in two faces of the cube provided a mirror, and the current elements in the other four faces provided two orthogonal cusps. Examination of this configuration shows that two of the current elements in each mirror face are redundant, and the minimum B can be accomplished by a single continuous current element lying along eight edges of a cube. This rectilinear configuration is similar to the cylindrical configuration of Perkins and Furth, but neither of these coil designs provides a suitable minimum B, mainly because of their poor mirroring properties. It was necessary to recognize that by warping the coil so as to bring the mirror current elements closer together the mirroring properties could be improved. At about this same time the theoretical division at Culham had been pursuing the separate question of what coil configurations would generate spherical constant-B surfaces. From these two lines of thought and after several months of theoretical analysis and numerical computation, the tennis-ball winding evolved sometime in the late winter or early spring of 1964. In May I attended a colloquium at Culham in which the speaker outlined the recent development and in his presentation made use of several models of coils, including the tennis-ball winding.

Late in June, V. K. Neil of Livermore paid a visit to Culham and was informed of the tennis-ball developments, which he in turn communicated back to Livermore by letter. On receipt of the Culham letter at Livermore, it was recognized immediately that the tennis-ball winding offered the possibility of easy beam access for a type of wide-angle neutral injection fashionable at the time. At this time the Alice group had successfully stabilized their mirror plasma by addition of a 12-rod Ioffe winding, had abandoned the axially symmetric designs and was studying the possibilities of a mirror-quadrupole system. After hearing of the British developments, a study program was initiated to explore the field configuration and Lorentz ionization properties of the tennis-ball winding and from this study program there later developed the design for the Alice baseball coils.

The above discussion of a particular scientific invention and approach to controlled thermonuclear research is, I believe, an illuminating and instructive example of the course of an international scientific-technical development. Whether the "baseball" (née "tennis-ball") field or the simpler mirror quadrupole geometry will prove experimentally the most profitable approach remains for the future to determine.

John R. Hiskes
Lawrence Radiation Laboratory, Livermore

Prolongation of life

I believe as Feinberg in his recent article that appeared in the November issue of PHYSICS TODAY that sometime in the future mankind will eliminate aging and also death. Feinberg, however, seems to leave one with the feeling that the solutions to the technical problems of life prolongation should be hastened as they would make voyages to far interstellar areas more probable and such voyages would be a major contribution of science to humanity.

My view is that the religious and social institutions may not be able to change rapidly enough to cope with the developing social problems such as the regulation of births to prevent the population from becoming larger than the availability of food supply and standing room. Even now the world per-capita food supply is decreasing because of rapid increase in population.

Feinberg should also bear in mind that if science is to serve all of humanity, it must be more closely coupled with its consequences to all of society. I believe that if this is not done, the death of our new freedoms of intercourse in return for death immunity is a high price. You can have it.

Victor E. Grob
Glastonbury, Connecticut

In connection with Feinberg's article one additional point seems worth noting. Recent experiments on the biological role of ice (see, for example, the article by N. H. Grant, *Discovery*, August) have indicated that some impor-

tant biological reactions are actually *accelerated* by freezing rather than slowed down as expected. Although the mechanism for this acceleration is not fully understood at present, it is not difficult to construct models in which ice acts as a catalyst for various "geometrical" reasons. Since the preservation of an organism over an extended period would presumably require freezing rather than simply cooling it, the problem of deterioration by ice-catalyzed reactions must be added to those already mentioned by Feinberg. In the spirit of his article, however, this problem does not seem insurmountable.

Ephraim Fischbach
University of Pennsylvania

Reviewer and author anonymity

Moody L. Coffman's letter (PHYSICS TODAY, November, page 12) contains two suggestions for change in editorial policy of scientific journals: either making the writer of an article as anonymous as the reviewer has traditionally been or revealing each's name to the other. The first alternative may sound attractive, but in practice it succeeds only a small fraction of the time. Despite removal of author and institutional affiliation from a manuscript, no phenomenal deductive powers are required, for example, to guess the authorship of an article that begins, "Earlier work (Coffman, 1962, Coffman and Moody, 1965) has shown . . ." The *Journal of Speech and Hearing Research*, for which I occasionally review, indeed experimented with this scheme last year but quickly abandoned it.

The other proposal, though, is not only more workable but, it seems to me, eminently fair. If one is to pass unfavorable judgment on a scientific article, one should do so as an individual and not as an anonymous godlike censor. If a reviewer feels an article to be poorly conceived, sloppily executed or incoherently reported, he surely should be able and willing to support his views.

Of course objections immediately pop to mind. One is that some authors simply can not see defects in their work even when these shortcomings are spelled out in exquisite detail and will tend to embroil reviewers in lengthy but nonrewarding exchanges of letters. However, the reviewer might be able to feel a warm glow from the knowledge that he rescued the editor from such exchanges.

A second objection is recognized by Coffman, who mentions the problem of keeping friends. Alas, it is quite true that a good reviewer may make some enemies at times. Nevertheless I believe that *good* writers and reviewers can maintain friendship despite interchange of criticism. Besides, in the last analysis, which is more important—friendship or truth?

W. Dixon Ward
University of Minnesota

Moody L. Coffman suggests that articles be sent to reviewers anonymously. This is an excellent idea and has been proposed many times. Unfortunately it is impossible. Removing the name and affiliation of the author does not make a manuscript anonymous. A competent reviewer can tell at a glance where the work was done and by whom or under whose guidance. One must also remove all references to previous work by the same author, all descriptions of special equipment and other significant parts of the paper. Nothing worth judging or publishing would be left.

S. A. Goudsmit
Managing Editor,
American Physical Society

A correction

I would like to call your attention to two small historical mistakes in your September issue.

On page 45: It was not Marcel de Broglie, but Maurice de Broglie, who attended the Solvay Congress.

On page 121: Georges Lemaître did not go to Louvain to study humanities. The Collège du Sacré-Coeur is in Charleroi, where Lemaître was born.

C. Courtoy
Namur, Belgium

The editors apologize for the mistakes, both of which were their own. □