ments to view the assemblage of artifacts. Arrangements should be made with Miss Simpson, Box 535, Yermo, California.

Toward the end of 1968 it is hoped to hold a symposium at which we will present the whole of our evidence to our colleagues. Those who support us will then be able to do so openly, while those who disagree can put forward their reasons for their opposite view.

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Cold Flour Beetle: Reminiscence or Change of Bias

Alloway and Routtenberg report (*1*) that if beetles (*Tenebrio molitor*) are cooled after learning, they perform less well on retest after 2 days during which they have been cooled than after 1, 3, 4, or 5 days of cooling. They interpret this in terms of an alteration in memory and postulate various physiological mechanisms. However, it is not clear that changes in memory are involved. Although the control experiment they report is ingenious, it fails to exclude important possibilities. To exclude the possibility that the decrement in performance they observed after 2 days was due to "nonspecific motivational factors," they required another group of beetles to learn the reverse of an initially learned habit after they had been cooled for various numbers of days. Their finding that reversal learning was faster after 2 days of cooling supports the idea that their original result was not due to some general disorientation or a lack of attractiveness of the reward. However, there are other nonspecific motivational factors which it does not exclude, and which might be responsible for their results. For instance, we may note that, before original training, "subjects were given a test for turning bias, which consisted of five runs through the maze. In original learning, training was against the subject's measured bias." Now if different periods of cooling produced systematic changes in the beetle's bias, then we should observe that there were changes in performance of a previously learned habit. If the initial bias had been to turn left, then the habit taught the beetle would have been a turn to the right. If a given treatment increased the bias toward left turning, then the beetle would appear to have forgotten the habit of turning right, but would, by the same token, more easily learn a reversal habit—a turn to the left.

At present, such an alternative interpretation in terms of bias remains open. Changes of bias as a result of treatment are a commonplace and cannot be dismissed a priori. Scrutiny of the data of Alloway and Routtenberg reveals discrepancies which need to be explained. The number of trials to learn a reversal when the beetle has allegedly suffered from "apparently total forgetting" is much smaller (something like 2.5 trials) as against 4.62 trials in relearning and 4.78 trials in original learning. This difference looks as large as the differences between difficulty of reversal on different days, reported as highly significant. This might be explained as a manifestation of the original bias. However, as the authors stress, choice of the correct alley on the first trial of retest after 2 days of cooling was precisely at chance, an indication that, if memory was completely lost, the original bias was also lost.

Can these two pieces of evidence be reconciled on the hypothesis that the beetle is suffering from an amnesia? A complete amnesia for the learned habit should lead to a reappearance of the original bias. Then we cannot account for the fact that "the best available index of retention" indicates that on the first trial of the second task 50 percent of the subjects make a response which was correct in original learning. This could only be explained by relinquishing the claim that "apparently total forgetting" took place. But suppose that forgetting was only partial and that the remaining memory compensated for the bias of the beetles. With such cancellation of two opposing tendencies, it is difficult to see why reversal should be so much faster on the 2nd day than original learning and why relearning should only be as fast as original learning when memory sufficient to overcome original turning bias was already there at the beginning of training. If partial memory of the original habit was present, we should expect the opposite of what is actually the case; that is, reversal learning would be expected to take longer than relearning of the original habit, as original bias has been counteracted by the remaining memory. Original learning was against the bias of the beetle; reversal was in the same direction. Any lessening of the effective bias by the partial memory should therefore speed up relearning of the original learning and retard reversal turning.

Experiments are needed to determine the biases of the beetle before cooling and then after cooling for various lengths of time. To dispel doubts, there should be an experiment in which beetles would be trained as in the report, but various numbers of uncooled days would be interposed between initial training and later cooling and retesting. If the effect is due to some phase of memory storage, such a procedure should be critically affected by the number of uncooled days between initial training and subsequent cooling and retesting. If the effect is simply one on biases, then a reasonable number of interposed uncooled days should not crucially affect the outcome of the experiment.

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Reference

1. T. M. Alloway and A. Routtenberg, *Science* **158**, 1066 (1967).

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The preceding interpretation of our results (*1*) rests on the assumption that "different periods of cooling produced systematic changes in the beetle's bias." Recent results obtained in our laboratory do not support this view. First, one group of ten animals was run for 5 successive days on a bias test consisting of ten trials. Between days, animals were maintained in the cold. Correlations between successive days revealed no significant relation between bias shown on any 2 successive days. Thus, the very concept of bias may be misleading since a preference for one arm of the T-maze shown on any 1 day was not reflected in a similar preference (or aversion) on the subsequent day.

