Letters

NAWAPA

Let me respond to your editorial (8 Jan., p. 113) on the $100-billion-plus North American Water and Power Alliance by agreeing that it would be large and that it is imaginative, but only within engineering limits. Four sorts of criticisms may be voiced.

1) It would destroy a great deal of the low-altitude wildlands of Alaska and Canada and a large fraction of the vestiges of such wildlands in the western states. No one, thus far, has undertaken to compare our need for these wildlands a century hence with our need for NAWAPA's boons, and no one on earth is either competent or in a position to do so objectively.

2) As premises, NAWAPA takes forecasts and definitions of use which are self-fulfilling, subjective, and self-serving. Burton and Kates, in a review [*Economic Geography* **40**, 82 (1964)] of three books published by Resources for the Future (RFF), have said:

> Demand for [water] . . . is not to be interpreted in conventional economic terms. . . . Water "demand" for the base year 1960 is really an estimate of the amount of water actually supplied. Similarly, projected "demand" for the year 1980 is really that amount of water expected to be supplied. . . . [The RFF authors] are caught in the prison of their own assumptions . . . [namely] that things will change but only in the way and at the speed that they are now changing. . . . The danger which we foresee is that projections may become self-fulfilling prophecies.

3) The NAWAPA proposal is in no sense an optimum, because imagination in seeking alternatives has been limited to engineering alternatives. Imagination must not be limited to alternate conduits or tanks. It must also ask whether we should electrodialyze the Colorado River to reduce the surplus of water required for irrigation in order to carry salts out of the soil; whether we should distill treated sewage in order to escape the free energy of removal of salt; what the economies of scale in desalination are. And we must be just as sure about the economies of scale and the technology of desalination a century hence as we are about the technology of water storage and transportation. Imagination must also inquire into the nature of "use." By and large, water is used only to carry something and, excepting for the transport of nutrient in irrigated plants, to carry away something not wanted, including heat. Is our imagination really so narrow that we can envision no other way to serve these purposes?

4) The major problem is quite separate: Common practice among those concerned with resource development is to take population as an independent variable, and population projections as immutable, sacred. Discarded by the engineers are the host of "if's" either tacit or explicit in the demographers' projections. . . . In fact, we do not know what the future will bring. While a ten-year projection seems to leave little room for uncertainty, still the halving of Japan's birth rate between 1949 and 1957, the failure of the American birth rate to drop as Sweden's dropped following a post-World-War-II "bubble," the persistence of California's growth, each of these has shown that earlier projections were not prophetic. The hundred-year projection should have uncertainties at least as great as the tenth power of the uncertainties in the ten-year projection.

One point comes clearly out of this: If we must plan for the century ahead, we cannot regard population as the independent variable. Whether or not we wish to plan populations as well as the facilities to serve them, we cannot escape the proposition that virtually everything we undertake will in some obscure way affect population changes and, thereby, the facilities needed. Even if we can identify all the significant influences and ask our computers where we go, the answer that results will itself be a significant influence. And when we include it in the computation, will we have an iteration sequence that converges or diverges? For, whereas the short-range forecasts seem to have a self-fulfilling quality, in the long range they should be self-defeating, whether they forecast "Our Plundered Planet" or "Enough and to Spare." Certainly a society convinced by the pessimistic forecast will modify its course to avoid such a fate, and a society convinced of the other will probably expand until there is nothing to spare. How far ahead does "self-fulfilling" neutralize "self-defeating"?

NAWAPA is a program of bankruptcy. After the water "for as long as 100 years" supplied by this development has all been "used," what next? And if we find a way to wiggle out of the predicament of A.D. 2065, how about that of 2165? No problem of population growth stemming from a static pattern of family size and death rates can be solved either by emigration or by technology. It can only be solved by a changing pattern of family size or of death rates. Science, technology, emigration can only postpone the issue. If the time gained by postponement is not used to find a solution in smaller families, then the problem will only have been enlarged. At today's growth rates, in each 35 years the piper's bill is doubled. It would seem the part of providence to attack the issue now rather than when time has come to an end. It would seem the part of providence not to lull an audience by telling them they may go their way secure in the knowledge they will be cared for.

If we must build NAWAPA, let us wait until we know our doom is at hand, and when our last realizable ambition is to amaze future archeologists.

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Cancer Chemotherapy Program

The Wooldridge Report on the activities of the National Institutes of Health (see News and Comment, 26 March, p. 1556) has failed to direct attention to the most significant achievements of the Cancer Chemotherapy Program. Whereas most research is rapidly reported at scientific meetings or disseminated in the form of articles in the scientific journals, the vast data accumulated under contract research is much slower in rising to the surface.

