Letters

Memory Mechanisms

The article "Molecular theories of memory" (*1*) appeared as welcome relief from the generally cursory discussions of this topic that have recently appeared in both technical and popular publications. Although I find myself in agreement with Dingman and Sporn's treatment, I believe there are basic difficulties in any molecular theory of memory which are not mentioned in their article.

Recent discussions of a molecular mechanism of memory have assumed a possible analogy between mental and genetic events. As Dingman and Sporn wrote, "The spectacular success of recent investigations of the molecular basis of transmission of genetic information has suggested that there may be an analogous molecular mechanism for storing and utilizing experiential information. . . ." In more explicit form, this analogy with "genetic memory" has led to serious consideration of a possible nucleic acid engram for memory. Gaito, for example, argued, "In that DNA provides a genetic code via the linear sequence of bases, it is plausible to expect that DNA or RNA provides an experiential code in the same way" (*2*).

For this reader, such an analogy is misleading, because it fails to consider the fundamental difference between genetic information, which remains constant during the life of the organism, and information in memory, which is the product of the individual learning process. Whereas genetics is Darwinian, memory is distinctly Lamarckian. Although there exist molecular mechanisms within the cell for exact replication of hereditary information, we do not know of any such mechanisms for the codification and transmission of acquired characteristics. Any molecular mechanism of memory, however, must constitute a molecular method of acquiring new characteristics.

The non-Lamarckism of genetic processes is reflected on the molecular level in the fact that nucleic acids only reproduce preexisting structures through the necessarily complementary relationship between nucleotide sequences. Thus DNA and all known species of RNA (viral, messenger, ribosomal, and soluble) are produced on cellular templates. This experimental fact has been disregarded in a number of molecular theories of memory (*2, 3, 4*), which have virtually posited an ability of RNA to change spontaneously as a result of cellular experience. For example, "If DNA, which is considered exceptionally stable and unchangeable, encoded an organism's 'racial memories', perhaps RNA, which is known to be much more malleable, could act to encode an organism's 'individual' memories; hence RNA would be what is now called the 'memory molecule' " (*3*). If this malleability of RNA means a Lamarckian freedom from the necessary duplication of preexisting sequences, it is simply not true in terms of known biochemistry.

The main point of this letter is a reminder that any molecular theory of memory (unlike a genetic theory) must include a molecular theory of learning. The very Lamarckian nature of the learning-memory process thus presents a fundamental objection to "the particular hypothesis that specific changes in neuronal RNA represent the molecular engram of memory" (*1*).

These objections seem quite undermining to virtually any molecular theory of memory. In addition, there is the necessarily related problem for any theory of memory of how the memory trace is unraveled—that is, what the relationship is between the engram and the memory itself. Such questions seem at least for this reader more easily approached in a memory theory in which the engram is not at the molecular level. In this context the arguments of Briggs and Kitto (*5*) suggesting cellular changes in learning through some process similar to enzyme induction seem quite valuable. It is obvious (*1, 5*) that RNA would have an important role in any such cellular process, although it would not serve as the memory trace itself. One basic advantage of such a theory—which can be conceived of as merely a biochemical mechanism for a morphological theory such as that of Hebb (*6*)—is that it does not further confuse the memory problem with what is obvious biochemical nonsense.

ALFRED L. GOLDBERG
Churchill College, Cambridge, England

References

1. W. Dingman and M. Sporn, *Science* **144**, 26 (1964).
2. J. Gaito, *Psychol. Rev.* **70**, 471 (1963).
3. J. McConnell, *New Scientist* **21**, 465 (1964).
4. H. Hyden, in *The Cell*, J. Brachet and A. E. Mirsky, Eds. (Academic Press, New York, 1960).
5. M. H. Briggs and G. B. Kitto, *Psychol. Rev.* **69**, 537 (1962). The position of these authors is similar to that expressed here.
6. D. O. Hebb, *The Organization of Behavior* (Wiley, New York, 1949).

Bigotry in Scientists

After several vacillations in my reaction to your editorial "Bigotry in science" (24 Apr., p. 371), I have settled upon being sad about it.

It is clear that every growing and maturing person is inevitably changed in view, thought, and response by the rigors of the life experience, but to attribute bigotry among scientists altogether to environmental exigencies during graduate studies is a depressing oversimplification. Since scientists are people, it seems much more likely that their capacity for bigotry is fixed long before they attain even undergraduate status. It therefore cannot really be astonishing that some scientists, like some butchers, bakers, or candlestick makers, are bigots. I can agree that to persist in narrow and uncompromising views is a debilitating waste on the part of highly trained and intellectual people who might otherwise contribute much to our society in areas outside their specialties. But it also may be that some scientists' inability to do so is another proof that they are merely human.

