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Contamination of Mars

Since a significant chance of contamination exists, Mars-bound spacecraft should be sterilized carefully.

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Two papers concerning the problem of biological contamination of Mars have recently appeared in *Science*. Horowitz *et al.* (*1*) argue that the probability of release of entrapped organisms from the inside of a spacecraft is extremely small, and that the probability of reproduction of a terrestrial microbial contaminant on the Martian surface is negligible. Murray *et al.* (*2*) argue that, in any case, Mars (and also Venus) may have been exposed to terrestrial microorganisms deposited by Soviet spacecraft. Both papers conclude that the present sterilization standards adopted by the Committee on Space Research (COSPAR) of the International Council of Scientific Unions should be significantly relaxed. These recommendations are that the probability of a single viable organism aboard any spacecraft intended for Mars landing be less than 1×10^{-4}, and that the probability of accidental impact over a period of decades by each unsterilized flyby or orbiter be 3×10^{-5} or less. We have grave reservations about the arguments presented in both papers, and wish to clarify the difference between their views and ours, and to delineate problems requiring further study.

The COSPAR recommendations were based on an analytic equation for σ, the number of viable microorganisms per capsule deposited on the Martian surface, obtained from probability theory by Sagan and Coleman (*3*, *4*). The focus of this discussion was the desirability of performing a large number of biological experiments on the Martian surface before there is a sizable probability of contamination. (It can also be argued that more emphasis should be placed on the risks of the very first missions, since these may help set wider goals than the biological exploration of Mars.) Our commitment to this goal, measured by the parameter p, was put at 0.999. Although the discussion of Sagan and Coleman inserted representative numerical values into the analytic expression, it was indicated that these were tentative values, subject to revision by future research, and the reader was invited to make his own selection of numerical values.

The COSPAR deliberations on acceptable levels of contamination occurred in Florence in May 1964 when a study group on standards for space probe sterilization met under the chairmanship of Professor C.-G. Hedén, of the Karolinska Institute, Stockholm. The other members of this group were Alan Brown, United States; Auduoin Dollfus, France; Marcel Florkin, Belgium; Lawrence Hall, United States; A. A. Imshenetskii, U.S.S.R.; Carl Sagan, United States; P. H. A. Sneath, United Kingdom; and Wolf Vishniac, United States. Each member (Professor Imshenetskii was largely absent because of illness) gave his best estimate for each numerical parameter entering into the analytic framework. The resulting values of σ were then derived. The discussion converged on a final value of $\sigma \simeq 10^{-4}$. Contrary to the assertion of Horowitz *et al.* (*1*), the numerical values adopted by Sagan and Coleman were not in all cases those adopted by the COSPAR group, and the value of 10^{-4} adopted by the study group, and approved by the executive council of COSPAR, represented the best opinion of the study-group members. Subsequent COSPAR deliberations have recommended $\sigma = 10^{-3}$ (*5*).

We believe that all workers recognize the difficulty of assigning such numerical values rigorously. In the absence of a large body of experience with Mariner and Voyager spacecraft, and in the absence of studies *in situ* on the Martian surface and subsurface, we are ignorant of the relevant parameters. One terrestrial microorganism reproducing as slowly as once a month on Mars, without other ecological limitations, in less than a decade would result in a microbial population of the Martian soil comparable to that of Earth. This example is of heuristic interest only, but it does indicate that the errors in problems of planetary contamination may be extremely serious. If we agree that we desire $1 - p = 10^{-3}$ with an uncertainty in our commitment of, say, a factor of 100, then each other parameter of relevance (such as the probability of release of encapsulated contaminants, or the probability of multiplication of terrestrial microorganisms on Mars) must be known to roughly similar precision. For each of the objections posed by Horowitz *et al.* (*1*), we believe there are uncertainties in the relevant probabilities of factors of 100 or more, to say nothing of the possible presence of unforeseen factors. Where there are legitimate differences of opinion in discussions of planetary

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quarantine, the burden of proof must fall on those advocating a relaxation of standards. Horowitz *et al.* state that a large upward revision of the allowable microbial load per spacecraft is now possible on the basis of new evidence. We now examine this evidence. It falls into four categories: release, survival, dissemination, and growth of terrestrial microbial contaminants on Mars.

