of these issues will be found in G. H. Beaven, E. A. Johnson, H. A. Willis, and R. G. J. Miller, *Molecular Spectroscopy* (Heywood, London, 1961), pages 13–15, and in A. E. Gillam and E. S. Stern, *Electronic Absorption Spectroscopy in Organic Chemistry* (Arnold, London, 1958), page 14.

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Skeptic

E. G. Sherburne, Jr., in editorializing (17 Sept., p. 1329) on TV coverage of the Gemini program, expresses confidence in the television industry as a competitive enterprise. He expects that TV coverage of this area of technology and science will improve because "the networks which excel in their scientific homework [and hence, presumably, in their performance] will excel in the marketplace."

This is a rather remarkable conclusion for someone to reach—unless, of course, he spends little time watching commercial television.

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Mass Extinctions of Mesozoic Biota

My brief summary of speculation on the subject of mass extinctions of Mesozoic biota (25 June, p. 1696) was published largely with the hope of evoking critical evidence (pro and con) before pursuing further any hypothesis that may prove too improbable. The vulnerability or the needed documentation of several points was, I hoped, made evident. My brevity, however, may account for some misunderstanding indicated by comments in a letter by Newell (27 Aug., p. 922) and in personal communications from others. Points considered "vulnerable" by Newell seem to require additional comment for their more adequate consideration.

Newell's evaluation of the supply of nutrients by run-off from the land to the oceans considers only the annual contribution. That this is almost negligible as compared with the upwelling nutrients from the ocean reservoir seems well recognized in my statement, "The volume of nutrients in the depths of the vast oceanic reservoir might appear nearly inexhaustible to the biologist, but it appears that the supply of nutrients from the ultimate source on land decreased over some millions of years." Considerable (if inadequate or unconvincing) support for this statement formed a large part of my paper. Criticism of this would seem pertinent, rather than of what would have been an obvious inadequacy if Newell's point had not been recognized or had been questioned.

Important, although still inadequate, data from geochemists on the residence-time of inorganic elements in the oceans are now well known and were not reviewed in my brief paper. Such data are even less satisfactory, however, on organic constituents among the nutrients. The nutrient requirements of various groups of microplankton under diverse conditions involve many complications, but both the organic and inorganic substances must have the land surface as their principal original source. Hutchinson, in a paper in the just published *The Scientific Endeavor* (Rockefeller Inst. Press), makes the interesting statement regarding the open oceans that "it is possible that iron, which is almost insoluble under oxidizing conditions in inorganic aqueous systems, usually limits the amount of living matter. . . ." This may prove especially significant under my suggested rather long-term conditions in the oceans.

However, even those nutrients that are most effectively recycled through upwelling and other ocean currents are partly lost to the bottom sediment in the process—especially to the relatively rapidly accumulated hemipelagic and nearer shore bottom sediments. Subnormal replenishment of the reservoir involving a geologic time of some millions of years seems expectable from the indicated conditions on land of that time, and thus any of many critical substances needed by phytoplankton could have become inadequate.

The importance of ocean currents, and especially of upwelling, is so well known that it could hardly have been overlooked by one associated with an oceanographic institution, but some evidence suggested that their intensity and effectiveness may have decreased under the conditions of that time. Indeed, it was this result from the warmer, more uniform, and perhaps thicker, surface water—making the conditions somewhat more comparable to those of laboratory cultures—coinciding with a then "deficient diet" in the deep ocean reservoir, that might best account for the worldwide destruction among marine populations.

In a personal communication Roger Revelle has commented that the long-term and widespread stabilizing effect of more marked stratification, deterring upwelling currents, might have been a more important and immediate factor than an impoverished reservoir in profoundly affecting marine life. This may well be, although without the additional factor of a considerably depleted reservoir it would seem to me probable that some large regions would have had sufficient current movements for adequate nutrient supply. Under the latter conditions alone, a continued or perhaps increased "geographic speciation" might be more expected than the wholesale and worldwide extinctions of so many previously thriving populations that are recorded. In any case, the relative importance of the two factors (and other, perhaps related, ones) seems more difficult to test and evaluate than whether or not a partially depleted ocean reservoir could have been a significant factor in the event.

There seems little question on the less pronounced or abrupt effects upon land plants at that time, and I will not here attempt additional discussion of the land animals. Perhaps these land animals indicate more profound and abrupt destruction of many thriving populations than my limited information would indicate. Certainly there were important evolutionary changes during that time, and perhaps one of the "explosive evolutionary periods." Newell is in a position to obtain more complete information on this than am I, and its presentation would permit a better consideration of whether or not abrupt *extinctions* on land were comparable to those in the open oceans.

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Extrasensory Induction of Brain Waves

Duane and Behrendt believe they have demonstrated "extrasensory electroencephalographic induction between identical twins" (15 Oct., p. 367). If

they have indeed established that alpha rhythm can be made to appear in one twin as a result of evoking it in the other, this finding is surely the most profound scientific discovery of the present century. Such coupling from one brain to another over a distance of 6 meters would constitute as great a mystery for physics as for biology or psychology. The authors do not appear to appreciate the revolutionary implications of their results. Otherwise, they would certainly not have failed to present their data in such a way that the reader could evaluate them. The authors have not supplied the following necessary information:

1) How many non-twin pairs were studied?

2) How long a time sample was obtained from each pair of subjects?

3) How many elicitations of alpha were performed with each pair of subjects?

4) What proportion of those elicitations displayed the "induction" effect?

5) What proportion of the time did alpha spontaneously occur?

6) The authors report that the records were analyzed by gross inspection. Were those doing the inspection aware or unaware of whether or not the records were obtained from twins or nontwins? Were they aware of the points in the record at which one of the twins was instructed to close his eyes?

