

Average Evoked Responses and Learning

Horel and Vierck point out in their communication that the stimulus consequences of learned responses might produce the "intention" and "reinforcement" waves observed in our experiment. They note that the visual system is an input system and that inputs such as those from eye and head movements could become synchronized with the presented stimulus as learning proceeds and thus could evoke the observed responses in the cortex. They suggest as control for this contingency that the visual environment after stimulus presentation should be entirely uniform—either dark or homogeneously illuminated.

We must first admit that few experiments are perfect and that ours suffered from a number of weaknesses due to difficulties usually spoken of as "the state of the art." Having learned a good deal in performing the study, we are now engaged in replicating and extending our observations. However, despite these limitations we did take precautions to the best of our ability. Though absolute darkness or a completely uniform "Ganzfeld" is impractical in a situation where a monkey is to learn to press one of two panels,

we did train in an enclosed unlit box located in a room so dark that we had to use a shielded light to record our observations. The monkeys were observed through a slit in the enclosure wall behind them; only during stimulus display could their forms be readily distinguished. In addition, the two panels on which the stimulus was displayed consisted of a large square directly in front of the subject so that there would be as little differential movement as possible when either half of the panel was depressed.

Of course, there exists the possibility that a response is evoked in striate cortex by eye (and even head) movements and that these became sufficiently synchronous with learning to appear in our records. Responses with a latency of 30 msec as a result of eye movement in the alert monkey have been reported; however, these responses disappear in the dark (1). Yet these and other types of peripheral orienting responses cannot be completely ruled out. However, we have found that such peripheral responses gradually diminish in amplitude and frequency until they are practically or totally unmeasurable. In such circumstances, we have inferred that the orienting has become "neuralized" and restricted to the operation of a central mechanism. We believe

that we now have the tools to tap this central mechanism directly, although our technique is still far from perfected.

Perhaps more to the point is the fact that our "intention" and "reinforcement" waves are locked not to the stimulus but to the response. Thus, "the changes in visual input that is time-locked to the stimulus," referred to by Horel and Vierck, should have appeared in the stimulus-averaged record—and they did not. The importance of finding a process in the striate cortex that is sensitive to response-linked events should not be underestimated, regardless of whether the mechanism turns out to be central or a response-initiated peripheral stimulus. In either case, a mechanism exists within the primary sensory receiving systems for collating information about environment-initiated events with those that are response-dependent.

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Reference

1. M. Feldman and B. Cohen, *The Physiologist* 10, 168 (1967).
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Of course, there exists the possibility that a response is evoked in striate cortex by eye (and even head) movements and that these became sufficiently synchronous with learning to appear in our records. Responses with a latency of 30 msec as a result of eye movement in the alert monkey have been reported; however, these responses disappear in the dark (*1*). Yet these and other types of peripheral orienting responses cannot be completely ruled out. However, we have found that such peripheral responses gradually diminish in amplitude and frequency until they are practically or totally unmeasurable. In such circumstances, we have inferred that the orienting has become "neuralized" and restricted to the operation of a central mechanism. We believe

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RESULTS

Since sucking time data was not available on all Ss until the second day of life, the following analysis is based on the sucking time scores for all the kittens from the second through the forty-second day of life. An analysis of variance of these data for the two feeding groups over the 3 hr. of observation yielded a significant Day's effect ($F = 6.881$, $df = 40/320$, $p < .001$), indicating that the amount of time spent sucking decreased as each feeding group grew older.

In addition the Group \times Days interaction was significant ($F = 1.993$, $df = 40/320$, $p < .001$) although the overall Groups effect was not. This, in conjunction with an inspection of the data, indicates that for approximately the first 3 wk. the two feeding groups spent about the same amount of time sucking. In the last 3 wk., however, the dry-sucking group spent increasingly less time sucking relative to the milk-sucking group. Furthermore, this trend was similar in each of the 3 hr. of observation. As an indication of this trend the mean sucking time scores for the first hour of observation during each of the first 6 wk. of life is presented for the two rearing groups in Table 1.

TABLE 1
Mean Sucking Time (in minutes) For
First Hour of Observation

Week of Life	Rearing Groups	
	Dry	Milk
1	38.4	40.3
2	34.7	37.6
3	33.5	34.8
4	21.0	29.4
5	14.4	29.7
6	13.4	28.4

A significant Hours effect ($F = 20.80$, $df = 2/16$, $p < .001$) indicates that the kittens in both feeding groups sucked progressively less during the 3 hr. of observation. That this trend was more pronounced in the early part of the 6-wk. period is indicated by a significant Hours \times Days interaction ($F = 1.536$, $df = 80/640$, $p < .005$).

An analysis of the weights recorded for the kittens over the 6-wk. period indicated no significant differences between the milk-sucking and dry-sucking groups.

DISCUSSION

The fact that the dry-sucking group sucked about as much as the milk-sucking group in the first weeks after birth and continued to suck to some extent throughout the preweaning period is difficult to explain on the basis of the acquired drive hypothesis. Since the dry-sucking group never experienced milk in direct conjunction with sucking, an oral drive could not have been acquired on the basis of food reward. Thus, their sucking behavior could not be explained in terms of the acquired drive hypothesis though it would be compatible with the hypothesis of an unlearned oral drive.

On the other hand, the results indicated that food reward did have some effect on sucking behavior in that the milk-sucking group sucked more than the dry-sucking group in the latter part of the 6-wk. period. Since both groups were given an equal opportunity to suck in a relatively normal sucking situation, this differential behavior is difficult to explain on the basis of a strictly formulated unlearned oral drive hypothesis. If sucking is merely the expression of an unlearned oral drive, then both groups might have been expected to exhibit about the same amount of sucking throughout the 6-wk. period.

The maintenance of sucking behavior during infancy appears to be only partially determined by food reward and then only at certain times. Neither the unlearned, nor the acquired oral drive hypothesis alone appears sufficiently comprehensive to provide an adequate explanation of sucking behavior during infancy.

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