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