Iodine-131 Levels in Milk

In his report "Nevada test fallout and radioiodine in milk" [*Science* **137**, 756 (1962)], R. E. Lapp suggested that an iodine-131 level of 100,000 micromicrocuries per liter might have been attained after a "rainout" of I^{131} over the Troy, New York, area on 26 April 1953. The calculations used, while based on many assumptions, are reasonable and indicate that high levels of iodine in milk theoretically can be produced as a result of weapon testing. I would like to make the point that for Troy, New York, at that particular time, the levels of I^{131} in milk in all probability never reached the calculated levels.

In upstate New York it is a usual practice of farmers to feed the cows in the barns until the grass has started to grow, the weather is warm, and the pastures have dried sufficiently to prevent destruction of the sod by the cows' hooves. Usually it is well into May before most farmers allow the cattle to spend all their time on pasture and to obtain all their roughage from pasture.

The weather records for the Albany Airport in 1953 reveal that the average temperature during April was 48.3°F. For 19 of the 30 days, nighttime temperatures of 40°F or less were recorded, a value of 27°F having been reported for the night of 22 April.

From the rainfall records it appears that the spring of 1953 was exceptionally wet. Precipitation collections of 3.4, 2.1, and 6.2 inches above normal were recorded during March, April, and May, respectively. The normal rainfall for these months is about 2.5 inches. In April there were only 8 days in which no rain fell. Since 26 April is early spring in New York, it is probable that there had been little growth of grass. This lack of forage, the wet days, the cold wet nights, and the wet soil make it unlikely that many cows had had access to pasture during that period. Even cows that may have been grazing during the day were certainly being supplied with their normal winter amounts of silage, hay, and grain during the night in the barn. For these reasons it is very unlikely that the levels of I^{131} in milk of the Troy area during the period in question came anywhere near the levels calculated by Lapp. In view of the wide publicity the calculations are likely to get it is important that the situation be assessed correctly.

The I^{131} level in milk is not the only

point of Lapp's article; the opportunity to use this situation to test the "linear response" theory is proposed. If the I^{131} levels in milk never did attain the calculated values, as appears to be the case, failure to observe an increase in the incidence of thyroid cancer in children in the 9- to 11-year group would not be valid grounds for rejecting the "linear response" theory of radiation damage. Moreover, in the event that an increase in the incidence of thyroid cancer was observed in this age group, how valid would it be to assume that this was due solely to the high I^{131} levels that might have been present in milk in April 1953?

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Lengemann contends that because of the late spring of 1953 there was delay in putting the cows out to pasture in northern New York and that thus the iodine-131 levels in milk did not come "anywhere near" those which I estimated. In preparing my report I did check the normal grazing period for the Troy area and found that it coincided with the time period of relevance to the Simon shot fallout. I double-checked this point by referring to Public Health Service measurements of I^{131} contamination in milk in the Troy area in May of this year. While inclement weather may have delayed normal pasturing in 1953, I estimate that the radioactive decay involved would not have reduced I^{131} uptake by a factor of more than 2 or 3. Other factors, such as sluice-off effects due to the heavy precipitation, could have been more significant. However, I did reduce the estimated maximum radiation dose to the thyroid by a factor of 3.

In reviewing my original paper I find that I underestimated the incidence of thyroid cancer by a factor of about 4. I used statistics on thyroid cancer deaths rather than thyroid cancer cases. The latter would of course be applicable to a thyroid survey.

Measurements of the intensity of fallout in the Troy area were very limited; it is entirely possible that the fallout field was more intense than that inferred from the actual spot data.

As for the question raised by Lengemann about the validity of attributing any thyroid cancers to the Simon test fallout, I pointed out in my report that the case histories would have to "correlate with the intake of fresh milk in the April-May period." Apart from

this, I assume that the investigators would employ reasonable methods to rule out cancers induced by radiation diagnosis or therapy and that no cases of previous pathology would be included.

However, Lengemann seems to have missed the main point of the report—namely, that the continental tests involve radiation dosage to humans in excess of any previously acknowledged levels. The single instance of Troy, New York (for which I managed to assemble some data), suggests the possibility that a thyroid survey might reveal evidence of radioiodine injury. But should this not be the case, other areas closer to the Nevada Proving Grounds, such as the Salt Lake City region, should be surveyed. I hope that the Public Health Service will undertake such surveys in the near future.

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Life on Mars

The article by G. V. Levin *et al.* [*Science* **138**, 114 (1962)] on a device ("Gulliver") for detecting microorganisms on Mars is fascinating. There are so many parameters of the testing apparatus which might be changed to improve the chances of a successful test that I suspect the authors will be inundated with suggestions.

The article suggests that a solid medium may be used instead of broth. If this is done it should be possible to use several different media in separated compartments, with a single detector for radioactivity. This would be an important change, since the medium seems to me to be the point most susceptible to improvement.

The medium outlined by Levin *et al.* would be too rich for many terrestrial microorganisms. It might be worse for organisms on Mars. If Martian life forms originated there, their stereospecificity could be the reverse of that found on earth—D-amino acids and L-sugars. In this case—and in many others which can be imagined—the complex medium would probably be toxic. (Similarly, the solidifying agent in the medium should be one of the silicones rather than agar.) I suggest an inorganic salts medium whose only carbon is traces of labeled acetate, glycerol, and glycine.

The article on Martian environ-