been given a respectful—if not hospitable—reception.

There are four plausible explanations for this apparent difference in the treatment of scientific opposition. First is the possibility that the radiation argument is objectively more sound than the fluoridation argument Second, the anti-radiation scientists might have higher professional stature than the scientists who oppose fluoridation, particularly if we add Dr. Linus Pauling to the list. Third, the anti-fluoridation movement was associated with the anti-communist campaign of the late Senator McCarthy and this has been anathema to the American academic and scientific communities. Fourth, it is only in the last two or three years that scientists, and the public, have become acutely aware of, and concerned with, " traces " of mercury, DDT, etc., in the environment. It is ironic to read the facetious discussion of Crain and his colleagues of claims against fluoridation " on alleged medical grounds ". This passage was published in 1969 in a sociological survey of attitudes towards fluoridation, just as the environmental movement was beginning to sweep the United States:

> [Most of the] . . . claims made against fluoridation on alleged medical grounds . . . have their basis in the fact that in concentrated dosage fluorine is a poison. When the proponents of fluoridation try to argue that one part per million is a highly diluted dose, the critics reply that the fluoride will collect in out-of-the-way corners of the water mains to build up to deadly dosages. The reputed side effects of fluoridation run from destruction of teeth to liver and kidney trouble, miscarriages, the birth of mongoloid children, and psychological dis-

[5] Mausner, Bernard and Judith, " A Study of the Anti-scientific Attitude ", *Scientific American*, CXCII, 2 (February, 1955), pp. 35–39.

[6] This research is reviewed in Crain, Robert, Katz, Elihu, and Rosenthal, Donald, *The Politics of Community Conflict* (Indianapolis: Bobbs-Merrill, 1969).

turbances, including suspectibility to communism and nymphomania. When the public-health officer points out that nearly a tenth of the drinking water in the United States has always had traces of fluoride in it without causing ill effect, the critics then charge that fluoridation damages car batteries, rots garden hoses, and kills grass.[7]

Some of these arguments do not sound quite as nonsensical in 1973 when many persons worry about a " highly diluted dose " of mercury or cyclamates. Some radionuclides, mercury, and DDT " build up " through the now well-known ecological process of chain-concentration in food —flourides concentrate in fish and tea. And the public health officer's argument that some water " has always had traces of fluoride in it without causing ill effect ", sounds very like the current pro-nuclear argument that we have always been exposed to background radiation without injurious effects.[8]

In most of what follows, I will examine the scientific or technological content of these disputes, but our comparative analysis suggests that the political, non-scientific context of the dispute—e.g., McCarthyism or " environmentalism "—might be equally important in determining the outcome.[9]

Rhetorical Devices in Technical Disagreements

Before examining the technical similarities in the disputes over radiation and fluoridation, I shall look briefly at the rhetorical similarities in the technical controversy as they have appeared in periodical articles, speeches, congressional hearings and in reports in the press. Perhaps the rhetorical devices, more than conflicting substantive arguments, are the main source of public confusion. Even a casual reading of the literature of technical opposition to fluoridation and nuclear power reveals their similarity. In what follows, I present several passages from Gofman and Tamplin opposing the nuclear power programme of the AEC on grounds of the hazard of radiation, each followed by a similar passage from an opponent of fluoridation.

Radiation: The freshwater-to-fish pathway can concentrate radioactivity easily 1000-fold or more. . . . Thus, even though the water effluent at the release point may make the water drinkable . . . the fish grown in such water, 1000 times as radioactive, cannot be eaten in any quantity without grossly exceeding " tolerance levels ".[10]

[7] *Ibid.*, p. 4.

[8] A counter-argument, claimed by Gofman and Tamplin, is that 3 per cent. of the cases of cancer and leukaemia are caused by naturally occurring background radiation. *GT-102-69* (1969), pp. 12–13. " GT " reference numbers refer to a mimeographed set of position papers by J. Gofman and A. Tamplin. These papers are available from the authors at Lawrence Radiation Laboratory, Livermore, California.

[9] A 1952 congressional committee, one of the only informed bodies to take the low-dose fluorine " danger " seriously, had been investigating the dangers of chemical food additives for a year before it took up the question of fluoridation. It is not surprising that the committee was extremely sensitive to the possible toxic effect of adding a chemical to the water. *Hearings Before the House Select Committe to Investigate the Use of Chemicals in Foods and Cosmetics*, 82 Congress, session 2, part 3 (Washington, D.C.: U.S. Government Printing Office, 1952).

[10] Gofman, John and Tamplin, Arthur, *Poisoned Power* (Emmaus, Pa.: Rodale, 1971), p. 307.

Fluoridation: People living in fluoridated cities who eat a good deal of seafood and drink tea and beer may easily ingest a combined fluoride intake far beyond even the tolerance limits assumed by the Public Health Service (PHS).[11]

Radiation: . . . the AEC clearly demonstrated that when the chips are down on questions of protecting human beings and their environment, the promotional, huckster role wins out handily over the public protector role.[12]

Fluoridation: . . . the reckless arrogance, obstinacy, and unscrupulousness of the United States Public Health Service in continuing to promote the program while ignoring and, where possible, suppressing evidence that it is neither safe nor genuinely efficacious.[13]

Radiation: Where unknowns exist (in the evaluation of a technology), *always* err on the side of protecting the public health.[14]

Fluoridation: . . . the public should have the benefit of the doubt and the procedure should be considered harmful until proved otherwise.[15]

Radiation: Where environmental poisons are concerned, it has always been up to the public to show harm, rather than up to the pollutor to prove safety.[16] The promoters of atomic energy . . . said, in effect, the public must prove it is being harmed by radioactivity. . . .[17]

Fluoridation: When a potentially dangerous substance such as fluoride is added to a public water supply, the burden should rest on those who add it to prove beyond reasonable doubt that it is safe for everyone. This has not been done. In fact, there is a strong reverse tendency to require incontrovertible proof of damage from opponents. . . .[18]

