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Dynamical Arguments Which Concern Melting of the Moon

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Dynamical arguments which concern melting of the Moon

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This paper points out that the observed differences of the moments of inertia of the lunar globe about its principal axes – determined astronomically and verified more recently by laser rangings – are inconsistent with the assumption that the whole Moon was ever covered by a global layer of molten material, extending to a depth of a few hundred kilometres. Moreover, laser determinations of the shape of the Moon (along the tracks overflown by Apollo 15 – 17 missions) make it quite clear that the Moon's surface did not solidify from a global ocean of lava even 10–20 km deep.

Therefore, any melting which occurred on the Moon (and produced the observed chemical differentiation of the crustal rocks) could have taken place only *locally* – over areas of the size of the lunar maria, but *not* over the Moon as a whole at the same time.

The first arguments which could throw light on the internal structure of the Moon emerged – long in advance of the space-age – in the guise of the 'physical librations' of our satellite, the existence of which was predicted already by Newton in volume 3 of his *Principia* in 1687. The actual range of such motions proved, however, to be so small (their selenocentric amplitudes do not exceed 2′) as to remain observationally undetectable for more than $1\frac{1}{2}$ centuries following Newton's time. Throughout the eighteenth century the observations by Mayer and Lalande failed to detect any significant displacement of the apparent positions on the Moon which could be due to this cause; and the same was true of subsequent efforts by Bouvard and Arago, undertaken at the encouragement of Laplace.

The reasons why these efforts remained unavailing for so long a time were mainly instrumental. At the distance of the Earth, a selenocentric amplitude of physical libration amounting to 2′ corresponds to a displacement of $2'/221 = 0.54''$ (where the factor 221 stands for the mean Earth–Moon distance expressed in terms of the lunar radius); and to detect such displacement required telescopic techniques which were not introduced into astronomical practice much before the middle of the nineteenth century. Success in the age-long quest for the detection of physical librations of the Moon finally came with Bessel (1839) and his school at Königsberg, who utilized for this purpose a heliometer – a telescope with split objective, particularly suitable for accurate on-axis measurement of large angles. A telescope of this type was, to be sure, no invention of Bessel's; and Lalande used it already in the second half of the eighteenth century to measure the apparent diameter of the Sun (hence, its name). However, Bessel and his followers were the first to apply it for studies of lunar librations; and their efforts were continued for the next hundred years or so at several observatories in the world (mainly in Germany and Russia) which specialized by tradition in this work. At present the only institution at which visual heliometer is still in active use is the Engelhardt Observatory in Kazan, U.S.S.R.; while photographic techniques pioneered in more recent years independently by several investigators (Habibullin 1958; Moutsoulas 1970, 1972) failed as yet to produce results of weight comparable with those of the visual work of their predecessors.

A dynamical theory of physical libration of the lunar globe has, since the days of Newton and Lagrange, been developed by many investigators – among whom the names of Hayn (1923), Koziel (1948, 1949, 1957), Jeffreys (1957, 1961), Eckhardt (1965, 1967) and Mietelski (1967, 1968) may, in particular, be mentioned in this connection. For a reduction of the heliometric observations of lunar librations and their interpretation in terms of the physical parameters involved in the underlying theory, of fundamental importance remains the work of Koziel (1967*a*, *b*).

Of the parameters specified by the periods and amplitudes of lunar physical librations, of paramount importance are the ratios

$$\alpha = \frac{C-B}{A}, \quad \beta = \frac{C-A}{B}, \quad \gamma = \frac{B-A}{C}, \tag{1}$$

where A, B, C denote moments of the Moon about its principal axes of inertia. The value of β can be determined, in principle, from the amplitudes of the physical librations (i.e. from the inclinations of the Moon's orbit and equator to the ecliptic, and the rate of regression of the nodes of lunar orbit); and that of γ, from their observed periodicity; while the value of α corresponding to known β and γ can be evaluated from the identity

