three samples no hemoglobin was detected in the zone corresponding to the adult fractions A and A_2 (Fig. 1). Traces of hemoglobin A could be detected by agar-gel electrophoresis in a fetus of approximately 4 months gestation and 150-mm crown-rump length.

Obviously, the component which moves on starch gel like hemoglobin A is due to a different hemoglobin. At this moment we can offer no further data on the nature of this fraction. Starch-gel electrophoresis may be insufficient to distinguish clearly hemoglobin A from certain other hemoglobins, especially some fetal components. As in the present instance, agar-gel electrophoresis may then be of particular value.

Furthermore, the methods of alkali denaturation or hemoglobin elution are relatively insensitive for identifying small amounts of nonfetal hemoglobin; thus the question of the exact time at which synthesis of β-chains begins remains open. It appears, however, that no β-chains are formed in small embryos whose crown-rump length is less than 100 mm (*6*).

ANASTASIA KALTSOYA
PHAEDON FESSAS
Hematology Section and Laboratory
ARISTOTELIS STAVROPOULOS
Department of Obstetrics and Gynecology, Alexandra Maternity Hospital, Athens 611, Greece

References and Notes

1. F. Hecht, A. G. Motulsky, R. J. Lemire, T. E. Shepard, *Science* **152**, 91 (1966).
2. E. R. Huehns, N. Dance, G. H. Beaven, J. V. Keil, F. Hecht, A. G. Motulsky, *Nature* **201**, 1095 (1964); E. R. Huehns, N. Dance, G. H. Beaven, F. Hecht, A. G. Motulsky, *Cold Spring Harbor Symp. Quant. Biol.* **29**, 327 (1964).
4. C. A. J. Goldberg, *Clin. Chem.* **4**, 484 (1958).
5. A. R. Robinson, M. Robson, A. P. Harrison, W. W. Zuelzer, *J. Lab. Clin. Med.* **50**, 745 (1957).
6. A. Karaklis and P. Fessas, *Acta Haematol.* **29**, 267 (1963).
7. Supported by the Royal Hellenic Research Foundation and the USNIH (HE 04541-06).

14 July 1966

Hot Shadows on Jupiter

R. L. Wildey has reported a series of intriguing observations in which shadows cast on Jupiter by its satellites have been found hotter at times than their surroundings (*1*). The enigma has been attributed to complicated processes resulting in greater atmospheric transparency, to chemical reactions, and even to magnetohydrodynamics. I feel that there is a much simpler explanation based on a familiar phenomenon.

A clean liquid may be cooled, with care, considerably below its normal freezing temperature. When the liquid finally solidifies the temperature rises abruptly, and it may reach the melting point. The clouds on Jupiter are believed to consist of ammonia, for their infrared reflection spectra do not match ice clouds. The normal freezing temperature of ammonia is 195.5°K.

The usual temperature of the Jovian clouds is about 128°K. Wildey reports that the hot shadows of Ganymede and Europa reached 184.5°K and 191°K, about what would be expected if the sudden passage of the shadows caused enough momentary cooling to trigger the *freezing* of supercooled ammonia cloud droplets.

The effect would not always be present. For it to occur, there must be an adequate number of supercooled liquid droplets carried to the tops of the Jovian clouds by convection. Presumably each of the cloud bands visible on Jupiter has a convectively ascending edge and a convectively descending edge, and supercooled droplets should be more abundant near the ascending edges. The convective motions might be studied by monitoring the shadow temperatures.

Supercooled water droplets account for another puzzle, the observed equality of the light and dark hemisphere cloud temperatures on Venus (*2*). On the dark side, the heat radiated is supplied by the gradual release of latent heat as cloud water freezes. If Venus had a moon we would no doubt observe that its shadow upon the clouds was also hotter than the surrounding clouds, perhaps more consistently than Wildey has observed the effect on Jupiter.

WILLIAM T. PLUMMER
Laboratory of Astrophysics and Physical Meteorology, Johns Hopkins University, Baltimore, Maryland

References

1. B. C. Murray, R. L. Wildey, J. A. Westphal, *Astrophys. J.* **139**, 986 (1964); R. L. Wildey, *Icarus* **3**, 332 (19 *Science* **147**, 1035 (1965); *Astrophys. J.* **144**, 1241 (1966).
2. M. Bottema, W. Plummer, J. Strong, R. Zander, *J. Geophys. Res.* **70**, 4401 (1965); W. T. Plummer and J. Strong, *Astronaut. Acta* **11**, 375 (1965).

