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WATER ON THE MOON

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The problem of long-term operations on the moon is a problem of resources. On earth human beings use more water than anything else, by far. There is plenty of oxygen in lunar soil and rocks, and solar wind hydrogen in the lunar soil is an obvious though expensive source of the other component. Thorough outgassing would yield hydrogen for one liter of water from a few tons of soil.

The real water mines on the moon are almost certainly to be found, as Watson, Murray and Brown point out (J. Geophys. Res. 66, 3033-3045, 1961), in the permanently shadowed regions near the poles. They estimate a steady-state temperature $< 120°$ K, but it is probably much lower than that. It the heat flux from below and solar wind energy input are the dominant terms, as seems reasonable, the temperature must be of the order of 40°K. At such temperatures not only ice but other volatiles, even CH_4, should be retained. These unseen features, at least 2×10^5 km^2 in area, and extending down to at least 60° latitude, are the repository of a significant fraction of all the water vapor emitted from the moon since the poles reached their present position.

The bombarding meteorite flux has two effects: it evaporates some solids (which in part re-condense), and it buries material beneath the surface.

The mass of volatiles so trapped is of course very uncertain. It seems unreasonable to suppose that the orbital plane of the moon has been fixed since the earliest stages of melting and differentiation, or even during and since the period of mare basalt deposition. However, it may not be too radical to postulate that the poles have been approximately fixed with respect to the sun since about 3×10^9 years ago. Nearly all the major topographic features producing the shadows were formed before this. If such "recent volcanic" events as the formation of the Aristarchus plateau and the Marius Hills have taken place since then, I estimate (guess) that the deposited ice at the poles may exceed 100 km^3, or 10^{11} tons of H_2O. This would be a layer of the order of one meter in thickness. If present, it is vastly larger than any imaginable mass of water which could be carried by humans from the earth. Other sources of H_2O, such as comet ice released on impact with the moon, and re-emission of solar wind hydrogen as H_2O, are also capable of making major contributions. The uncertain status at present of observations of lunar transient events raises a question about inclusion of this potential source.

The detection of this icy material, and measurement of its regional distribution, can be accomplished by the gamma-ray chemical mapping experiment proposed by the author and his colleagues for the Lunar Polar Orbiter (recently renamed Terrestrial Bodies Orbiter-Lunar). The capture of neutrons by ^{1}H produces a characteristic 2.22 MeV line. Such a study must of course precede attempts at exploitation.

The recovery of the ice should be easy, since it is located at or near the surface. Impact gardening has mixed the lunar surface to a depth of a few meters in the last 3×10^{9} years here as elsewhere, and one may guess that the surface material will be "dirty ice" rather than icy dirt. In the absence of direct sunlight energy for mining and extraction must be supplied from the outside.

The two resources required in largest amounts for human operations on earth are water and soil. The area of the moon is comparable to that of Columbus's New World, so soil is no problem. Colonists must in any case treat water as precious, but the presence or absence of these polar ice caps may still be decisive for the practicality of colonies, either on the moon itself, or at the lunar Lagrange points as proposed by O'Neill. Other resources such as carbon, nitrogen, oxygen and iron are unlikely to be a serious problem, if the abundant solar energy can be suitably exploited.

DISCUSSION - (Arnold Presentation)

SPEAKER 1: In their (Watson et al) calculation of the stability of water in this shaded region against thermal, solar insulation, did they calculate or take into account the energy deposition due to impacting particles?

ARNOLD: No, they did not. I've thought about it a little bit. My impression, and I haven't really solved the problem, is optimistic for the following reason. If I'm right about the 40 degrees K, then the volatility of water at that temperature isn't grossly different from the volatility of silicates at room temperature. What I expect would happen if there were an impact is, just as in the case of craters in soil, there would be a vaporization of a few times the projectile mass and then perhaps a melting of another similar layer and then thousands of times as much material excavated and thrown about. On that basis, there would be a small amount of water lost, some of which would recondense in this very cold area, some of which would be lost in space. The surface of the soil, if there were a lot of very small scale impacts (and, of course, the small particles dominate), would rather quickly be covered with a layer of soot or debris, which would be an effective thermal shield against any but the larger impacts. This is something people need to study, but I would bet anybody that the answer will be favorable.

SPEAKER 2: Didn't some of the Apollo missions go to areas that were shielded, for example, around boulders and areas where you would expect to see meters of water or ice as you would suggest?

