starting-point, was not placed high enough. It appeared that the Carrels, neglecting their instructions, had made a push towards the summit, but had reached a height of only about 19,000 feet. As they were quite unencumbered, carrying no instruments, and only enough food for their own use, and had no traveller to look after, and yet came back quite exhausted, it was obvious that we should have to get still higher up before we could make a serious effort to reach the summit. So, as soon as he was well enough, I sent Louis with Perring down to the first camp to fetch up a tent, which had been left there, and when this arrived we were in a position to go forward again.

"On the following morning I went myself up the ridge to look for a higher camping place, and found one on the eastern side on some broken rocks, at a height of 17,400 feet. By this time I was in rather better condition than the Carrels. Feverishness had disappeared, and my blood had resumed its normal temperature. The gaspings had entirely ceased, and headache had gone. You will perhaps inquire how I knew that I was feverish; for in regard to this matter one is often mistaken, and fever is supposed when it does not exist. By the advice of the distinguished physician whose name has been already mentioned, Dr. Marcet, I had provided myself with a registering clinical thermometer for the purpose of taking blood temperature at great elevations. This was duly done, and in respect to this matter nothing more need be said than that at our greatest heights the temperature of the blood was (just as it is at the level of the sea) higher during periods of warmth, and lower when it was unusually cold; but stood at its normal height, when the thermometer was at 60° or thereabouts, and did not appear to be affected by low atmospheric pressure at all. In recommending me to take this little instrument (which I have in my hand), Dr. Marcet rendered me a great service; and amongst all the devices and instruments which have been pressed upon the attention of travellers in general, of late years, I know nothing equal to it in importance. By constant observation, I was able to detect the earliest advances of fever; and by taking proper steps in time, was able to get through the entire journey without having an attack of fever worth mentioning. Its expense is trifling, and it can easily be carried in the waistcoat pocket. When we were first laid on our backs by mountain sickness, it showed that my blood temperature mounted to 100°·4, but by the end of the year it had fallen to its usual height, viz., 98°. Still, although the more disagreeable symptoms had gone, we found ourselves remaining comparatively lifeless and feeble, with a strong disposition to sit down when we ought to have been moving." * *

Mr. Whymper then described his first ascent of Chimborazo, and concluded his account of this mountain by saying, "My residence on Chimborazo thus extended over seventeen days. One night was passed at a height of 14,400 feet, ten at a height of 16,500 feet, and six at 17,300 feet. During this time, besides ascending to the summit, I also went three times as high as 18,500 feet. When we quitted the mountain, *all trace of mountain sickness had disappeared*, nor did it touch us again until we arrived at the summit of Cotopaxi. * * *

'The height of Cotopaxi is 19,600 feet. Our camp was placed about 130 feet below the loftiest point, and it was the most elevated position at which any of us had ever slept. We remained there twenty-six consecutive hours, feeling slightly at first the effects of low pressure, having the same symptoms as we had noticed on Chimborazo; and we used chlorate of potash again with good effect. All signs of mountain sickness had passed away before we commenced the descent, and *they did not recur again during the journey*. * * * * *

"This, ladies and gentlemen, nearly brings my remarks to a close, and, in conclusion, permit me to say a word more in respect to mountain exploration in general. Amongst certain persons it is still fashionable to affect a description of scorn, bordering on contempt, for anything in connection with mountains and mountain work. None of us feel, perhaps, very deeply the criticism of those who are evidently ignorant of the subjects on which they talk; and, in this matter, speaking for myself, I rather look forward to the time, which will surely come, when the study of mountains, the ascent of mountains, and even prolonged residence on mountains, will be found essential for the prosecution of a score of sciences. Before this could be carried out, it was necessary to learn whether life could be made endurable at great heights. We were always haunted by the fear of an invisible enemy who might strike us down at any moment. What we wanted to know was, not whether life could exist at a height of 20,000 feet (that was settled seventy-five years ago, by Lussac), but whether man could become so far habituated to the low pressure which is experienced at that height, as to be able to live without inconvenience, and to do useful work. I went to the Andes in search of an answer to these questions, you have heard the story, and can form an opinion whether it affords encouragement for the prosecution of exploration in other quarters."

ON SOME POINTS RELATING TO THE DYNAMICS OF "RADIANT MATTER"

As the important researches of Mr. Crookes may be said to have made the evidence of the molecular state of matter (grounded on indirect reasoning) almost ocularly visible—the mechanics of gaseous matter therefore acquires a fresh interest. As some years back the present writer devoted much thought to the clear realisation of the nature of the motions of the molecules of gases in connection with a proposed explanation of the mode of propagation of sound on the basis of the kinetic theory (published in the *Philosophical Magazine* for June, 1877), it then appeared to him that the systematic *regularity* of the motions of the molecules of gases was not in practice so generally appreciated as it might be; although of course the mathematical basis of the subject was well established. It has been not unusual to speak of the extreme "irregularity" of the normal motions of gaseous molecules—which is undoubtedly true of any molecule taken individually. The comparison of the molecules of a gas to a "swarm of bees" (sometimes adopted), though no doubt highly convenient and useful to aid the conceptions in some respects, has probably gone to support (rather than not) the idea of a kind of confusion in the motions of the constituent molecules of gases; whereby the systematic *regularity* (or symmetry of the motion) tends to be left out of view. This will perhaps appear more evident if I state the following proposition in regard to a gas, which is only a direct corollary from the established mathematical principles—true in every state of the gas, but emphasised by rarefaction.

