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CONTAMINATION OF MARS

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Introduction

Two papers concerning the problem of biological contamination of Mars
have recently appeared in this journal. The first (1) argues that the proba-
bility of release of entrapped organisms from the inside of a spacecraft is
vanishingly small, and that the probability of reproduction of a terrestrial
microbial contaminant on the Martian surface is also negligibly small. The
second paper (2) argues 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 be abandoned. These recommendations are that the prob-
ability of a single viable organism aboard any spacecraft intended for
Martian landing be less than 1×10^{-4}, and that the probability for accidental
impact over a period of decades by each unsterilized flyby or orbiter be
3×10^{-5} or less. We have very grave reservations about the arguments
presented in both papers, and wish here to clarify the difference between
their views and ours, and to delineate problem areas crying out for fur-
ther study.

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California.

The COSPAR recommendations were based upon 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); slightly revised in (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. While 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, in a study group on standards for space probe sterilization, chaired by Professor C.-G. Heden, of the Karolinska Institute, Stockholm. The other members of this group were Alan Brown, U.S.A.; Auduoin Dollfus, France; Marcel Florkin, Belgium; Lawrence Hall, U.S.A.; A. A. Imshenetskii, U.S.S.R.; Carl Sagan, U.S.A.; P.H.A. Sneath, United Kingdom; and Wolf Vishniac, U.S.A. In the deliberations of this study group each member present (all but Professor Imshenetskii, who was 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 iterated in a final value of $\sigma \simeq 10^{-4}$. Contrary to the assertion in (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 menbers. Subsequent COSPAR deliberations have reduced σ to 10^{-3}.

In the absence of a large body of experience with Mariner and Voyager spacecraft, and in the absence of in situ studies of the Martian surface and subsurface, we are very ignorant of the relevant parameters. A single terrestrial microorganism reproducing as slowly as once a month on Mars

would, in the absence of other ecological limitations, result in less than a decade in a microbial population of the Martian soil comparable to that of the Earth's. This is an example 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 encapsuled 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 exist uncertainties in the relevant probabilities of factors of 100 or more, to say nothing of the possible presence of unforseen factors. Where there are legitimate differences of opinion in discussions of planetary 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 in (3) and (4) appears to have been missed by the authors of (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), italics in original]. The COSPAR recommendations refer to the "probability that a single viable organism be aboard any vehicle intended for planetary landing" [(5), italics added]. The difference clearly relates to the mean probability of release of a contained microorganism. The COSPAR deliberations and the discussion of (3) and (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 vanishingly 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.

One serious contingency for release of contained microorganisms is a crash-landing, and, particularly, a hypervelocity impact. Judging from experience with lunar cratering, a spacecraft impacting Mars with a velocity about 6 km/sec will be totally pulverized. But experience with missile impact indicates that even at impact velocities of 0.6 km/sec or less, a significant fraction of the missile's mass is not in the impact crater and is unrecoverable (6). The shells and grenades used in bacteriological warfare indicate that contained microorganisms will survive such impacts. The escape velocity from Mars is 5 km sec^{-1}. Capsules intended for entry into the Martian atmosphere can be designed so that aerodynamic braking will slow the vehicle down to a velocity << 1 km/sec with some high reliability, and for a range of such contingencies as the approach orientation of the capsule (7). But what the probabilities actually are is not well known. There are other contingencies such as an unfavorable retrorocket firing accelerating rather than decelerating the capsule, accidental failure of a bus-deflection maneuver, etc. A specification of each contingency of hypervelocity impact with associated probabilities needs to be made; until then probabilities $\sim 10^{-2}$ of hypervelocity 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.

Information on the fragmentation size distribution of lower velocity crash-landings 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 (8) 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, there is a variety of possible release mechanisms. It is argued in (1) that aeolian erosion is negligible for a period of decades on Mars. One argument is that calculations by Leovy (9) put the maximum wind velocity on Mars as 80 to 160 km/hr, while the authors calculate that 145 to 250 km/hr 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, in fact, so uncertain that the results can more accurately be considered identical. The large temperature fluctuations on Mars lead naturally to the presence of dust devils with large vortex velocities (10) and frequent observations of yellow clouds with the same photometric properties as the Martian bright areas strongly point to windblown dust (11). The idea that all, or even most, of the dust storms are produced by meteorite impacts is highly unlikely, and contrary to the opinion of the majority of observers on Mars. The Mariner 4 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 the Moon.

Horowitz et al. quote aeolian erosion rates in lucite for terrestrial deserts of just under 1 mm 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 surely 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.

It must also not be forgotten that the values of parameters chosen in (3) and (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, but 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 some parts of the space-craft but not from others to reach the Martian surface.

There are several other spacecraft fractionation mechanisms that have not been given nearly enough attention. The high prevailing winds, both diurnal and seasonal (the seasonal thermal and geostrophic winds are probably even higher than the diurnal ones) will produce vibrational stresses over long periods. There is possible metal fatigue and thermal erosion due to the very large diurnal and seasonal temperature variations. While there is some data on rocks, 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 probably can be constructed to survive a few decades on Mars. But 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 (e.g., if halide-substituted hydrocarbons are present) chemical weathering of the exposed metallic sur-faces will occur. While many nonaqueous chemical corrosives are also sterilants we have no assurance that all organisms released by chemical weathering will automatically be killed. There may even be erosion

mechanisms involving an indigenous biota. There may well be other space-craft erosion mechanisms that we have not been clever enough to think of. In order to allow for such possibilities, the interiors of spacecraft must be sterilized until better information is available.

