portions of these findings to criticize. Nowhere does he describe the "shortcomings" of the conditioning paradigm. I made clear in the article that a "demonstration that the cerebellum is required for adaptation or association is not unique" and that it is also "required for adaptation of the vestibulo-ocular reflex [VOR]." But, to our knowledge, our work is the first to show that the cerebellum is necessary for associative learning and memory. VOR adaptation and associative learning differ markedly in their behavioral properties.

"[W]hether these data prove that the learning process actually occurs within the cerebellum itself" is, of course, the $64 question. It has not yet been settled definitively for any form of learning or any brain structure in mammals, but our evidence is clearly supportive of this hypothesis.

Bloedel cites a paper by Gellman *et al.* (*3*) that is a comprehensive study of the responses of olivary neurons to contact and passive body deplacement in the awake cat. The inferior olive stimulation was an incidental part of the study reported in one paragraph without anatomical reconstructions of electrode locations. The strongest current used in that study was 60 uA. The lowest effective current we have found to elicit behavioral movements, with otherwise relatively similar stimulus parameters, is about 60 uA, in the rabbit (*4*) (The parameters *are* specified in the article.) Bloedel does not note one of our other findings, namely, that whatever movements are elicited by DAO stimulation, these exact movements are learned to a neutral tone CS. Stimulation outside the DAO, for example, in the reticular formation, also elicits movements, but these cannot be trained to neutral stimuli.

Our argument for the mossy fiber CS pathway cites the immediate transfer of the CR from pontine stimulation on one side to stimulation of the other side. Bloedel's argument—that [this finding] "may reflect the fact that both stimulus sites are activating pontine projections to both sides of the cerebellum" (because of the bilateral course of the pontine projections)—only has merit if the trace is formed in the cerebellum rather than in the pontine nuclei or in other brain stem structures possibly activated by the initial training electrode. If, as he suggests, the second electrode is stimulating exactly the same fibers stimulated by the first electrode, which seems unlikely, then the plasticity could be at the mossy fiber terminals, rather than beyond. But they are *in* the cerebellum. He ignores the other lines of evidence cited in the article for the mossy fibers being the essential CS pathway.

Bloedel's statement that the permanent memory trace cannot be in the cerebellar cortex "since ablation of this structure does not permanently abolish the conditioned response" is a non sequitur. Neither we nor anyone else has removed all cerebellar cortex. Our findings to date indicate that removal of H VI, Crus I, Crus II, and paramedian lobules does not permanently abolish the CR. But the trace could have multiple cortical representations including areas not yet removed (*5*).

Bloedel discounts the elegant studies by Ito and his associates of long-term depression with conjoint stimulation (of mossy or parallel and climbing fibers) in vivo and in the cerebellar explant and then describes his own in vivo studies on locomotion in high decerebrate animals, where he finds a short-term enhancement of Purkinje cell response after a climbing fiber discharge. This is another non sequitur. Bloedel's observations and paradigms concern locomotion and have nothing to do with adaptation or learning and memory. In both Ito's work and in our own, changes over time are looked at as a result of repeated stimulation, that is, the effects of *training*.

Bloedel argues that I do not fully address the extent to which the studies of Miles *et al.* (his reference 6) challenge the view that plasticity occurs in the cerebellum in VOR adaptation. The focus of my article was not on adaptation of the VOR, but the close parallel with classical conditioning of discrete behavioral responses—the fact that the cerebellum is essential for both—merits emphasis. Two major current views are that the locus of VOR plasticity is either the cerebellar cortex of the flocculus (Ito) or the medial vestibular nuclei. In terms of connections (monosynaptic inhibition from cerebellar cortex), flocculus target neurons are equivalent to deep cerebellar nuclei, and the parallels between the VOR and other motor systems suggest that the deep cerebellar nuclei may be the site of many kinds of motor learning. Thus, both views support my contention that the memory traces for discrete learned responses are in the cerebellum, either in cortex or interpositus nucleus or both.