$$\alpha-\beta+\gamma = \alpha\beta\gamma. \tag{2}$$

The facts that the differences $C-A$, $C-B$ or $B-A$ (and, therefore, the ratios α, β or γ) must be different from zero is amply attested by the observed synchronism between rotation and revolution of our satellite; for if the shape of the Moon were a sphere – with a spherically-symmetrical distribution of matter in its interior – the Earth's attraction would be powerless to effect synchronization. The existence of the latter cannot, to be sure, by itself specify the actual amounts by which α, β or γ may differ from zero; this can come out only from a study of the librations performed by the distorted globe of the Moon about its centre of gravity. The arduous task of determining the values of β and γ from long series of heliometric observations (extending over almost one century) was carried out in 1962 at Manchester by Koziel (1967*a*, *b*), with the results disclosing that

$$\left.\begin{aligned} \alpha &= 0.000398 \pm 0.000008 \text{ (m.e.)},\\ \beta &= 0.000629 \pm 0.000001 \text{ (m.e.)},\\ \gamma &= 0.000231 \pm 0.000006 \text{ (m.e.)}.\end{aligned}\right\} \tag{3}$$

It was noted many years ago (cf., for example, Jeffreys 1924) that *the observed values of α, β and γ are completely at variance with those to be expected if the lunar globe were in hydrostatic equilibrium* under the field of force to which the Moon could have been exposed. In particular, the actual value of β proves to be approximately 17 times as large as a hydrostatic one under the prevalent field of force; while α and γ are 42 and 8 times as large, respectively.

This is true, to be sure, at the present distance of the Moon from the Earth. This distance need not have remained constant throughout the long astronomical part of our satellite; and could once have been very much less – which would have influenced the absolute values of A, B and C. But regardless of the possibility of bringing any one of the observed values of α, β or γ in agreement with hydrostatic theory by allowing the Moon to have acquired its form in closer proximity of the Earth, the requisite proximity does *not* turn out to be the same for all three. In other words, the *ratios* of the observed lunar values of α, β and γ prove to be inconsistent with the existence of hydrostatic (or lithostatic) equilibrium at *any* distance of the Moon from the Earth.

In order to demonstrate this, let us form the ratio

$$f \equiv \frac{\alpha}{\beta} = \frac{B}{A}\left(\frac{C-B}{C-A}\right). \tag{4}$$

In hydrostatic equilibrium, this should be (cf., for example, Kopal 1969; p. 88) equal to

$$f = \frac{m_{\oplus} + m_{☾}}{4m_{\oplus} + m_{☾}}, \tag{5}$$

where $m_{\oplus}$ and $m_{☾}$ denote the masses of the Earth and the Moon, respectively; and this equation should hold good regardless of the Moon's distance or internal structure. As, moreover, the latest value of the ratio $m_{\oplus}/m_{☾}$ is equal (cf. table 3-1 of Kopal 1974) to 81.302 ± 0.001 (m.e.), the 'hydrostatic' value of f as given by equation (4) should be equal to 0.2523; whereas the observed value of f consistent with Koziel's values of α and β is equal to

$$f = 0.633 \pm 0.006 \text{ (m.e.)}; \tag{6}$$

and some previous investigators (e.g. Gorynia 1965; Habibullin 1966) have put it even higher.

These values are so much at variance with the requirements of hydrostatic equilibrium that the discrepancy must be regarded as real. Several thoughts should be considered in this connection. On the observational side, it is true that Koziel's results are based on heliometric measurements made with instruments of apertures smaller than 17.5 cm; and their Rayleigh limits of resolution in visible light was, therefore, less than 0.8″. In order to obtain, from such measurements, results significant to 0.1″ (as quoted by heliometric observers); the mean of a great many individual settings must be made on the assumption that no systematic errors are present to impair their mean to this accuracy.

Positional astronomers have, to be sure, long been accustomed to search for the desired information inside optical diffraction patterns of their objectives; and have done so extensively when measuring, for example, stellar parallaxes. The success of such a process requires, however, a knowledge of the geometrical relation of the actual shape of the light source to that of its diffraction image (i.e. a point to a disk in the case of a star) in addition to a great many individual measures to minimize their accidental errors. In the case of selenodetic measurements the first condition cannot, unfortunately, be met; for the actual shape of lunar details on which heliometric settings are made are not known to us *a priori*; and neither is, therefore, the form of their diffraction image (which may, moreover, vary with the phase as a result of different illumination).

