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

only the November 1955 test of the series can be treated as a source of stratospheric fallout. None of the other three shots of the Soviet autumn 1955 tests were announced by the U.S. Atomic Energy Commission as having been large. Actually, Martell and Drevinsky state that about 50 percent of the fallout was from the Soviet 1955 testing (a figure derived, I presume, by using the mid-point for the entire September–November 1955 series), but they employ 100 percent of the fallout in their arithmetic. Thus, their normalized value for fallout from the Soviet 1955 tests should be reduced by a factor of 4. These differences are illustrated in Table 1.

Martell and Drevinsky gave a ratio of fallout from Soviet 1955 tests to fallout from Redwing as 10.0; my figure is 1.1. They give a ratio of 57 for the Soviet 1955 and the Castle tests; my figure is 4.8 (2.4?).

It should be noted that various periods up to 20 months were used in Martell and Drevinsky's computations while I used only the first February–June period after the test series, or corrected to this period in the case of Redwing.

The determination of fallout per unit source strength is subject to many uncertainties. I feel that one is on especially weak ground in making any such specific assignment of fallout to Redwing as Martell and Drevinsky suggest.

There are two additional test series not included in Table 1 which may be cited in considering the two sets of figures. They are the Soviet October 1958 series in the Arctic, whose fallout may behave like that of the Soviet 1955 test series, and the Hardtack test series, which, as indicated above, may be similar to the Redwing test series. The analysis is derived from Fig. 5 and Table 1 in the article by Martell and Drevinsky. The results, again limited to the first February–June period after the series, are as follows: Hardtack, 0.9 $\mu\mu$c/lit. per megaton; Soviet tests of October 1958, 1.2 to 1.5 $\mu\mu$c/lit. per megaton. The ratio of normalized values for fallout from arctic tests to values for fallout from equatorial tests is, then, 1.3 to 1.7, a value similar to my value of 1.1 for the ratio of fallout from Soviet 1955 tests to fallout from Redwing but much smaller than the value 10 given by Martell and Drevinsky.

I should point out that the differences in fallout relative to source strength from equatorial (U.S. Pacific) and temperate or arctic (U.S.S.R.) sources of stratospheric atomic clouds probably have more significance for the meteorology of stratospheric motions than for evaluating health hazards.

Table 1. Normalized values for fallout per unit source strength (in $\mu\mu$c of Sr^{90} per liter of rainwater at Milford Haven, Wales, per megaton).

Castle (equatorial, high altitude)	Redwing (equatorial, low altitude)	Soviet 1955 (temperate)
	Martell and Drevinsky	
0.07	0.4	4.0
	Machta	
0.22 (0.44?)	0.9	1.0

If Martell and Drevinsky concur in the foregoing analysis, I wonder if they would still claim that "fallout from equatorial tests [is] substantially less significant per test unit than fallout from tests in other latitudes," as they argue in the concluding sentence of their article.

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Machta has gone to great lengths in an attempt to show that we have overstated the difference in Milford Haven fallout per unit source strength for the 1955 Soviet tests as compared to the equatorial Castle and Redwing tests by an order of magnitude. In so doing, he has changed the basis of comparison and uses arguments of questionable merit. Since our own interpretation of the data is adequately discussed in our article, we will restrict our attention to Machta's main points.

Machta first raises the normalized fallout rates for the Castle and Redwing tests by a factor of 2.5, apparently in order to eliminate seasonal effects and to compare fallout rates at the time of the spring peak. This procedure appears to be pointless. Since stratospheric storage times, regardless of source location, are short as compared to the half-lives of Sr^{90} and Cs^{137}, the significance of relative fallout rates is associated primarily with the short-lived radioisotopes in fallout. For this reason, we chose to compare fallout rates at corresponding early times after each test, during periods for which isotope-ratio data allowed approximate assessment of origin.

If one wished to compare the relative Sr^{90} fallout rates from various sources in a given season, he should direct his attention to isotope-ratio and concentration data in that season at the point of interest. Machta's arbitrary use of seasonal factors to construct data for one season from observations in another is hardly an acceptable scientific procedure. Furthermore, his application of a seasonal factor observed for one location and test series to another location, other years, and other tests of radically different cloud height distribution involves an unreasonable degree of generalization from a limited set of observations. The same limitations apply to his drawing of a parallel between the October 1958 and the fall 1955 Soviet tests.

Machta next reduces the fallout contribution of the 1955 Soviet tests by a factor of 2 by assigning all of the contribution from that test series to the high-yield 23 November shot alone. Other shots of that series are reported to have been fired on 4 August, 24 September, and 10 November. In the absence of reliable and complete shot-yield and cloud-height data for that test series, we preferred to take the mid-point of the test period as the mean production date and accept an uncertainty which includes Machta's assignment as a lower limit. It should be pointed out that clouds from surface and air shots of about 100-kiloton yield rise well into the stratosphere at Soviet test latitudes [for example, see W. W. Kellogg, *U.S. Atomic Energy Comm. Rept. No. AECU 3403* (14 June 1956)]. Machta has yet to prove that the only spring 1956 fallout at Milford Haven from the 1955 Soviet test series came from the 23 November shot.

He further reduces the 1955 Soviet test contribution by an additional factor of 2 by using the exact fraction indicated by the Sr^{89}/Sr^{90} ratio. However, for the post-Castle period, he disregards the isotope-ratio data and assigns all Sr^{90} fallout to Castle. He invokes tropospheric fallout from the February–May 1955 Nevada Teapot tests to explain the Sr^{89}/Sr^{90} ratio data at Milford Haven for that period. Machta's interpretation affords no explanation of the Sr^{89}/Sr^{90} value of 21.9 for the period 11–31 January 1955. Machta apparently has missed the significance of our Ba^{140}/Sr^{90} results, which indicate the stratospheric origin of most short-lived radioisotopes in long-range fallout.

In view of the marked differences in assumptions and basis of comparison, to compare Machta's conclusions with our own is meaningless. Nothing of Machta's argument would provide an acceptable basis for altering our interpretation of the Milford Haven data beyond our stated uncertainties. In our interpretation of the isotope-ratio data, we have tried to be as objective as possible and have studiously avoided generalizing too broadly from one set of fallout observations to other tests, other years, and other sites.

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