ARNOLD: No, the Apollo missions were all confined to near equatorial latitudes. On Apollo 16 they went under a place called Shadow Rock to collect some permanently shadowed soil. There are a lot of things about that that are completely different. The rock itself reaches a daytime temperature of the order of 400 degrees K and reradiates. The lateral distance is of the order of a meter for thermal conductivity rather than a kilometer and diffusion goes as the square of the distance. Besides, the region they happened to select was not permanently shadowed. I don't think they had the opportunity to do a real test of this hypothesis. The large permanently shadowed regions do extend down to about 60 degrees latitude. There are a few places where there are high walls, but none of those have been sampled. There were high-inclination lunar orbiters that have been up there, but, of course, they're photographic sensors and they don't see the dark places.

SPEAKER 2: Could you say again how you compute 10^{11} tons as being the total amount there would be and the few meters depth that you estimate?

DISCUSSION (Arnold Presentation)

ARNOLD: All I did was to say there is an area about $2 \cdot 10^5$ square kilometers (which is like Lake Superior or the Caspian Sea). I then made a calculation based on an absolute guess as to the degassing rate. Aristarchus degassed so much material and then I used Watson, Murray, and Brown's ballistic calculations that about 1 percent of it, a fraction roughly equal to the area of traps, will arrive in these cold traps by ballistic degassing. You can read in their paper what I think is a sound argument. So, it was on that basis that I arrived at a guess, which I happen to think is a rather conservative guess, as to the amount of material there.

SPEAKER 3: Given the amount and extent of this ice deposit, and the known rate of impacting of objects of various sizes, should there have been transient events detected by mass spectrometers emplaced? That puts a limit of sorts.

ARNOLD: I haven't frankly thought that either. I would kind of doubt it.

SPEAKER 4: I've done that kind of calculation that I'm supposed to be talking about over at LSI this afternoon. I'm going to mention it before their panel discussion this evening. But I did calculate for an outgassing event like at Aristarchus what you would have to have in order for it to have been detected by the ALSEP instruments. And, it turns out that an event like Kozereff has suggested and has been reported for a TLP, that SIDE would have detected it, had one occurred in the last few years. Also, as far as numbers go, if there's an impulsive event of a few hundred kilograms to maybe 1000 kilograms, it would have been detected by the ALSEP experiments (R. Vondrak).

SPEAKER 5: If I've read all those abstracts right, they say nothing's (transient events) happened. So the question I'd like to ask you, Jim, is since all of our lunar samples are devoid of water, why should we think that there's any water in the youngest volcanic materials. There's gas, but there's no evidence it was water.

ARNOLD: We know what the primordial material is. We know what the meteorites are. We know what the cosmic abundances are and we start with an object that was made from that material. The only question is, how completely the Moon did fractionate in the various processes of its formation, the large amounts of water that were originally present. Indeed, one sees things now which are very dry. It's obviously much easier to degas the outside of an object than the inside of an object. I'm simply, Bill, relying here on a chemist's intuition that the last traces of water are hellishly difficult to get out anything. Take the glass in a piece of Pyrex - the amount of water contained in a piece of Pyrex tubing in a vacuum system is of the order of the sort of thing I'm talking about; of course, that Pyrex was formed in an Earth environment. But, it may be that all the water was degassed down to a part per million or a tenth of a part per million. I would regard that as exceedingly strange, but the world, of course, doesn't have to behave as my intuition dictates.

DISCUSSION (Arnold Presentation)

SPEAKER 6: I'm glad you raised that Watson, Murray, and Brown paper because I feel it's possible to criticize it on several grounds in the light of what we've learned since 1962. First, as for these allegedly permanently shadowed regions, this assumption of their permanence assumes that the Moon's obliquity had remained unchanged in geologic time and recent work by Bill Ward at SAO shows that this is not the case and that the lunar obliquity in remote ages was very markedly different from what it is today. And a second major criticism, of course, is the fact that, in view of the degassing of the returned lunar samples, it's not entirely clear that there is water of hydration anywhere. And in particular, Watson, Murray, and Brown proposed that serpentine and other hydrated rocks would be found in the vicinity of wrinkle ridges, which they attribute to volcanic activity. Again, it's not clear in the light of returned lunar samples that we would find serpentine or similar rocks there or anywhere. Perhaps you could comment on these points.

ARNOLD: The last one seems to me trivial, I must say. Of course, these people in 1961 had no idea of the lunar chemistry and were bound to make wrong guesses as any one of us would have. The question of whether indeed the rocks were completely degassed, I think we've been around on. Admittedly, that is a source of great uncertainty. Admittedly, the Moon is much drier than they thought it was. The numbers I've given can be criticized as quesses, but I don't think there's any information either to confirm or deny them at present. As far as the obliquity of the pole, I certainly hope that I'd taken that into account. Had I assumed that the poles had been in their present position 4.5 billion years ago, I would have been very much more optimistic than I am because there certainly has been an enormous amount of differentiation and evolution, thermal processes in that earlier period. I took as a ground rule that the pole had arrived at its present position only after the major mare events had taken place. If that's false, then the story is very much strengthened.