Release

An important point in the connection between the COSPAR conclusions and the arguments presented (*3*, *4*) appears to have been missed by Horowitz *et al.* (*1*). The parameter σ is defined as "the mean number of viable microorganisms per capsule which are distributed outside the capsule, on the Martian *surface*" (*4*). The COSPAR recommendations refer to the "probability that a single viable organism be *aboard* any vehicle intended for planetary landing" [(*6*), italics added]. The difference clearly relates to the mean probability of release of a contained microorganism. The COSPAR deliberations and the discussions (*3*, *4*) were influenced by our ignorance of release over a period of decades. Horowitz *et al.* argue that we can now decide that this release probability is extremely small, and that surface sterilization techniques should therefore be adequate. The principal difference between our approach and theirs is our unwillingness to consider a compound risk that may be small by conventional standards—say, 10^{-2}—as equivalent to zero when the stakes are very high.

Crash-Landing

One serious contingency for release of contained microorganisms is a crash-landing, and, particularly, a high-velocity impact. Judging from experience with lunar cratering, a spacecraft making an impact on Mars with a velocity about 6 kilometers per second will be totally pulverized. But experience with missile impact indicates that even at impact velocities of 0.6 kilometer per second or less, a significant fraction of the missile's mass is not in the impact crater and is unrecoverable (*7*). The shells and grenades used in bacteriological warfare indicate that contained microorganisms will survive such impacts. The escape velocity from Mars is 5 kilometers per second. Capsules intended for entry into the Martian atmosphere can be designed so that aerodynamic braking will slow the vehicle down to a velocity $<<$ 1 kilometer per second with fairly high reliability, and for a range of such contingencies as the approach orientation of the capsule (*8*). But what the probabilities are is not well known. There are other contingencies such as an unfavorable retrorocket firing, accelerating rather than decelerating the capsule; or accidental failure of a bus-deflection maneuver. A specification of each contingency of high-velocity impact with associated probabilities needs to be made; until then, probabilities $\sim 10^{-2}$ of high-velocity impact are not obviously unreasonable for planned U.S. and Soviet missions. This probability can be made $<< 10^{-2}$ by failsafe precautionary terminal corrections in case of imminent high-speed impact, or by the development of failsafe terminal-destruct sterilization procedures (*3*, *4*); but such devices are not now planned in the Mariner and Voyager programs, or, so far as we know, in the Soviet Mars program.

Information on the fragmentation size distribution of crash-landings at lower velocities is needed and almost totally lacking. This should be done for a variety of impact velocities, and the possibility that the fragmentation-distribution function is bi- or polymodal should not be overlooked. If fragmentation tends to occur along identifiable fracture planes, surface sterilization of such planes will be very effective, as Horowitz (*9*) has emphasized. Unfortunately there are almost no data available on fracture modes for various scenarios of mission failure. An engineering examination of this subject is urgently needed.

Even after the successful landing of an intact spacecraft, a variety of release mechanisms is possible. It is argued (*1*) that aeolian erosion is negligible for a period of decades on Mars. Calculations by Leovy (*10*) put the maximum wind velocity on Mars as 80 to 160 kilometers per hour; Horowitz *et al.* calculate that 145 to 250 kilometers per hour is required for rolling and saltation. Horowitz *et al.* conclude from this that the Martian winds are "*probably* too low to initiate grain movement" (our italics). Both calculations are so uncertain that the results can more accurately be considered identical (*11*). The large fluctuations of temperature on Mars lead naturally to the presence of dust devils with large vortex velocities (*12*), and frequent observations of yellow clouds with the same photometric properties as the Martian bright areas strongly point to windblown dust (*13*). The idea that all, or even most, of the dust storms are produced by meteorite impacts is highly unlikely, and is contrary to the opinion of the majority of observers of Mars. The Mariner IV photographs show a very marked filling of Martian craters, probably by dust. Martian craters are generally much more eroded, and have much flatter bottoms and more filled-in appearances than craters on Moon.

