Letters

Retinal Flicker and Imprinting

A recent report by P. H. Gray [*Science* **132**, 1834 (1960)] contains the statement, "Moltz . . . has raised to the near status of law the conclusion that retinal flicker is an irreducible condition of imprinting." I have done nothing of the sort. Indeed, one entire section of my paper [*Psychol. Bull.* **57**, 291 (1960)] was devoted to showing that the imprinting response could be induced by stimuli other than those which produce retinal flicker. In several other sections as well I explicitly pointed out that any non-noxious stimulus (either visual or auditory) which dominates the sensory environment of the bird during an early period of development should subsequently evoke close following.

It appears that Gray's reference indicates either that he had examined my paper in a cursory manner and consequently had overlooked an important point or that he wished to construct a "straw man" which his experimental data could demolish.

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Early in his paper Moltz said: "Thus, imprinting will be defined as the procedure of visually presenting to an animal a large *moving* [italics mine] object during the first several hours of its life under conditions that insure that the object is not associated with such conventional reinforcing agents as food and water." He later departed from this definition to discuss auditory imprinting (irrelevant here) and the several statements by Menner (*1*) and James (*2*), the first stating the role of the pecten in enhancing the sensitivity of the avian retina to movement and the second concluding that retinal flicker was a critical factor in imprinting.

Since these were the exceptions Moltz made to the requirement of motion in his definition, and since he did not change the definition, it would be logical to think that he was taking a stand on this theoretical issue. According to his letter he was not. But contrary to the impression that could be gained from this letter, he did present his discussions in such a manner that if he *had* held retinal flicker to be the irreducible condition of imprinting, then nothing whatever in his paper would have required change. It would seem that he wrote the paper in such a manner that, if it were later incontestably proven that retinal flicker was necessary, it would not be possible to say Moltz had not championed the hypothesis.

This is a style of writing of which it is almost impossible to make a short and accurate statement on a contextual point. But I admit without reluctance that whatever my intentions, the peculiar wording of my statement about Moltz in my report did not come even close to what I should have said, which is essentially what I have said here. This was a failure in exposition for which I accept full censure. But it is the expository failure only which I admit; I do not find anything in Moltz's paper that can be interpreted in context as evidence that he foresaw the possibility of a positive preference for a motionless imprinting object not associated with a flicker-inducing light.

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References and Notes

1. I am not familiar with Menner's article; H. James or Moltz should be consulted for the reference.
2. H. James, *Can. J. Psychol.* **13**, 59 (1959).

Stratospheric Fallout

There are several aspects of the recent article on the transport of artificial radioactivity, by Martell and Drevinsky [*Science* **132**, 1523 (1960)], to which I take exception. I should like to discuss one of them.

Martell and Drevinsky contend that the fallout in Milford Haven, Wales (presumably typical of the North Temperate Zone), from temperate-latitude stratospheric atomic clouds from Soviet testing in 1955 is about 10 times greater than fallout from equatorial low-stratospheric atomic clouds from the U.S. Redwing tests and almost 60 times greater than that from the equatorial high-stratospheric atomic clouds from the U.S. Castle tests, when values are normalized to an equivalent source strength. This remarkable difference in fallout from clouds in different stratospheric zones forms one of the cornerstones in their interpretation of weapon-test fallout.

It is my view, as expressed in the 1957 *Congressional Fallout Hearings* (p. 156), that the sense of the difference in amount of fallout, relative to source strength, from temperate and from equatorial stratospheric injections is correct. However, I think that the numbers 10 and 60 should be reduced to something like 1.2 and 5 or less, respectively.

The bases for my differences with Martell and Drevinsky are as follows:

1) Martell and Drevinsky confuse fallout in different seasons, despite the seasonal trends in their own as well as in other data. Thus, they should correct upward by a factor of 2.5 their figure for fallout in August–December from the Redwing tests to make it comparable to the figure for fallout in February-June 1956 from Soviet tests in the fall of 1955; the factor 2.5 is based on their own finding for tungsten-185 fallout from the U.S. Hardtack equatorial atomic tests (see their Fig. 5). This latter test series was similar to the Redwing atomic tests.

2) If one limits oneself to the same period (February–June) for which the fallout from the Soviet tests of 1955 was computed, then the normalized value for fallout from the U.S. Castle atomic clouds is increased by a factor of 2.5 over the value given by Martell and Drevinsky. Further, if all the fallout during the period February–June 1955 is assigned to Castle, then the factor of increase becomes 3. Whether one attributes only part of the February–June 1955 fallout or all of it to the Castle tests depends on whether one assigns any of the fallout in this interval to the fall 1954 Soviet Temperate Zone tests on the basis of isotope-ratio data. Martell and Drevinsky assume that debris from the fall 1954 tests entered the stratosphere. But information sources with which I am familiar do not assign any radioactive debris from these tests to the stratosphere; thus, essentially all of the February–June 1955 fallout would be from Castle. The isotope data leading to the contrary viewpoint are confused by fallout data from the Nevada Teapot atomic tests in progress from February to May 1955.

3) The Department of Defense [A. K. Stebbins, *DASA No. 532B* (1960)] has estimated an amount of stratospheric fallout from the Castle tests less by a factor of about 2 than was assumed by Martell and Drevinsky. If the Department of Defense estimate is correct, this would increase the normalized value for fallout by an additional factor of 2. However, the uncertainty about residual stratospheric debris is such that the validity of this additional correction to the normalized value for fallout from the Castle tests must be questioned.

4) The fallout from the Soviet autumn 1955 test series in the period February–June 1956 selected by Martell and Drevinsky was only 25 percent or less of the total fallout for that period. This conclusion derives from arguments used by them, except that