most subjects were unaware of a conflict in these experiments.

Although about one subject in five did become aware of the conflict, it is remarkable that there were so few. There was a tendency for these subjects to resolve the conflict more in the direction of touch than naive subjects do. Reporting a conflict does not necessarily indicate a spontaneous registration of contradictory perceptions. The subject may have been suspicious of the apparatus or (and this occasionally did occur despite efforts to prevent it) looked away or up, giving him an uncontaminated tactual impression so that he became aware of the conflict. In any event, the answer to our starting question as to whether a unified impression would be experienced in the conflict condition is a qualified "yes". Generally the subject is unaware of the conflict, which means he does have a unified impression. Further, that impression is dominated by what he sees. It seems clear that it is possible to study this type of sensory conflict experimentally and this is an important first step.

At this stage of the work no attempt has been made to differentiate the various aspects of touch perception, that is, whether based on tactile or kinesthetic sensations, passive or active components, simultaneous or temporal integration, and the like. We are using the term "touch" in the broadest possible way to stand for any and all aspects of sensory experience based on the mechanical contact of the observer with objects which can yield information as to the properties of such objects (*1*).

The results have implications for theories of the genesis of visual perception (for example, the visual shape derives from tactual shape) and for theories concerning spatiality in general. Bishop Berkeley had said, "Visible figures are the marks of tangible figures; and . . . it is plain that in themselves they are little regarded or upon any score than for their connection with tangible figures, which by nature they are ordained to signify" (*2*). Our results point in a very different direction.

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Radiocarbon Dating: A Case against the Proposed Link between River Mollusks and Soil Humus

In a recent report, Keith and Anderson (*1*) conclude that "Modern mollusk shells from rivers can have anomalous radiocarbon ages mainly owing to incorporation of inactive (C^{14}-deficient) carbon from humus probably through the food web, as well as by the pathway of carbon dioxide from humus decay." A necessary consequence of their hypothesis is that appreciable oxidation of the humus takes place within the water body rather than in the soils of the drainage basin. Although the possibility that this hypothesis is true cannot be completely eliminated, a far more reasonable hypothesis exists which explains their observations equally well. Their hypothesis cannot be verified by isotopic data alone.

Table 1. Approximate isotopic composition of carbon from various sources.

Source	δC^{13} (per mil)	C^{14}/C^{14}_{atm} *
Atmospheric CO_2	0†	1.0
Soil CO_2	−25	1.0
Soil humus	−25	0.6‡
Limestone	0	0.0
Typical river	−12	0.8
Typical lake	−5	0.9
Surface ocean	0	1.0

* Ratio of C^{14} in source to C^{14} in atmosphere. † Composition of dissolved CO_2 at equilibrium with atmospheric CO_2. ‡ Equivalent to an age of 4100 years.

The observations upon which Keith and Anderson's conclusions are based are (i) a correlation exists between C^{14} and C^{13} deficiencies (relative to the surface ocean) in the carbon of shells formed in terrestrial waters; and (ii) river mollusks show greater deficiencies than those from lakes. They explain the first observation by assuming that both the C^{13} and C^{14} deficiencies in mollusks formed in terrestrial waters are largely the result of humus oxidation. The greater the contribution of CO_2 derived from humus (compared with the contribution of atmospheric CO_2) the greater the deficiency of both isotopes. The second observation is explained by postulating a lower concentration of humus in lake water relative to river water as the result of settling. Oxidation of humus would then, presumably, contribute a smaller fraction of carbon to lake mollusks than to river mollusks. The isotopic composition of the various types of carbon referred to here are summarized in Table 1.

Table 2. Percentage of carbon from various sources required to give the concentrations of C^{13} and C^{14} found in the shells of typical river mollusks and typical lake mollusks. A, According to the Keith-Anderson hypothesis (extreme case, no contribution from limestone); B, according to the generally accepted hypothesis.

Source	River		Lake	
	A	B	A	B
Atmospheric CO_2	50	30	80	70
Soil CO_2		50		20
Soil humus	50		20	
Limestone		20		10

In my estimation, the generally accepted explanation (*2*) that the C^{13} deficiency should be attributed to the uptake of soil CO_2 by ground water (oxidation of humus in the soil, rather than after it has been transported into streams) and the C^{14} deficiency to the solution of limestone is far more plausible. In this case both the apparent correlation between the C^{14} and C^{13} deficiencies and the difference between rivers and lakes would be attributed to exchange with atmospheric CO_2 (relatively rich in both C^{14} and C^{13}). A water initially deficient in both C^{13} and C^{14} would gradually lose both deficiencies as it exchanged its carbon with the atmosphere. For lakes the exchange would, as shown by Broecker and Walton (*3*), be more extensive than for rivers, and on the average, lakes might be expected to show smaller deficiencies in both isotopes.