Radiation (referring to the strategy of the AEC): Tell a big lie, and tell it again and again and again as widely as possible.[19]

Fluoridation (referring to the strategy of the PHS): . . . a colossal lie, if repeated often enough, will be accepted as truer than truth.[20]

Radiation: Tamplin and Gofman presented evidence . . . that our allowable radiation exposures . . . are grossly unsafe. . . . The AEC response: Derision, denial, slander—but *no* evidence in refutation.[21]

Fluoridation: [Critiques] of the proponent scientific data have been presented to the Public Health Service. . . . Instead of dealing with the subject matter itself, they [the PHS] attempt to show that the author is not qualified to discuss matters relative to fluoridation.[22]

11 Exner, F., Waldbott, G. and Rorty, J., *The American Fluoridation Experiment* (New York: Devin-Adair Co., 1957), p. 20.
12 Tamplin, Arthur and Gofman, John, *"Population Control" through Nuclear Pollution* (Chicago: Nelson-Hall, 1970), p. 123.
13 Exner, *et al., op. cit.*, pp. 12–13.
14 Gofman and Tamplin, *op. cit.*, p. 257.
15 Taylor, Alfred in *Hearings, op. cit.*, p. 1535.
16 Gofman and Tamplin, *op. cit.*, p. 246.
17 *Ibid.*, p. 257.
18 Exner, *et al., op. cit.*, p. 45.
19 Tamplin and Gofman, *op. cit.*, p. 123.
20 Exner, *et al., op. cit.*, p. 145.
21 Tamplin and Gofman, *op. cit.*, p. 223.
22 Exner, *et al., op. cit.*, p. 188.

Lest this similarity be considered a characteristic way of dealing with denial of danger, I present some comparable passages from the statements of those who argue that the nuclear power and fluoridation programmes are safe and desirable.

Radiation: . . . radiation is by far the best understood environmental hazard.[23]

Fluoridation: . . . never before has a public-health measure been subjected to such thorough scientific scrutiny. . . .[24]

Radiation: It seems that a number of national concerns have converged to make up what we call the nuclear controversy. . . . [One of these is] an increasing distrust of science and technology in general.[25]

Fluoridation: The strength of the opposition to fluoridation can be attributed to three important factors. . . . [One of these is the public's] current suspicion of scientists.[26]

Radiation: It must indeed be confusing to the public to have two scientists present such opposing views, and the important question arises as to which to believe. In making up your mind, I believe it important that you consider the views of the majority of scientists on these issues.[27]

Fluoridation: The issue of fluoridation . . . came down to a question of . . . what authority . . . [the public] are to trust—the professional organizations [which supported it] or the few individual doctors, dentists [and] scientists . . . [who] opposed it.[28]

Radiation: . . . the risk of nuclear power is very much lower than the risk of alternate power sources. . . . Compared to the benefits of electricity . . . nuclear power is a very satisfactory system.[29]

Fluoridation: . . . the risk that such patients [with chronic kidney disease] might be harmed by the fluoridation of water appears to be small in comparison with the dental benefits to be obtained [for the community].[30]

Radiation: Part of the rationale behind permitting the release of small quantities of radioactivity to the environment is the knowledge that the environment has been radioactive from natural causes since the beginning of time. All natural solids, liquids and gases contain radioactivity in varying amounts. Further, radiation due to cosmic rays continuously bombards us.[31]

Fluoridation: We have analyzed foods very common to our diet which were purchased on the open market and have found that they contain fluorine in

[23] "Electric Power from the Atom" (Minneapolis: Northern States Power Company, undated), p. 12.

[24] Forsyth, B. in *Hearings, op. cit.*, p. 1484.

[25] Slater, H. in *INFO*, 32 (New York: Atomic Industrial Forum; December, 1970), p. 2.

[26] Mausner and Mausner, *op. cit.*, p. 39.

[27] Bond, Victor, *Radiation Standards, Particularly as Related to Nuclear Power Plants* (Raleigh, N.C.: Council for the Advancement of Science Writing, 1970), p. 8.

[28] Hutchinson, A., reported in McNeil, Donald, *The Fight for Fluoridation* (New York: Oxford University Press, 1957), p. 171.

[29] Starr, Chauncey, "The Electric Power Crisis in America", *Look* (10 August, 1971), p. 40.

[30] Heyroth, F. in *Hearings, op. cit.*, p. 1504.

[31] Seaborg, Glenn and Corliss, William, *Man and Atom* (New York: Dutton, 1971), p. 70.

amounts varying from 0.14 to 11.2 parts per million. Therefore, the addition of fluorine at approximately 1.0 part per million to the water is not introducing a new element into our dietary.[32]

Radiation: After more than ten years of experience with nuclear power . . . no utility-operated nuclear station in this country has ever had an accident that adversely affected public health.[33]

Fluoridation: In the more than 200 municipalities that have fluoridated their water supplies, no serious problems have occurred.[34]

Many of these statements could be transposed from one controversy to the other simply by changing " radiation " to " fluoridation ", and " AEC " to " PHS ", or vice versa.

The most common rhetorical device appears to be the phrase, " There is no evidence to show that . . . " or one of its variants. This blank denial of the claims of the opponent on the ground that there is no basis for his position appears in both controversies.

