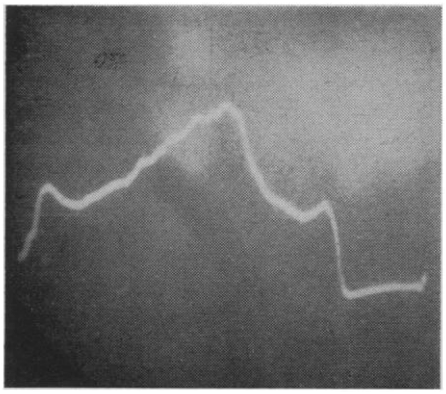

Fig. 2. Representative record traced by a recorder connected in the meter position of Fig. 1 as the probe is advanced against the cornea of a human eye and then withdrawn. The height of the trough measures intraocular pressure and the magnitude of the dip is a measure of corneal rigidity. The first trough indicates 17 mm-Hg, the second, 15 mm, because of the expression of fluid during the interval which was 1 second over-all.

piston 2 mm in diameter and employed a flattening of the eye over a diameter of 3 mm. However, it now seems desirable that both of these diameters be decreased by a factor of approximately two. This is desirable because it is not necessary to cancel extraneous factors which do not enter in the application of this instrument, and because the lessened diameter results in a smaller artificial increase in pressure due to the process of application. To make an accurate interpretation of the cause of the dip in the response curve shown in Fig. 2 it was necessary to perform a series of experiments with specially constructed tonometers having variable piston diameter. From measurements made in this way it was possible to prove that the dip was not caused by buckling of the cornea, in the snap-action fashion of the bottom of an oil can. That is, since the dip was always found to occur at a degree of flattening corresponding to the diameter of the piston, it was proved that the cause of the dip was the expansion of the bent region to a perimeter beyond the margin of the plunger rather than the sudden formation of an inverted vault, which process would be expected to take place at a constant diameter for a given eye or not at all.

Other forms of the tonometer have been tested, and some have proved promising. Thus, forms in which there was no mechanical feedback have been constructed by using a rather stiff force transducer which would record the motion of the plunger while yet keeping the tip of the plunger essentially in the plane of the surrounding area. In any hand-held instrument it is desirable that the mass of all moving parts be kept to a minimum to reduce irregularities introduced by an accelerometer or seismograph effect. Other methods of recording the reading can be employed than the one indicated. Thus electronic circuits can be arranged to store the reading that exists as the current passes through its minimum.

These small probes have many applications in biological experimentation because of their ability to measure intracavity pressure through an intact tissue wall. One of the more obvious examples is the continuous monitoring of blood pressure through the intact wall of a blood vessel. There are sensitivity limitations in every case and these will determine the thickness and stiffness of the wall through which one can measure pressure, and these same factors will influence the most desirable size for the pressure sensitive region. The competing method of performing intracavity measurement is to employ the small swallowable radio transmitters that have been developed in recent years (*3*). But these cannot always be implanted where desired and so the two methods are usually complementary in being applicable to different cases. The primary intention for the present device is to make glaucoma survey more general and routine.

R. Stuart Mackay*
Elwin Marg
Raymond Oechsli
School of Optometry, University of California, Berkeley

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* Address: Radiological Research Laboratory, Univ. of California Medical Center, San Francisco.

18 June 1959

Density of the Lunar Atmosphere

John C. Brandt's paper (*1*) contains criticisms of an earlier note of mine (*2*) dealing partly with the same subject, and I have been invited to reply.

It seems to me that Brandt has greatly oversimplified the issue. The tenuity of the permanent atmosphere of the moon is beyond dispute, but I submit that comparatively large amounts of gas may be held by persorption in the porous or pulverous materials, or both, of the lunar surface (*3*), which is in the condition of permafrost at a depth of the order of 1 m (*4*). Since sorption decreases with rising temperature, some of this gas should be liberated by the heat of sunrays, forming a low atmospheric "skin," which is resorbed in the cold of the night. Thus, the lunar atmosphere in the lower selenographic latitudes may yet have in the daytime the ground density attributed to it by Lipsky, even though it is undetectable by Dollfus's method on the night side of the cusp, close to the first quarter, 200 km above the surface of a polar region (*5*).

