Around 1 December 1976, I was furnished with copies of three independently made forecasts for the forthcoming winter (December, January, and February) by the National Weather Service; by Robert R. Dickson, a visiting scientist at the Scripps Institution of Oceanography on leave from Lowestoft Laboratories, England; and by Joseph Chase of the Woods Hole Oceanographic Institution. These predictions were compared with forecasts made by me at Scripps. The methods used in preparing these forecasts, while differing with respect to the weights given to certain parameters, include some common factors, particularly atmospheric behavior in antecedent months along with the month-to-month behavior of sea-surface temperature patterns over the North Pacific. A more detailed account of the methodology was given in an earlier report of a 5-year experiment (*1*) and more recently at a 1976 American Geophysical Union symposium held in San Francisco (*2*).

These forecasts have been verified by comparing them in three equally likely classes: below, near, and above normal temperatures for 99 equally spaced points over the contiguous United States. The ranges of temperature which define these categories (terciles) were determined from a 30-year climatic record.

There was reasonably good agreement among all four forecasters. For example, my forecast agreed with the National Weather Service's predictions at 50 of the 99 points, with Dickson's at 84 points, and with Chase's at 82 points. Chance agreement would be 33 points.

As for the temperature predictions, the National Weather Service's forecast was correct at 44 of the 99 points, Dickson's at 63, Chase's at 63, and mine at 59. The corresponding large errors of two classes (that is, when above normal was forecast and below was observed, or vice versa), were four in the National Weather Service's predictions, six in Dickson's, two in Chase's, and four in mine.

All forecasts correctly anticipated the switch to cold weather in the East relative to the past five warm winters. The major region of disagreement was at the boundaries between the warm West and the cold East. Agreement between all forecasts and observed temperatures occurred at 27 points of the 99 points. Chance agreement among the forecasters with observed temperatures would be at about 1 point [actually $(1/3)^4$]. The major regions of agreement occurred in the northwest and southeast quadrants of the United States.

The scores for temperature prediction cited above are in general higher than those obtained from the past record of a dozen experimental seasonal forecasts I have made, where the number of correct points averaged 42 and the number of two-class errors averaged 13. These past forecasts were significantly more accurate than climatological probability alone (33 correct and 26 two-class errors). The point I would like to stress is that, *at times*, premonitory "signals" may be sufficiently loud that moderately successful forecasts can be made by different individuals using objective methods as a base (*3*).

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Low-Level Radiation: Predicting the Effects

In my letter to *Science* of 29 October 1976 (p. 478), I questioned predictions of cancer incidence based on the linear theory used by the American Physical Society in their reactor safety study (*1*), by the Environmental Protection Agency (*2*), and by others. Despite the arguments given in the letters from von Hippel (29 Oct. 1976, p. 479), Morgan (28 Jan., p. 344), and Brown (28 Jan., p. 348), no meaningful evidence of the validity of the linear theory is presented.

Von Hippel and Morgan cite an article by Modan *et al.* (*3*), on thyroid cancers produced in Israeli immigrants exposed to a thyroid dose of 6.5 rads during x-ray therapy for ringworm. Neither author notes that the 6.5-rad thyroid dose was the result of a cumulative scalp exposure of 350 to 400 rads. Evans (*4*), on the basis of personal communication with Modan, points out that the confounding effect of the concomitant irradiation of the pituitary, as well as the large statistical uncertainties, make use of the thyroid data questionable.

Morgan refers to an article by Stewart and Kneale (*5*) that is discussed in the BIER report (*6*, p. 165). That report points out that the linear relationship inferred by Stewart and Kneale is inconsistent with effects on 1250 children exposed to atomic bomb radiation in Japan.

The data of Stewart and Kneale would predict 18 extra cancer deaths, whereas none were observed.

Morgan's article (*7*), referenced by both von Hippel and Morgan in support of the linear theory, contains no new data which would add to the verification of the linear theory.

The theoretical discussion by Brown which focuses on high linear energy transfer (LET) radiation is interesting, but its conclusions are at variance with extensive data available on high LET effects. Evans (*4, 8*) reports on cancer incidence as a function of the bone dose of radiation received by those who worked on radium watch dials and by others who received medical treatment with radium-226 solutions (as was practiced before 1930). In a population of some 600 subjects, the 500 who received less than a cumulative bone dose of about 1000 rads were free of cancers. Of the 100 people who received between 1000 and 50,000 rads, there was a mean cancer incidence of 28 percent that was found to be essentially independent of dosage. I leave it to the reader to draw this step function distribution and to try to find a good fit of the linear theory that would be a straight line going through the origin. Evans points out (*4*) that additional data, gathered since publication of his work, now extends to 1700 people; again there were no cancers among people who received low doses. A similar threshold apparently exists for radium-224 (*6*, p. 126) and for radon inhalation (*9*). All of the above refer to high LET radiation.

In the case of the radium exposures, Evans notes that there is an inverse logarithmic relationship between dose and latent period before cancers show up. A similar relationship was found by Bair (*10*) in experiments on beagles inhaling plutonium and by Jee (*11*) in experiments where plutonium solutions were injected. The effect is also reported for uranium miners (*9*). A similar trend was noted by Jones and Grendon (*12*) after they reviewed data on exposure of animals to x-rays and studies of leukemia incidence among Japanese atomic bomb victims. As pointed out by Evans (*4, 8*), by Jones and Grendon, and others, the increase of latent period with decreased radiation exposure implies a practical threshold where the latent period is longer than the lifespan. Jones and Grendon provide a theoretical explanation of this effect. This theory and its supporting evidence is at variance with the linear theory as used by those cited in my original letter.

Morgan suggests that experimental data might be obtained by low-level irradiation of those who are not sure that beneficial effects are precluded from millirem per year dose rates. Such tests are, in fact, already in progress; both Morgan and I are participating. Background radiation varies by more than a factor of 2 in this country. Frigerio *et al.* (*13*) performed a comprehensive analysis of more than 40 geographical, social medical, meteorological, economic, radiological, educational, ethnic, and pollution parameters. He also analyzed mortality rates from malignancies for various age and geographic groups and for each of 56 separate malignant types. The study concludes: "Observations of the populations at risk showed not only no increment in malignant mortality with increasing background (radiation) but a consistent and continuous decrement." The linear theory as used in the BIER report predicts that a background radiation exposure of 170 millirems per year would cause an increase of 2 percent in the cancer death rate. In fact, the data of Frigerio *et al.* show a decrease in cancer mortality of about 20 percent from this exposure.

With present knowledge, no responsible person proposes the needless irradiation of people or opposes prudent standards to limit radiation exposures. My plea is merely for common sense in setting radiation standards and adherence to reasonable standards of scholarship and objectivity in public predictions of low-level radiation health effects.

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