Letters

Project Chariot

In a recent issue of *Science* [**133**, 2000 (1961)] there appears an article by Howard Margolis about the separate reports on Project Chariot (the AEC proposal for underground explosions at Cape Thompson, Alaska) issued by the AEC and by the St. Louis Committee for Nuclear Information (CNI). Margolis' comments on the CNI report comprise the following items: (i) a summary of the contents of the report; (ii) an account of supposed "technical errors" (only one is mentioned) in the report and Margolis' criticism of certain of its conclusions; (iii) Margolis' own statement regarding the relative risks to Alaska Eskimos from radiation due to television watching, current levels of fallout, and the fallout that might result from Project Chariot.

In our opinion Margolis' discussion of the contents of the CNI report is incomplete, inaccurate and misleading. Item 3, which represents Margolis' own effort at analyzing radiation risks, is simply incorrect. To support these views we submit the following:

Margolis' summary of the contents of the CNI report is incomplete in a number of important respects. Among the observations made in this report, and absent in Margolis' account of it (and also absent from the AEC report on Project Chariot) are the following:

1) The basic data (the results of four underground nuclear explosions in Nevada) on which the AEC estimates of the total amount of radioactive debris which the Chariot explosion is expected to eject into the air, are inadequate for this predictive purpose. This is noted in an AEC technical report (*UCRL 5676*, p. 21, quoted in the CNI bulletin) which states: "From these four events it is obviously a great stretch of one's imagination to predict a great deal about the variation of crater width as a function of nuclear yield and depth of burial." The AEC prediction that 5 percent of the proposed Chariot explosion's total radioactivity will be vented into the air is based on an interpolation between two points in the four-point curve which describes the results of the foregoing test explosions. The two points which anchor this interpolation have the values 90 percent and 1 to 2 percent venting of total yield; the theoretical curve drawn between them (for which no mathematical basis is given anywhere in the available AEC literature) is a steeply concave curve. That reliance on such a curve does not meet the ordinary standards of scientific evidence will perhaps be most clear to the readers of *Science* from examination of the accompanying Fig. 1. The CNI report concludes that, given these data, the radioactivity ejected into the atmosphere by the Chariot explosion might with apparently equal probability be anywhere from 1 to 25 percent of the total radioactivity. The more volatile reaction products will be more efficiently vented, and a general factor of 4 to 5 is used by AEC investigators to estimate strontium-90 enrichment for this reason (*UCRL 6249T*, p. 11). However, this factor must be rather imprecise, for it is affected by the parameters that govern the physical events in an underground explosion—which are, as we have seen, still poorly understood. A 5 percent general venting (the AEC prediction) therefore implies 20 to 25 percent Sr^{90} venting and 25 percent general venting (our suggestion of what is possible) implies that a completely vented yield of Sr^{90} apparently cannot be excluded.

2) AEC calculations of the shape of the fallout zone, which depend strongly on wind velocity, are based on winds measured at Kotzebue, 120 miles from the site of Project Chariot. However, as reported in the AEC report on Project Chariot (p. 45), the average peak wind speed at the actual Chariot site (Ogotoruk Creek) was 65 mi/hr, at a time when the corresponding value

