7. There is no necessity for further discussion of the subject, but I wish to cite D. D. Jackson's comments in *Science* [134, 1910 (1961)] with reference to another article: "Indeed, Passamanick . . . has recently suggested that his studies show the human brain to be so highly adaptable that the lower end of the I.Q. scale is related not to heredity but to opportunity—including the circumstances of birth and early rearing."
8. M. Nyiszli, *Medecin à Auschwitz* (Julliard, Paris, 1961).
9. American Association of Anthropologists, *Fellow Newsletter* 3, 1 (Jan. 1962).

The Search for Other Civilizations

The article by Sebastian von Hoerner, "The search for signals from other civilizations" [*Science* **134**, 1839 (1961)], contains at least one statement that seems to be so out of line with observed facts that it should not go unchallenged. This is the statement that the ancient Greeks could have found the distances between neighboring stars to within 5 percent.

Let us accept, for the moment, von Hoerner's statement that by assuming the earth to be an "average" planet—that is, comparable to the average for the five planets visible to the naked eye—the Greeks could have obtained a value for the distance of the sun too large by a factor of 2, and thus a value for its luminosity too high by a factor of 4. By subsequently comparing the brightness of the "ten brightest" stars (presumably the brightest ten are meant) with that of the sun and assuming the average luminosity for these stars equal to the luminosity of the sun, the Greeks could have "determined" the average distance of these stars—but they would have been way off. The brightest ten stars visible from Athens 2400 years ago would have been the same as the brightest ten in the sky now, except for Achernar which was then not really visible north of Khartoum; the ten would have been Sirius, Canopus, Alpha Centauri, Arcturus, Vega, Capella, Rigel, Procyon, Beta Centauri, and Betelgeuse. At about 450 B.C. Alpha and Beta Centauri had declinations around −49°; Canopus had a declination of −52°, but since the Greeks presumably named this star, they must have been aware of its existence, and it must have been easily visible from Alexandria. If, however, one does not include it, Altair would be added to the list.

Using the latest available data—and for at least four stars these are none too good even now—we find the average luminosity of the brightest ten to

be 3300 (with Canopus) or 3100 (with Altair), or something like 800 times the value which von Hoerner thinks the Greeks would have got for the luminosity of the sun. Hence, instead of obtaining distances too small by 5 percent, the Greeks, had they gone about it in this manner, would have obtained distances requiring an upward correction of 2700 percent! It is possible that von Hoerner meant that if the Greeks had used their erroneous distance to the sun, had further assumed (erroneously) that the average luminosity for the brightest ten stars was equal to the luminosity of the sun, and had still further assumed (also erroneously) that the brightest ten were also the nearest ten, *then* they would have come out only 5 percent off, instead of 2700 percent.

It is always easy, in the light of hindsight, to show what erroneous assumptions should have been made in order to come out with the right answer—but isn't such a procedure tantamount to saying that if one makes a large enough number of assumptions, all of them erroneous, the "wrongness" of these is bound to cancel out and one will inevitably get the correct answer? This may work in economics, but it is thoroughly unscientific, and brings to mind Kettering's famous definition of logic as "an organized way of going wrong with confidence."

With one statement of von Hoerner's I am in complete agreement: "the search for other civilizations will have either a tremendous result or none at all." But it seems to me that any scientist should feel duty bound to add that at the present time not only are we completely and totally ignorant of the relative probabilities of either occurrence but most scientists have a strong feeling that the odds are strongly against the former occurrence. If this is not said, someone may cite d'Alembert's easy definition of probabilities (that since all events either can or cannot happen, the probability of any event's happening is 50 percent) and come up with the conclusion that *therefore* project OZMA has a 50–50 chance of immediate success.

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First, it is nice to see that my suggestion of what the ancient Greeks should have guessed has been taken *seriously*; but this should not be done to such an extent that the ***fun*** in it is overlooked.

Second, I must apologize for not having mentioned explicitly the assumption involved, that the brightest stars are the nearest ones. That this assumption was used might have been seen from the phrase "the distance between *neighboring* stars" (the goal of this estimate), but I should, of course, have mentioned it.

Third, that we draw conclusions from assumptions which later on turn out to be erroneous—this is true but unavoidable in the whole realm of science; just the degree of error varies from one case to another. Take, for example, such an important figure as the cosmic distance scale (the Hubble constant). If Sandage's latest value is right, then Hubble's original one was wrong by about a factor of 8. But many even of our well-established and most accurate figures would never have become available if nobody had dared to make the first guess.

In many instances we even use assumptions when we know definitely that they are wrong. For example, the whole theory of stellar dynamics is based on assumptions of this type; they are necessary in order to avoid unbearable complications, and it is hoped but not proved that they will not in-

fluence the results too greatly (I am prepared to give details on request).

Fourth, I did not mean to give the impression that "one will inevitably get the correct answer" by these methods of estimating. Instead, I said: "The resulting estimate can be, of course, completely wrong, but the probability that it will be is very small, and the probability that the result will be right is high." And, later in the article: "only by trying can we tell whether it is valid or invalid."

Fifth, as to the strong feeling of most scientists, I would like to reply that there are a number of good scientists, too, who have criticized my paper the other way, for being too pessimistic, as I have learned from various discussions and letters.

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UNESCO's Role in the Indian Ocean Expedition

I have read the recent article on the International Indian Ocean Expedition by Knauss [*Science* **134**, 1674 (1961)] with great interest. One significant omission should be pointed out. This concerns the role of UNESCO, which in addition to sponsoring the Indian Ocean Biological Center in India is also cosponsor of the expedition. This sponsorship was authorized by a resolution of the 11th session of the general conference of UNESCO in 1960.

Sponsorship of the expedition by UNESCO has been, and continues to be, more than nominal. Working in close collaboration with the International Council of Scientific Unions' Special Committee on Oceanic Research, organizer of the expedition, UNESCO has provided the financial means for bringing together participating scientists in the fields of marine meteorology (Bombay, July 1961), zooplanktology (Cochin and New Delhi, August 1961), and nutrient chemistry and primary productivity (Honolulu, September 1961). During the meeting in Honolulu, scientists from seven nations worked together, ashore and aboard the research vessels *Vitiaz* (U.S.S.R.) and *Gascoyne* (Australia), in a comparison of methods to be used during the expedition. Scientific equipment is being provided for research vessels of India and Pakistan. Fifteen fellowships have been awarded young scientists