Under these conditions, many of us have been veritably holding our breath until the result of heliometric studies of lunar librations could be confirmed by independent methods. This has come to pass since the landing on the Moon of Apollo 11 in July 1969, when a cube-corner retro reflector for laser signals has been installed on the shores of the lunar Mare Tranquillitatis; followed by similar devices (of improved design) installed by Apollo 14 and 15, or the unmanned Lunas 17 and 21, in the proximity of their landing places. Observed echoes of laser pulses flashed on the Moon from the Earth and returned by these devices – permitting determinations of the instantaneous distance between the terrestrial transmitter and the particular cube-corner reflector on the Moon from the time-delays of the respective light-echoes with a precision of the order of one part in 10^8 – have opened a new epoch in several branches of lunar studies – including that of lunar librations.

At present these laser-tracking studies are still continuing; but the results already published (cf. Bender *et al.* 1973; Williams *et al.* 1973) led to the values of

$$\left.\begin{aligned} \alpha &= 0.0004043 \pm 0.0000011, \\ \beta &= 0.0006311 \pm 0.0000004, \\ \gamma &= 0.0002268 \pm 0.0000010, \end{aligned}\right\} \tag{7}$$

virtually identical with those derived from ground-based studies of lunar librations by means of terrestrial telescopes; their mean errors are somewhat smaller, but of the same order of magnitude. The internal agreement between these two sets of the data obtained by completely different means (and subject to entirely different types of errors) is indeed excellent, and inspires full confidence in the correctness of the results. It vindicates brilliantly the patient efforts of investigators like Ernst Hartwig (1851–1923) who dedicated a major part of their lifetime to this exacting task; as well as the skill which inspired investigators like Koziel to extract the requisite information from long series of observations inherited from bygone days. The feat is reminiscent of Chapman's success (cf. Chapman 1918) in the detection of lunar atmospheric tides in the terrestrial barometric data, whose individual errors were 10–100 times as large as the quantities sought after.

The reality of the discrepancy between the hydrostatic and observed values of the ratios α, β, γ of the momenta is, therefore, undoubted; and testifies to the extent by which the Moon must depart from hydrostatic equilibrium somewhere in its interior. Moreover, it is easy to show that the region responsible for it cannot be located too far from the surface; for the deeper interior is very ineffectual for this purpose. According to the latest models of this interior consistent with known seismic data, an outer shell of the Moon 200 km in depth contains 31 % of the Moon's mass, but accounts for almost 46 % of its total moment of inertia. In addition, as a depth of 200 km the lithostatic pressure attains a level of about 10 kbar (1 GPa) – comparable with one at which rocks of lunar composition, at moderate (let alone elevated) temperatures, can no longer behave as rigid. Therefore, we must expect that the core of the Moon extending up to more than 1500 km from its centre should be reduced by pressure to the state of hydrostatic equilibrium; and departures from this equilibrium could be tolerated only in the outer and relatively thin zone, in which lithostatic pressure becomes less than 10 kb.

But if so, it follows that *it is this outer shell which must be primarily responsible for the bulk of the observed anomalies in the lunar moments of inertia*; and for the fact that the ratio f as given by equation (4) lies between 0.6 and 0.7 rather than being close to 0.25. Needless to stress, *no shell so constituted could have acquired the observed properties if it ever solidified from a liquid state* – at whatever distance from the Earth and at whatever time. The conclusion seems, therefore, inescapable that it has never been molten as a whole. Such melting as may have taken place on the lunar surface from time to time (cf., for example, Toksöz *et al.* 1972*a*, *b*) to bring about the observed chemical differentiation of lunar rocks (if these did become differentiated on the lunar surface, and not in their pre-lunar past) must have occurred at a relatively shallow depth and be severely localized – without much influence on the large-scale distribution of mass inside the Moon.