Fluoridation

No evidence has ever been produced that 1.0 part per million of fluoride in drinking water has or will harm any living person or thing.[35]

I would say that as far as any evidence has brought out to the present time, there is no danger to our health and welfare.[36]

Some surveys of the amount of certain types of kidney disease in fluoride as compared to nonfluoride areas have not produced any evidence of harmful effects upon the kidneys by fluorine at the levels proposed for the fluoridation procedure.[37]

The councils [of the American Medical Association] are unaware of any evidence that fluoridation of community water supplies up to a concentration of one part per million would lead to structural changes in the bones or to an increase in the incidence of fractures.[38]

In the accumulated experience there is no evidence that the prolonged ingestion of drinking water with a mean concentration of fluorides below the level causing mottled enamel would have adverse physiological effects.[39]

Nuclear Power

No evidence exists for such an effect (i.e., differential harm depending on the rate of radiation delivery) on cancer or leukemia induction by radiation in man.[40]

32 Blayney, J. in *Hearings, op. cit.*, p. 1548.
33 Electric Light and Power Companies' advertisement, *Time* (20 December, 1971).
34 Doty, J. in *Hearings, op. cit.*, p. 1678.
35 Forsyth, B. in *Hearings, op. cit.*, p. 1485.
36 Blarney, J. in *Hearings, op. cit.*, p. 1559.
37 Doty, J. in *Hearings, op. cit.*, p. 1678.
38 Lull, G. in *Hearings, op. cit.*, p. 1709.
39 Heyroth, F. in *Hearings, op. cit.*, p. 1504.
40 Gofman, John and Tamplin, Arthur, GT-102-69 (1969), p. 14.

At the present time *no valid evidence*, based upon scientific observation, has been brought forward to prove that natural sources of radiation have produced injury to man in any way.[41]

There are no experiments that show that the integrated low-level effect (of radiation) is higher than that of the same amount given at one time.[42]

The device occurs on both sides of a single controversy. Thus, the first two " Nuclear Power " statements above are by Gofman, the first made from his current role as a nuclear critic, and the second 12 years earlier when he was a proponent. Not surprisingly, there are counter-moves to the " no evidence " rhetoric.

Witness a typical statement by Mr. Frederick Draeger of the Pacific Gas and Electric Company: " There is no evidence that 170 millirads is harmful and any new plant will actually emit only an infinitesimal fraction of that amount ". Apparently, Mr. Draeger hasn't the slightest comprehension of what his statement " no evidence " really means. " No evidence " here means no one has even looked! [43]

We find the same mode of argument in the fluoridation debate. For example, when the American Medical Association stated that its responsible councils " are unaware of any evidence " that fluoridation would be harmful, an opponent responded:

[If] the Councils had actually *considered the evidence* instead of trustingly accepting what McClure said about the evidence, they would not have been unaware of dangers in fluoridation.[44]

Arguing about different problems

Some observers, in the course of trying to place their finger on the points of disagreement between two experts, have concluded that the two do not disagree at all, but rather are each arguing about different points. This failure to confront each other's arguments is clearly present in the dispute surrounding the Gofman-Tamplin analysis of the expected number of deaths from nuclear power programmes of the AEC.

Drs. Gofman and Tamplin calculated that the United States would have " 32,000 cancer plus leukemia deaths annually from population exposure to FRC Guideline radiation." [45] One of their opponents, Dr. V. P. Bond, of the Brookhaven National Laboratory, clearly stated the opposing view (Table I). Note that Dr. Bond shows " cancer cases per year " to be the product of three factors: " risk " (measured in cases of cancer per million population per mrem of exposure), " dose per year " (measured in mrem

[41] Gofman, John, quoted in Bond, *op. cit.*, p. 3, original italics.

[42] Starr, *op. cit.*, p. 38.

[43] Gofman, John and Tamplin, Arthur, *Poisoned Power* (Emmaus, Pa.: Rodale, 1971), pp. 104–105.

[44] Exner, *et al.*, *op. cit.*, pp. 79–80.

[45] Gofman, John and Tamplin, Arthur, GT-117-70 (1970), p. 1, italics added. The Federal Radiation Council (FRC) recommended exposure guidelines adopted by the AEC, including the provision that the average dose to the entire U.S. population shall not exceed 170 mrad. per year.

TABLE I

Comparison of Gofman and Bond Calculations

Risk	×	Dose per Year	×	Number of Persons	= Cancer Cases per Year
(cases per 1,000,000 population/ mrem)	×	(mrem per year)	×	(200m.)	= ?
Gofman's calculation 0·94	×	170	×	200	= 32,000 cases per year
Bond's calculation 0·1	×	0·001	×	200	= 0·02 cases per year

SOURCE: Bond, Victor, *Radiation Standards, Particularly as Related to Nuclear Power Plants* (Raleigh N.C.: Council for the Advancement of Science Writing, 1970), figure 1.

per year), and " number of persons " (taken as 200 millions). Drs. Gofman and Bond differ in the values they assign to " risk " and " dose per year ", and therefore they arrived at markedly differed values of " cancer cases per year." The values for " risk " differ by an order of only ten (0·94 versus 0·1), and we will pass over this difference for the present. The values for " dose per year " differ by an order of 10^5 (170 versus 0·001). Dr. Gofman's value is based on the *permissible* (but not actually achieved) level of exposure. Dr. Bond's value is his estimate of *actual* average exposure to the population, which is much smaller than the permissible exposure. The two calculations are about two different things. Bond concludes:

> Dr. Gofman's speculations that 32,000 additional cancer deaths per year will result from radiation exposure of the public under current " standards " simply do not conform to reality. They are in fact *in error* by a considerable margin for the present and for the foreseeable future. His figures have essentially zero validity in the context of power reactors. In this context, an upper limit estimate of the *correct* figure is well below one death per year in the entire USA.[46]

Of course, the question of what is " in error " and what is " correct " depends on what is being calculated.