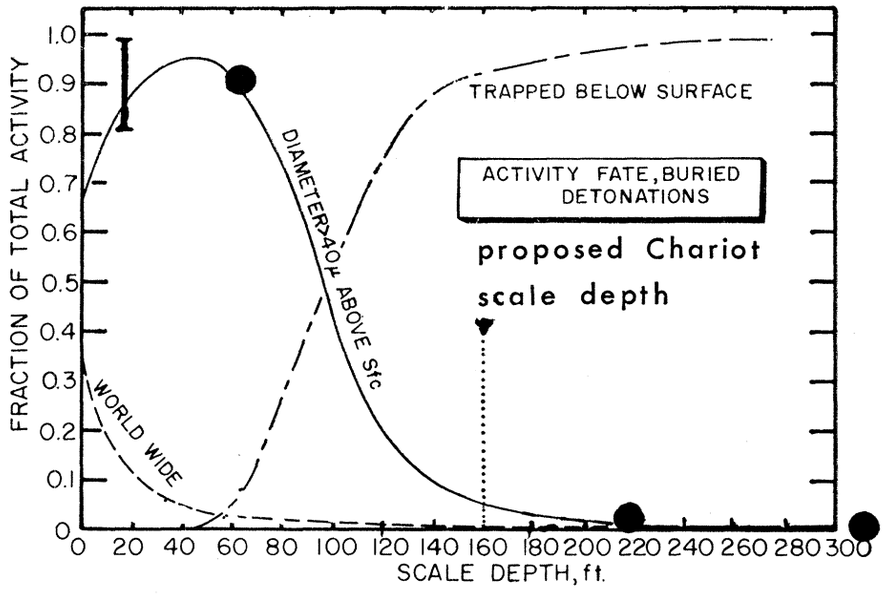

Fig. 1. This figure is taken from a paper by M. A. Harrison ("Excavation with nuclear explosives") in *Proceedings of the Second Plowshare Symposium, II, UCRL-5676*, to which we have added four points, and a dotted line indicating the scale depth of the proposed Chariot explosion. The values of the four points, which represent the actual data from past underground explosions (Jangle-U, Teapot-S, Neptune, and Blanca, in order of increasing scale depth) are taken from a paper by G. W. Johnson ("Excavation with nuclear explosives," UCRL-5917). The value of the scale depth of the Chariot explosion, as presently proposed, (160) is also taken from Johnson's paper. The point at issue is how well the curve relating vented yield (the solid curve, marked "Diameter $>$ 40 μ above sfc.") to scale depth can predict the vented yield at scale depth 160. The AEC prediction of 5 percent vented yield is represented by the point at which the curve crosses this scale depth. In the CNI report on this problem, it is pointed out that a number of curves can reasonably be drawn between the four points which represent the actual data, which will indicate, at scale depth 160, any value up to 25 percent vented yield.

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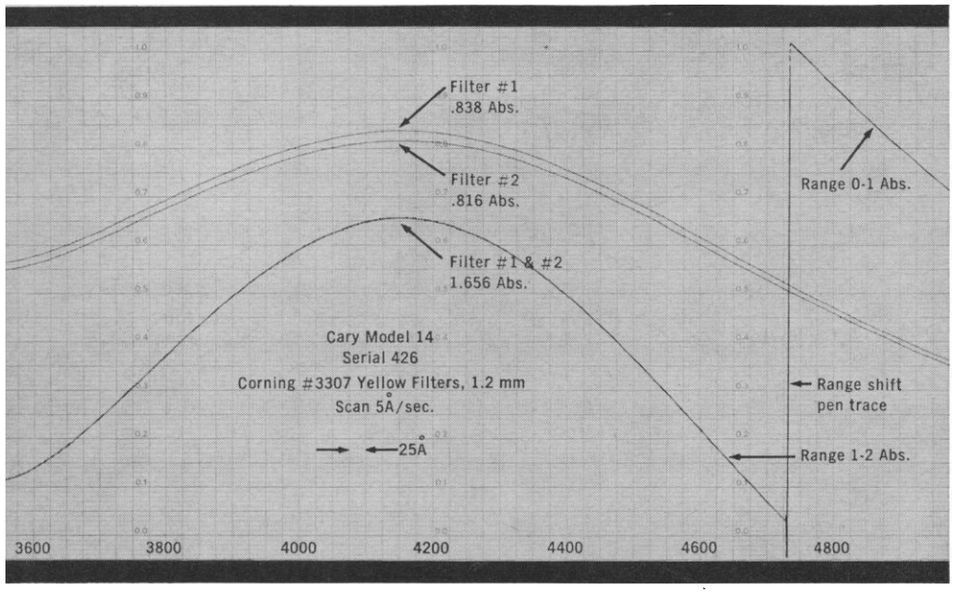

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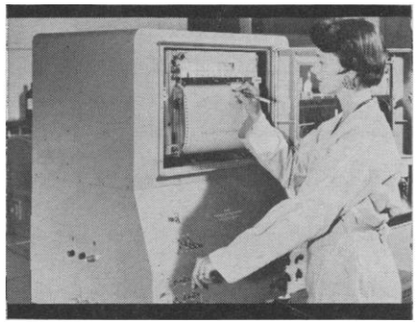

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for Kotzebue was 19 mi/hr. Since wind velocity has a decisive effect on fallout distribution, the AEC estimates of the fallout pattern must be in error.

3) Although fallout on the ground in northern Alaska from past nuclear tests is very low compared to that in temperate zones, the Sr^{90} levels of caribou and of the few Eskimo bones that have been analyzed thus far are much higher than the values for cattle and people in temperate United States. The CNI bulletin reports data which show that this is due to the unusual mineral nutrition of lichens. This remarkable situation is not mentioned in the AEC report, though we were pleased to learn recently that the responsible officials are now taking steps to institute a research program on the problem.

4) The CNI bulletin contains eight pages of detailed discussion, written by the biologists who investigated the problem for the AEC, of the unusual food chain in the Arctic (lichen-caribou-man). This discussion shows that predictions of biological Sr^{90} distribution based on the temperate-zone food chain (grass-cow-milk-human) do not apply to Alaska. Nor is the ecological behavior of Sr^{90} in tropical environments applicable to Alaska. Nevertheless, the AEC report on Project Chariot states that "possible radiation effects upon the biota of the Chariot site have been estimated from the Nevada Test site and the Pacific Proving ground data" (p. 55).