21 June 1966

W. T. Plummer has offered an interesting physical interpretation of my observation that eclipsed regions of Jupiter's atmosphere radiate more at thermal wavelengths than do their surroundings, and do so by a mechanism dependent on some parameter which varies with time (*1*). In defense of his hypothesis put in familiar terms, it must be noted that the mechanism he offers is physically explicit and not *ad hoc* as have been most of my own (*2*). Nevertheless, what is offered is by no means a panacea, for the following reasons.

1) The usual temperature of the Jovian clouds is not 128°K, but more nearly 170°K, as determinations of rotational excitation temperature in the visible indicate (*3, 4*). The low brightness temperature observed at 10 μ requires that matter exists above the clouds whose optical thickness in the visible is negligible, yet of sufficient opacity at 10 μ that our "vision" at that wavelength only penetrates this outer atmosphere deep enough to record a brightness temperature of 128°K. If we do not succeed in producing a transparency in this upper layer, it makes no difference what transient temperature effects occur in the cloud layer because the radiation transients thus produced will not penetrate this outer layer, whose own thermal response to the transient can only be on a very long time scale. Trafton (*4*) has found that hydrogen, known to be above the cloud layer, has the necessary opacity in the form of rotational and free-free transitions in the field interactions of the pressure-induced dipole of H_2 molecules and the intermolecular potential in H_2-He and H_2-H_2 mixtures. Overlying hydrogen will obscure rises in cloud temperature regardless of differences between kinetic temperature and rotational temperature.

2) I have, in fact, monitored the infrared radiation from the satellite shadows, and the published results (*5*) show that when the phenomenon is absent, it is absent regardless of the position of the shadow on the disk, thus in no way depending on the eclipse taking place on a convectively descending cloud edge, as opposed to an ascending edge.

3) Although ammonia has an absorption band completely enveloping the 8- to 14-μ band, its discrete components are extremely sharp, even in the solid state (*6*), so that a "Rosseland" mean opacity over this band would be very low. In the visible, of course, the ammonia opacity is due to pure scattering, which would probably be negligible at 10 μ. It thus appears that even if ammonia were all we could see at 10 μ,

very little of the radiation would be coming from the uppermost part of the cloud in which the vast majority of the solar radiation is absorbed. Thus very little of the emergent infrared would be representative of a source function which was at all effected by temperature changes due to a change-of-state.

4) If freezing were to be locally triggered at the passage into shadow, the entropy would increase and the system would not return to its original state on the return of sunlight. The ammonia ice would remain ice and would merely cool down to a reasonable equilibrium temperature. There is no elongated trail of enhanced infrared radiation, observed to follow the shadow as it passes across the Jovian atmosphere, on those occasions when the phenomenon is observed to take place. Yet the entire shadow passes in about 15 minutes. A highly improbable cooling curve is thus required of the ammonia ice.

ROBERT L. WILDEY*
Mount Wilson and Palomar Observatories, Pasadena, California

References

1. R. L. Wildey, *Science* **147**, 1035 (1965).
2. ———, *Icarus* **3**, 332 (1964); *J. Geophys. Res.* **70**, 3796 (1965).
3. F. R. Zabriskie, *Astron. J.* **67**, 168 (1962); H. Spinrad and L. M. Trafton, *Icarus* **2**, 19 (1963).
4. L. M. Trafton, *Astrophys. J.* **140**, 1340 (1964).
5. R. L. Wildey, *ibid.* **144**, 1241 (1966).
6. C. G. Cannon, *Spectrochim. Acta* **10**, 426 (1958); H. M. Mould, W. C. Price, G. R. Wilkinson, *ibid.* **15**, 314 (1959).
7. S. B. Nicholson, *Astron. Soc. Pacific Leaflet No. 408* (June 1963).

* Present address: Center of Astrogeology United States Geological Survey, Flagstaff, Arizona.