In both controversies, there are similar patterns of conflicting contentions based on differing premises; one of these conflicts involved the difference

[46] Bond, *op. cit.*, p. 12, italics added.

between *acute* and *chronic* forms of radiation or fluoridation poisoning. Proponents of fluoridation and the nuclear power programme have occasionally argued for the safety of their proposed technology by indicating how difficult it would be for a person to receive the relatively high dose associated with *acute* poisoning:

[Even] at one part of fluoride per million parts of water, to get a lethal dose from it you would have to drink 400 gallons at one sitting.[47]

The critics on the other hand are concerned about *chronic* poisoning which is associated with much lower dosages:

All the talk about the hundreds of gallons [of water] you would have to drink at one time to get sick refers to acute [fluoride] poisoning, which isn't even under consideration. . . . What is important is that the presence of tiny amounts of fluoride in the tissue fluids for long periods interferes with the proper growth, development and function of many parts of the body.[48]

These conflicts based on divergent premises appear to result from poor communication between adversaries, and/or from a strong motivation to win the argument. As such, they could probably be eliminated from a debate open to public scrutiny by qualified persons so that the technical issues would stand out more clearly. There is another source of confusion, however, which appears at the heart of technical disagreements and which could probably *not* be eliminated from any debate because it is intrinsic to disagreements. Even with perfect communication, and eschewing rhetorical devices which are intended simply to put the opposing argument in an unfavourable light, experts may disagree on ambiguous observations and assessments which cannot be resolved by available objective means.

Ambiguities

The theories, models, procedures and formulae of science and technology are generally believed to allow one trained in their use simply to calculate an unambiguously correct answer. A technologist or scientist soon comes to recognise that the complex technical problems of the state-of-the-art require subtle perceptions of the sort which cannot be easily articulated in explicit form. When it is necessary to make a simplifying assumption, and many are reasonable, which simplifying assumption should be made? When data are lacking on a question, how far may one reasonably extrapolate from data of other sources? How trustworthy is a set of empirical observations? These questions all require judgements for which there are no formalised guides and it is here that experts frequently disagree. I will call these points of disagreement "ambiguities" and I will demonstrate how they enter into technical controversy.

Most experts agree that radiation increases the incidence of leukaemia and thyroid cancer in an exposed population. However, there is disagreement on whether other forms of cancer are similarly induced by radiation:

Wanebo *et al.* . . . have recently reported that "accumulated information . . . strongly suggests that exposure to ionizing radiation has increased the risk of

[47] Forsyth, B. in *Hearings, op. cit.*, p. 1504.
[48] Exner, *et al., op. cit.*, p. 37.

lung cancer among atomic bomb survivors ". These investigators observed 17 such cases, as compared with 9 expected. . . . [They also] have reported that " information on breast cancer . . . has now accumulated to the point where a fairly definite carcinogenic effect seems established ". Six cases were observed . . . as compared with 1.53 cases expected—an excess of only 4.5 cases. . . .

It may be difficult or impossible to avoid certain biases that could produce such a small excess. . . . Wanebo *et al.* considered the possibility of biases and believed that none were present. . . .

One may conclude that . . . [the] evidence pertaining to cancer of the breast or lung *is still very much in doubt.*[49]

Consider, now, the plight of someone who is trying to calculate the number of cancers to be expected in a population exposed to a given level of radiation. Does he calculate an increase in just leukæmia and thyroid cancers, or does he calculate an increase in all forms of cancer? Since leukæmia and thyroid cancers constitute about 10 per cent. of all cancers in the United States, these two calculations will differ by about a factor of 10. Gofman's and Bond's calculations (Table I) showed their " risk " values differing by about a factor of 10 (0·94 versus 0·1). Gofman's calculation dealt with " all cancer " and Bond calculated leukæmia and thyroid cancer. We are in no position to say which one is " correct " given the indeterminacy of the present state of knowledge. The adversaries take a less equivocal position:

Dr. Gofman's excessive estimates are based on the untenable assumptions that all forms of cancer are increased by exposure at low doses and rates. . . . These assumptions do not square with the facts.[50]

And on the other side:

[Almost] all the major forms of human cancer were by (1969) . . . already known to be produced by ionizing radiation. . . . So it became possible to state a primary principle, or " law " of radiation production of cancer in humans.

That principle or law states, " All forms of human cancer are, in all probability, induced by ionizing radiation ".[51]

Each has chosen to accept as a firm conclusion what others regard as only tentative hypotheses. But their conclusions cannot be considered " wrong " in the sense in which an arithmetic solution can be wrong. Scientific " truths " are never proved but only gain increasing acceptance (and even then are often found to be incorrect). The point at which a decision is made that an hypothesis becomes a conclusion differs from one scientist to another.

Given the inconclusive nature of available data, it is possible to postulate several different relationships between the radiation dose delivered to a population and the resultant increase in leukæmia. Presumably, with more complete data, some of these relationships would be demonstrably inappropriate, but the data are incomplete. There are two commonly assumed

[49] Miller, Robert, " Delayed Radiation Effects in Atomic-Bomb Survivors ", *Science*, CLXVI (31 October, 1969), p. 572, italics added.
[50] Bond, *op. cit.*, p. 2.
[51] Tamplin and Gofman, *op. cit.*, p. 13.

" dose-effect " curves relating cumulative dose of radiation (*i.e.*, from birth) to the population, to the incidence of leukæmia in that population (measured in, say, the number of cases per year per million persons). These are the " linear " and " threshold " models (Figure 1).

