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.

GEORGE WALD
Biological Laboratories,
Harvard University,
Cambridge, Massachusetts

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.

LESTER GOLDSTEIN
1424 Bellaire Street,
Denver, Colorado

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.

M. N. BRAMLETTE
Scripps Institution of Oceanography,
La Jolla, California

Extrasensory Induction of Brain Waves

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