There are further objections to this method. The gas is assumed to be CO_2 at 0°C, although the ground temperature in these conditions will be −150°C or less, so that most of the CO_2 would have been precipitated. A Wratten 12 filter was used, which suppresses the blue and violet part of the spectrum. Now, the most likely gas to look for in these circumstances is argon, considerable quantities of which should be produced, as was suggested by Shapley, by the decay of the radioactive isotope of potassium. Since argon is monatomic, as against the triatomic CO_2, an atmosphere of this gas will scatter primarily shortwave radiations and appear much "bluer" than one of carbon dioxide. The use of a Wratten 12 filter should make it largely invisible.

Costain, Elsmore, and Whitfield (*6*) have not published, to date, the full particulars of the method by which they estimated the upper limit of the ground density of the lunar atmosphere. Öpik (*7*) supplies some of the missing reasoning, but he is not very explicit either. The electron density will clearly depend on the assumed chemical composition. If the lunar atmosphere is chiefly composed of argon, an inert gas with a high ionization potential, it may not be ionized to any extent in depth, and this might wholly falsify the result. Moreover, on 12 September 1956, Rishbeth and Little observed an occultation of the discrete radio source associated with Kepler's nova (*8*). There was positive response when the source was still 3′ behind the lunar disk, a figure which is in excess of the previous estimates and which may indicate a refraction far above that found in the occultation of the Crab nebula.

For these reasons one should be chary of ascribing too much importance to these negative estimates, the more so as the density of the lunar atmosphere may vary with phase and lunar latitude.

Not having seen Chamberlain's unpublished paper, I am unable to express any opinion on its merits, but I see no reason to dissent from the results quoted by Brandt (*1*). Brandt's mathematical argument has been omitted from the thermofax copy of his report sent to me, but again I am prepared to accept it as correct within the assumptions he makes. It is his assumptions that I find questionable.

The interplanetary medium may consist chiefly of the coronal proton-electron gas having a mean particle weight of ½, as he assumes. Yet it must certainly contain some heavier particles as well, derived, if not from the sun itself, at least from the action of cosmic rays on meteoric matter, from cometary sources, from concentration of interstellar gas in the sun's gravitational field, and, especially in the case of the moon, from the molecular spray of planetary exospheres. The proportion of these particles may be low, but this does not make them unimportant, because the gravitational concentration of interplanetary gas about the moon must be considered as a secular process extended over the whole of the geological time.

Brandt takes into account only what may be termed the instantaneous or differential effect of lunar gravity but leaves its cumulative or integral consequences completely out of consideration.

Let us suppose that the process of gravitational condensation begins to operate at a moment t_0 and that a sample of interplanetary gas, condensed according to Brandt's assumption, comes into contact with the cold body of the moon. It will lose some of its energy and its particles will be substantially slowed down. The next sample of gas, approaching the lunar surface at the moment t_1, will encounter the first sample in its path and be cooled by contact therewith before reaching the moon's surface and experiencing a further chilling. This process will continue indefinitely, the circumlunar gas steadily becoming colder and more condensed, and thus further accelerating the loss of energy by the incoming particles.

After a time the interplanetary particles will no longer reach the surface of the moon, and the zone of intermixture and chilling will move steadily outwards. The temperature of the atmosphere will tend towards that of the subjacent ground, which is shielded from sunrays for a fortnight at a time, indeed, probably dropping below this temperature, since most gases absorb but little of the solar or planetary heat.

This is not the end of the story. Interplanetary gas is a mixture, whose constituents will diffuse outwards; the lighter they are the more readily will they diffuse. Thus, we have here something similar to the process of washing gold dust out of sand. There may be only an ounce of gold dust per ton of sand, but in the end the deposit is almost pure gold. So, too, the atmosphere of the moon gravitationally derived from the interplanetary gas will consist of the comparatively heavy gases which lunar gravity is capable of retaining for sufficiently long periods of time for their loss to be replenished from the surrounding space (that is, if there is no exhalation).

To sum up, the integral result of the gravitational process will be, not an isothermal atmosphere at the same temperature and of the same molecular weight as the interplanetary gas from which it has been condensed inwards, but an atmosphere, isothermal or not, at a low temperature and composed of heavy gases, which has developed outwards from the moon's surface.

How far out could such an atmosphere extend?

This is not an easy question to answer, but the limit of 1000 km assumed by me does not seem unreasonable (*2*). It was my object to make a rough estimate, to the order of magnitude only, of the lower limit for the ground particle density of such an atmosphere. I made what I regard as unfavorable assumptions, such as taking 25 for the mean molecular weight of the atmosphere and 250°K for its temperature, assumed to be the same throughout. As a further precaution, 1000 km was taken for the lunar radius and 150 cm sec^{-2}, for surficial gravitational acceleration. This is a wholly legitimate procedure.