Margolis states that AEC officials believe "that the CNI assertion that the Sr^{90} yield might be ten times greater than the AEC believed likely was based on misreading of an AEC sponsored study. The study gave 5 percent as the most probable portion of the total radioactive yield that might get into the fallout." Margolis himself finds fault with the CNI conclusion and endeavors to show that at most the AEC estimate of fallout is low by a factor of 4 rather than by the factor of 10 suggested in our bulletin. As stated in the CNI report, the factor 10 is derived from two sources: a possibly fivefold underestimation by the AEC on the vented yield (which is explained above) and an estimated twofold error regarding the pattern of fallout deposition. The factor of 5 has been discussed already. The other factor of 2 can arise if the winds at the Chariot site are stronger than the values used in AEC estimates of the fallout pattern. Stronger winds could blow more of the fallout away from the immediate site (which can be

closed off from animals) and further downwind where it becomes accessible to the food chain. Of course, if the intensity downwind should rise the intensity at the site must drop accordingly, but for the reasons stated we are mainly concerned with some distances downward from the site. It seems reasonable that these effects could increase the AEC estimate of the downwind deposition of fallout by a factor of 2, because the wind velocities at the Chariot site tend to be significantly higher than the value used in the AEC estimate.

Margolis states on his own authority that "As it happens, the exposure from habitual television watching, or from current levels of fallout, is roughly the same as the exposure the 700 Eskimos might receive if pessimistic assumptions about the absorption of Sr^{90} are correct." Now, this sentence would be roughly correct if Margolis had added as an important condition, that the statement refers only to the effects of these three sources of radiation on the gonads. If Margolis were to amend his statement in this way it would then be technically correct, but still quite misleading to a reader interested in comparing the relative risks to his health from these three sources, because it fails to mention the risks to the bone marrow from these sources of radiation. It is an elementary fact now well established in the relevant literature that the risk from fallout radiation is of two kinds: (i) a genetic risk of deleterious mutations due to exposure to the gonads, and (ii) a somatic risk (from leukemia and other forms of cancer) due chiefly to irradiation of the bone marrow. The gonadal exposure is due to cesium-137; the marrow exposure is due to Sr^{90}. All published comparisons [see for example, the report of the British Medical Council, *The Hazards to Man of Nuclear and Allied Radiations* (Medical Research Council, HMSO, London, 1956)] of the radiation risk from television watching and from fallout refer only to gonadal exposure for the simple reason that while television watching may lead to a maximum of 1 mrem of exposure to the gonads per year, it has no measurable effect on the bone marrow, because the radiation is too soft to penetrate more than a few millimeters of body tissue. Hence any estimate of the risk from Sr^{90}, which necessarily refers to an effect on the marrow, will be incomparably greater than the hazard, to the bone marrow, of television

watching. It will be noted that Margolis' comment also includes a statement of equality between exposure to Eskimos from current levels of fallout, and from Sr^{90} that might result from the Chariot explosions. The exposure to Eskimos from present fallout is approximately known (about 1 to 3 $\mu\mu$c of Sr^{90} per gram of calcium in the bones; see *Radiological Health Data,* Jan. 1961, p. 21). We would expect Margolis to show, in support of his statement, that the Sr^{90} that Cape Thompson Eskimos might absorb from the Chariot explosion also amounts to about 1 to 3 $\mu\mu$c of Sr^{90} per gram of calcium in the bones. We suggest that he produce such calculations. For our part, after careful study of the available data, we concluded, as stated in the CNI report, that there were not sufficient data about the relevant parameters (for example the mineral nutrition and feeding habits of the caribou; the total Sr^{90} in the Eskimo diet) to warrant such a calculation.

None of the foregoing observations are adequately represented in Margolis' account of the CNI report on the Chariot Project. Nevertheless, Margolis had ample opportunity to become acquainted with these matters in advance of the preparation of his article. A few days after the CNI report had been made public, one of us received a long distance telephone call from Margolis. In this call he made several criticisms of the CNI report, and asked for comment on them. During this conversation Margolis acknowledged that he had not yet seen a copy of the CNI report. Accordingly, a copy of the report was sent to him immediately. After several days he called again. In this second conversation nearly all of the points which we have enumerated above (including an explanation of the so-called "technical error") were explained to Margolis at some length. We regret that they do not appear in his article. In particular, we believe that ordinary journalistic practice would recommend that the specific reply given to his query about the supposed technical error in the CNI report should appear in his article alongside his discussion of the AEC "complaint" about it.