Survival

Horowitz et al. do not question experiments that show that many varie-ties 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 [cf. (3, 4)]. Most such contaminants are probably of human origin or 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 psychrophils? 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, we must proceed cautiously.

Dissemination

In the case of a hard landing, and, particularly, of a hypervelocity impact of a spacecraft on Mars, spacecraft fragments will 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 travelling in parabolic trajectories at 6 km/sec will cover a lateral distance $\sim$ 1000 km. A typical microbial load of a surface-sterilized spacecraft assembled under clean room procedures is 10^7; distributed uniformly over the Martian surface, it results in a loading density of one microorganism every 10 km^2 on Mars. However, there

are failure modes that lead to much higher loading densities: e. g. , inflight rupture of nutrient broth containers carried for bioassays on Mars. Spacecraft should be designed to minimize the risks attendant to such failures.

Aside from crash-landings there are other possible dissemination mechanisms. The prominent dust storms and high wind velocities previously referred to imply that aerial transport of contaminants will occur on Mars. While it is probably true that a single unshielded terrestrial microorganism on the Martian surface — even the most radiation-resistant variety — would rapidly be enervated and killed by the ultraviolet flux, this by no means applies to all contamination scenarios. The Martian surface material certainly contains a substantial fraction of ferric oxides, which are extremely strongly absorbing in the near ultraviolet. In fact, 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 do not distribute themselves singly, but tend to clump. While the peripheral organisms may be rapidly killed by solar ultraviolet radiation, 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 the Earth (12). Here microorganisms were observed to survive 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. There are simple laboratory experiments on the ultraviolet illumination of an aerosol of bacteria and pulverized limonite, and subsequent scoring, which can clarify these issues.

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

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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 m^{-2} and much higher densities in the biologically most interesting areas are by no means out of the question.

Growth

Before we discuss the problem of growth of contaminants on Mars, we wish to stress that 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 level of 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 even of nonpropagating terrestrial microorganisms in the form of spores that survive the present environment.

The question of growth of terrestrial microorganisms on Mars is tied to the question of the availability of liquid water. While the mean Martian conditions are inconsistent with liquid water, there are a variety of possible microenvironments that permit liquid water, at least in isolated times and places. We have suggested (13) that geothermal activity on Mars provides water and local high temperatures conducive for the replication of terrestrial-type organisms. Horowitz et al. (1) believe that they know enough about the Martian environment to exclude such possibilities, and state their belief that Mars is an undifferentiated and geologically inactive planet. They also state that, while there may be lineaments observed in the Mariner 4 photographs, on the Earth the water outgassing associated with such lineaments is recirculated surface water, and not juvenile water.

However, Mars very likely contains a subsurface permafrost layer, and there is now a variety of evidence for loosely bound or adsorbed water in the surface material (14, 15). 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, however. It is also possible that the permafrost cap is breached occasionally by meteorite impact. The question of the differentiation of Mars is a highly debatable one. Meteorites whose parent body radii are far smaller than Mars show clear signs of differentiation. The 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 (16) predict a fairly differentiated planet. Radar and other evidence for major elevation differences on Mars, and ridges resembling submarine tectonic ridges on Earth, has been published (17, 18).

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 briney pools on Mars. Horowitz et al. maintain 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 (19). The triple point partial pressure of pure liquid water is just under 6 mb. Average total pressures on Mars probably range from a few mb to almost 20 mb (18), but the mixing ratio of water is $\sim 10^{-3}$. Equatorial daytime temperatures range up to 20 or 30° C (20), 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 $\sim 10^{-2}$ g/cm^2/sec. 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 freezing the upper surface of the pool, cutting down the vaporization rate substantially. It may be that some pools on Mars are thus 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 μ 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 min 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 performed in the laboratory of one of us (C. S.) under Martian pressures and temperature cycling, and with a variety of wind velocities, tend to support the conclusion that thin layers of liquid water are generally formed each morning at tropical latitudes on Mars. These experiments will be reported more fully elsewhere. A variety of experiments ([21]) indicates that many terrestrial microorganisms can grow, even if liquid water is available for only 15 min/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, many microorganisms seen 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 clever 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 at a place

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 and Interaction with the Soviet Space Program

We believe that the probabilities of spacecraft erosion and fracture, survival from the ultraviolet flux, and subsequent ultimate deposition in a warm and 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 4 mission to these deliberations is its demonstration that a probability of 3×10^{-5} or less 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 any level of commitment to prevention of 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 an orbiter mission at our present level of ignorance. This implies a planetary exploration strategy of the following sort: Flyby and especially orbiter missions are carried out within the present constraints on accidental impact to obtain information that can lead to an improved assessment and refinement of the risks of planetary landing and the parameters associated it. At the same time, efforts to improve sterilization skills are necessary in any case. Such remote observations from a planetary orbiter might, for example, indicate the location and distribution of areas of increased moisture and

temperature (13). 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.

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 2 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 argument of (2) is inconsistent with that of (1): if there is negligible chance of contaminating Mars, then Zond 2 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 by no means clear that any viable microorganisms have landed on the Martian surface as a result of the Zond mission (22). Further information from the Soviet Union would be extremely relevant in this regard. We note that an accidental impact, such as suggested for Zond 2, 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 2 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 needs for caution and the methods for achieving it might be grasped by our companion.

Our planetary quarantine program must take into account the total 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 that there is an interrelationship between the policies of our respective nations. This interrelationship is also "multilateral."

The conclusion in (2) however, 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 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 that are a consequence of 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 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. There is a real possibility that a credible implementation of our present sterilization policies will have a salutary influence in increasing the concern

about planetary quarantine 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 those of the executive council. The Soviet Union has also, in the recent space treaty, indicated its concern with planetary quarantine. 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.

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