Horowitz *et al.* quote aeolian erosion rates in lucite for terrestrial deserts of just under 1 millimeter every few decades. While lucite is a relatively soft material, the extrapolation of these results to the erosion of exterior spacecraft components on Mars is uncertain to several orders of magnitude. Further information on expected aeolian erosion rates on Mars is badly needed. There are many prospective components of Mars landing vehicles that have structure and crevices at a depth of some millimeters, and it appears obvious that, for the present, sterilization at least down to such depths is necessary. Surface sterilization may be inadequate for this task. Heat soaking is probably required, but for this purpose lower temperatures or shorter times might be adequate: the thermal wave need not penetrate to the deep interior of the spacecraft.

Also, it must not be forgotten that the values of parameters chosen in (*3*, *4*) represent averages over many missions. Our immediate concern is with the choice of parameters for the first landed missions, and here we must be more cautious. The chance of an accidental crash-landing is probably not less than 0.1, and might optimistically be made as small as 0.5. Even this number in the arguments presented in (*1*) fails to take into account the large range of intermediate possibilities between complete failure due to crash-landing, and aeolian erosion down to a depth of some millimeters. For example, $1 - P_t$ is not the chance of a catastrophic crash-landing as stated in (*1*) but rather the probability that some fraction of the experiments in a lander do not operate as planned; P_t is the probability of complete engineering and scientific success. We must take into account the possibility of partial damage, which allows organisms from only some parts of the spacecraft to reach the Martian surface.

Other Fractionation Mechanisms

Several other spacecraft fractionation mechanisms have not been given nearly enough attention. There is possible metal fatigue and thermal erosion due to the very large diurnal and seasonal temperature variations. There are some data on rocks, but there is no information on the survival of spacecraft after the 14,000 cycles over 100°K represented by a few decades on Mars. Since spacecraft are constructed to survive the thermal environment of space during transit, they can probably be constructed to survive a few decades on Mars. However, there is no evidence that they are being so constructed. There are possibilities of chemical weathering. The oxide coats of surface metals (thickness, a few microns) will certainly be breached by aeolian erosion. If the Martian environment is slightly reducing, chemical weathering of the exposed metallic surfaces may occur. Although many nonaqueous chemical corrosives are also sterilants, we have no assurance that all organisms released by chemical weathering will automatically be killed. Erosion mechanisms may even involve an indigenous biota. There may well be other spacecraft erosion mechanisms that we have not thought of. To allow for such possibilities, the interiors of spacecraft must be sterilized until better information is available.

Horowitz *et al.* do not question experiments that show that many varieties of terrestrial microorganisms survive simulated Martian conditions indefinitely. But legitimate questions can be raised about what fraction of the likely microbial contaminants of a spacecraft can survive Martian conditions (*3*, *4*). Most such contaminants are probably of human origin or are derived during assembly and storage at the launch facility (generally in warm climates for the United States, in colder climates for the Soviet Union). Do the likely contaminants include anaerobic psychrophiles? Are those organisms that survive terminal heat soaking likely to survive Martian low temperatures? What fraction of released contaminants will be killed on Mars by ultraviolet light before being shielded by absorbing grains of surface material, or by other microorganisms? Each of these questions can be approached experimentally; until they have been, we must proceed cautiously.

If there were a hard landing, and, particularly, a high-velocity impact of a spacecraft on Mars, spacecraft fragments would be distributed over a wide area. In a period of minutes—less than the time for many unshielded terrestrial organisms to accumulate an ultraviolet mean lethal dose on Mars—fragments traveling in parabolic trajectories at 6 kilometers per second will cover a lateral distance ~ 1000 kilometers. A typical microbial load of a large surface-sterilized spacecraft assembled under clean-room procedures (*14*) is 10^7; distributed uniformly over the Martian surface, it results in a loading density of roughly one microorganism for every 10 square kilometers on Mars. However, other failure modes lead to much higher loading densities, for example, in-flight rupture of nutrient broth containers carried for bioassays on Mars. Spacecraft should be designed to minimize the risks attendant to such failures.