In sum, the evidence from our laboratory and the related work from other laboratories, which includes electrophysiological recording, lesion behavior, microstimulation, infusion of pharmacological agents, and anatomical pathway tracing, is consistent with, and supportive of, the view that the memory traces for classical conditioning of discrete behavior responses learned associatively to neutral conditioned stimuli, with the use of aversive unconditioned stimuli, are stored in the cerebellum. Has this been proved to everyone's satisfaction? Of course not. Scientific truth is a matter of probability. We have now succeeded in identifying key aspects of the essential memory trace circuit (which includes the cerebellum) for this category of learning and memory beyond a reasonable doubt and are approaching the point when it will be possible to localize the essential memory traces themselves.

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Monoclonal Antibodies as Phylogenetic Labels

The report by Susan Hockfield (*1*) of the development of a monoclonal antibody, Rat-302, specific for a subset of cells in the granule cell layer of the vermis, paraflocculus, and flocculus of rat cerebellum, raises several points of interest. She notes that "in the flocculus and vermis, Purkinje cells project directly" to the vestibular nuclei, bypassing the deep cerebellar nuclei.

Vestibular input to this area is also partially direct. Nauta (*2*) describes the vestibular nuclei as projecting "to part of the vermis . . . and also to the flocculonodular lobe, the caudalmost part of the cerebellum." He notes that (in humans) some fibers of the vestibular nerve bypass the vestibular nuclei, constituting a direct cerebellar input, adding that "no other instance is known in which the cerebellum gets primary sensory input."

The cerebellar structures involved appear to be phylogenetically old. In *Amblystoma*, Herrick (*3*) defines "three of the primordia from which the mammalian cerebellar complex has been assembled."

These are, "1) the vestibulo-lateralis system in the auricles, primordia of the floccular part of the flocculonodular lobes; 2) the median body of the cerebellum which is ancestral to the larger part of the vermis and adjoining regions; and 3) the nucleus cerebelli, internal to the other two, and in intimate relations with both. . . ."

Herrick defines the auricle as "composed of tissue which is transitional between the body of the cerebellum and the acousticolateral area of the medulla oblongata. This auricle contains the primordia of the vestibular part of the cerebellar cortex. . . ."

Finally, the premammalian nucleus cerebelli appears, in mammals, to become "subdivided and incorporated within the cerebellar mass as the deep nuclei" (*3*).

That vestibular cortex of the mammalian cerebellum has the direct output and partially direct input lines just described may be an arrangement of some antiquity that has survived for adaptive reasons because it favors rapid computation, or rapid adjustment, of the body's position in space.

Apropos that point, Turkewitz and Kenny (*4*) note that in newborns "the sequence of functional onset—vestibular, cutaneous, olfactory, auditory and visual, is invariant across all species of birds and mammals thus far studied."

What Hockfield's study suggests is that monoclonal antibodies may in some instances be used to label neuronal subpopulations according to their phylogenetic age. Characterization of this type might be of particular interest in structures such as the dentate gyrus, in transitional areas such as entorhinalis, or in parts of association cortex defined by Graybiel (*5*) as "distal."

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Response: Fair states explicitly an implication of my observation that antigenic differences correlate with anatomical and functional differences among neurons in the rat cerebellum. The functional properties of neurons are presumably derived, like other cellular characteristics, through the selective evolutionary retention or loss of specific molecular species. One might then predict that the isolation of cell specific markers could reveal functional characteristics that reflect phylogenetically conserved properties.

While we have not yet tested Fair's predictions in the systems he describes, our results in another system (*1*) suggest a phylogenetic conservation of molecular expression to a functional class of neuron. Monoclonal antibody Cat-301 recognizes functionally defined neuronal subsets in the feline and primate dorsal lateral geniculate nucleus (LGN). The cellular organization of the LGN is quite different in cat and in monkey, yet the Cat-301 antigen is expressed by functionally equivalent neurons in both animals irrespective of their distribution or nearest neighbor relationships. These results indicate that some molecular properties of neurons may be phylogenetically conserved by functionally related neurons. The conservation of molecular traits could be useful in identifying homologous neuronal populations when differences by cytoarchitecture, physiology, or connections might obscure evolutionary relationships.

