any other species, is that of certain ecologists to whom destruction of any natural habitat is wrong, no matter how useful the replacement environment may be to man. To this viewpoint we owe our national parks and wild rivers. But even these programs correspond to human objectives, in that they give intellectual satisfaction to a human elite, and furnish a base for further scientific studies.

In order to clean up the polluted parts of our environment, while seeking to limit the population growth and crass materialism which are the basic cause of it, we should try to unite scientific inquiry, technology, and political and social reform. The opposing forces are ignorance, prejudice, misinformation, selfishness, and inertia. . . .

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Some of Harold Schneider's objections to my argument stem from misreading on his part or lack of clarity on mine. In any case, I surely do not attribute this nation's unmatched rate of consumption to any peculiar American "propensity to consume," whatever that might be. I attribute it, as Schneider evidently does, to institutions (and resources) peculiarly well-suited to that purpose. Whether the special character of our consumer economy can be adequately described by the concept of an "open" society is another large question. If Schneider's definition of "open" includes the whole system of production for private profit, the superb want-creating apparatus called advertising, the unjust distribution of wealth, and the gross neglect of the "public sector" of human needs, including the need for a life-enhancing environment, I suppose he is correct. But then the question is: Can we any longer afford a society quite that "open" to the denial of our collective interests?

Schneider also seems to think that I consider the "urge" to consume, in the abstract, somehow "immoral." But of course that would be silly. What is immoral is not the urge but the failure to control the urge in the interests of justice and plain decency. What is immoral, in short, is capricious and excessive consumption when it means depriving others of their essential needs. Humanity's capacity for consumption is, for all practical purposes, limitless. But resources and productive power are strictly limited. The moral issue arises whenever we try, as we surely must, to minimize the suffering caused by that discrepancy. Therefore I do not feel the force of Schneider's attack upon me for making the ecological issue a basis, as he says, for condemning "socio-politico-economic enemies" and praising "friends." Why not? Ecological problems are in fact "socio-politico-economic" problems, and since some people relate to those problems in a self-serving, negligent, or otherwise irresponsible way, and others try to meet them with critical intelligence and imagination, why not make judgments? Making judgments about other people's behavior is, I think, what Schneider would call a "human trait." I plead guilty.

And then, finally, there is the insinuation that I am one of those softheaded types who envisage (as an alternative to what we have) "a utopian, pastoral America bereft of immoral, self-seeking aggrandizers." What could be more damaging to an American male, especially before an audience of tough-minded scientists, than the charge of being a utopian dreamer? My first impulse, I confess, was to present my credentials as a practical, feet-on-the-ground, realistic fellow. But then on second thought I *do* believe that it is possible to control the worst "aggrandizers." And I also believe that our magnificent and largely unused and uninhabited countryside could be transformed in accordance with certain "pastoral" (or ecological) ideals. This is not to imply that we suddenly would cease to be what we are—an advanced, urban, industrial society. But it would mean a sharp turn away from our reckless and mindless commitment to economic growth for its own sake, and I suppose that in our present situation that sounds—well, why not admit it?—utopian. Again: guilty as charged.

Turning now to William Whipple's thoughtful comments, I know that he is correct when he says that many scientists are skeptical about the alarmist predictions of ecological disaster. Many of them think, as he apparently does, that the problem is in essence technological. But I am dubious. It is one thing to say that technological remedies are conceivable, but it is quite another to believe we therefore will apply them in time. In America we have had the technological power to abolish poverty for years, but we have not yet developed the will to do it. The issue, as I see it, is deciding what we want, and whether we want it badly enough to relinquish the satisfaction of other, lesser wants. After that comes the political problem of acquiring and applying sufficient power to do the job. But in any event, my point is that we laymen want to hear men like Whipple talk back to their alarmist colleagues. We want them to argue, and we want to hear the argument. Scientists, in my view, have a responsibility to enlighten the public about the technological and scientific options, and how they relate to moral or political choices. But I also share Whipple's skepticism about the possibility (or even, for that matter, the desirability) of adopting a "pure" ecological perspective—one from which men would see themselves as no more important than any other species. Between that saintly viewpoint, however, and the arrogant Prometheanism of our expansionary system, there is plenty of scope for a relatively modest expression of mankind's relatively enlightened self-interest.

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Radiation and Leukemia Rates

In a report for the Cooperative Thyrotoxicosis Study we reported that the rate of occurrence of leukemia in patients with hyperthyroidism treated with iodine-131 was found to be essentially the same as that of a control group of hyperthyroid patients treated surgically (*1*). The radiation dose to the bone marrow was calculated to be 7 to 15 rads. At this dose and with the relatively low dose rates of iodine-131 (*2*), no excess of leukemia was found. We pointed out that the hyperthyroid group (those treated with iodine-131 and those treated surgically) when pooled showed a 50 percent increase in leukemia when compared to the age- and sex-corrected U.S. population at large. This study was subsequently cited by Holcomb (*3*) in support of a statement that "there are no studies that show increases in cancer at low (below 50 or 100 rad) doses although there are a few that should have detected it if it had occurred."

Our study was criticized by E. B. Lewis (*4*) as follows:

> The only study cited by Holcomb in support of the aforementioned statement actually showed that in a population of hyperthyroid patients treated with radioiodine, surgery, or both, the age-adjusted

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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