Letters

Clean Engines for Stratospheric Aircraft

Whether Concorde landing rights are denied or not, it is important for the nations to act now to protect the stratosphere from the effects of whatever future growth in high-altitude flight operations could possibly develop—anywhere, anytime. Therefore, before the question of landing rights is addressed, we should expect each nation involved to have committed itself voluntarily to a firm schedule for setting and enforcing clean engine standards that will guarantee the protection of Earth's ozone shield under any future circumstances.

As the person who started and made happen the Department of Transportation's Climatic Impact Assessment Program (CIAP) (*1*), I wrote Secretary of Transportation Coleman to this effect last month. In my letter I reiterated the understandings gained from CIAP that

1) Preservation of our ozone layer is imperative in order to shield us from biologically harmful ultraviolet radiation from the sun.

2) Any really large-scale commercial operations in the stratosphere (supersonic or subsonic) anywhere in the world would have to include strict engine-cleanliness standards in order to avoid significant worldwide reduction of the ozone layer.

3) Clean engine development is feasible, technically and economically, but will require a lead time of at least 10 years.

These understandings were confirmed by a committee of the National Academy of Sciences and the National Academy of Engineering (*2*). The possible danger of ozone reduction due to emission of NO_x by high-altitude aircraft was emphasized by Johnston in 1971 (*3*). The CIAP results have now shown his concerns to have been valid ones.

As a result of these findings, and even though current operations are not a significant threat, ironclad commitment to appropriate future engine cleanup should come from each nation before any further expansion of stratospheric operations. Included are France and Great Britain, now seeking landing rights for the Concorde; the Soviet Union, operator of the TU-144; and the United States and any other nations contemplating extensive *subsonic* operations at altitudes above those now customary.

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References

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2. Climatic Impact Committee, *Environmental Impact of Stratospheric Flight* (National Academy of Sciences–National Academy of Engineering, Washington, D.C., 1975).
3. H. S. Johnston, *Science* **173**, 517 (1971).

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Trend of the U.S. Birth Rate

June Sklar and Beth Berkov suggest in their article (29 Aug. 1975, p. 693) that "the decline in the nation's birth rate is coming to a halt and that an upturn is in the making." This interpretation seems to rest largely on a 2 percent increase in the California fertility rate between 1973 and 1974. National data show, however, that annual changes in the direction of the fertility trend have occurred many times before, only to be reversed a year or two later. Sometimes such temporary reversals occurred when the larger, secular trend was downward, sometimes when it was upward, as may be seen in the accompanying table (*1*).

Not only have there been annual reversals, but monthly data for the United States show that there have been many briefer reversals over time. For example, the national fertility rate increased, on a seasonally adjusted basis, for a few months in the latter part of both 1971 and 1972, but the increases were not sustained. Instead there was an overall 1 percent decline between 1973 and 1974, a point the authors ignored. And since then, through September 1975, there has been no evidence of an increase in the fertility rate for the United States as a whole (*2*).

Period	Size of reversal (%) during a period of	
	Downward trend	Upward trend
1856–58	0.8	
1890–92	3.6	
1919–21	8.0	
1938–39		2.3
1947–50		7.5

Two other pieces of statistical evidence, both cited by Sklar and Berkov, are incompatible with a strong resurgence in the U.S. fertility rate: (i) the continuing reduction in the proportion of women who are married and living with their husbands (*3*) and (ii) sustained reductions in the childbearing expectations of young women. Surveys conducted by the U.S. Bureau of the Census (*4*) show that married women 25 to 29 years of age reduced their total childbearing expectations by over 25 percent between 1967 and 1974, from 3.04 to 2.34 children. It has been shown that, in the aggregate, such expectations are a reliable guide to the average number of children women will actually have (*5*). To the extent that current fertility rates of younger women are below their stated expectations, one could expect temporary increases on the order of brief reversals in the past. However, these are likely to be swamped in the long run by the overwhelming demographic consequences of continued reductions in proportions of women married and in the number of children they expect to have. Sklar and Berkov correctly point out that the age structure of the female population, swollen with young women born during the 1950's, may contribute to an increase in the crude birth rate, or at least may sustain it above levels associated with a less fertility-enhancing age composition. But they have not made a convincing argument—in view of the other evidence—that the 1973–74 increase in California has potential national significance.

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2. National Center for Health Statistics, *Monthly Vital Statistics Report* (National Center for Health Statistics, Rockville, Md., Nov. 1975), vol. 24, No. 9, p. 2.
3. Bureau of the Census, *Current Population Reports*, Series P-20, No. 271 (Government Printing Office, Washington, D.C., Oct. 1974), table 1.
4. ———, *Current Population Reports*, Series P-20, No. 277 (Government Printing Office, Washington, D.C., Feb. 1975), table 1.
5. H. M. Rosenberg, R. E. Thomas, R. W. Cote, A. J. Kuntz, in *Proceedings of the American Statistical Association, Social Statistics Section, 1975* (American Statistical Association, Washington, D.C., in press).

