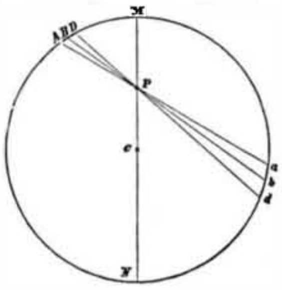
Correspondence.

The Weight of a Body inside a Hollow Sphere.

To the Editor of the Scientific American:

I am surprised at the half knowledge shown by your correspondents in their discussion of the attraction of a hollow sphere on a body within it. Can any one name a scientific man of repute who has repudiated it, or the demonstration of it, which is to be found in "Newton's Principia"? If Mr. Whitmore chooses to represent the mass which exerts the attraction on the body, P (see the illustration on page 84), by the cup-shaped fragment, B E F G C B, his position is undoubtedly correct; but the calculation of the attraction becomes so troublesome that we may well ask for a simpler way. Newton's theorem furnishes this.



In a thin shell, whose section is the circle at A M a N A, let a body, P, be found; draw through P in any direction the line, B P b, and revolve around it the line, A P a, which makes with it the small angle, A P B: the resulting circles, shown in section at A D, a d, will have the areas π(A B)^2, π $(d\,b)^2$, and the masses $=\pi\, m\, t$ (A B)^2, $\pi\, m\, t (d\,b)^2$, where $t=$ the thickness of the shell, and $m=$ the quantity of matter in the unit of volume: the attractions on P will equal these masses divided by the squares of the distances from P, namely, P B, P b, and multiplied by a constant, f, thus: Attraction at B : attraction at b :: $\frac{2\pi\, m\, t f\, (\text{A B})^2}{(\text{P B})^2} : \frac{2\,\pi m t f\,(d\,b)^2}{(\text{P}\ b)^2}$ :: $\frac{(\text{A B})^2}{(\text{P B})^2} : \frac{(d\,b)^2}{(\text{P}\,b)^2}$. But from the similarity of the very acute angled triangles, A B P, $d\,b$ P, we have A B : B P :: d b : P b. $\therefore \frac{(\text{A B})^2}{(\text{B P})^2} = \frac{(d\,b)^2}{(\text{P}\ b)^2}$, and therefore attraction at B $=$ attraction at b. That is, the body, P, will not move in either direction along the line, B P p; and as this line may be drawn in any direction whatever in the shell, the body at P will not move in any direction, and will therefore be in equilibrium at every point. To prove this for thick shells or hollow spheres, it is only necessary to conceive them as made up of an indefinite number of thin ones.

Professor Olmsted has been placed in apparent contradiction with this truth because it was forgotten by the writers who quoted from him that the attraction of gravitation varies inversely as the square of the distance. Thus, if the body be lowered half way to the center, it would be attracted by a mass equivalent to one eighth of the original sphere; but as the distance between the body and the center of the sphere is only one half of what it was before, the attraction will equal $\frac{\frac{1}{8}}{(\frac{1}{2})^2} = \frac{1}{8} \div \frac{1}{4} = \frac{1}{2}$: or in general, if the force at the surface of a sphere, of radius r, be represented by 1, and the portion lost in descending a distance, d, by x, we have: $1 : 1 - x :: \frac{r^3}{r^2} : \frac{(r-d)^3}{(r-d)^2}$; $\therefore 1 : 1 - x :: r : r - d$. $\therefore 1 - x = 1 - \frac{d}{r}$ or $x = \frac{d}{r}$; that is, a body lowered toward the center of the earth would lose in weight and proportion to its distance downward, as Olmsted says.

Your correspondent further confounds attraction with weight when he says: "Guided by this theorem, we should expect a hollow sphere to balance if suspended from any possible point within the void." Not at all. The confusion comes from not distinguishing between the attraction between the earth and the portions of the shell on opposite sides of the point of support, and the almost infinitesimal attraction between these portions and any body at this point.

The theorem is in fact not to be proven experimentally, but is an inevitable consequence of the grand, often verified, never disproved law that every body attracts every other with a force directly as the product of its mass, and inversely as the square of the distance between them, that is, $f = \frac{m\,m'}{d^2}$.

It should perhaps be added that the demonstration above given, as Newton himself pointed out, is only true when each shell is homogeneous, though neighboring shells may vary in density to any extent. In the case of the earth, the curious result is found that the center of the earth is so much denser than the part near the surface that the force of attraction increases at first on descending; and so Professor Airy's clock, in the mine 1,250 feet deep, gained 274 seconds daily.

C. K. W.

Malone, N. Y.

South American Birds.