11 July 1966

"Dust" on the Moon

In 1955 Gold (*1*) discussed many features of the moon's surface, suggesting that the maria are not composed of lava flows (pumice or basalt) but are filled with dust produced by erosion processes and transported from mountainous areas over the surface of the moon by a "fluidization" process. Gold's suggestion has been discussed by many scientists and in the press, and in many instances his ideas have been misrepresented. (I am not entirely without sin in this respect.) In particular, it is generally assumed that Gold suggested that great depths of fluid dust filled the maria and that space ships would sink out of sight in these fluid seas. In fairness to an eminent and very intelligent colleague, critics of Gold should read his paper. Briefly, three points are important in this connection: (i) the origin of the dust, (ii) the method of transport, and (iii) the physical strength of the pool of dust.

Gold suggested that the finely divided material was produced by the actions of electromagnetic and particle radiation on the surface rocks and by collisions of micrometeorites; he presented what appeared (and still appear to me) to be cogent arguments against lava flows. He assumed that this dust had flowed from mountainous regions to the lower areas by a fluidization of a thin surface layer by various physical processes, and that this process has filled the maria to great depths. He did not, however, suggest that the deep layers of dust have low physical strength. He stated:

> There appear to be some very steep slopes in the filler material. In particular there are the small rills which possess very steep sides. This is not contrary to the known behaviour of dust. At a depth of more than a few metres the compaction under the overlying weight would suffice to convert the material effectively into a solid; in the absence of an atmosphere grains stick together with intermolecular forces with even less compaction than is necessary when atmospheric molecules intervene. If the material can be regarded as resistant to internal flow but possessing a thin fluidized surface layer, then the filling up of steep holes or gaps would proceed at a speed limited by the fluidization process, but the steep sides would persist until the filling up is complete. It is not a requirement that such rills should survive for a great length of time; Baldwin speaks of them as the youngest features on the Moon, yet has to add that some appear partly "filled with lava." These rills are to be thought of within the framework of the present interpretations as the small signs of the small isostatic adjustments that keep occurring as the maria fill up with dust and the highlands are denuded; and these rills are in turn comparatively quickly filled in. Their distribution mainly around the edges of the great maria is entirely in accord with this interpretation.

My own ideas regarding the maria were greatly influenced by Gold's, even though I disagreed with certain features of his arguments (*2*). I did not and do not now believe that the maria were filled by erosion from mountainous areas, but rather that the great collisions were dominant in the production of fragmented material. This belief does not exclude the possibility that Gold's fluidization process for a thin surface layer has been effective to some degree, but it is that the great collisions were much more effective in distributing the fragmented material over the surface.

In my criticism and extension of Gold's ideas I specifically mentioned evidence from the circular walled plain near Flamsteed, within which Surveyor 1 landed; I pointed out that this nearly buried large crater is not distorted in a manner to be expected if a great lava flow had moved over Oceanus Procellarum from some one direction. Moreover, the crater Prinz is filled with what appears to be fragmented material covering one wall and sloping smoothly down the interior and outer ramparts in a way that one hardly expects to result from liquid or *dust* flow—but that might be expected from unequal settling of fragmented material from a temporary atmosphere. Other craters suggest similar conclusions.

One argument of Gold appears to be especially convincing: that the many partially filled craters in mountainous areas and the many smooth areas between them could hardly result from separate lava pipes coming from deep in the lunar interior. But erosion from the surrounding mountains (as Gold suggests) or the falling of fragmented material from above (as I prefer to believe) constitute reasonable mechanisms for producing some of the smooth interiors of craters. There are some troublesome points: Wargentin is filled to the brim with smooth material that may possibly have fallen from above, or indeed it may have been filled by a lava flow; but the fill could hardly have been supplied by erosion of the rim material. Moreover, the flow of dust to the maria should have left some indication of paths by which dust moved from mountains to maria.

The idea that the lunar surface consists of fragmented material, the cosmic-ray ages of meteorites, the curious types of mixtures of fragmented materials in these objects, and their so-called polymict character led to the suggestion that stone meteorites may come from the moon (*3*). This suggestion has not been generally accepted but recently has been discussed seriously by some.

Another source of the mare material was suggested in 1964 (*4*): the flow of finely divided gas-laden solids from the lunar interior, resulting from the great collisions that produced the maria.