It appears more probable to an astronomer that a conspicuous disparity between the observed and hydrostatic differences of the moment of inertia could – partly or wholly – *be a consequence of the initial irregularities in accretion of the lunar globe* as a whole. That this accretion could have led to the formation of a body lacking strict radial symmetry is natural enough; for the converse would indeed have been more surprising for an astronomical body which grew up by accumulation of solid particles of macroscopic size. But how to explain then the igneous nature of at least the rocky layer deposited on the Moon at the end of the formative period of our satellite (we still know next to nothing about chemical composition of material more than 100 km below the surface), much of which is 4.6 Ga old? One explanation would be to assume that at least *the outermost layer surrounding the lunar globe consists of material which was chemically differentiated already prior to its accretion.*

A tentative suggestion of this nature does not, unfortunately, lend itself readily to any cosmochemical or petrographic checks. The gravitational field – which would have been very small for differentiation occurring in the pre-lunar past, and virtually the same as that prevalent now on the lunar surface (if this is where the lunar crystalline rocks we possess last solidified) – does not seem to influence the chemical composition or crystal structure of these rocks to any appreciable extent.† We also possess still next to no detailed information on the 'anatomy' and time-scale of the actual accretion; or have no clear idea on whether the chemical differentiation exhibited by lunar surface rocks is merely skin-deep, or extends well into the interior of our satellite. As long as this is the case, it may be unwise to insist that all lunar rocks must have acquired their present observed characteristics only after they became parts of the Moon, and to close our eyes to alternative possibilities.

But let us return again to the dynamical limitations on the depth of any hypothetical molten layer on the Moon, imposed by departures of the observed values of the ratios α, β and γ o lunar moments of inertia from those appropriate for hydrostatic equilibrium; and inquire as to the maximum depth of such a layer consistent with known dynamical evidence. Is there no escape from our previous conclusions? If, for the sake of argument, we restrict a hypothetical fluid shell to a depth of (say) 20 km, it would contain only 3.6 % of the Moon's mass, and 5.6 % of its momentum. At the bottom of such a shell the pressure would barely attain 1 kbar; and the underlying strata could depart from lithostatic equilibrium to account effectively for the observed momenta. However, even though the argument based on the momenta alone thus cannot deny the existence of a shallow (10–20 km deep) global layer of molten lava at some time in the past, such a possibility is emphatically ruled out by what we know today on the *shape* of the lunar surface.

The common observation that the Moon is very approximately spherical is a testimony to the fact that, throughout most part of its interior, the pressure is essentially hydrostatic; and the force exerted by it has reduced the Moon's mass to occupy a configuration of minimum potential energy. This is, in turn, a necessary consequence of the magnitude of this mass, and of the elastic properties of its material; for only celestial bodies of mass very much smaller (such as asteroids, for example) can maintain indefinitely an irregular shape.

The lunar globe is, however, not exactly a sphere: its surface departs from spherical form by amounts which may locally attain several kilometres (i.e. up to about 0.5 % of its radius). Determinations of its actual shape by stereogrammetric methods from the distance of the Earth are extremely difficult on account of the smallness of lunar optical librations (except for the limb regions, which can be seen in projection against the background sky); and all work in the

† The writer owes this remark to his colleague Professor Jack Zussman.

field prior to the mid 1960s should be regarded as 'pre-historic' – when astronomers concerned with these tasks were still learning to face the full difficulties of this subject. Subsequent work at the pre-Apollo stage (cf., for example, Meyer & Ruffin 1965; Eigen & Hathaway 1967; Mills 1967, 1968; Mills & Sudbury 1968) gave us the first inkling of the real complexity of the departure of the lunar surface from a sphere; but it was not till the advent of Apollo 15 and 16 in 1971–2 which disclosed the full complexity and fine structure of these departures, measured with the aid of the laser altimeters aboard their Command Modules.