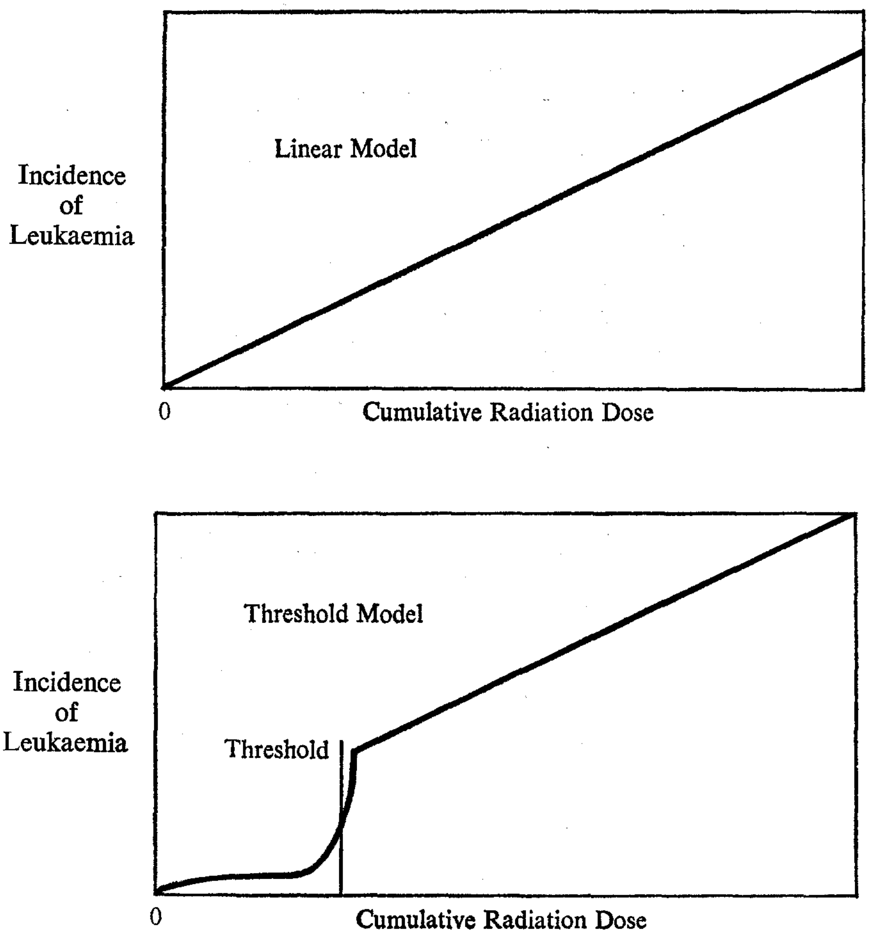

FIGURE 1

The first assumes a simple " linear " relationship between dose and incidence of leukæmia; it is the model favoured by Gofman and Tamplin. The second assumes that there is a " threshold " dose level below which there is practically no incidence of leukæmia. The scanty data available are not inconsistent with either model:

> There is evidence for linear dose-effect relationships of various slopes depending upon the specific effects; there is also evidence for at least practical thresholds

of effects, but generally speaking there has been no statistically significant information obtained on dose-effect relationships for doses of less than a few rads, or tens of rads, delivered more or less all at once.[52]

At present there is little basis for saying that one model is " true " and the other is not. It is easily conceivable that new data could prove one model wrong, but it is difficult to see how one could be " proved " correct. One could show that a given model is consistent with all available data, but it is always possible to design alternative models which fit a given data set. Thus there is always an element of judgement in selecting one model over another empirically-consistent alternative.

This theoretical ambiguity has major implications for the technical debate over permissible radiation standards. It should be noted that the " threshold " model implies that dose levels below the threshold will not harm the population (through leukæmia). The " linear " model implies that there will be some incidence of leukæmia no matter how low the dose to the populations. The two models differ, then, on whether or not there is a " safe " level of radiation exposure for the population. The ambiguous nature of the dose-effect curve is well recognised by radiation biologists, and many (including opponents of Gofman and Tamplin) assume the " linear " model, not necessarily because they consider it true, but because it is the most conservative model for purposes of public safety.[53]

It would be reasonable, given these ambiguities, for opposing experts to " agree to disagree " and to suspend the debate, at least until new data permitted the issue to be reopened. That does not usually occur, however. Instead, each opponent tries to build his case, not necessarily for his adversary, but frequently for a third party: the public, a congressional committee, scientific peers, etc. Since the initial disagreement was made possible by ambiguity, it is not surprising that many adversary arguments are based on ambiguity and have divided these into two categories: those which reject discrepant data, and those which present alternative interpretations.

Rejection of Discrepant Data

A common way to deal with data which are inconsistent with one's own position is to deny their scientific validity. We have already seen Miller reject the Wanebo *et al.* conclusion (that breast and lung cancers are radiation-induced) by noting: " It may be difficult or impossible to avoid certain biases that could produce such a small excess. . . ".[54] I can elaborate by considering one of many similar examples from the fluoridation controversy.

Dr. Alfred Taylor showed experimentally that when a strain of mice normally susceptible to mammary cancer were regularly fed fluoridated water, the tumours appeared earlier than in control mice fed non-fluoridated

52 Taylor, L., " What We *Do* Know About Low-Level Radiation ", *INFO* (New York: Atomic Industrial Forum, undated), p. 13.

53 *Ibid.*, pp. 15–18.

54 Miller, *op. cit.*

water.[55] Taylor reported his findings to the PHS whereupon H. Andervont visited Taylor's laboratory. Andervont later testified that the experiments were not valid.

We came to the conclusion that inasmuch as the food he [Taylor] was feeding to his mice contained 30 to 40 parts per million of fluoride, that the 1/2 part per million [fluorine] in the drinking water could not conceivably have had much influence on his results.[56]

One could discredit Andervont's denial of validity to Taylor's analysis by pointing out that fluoride consumed in water is almost completely absorbed into the blood, whereas fluoride consumed as a solid must first be digested and smaller amounts will be taken up by the blood; therefore the high fluoride content of the food does not necessarily overwhelm the fluoride in the water. Exner made essentially that objection and also emphasised the fact that both experimental and control groups were given the high-fluoride food, but only the experimental group received fluoridated water.[57]

Armstrong, Bittner and Treloar[58] conducted an experiment to check Taylor's result. The mean age at which tumours appeared in their experimental mice (which had been given fluoridated water) was lower than that of their control mice. However, the difference between conditions was not "statistically significant" at the ·05 level, and they considered the result attributable to chance. Taylor tried to deny the validity of these findings by arguing that Armstrong *et al.* did not use a large enough number of mice. "A control group consisting of 31 animals would be insufficient to reveal differences of the order of those encountered in the work here."[59] But the control groups in Taylor's own experiments always contained an even smaller number of mice! The subjective nature of these attacks and rebuttals is clear.