Quantitatively, the explanation given here is far more acceptable. Table 2 shows the ingredients necessary to make a typical lake and river shell by each hypothesis. As indicated in Table 1, soil CO_2 has a C^{14} concentration approximately equal to that in the atmosphere rather than that in the humus of the soil itself. By using bomb-produced C^{14} as a tracer, it has been shown that most soil CO_2 originates from the decay of the previous year's vegetation (*4, 5*). It should also be noted that whereas humus of about 4000 years in age is needed to yield the observed compositions of river mollusk shells according to Keith and Anderson's hypothesis, existing radiocarbon measurements suggest that humus in living soils is rarely older than 3000 years and averages closer to 500 years (*5*, *6*). Hence, if the Keith and Anderson hypothesis is valid, the humus they call on must come predominantly from fossil rather than living soils.

Several additional points are pertinent.

1) Since lakes receive the majority of their salts from the rivers that feed them, were it not for alteration through exchange with atmospheric CO_2, the carbon isotope composition of lake carbon would be expected to be similar to that of the supply river.

2) That a major portion of the dissolved carbon in river water is derived from the oxidation of humus in the water, after it has been transported from soils, represents a strong departure from the usual thinking on the subject.

3) From the chemical composition of many terrestrial waters, solution of carbonate rocks can be conclusively demonstrated to be a major source of dissolved carbon and an entirely adequate source of the observed C^{14} deficiency (*3*).

4) Incorporation of metabolic CO_2, derived from oxidation of the humus by the mollusk, cannot be called on as a means to enhance the humus contribution to the shells of these organisms over that of the dissolved CO_2 in the water, because subaqueous plants coexisting with the shells show the same C^{14} anomaly (*3*).

In conclusion, any relationship between the radiocarbon concentrations in soil humus and fresh water mollusks is almost certainly coincidental.

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X-rays: Are There Cyclic Variations in Radiosensitivity?

It is encouraging to see the interesting work of Pizzarello and his co-workers (*1*) being pursued with another organism. However, there are several gaps in the information given us by Rugh and his collaborators (*2*) which make it difficult to evaluate their results. Most important, perhaps, is that the environmental conditions are not adequately specified.

"Standard laboratory conditions" are likely to vary considerably from one laboratory to the next, and one would like to know the temperature regime and exactly when the lights went on and off. In addition, it is of extreme importance to know how long the animals had been kept under these conditions before irradiation. When transferred to a new light regime, mammals often take many weeks to become fully entrained to the new cycle. If, for instance, a large colony of mice were transferred from constant light to a light-dark cycle, one would expect that even the running activity of the colony as a whole would be conspicuously arrhythmic for at least 8 to 10 days or until the majority of animals became entrained to the cycle. If they were irradiated during those 8 to 10 days one might expect results very like those obtained by Rugh *et al.*, even though a further experiment in which the animals were left in the light cycle for 2 or 3 weeks might show a pronounced cyclic variation in radiosensitivity.

As Halberg has elegantly demonstrated (*3*), there may be rhythms of susceptibility to different drugs in the same organism (mice) which have very different phase relationships to the external light cycle. It would be naive, in the light of this work, to assume that a rhythm of radiosensitivity in mice would have the same relation to the light-dark cycle as one in rats. By choosing 9 A.M. and 9 P.M., Rugh *et al.* could conceivably have picked two points of equal value on a curve of large amplitude. Irradiating at 6-hour intervals as in Rugh's Fig. 2 would, of course, define a sine wave, but a sharp peak of sensitivity might easily be missed. It is therefore most interesting that the strongest indications of a rhythm of radiosensitivity in Rugh's study appear in the data from irradiations at 6-hour intervals.

Although there is not sufficient space to do so fully here, the same general kind of discussion might profitably be applied to the report of Straube (*4*) on the same topic. The 3-hour difference in photoperiod is only one of the four differences in protocol between his experiments and those of Pizzarello *et al.* listed by Straube. If rats do have rhythms of radiosensitivity, then surely, as in other rhythms of sensitivity described in the literature, the variation is not a simple matter of day versus night, but follows a curve of some particular shape. Of course at present the amplitude of the peak(s) of this hypothetical curve is unknown, as is its phase relative to an external light cycle. There are published data (*5*) showing that the phase of at least one animal rhythm is strongly dependent on photoperiod. There exists therefore the possibility that by using a different photoperiod Straube has shifted the phase of his rats' sensitive period (relative to that of Pizzarello's rats) and then missed this sensitive period by irradiating at the same times of day.

Finally, it seems completely unwarranted to criticize Pizzarello *et al.* on statistical grounds, as do Rugh and his collaborators. The sample size (40 animals, 10 each for 4 separate experiments) is not large but the data are so clearly significant by any statistical test one could rationally apply that the authors quite properly refrain from bothering the reader with such tests.

It is of course important that the work of Pizzarello *et al.* be repeated by other investigators and that, if the effect is confirmed, its generality be examined with due regard to its possible importance in human diagnosis and therapy. However, if rhythms of radiosensitivity exist in organisms, they probably have much in common with other physiological rhythms and, if one is to look for and study them, one must be aware of the techniques and precautions in general use in this field.

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