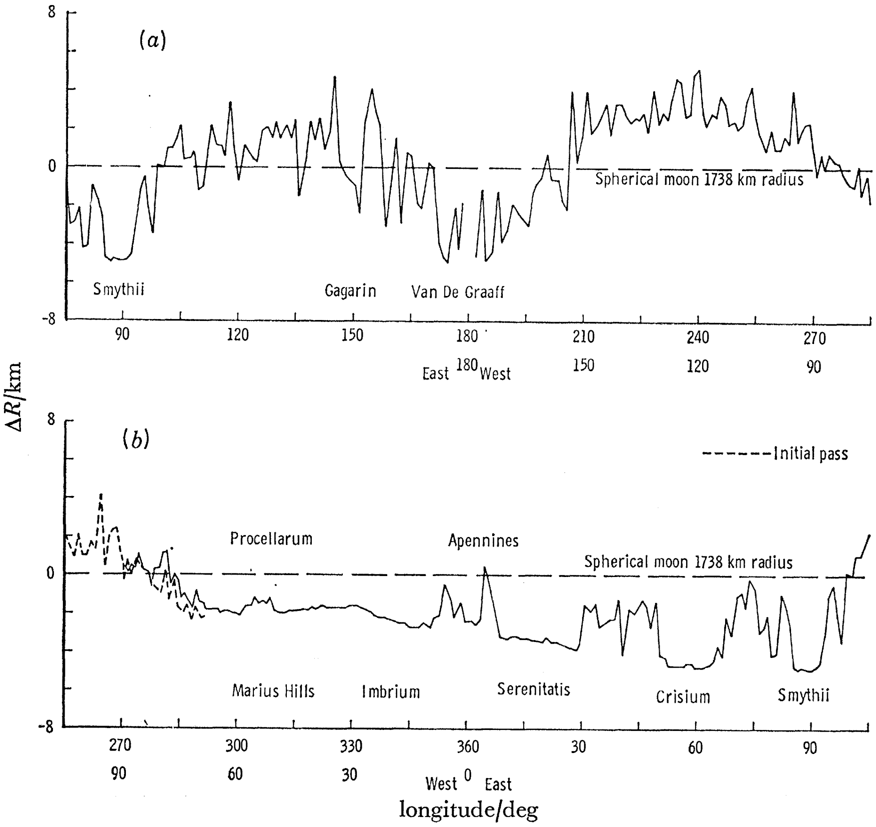

FIGURE 1. Apollo 15 altitude profiles of the lunar globe, showing deviations of the actual surface from a spherical Moon 1738 km in diameter: (*a*) far side, (*b*) near side.

The attitude profiles of lunar circumferences overflown by these missions are shown on the accompanying figures 1 and 2 (after Wollenhaupt & Sjogren 1972*a*, *b*). A glance at these figures (on which the uncertainty of the measurements is smaller than the actual thickness of the lines tracing the respective lunar cross-sections) should dispel with final validity the existence of any 'tidal bulge' on the Moon allegedly directed to the Earth and produced by its attraction – a notion which originated in the nineteenth century (at a time when all celestial bodies were supposed to have initially been fluid), and which is still dying hard in the minds of some of our more conservative confrères today.

Laser rangings of 1971–2 have, however, demonstrated that the lunar hemisphere facing us is *compressed* by 2–3 km *below* the mean Moon level, rather than elongated towards us.† Two alternative conclusions could be drawn from this fact: either the centre of symmetry of the

† A fact foreshadowed by Sjogren's 1967 study of the impact times of hard-landing Rangers on the Moon in 1964–5.

Moon is displaced by 2–3 km from the Moon's centre of mass towards us†; or the two centres coincide and the pear-shaped form of the observed lunar cross-sections is due to a superposition of the effects of odd harmonics in longitude commencing with the third. Whatever the case may be, the observed facts are sufficient to rule out the possibility that the actual shape of the Moon has anything to do with tides raised on the Moon by the terrestrial attraction. *The present shape of the lunar surface is not one in which a global ocean of fluid magma could have solidified at any time, and any distance from the Earth.*