To the Editor of the Scientific American:

On the eastern shore of the Uruguay river, from Paysandu to Independençia, there is an open rolling country with frequent small ravines, most of which are bordered with a narrow skirt of timber of stunted growth and flowering shrubbery, which makes a fine retreat for the birds, and also frequently shelters the deer, South American tiger, and wild cat, which, however, are not abundant. The hill tops are also crowned with timber of similar growth, making a pleasant shade and resort from the scorching sun. Except on the hill tops and in the ravines, the country is partially covered with tall coarse grass, which makes a fine cover for quail and partridge. On approaching a ravine, the first thing that attracts your attention is the hum of the humming birds, which are of numerous different varieties, each bird balancing nicely on its wings while it inserts its long slender bill and extracts sustenance from the desert flower. Along the ravines, wild pigeons, similar to ours, are to be found in plenty, and are easily bagged. Next is the small partridge, very much like our northern quail, which are difficult to bag on foot and without a dog, as they will hide in the tall grass; but with a trained dog, the sport is fine. On horseback, you may almost ride over them before they will fly up. They are in flocks generally, yet they do not huddle; and it is difficult to get more than one at a shot. But you may sit on your horse and shoot a whole flock singly, as they seldom fly except they are flushed by a dog. The large partridges, which closely resemble English pheasants, are generally found singly, and the mode of catching these birds is rather peculiar.

They are found amongst the tall grass. The sportsman is mounted (carrying no gun, however) and has his dog trained to the work. He walks his horse slowly along, while the dog hunts about amongst the grass; and when he comes close upon the bird, the latter breaks cover, rises a little above the grass, and flies off on a level. When the bird flies, the sportsman puts his horse to his mettle and follows to the spot where he sees the bird alight (probably a hundred yards), and waits the arrival of the dog, who follows at his top speed and rushes in amongst the grass; and soon again the bird breaks cover and flies as before, but only about half as far. The sportsman and dog follow up as before, and the bird is hunted out again by the dog, and divides the distance again, and drops into the grass, pursued by sportsman and dog, this time closing the race for life. The dog rushes into the grass and directly comes out again with the bird unharmed in his mouth; the sportsman in the meantime dismounts and receives the bird, and disposes of him as he thinks proper. I was once an eye witness of such a race, and was told that these birds never break cover but three times, which seems to me rather strange.

Stratford, Conn. TRUMAN HOTCHKISS.

The Atmosphere of the Moon.

To the Editor of the Scientific American:

The moon is considered, by some astronomers, to have no atmosphere, as you mentioned in a recent issue; and in the article you gave some very plausible reasons for supposing that there may be an atmosphere of some kind on that body.

Heat, as you say, would have a great influence in expanding the air to a great extent, and rendering it so rare that it would extend out from the surface of the moon a great distance, so that its presence could hardly be detected by us. Yet when the moon cooled, the air would be condensed, and then be as dense or denser than our atmosphere, and could be easily detected.

To prove that the detection of the presence of the atmosphere would be difficult when the air was rare, and comparatively easy when the same bulk of air is made to occupy a smaller space, is very simple; for if we take a cubic foot of air or any other gas of the density of our atmosphere, the refraction of a ray of light passing through it would be very evident; but, if the same amount be made to occupy one hundred cubic feet, the refraction would be very much more difficult to detect, for, according to the old rule, "the greater the difference of the densities of the two gases, the greater the refraction, and *vice versa*."

Covington, Ky. WILLIAM L. DUDLEY.

The Direct Motion of the Radiometer an Effect of Electricity.

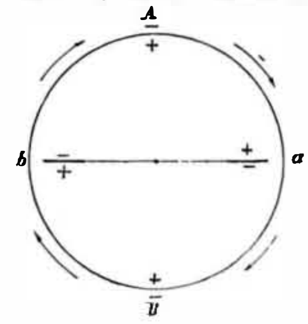
To the Editor of the Scientific American.

In the communication I sent you a few days ago, upon the radiometer of Professor Crookes, I showed that the exterior of the glass globe was electrified negatively when exposed to luminous or calorific radiations. Having made, since that time, some more experiments, I have discovered new facts which enable me to explain at least some of the motions of this wonderful instrument. The facts are as follows:

I took a strip of mica two diameters (7·8 inches) in length; and having coated one of the sides with lampblack, when it was quite dry I suspended it in a Coulomb's torsion balance, having previously electrified the metallic disk of the balance needle with positive electricity. The blackened side of the mica faced the electrified disk. When the needle had come to rest I allowed the radiations from a large gas flame to fall upon the blackened surface of the mica. Notwithstanding the light was at a considerable distance and had to penetrate the thick glass shade enclosing the balance, the needle was rapidly repelled several degrees, showing that the blackened face was positively electrified under the influence of radiation. I then turned the strip of mica so that the bright side faced the disk and allowed the radiation to fall as before, upon the blackened surface. This time the needle indicated an attraction between the disk and the mica, thus proving that the bright surface was negatively electrified.