Brandt and I have set ourselves entirely different problems and, therefore, our numerical results cannot be expected to be the same. As regards his closing remarks (*1*), these are so skimpy and obscure that it is difficult to come to grips with them. I have made three points. (i) The density at the escape level of planetary atmospheres, and so the rate of escape, declines with declining gravity (incidentally, my original note contains a regrettable *lapsus calami*. It is stated [*Science* **130**, 1337, col. 2, par. 5 (1959)] that the rate of molecular dissipation will be *inversely* proportional to g, but it is clear from the context that the exact opposite is meant). (ii) If the escape layer is ionized, the free electrons should escape equally to space and to the subjacent layer, producing two oppositely charged atmospheric layers held together by an electrostatic bond. (iii) Because the density at the escape level declines with declining gravity, interplanetary gas will offer increasingly effective collisional opposition to, and diffusive compensation for, the particles escaping from the atmosphere. Theoretically, a point must be reached where dissipation ceases, so that, paradoxically, the atmosphere of a body of sufficiently small mass would become immune to dissipation. I added that this reasoning is not exhaustive.

Brandt says that my first point is "inherent in the conception of the base of an exosphere." Whatever this may mean, somehow or other I have never encountered a clear statement of this situation in the literature on the subject.

My second point is said to be "incorrect," and I am referred to page 306 of *The Sun* by G. P. Kuiper *et al* (*9*), which I have consulted. Page 306 and the following pages relate to the solar corona and not to planetary atmospheres. The gist of the argument is as follows. If m_p be the mass of the proton and m_e that of the electron, $m_p/m_e = 1840$. The two particles carry the same electric charge, of opposite signs, so that electrostatic forces as applied to an electron will be 1840 times more effective than gravity in comparison with the same forces acting on a proton. If protons and electrons are to have the same particle density in an isothermal atmosphere with gravity g, the ratio β of the positive electrostatic force to the gravitational force acting upon a proton must equal 0.5. Then the total restraining force acting on a proton will be

$$m_p g\,(1-\beta) \approx m_e g + m_p\, g\, \beta$$

the latter being the restraining force acting upon an electron. It can be shown that, the restraining force being the same, both types of particle will escape at the same rate. However, this condition cannot be satisfied without a positive electrostatic force as defined above.

Let us now take a planetary atmosphere, to which my argument applies, at night, assuming that this atmosphere's electrical potential is 0 and its ionization is inappreciable. If this gas now emerges into full sunlight and ionization ensues, positive and negative charges, the latter carried by electrons, will be formed in equal numbers, so that they will cancel out and there will be no electrostatic field. Consequently, gravity will operate alone and electrons will escape rapidly to space and to the subjacent layer, as stated, until a sufficient positive charge has been built up to bring about a state of equilibrium. With a weak ionization this state may never be reached, but even if it is, my argument stands: there will be a retaining charge.

Indeed, on the selfsame page 306 van de Hulst himself says, "In an isothermal gas the light electrons have a tendency to segregate from the heavier protons." And if the atmospheric gas is not hydrogen, this tendency will be so much the greater.

Finally, Brandt maintains that my third point is "well known" and refers me to a paper by Öpik (*7*) which contains no mention of it whatever.

V. A. Firsoff
8, Wells Road, Glastonbury, Somerset, England

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21 March 1960

I have read Firsoff's criticism of my earlier note and I find that no changes are necessary either in my results given there or in my comments relating to an earlier note by Firsoff.

Costain, Elsmore, and Whitfield have published the details of their method of estimating the maximum density of the lunar atmosphere from the radio observations of the lunar occultation of the Crab nebula (*1*). Firsoff is further in error when he states that Rishbeth and Little have observed a response from the radio source associated with Kepler's nova when the source was 3′ inside the limb of the moon. It is explicitly stated in the Rishbeth and Little paper that it was the visible remnant of Kepler's nova that lay 3′ inside the limb of the moon. It is further explained by Rishbeth and Little that the best available position would put the radio source much nearer the limb than the optical remnant.

It seems clear that the kinetic temperature of particles rebounding from the surface of the moon will depend very little on the radiation temperature of the moon, as assumed by Firsoff. Hence, the lunar atmosphere will undoubtly not be at the surface temperature of the moon but will approximate the temperature of the interplanetary gas. The issue is somewhat obscured by Firsoff's manipulation of rather well established astronomical constants such as the mass and radius of the moon, and because he arbitrarily cuts off the lunar atmosphere 1000 km above the lunar surface. Thus it happens that the particle density of $10^7/\text{cm}^3$ does not follow from Firsoff's assumptions in a straightforward manner. Firsoff's assumptions given in his original report (which include the claim that the interplanetary density of heavy particles with molecular weight 25 is about $10^3/\text{cm}^3$) lead to an atmospheric density at the lunar surface of about $6 \times 10^{17}/\text{cm}^3$, a value which is too high by orders of magnitude.

