Letters

Soviet Practices in Space

In "Planetary contamination II: Soviet and U.S. practices and policies" (24 Mar., p. 1505), Bruce Murray and his associates conclude "that Soviet practice has already led to the transfer to Venus, and probably to Mars, of a considerable number of viable terrestrial microorganisms."

Their conclusion with respect to Venus is based largely on what was *not* said by the Soviets about sterilization of the flyby bus and on the authors' assumption that the capsule separation and bus deflection maneuvers were not made automatically, even after loss of radio contact. As a firm conclusion, then, it seems more arbitrary than can be justified. It would have been more appropriate, for example, to have stated that "lacking more information about sterilization procedures applied to the bus or its components and about the probability that terminal maneuvers might have been carried out automatically, it must be assumed that viable organisms may have been transferred to Venus by the impact of the Soviet Venus 3."

Their conclusion with respect to Mars, also seems to be stronger than the known facts will support. It is apparently based on: (i) an indication in the Soviet press that Venus 2 and 3 were "of the same type" as Zond 2, (ii) an observation that the trajectory parameters for Zond 2 seem to have been selected to minimize approach velocity, at some expense in Earth-departure energy and communication distance, (iii) an informal statement reported by Sir Bernard Lovell to have been made by professor M. V. Keldysh, president of the Soviet Academy of Sciences, to him in February, to the effect that Zond 2 would miss Mars by 1500 kilometers, and (iv) tracking data of unstated quality from Jodrell Bank.

One might stretch (i) and (ii) enough to justify a conclusion that Zond 2 was probably intended to be a landing capsule mission, although alternative explanations can be postulated. I cannot, of course, comment knowledgeably about the circumstances under which the Keldysh statement was made or the accuracy with which it was reported. It should be noted, however, that this statement was not included in any formal release by the Soviet Academy or any other agency of the Soviet government. The U.S. National Academy of Sciences was naturally much concerned by this statement at the time and I personally discussed it with several highly placed Soviet scientists during the spring of 1965. Not only was I unable to verify the Keldysh statement, but I received a rather strong impression that the miss distance was likely to be much greater than 1500 kilometers.

Despite efforts to track the Zond 2 by other organizations with suitably located large antennae, I believe Jodrell Bank is the only station outside the U.S.S.R. to have picked up the signal and it is my understanding that even at Jodrell Bank, the signal was extremely weak and the tracking accuracy somewhat uncertain.

All this seems to be insufficient evidence on which to base the conclusion that "there is already a high probability that viable terrestrial organisms have been transported to Mars," especially if, as suggested by the authors, this conclusion is to be used as a basis for making policy decisions.

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It would be tragic if false conclusions were drawn from uncertain information about the probable contamination of Mars by Zond 2. The probe was located at Jodrell Bank on 2 December 1964, the day after it was launched. Although the Soviet scientists failed to answer a number of inquiries, contact was maintained in spite of the irregular and intermittent nature of the transmissions. During January 1965 contact was maintained on a number of successive days. This daily sequence ceased before the end of the month and in February the transmissions appeared to be on a weekly timetable. Contact became increasingly difficult, not because of weak signals, but because the probe's transmissions occurred even less frequently in March and April. Our last definite contact with Zond 2 was on 7 April 1965.

On 17 February 1965 Keldysh and some of his colleagues visited Jodrell Bank (they were in the U.K. at the invitation of the Royal Society). We replayed the Zond 2 signals to Keldysh and referred to the orbital computations made by one of my colleagues on the basis of our tracking data that Zond 2 was on a collision course with Mars terminating on 6 August 1965. Keldysh said that "the original trouble with the probe had been overcome but another difficulty had occurred which it was hoped to correct." He also said that the probe would "pass within 1500 km of the planet."

With respect to these statements we assumed that the "original trouble" was that with the power supplies referred to in the Tass communiqués of December 1964. We have no idea what the other difficulty was, but clearly it led to the cessation of signals some time in April 1965. The Jodrell orbital calculations inevitably contained a number of simplifying assumptions, but, for example, they fitted the good contact made on 17 February 1965 (a few hours after Keldysh's visit) when Zond was at a distance of 30 million km at right ascension, $08^h\ 10^m$; declination $+\ 06°\ 00'$. In view of these assumptions we consider that the calculations are entirely consistent with the miss distance quoted by Keldysh.

