that is, fitting the linear functional relation when the error variances of x and y are unknown. According to his method, the slope of the line is 1.74 with 95 percent confidence limits of 2.84 and 1.00 passing through $\bar{x}$ and $\bar{y}$; such a line is practically identical with the solid line (Fig. 2). In answering point 3 we would verify that the ^{3}H-leucine activity incorporated was proportional to the amount of protein.

Thus, the only way to compare specific activities of the protein from different samples is to use the correction for variation in ^{3}H-leucine concentration based on the linear relationship between (uncorrected) specific activity and ^{3}H-leucine concentration.

In Table 1, the sums of the squares are calculated about their respective group means. The errors given are standard errors of the mean.

Bowman and Harding object to the use of the single-sided t-test. We used this t-test because the specific activities of defined proteins of the hippocampal nerve cells are significantly higher in the experimental material than in the control. These data prompted the question whether one side of the hippocampus responded with a higher activity than the opposite side, in view of the fact that nerve cells in a control area of the sensorimotor cortex contralateral to the training paw respond on training with a synthesis of small amounts of DNA-like RNA (*1*, *8*). Furthermore, Booth (*9*) conducted experiments on the transfer of handedness in rats in which he gave intracerebral pulses of radioactive orotate at various times before and after an hour of training. Autoradiographs showed a marked concentration of the labeled component only in pyramidal neurons of the anterior motor cortex and in the hippocampus, with greater intensity on the side contralateral to the practicing paw. Booth concluded that (*9*) "these results support and extend the RNA base composition analysis." Therefore we questioned whether the cells of the hippocampus contralateral to the training paw would show higher values of the specific activity than those of the ipsalateral side. By using the single-sided t-test, we found a trend to lateralization of the highest degree of protein synthesis on the side contralateral to the training paw, a result which requires further elucidation.

Bowman and Harding consider stress as a factor which induces biochemical changes in nerve cells. We found an increase of nuclear RNA in the nerve cells of rats during a stress experiment (*10*). This response was considered to be unspecific because of the RNA composition. The newly synthesized RNA was not characterized by high proportions of adenine and uracil, as in the establishment of a new behavior. Neither did training with the preferred paw give a specific response. Similar controls on the RNA response of nerve cells have been reported for five additional cases (*11*). We have thus performed the necessary control experiments.

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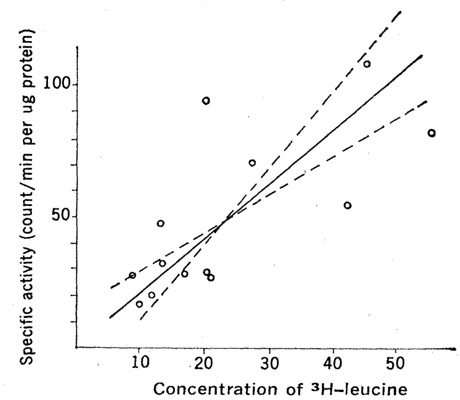
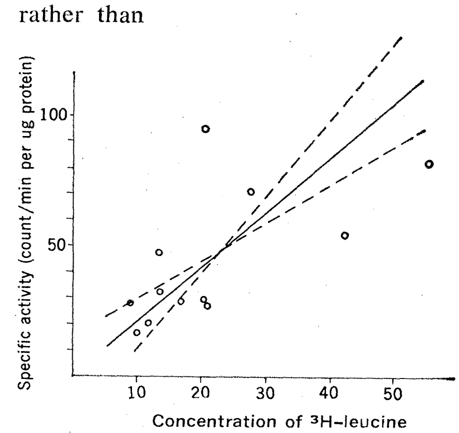
11 September 1968

Erratum: In the report "Protein synthesis in the hippocampal pyramidal cells of rats during a behavioral test" by H. Hydén and P. W. Lange [**159**, 1370 (1968)], Fig. 2 [Linear relation between specific activity of protein and ^{3}H-leucine concentration in the CA3 region of hippocampus. Regression lines dotted.] should have been



rather than



Stratigraphic Data and Length of the Synodic Month

Pannella, MacClintock, and Thompson (*1*) presented evidence of variations in length of synodical month based on studies of growth bandings in living and fossil organisms, and suggested that the slowing down of the earth's rotation has not taken place at a uniform rate. A curve drawn on nine points derived from organisms ranging in age from Late Cambrian to the present appears to show that the length of the synodical month decreased rapidly through the Paleozoic, held steady through the Mesozoic, and decreased rapidly through the Cenozoic to the present. We would like to call attention to (i) errors in the age assignments of specimens due to incorrect stratigraphic data; (ii) discrepancies in radiometric dates due to differences between published time scales; and (iii) weaknesses in the primary growth banding data due to biological and statistical considerations.