The primary screening program dealt with 75,000 compounds. It was fairly easy to publish the negative data. Most of the positive data have not been published, as the development of these leads is still in progress. Interim re-

ports on promising compounds have been sent to study sections. Some of these leads have formed the basis for individual or small-group research supported by other branches of NIH. In the field of hormone bioassay, over 2500 compounds have been tested to determine a profile of endocrine activity. Relationships of chemical structure to biologic activity cannot be properly presented until enough of the profiles are completed. The first of a series of monographs has recently been prepared on androgenic and myogenic compounds. Others are in preparation on estrogens, progestins, and corticoids. Numerous compounds tested for pharmacologic and toxicologic activity have given rise to promising therapeutic approaches which will take years to carry out. The reports, again, have been made not in the usual manner but in the form of internal reports to clinical groups, drug evaluation committees, and so forth.

It is suggested that more funds be made available to the administrative section of Cancer Chemotherapy, for the employment of a staff for data analysis, condensation, and dissemination to the scientific world as well as to the general public. Simple data such as the LD_{50} in mice of 75,000 drugs are now available and need to be prepared in a more accessible form.

The efforts of the Cancer Chemotherapy Program may be compared to an iceberg in that only a fraction of its bulk is apparent. The investment of relatively small amounts should make available masses of useful information.

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Biologists' View of Mars

The recent report, *Biology and the Space Sciences* (National Academy of Sciences–National Research Council, Washington, D.C., 1965), by a committee of biologists set up to advise the federal government on space studies, is disturbing. The report implies that there is every reason to believe that life on Mars is a reality and that this nation is justified in putting enormous effort and much treasure into an attempt to identify such life. It is apparent that the sense of this report is diametrically opposed to the view expressed so well in Abelson's editorial "The Martian environment" (12 Feb., p. 683). I am particularly concerned about the *certainty* expressed by the committee. One would think from the tenor of the report that there is available solid information which would allow us to conclude that life is present on Mars. It may be that secret information not available to run-of-the-mill biologists was available to the committee, for I do not know where in the published literature they could obtain sound data to support their very strong conclusions and recommendations.

It seems clear from a variety of sources that virtually no free oxygen can be expected in the atmosphere of Mars. One speculation suggests that oxygen in the form of ozone may be found at the surface of Mars; as we know, ozone is somewhat toxic to life. In addition there is little probability that water exists in any biologically usable form on Mars. One estimate suggests that the partial pressure of any water, in all the possible forms in which it might exist on Mars, equals only about 0.043 g/cm^2. These two factors alone suggest great caution in predicting that life is present on Mars. Estimates of the energy required to obtain biologically usable water from the rocks suggest that temperatures as high as 538°C and up to 3704 kcal per liter of water would be needed. It is a little difficult to associate these demands with any reasonable speculation concerning life on Mars. One must not forget that ultraviolet radiation, a potent sterilizing agent, is not filtered out by the Martian atmosphere as it is on earth. Consequently one expects that the surface of Mars would be constantly exposed to the sterilizing effect of unfiltered radiation of solar origin.

There are a respectable number of responsible scientists who feel that there are critical and challenging problems right here on earth—problems which have every probability of being solved if sufficient energies and treasure can be expended on them. These problems need immediate attention; they should be supported with enormous sums of money. By the same token, some of us who have devoted our lives to science for a good number of years feel that the low degree of probability of finding life on Mars does not justify giving this effort a very high priority in our overall national scientific endeavor.

In this age, when the opinions of scientists are listened to with great respect by the general public, it is mandatory that scientists be extremely cautious in making statements and proposing programs. I think the committee's report is not characterized by appropriate caution. In my view it contains all too little science and entirely too much enthusiasm. It certainly does not represent a consensus or a majority opinion of American biologists.

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Population Control: Births, Deaths, and Statistical Inference

In his letter (14 May, p. 893) discussing Wynne-Edwards's article "Self-regulating systems in populations of animals," H. Frederiksen concludes that "the reduction in the birth rate appears to be a consequence of the reduction in the death rate," but this is only one of the possible interpretations of the correlations he describes. High correlation between crude or adjusted rates of natality and mortality could reflect common cause quite as easily as cause and effect. Moreover, a decline in the crude death rate must reduce the crude birth rate unless the absolute *number* of births increases.

Concerning the relation between the numbers of births and deaths, correlation between the rates may be totally uninformative. Even in the unlikely circumstance that all three variables—births, deaths, and population—were totally uncorrelated, there would usually be positive correlation between ratios formed by using any one as a denominator. If the coefficients of variation were comparable in size, this positive correlation would be moderately strong; and if the coefficients of variation of the numerators were smaller than that of the denominator, it might be very strong. A reduction in the death rate may be, as Frederiksen says, a "precursor" of a reduction in the birth rate, but in itself permits no inference about changes either in the numbers of births and deaths or in the population density.

If the number of deaths could be sharply reduced or eliminated without any change in the number of births, the birth rate would drop sharply but there would be a substantial *increase* in population density. If it were the birth and death rates that required food