Distribution of Microorganisms

There are other possible dissemination mechanisms besides crash-landings. The prominent dust storms and high wind velocities previously referred to imply that aerial transport of contaminants will occur on Mars. Although a single unshielded terrestrial microorganism on the Martian surface—even the most radiation-resistant variety—would probably be rapidly enervated and killed by the ultraviolet flux, this by no means applies to all contamination scenarios. The Martian surface material probably contains a substantial fraction of ferric oxides, which are extremely strongly absorbing in the near ultraviolet. In fact, quite apart from the ferric oxide identification, the red color of Mars clearly indicates major electronic transitions at short visible wavelengths. A terrestrial microorganism imbedded in such a particle can be shielded from ultraviolet light and still be transported about the planet. In addition, microorganisms generally are not distributed singly, but tend to clump. The peripheral organisms may be rapidly killed by solar ultraviolet radiation, but microorganisms in the clump interiors may survive for long periods of time. These points have experimental confirmation, in the Luster program of spaceflights in the vicinity of Earth (*15*). Here the microorganisms observed survived solar ultraviolet light and other radiation for much longer than the time calculated on the basis of individual death curves. Not all released contaminants will successfully adhere to shielding particles. Simple laboratory experiments, such as the ultraviolet illumination of an aerosol of bacteria and pulverized limonite, and subsequent scoring can clarify these issues.

We do not know enough about Mars to exclude subsurface transportation of contaminants, as by underground rivers. Terrestrial volcanic belts tend to be connected by such river systems, and some evidence exists for tectonic activity on Mars (see below).

If aqueous environments exist on Mars, we shall undoubtedly bias our landing areas to such sites. The resulting microbial load can thereby be amplified by many orders of magnitude; and if aqueous underground transport is a real possibility, dissemination will also be amplified. In either case, mean contaminant loading densities of 1 per square meter and much higher densities in the biologically most interesting areas are by no means out of the question.

Before we discuss the problem of growth of contaminants on Mars, we wish to stress that even the dissemination of viable microorganisms without growth may have serious consequences. One result of continuing observations could be an assessment of the potential future utility of the planet Mars by the human species. No such assessment has thus far been made, and such considerations have had, to date, no effect on our commitment to a decontamination program. It is obvious that if the future utility of Mars were thought to be great, we would be willing to increase the costs and efforts to achieve effective sterilization. Any future exploitation of Mars might be seriously compromised by the dissemination of even nonpropagating terrestrial microorganisms in the form of spores that survive the present environment, but which might grow in more favorable environments, to be introduced in the future.

Growth of terrestrial microorganisms on Mars depends on the availability of liquid water. Although the mean Martian conditions are inconsistent with liquid water, there are many possible microenvironments that permit liquid water, at least in isolated times and places. We have suggested (*16*) that geothermal activity on Mars provides water and local high temperatures conducive to replication of terrestrial-type organisms. Horowitz *et al.* (*1*) exclude such possibilities, and believe that Mars is an undifferentiated and geologically inactive planet. They also state that, while there may be lineaments observed

in the Mariner IV photographs, on Earth, the outgassing of water associated with such lineaments is recirculated surface water, and not juvenile water being outgassed for the first time.

However, Mars very likely contains a subsurface permafrost layer, and there is much evidence of loosely bound or adsorbed water in the surface material (*17*). In either case, geothermal activity would release otherwise unavailable water to the surface. At any given time the fraction of the planet undergoing such geothermal activity should be very small. It is also possible that the permafrost cap is breached occasionally by meteorite impact. The differentiation of Mars is a highly debatable question. Meteorites whose parent body radii are far smaller than Mars show clear signs of differentiation. Moon, with a mass much less than that of Mars, shows unambiguous signs of vulcanism. There are also several cases of lunar outgassing, apparently well documented. Studies of the thermal history of Mars are in a preliminary state, and at least some models (*18*) predict a fairly differentiated planet. Radar and other evidence for major elevation differences on Mars, and for ridges resembling submarine tectonic ridges on Earth, has been published (*19, 20*).

Aqueous Areas of Mars

The lifetime of liquid water on Mars is a more serious question. Salt deposits will lower the eutectic point—in many cases by several tens of degrees. There may be frequent briny pools on Mars. Horowitz *et al.* imply that all halophiles are aerobes, but we believe this must be an artifact of the experimental conditions; not much effort has been put into finding anaerobic halophiles (*21*).

