couple, one child"; (iii) to persuade couples wishing for a second child to have planned spacing; and (iv) to avoid second or multibirths outside planning (with proper flexibilities for national minorities).

Advocacy of "one couple, one child" does not mean that every couple must have only one child. The 1985 statistics show that only about 20% of China's 190 million married couples of childbearing age have expressed a wish for one-child families; nearly 20% of eligible couples currently have third and subsequent births.

Three points should be clarified.

1) Some Chinese regulations do stipulate certain conditions under which couples may have two children, with the implication that other couples, if having a second child, will have to pay certain fees to the public welfare fund to lighten the burden on society. This is not meant to stop a second or third birth entirely, since even children born out of wedlock are protected by law.

2) Psychologically speaking, if couples were encouraged to have one child each, a portion of them would tend to have two. If they were encouraged to have two children each, then there would be more cases of third and high-order births. That is the general phenomenon in rural areas of China.

3) The important thing is that people's wishes are respected. China has carried out public education in various forms to encourage the practice of the one-child family norm, but has not used coercion to force it upon the people. In China, human responsibilities are stressed as well as human rights. All policies, no matter how good, must be adopted on the condition that the majority of the people will accept them.

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NOTE

1. This letter is condensed from a longer manuscript available from the author.

Response: In writing our article, we hoped to stimulate research and debate on alternatives to the one-child-per-family policy, a policy of obvious high cost both to the Chinese population and, in political terms, to the Chinese government.

Zhao does not address the nature of the analysis presented in the article. The article was not about China's current fertility policy and whether or not it has "broken down." (In fact, as we point out, it has been extraordinarily successful in meeting its demographic objectives.) Rather, the piece addressed the issue of policy criteria—the number and kinds of factors that should be considered in the formulation of a demographically effective, socially and economically desirable, and culturally acceptable population policy.

Between the late 1970s and the mid-1980s the number of factors considered in the formulation of China's population policy appears to have widened. In our article we further expanded the range of factors that, arguably, should guide policy choice to include the policy's macrodemographic impact on population size and aging; its microdemographic effects on the family's ability to support the elderly, its economic capabilities, and the position of women; and the cultural acceptability to the Han Chinese population. Most of the article consisted of an evalution of the present policy and five hypothetical policy alternatives with regard to these criteria. Our conclusion suggested that when the policy options are weighed with all these criteria, the present policy ranks below the other options. We argued that the 27-4 option is appealing not so much because it meets China's specified demographic objectives—although it does so—but because it succeeds in achieving a wide range of desirable social, economic, and demographic goals.

With regard to the timing of childbearing, we maintain that the demographic impact of delayed childbearing has consistently been underestimated in China. Even though, as Zhao indicates, current policy continues to advocate late marriage and long spacing, in fact these policy elements have been given less emphasis than the reduction in the number of children. As a result, in some parts of the country the age at marriage and childbearing has recently been falling, with obvious consequences for population growth rates.

Zhao's comments on "cultural conditioning" as expressed in peasants' marriage and childbearing desires are very much to the point. Where we differ from Zhao is in the policy implications of these cultural preferences. He implies that because "there is not likely to be a single woman in the rural area who would wish to delay having her first child until the age of 27," a policy stipulating such a delay is not worth considering. Our view is that all options should be open for discussion. On the issue of delayed childbearing, we would point out that demographic preferences of individuals do not exist in a vacuum, but are subject to constraints—most particularly, in the case of China, constraints imposed by the needs of society as determined by its leaders. With regard to delaying childbearing until age 27, our argument is not that peasants would wish to do so, but that, given a societally dictated choice between having one child any time or two children beginning at age 27, a rural Chinese couple might well prefer two children at a later age. Our general aim here was not to advocate any specific policy, but to expand the range of options available to policy-makers, offering policy alternatives that may be more in line with individual preferences and more effective in achieving national demographic targets than the current policy.

A trip to China in the fall of 1987 indicated that scholarly interest in population policy alternatives is growing. We welcome further dialogue on the desirability of different options and the criteria underlying policy choice.

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Newton Gravitational Constant

A controversial topic of late is the possible variation with distance of the Newtonian gravitational constant, G. One explanation for this phenomenon is a possible fifth force in nature. Because the ramifications of such an important new effect would be far reaching, there is considerable enthusiasm among scientists to perform experiments that might shed light on the issue. The strongest evidence for non-Newtonian behavior is the difference between the value of G measured in an Australian mine shaft, where the length scale of the experiment was hundreds of meters (*1*), and the value of G determined very accurately in the laboratory over a length scale of tens of centimeters (*2*).

In his report "Borehole measurement of the Newtonian gravitational constant" (21 Aug., p. 881), A. T. Hsui correctly concludes that his results for G are "generally consistent with those of the Australian experiment." Although it is difficult to compare the uncertainties of the various results because the geophysical error bars contain a subjective element, Hsui's results are also consistent with the value of G found by Luther and Towler in the laboratory and, indeed, with the value surmised by Isaac Newton some 300 years ago (*3*). The unavoidably large uncertainty in Hsui's measurement that comes about from inadequate density information does not allow a conclusion to be drawn about the discrepancy between the value determined by Stacey's group in Australia and the laboratory value. Contrary to the statement in This Week in

Science (21 Aug., p. 819), Hsui's G value neither confirms nor refutes the anomalous value of G reported by the Australian group.

