Optical Environment in Gemini Space Flights

In their report [*Science* **153**, 297 (1966)] Ney and Huch give a detailed discussion of the scattering mechanisms they think may be responsible for the inability of orbiting astronauts to see stars in the daytime. They overlook an additional cause of the difficulty—scattering in the observer's eye. The intensity of ocular scattering is sufficient by itself to make impossible the observation of first-magnitude stars if the level of illumination on the face of the observer exceeds about 1000 lux (100 ft-c). Unless the viewing window of the space capsule is protected by a conical sunshade it will be difficult to reduce the interior illumination below this critical figure, even if the other window is obscured by a blind, as 1000 lux is only about 1 percent of the outdoor daylight level.

This fogging effect of ocular scattering is often experienced by city-dwelling astronomers who find that it is impossible to see the Milky Way within about 90 deg of the direction of a single street lamp that produces an ambient light level only about 0.01 percent that of daylight. That ocular scattering, rather than atmospheric scattering, produces the observed loss of contrast in the visual image of the sky can be shown by stepping into the shadow of the lamppost. The Milky Way can be seen immediately.

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Martian and Lunar Craters

In the next decade, it seems, the study of Mars may include as much prejudice and diversity of unqualified opinion as interpretation of the moon has suffered in the past. With the imminent advent of manned exploration of the moon, the interpretation of the lunar surface is approaching a definitive phase, and it would seem a pity if the same unfounded prejudices and fallacies regarding the lunar surface were transferred to Mars prior to direct exploration of the planet. Diversity of opinion, however, is to be welcomed as a stimulus to new fields, provided individual opinions are schooled with a variety of experience and provided the explanations for a given set of observations are scientifically acceptable. This approach is now particularly important in the field of planetary science, which calls for a combination of many different disciplines—for example, astronomy, physics, geology, and meteorology.

With these points in mind we wish to comment on a paper by Öpik (*1*) in a recent issue of *Science*.

Citing Fielder (*2*), Öpik states that attempts to ascribe a volcanic origin to Martian features can be "ignored completely." Such a statement made in connection with the evaluation of photographs that are so recent as the Mariner photographs is surprising! Decades of study of lunar photographs of a similar type have not resulted in lunar volcanism's being disregarded by impact-hypothesis adherents of even Baldwin's (*3*) standing. Indeed, as far as the moon is concerned, the general tendency is for opinion to be swaying over to admit an increasing proportion of endogenic features among features previously considered impact phenomena (*4, 5*).

Öpik goes on to say that the presence of volcanic formations on the moon or Mars remains to be proved. Many authors, ourselves included, would dissent from this view. The evidence for lava flows and volcanic craters on the moon is indisputably strong (see, for example, *5*).

Fielder's note (*2*) on Martian volcanism was based on the following argument (*6*). Many years of study have shown that the moon is partly volcanic; the ring structures, craters, and lineaments of Mars are remarkably like those of the moon; therefore Mars has probably been shaped in part by volcanic forces. This view contrasts with Öpik's categorical statements (*1*) against volcanism, which are not adequately backed, in his articles, by destructive or even critical arguments.

Öpik's next statement is equally misleading: "The lunar and Martian craters bear close resemblance to terrestrial meteor craters and are very different in structure from terrestrial volcanoes and calderas." First, he fails to recognize that the lunar craters and rings cannot be grouped together as one type; there are many different types, and Öpik is clearly displaying strong prejudice in assuming that the craters are virtually all impact phenomena. The vast majority of lunar craters and rings and Martian rings do not bear a close resemblance to proved terrestrial meteoritic craters. Second, there is a strong morphologic similarity between certain lunar and Martian rings, on the one hand, and terrestrial volcanic features on the other; this statement is contrary to Öpik's and is based on a protracted study reported in *Lunar Geology* (*7*), from which we may quote, concerning a terrestrial volcanic ring: "This caldera is much more lunar than any known meteoritic crater."

Öpik ends his paragraph or arguments against lunar and Martian volcanism with the comment that meteor craters are an observational fact. We feel tempted to ask if volcanic craters are not even more of an observational fact!

Regarding Mars, Öpik states that "the evidence of 'leeward clouds' occurring on the maria borders . . . would appear rather dubious to anyone who has systematically observed the planet. . . ." If Öpik is referring to the observations' being dubious, then his statement is erroneous, since the observations Wells has discussed elsewhere (*8*) were originally made by some of the most systematic astronomers who have ever observed the planet—Lowell and Douglass (*9*), Antoniadi (*10*), Dollfus (*11*), and Focas (*12*), the latter two observers having contributed the most recent observations which originally led to the comparison with lee-wave clouds.

In a similar manner Öpik regards as improbable the suggestion that the Martian maria are highlands, simply because the "darkish" dust covering them would be continuously wandering into the lowlands and thus blurring the observed sharp boundaries of the maria. Also he assumes that the reappearance of the dark maria after being covered with light-colored dust from the deserts is only attributable to some "peculiar" property of the maria—that is, to plants shaking off the dust covering.