The triple point partial pressure of pure liquid water is just under 6 millibars. Average total pressures on Mars probably range from a few to almost 20 mb (*20*), but the mixing ratio of water is $\sim 10^{-3}$. Equatorial daytime temperatures range up to 20 or 30°C (*22*), corresponding to saturation vapor pressures of 25 mb and higher. A pool of liquid water exposed on Mars at temperatures $\simeq$ 273°K will evaporate initially at a rate $\sim 10^{-2}$ gram per square centimeter per second. The evaporation is so fast that vertical and horizontal transport of water away from the pool will be the rate-limiting steps in the early phases of vaporization. Because the saturation vapor pressure at these temperatures is of the same order as the total pressure, the pressure difference will lead to hydrodynamic flow, and winds will carry the vapor away. The transport rate has not, to the best of our knowledge, been calculated. Before long, however, the high latent heat of vaporization of water should result in cooling and perhaps freezing of the upper surface of the pool, cutting down the vaporization rate substantially. Some pools on Mars may thus be exposed during the hottest part of the day, and sealed for the remainder of the day. Other locales where the partial pressure of water may temporarily be comparable to the total ambient pressure are the edge of the polar caps, and the dawn limb of Mars. Because of the inhibition of horizontal eddy diffusion, a pool at the bottom of a crater (perhaps formed during crater formation) is of particular interest.

The frost point of 20 microns of precipitable water vapor is about 190°K, a temperature reached before sunrise, probably in most locales on Mars. A dawn haze is actually seen frequently. After sunrise the computed lifetimes of the condensates are 15 minutes or less. However, diffusion will again be the limiting step, and saturation vapor pressures may be maintained in crevices and soil interstices for periods approaching an hour. As the temperature goes above 0°C, liquid water will be formed temporarily. Experiments under Martian pressures and temperature cycling, and with a variety of wind velocities, performed in the laboratory of one of us (C.S.), tend to support the conclusion that thin layers of liquid water are generally formed each morning at tropical latitudes on Mars when diffusion is limited. A variety of experiments (*23*) indicates that many terrestrial microorganisms can grow, even if liquid water is available for only 15 minutes each day, in otherwise subzero conditions; or in soil microenvironments where liquid water is available as a consequence of freeze-thaw cycling. Although much more work is required, the evidence strongly suggests that many microorganisms are capable of growth at slow rates, at temperatures near 0°C. There are too many possibilities still open to dismiss significant growth and replication of terrestrial microorganisms on Mars, over a period of decades.

The use of 10^{-2} for the probability that a terrestrial microorganism deposited on the surface of Mars will grow and contaminate the planet may in fact be too low rather than too high. We anticipate that we will be thoughtful in planning the strategy of planetary exploration; that is, over a long time scale we will land not just at an average location on the planet, but where terrestrial microorganisms are most likely to grow. Our subsequent concern is not necessarily with growth over large areas of the planet, but rather with growth at those favorable sites chosen for subsequent investigations. Thus, deposited microorganisms must simply survive exposure to average Martian conditions (which we already know will occur with some likelihood), and subsequently grow upon arrival at a favorable locale.

Mission Planning

We believe that the probabilities of spacecraft erosion and fracture, survival from the ultraviolet flux, and subsequent ultimate deposition in a warm, wet locale on Mars may be rather high. If sterilization standards are to be relaxed, we must be quite certain—much more certain than we can be in our present state of ignorance—that such will not be the case.

Additional considerations may be drawn concerning the reassessment of flyby and orbiter missions to Mars. One principal contribution of the Mariner IV and V missions to these deliberations is their demonstration that a probability of about 3×10^{-5} for accidental planetary impact by unsterilized flyby or orbiter spacecraft does not preclude the carrying out of useful missions. Because of the low atmospheric pressure on Mars, spacecraft may be placed in very close orbits permitting extremely high topographical resolution at periapsis, without compromising the sterilization standards. The cost of preventing contamination is much greater for landers than for orbiters or flybys. It is also clear that any evaluation of the risks associated with a lander mission is subject to a wider margin of error than is an orbiter mission at present. This implies a planetary exploration strategy of the following sort: Flyby and especially orbiter missions are carried out under present constraints on accidental impact to obtain information leading to an improved assessment and refinement of the risks of planetary landing and the associated parameters. At the same time, efforts to improve steriliza-

tion skills are made. Such remote observations from a planetary orbiter might, for example, indicate the location and distribution of areas of increased moisture and temperature (*16*). Whatever lander missions are carried out during this period would be such that even conservative estimates of the load of terrestrial microorganisms will be satisfied. Only after such a program would a major relaxation of sterilization standards be allowed, depending on the results attained.

