# Letters

## AID's Family Planning Strategy

In view of the continuing controversy concerning the contribution of family planning toward solution of the world population crisis and the current role of the Agency for International Development as the "largest single supporter in the advanced nations of efforts to check population growth in the underdeveloped world" (*1*), many may be interested in the philosophy and strategy upon which the AID population and family planning program is based.

The ultimate goal of this program is to improve the health, well-being, and economic status of the peoples of the developing countries by improving the conditions of human reproduction in these societies. We propose to move toward this goal by support of broad gauge population and family planning programs, designed to make family planning information and services fully available to all elements of these societies so that women everywhere need reproduce only if and when they choose.

It has been argued that enlargement of human freedom by extension of family planning programs will fail to adequately curb the population increase and, therefore, other and even coercive social measures of population control are needed. This argument is largely based upon a narrow concept of what constitutes a family planning program. Some regard it simply as the provision of clinical and contraceptive services; others consider the many relevant actions contributory to greater use of available services and improved practice of family planning as an integral part of an effective family planning program. Viewed from this latter perspective, such actions as providing information and education, raising the marriage age, rescission of pronatalist laws and incentives, and repeal or liberalization of abortion laws are considered integral parts of a comprehensive family planning program.

On the other hand, population planning and control programs are distinctive from family planning programs to the extent they plan or control population dynamics by means other than fertility control, that is, by manipulation of mortality or migration or both. In AID, to avoid confusion, we usually speak of the population and family planning program.

A key judgment often heard is that the full implementation of family planning programs and the full exercise of fertility control by women and couples everywhere will fall far short of the goal of zero population growth because attitude studies have shown that women want too many children. But this judgment appears to be based upon the simple assumption that the composite response of women of all reproductive ages and experience to survey questions such as "What do you consider the ideal number of children?" bears a close and reliable relationship to the number of children women would have if they reproduced only if and when they wished, each month from menarche to menopause. This assumption is not supported by logic or definitive study.

Bearing and rearing children is hard work, and few women have unlimited enthusiasm for the task. If given the choice, each month, of whether they wish to be pregnant that month, many considerations other than ultimate family size guide their reproductive behavior; and for many women postponement of pregnancy means reduction in completed family size.

The current large excess of births over deaths in many developing countries has caused some pessimism concerning the collective wisdom of people individually exercising freedom of choice; but, on the other hand, there appears to be a sound basis for optimism: where in the world is there a country wherein people truly have the freedom and ability to control their fertility and where there is a continuing large excess use of their reproductive powers? The pattern seems clear that in those countries where women need not reproduce except if and when they choose (meaning they have access to hindsight [abortion] as well as foresight [contraceptive] methods of fertility control), the situation is encouraging. In Japan and several countries of Eastern Europe the net reproduction rate has fallen below 1, and social concern has shifted from the problem of too-great reproduction to concern for the possibly too-low reproductive rate. Certainly the high rates of abortion, even where illegal, as in the developed areas of Europe and the United States, and also throughout the developing world, bear witness to the determination of women in all cultures to limit their reproduction. Many women, whose foresight, knowledge, and means prove inadequate to prevent conception, will pay whatever they can and risk their lives to terminate pregnancies which they deem undesirable for reasons best known to them.

Because the extent of availability of family planning information and means is now usually a dominant determinant in the complex of forces influencing reproductive behavior, no definitive studies nor final judgments of additional measures which may *ultimately* be needed to achieve a desired rate of population growth can be made *in advance* of the full extension of family planning services. But as family planning information and services are made appropriately available, key impediments to optimal utilization of such services can be identified. Thereupon research studies should be performed as needed to overcome recognized obstacles and for advancement of the program. Naturally, many nonclinical actions, such as rational alteration of legal and fiscal codes, should be taken concurrently with clinical actions to enhance the effectiveness of the population and family planning program.

Regardless of what special social measures may ultimately be needed for optimal regulation of fertility, it is clear that the main element initially in any population planning and control program should be the extension of family planning information and means to all elements of the population. It seems reasonable to believe that when women throughout the world need reproduce only if and when they choose, then the many intense family and social problems generated by unplanned, un-

wanted, and poorly cared for children will be greatly ameliorated and the now acute problem of too rapid population growth will be reduced to manageable proportions.

R. T. RAVENHOLT
*Agency for International Development, Department of State, Washington, D.C. 20523*

**Reference**

1. K. Davis, *Science* **158**, 730 (1967); L. J. Carter, *ibid.* **159**, 611 (1968).

## Homology: A Definition

I regret the error in citation (the journal name was given as *Nature*, rather than *Science*), which crept in among the 462 references of the review (*1*) to which Winter, Walsh, and Neurath take exception (Letters, 27 Dec.). In that review, the term *homologous* was taken to imply, in parallel to universal biological usage, "that the genes coding for the polypeptide chains considered, in all the species carrying these proteins, had at one time a common ancestral gene," and we stated that when this concept is not intended "it would be best to use any of the numerous synonyms of 'similar' and 'similarity' and not appear to be prejudging the issue of evolutionary relations." The "pointed and specific criticism" followed, and was entirely contained in the sentence: "Other definitions may cause confusion and are unlikely to supplant well established biological usages." The "other definitions" referred to the article by Neurath, Walsh, and Winter (*2*), in which they state, "The term homology as applied to proteins refers to similarity in amino acid sequence," and later, that comparisons of protein structures "must be interpreted on a statistical basis lest we misinterpret random similarities."