By the time a bigot has grown up to be an unhumble scientist, it is probably too late for salvage, although the kind of self-renewal forcefully espoused by John W. Gardner (*Self-Renewal: The Individual and the Innovative Society*, Harper and Row, 1963) offers a great deal of hope for palliation through insight. The only really effective method, however, is prevention, and this is the sad thing: that we cannot lead our children through the undergrowth of life experience into their places in an orderly, free, and responsible society without somehow passing on to them our own prejudices, fears, and bigotry. These hobbling traits are not acquired in graduate school; they are taught to us as children. Therefore, we must somehow contrive to change our ways at all levels of education, remembering that that process—like charity—begins at home.

JOHN E. JESSEPH
Brookhaven National Laboratories, Upton, Long Island, New York

Junior Scientists' Problem

I would like to call attention to a disconcerting trend in the sales policy of some of the major scientific and biological supply companies serving schools and colleges. Such companies more and more will serve institutions only, not individuals. In New York, for example, the major source of biological and chemical supplies of good quality, but in smaller quantities than are offered by the "professional" suppliers, will no longer meet the needs of the enthusiastic student or amateur. I suppose that it is much more profitable to confine one's business to large orders, but it is a pity that the needs of the young have to be sacrificed. Those young people with their "nuisance" orders of a dozen test tubes or two flasks are often enough the scientists of tomorrow. At least they will become those interested and informed laymen the community of scientists so urgently requires.

Happily, there are exceptions to this trend among suppliers, but I am beginning to wonder how long they will hold out.

JOSEPH A. MAZZEO
470 West 24 Street, New York 11

Rhythm Method and Mate Selection

In trying to meet my criticism (Letters, 6 Mar., p. 995) of the rhythm method of birth control as a method that selects for its own failure, R. C. Baumiller (24 Apr., p. 365) proposes a model that makes the criticism not less but more telling. He suggests that perhaps some day "intelligent, responsible and self-sacrificing" males will select as mates "only those women who have regular cycles." Even if we grant this somewhat implausible method of choosing a wife, we cannot agree with the author's conclusion that "thus selection may soon turn in favor of regular ovulatory cycles."

Baumiller is proposing a system in which mating is selective rather than at random. It is a basic principle of population genetic theory that selective mating alters the frequency of genotypes without altering gene frequencies. When gene frequencies are altered, it is because of differential fertility among the genotypes. What differentials would we expect to find among the genotypes in the Baumiller model?

Since he says that in the unions postulated "the natural method of conception control [would] become even more effective," we can only suppose that the productivity of couples composed of altruistic males and regular females would be below that of other couples in the population, whom (by inference) we are surely justified in identifying as composed of nonaltruistic males and irregular females. Thus natural selection would lead precisely to the conclusion suggested in my letter: the progressive elimination of regular women. The introduction of mate selection would merely insure a parallel rapid elimination of "intelligent, responsible and self-sacrificing males." Perhaps Baumiller would call this process "natural"—a word which, I note, he uses without quotation marks; but surely he would agree that it would produce a result not devoutly to be wish'd.

GARRETT HARDIN
Department of Genetics, University of California, Berkeley

Since only a finite amount of time is available for editorial chores, the amount of care budgeted to the editing of letters, articles, or reports submitted to *Science* should, in some measure, parallel the potential social impact of the content. More explicitly, scientific matters which pertain directly to the issues of peace and nuclear war; the "population explosion," birth control, and the future evolution of man; and automation, in so far as it may effect serious economic and cultural upheavals, should receive priority in editorial scrutiny. . . .

It was therefore with amazement and dismay that I read the letter of R. C. Baumiller pretending, in a jesting manner, to weaken the serious, lucid, and valid argument of Garrett Hardin's letter on the "Ultimate failure of rhythm" as a birth-control method for world population control. . . .

That the editors of *Science* . . . overlooked the semantic bobble of the interchange of two very different meanings of the term "selection"—the selection of a mate by an individual, as opposed to natural selection, which ultimately "selects" the members of future generations by virtue of their greater-than-average reproductive efficiency—is inexcusable.

LEONARD ORNSTEIN
Division of Cell Biology, Mount Sinai Hospital, New York 29

Credit Due

The College of the University of Chicago deserves, I believe, much more credit than it ever gets in such histories of curriculum reform as John Walsh's article (8 May, p. 642). Certainly this is the case in regard to mathematics. I remember my own surprise, as a young assistant professor at Purdue around 1945, when I became aware that Chicago was teaching to freshmen and, even worse, to high school juniors concepts and approaches in mathematics that I myself had had only as a graduate student. At the time, I was engaged in proposing such revolutionary ideas as that engineering freshmen could start their first course with quadratic equations. The result of Chicago's pioneering work was to make very many younger mathematicians intensely dissatisfied with what they were teaching.

I could list many names of workers in mathematical curriculum reform who were influenced by Chicago's work. I know that the college also had novel programs in the other sciences. Did they have a similar impact?

GAIL S. YOUNG
Tulane University, New Orleans

"Open" Reviews

I would like to present herewith some arguments in favor of open reviews of research proposals and of scientific and technical papers submitted for publica-