A second experiment was performed to test directly the suggestion made by Deutsch. Thus, five groups each of ten animals were tested for bias on day 1. All animals were placed in the cold, and each group was retested for bias on one of days 2 through 6. According to Deutsch there should be a systematic change in the bias across days, with the highest correlation between original and subsequent bias being demonstrated on day 2. No such effect was observed. The correlations between original bias and biases on days 2 through 6, respectively, were -0.34, 0.10, 0.09, 0.18, and 0.02. None of these was statistically significant. Also, such changes in bias as did exist from original test to subsequent test did not differ across days in any systematic fashion ($F = 1.39$, d.f. $= 4.45$, $P > .25$).

It seems reasonable to conclude, therefore, that the procedures used do not give rise to any systematic alteration of the bias of the beetle. Although alternative interpretations of our results may still be possible, the available evidence suggests that analysis of the phenomenon in terms of physiological and biochemical memory mechanisms is justifiable.

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1. T. M. Alloway and A. Routtenberg, *Science* **158**, 1066 (1967).

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Submarine Trenches and Deformation

Scholl, von Huene, and Ridlon report three traverses of the Peru-Chile Trench (*1*), showing flat-lying sediments with no evidence of compressive folding. They interpret this finding in terms of a widely used model: trench formation representing down-buckling consequent to horizontal compression. The origin of the stresses is not usually specified in the literature, but such stresses would have to be transmitted from far away, arising ultimately at the submarine ridges.

This common interpretation is not, however, in agreement with all the facts. At the trenches one finds the largest deviations from sea level, as well as by far the largest (negative) gravity anomalies. These facts indicate that the forces that generate and maintain the trenches originate locally rather than by transmission from very far away. As is well known, the downflow of oceanic mantle (and presumably of oceanic crustal material) occurs along seismically marked fault lines with a downward tilt toward the continent of about 45 deg. A body force on the descending material directed downward arises from two effects: (i) this material is colder than ordinary mantle material at the same level; and (ii) at greater depth the basaltic component is removed, to be added ultimately to the root of the mountain range.

Roughly speaking, there will then be a vertical tensile stress above the descending material, which, by Poisson's effect, leads to a horizontal compressive strain. But, since the upper mantle is not free but may be compared to a plate clamped at infinity, the effect of a local compressive strain will be a corresponding tensile stress. Thus all stresses above the descending material are essentially tensile; there should be no warping of the overlying sediments. The existence of downward movements near trenches is hard to deny in view of the vast gravity anomalies observed, but, as the observations quoted indicate, the associated forces are primarily of local origin.

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Fundamental to the hypothesis of a spreading ocean floor is the generation of oceanic crust at major ridges and rises, and a corresponding engulfment of this crust and younger superjacent sediment in trenches flanking continental margins (*1*). At the spreading rates proposed (1 to 4 cm/year), underthrusting should profoundly deform the sedimentary section deposited within the trench, provided sedimentary units deposited at the base of a continental margin respond to stress in a manner similar to that of their counterparts on land (*2*).

In the absence of a compelling reason for invocation of a special deformational process for trench sediments, we reason that the lack of structures in the trench fill that even remotely suggest pushing, swallowing (engulfment), or tectonic accumulation of sediment places a dynamic as well as a geographic limit on the hypothesis of spreading of the ocean floor. The principal purpose of our report (*3*), therefore, was to emphasize the incompatibility of our observation in the Peru-Chile Trench with the generally accepted model of spreading (*1*). Our observation that compressionally undeformed sediment occurs at the base of the continental margin has since been extended to include most of the margin of the Pacific basin (*2*).

Our hope to stimulate discussion of the tectonic implications of these observations for the Pacific basin has been rewarded by Elsasser's comment. Many of the effects that he proposes from a theoretical standpoint have in fact been observed by us in the Peru-Chile Trench, and by others in the Japan Trench (*4*), the Aleutian Trench (*5*), and the Middle America Trench (*6*). In all these areas only extensional features (that is, normal faults) are seen in the trench fill; compressional structures are either too small to be detected by seismic methods or absent. The fundamental trench structure appears to be a down-dropped block that has been rotated about a seaward hinge line and faulted against the base of the continental slope. Various lines of evidence from the area of the Peru-Chile Trench (*7*) also tend to support Elsasser's opinion that the tensile forces maintaining the trench are generated locally.

Unfortunately we cannot supply additional observational data that would aid in provision of a meaningful unifying model, other than what we have already proposed (*3*), to account for the *apparent* local (and tensile) tectonic origin of this trench and the *presumed* horizontal motion of the sea floor toward it.

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5 April 1968