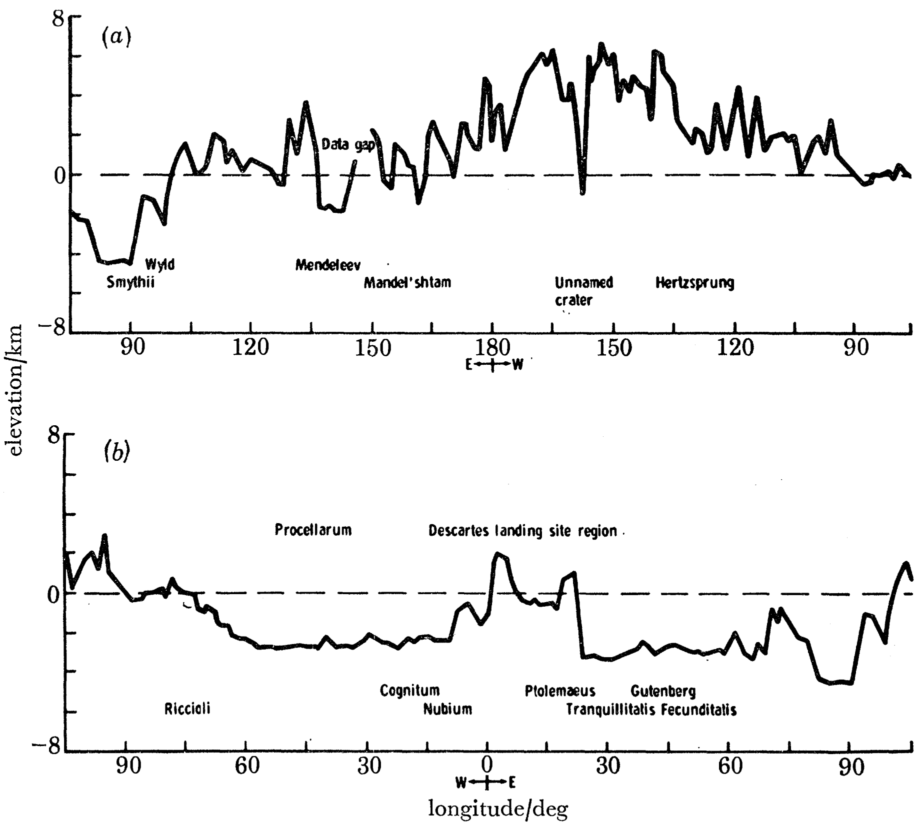

FIGURE 2. Appollo 16 altitude profile, showing deviations of the actual surface from a spherical Moon: (*a*) far side, (*b*) near side.

For no matter what the distance of the Moon from the Earth may have been at the time of solidification of a hypothetical molten layer, the dominant harmonic of the respective tidal distortion would have to be the second – one whose effects on the Moon are conspicuous by their absence. The pear-shaped smoothed profile may be formally represented by a superposition of *odd* harmonics – be it the first ($j = 1$) which would signify a mere bodily displacement between the centre of gravity and symmetry, or higher ($j = 3, 5, \ldots$) which could be invoked by tidal action. To establish a relative importance of these harmonics (if indeed the shape of the Moon is expansible in a convergent series of such harmonics – which the available data neither prove nor disprove) remains an important question to be settled by the lunar Polar Orbiters now under consideration.

† It is of interest to note that the same hypothesis was voiced already in the nineteenth century by Hansen (1854) in connection with his speculations on the existence of a hypothetical lunar atmosphere. However, Hansen would have had the centre of symmetry displaced in the opposite sense from that indicated by modern observations.

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From what we know now it should, however, be stressed that the long end of the apparen pear-shaped figure is directed *away* from the Earth; and not towards it, as it should if its caus were odd harmonics of tidal origin. As is well known, the heights of the partial tides of *j*tl spherical–harmonic symmetry varies, in general, with inverse $(j+1)$th power of the tide generating body (in this case, of our Earth); and, accordingly, under no reasonable circum stances (barring resonance) can a second-harmonic partial tide be lower than the third.

Since, as we already mentioned, the second-harmonic partial tide is conspicuous by it absence in the data secured by the Apollo 15–16 laser ranging, the consequences of a possibilit that even the top-most layer of the Moon bordered by the visible surface acquired its presen form by freezing out of a *global* ocean of fluid magma are so plainly contradicted by the observe facts that the underlying hypothesis must be abandoned. The observed shape is, therefore, agai probably due to irregularities in the last stage of accumulation of the Moon as a celestial body and such melting or magma extrusions as may have occurred on the Moon since that time mus have been of *local* – rather than global – character.

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