Alternative Interpretation

Even if both disputants in a technical argument accept the validity of a datum, the interpretation of that datum remains an ambiguous procedure. During the initial excavations for the contested nuclear plant at Bodega Bay, California, an earthquake fault was discovered running through the shaft. It was further determined that there had been no movement along the fault for about 40,000 years. Apparently no one contested this fact, but two opposite interpretations could be made: (1) The fault is inactive, and there is little likelihood of future movement, or (2) There is significant potential for a future earthquake along a known fault.[60].

[55] *Hearings, op. cit.*, pp. 1530–1535. All of the mice were normally tumour-prone. The mice receiving fluoridated water developed tumours sooner than the control mice, but their total incidence of tumours was no higher than in the control group. Confusion on this point evidently led to the belief that fluoridated water causes cancer. Taylor explicitly denied that his data showed any increase in cancer incidence (p. 1540).

[56] *Hearings, op. cit.*, pp. 1666–1667.

[57] Exner, *et al., op. cit.*, pp. 32–33, 71–72.

[58] *Hearings on H.R. 2341 before the Committee on Interstate and Foreign Commerce; Fluoridation of Water*, 83 Congress, session 2 (Washington, D.C.: U.S. Government Printing Office, 1954), pp. 307–309.

[59] Taylor, Alfred, quoted in Exner, *et al., op. cit.*, p. 185.

[60] Novick, Sheldon, *The Careless Atom* (Boston: Dell, 1969), pp. 42–44.

Statistical data are particularly amenable to alternative interpretations, especially if they contain substantial error variance, as is usually the case in epidemiological studies. H. Trendley Dean in 1938 analysed the incidence of caries in children in two sets of cities, one high in the presence of fluorine, the other low. He concluded that children using waters with a higher fluoride content were more caries-free than those using lower fluoride waters (Table II).[61] Exner, an opponent, analysed the same data in unaggregated form (Table II) noting:

It would appear to take some ingenuity and a certain amount of determination to deduce from these data the conclusion Dean drew.[62]

TABLE II

Fluoride versus Incidence of Caries (Permanent Teeth)

DEAN'S DISPLAY

Cities	Number Children Examined	Range of Fluoride in PPM	Percentage Caries-free
Pueblo, Junction City, East Moline	114	0·6–1·5	26
Monmouth, Galesburg, Colorado Springs	122	1·7–2·5	49

EXNER'S DISPLAY

Cities	Number Children Examined	Range of Fluoride in PPM	Percentage Caries-free
Pueblo	49	0·6	37
Junction City	30	0·7	26
East Moline	35	1·5	11
Monmouth	29	1·7	55
Galesburg	39	1·8	56
Colorado Springs	54	2·5	41

SOURCES: Dean, H., "Endemic Fluorosis and its Relation to Dental Caries", *Public Health Reports*, LIII (19 August, 1938), pp. 1443–1452. Exner, F., Waldbott, G. and Rorty, J., *The American Fluoridation Experiment* (New York: Devin-Adair Co., 1957), p. 114.

61 Dean, H. *op. cit.*, pp. 1443–14452.
62 Exner, *et al.*, *op. cit.*, p. 114.

McClure, a proponent of fluoridation, recently used the same data but again in dichotomised form.[63]

Adversaries in the nuclear controversy treat data regarding radiation-induced cancer in a similar way to support their own positions. Evans [64] collected data on radium workers showing that no cancers occurred below a median dose level of ·55 microcuries (Table III). Evans considered this support for the "threshold" dose-effect curve. Gofman and Tamplin,[65] however, believe that the same data fit their "linear" dose-effect curve. Here is their reasoning.

First they estimate the probability of finding cancer in a subject who has been exposed to a given dose of radiation. Focusing on the 5·5 micro-

TABLE III

Exposure to Radiation versus Incidence of Cancer

Number of Cases	Median Dose (in microcuries Ra equivalent residual)	Number of Cancers
42	<0·001	0
61	0·0055	0
80	0·055	0
32	0·55	3
40	5·5	14
14	55	2

SOURCE: Evans, Robley, *op. cit.*, pp. 881–895. Reproduced in Gofman, John and Arthur Tamplin, GT-103-69 (1969), p. 3.

curies-median dose group (as the largest and hence most statistically reliable), they note 14 cancers out of a total of 40 cases, so there is a 14/40 probability of cancer per person. This is for a median dose of 5·5 microcuries. The probability of cancer per person per microcurie is then $14/(40 \times 5{\cdot}5) = 0{\cdot}064$. Now, there are 80 cases with a median dose of ·055 microcuries so, assuming the linear hypothesis, the expected number of cancers in that group is $0{\cdot}064 \times {\cdot}055 \times 80 = 0{\cdot}28$ cases. But human cancers cannot occur in fractions, so the most likely outcome is zero cancers in this group, and that is what is found. A similar analysis for the lower median doses shows that in each group the expected number of cancers is near zero. Gofman and Tamplin thus argue that the data are fully consistent with the linear dose-effect curve, and that the apparent threshold is due to very small groups of persons being exposed.

Alternative modes of interpretation are often used to explain away an opposing argument. Pro-fluoridationists found no fluoride poisoning in

[63] McClure, Frank, *Water Fluoridation: The Search and the Victory* (Bethesda, Md.: U.S. Dept. of Health, Education and Welfare, 1970), p. 81.

[64] Evans, Robley, "The Effect of Skeletally Deposited Alpha Emitters in Man", *British Journal of Radiology*, XXXIX (1966), pp. 881–895.