It is, in fact, not necessary to the hypothesis for dark dust to be moved about on the surface. A variation in the size of grains making up the maria would produce the observed albedo changes. Fractionation of grain sizes in relation to elevated areas and its effect on the maria have been discussed by Rea (*13*). If the maria

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(large grains) were covered by desert dust (small grains), rejuvenation could be explained simply as due to the action of the winds in clearing off the smaller (and lighter-weight) grains and leaving the course grains exposed; or the smaller grains could be partially cemented into larger particles, like Hapke and Van Horn's "fairy castles." On the other hand, Rea and O'Leary (*14*) have shown that the variation in aerosol content of the atmosphere could similarly produce the observed polarization and albedo changes. Either the "dust" model or the "aerosol" model or a combination of the two is as consistent with the evidence provided by the Mariner photographs as is Öpik's interpretation, if not more so.

As Mariner crossed from the bright area Zephyria to the dark region Mare Sirenum, no visible "line of demarcation" was evident, although a subsequent analysis (*15*) has shown that the albedo did in fact change from that expected in the deserts to that expected in the maria as the normal projection of the trajectory on the surface passed from one region to the other. In addition, the only quantitative differential spectrophotometric measurements between the dark and light areas that have been made are those of Dollfus (*16*), and they indicate that the maria appear spectrally reddish like the deserts, though less red. These facts, summed up, tend to indicate that the shape and distribution of the maria are more dependent on size or roughness of material than on differences in chemical composition of the maria and the deserts, although such differences may indeed have some influence.

We have selected these points from two specific paragraphs of Öpik's paper to illustrate the fact that the interpretation of Martian phenomena should not be approached in a dogmatic manner.

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1 September 1966

Wells and Fielder take exception to my view that the possibility of a volcanic origin of the Martian craters can be "ignored completely" and stress that a dogmatic approach in this matter is undesirable. I heartily agree with this, if dogma is defined as convictions unsupported by fact and upheld against heavy probabilities or, at worst, against facts that are ignored. But then it may be asked, Who is dogmatic in the present case? A refusal to discuss improbable propositions is not always attributable to dogma, as can be seen from the following contemporary case. A just-published learned treatise (*1*) by Sheikh Abdullah ben Baz, vice-president of the Islamic University of Medina, attacks a "Western fallacy"—namely, "the much publicized theory that the earth rotates round the sun"; hardly anyone would accuse Western scientists of a dogmatic attitude for not reacting polemically to this challenge; the dogma is on the other side.

Of course, the case of lunar (and Martian, by inference) volcanoes is not, or not in all respects, as clear as that, although some analogy can be traced. Proponents of the volcanic theory have shown so much wishful thinking, especially by denying the impact hypothesis completely, that even a plausible kernel of substance regarding traces of primeval volcanism on the moon has sometimes fallen into disrepute. Fielder is one of those who had persistently ignored the impact theory, and quite recently he stated (*2*, p. 51): "the lunar craters, or at least the majority of them, are of internal origin." From a study of the randomness of the distribution of lunar craters he first arrived at a similar conclusion, but he then changed it to a different one for a region within Ptolemaeus: "the proportion of endogenic craters is at least 38%" (*3*)—thus no longer 100 percent. Although Fielder's statistical method (comparison with Poisson's formula) is absolutely irrelevant and unable to answer the question of origin of the craters (*4*), the concession to the impact theory of some 62 percent is ominous. It already means a retreat from the dogma of the volcano-selenologists.

Indeed, the theory of endogenic lunar volcanism (as distinct from volcanism caused as secondary effects of impacts) has been founded on dogma. That the stray bodies of the solar system are colliding with all the exposed planetary surfaces is an undeniable fact which has been subjected to statistical and mechanical analysis by myself and others. The "volcanists" have ignored, and some still are ignoring, this fact. On the other hand, Wells and Fielder are asking "if volcanic craters are not even more of an observational fact!" Terrestrial volcanoes, certainly; but no volcanic events have ever been pinpointed on the moon or Mars (except for misleading or wishful interpretations). To pretend that the occurrence of volcanoes on earth is a fact applicable without reserve to other planets goes against some accepted geophysical truths. Volcanism on earth is closely related to mountain building, and the succession of orogenic cycles during the earth's history is explained by imbalance in the crust caused by powerful erosion. There is little erosion on Mars and practically none on the moon, nor are there any traces of mountain chains similar to the mighty Alpine, Variscian, Caledonian, and earlier orogenes on our planet. Talk about lunar or Martian volcanoes that is based on terrestrial analogy cannot be justified. It has led also to misinterpretation of observational data, such as the remarkable spectrogram obtained by Kozyrev in 1958 which showed luminescence on the peak of the crater Alphonsus. Kozyrev and many others interpreted this as eruption of gases and a sign of volcanism