Interaction with the Soviet Space Program

We now comment on the contention of Murray *et al.* (*2*) that the Soviet space program is not failsafe in the sense that American flybys are deflected away from the planet during midcourse maneuvers; that there is consequently some possibility that Zond II impacted Mars; that future Soviet space vehicles, undergoing sterilization procedures not evaluated in the West, will also impact Mars; and therefore that the sterilization requirements on American space vehicles can be considerably relaxed. We first point out that the arguments of (*1*) and (*2*) together imply little likelihood of Mars being contaminated: if there is negligible chance of contaminating Mars, then Zond II has not contaminated it. But if, as we argue, there is a significant chance of contaminating Mars, do the arguments in (*2*) necessarily follow? Each contention of Murray *et al.* has some associated uncertainty, and it is not clear that any microorganisms have landed on the Martian surface as a result of the Zond II mission (*24*). Further information from the Soviet Union would be relevant here. We note that an accidental impact, such as suggested for Zond II, interacts with the mean Martian environment, while a partially successful lander has failure modes that permit interaction with the most favorable environments for microbial growth on Mars. If an unsterilized Zond II space vehicle has impacted Mars, it does not follow that the United States may with abandon land spacecraft that have been only surface sterilized. An analogy that has been useful in discussing such sterilization issues concerns a dry forest in tinderbox conditions. If the individual in front of us throws a lighted match into the forest, it does not follow that we may throw large numbers of lighted matches as well, particularly if we are seeking out the driest parts of the forest. His match might not ignite the forest; ours might. Also, if we are cautious with matches, the need for caution and the method of achieving it might be grasped by our companion.

Our planetary quarantine program must take into account the history of the planet in question, and must certainly consider possible contamination resulting from missions other than those of the United States. It is also useful to make Soviet scientists aware of the interrelationship between the policies of our respective nations. This interrelationship is also "multilateral."

The conclusion (*2*) that the U.S. contribution should equal some specified fraction of the probable number of viable terrestrial microorganisms transferred to Mars by present and foreseeable Soviet efforts, however, seems to be based on some misunderstanding of the purpose of planetary quarantine. It is not an immigration policy designed to keep the total number of migrants matched to the resources of the target planet. It is a quarantine program designed to minimize the possibility that the planet will be infected with a single viable organism that might propagate now or in the future. Our choice of quarantine policy, and the costs and efforts consequent to that policy, are a measure of the importance that we ascribe to an uncontaminated planet. Our knowledge of Soviet missions to Mars is relevant in permitting us to estimate the probability that the planet is now infected with viable or replicating organisms. This implies some proportionate reduction in the utility of maintaining our sterilization efforts. If we wish to keep the ratio of the cost of our efforts to the potential gains that justify those efforts constant, then a proportionate reduction of our sterilization efforts could be justified. But this would then lead to a small change in the present quarantine policy. This is a very different result from that of Murray *et al.* They seem to argue that if there is a probability, P, that Soviet missions have infected Mars with N viable organisms, then a satisfactory United States policy would be to deliver some specified fraction of $P \times N$ organisms per mission.

Keeping in mind the scientific goals of Martian exploration, and their multilateral nature, we might conclude that these goals can be better achieved by delivering sterilizable batteries to the Soviet Union rather than viable microorganisms to Mars. We should make certain that specific information about our advances in decontamination technology are widely disseminated. Credible implementation of our present sterilization policies might certainly have a salutary influence in increasing the concern about planetary contamination within the Soviet Union. The proposal of Murray *et al.* would have exactly the opposite effect. The COSPAR recommendations of the Study Group on Standards for Spacecraft Sterilization were approved by its parent body, the COSPAR Consultative Group on Potentially Harmful Effects of Space Experiments and by the executive council of COSPAR. Representatives from both the United States and the Soviet Union participated in the activities on all levels, including in those of the executive council. The Soviet Union has also, in the recent space treaty, expressed concern about planetary contamination. Methods should be explored by which the agreements at COSPAR and within the space treaty can be used to increase positive feedback on missions planning and sterilization technology between both spacefaring nations.