The concept of a critical level or base of an exosphere has been in the literature for years (*2*). A simple definition of the critical level may be given as the region in an atmosphere where a characteristic mean free path is equal to the local scale height. Firsoff's "important factors" (i) and (iii) follow immediately from this definition, and (iii) has also been discussed by Öpik, who considers how the escape rate decreases when the thickness of an atmosphere becomes less than one mean free path. Firsoff's point (ii) is still incorrect, and he is further guilty of quoting van de Hulst out of context. The sentence quoted by Firsoff together with the very next sentence read as follows: "In an isothermal gas the light electrons have a tendency to segregate from the heavier protons. Long ago Pannekoek and Rosseland showed that a minute separation of charge suffices to create an electric field **E** that compensates this tendency by drawing the protons up and the electrons down."

JOHN C. BRANDT

Yerkes Observatory, University of Chicago, Williams Bay, Wisconsin

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28 March 1960

Electrographic Evidence of Impaired Brain Function in Chronically Anxious Patients

Abstract. In a study of cerebral function by electroencephalographic techniques the following observations have been made. (i) In intact subjects, repeated stimulation with bright light causes a predictable change (alpha blocking) in the electroencephalogram, whereas repeated auditory stimulation does not. (ii) If, however, an auditory stimulus is presented repeatedly just before the visual stimulus, the sound temporarily but predictably acquires the property of the light to suppress the alpha activity. (iii) This linkage between sound and light occurs much less frequently in human subjects with known amounts of structural brain damage. (iv) A similar electrophysiological defect, implying impairment of brain function, occurs in patients showing severe anxiety during prolonged periods of difficulty in over-all adaptation.

While it is obvious that prolonged periods of poor life adjustment linked with anxiety impair the ability of an individual to function at his most effective level, the demonstration of a defect in brain function by electrophysical means has been difficult to obtain. Studies of the microscopic structure of the nervous system have revealed no significant changes in the brains of persons suffering from the common neuroses and psychoses; studies of the function of the brain as recorded in the resting electroencephalogram have shown only slight, if any, deviation from the normal; studies of the highest integrative functions as evidenced by behavior, attitudes, and thought are limited by the wide variability of motivation and cooperation of such patients.

Early study of electroencephalograms of human subjects showed that repeated stimulation by a bright light predictably provoked disappearance of the alpha activity whereas repeated presentation of an auditory stimulus soon ceased to have any effect on the electroencephalogram. In the 1930's it was noted (*1*) that after a subject's brain waves failed to show a response to an auditory stimulus, if the sound were then paired with a bright light and made to routinely precede the light by a fixed time interval, the sound itself might suppress the alpha activity just as the light had. Such a phenomenon has been known as a temporary cerebral connection or a conditioned cerebral response (it being understood that the phenomenon does not fulfill the criteria for Pavlovian conditioning). These conditioned cerebral responses, for reasons not yet established, are poorly sustained in man.

Since the development of conditioned cerebral responses is a measurable manifestation of brain function which demands minimal cooperation of the subject, and since such responses were found to occur much less frequently in human subjects with impaired function due to brain damage resulting from loss of known amounts of the cerebral hemispheres (*2*), it seemed appropriate to study this phenomenon in patients exhibiting sustained and severe anxiety. Studies were carried out on 23 "control" subjects without evidence of central nervous system dysfunction and on 15 patients who exhibited the consequences of long unresolved adaptive difficulties. They expressed severe anxiety most of the time and conspicuously showed many signs of it. They complained of thinking difficulties, low frustration tolerance, and fatiguability and were to some degree depressed and hostile. Adaptive and compensatory devices were few, poorly developed, and poorly maintained. They were free of the effects of drugs, of infectious, degenerative, neoplastic, or traumatic disease, and of other evidence of gross structural defects of the cerebral hemispheres. Their mean age was 35 as compared with a mean age of 30 for the control group.

The subject was seated in a quiet and semidarkened room and the test procedure was described to him in general terms in order to allay apprehension. Light stimulation was provided by a 150-watt frosted bulb, with a white reflector placed approximately 12 in. from the subject's eyes. Auditory stimu-