[65] Gofman, John and Tamplin, Arthur, GT-103-69 (1969), p. 3.

cities with naturally fluoridated water (at about one ppm) and concluded that low concentrations of fluoride must be safe. Critics argued that low concentrations of fluoride *would* cause poisoning, but that calcium is an antidote. Since calcium usually occurs naturally in the same waters where fluoride occurs naturally, this explains the lack of poisoning.[66]

Proponents of the nuclear power programme minimise the harmful effects of long-term low level radiation because animal experiments have indicated that a given dose of radiation over a protracted period is less harmful than the same dose delivered in a short period of time.[67] In a typical experiment of this sort, one group of 10-week-old mice is placed on a daily schedule of small doses. A second group of 10-week-old mice is given an acute dose equal to the integrated protracted dose of the first group. The final incidence of cancer is usually higher in the second group than the first group. Gofman and Tamplin dismiss the mitigating effects of protracted dose by pointing out that for a given dose, and dose-rate, of radiation, more harm will be done to a younger organism than to an older organism. The mice receiving acute doses are fully irradiated at 10 weeks of age, whereas the protracted group receives most of its radiation at an age beyond 10 weeks. Therefore the lower incidence of cancer and leukæmia in the protracted groups is simply a consequence of their being older when they were irradiated:

> If most experimenters had delivered their acute radiation dose at the *end* of the protraction period rather than at the beginning, the literature would by now be filled with a different illusion—namely, that protracted radiation is more carcinogenic than acute radiation.[68]

Such an experiment is perfectly feasible and could test whether Gofman and Tamplin are right or wrong. In the absence of that data, the point remains ambiguous, and experts are free to differ.

Polarisation

If an expert may reasonably take any one of several positions on a technically ambiguous point, then we should ask why some experts take one position while other experts take another—often opposing—position. One's interpretation of ambiguous data is often tied to one's position on the innovation about which the controversy exists. Thus, since a "threshold" radiation dose-effect curve is more congenial to the realisation of the nuclear power programme than a "linear" curve, it is not surprising that proponents of that programme are more likely than critics to believe that the "threshold curve" is the valid one. Experts may espouse a particular position where the data are ambiguous because they are used to it and have never questioned it. An expert may take one side because his friend has taken that side, or because his enemy has taken the opposite side. In any case, these differences of opinion sometimes become very

[66] Exner, *et al.*, *op. cit.*, pp. 38, 101–102.

[67] *E.g.*, *Hearings Before the Joint Committee on Atomic Energy; Environmental Effects of Producing Electric Power*, 91 Congress, session 2, part 1 (Washington, D.C.: U.S. Government Printing Office, 1969), p. 654.

[68] Gofman, John and Tamplin, Arthur, GT-109-70 (1970), p. 15.

bitter, as has in fact occurred in the controversies about fluoridation and nuclear power.

Experts tend to behave as other persons behave when they engage in a controversy. Coalitions tend in general to solidify and disagreements become polarised when conflict becomes more acrimonious.[69] The same processes occur in technical controversies. For example, one proponent of the nuclear power programme stated:

> It's hard to maintain a detached position. I find myself forced to sweeping generalizations and extreme statements. I now find myself resenting any criticism of nuclear power, without considering the merits of the criticism.[70]

Drs. Gofman and Tamplin's movement into a polar position is demonstrated by the changing position which is set forth in their books and papers over three years. Their estimates of the damage to be expected from

TABLE IV

Chronology of Gofman and Tamplin Polarisation

Date	Estimate of Annual Harm	Recommended Change in Policy
29 Oct., 1969	16,000 additional cancer plus leukæmia cases	Reduce FRC guideline exposure by a factor of 10
18 Nov., 1969	16,000	Factor of 10
28 Jan., 1970	Above 16,000; nearer to 32,000 or even higher	Factor of 10
9 Feb., 1970	16,000 cancer plus leukæmia cases	Not mentioned
20 Feb., 1970	32,000 cancer plus leukæmia cases	Not mentioned
30 March, 1970	Not mentioned	Specific reductions for radiation workers
7 April, 1970	32,000	Guideline exposure should be zero, and the privilege of releasing radiation must be negotiated
22 April, 1970	32,000 cancer and leukæmia cases plus a large number of genetic deaths, plus a large number of deaths from other causes	Zero release

[69] Coleman, James, *Community Conflict* (New York: Free Press, 1957); Coser, Lewis, *The Functions of Social Conflict* (New York: Free Press of Glencoe, 1956); Mazur, Allan, " A Nonrational Theory of Conflict and Coalition Formation ", *Journal of Conflict Resolution*, XII (June, 1968), pp. 196–205.

[70] Sailor, V. quoted in *Nuclear Industry*, XXII, 6 (June, 1971), p. 25.

TABLE IV—*continued*

Date	Estimate of Annual Harm	Recommended Change in Policy
29 June, 1970	32,000 cancer plus leukæmia cases, plus increases in genetically based diseases	Not mentioned
20 August, 1970	32,000 cancer and leukæmia cases, 150,000 to 1,500,000 genetic deaths, plus a 5 per cent. to 50 per cent. increase in such diseases as schizophrenia and rheumatoid arthritis	Five-year moratorium on above-ground nuclear power plants, and also an injunction against fast breeder reactors for an indefinite period
ca. Sept., 1970	Same as above	Zero release. Stop construction of experimental fission reactors and increase spending on fusion research
ca. Feb., 1971	Same as above	Zero release. Moratorium on construction of new nuclear power plants
3 March, 1971	Number of cancer plus leukæmia cases may be closer to 100,000	Not mentioned

SOURCES: The data used for this table are 21 position papers by John Gofman and Arthur Tamplin with "GT" reference numbers (see footnote 8). Also Gofman, John, *et al.*, "Radiation as an Environmental Hazard", mimeo (Lawrence Radiation Laboratory: Livermore, California, 1971). Also Gofman, John and Tamplin, Arthur, *Poisoned Power* (Emmaus, Pa.: Rodale, 1971). Also Tamplin, Arthur and Gofman, John, "*Population Control*" *Through Nuclear Pollution* (Chicago: Nelson-Hall, 1970).

population exposure to the Federal Radiation Council guideline of 170 millirads have become increasingly higher (Table IV), and their recommendations for control have become increasingly stringent. Although some of their opponents might regard this inconstancy as an indication of the weakness of their position, it is also a normal process of polarisation which must be expected in any intense controversy.