Sterilization of Soviet Spacecraft

According to press reports from TASS, received after this article had been submitted for publication, both the entry capsule and the bus of the Soviet Venera 4 mission entered the atmosphere of Venus. The capsule was described as "sterilized," but there is no suggestion that anything beyond surface sterilization with gaseous germicides and high-energy irradiation of selected components was attempted. No claims were made about sterilization of the bus, which apparently contained a circulating coolant. Although much previous evidence pointed to high surface temperatures on Venus—which the Venera 4 mission brilliantly confirmed —there are other conceivable habitats above the surface (*25*). Also, COSPAR has recommended $\sigma \sim 10^{-2}$ to 10^{-4} for Venus. For all that is now known, the bus of Venera 4 broke open above the 1 atm pressure level and contaminated the clouds of Venus with terrestrial microorganisms. We regard this aspect of the Venera 4 mission with very great seriousness.

Of even greater seriousness is whether a similar mission is planned by the

Soviet Union for the 1969 Mars opportunity. If any mission is planned for 1969, its development must by now be well along. The impact of an unsterilized bus or of a superficially sterilized capsule could be a catastrophe for the biological exploration of Mars.

Uncertainty about sterilization also has serious consequences. If there is not a detailed exchange of knowledge concerning mission sterilization and procedures, then a wide range of estimates can be made about the probability that the planet is infected. This uncertainty, and pressure of other competition for resources, will lead to an estimate that will serve to minimize allocations for sterilization efforts.

It should be stressed that, to be effective, information exchange among space-faring nations must take place enough years before planetary missions to have a meaningful impact on design and planning. We hope that all our colleagues throughout the world will consider these issues carefully.

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26. We are indebted for useful discussion to the following individuals, who do not, however, necessarily support all our conclusions: R. W. Davies, P. K. Eckman, T. Gold, N. H. Horowitz, J. Hotchin, P. M. Hurley, B. N. Khare, J. Lovelock, J. B. Pollack, R. W. Porter, A. Rich, R. P. Sharp, W. Vishniac, R. S. Young, and, especially, B. C. Murray. Supported in part by NASA grants NGR 09-015-023 and NSG 81-60.

Transplantation: Problems of Histocompatability Testing

Typing and matching tests are useful in pairing donors and recipients for organ transplantation.

Fritz H. Bach

Pairing (*1*) of donors and recipients for organ transplantation has been important in determining the success and survival of kidney allografts. Two methods are available to achieve pairing: "typing," in which specific histocompatibility antigens are detected with suitable isoimmune antiserums, and "matching," in which tests in vivo or in vitro, such as the mixed leukocyte culture test (*2*), are used to measure the degree of incompatibility between two individuals.

It is increasingly evident that results of typing are predictive of skin graft survival (*3*), and that they are correlated, at least retrospectively, with kidney graft survival (*4*). Although these correlations are very strong in sibling pairs, predictions are less reliable in unrelated pairs (*5*). Although less extensively studied, there is evidence that the mixed leukocyte culture test is also predictive of skin graft survival between siblings (*6*), and that it correlates, retrospectively, with kidney graft survival (*7*).

I will discuss the genetic reasons that make predictions more simple in sibling pairs and an approach to the problem of histocompatibility pairing.

The purpose of pairing tests is to achieve "compatibility." Complete compatibility exists when donor and recipient tissues have identical histocompatibility antigens or when donor tissue has no foreign histocompatibility antigens (antigens which the recipient does not also have). Incompatibility exists if the donor has some antigens foreign to the recipient—the degree depending on the number and relative "incompatibility strengths" of such antigens.

Recent evidence (*8*) confirms earlier suggestions (*9*) that most of the human leukocyte antigens measured by antiserums now available are determined by alleles of a single genetic system, *HL-A*. A single polymorphic genetic locus also controls reactivity in mixed leukocyte cultures (*10*). Comparison of results obtained by such tests with those of leukocyte typing suggests that the locus controlling reactivity in mixed leukocyte cultures is the same one which determines the leukocyte antigens. We have proposed that this is the major histocompatibility locus in man (*11*). An undetermined number of "minor" loci undoubtedly exist, each determining histocompatibility antigens which, however, probably represent weaker incompatibility barriers.

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