ate plus graduate-or-professional type of school). In the case of technical schools, the best translation would be "institute (of technology)." On the other hand, where we speak of a "high school," Europeans usually speak of a "middle" or "secondary" type of school—never "high."

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I wish to thank Leo Pap and others who have written correcting my misimpression concerning the government of Portugal.

With regard to his second point, I think it is worth while to point out that the connotation of the term *Hochschule* in German is not the literal translation to "high school." However, I assumed that most scientists realized that the use of this term in European science did imply a school of higher learning on the level of a university or college.

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Acetylcholine Metabolism and Behavior of Rats

Before Chow and John submitted their paper (*1*) for publication, they were kind enough to correspond with us at length about their findings. At that time we indicated to them our reasons for believing that their study did not afford an adequate test of our major hypothesis. In their published article no reference is made to the questions we raised, and since Chow and John interpret their data as contradictory to our major hypothesis, we would like to point out publicly why we believe that their experiment does not provide a test of our hypothesis.

In our original *Science* article (*2*) we suggested that a higher rate of cortical acetylcholine metabolism is related to a greater number of spatial responses in the Krech hypothesis apparatus. In our second *Science* article (*3*) it was made explicit that this referred only to the animal's initial problem-solving behavior. Pentobarbital sodium (which retards acetylcholine synthesis) was shown in that article to affect the animal's choices strongly if it was administered at the outset of maze experience; if it was given after four days of maze experience, the drug had little or no effect.

Chow and John gave their animals six days of maze experience. By that time the animals had adopted different response patterns. In subsequent testing Chow and John found that anticholinesterase drugs had little or no effect on the animals' choice behavior. They conclude, "The fact that such injections did not alter the hypotheses displayed by the animals in running a maze seems to indicate that hypothesis behavior is not dependent on cortical levels of acetylcholine." Actually, our results and theirs seem to be similar where they can be compared: When animals have had prior maze experience, drugs that affect acetylcholine metabolism do not appear to affect behavior. We urged Chow and John to test the effects of injections on behavior at the *outset* of maze experience, the condition under which we did obtain drug effects. Unfortunately this has not been done, so no comparison can be made under this critical condition.

Quite aside from this major point, there are a number of additional features about the report of Chow and John that make it difficult to evaluate their results.

1) Their Table 1 indicates that on a random reward schedule animals of the S1 strain made predominantly spatial choices and animals of the S3 strain made predominantly visual choices. Such a large strain difference in behavior—in the same direction but far larger than we have ever obtained—would appear to provide striking corroboration of our hypothesis, since we have shown the two strains to differ significantly in cortical cholinesterase activity. The data of the table cannot, however, be taken at face value. Correspondence revealed that over half the S1 and S3 rats were *trained* to give spatial or visual responses. There is no indication in the table as to which animals were trained to give specific response patterns and which adopted such response patterns spontaneously. Therefore it is impossible to evaluate the apparent strain differences, and we would prefer not to interpret these findings as supporting our hypothesis.

2) It has been our experience that when animals are transferred from a schedule that rewards one type of choice to a random-reward schedule, they tend to give up the previously rewarded choice rather rapidly. This did not occur in the Chow and John experiment. Their "no injection" results were obtained on the fifth and sixth days of a random-reward schedule, but many previously rewarded response patterns still persisted. This testifies to the strength of the prior training and indicates further why it would have been difficult to find a drug effect. It is not stated whether the animals were retrained between successive drug experiments (as many as five 6-day sequences were given to an animal). If response patterns persisted through such a long period, it is indeed interesting, and raises additional questions about the meaning of their data.

3) The legend of their table indicated that the number of times a rat was tested "varied from 4 to 30." No explanation is given as to why some rats were discarded after only four tests whereas their data up to that point were retained. Because of this feature of the experimental design, the subjects are repre-

sented very unequally in the reported data. With such a design, it is difficult to make valid comparisons among the experimental conditions.

4) Diisopropyl fluorophosphate (DFP), unlike eserine, is a persisting anticholinesterase agent; we have found the "half time" of the recovery of cholinesterase after DFP injection to be about 20 days. Therefore the animals that Chow and John tested under "saline" or "no injection" conditions during the six days after the injection of DFP would be poor controls since the effects of DFP would still be strong. There is no way of determining how many of the "control" data of Chow and John were actually obtained under the effects of DFP.

It is clear that our hypothesis relating acetylcholine metabolism and behavior has not yet been adequately confirmed (either by other workers or ourselves). On the other hand, it does not seem that the experiment of Chow and John has rendered it untenable.

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The disagreement which Rosenzweig, Krech, and Bennett expressed with the conclusions stated in our recent article (*1*) is based primarily upon the fact that we investigated the effects of intracerebral anticholinesterases upon previously established response patterns. They stated that their major hypothesis is that the rate of cortical acetylcholine metabolism is related to the mode of response in the initial problem-solving behavior of rats in their apparatus, but not to the performance of previously established response patterns. Although in various of their publications (*2*) they emphasized the role of acetylcholine levels in "adaptive" behavior in new situations, they did not make explicit the latter part of the hypothesis.

Yet, the data about the effects of pentobarbital sodium to which they referred at first seem compatible with that hypothesis and with our own results. We have difficulty, however, in reconciling the data reported by them on the effects of pentobarbital sodium with the explanations and hypotheses they offered. During the first four days, their drugged group (group II) displayed an extremely marked preponderance of light hypotheses, and a consistent mode of behavior was established. They attributed this effect to depression of cortical acetylcholine metabolism by the drug. They further reported that when the drug was discontinued, light hypotheses diminished, presumably as a consequence of restored acetylcholine levels.

To us, this seems to constitute a paradox: Although cortical acetylcholine levels are presumed not to affect the performance of established response patterns, a pattern established with a cortical acetylcholine level altered by a drug becomes modified when the acetylcholine level is restored after the drug is discontinued. Further, they reported that subsequent administration of the drug after the animals had been run for two days without drug again raised light-going choices. These various considerations, together with the complete failure of intracerebral eserine to alter hypothesis behavior in our experiments, seem incompatible with their major hypothesis.

With respect to the additional questions raised by our colleagues, two comments seem in order. No data were discarded, as suggested in their point 3. Animals were run so long as their physical condition permitted. Finally, while we agree that the effects of diisopropyl fluorophosphate (DFP) persist for some time, the results of DFP experiments were essentially comparable to results obtained with eserine, and certainly the failure of DFP to alter hypothesis behavior cannot be attributed to its subsequent prolonged effects on cholinesterase activity.

We have no explanation to offer for the interesting discrepancy between the effects of pentobarbital sodium before and after appreciable maze experience. Certainly other consequences of this drug besides altered acetylcholine synthesis are well known and might equally well be relevant to these effects, just as factors besides cholinesterase concentrations are relevant to the regulation of acetylcholine metabolism. In view of the inconsistencies outlined above, we doubt that effects on acetylcholine levels play an important role in the phenomenon. We agree that it would be of interest to observe the effects of anticholinesterase injections at the outset of maze experience. Since our current research activities lie in other areas, and since Rosenzweig, Krech, and Bennett are admirably qualified to pursue such problems, we look forward to the publication of further data which will clarify these issues.

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