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Color Vision and the Retinex Theory

D. J. Ingle (*1*) presented evidence for color constancy and the Retinex theory of Land (*2*) in goldfish. From the results of one experiment, he concluded that fish can discriminate "a green paper whose spectral distribution of reflected light was identical to that of the gray paper." That is, papers of different spectral reflectances must look different even when illuminated so they reflect identical light spectra, a result impossible to explain by means of colorimetry. However, the methods in such Retinex experiments equate only the integrated light of spectral distributions, not the spectral distributions themselves, as Ingle stated. Hence virtually any detector other than the specific detector used for the match could still respond differentially to the "equated" papers.

On each side of the match is a projector passing light through a band-pass filter with transmission spectrum $\tau(\lambda)$. The projector sending light to a paper with chromatic reflectance $\rho_1(\lambda)$ is equipped with a tungsten light bulb at temperature T_1, and the projector sending light to a paper with another chromatic reflectance $\rho_2(\lambda)$ has a tungsten light bulb at temperature T_2. Given that the lighting and viewing geometries are the same for both projectors and for both reflectances, the lights entering the eye from the two papers have spectral power distributions

$$\begin{aligned}\phi_1(\lambda) &= h(\lambda,T_1)\tau(\lambda)\rho_1(\lambda)\\ \phi_2(\lambda) &= h(\lambda,T_2)\tau(\lambda)\rho_2(\lambda)\end{aligned} \qquad (1)$$

where $h(\lambda, T)$ is the spectral distribution of tungsten as a function of temperature.

A light meter with spectral sensitivity $S(\lambda)$ is then used to adjust T_1 so that when the projector illuminates $\rho_1(\lambda)$, the light meter registers a match to the integrated light received from $\rho_2(\lambda)$:

$$\int \phi_1(\lambda)S(\lambda)d\lambda = \int \phi_2(\lambda)S(\lambda)d\lambda \qquad (2)$$

An artifact arises from the fact that the filters $\tau(\lambda)$ have a spectral bandwidth (that is, they are not monochromatic). As a result, Eqs. 1 prove that the spectral distributions $\phi_1(\lambda)$ and $\phi_2(\lambda)$ reaching the eye must be different because (i) $\rho_1(\lambda)$ has a different spectral shape from that of $\rho_2(\lambda)$; and (ii) the illuminant spectrum $h(\lambda,T)$ depends on T, which is different for $\phi_1(\lambda)$ and for $\phi_2(\lambda)$.

The match of Eq. 2 therefore holds only

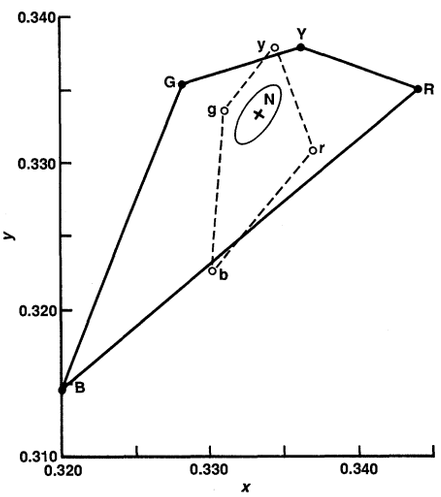

Fig. 1. Chromaticity coordinates for blue (b, B), green (g, G), yellow (y, Y), red (r, R), and neutral (N) test papers with identical triplets of light meter readings for 50-nm (solid line) and 10-nm (dashed line) bandwidth spectral filter Retinex illumination conditions. For the 50-nm filter condition (filled circles), the "equated" papers show a total variation about 22 times the area of the nearest MacAdam ellipse (*3*). [The nearest ellipse is at $(x, y)=(0.305, 0.323)$, but is centered at the neutral reflectance for easy visual comparison.] For the 10-nm condition (open circles), the variation is less than for the 50-nm condition, but it is still about eight times the area of the MacAdam ellipse. Any chromaticities outside the bounds of the MacAdam discrimination ellipse shown would be discriminable from neutral by a human.