The stratigraphic errors detected by us involve the Eocene point, which is based on growth increment counts from two specimens, probably neither of which is from the Eocene. The upper Eocene is represented in the data by *Crassatella mississippiensis* Conrad, a poorly known bivalve species described 120 years ago from "Newer Eocene" deposits at Vicksburg, Mississippi (*2*). There is no Eocene exposed at Vicksburg. It is the type locality of the Vicksburg Group (Oligocene), however, and prior to the proposal and general acceptance of Beyrich's Oligocene, rocks of that age were often included in the

"Newer or Upper Eocene." Just where in the Vicksburg Group the specimen Pannella, MacClintock, and Thompson analyzed came from is not clear.

A specimen of *Cardita planicosta* (Lamarck), said to come from the Claiborne Formation at Bells Landing, Alabama, is used for a middle Eocene point. The Claiborne Group is indeed middle Eocene; however, none of its formations crop out at Bells Landing (*3, 4*). This is the type locality of the Bells Landing Member of the Tuscahoma Formation, which is latest Paleocene in age (*5*). If the locality data are assumed to be correct, the species they used is probably *Venericor aposmithii* (*3*).

For radiometric data, Pannella, MacClintock, and Thompson refer to Kulp (*6*) rather than to the more recent scale in Harland *et al.* (*7*). Use of the latter scale alters the ages of some of their points. For example, their only late Tertiary point is based on *Mercenaria campechiensis ochlockoneensis* (Mansfield), which they correctly dated as late Miocene but to which they assign a radiometric age of 18 million years. Assuming that their identification of the specimen is correct, this subspecies is known only from the *Cancellaria* zone of the northern Florida upper Miocene, which is considered to be youngest Miocene on the basis of its molluscan fauna (*8*). Harland *et al.* date the beginning of the Pliocene at 7 million years ago and the beginning of the late Miocene at 12 million years ago. Therefore, the Miocene point of Pannella, MacClintock, and Thompson would have been better plotted at about 8 rather than 18 million years ago. Also, by reference to the Harland scale, the *Crassatella mississippiensis*, which we suggest is Oligocene rather than late Eocene, would probably be nearer to 31 than 40 million years ago. The specimen called *Cardita planicosta* (Lamarck), probably late Paleocene rather than middle Eocene, would be dated at about 55 million years ago.

Biological and statistical weaknesses are indicated by the tendency of Pannella *et al.* to use single specimens to represent synodical-month patterns [6 out of 11 time intervals in their Table 1 (*1*)]. If poor preservation and ambiguity of growth patterns make the counts of growth increments highly subjective, as they state, it would be essential to derive magnitude of the synodical month from a statistical sample of a fossil population rather than from an individual specimen.

Replotting the increments per month at the corrected absolute ages changes the curve for the Tertiary somewhat. Instead of a more or less steady decrease from the Maestrichtian (Fox Hills Sandstone, which is probably better plotted at about 70 rather than 72 million years ago) to the present, there is no significant change in the length of the synodical month from the Maestrichtian to the late Paleocene, but then there is a rapid decrease from the late Paleocene to the present.

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11 December 1968

Hazel and Waller checked the information which we should have verified with some of our museum specimens. The fact that they did not need to look at the specimens to discover the taxonomic errors shows how patent these are. Concerned as we were with the chronological information recorded in the shells we neglected taxonomic questions.

We selected the data from the safest counts, and, if these happened to be from one specimen only, we accepted them as preliminary figures and weighted them accordingly for mathematical analysis. The use of a single specimen was unfortunate, but, when chronological data are sought, the suggested analysis of a population of probably contemporaneous specimens is not more sound from a statistic viewpoint. One would risk repeated counts of the same time interval and might think that he is gathering a random sample. In theory the best paleontological clocks would be continuous suites of not entirely contemporaneous but slightly overlapping individuals that reach the age of Methuselah.

We ran the program again using the same polynomial formula (*1*) and another program to obtain the chi-square statistic in order to determine the statistical significance of the age corrections suggested by Hazel and Waller. The best-fitting curves were, again, the polynomial of order 4; the chi-square showed statistically no better fit with the corrected data than with the uncorrected data. Since the magnitude of the suggested changes is small in relation to the uncertainties associated with each point, the fit of the data is not significantly affected. When we lump together counts from specimens coming from lower and upper parts of an entire period, as we have for the Pennsylvanian figure, the corrections are not critical, and it is premature to conclude that the events which brought about the change in slope occurred later than the speculated time in our paper. However, as more data become available (we are concentrating on the Mesozoic-Cenozoic interval) the suggested corrections will probably become statistically important.

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Bird Feathers and Radiation

Working independently, I (*1*) reported the results of experiments paralleling the feather-reflectance measurements made by Lustick (*2*) on white and dyed zebra finches. We were both led to similar conclusions by our results—an increase in surface feather temperature due to absorbed radiation leads to a decreased thermal gradient from the skin to the feather surface.

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