It is undeniable that the decline in the American birth rate virtually stopped by 1974, as Rosenberg implies. Whether the rate rises or falls slightly in 1975, we

believe there are good reasons to expect a rise in the subsequent years. Our article summarized these reasons, using data from a variety of sources, which went far beyond the 2 percent increase in the California fertility rate in 1974. We were concerned not with small annual fluctuations in fertility rates but rather with the longer cycles which have characterized period birth rates in the past, and which have been the source of considerable economic and social disruption. We presented arguments indicating that this cyclical change is continuing. We noted that the proportion of young married women who are childless is now very high and there is evidence that very few of them wish to remain childless. We also noted the possibility that the large baby-boom cohorts now entering their early 20's might not continue the present pattern of postponing marriage and childbearing. In addition, we pointed out that some of the rise in illegitimacy probably reflects women's desires to have children regardless of marriage. For these and other reasons including the age and parity specific pattern of the rise in fertility in California, we suggested that an upturn was in the making. We did say, however, that "precisely when the expected rise in fertility will occur and how long it will last will depend both upon economic conditions and upon the willingness of women to continue postponement of marriage and childbearing."

Much work has been done in measuring ideal and expected lifetime family size, but almost no investigation has been made of short-term plans for timing and spacing of births which would permit much more precise prediction of changes in annual birth rates. With regard to lifetime birth expectations the question has been raised whether we can believe recent data which show unprecedented concentration on the two-child family (*1*). Blake has noted that the massing of responses in the two-child category is not congruent with what she finds is a continued tolerance for the large family and a continued aversion to childlessness and the one-child family. She suggests that recent responses have a heavy stereotypical component and that there be some conservatism in accepting recent birth expectations data at their face value.

Rosenberg's comments seem to exemplify the tendency to assume current conditions to be constant. In the depression, there was no anticipation of the subsequent postwar rise in fertility. In the baby-boom years, there was no expectation that birth rates would fall as fast or reach as low a level as they did. Rosenberg appears to have fallen into the same trap. He argues that any increase in the birth rate will be insignificant because of "continued reductions in proportions of women married and in the number of children they expect to have." He presents no evidence for this argument and ignores our discussion of past swings in attitudes and behavior with regard to both marriage and childbearing—swings that we argued may well occur again.

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Funding of Controversial Research

The topic discussed by Solomon Garb (Letters, 28 Nov. 1975, p. 834)—federal funding of research into genetic contributions to intelligence—is a delicate one, and the issues are far from one-sided. Nonetheless, I firmly believe that the reasoning of Garb's letter is faulty and that its implications are disturbing.

The basis for Garb's opposition to federal funding of research into the genetic contribution to intelligence is that "satisfying the intellectual needs or desires of a few scientists is not an adequate reason for spending public funds." Granting plausibility to such a criterion, I nonetheless find it impossible to accept unless it is applied evenhandedly. As far as I know, lack of general interest in a topic has rarely been invoked as a criterion for the funding of other federally supported research. If such a standard is not applied to other federal funding, the implication is almost inescapable that, although plausible, the standard is being used for unstated purposes. In fact (and ironically), the principle of looking behind reasonable-sounding rules to see how they are applied is commonplace in combating racial discrimination.

Discrimination aside, the standard of refusing funding when it is sought only to satisfy the "needs or desires of a few scientists" is inconsistent with what we know about the development of scientific knowledge. It is almost characteristic of some kinds of basic research that they begin as the interest of one person, or of a few people. Would Charles Darwin have received federal funding under Garb's criterion?

In criticizing Garb's arguments against such research, I do not mean to deny that a problem exists. If, for example, research should indicate with a high degree of certainty that there is a significant genetic contribution to intelligence, and that average intelligence of differing ethnic groups differs, there might be serious social consequences. That fact, however, does not justify discrimination against research into the area. Instead it counsels in favor of serious thinking about what we should do if it turns out that the data support a conclusion of differing average intelligence among ethnic groups. If that is the result of such research (and I suspect that it will not be), (i) the information would eventually emerge anyway, with or without federal funds, so that the social problems would be inevitable; (ii) the knowledge would prove in part beneficial because it would allow a wiser allocation of resources to the task of combating discrimination (for example, possible de-emphasis of programs stressing manipulation of environmental factors affecting a minority group); and (iii) the focus would then have to be shifted to where it should always have been—on the individual.

The last point is critical. The possibility that the *average* intelligence of any identifiable group is lower than the national average is irrelevant when dealing with any given individual—he or she should be treated on the merits, not by labels. If that is the direction we can take, research into the genetics of intelligence can take its proper place—no more relevant to interpersonal relations than the existence of quarks, but an equally valid subject of scientific inquiry.

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Migrating to Boston?

We were intrigued with the photograph of migratory wildebeest on your cover of 9 January. Our first reaction was that we were looking at a copy of the *Wall Street Journal*. Then, realizing that it was indeed *Science*, we concluded that, for reasons of your own, you had been down on the floor of the New York Stock Exchange when the Dow-Jones average hit a high for the last twelve months. Finally, however, when we read that it was the "Preconvention Issue," we realized, albeit belatedly, that you were giving us a dramatic pictorial photograph of several hundred AAAS members headed for Boston. We even found ourselves among them—seven in from center left, just below the staple.

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