leukemia mortality rate was significantly higher, and higher by a factor of 1.5, than the leukemia mortality rate in the general population. This, as yet, inconclusive study by no means excluded radiation as the cause of an observed higher leukemia mortality in the patients treated with radioiodine.

If one reads our study carefully one can realize that when one compares a group of hyperthyroid patients treated with iodine-131 to a similar group treated with surgery, no difference in rates of subsequent leukemia is found. If Lewis believes that an excess of leukemia is attributable to the radiation of iodine-131, he would also have to attribute a similar excess of leukemia in the surgical group to some factor associated with the procedure of thyroidectomy. In our experience, the circumstances surrounding subtotal thyroidectomy or many other surgical procedures have not hitherto been associated with a postulated increase in leukemia. In analyzing the results of the Cooperative Thyrotoxicosis Study it does seem difficult to reach the conclusion that radiation can by no means be excluded as the cause of an observed higher incidence of leukemia without being logically directed to the same conclusion in regard to the patients treated surgically.

It is always difficult to determine when a study becomes conclusive. Three years after our initial report (*1*) there have been no changes in leukemia rates in any of the treatment groups. Furthermore, this study is unique in having a well-defined population of 36,050 persons with a follow-up rate of 98.8 percent yielding 35,613 studied patients and with reasonably well-calculated radiation doses. The sensitivity of detecting a doubling of leukemia with $P = .9$ is more clearly stated than for some other studies where diagnoses, numerator, and denominator values seem somewhat less precise.

The fact that this properly designed study of the largest number of patients yet reported exposed to low-dose radiations with careful follow-up and relatively precise dosimetry fails to fit certain carefully nurtured concepts may relegate it to a permanently inconclusive state in the minds of some. To date prospective studies of radiation at low doses in human beings have failed to show association between such doses and somatic effects. For example, Lewis might want to review the statements in his letter (*4*) concerning the 1.4-fold increase in children exposed prenatally to doses of not more than a few rads in light of the report of Jablon and Kato (*5*) wherein they have been unable to confirm this relationship in children exposed during atomic bombing in Japan.

We agree enthusiastically with Lewis that these investigations should continue but would hope that one's conclusions would be reached with appropriate consideration of the many factors concerned.

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Anointed but Not Appointed

Some response must be made to the recent report in "Harvard: New president's task to unify, preside over change" (22 Jan., p. 264) of a statement by Francis Burr bearing on the appointment of William H. Danforth as chancellor of Washington University. The implication that the immediate occasion for Danforth's appointment (if not the sole reason) was the appearance of his name in the widely advertised list of those being considered for the presidency of Harvard is as presumptuous as it is arrogant and untrue. The truth is that Danforth's accomplishments as a member of the faculty since 1957, and as vice-chancellor for medical affairs for the past 6 years, made him the unanimous choice of the selection committee. To suggest that this committee was precipitated into a decision by a rumor from the Charles River Basin is as offensive to all concerned as if we were to point out that Derek Bok failed to make the list at Washington University.

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