To anticipate an objection to the theory of the radiometer which will be suggested by these facts, namely, that these electrical manifestations are too feeble to account for the rapid revolution of the arms, I made the following experiment: I rubbed the globe gently with a brush composed of fine threads of glass; the electricity developed on the globe, acting by induction upon the nearest mica disk, caused a brisk oscillation. I then measured the intensity of the electricity upon the glass globe by means of the proof plan and Böhnenberger's electroscope. There was no indication of greater intensity in this case than there was when the globe was electrified by the radiations from aluminous or obscure source and tested in the same manner.

From the above facts the following theory necessarily flows as a corollary; The hemisphere, A, being negatively electrified, as we have shown, upon its whole exterior surface, we justly conclude that the interior is positively electrified. The hemisphere, B, is electrified in the same way, but its intensity is different, the charge being less at B than at A.



The mica disk in the position, a, with its blackened side turned towards the radiant source, is electrified positively upon the black and negatively upon the bright surface, as we have proved above. As like electricities repel and unlike attract, the positive electricity at A will repel the arm, and that at B, acting upon the bright face, will attract it, so that it will necessarily rotate in the direction of the arrows, namely, A a B. When the arm has reached b, the direction of the rotation will not be changed, but A will now attract, and B repel, and it will continue to move in the direction B b A. The direct and most usual movement of the arms in Professor Crookes' radiometer is thus explained in the simplest manner. JOSEPH DELSAUX, S. J.

11 Rue des Recollets, Louvain, Belgium.

[For the Scientific American.]

NOTES ON THE RESISTANCE OF MATERIALS.

The ordinary formulæ and tables in technical works for proportioning the parts of machines and structures are based on the ultimate resistance of the material which is to be employed, accompanied by recommendations that a certain fraction only of the breaking load should be applied in practice. This fraction varies from $\frac{1}{3}$ to $\frac{1}{8}$, according to the views of different authorities. It has been found, however, that a material may be strained in such a manner as to become unsafe, by a load that is generally less than half the ultimate resistance, so that some of the best authorities consider that the fraction of the breaking load, or factor of safety, should be chosen with reference to the elastic limit of the material rather than its ultimate resistance. Still more recently, attention has been directed to experiments showing that materials could be ruptured by the repeated application of a comparatively small load. It is obvious that a rule for proportioning a machine, which provides for safety by using only a part of the strain allowed by the theory in which the rule is founded, is at best only a makeshift, and is unsatisfactory on many accounts. If the structures of the materials used in the arts were understood, so that the effect of strains could be accurately noted, it would of course be easy to give rules which would enable the material to be disposed in the most effective and economical manner. The experiments on the effect of repeated strains, referred to above, furnish some facts on which a novel and interesting theory of molecular structure has been based. Although this theory is far from being fully verified by experiment, it is, to say the least, not absolutely contradicted. A good discussion of the subject has recently been given by Professor Spangenberg of Germany, and a translation of the same has been published in this country, from which the following account has been condensed.

What is commonly regarded as a solid is supposed, in the theory referred to, to be made up of a number of atoms and molecules, surrounded by ethereal atmospheres, and grouped in various forms, according to the temperature and nature of external strains. Most readers know that the theory, so far, is in accordance with that generally adopted by scientists. Perhaps it neerv can be absolutely proved, although it has been shown to be extremely probable. Now it is known that when a mass of metal is melted and poured into a mold where it is rapidly cooled, it tends to crystallize in groups, and this is regarded as the first normal condition. Wrought iron and steel are generally rolled or hammered before use, and this breaks up the crystalline groups and produces a fibrous grain. When a metal is subject to strain, the grouping of the atoms will be changed, and they may return to their former position when the load is removed, or may take new forms, according to the amount of the strain and the rapidity of its recurrence. The effect of repeated strains is to break up the crystalline structure, and induce an amorphous condition. In changing to this state, the strain may act so quickly that all the crystals are not affected, and rupture will occur. The atoms of the body are supposed to have a mutual attraction for each other, and the other atoms attract those of the body and mutually repel each other.

It seems to be settled by experiment as well as theory that, contrary to general notions, the resistance to rupture of a body is less, the more crystalline is its structure, and increases as the amorphous structure is produced. It is supposed that the cohesion between separate crystalline groups is less than the cohesion of molecules forming a crystal.

The experiments given in connection with this theory show conclusively that the number and duration of strains are of quite as much importance as their magnitude. Whether then, the theory on which this action is explained is accepted or not, the facts seem to show the point to which future experiments on the strength of materials should be directed. Possibly the United States testing board may derive some hints from Professor Spangenburg's treatise.

R. H. B.