7) The authors say the tests were repeated on "several different occasions." How many replications is "several," and how many opportunities were provided for the effect to show itself or fail to appear?

In reading *Science* one comes to expect a standard of reporting far higher than this in matters of much less fundamental importance. It is paradoxical that this report should have been published completely unsupported by any of the usual experimental safeguards.

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. . . A great variety of factors influence the appearance of the alpha rhythm, and there is a very real possibility of contamination by one or more of these. While alpha itself is not under voluntary control, some of these factors are, including the one—eye closure—that the authors used to induce it. And, as the parapsychologists Rhine and Pratt (*1*) put it, "If a test [of ESP] is to be at all crucial, there is no excuse for using conditions that leave the question of sensory cues as one to be answered by judgment or interpretation." The report is almost devoid of the procedural detail that is essential to an adequate judgment of whether or not such sensory cues were, in fact, excluded. For instance, the first twin supposedly was instructed from time to time to close his eyes. What instructions were issued to the other twin, beyond being asked to sit quietly and keep his eyes open? Was he given any kind of warning signal that a trial was about to start? Where was the recording apparatus? Any auditory signal or any distraction from the visual "task" of keeping the eyes open can bring on alpha (*2*). On the other hand, was the second twin allowed to sit for a long time without any further instruction? If he did so without any "anxiety or apprehension," as the authors believe, then the danger arises of boredom developing; and boredom is known to bring on alpha (*2*). Were eye closures of both twins monitored? Since eye closure induces alpha, it is essential that we know whether the second twin's eyes were, in fact, open at the time the first twin was closing his; the records reproduced in the article show only the eye-closure record for one subject, the sender. It would also be helpful to know whether the two successful pairs, who "happened to possess a prior knowledge of biological sciences and were relatively unconcerned about the tests," were among the subjects whom the authors say they knew. Just how much did the successful subjects know about the purposes of the experiment?

The report also suffers grievously from a lack of firm data. . . . Two electroencephalographic records are offered as proof of the principal conclusions of the paper. One shows simultaneous alpha rhythm in both twins when only one was supposed to have his eyes closed. Since no sample of the prestimulation EEG is given, we are at a loss to interpret the poststimulation records. The fact that the presumed monozygotic twins gave highly similar records is, in itself, not remarkable, since it has been known for a long time that the EEG records of identical twins are indistinguishable from each other (*3*). The proper control for the phenomenon the authors wish their figure to show would be the demonstration of a lack of alpha in the second twin when the first twin

has not closed his eyes. Instead, we are given an irrelevant (in this context) record showing that the first twin's eye closure did not influence alpha in an unrelated subject. . . .

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1. J. B. Rhine and J. G. Pratt, *Parapsychology: Frontier Science of the Mind* (Thomas, Springfield, Ill., 1957), p. 32.
2. I. Oswald, *Sleeping and Waking* (Elsevier, Amsterdam, 1962), pp. 72–73.
3. H. Davis and P. A. Davis, *Arch. Neurol. Psychiat.* **36**, 1214 (1936).

The report of Duane and Behrendt . . . has so heated the mail to my usually quiet ivory tower that I now need insurance. One nonparascientist even asked: "Ought I not to resign from the AAAS?" Should the editors have accepted this paper? The pro answer is: Galileo. Science is hindered when the Establishment undertakes censureship. The contra answer is: Space is too precious nowadays to allow for the printing of raw data, and these data are raw, for they state merely an empirical relation, an empty correlation, that lies out of further relation to any understood body of scientific fact. Besides, there is a literature which these authors do not cite and seem not to know. It seems clear that Soal's marvelous Welsh schoolboys connived by what now seems clearly to have been trickery to fool many important investigators [S. G. Soal and H. T. Bowden, *The Mind Readers* (London, Faber and Faber, 1959)]. Those boys were in separate rooms. How well shielded were the twins of Duane and Behrendt from each other? Did the recipient twin have his eyes continuously open or continuously closed? Could he have known when the sending twin was asked to close his eyes? Identical twins are accustomed to cooperate, and these twins were the only ones who knew the biology of what was going on. Anyhow, the major difficulty is that these twins (two out of 15 pairs) presented the experimenters with a correlation that they could not explain. So it has always been. The parascientist (as does his complement) pits his ingenuity against the inscrutability of nature, and when the parascientist fails he has succeeded, for he has discovered the inexplicable!

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Evolution in Tennessee

In his letter headed "After Scopes" (22 Oct., p. 435), Thomas A. Cowan remarks, "Apparently no one else in the State of Tennessee has cared since the trial to challenge the constitutionality of the [anti-evolution statute]." No one, to my knowledge, has brought the matter to the state Supreme Court, but there has been concern about the issue. Around 1960, Arlo I. Smith led a group of professors, business leaders, and clergymen in appealing to the Tennessee legislature to remove this unfortunate law from the statute books. The legislative committee concerned refused to act, and consequently the matter was never brought to the floor of the legislature. Yet those of us who are native Tennesseans may still hope that the Renaissance will some day reach Tennessee.

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Cowan's letter implies that the theory of evolution is not being taught in state-supported schools in Tennessee. This is not true. At the University of Tennessee this subject is included in general zoology and botany courses, and courses on evolution and speciation are being taught at the upper-division and graduate level; this has been true for many years. I have made no survey to determine the extent to which evolution is taught within the state, but I know that the subject is included in courses in other state-supported universities and colleges and many of the textbooks used in high school biology courses.

One reason the statute has not been repealed is that it has so commonly been disregarded.

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Help Sought with History

I am writing a history of the Huxley family, which is to be published in the United States and Britain next year, and would be grateful to hear from anyone who has recollections or reminiscences of interest.

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