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Response: Although by no means definitive as stated in the conclusion of my report, evidence for a small positive deviation from the laboratory value of the Newtonian gravitational constant by Stacey and his colleagues can be derived from my results. The consistency of this discrepancy (both in sign and in magnitude) does provide some support for the scientific merit of the studies. Perhaps a clarification of how the uncertainty in my data were estimated would make this point more clear. As stated in the report, errors were estimated with the assumption of the largest possible uncertainty associated with each and every measured variable. The major uncertainty of the measurement is in the density determination of the strata. The reported error is calculated with the assumption that the uncertainties associated with each individual stratum are biased in the same direction and to the same extent. This is a conservative approach, and the resultant error bars are expected to represent overestimates. In order to have a narrower error bound, one needs to know the response of the gamma-gamma log to all the different rock types representing the strata. Unfortunately, the necessary information is not available for an improved error estimation.

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Big League Botany

In her interesting article on the botanists' newest effort to produce a "Flora of North America" through a large-scale, multiyear, collaborative project, spearheaded this time by the Missouri Botanical Garden, Marjorie Sun (News & Comment, 28 Aug., p. 967) does a fine job of putting forth the case for such an enterprise. I would, however, like to provide some additional information about the earlier Flora of North America (FNA) project headquartered at the Smithsonian (*1*).

The Smithsonian's FNA project did not fail mainly because of "cumbersome" computer technology. To be sure, it would have been much easier to complete with today's computer technology, but it was not stalling because of technological difficulties (*2*). Nor was it the National Science Foundation's withdrawal of support that ended the project, as that occurred only after the Smithsonian was unable to get a line-item appropriation and concluded it could not proceed. The project fell apart because the arrangement for guaranteed long-term support fell apart. It is my conviction (*3*) that no effort, including the present one, will succeed without first solving this problem. Both the flora-writing and the continuing maintenance and growth of the resultant database require such long-term support. Surely, this should not be a one-shot effort, but a permanent resource assessment and monitoring endeavor.

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2. H. M. Krauss, *ibid.* **25**, 119 (1973); S. G. Shetler, in *Biological Identification with Computers*, R. J. Pankhurst, Ed. (Academic Press, London, 1975), pp. 197–235; *Taxon* **23**, 71 (1974).
3. S. G. Shetler, in *Foundations for a National Biological Survey*, K. C. Kim and L. Knutson, Eds. (Association of Systematics Collections, Lawrence, KS, 1986), pp. 87–89.

Response: As Shetler states, the Smithsonian halted the Flora of North America project in 1972 because of lack of long-term support. More precisely, Congress, after supporting the program for several years, decided not to provide the Smithsonian a line-item appropriation for the program. As a result, the National Science Foundation (NSF), which had been prepared to match congressional funding, stopped its financial support. According to NSF official James Rodman, troubles with computer technology and the lack of familiarity with computer technology by taxonomists at the time were "some of the problems that torpedoed the project."—MARJORIE SUN

Sun's article about the proposed Flora of North America project attributes to Michael Strauss of the National Academy of Science's board on basic biology the suggestion that the project has failed to catch the fancy of NSF because it does not incorporate much basic research, and instead, the data will largely be accumulated from recent publications. There is an element of truth in this observation, but the matter needs to be expanded on just a bit.

Research in plant taxonomy has several facets, including (i) studies to produce new sources of information, for example, the recent interest in chloroplast DNA; (ii) studies to produce better integration of data, for example, cladistic and phenetic analyses; and (iii) studies to elucidate taxa, their interrelationships, and their evolution, such as monographic and revisionary studies of natural assemblages. Monographs and revisions normally incorporate elements of the first two, but they usually focus on distinguishing the entities under investigation. Monographs and revisions, in turn, provide basic data for a fourth facet of plant taxonomy—floristic treatments and similar works that are directly relevant to a broader scientific community and to the general public.

Digestion and synthesis of the accumulated information in monographs and revisions is a monumental undertaking, and it is notable that plant taxonomy has a well-developed, historical sense of the need for these syntheses. Taxonomists correctly understand that their works are of limited utility until they are synthesized into a "floristic whole" and thus become usable to the larger community of biologists. The Flora of North America project should not be seen merely as a compilation of data lying in the literature, for it is much more than that. It is a synthesis of the diffuse data that document our present understanding of the vascular flora of the continent, north of Mexico. Writing the Flora of North America will require a critical assessment of the available information, as well as decision-making about innumerable taxonomic and nomenclatural matters. The utility of the resulting Flora is beyond dispute, but perhaps the most basic contribution would be to provide a clear statement of the present level of understanding of the systematics, evolution, and geography of the flora.

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Erratum: In David Dickson's News & Comment article "Bumps and falls on the road to Stockholm" (16 Oct., p. 263), the name of Magnus Gösta Mittag-Leffler (p. 264, column 2) was misspelled.

Erratum: In the letter from Keith Bowker concerning journal price increases (30 Oct., p. 597), two conversion rates referred to in the sixth paragraph were incorrect. The rates should have been \$1.69 per £1 and \$1.67 per £1, respectively.