On this last score there is no argument. Winter, Walsh, and Neurath will surely agree that in this field erroneous conclusions are likely to arise from the lack of an appropriate statistical distinction between random similarities and similarities of structure greater than can result from random phenomena. An excellent method of performing just such a distinction was published by Fitch (*3*), and although Neurath, Walsh, and Winter acknowledge it in their article (*2*), they do not use any acceptable statistical techniques in their comparisons of proteases. Thus, even by their own definition they fail to show "homology."

Homology, in any biological evolutionary context has a generally understood and well-defined meaning, namely the one we have adopted for use in protein primary structure comparisons. One cannot argue that such comparisons represent an area of knowledge separate from evolutionary biology, and that therefore one may use the same words for other meanings, since such protein studies obtain their interest largely in terms of evolutionary concepts and have their major impact in the taxonomic-evolutionary field. Winter, Walsh, and Neurath justify their novel definition of "homology" by maintaining that, without fossil remains, it is not possible to decide whether the structural genes corresponding to a set of present-day proteins are or are not ancestrally related. Apart from the inherent danger of assuming that a problem is insoluble, it may be pointed out that six pages after the definition of "homology," the paper (*1*) reviewed a statistical method for demonstrating just such ancestral homology. One requires enough primary structures to derive a "statistical phylogenetic tree," as has been possible in the case of cytochrome *c* (*4*). From such a tree a simple statistical calculation permits one to approximate the number of residues in a set of proteins that will remain invariant, because of biological necessity, no matter how many species are examined (*5*). If, in the comparison of any two proteins of this set, the number of identical residues is substantially in excess of the number that remain invariant in the entire set of proteins, then clearly this excess cannot result from functional convergence from different phylogenetic origins, a process yielding *analogous* structures, and, therefore, it can only be attributed to ancestral homology. In such a procedure, the assumption of the constancy of the genetic code has replaced the fossils of the morphological evolutionist.

Even if one does not accept the validity of such a demonstration, it is difficult to understand why there is an insistence on using the word "homology" for "similarities of protein primary structure greater than random." Any of the over 30 synonyms of "similarity" (*6*) or a variety of elegant neologisms would do, and prevent an insidious misunderstanding likely to arise in biological literature. Rather than take Alice in her confused trip in Wonderland as a model for logical scientific nomenclature, I prefer to follow the 17th-century poet reacting against a form of debasement of the language then prevalent, and "call a cat a cat" (*7*).

E. MARGOLIASH
*Department of Molecular Biology, Abbott Laboratories, North Chicago, Illinois 60064*

**References**

1. C. Nolan and E. Margoliash, *Ann. Rev. Biochem.* **37**, 727 (1968).
2. H. Neurath, K. A. Walsh, W. P. Winter, *Science* **158**, 1638 (1967).
3. W. M. Fitch, *J. Mol. Biol.* **16**, 9 (1966).
4. W. M. Fitch and E. Margoliash, *Science* **155**, 279 (1967).
5. W. M. Fitch and E. Margoliash, *Biochem. Genet.* **1**, 65 (1967).
6. *Roget's Thesaurus* (St. Martin's Press, New York, 1965).
7. N. Boileau, *Satires I*, line 52 (1660). *"J'appelle un chat un chat, et Rolet un fripon."*

## Teaching's Third Dimension

Pitzer's article ("University integrity," 11 Oct., p. 228) focuses in part on the critical dimension of student-faculty relationships. The faculty role is depicted as composed of two factors—teaching and research, not necessarily in that order of importance. I would like to emphasize a third equally important charge of the faculty member—that is, the role of personal and educational counselor and adviser to the student. Any professor who is reasonably accessible personally and geographically will attest to the frequent, almost continuous, and apparently very important student-to-professor counseling sessions on every subject from personal problems to specialized career planning.

This third dimension is so much a part of the professional job that it is hard to question its appropriateness. Those who do, even in the glaring light of the present student unrest, should be reminded that advocates of good educational practices have long stressed the importance of interpersonal relationships as the basis for meaningful behavioral change—a basic goal of education. Even some of the more ardent proponents of technological aids to instruction [for example, Skinner (*1*)], support their positions with the observation that these aids will free the teacher to increase the personal component which no device, save the human, can accomplish. These interpersonal relationships have the greatest impact on the emotional concerns of the student and also support the cognitive or intellectual change we expect. Even the