Conclusion

Technical disputes over fluoridation and radiation are confusing, in part because of rhetorical devices which obscure the problem, and in part because of arguments which talk past each other because they are dealing with different problems and derive from different premises. A calm analysis of opposing views could clear this sort of verbal thicket, but there would still remain points of ambiguity upon which experts may legitimately disagree, and where it cannot be said that one is " right " and the other is " wrong ".

We generally assume that informed scientific advice is valuable to political policy-makers. However, in the context of a controversial political issue, and when the relevant technical analysis is ambiguous, then the value of scientific advice becomes questionable. A technical controversy sometimes creates confusion rather than clarity and it is possible that the dispute itself may become so divisive and widespread that scientific advice becomes more of a cost than a benefit to the policy-maker and to society. The value of technical expertise depends, in large part, on whether or not these disputes can be settled by reasonable procedures.

Several procedures have been suggested or used in the conduct of disputes among experts about scientific and technological propositions crucially required in the making of decisions by laymen on problems of policy. One approach has been to suppress, discredit or ignore the criticism. This is imprudent because such treatment is likely to embitter and aggravate the opposition. Thus, the fact that anti-fluoridationists were widely attacked and discredited may help explain why fluoridation lost over 60 per cent. of the 1,139 referenda held in local communities between 1950 and 1969 in the United States.[71] The more acrimonious the campaign, the more likely that fluoridation would be defeated in the referendum.[72]

Another procedure would try to have the disagreeing experts resolve their own differences, or at least make their differing premises explicit. In this approach, the experts would work together in a cooperative manner, perhaps aided by a formal code of professional ethics. This may be practicable in some situations, but it is less likely to be tried or to be effective if used when a controversial political matter is at issue or when the experts are themselves seeking to bring about one decision rather than another.[73] Then, the tendency toward polarisation of the discussion into extreme positions, as in the nuclear power controversy, probably renders this cooperative approach unworkable.

A third procedure would have technical criticism evaluated in the same manner as most other scientific work, *i.e.*, through review by scientific peers. Scientific work is typically published in technical journals after having been judged acceptable by one, or several (usually anonymous) referees. Presumably the work of, say, Gofman and Tamplin could be submitted to a few referees who would then decide to accept or reject it. The difficulty here is that, if the technical criticism is derived from ambiguous premises, the referees might reject the work simply because they do not recognise the ambiguity, or if they do recognise it, they might disagree with the critic's interpretation of it. This sort of rejection is particularly likely if the critic represents a minority viewpoint. For example, Drs. Gofman and Tamplin assume that the incidence of *all* cancers increases with radiation, but since that is not the prevailing view in

[71] *Fluoridation Census 1969* (Bethesda, Md.: U.S. Dept. of Health, Education and Welfare, 1970).

[72] Crain, *et al., op. cit.*

[73] Attempts at this sort of resolution of the ABM dispute show no signs of success. (See references in footnote 2.)

radiation biology, many referees would probably not accept that assumption. Their critique might then be rejected, not because it was "wrong", but because the referees had outvoted the critics on how to interpret the ambiguity.

Another procedure is to allow the disagreeing experts to confront each other as adversaries before a panel of judges. These judges, who are not personally involved with either side, are presumably able to make a fairly objective, dispassionate decision on the merits of the argument. It is important to emphasise that the judges would not make a decision of policy such as whether or not to accept x cases of cancer for y amount of electricity. Such scientific and technological judges have no special wisdom or moral prerogative to decide how many cancers their society should accept. They might, however, be particularly qualified to make a purely technical scientific decision, such as whether or not x cases of cancer might occur in a population receiving y amount of radiation. The judges must be scientifically and technically competent, though they need not be specialists in the field of the adversaries. In fact, judges from outside that field might be preferable since they are less likely to have preestablished interpretations of relevant ambiguities. As the adversaries confronted each other, attacking and rebutting, points of disagreement would become clear. The natural process of polarisation might even be helpful here since the opposing positions, pushed to their extremes, would be clearly contrasted. The judges might then be able to decide if either or both adversaries were wrong, or if the differences between them were legitimate, resting on points of irreducible ambiguity. In the latter case, they might then decide on the feasibility of reducing the ambiguity through further research. The judges' report would constitute part of the counsel given to policy-makers. If the report said that the scientific picture was inconclusive, then the policy-makers would have to proceed on that basis.

My own preferences lie with the adversary strategy, though that clearly has flaws too. A particularly persuasive adversary might sway the judges more by his oratory than by his evidence, just as a successful trial lawyer can win a jury to his side more by appeals to sympathy than to logic. For example, Dr. John Gofman is a particularly persuasive speaker, and on that fact alone he might be more successful before a panel of judges than a less eloquent speaker.[74] Additional procedures for improving the scientific quality of technical advice will certainly be proposed, and I am convinced that each will have its own advantages and disadvantages.

74 Rather than pursue an endless argument, it might be better to investigate some of these procedures on an experimental basis. Almost any large university has the resources to conduct such an experiment, including technical experts on opposite sides of a controversial issue. For example, we might test to see whether the adversary strategy does indeed suffer from an "oratory effect". Several separate adversary hearings could be arranged experimentally to deal with the same technical question. Each hearing would have a separate set of adversaries and judges. Thus the quality of oratory would be made to vary from one hearing to another. If the judges from these several hearings gave similar final decisions, then we could have some confidence in an objective outcome for the procedure. If, however, judges from the several hearings gave divergent final decisions, then the adversary strategy would clearly be faulty. Details of a similar procedure appear in Gofman and Tamplin, *Poisoned Power* (Emmaus, Pa.: Rodall, 1971), pp. 347–353.