We should also like to note that the quotations which Margolis attributes to "a spokesman for CNI" do not precisely reflect what was said to him, and it is pertinent that he neither asked for permission to quote (which would have readily been granted) nor checked the quotations with their source.

The foregoing comments explain why we believe that Margolis' article on the reports about Project Chariot is incomplete, misleading, and in some respects quite incorrect.

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In reply to the CNI letter:

1) The bulk of my article, contrary to the impression given by CNI's letter, was not concerned with CNI's technical errors or with my own predictions of radiation levels, but with the likelihood that the CNI report would mislead rather than inform the public on the central question of the magnitude of the fallout risk.

2) With regard to the material dealt with in the letter, much of it is simply a recounting of parts of the CNI report, and this recounting does not conflict with the summary of the report I gave in my article. Other parts are attacks on the AEC, to which I assume the AEC will reply if they deem it worthwhile. I should point out, though, that

the CNI summary of what the AEC said and did is not always, indeed is rarely, quite the same as what the AEC said and did. For example, the AEC began contracting for studies of the food chain, including the absorbtion of strontium, nearly 2 years ago, and indeed a good deal of the material CNI cites was developed as a result of studies sponsored by the AEC.

3) On the technical points directly questioning the reliability of my article: I don't see how CNI argues that it has not erred in its handling of the AEC fallout figures. The pertinent AEC report clearly states that its estimates are based on an assumed fivefold enrichment of Sr^{90} (not four- to five-fold, as stated in CNI's letter). The AEC prediction for the most probable average venting of fallout was 5 percent, and for Sr^{90}, 25 percent. Obviously CNI's report, which postulated a further fivefold increase in Sr^{90}, must be wrong, since the fallout can hardly contain more than 100 percent of all the Sr^{90} produced by the test. Further, Dr. Friedlander, in calculating CNI's estimate of the average Sr^{90} deposit throughout the zone, simply multiplied the AEC estimate by 10, and although CNI might well argue that the deposit "some distance downwind" might be 10 times the AEC estimate, the average deposit throughout the zone can hardly, for the reason noted above, be off by more than a factor of 4.

CNI is correct in criticizing my handling of the television example, although if readers will refer to my article I think they will find that the error is not as significant as CNI implies. What is curious is that CNI itself has included a grosser form of this same error in its own report. CNI does not inform its readers that there is no danger of genetic damage from Sr^{90}, but actually includes a reference to the possibility of genetic damage. Further, Dr. Commoner's article in the CNI report, in giving figures on the generally recommended guide lines for Sr^{90} absorbtion in humans, does not give the figure for Sr^{90} (67 units) but instead gives the figure for whole body exposure (including, of course, the gonads) and announces that "this corresponds to about 17 strontium units in the bone." Thus CNI misleads its readers into believing the generally accepted guide line is smaller by a factor of 4 than the actual figure, and this is done by applying the whole body rate, deliberately set this low because it includes genetically dangerous exposure, to calculate a rate for

bone marrow, where the danger is solely somatic.

This ties in with CNI's complaint about my statement that the probable increase in Sr^{90} in the 700 Eskimos, under pessimistic assumptions, would be about equal to present levels. CNI gives the current level of strontium in Eskimos as 1 to 3 units. This is based on a total sample of six. The values are: an infant (2.42), a 7-year-old (3.35), and four adults (0.18; 1.94; 0.47; and 0.59). Aside from the small sample size, it is difficult to know what would be a fair average calculated from this data to compare with the National Committee on Radiation Protection guide line, which is 67 units for an average for individuals within a population, and three times this, 200 units, for a single individual within the population. The levels for very young children are higher since they have been exposed to a given level all their lives. If, as with the Chariot test, the level is not to be kept up by continued testing, the level in the children will fall as they grow. This makes the CNI calculation seem somewhat excessive, but if we accept their figure of 1 to 3 units as the range for the average figure and compare it with the relevant NCRP guide line (67 units), then the current levels would be about 1/67 to 1/22 of the guide line. Even under the assumptions in the CNI report this seems unlikely to be increased more than several times, if that much, and the increase, like the base, will be some small fraction of the guide line, in other words (to repeat my error) an increase "roughly the same" as current levels.

The point of my comparison of the potential increase, under pessimistic assumptions, with exposure from television and current levels of fallout was not to imply that the type or amount of radiation was precisely the same, but to give the reader a general idea of the magnitude of exposure involved, in contrast to the CNI report which talked repeatedly in terms of "great uncertainty" concerning the harm that might be done, of the fallout "sealing off Cape Hope," of "little margin for error," and which, in general, could not have been better phrased to scare the daylights out of the lay audience for which it was written.

4) Finally, I must insist that my article reflects quite precisely Dr. Commoner's responses to my questions concerning the misleading nature of the CNI report.—H. M.