Search for Zond 2 signals, around the predicted date of close approach 6 August, from 5 to 7 August was made with the 250-feet (76-m) telescope. The results were negative, although on the basis of the signal strength at the last contact on 7 April, there should have been no difficulty.

Our conclusions at Jodrell Bank are therefore (i) that signals from Zond 2 finally ceased shortly after our last contact on 7 April 1965; and (ii) that Zond 2 passed the vicinity of Mars on 6 August 1965. We prefer to accept the statement of the President of the Academy about the miss distance. Clearly the Soviet scientists were in a position

to make far more precise calculations about the orbit without some of the assumptions which had to be made in the Jodrell calculations. Further, it must be emphasized that a miss distance of 1500 km was within the accuracy of our orbital computations.

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A Collector on Sagami Bay

May I call attention to the following publication: *A Review of Hydroids of the Family Clathrozonidae with Description of a New Genus and Species from Japan.* Hirohito, Emperor of Japan. In English, 16 pp.; in Japanese, 14 pp. 1 color sketch, 1 map, 14 plates (Biological Laboratory, Imperial Household, Tokyo, 1967).

This is a remarkable publication, although it looks like a reprint from some humble journal of systematic zoology. It is remarkable because of its being a scientific paper of a sovereign. The Emperor openly signs his name and takes responsibility as the author. As far as I know, there is no precedent of this sort, at least in the field of biology.

It is well known that Emperor Hirohito has a great interest in biological study. He has a laboratory in the compound of the palace in Tokyo. He goes there regularly every Saturday —and occasionally Thursday—afternoon unless his schedule is disturbed by some official function. He uses every odd hour available for his biological study.

The Emperor has a villa at Hayama, a seaside resort near Kamakura, where he spends his leisure hours in various seasons. Here he has a boat with equipment for collecting marine animals. During his stay in this villa, he frequently goes out collecting. I have had some chances to accompany the Emperor on such occasions. I found him to be a superb collector, observant, patient, and physically robust. In looking for littoral animals, he turns every stone and looks into every crevice. On board the boat, he closely examines debris dredged up from the bottom of the sea for any minute organism. It is also his habit that, after selecting materials of interest, he carefully returns stones to their original positions, leaving a part of the material attached to them, and he throws the dredged debris overboard to allow the minute organisms to thrive in their home.

Such being the Emperor's habit for many years, a good many specimens of various marine forms have been accumulated in the museum of his biological laboratory. Sagami Bay, where most of the Emperor's materials were collected, has been famous among foreign biologists for richness and variety of marine animals, especially through works by D. S. Jordan, B. Dean, F. Doflein, and others.

Abundant collected materials are sorted by the Emperor's biological assistant, and groups are sent to specialists to be studied. *Opithobranchia of Sagami Bay* (1949. 194 pp., 50 plates), its supplement (1955. 59 pp., 20 plates) by K. Baba, *Ascidians of Sagami Bay* (Iwanami, Tokyo, 1955. 315 pp., 80 plates) by T. Tokioka, and *The Crabs of Sagami Bay* (Maruzen, Tokyo, 1965. 298 pp., 100 plates) by T. Sakai are the results of such studies. Other publications of the same nature, on molluscs, gorgonids, asteroids, and other groups are forthcoming.

In addition, some forms of special phylogenetic interest were subjected to special studies, for instance: *Atubaria heterolopha,* a sheathless cephalodiscoid [T. Sato, *Zool. Anz.* **115** (1942); T. Komai, *Proc. Jap. Acad.* **25**, 19 (1949)] and *Lyrocteis imperatoria,* a large and beautiful sessile ctenophore (T. Komai, *Mem. Coll. Sci. Univ. Kyoto Ser. B* **17**, 1–36, 3 plates).

The present memoir forms part of the results of the Emperor's own painstaking studies over many years. Indeed, his preference for hydroids over other groups dates back nearly four decades. I remember that in June 1929, when I had a chance to accompany him on a trip collecting littoral animals along the Kii peninsula, he showed great interest in hydroids. The specimens of this group preserved in the museum of the Emperor's biological laboratory are certainly, in quantity and in quality, exceptional among collections in the museums of the world. The numerous fine slides of these forms preserved in a cabinet standing beside his working desk are also superb.

This memoir deals with the rare hydroids of the family Clathrozonidae that were known only from South Australian and Antarctic waters. The systematic affinity of this family had been disputed among previous authors, because the hydroids belonging to it have some characteristics eligible to the suborder Athecata and other characters