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on the moon. Yet, as I have pointed out on several occasions (*5*), inspection of details of the published spectrograms (*6*) *along the slit* or across the dispersion shows that no luminescent gases were emitted but that the luminescence was immovable and strictly confined to a 4-kilometer-wide area of the sunlit peak for all the 30 minutes of exposure, with no trespass into the sharply defined (to 1 kilometer) shadow. It was amazing though understandable that eminent scientists argued about the details of the spectrogram along the dispersion without paying attention to its appearance at right angles to the dispersion. However, after the point had been raised, everyone could easily see for himself; the claim that a gaseous eruption had taken place goes against an indisputable observational fact. Yet Fielder, to whom the point is known, still maintains that the observation is most naturally explained as a volcanic phenomenon (*2*, p. 166). On the same page there is a table giving the age of lunar maria as a mere 100 million years, as compared with 4.5×10^9 years for the age of the moon; this fantastically low figure is based on the crater density, arbitrarily assumed to be proportional to age. That the production rates of pre-mare craters (whether volcanic or impact) may be higher by orders of magnitude than the rates for the post-mare craters (the impact theory of the origin of the moon suggests a ratio of 10^9) is ignored. The assumption of constant rates just makes no sense. That the crater density in the maria agrees with the expected number of impacts over 4.5×10^9 years (*7*) is ignored in favor of an arbitrary and primitive calculation (*2*, p. 50) which gives a number of impacts one-tenth the number of craters observed in the continentes (a ratio which is irrelevant) but five times more than in the maria (a ratio which is ignored). Thus, Fielder's calculation leads to too many impact craters, not too few, as he wishfully concludes, taking the continentes as a standard of comparison.

Terrestrial calderas are considered by Fielder to be the prototype of lunar craters. A glance at the irregular outlines of the calderas (*8*), in contrast to the round (or regular polygonal, in some cases) outlines of lunar craters, should make the assumption dubious, to say the least. To see a similarity between the calderas and typical lunar craters requires a good deal of wishful thinking.

It is obvious that lava or ash flows and other volcanic phenomena occurred on the moon during the first million years of its existence, and maybe later, too. Probably they were produced or triggered by impacts of the "planetesimals" which built the moon and gave their finishing touch to its surface. Volcanism, even when secondary, is therefore a legitimate topic for selenographic study. It is highly desirable that someone with realistic physical and mathematical insight put together a coherent picture of the possible primeval volcanic phenomena of the moon, without arbitrary improbable assumptions or wishful disregard for facts. It will be a hard task to prove that some of the craters on the lunar continentes (not the maria) are endogenically volcanic, if this is in fact the case. Until we have such a proof, it is safe to work on the impact theory, which probably accounts for the overwhelming majority of craters and which leads to a plausible, noncontradictory picture of the origin of the moon at a distance of about 5 earth radii (*9*) and the subsequent evolution of its orbit and surface. The picture may be incomplete or even wrong, but to refute it would require more than the accumulation of *ad hoc* products of wishful thinking.

This does not mean that Fielder's work on lunar features is not appreciated. Even if it is guided by the wrong kind of ideas, it may yield useful and unexpected results. Wishful thinking has always been the stimulus of Western civilization; it led Christopher Columbus westward, and his discoveries are not the less important because he miscalculated the size of the earth and did not reach the lands of Eastern Asia as he intended. A working hypothesis, even an erroneous one, is better than none; it sets goals and leads to discoveries which could be missed on a more orthodox course. On the other hand, I for my part prefer a frame which is internally consistent and as free as possible from arbitrary assumptions; in my work, I cannot yet see where the volcanic theory would usefully apply. Our yardsticks of fact and fancy are so different that no useful dialogue can result.

As to Mars, too little is yet known of its surface features, but what is now known about its craters suggests complete analogy with the moon, the ancient Martian round impact craters, however, being more worn by erosion. As to the possible role of Martian volcanism in the past (as distinct from crater formation), the amount of nitrogen, less than 0.5 percent of the terrestrial amount per unit area, is a reliable indicator. Nitrogen is not easily removed by chemical reactions, nor does it noticeably escape to space from Mars. Its amount is thus a measure of outgassing and gives an upper limit for magma (lava) which has been in contact with the Martian atmosphere since the formation of the crust. Furthermore, the small amount of outgassing is readily accounted for by the impact destruction of the upper surface layer of the Martian crust, so that very little, if anything, is left over as evidence of genuine volcanism on Mars. For the moon, such a method cannot be applied because nitrogen escapes, but, since the moon is a smaller body, its volcanism is expected to be less by orders of magnitude than that on Mars, or completely negligible.

Regarding the interpretation of some Martian features as "leeward clouds," in making my remark I had in mind my own experience as an observer of Mars from 1911 till 1958—which I tried to conduct as critically as possible. The experienced observers to whom Wells refers may certainly have seen something somewhere, but to identify "leeward clouds" on Mars from these observations is rather far-fetched. Besides, water vapor is there too scarce to form observable clouds near the surface.

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