The normal motion of the molecules of a gas takes place in such a way, that every point in the gas is a "radiant point," such that matter passes to and from that point (*to a certain distance*) *in the direction of rays:* i.e. *as if a luminous point were situated at the point in question.* Or more generally put: If finely subdivided matter be in motion in space according to its own dynamics, every point of space becomes a radiant point; the extent of the radiation of matter depending on its fineness (other things being equal).

It is, I believe, the losing sight of the systematic regularity (or symmetry) of the motion of the molecules of a gas in its normal state, which (as it would seem, at least) has caused the connection of gaseous motion with the conditions for gravity to be overlooked—or the fact to escape realisation that on rarefying the gas, this symmetry of motion (existing in the normal state of the gas) gradually merges, without break of continuity, into the radiant streams of matter moving in the right directions to produce gravity under Le Sage's sheltering principle, without the necessity for adopting any of his postulates as to *direction* of motion, or assuming a *supply*[1] of matter from ultramundane space in continuous currents ("ultramundane corpuscles"). As this subject was carefully thought out and dealt with by me in the *Philosophical Magazine* for September and November, 1877, &c., I may perhaps claim some right to say a few words about "radiant matter."[2]

The immense importance—in its possible practical applications—of this remarkable self-correcting principle (directly based on the mathematical results of the kinetic theory) whereby particles of matter, left to their own dynamics, rigidly adjust their motions so as to move in a "radiant" manner [and to return energetically to this beautifully symmetrical kind of motion when after disturbance they are left to themselves], has, I venture to think, not been duly appreciated. For, looking at the case broadly, it would seem that this dynamical principle is capable of affording a means for substantially satisfying at least three fundamental objects in nature. For, firstly, it will appear evident that we can have thereby a means perpetually present in every point of space for *carrying* energy in a "radiant" manner (*i.e.* in the direction of the rays of light from a point) in all possible directions. Secondly, by this automatic system we can have a mechanism capable of causing (under the sheltering principle of Le Sage) the *approach* of the molecules of gross matter at any point of space—such as exhibited in the phenomena of "gravity" and (under modifying conditions probably) the other phenomena of approach, "cohesion" and "chemical action." Thirdly, since the "radiant" character of the motion is inevitably attended by an exact balance of the momenta at every point of space, we can have in this system an exhaustless store of energy in perfect equilibrium (and therefore concealed in its normal state), competent to throw some rational light on such unexplained phenomena as explosions, combustion, or the violent developments of motion taking place in the molecules of gross matter generally.[3]

As the phenomena of rarefied gases are attracting attention at present, perhaps some calculations I have made (based on the mathematical results of others) in regard to conditions attending extreme rarefaction, may not be without interest. The fact that the mean length of path of the molecules (of a gas) increases in the *triple* ratio of the mean distance on rarefying, leads to some remarkable results, which would scarcely be expected perhaps unless they had been worked out—and have their application in regard to the long mean path required for gravity. For it is a consequence of this that while the mean distance of the molecules of a gas increases with extreme slowness on rarefying, the mean path augments at a great rate.

This may be perhaps best elucidated by a mode of illustration, which I have chosen with the endeavour, if possible, to convey clear conceptions to the mind, which is far more important than the mere writing down of numbers (millions, &c.) which afford no defined idea at all. Some conception of what actually occurs when a gas is rarefied to a millionth of its normal density (a common amount in experiments) may perhaps be presented to the mind by supposing a cubical box, say one foot in the side, containing gas at normal density—hydrogen for instance—to be opened in a room one hundred feet in the side, containing a vacuum. This will then accurately represent the actual degree of rarefaction in the case under notice. The mean distance of the molecules will then be increased (from known principles) in the ratio of the linear side of the cubical box to that of the cubical room, *i.e.* as 1 to 100. Since the mean distance of the molecules at normal density is known to have been about one seven-millionth of an inch[1] (according to the mathematical results obtained by the late Prof. Clerk Maxwell and others); the mean distance or rarefying to a millionth will become one seventy-thousandth of an inch (a hundred times greater, but still a very small distance). The mean length of path will have increased as the *cubic* contents of the room (*i.e.* in the triple ratio of the mean distance). The mean length of path (which is known to have been about $\frac{1}{260000}$ of an inch at normal density) will now have rapidly risen to the very perceptible dimensions of four inches (nearly). Here we have the state of "radiant" matter (previously existing however in the normal state of the gas, but concealed) coming to be quite appreciable to the senses. For the gaseous molecules now "radiate" regularly to a mean distance of some inches from every point in the room; and if a portion of the gas were inclosed in a bulb, about four inches in diameter, the molecules would (on the average) strike across from one side to the other without colliding among themselves: the beautiful "radiant" character of the motion then becoming lost, and the motion (and consequent pressure) irregular, owing to the confined space and absence of those mutual encounters among the molecules by which the motion is forcibly corrected and made symmetrical.[2] It appears therefore that the truly "radiant" character of the motion (if we use the word in relation to the rays of light radiating from a luminous point) would then cease—though no doubt the term "radiant" may be also conveniently employed in another sense, viz. to express the fact [when a portion of gaseous matter is in a confined space where a proper adjustment of pressure is not possible] that the molecules may, by suitable means, be diverted from their paths, like the rays of light, so as to move in a parallel (or common) direction, and cast virtual shadows of objects placed in the bulb.

It will be apparent therefore that the establishment of

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[1] These postulates of Le Sage's theory relating to *supply* of matter from boundless space, &c., were unfavourably criticised by the late Prof. Clerk Maxwell (*Encyc. Brit.*, 1875, under article "Atom"). Prof. Maxwell remarks (p. 47) as follows:—"We may observe that according to this theory the habitable universe which we are accustomed to regard as the scene of a magnificent illustration of the conservation of energy as the fundamental principle of all nature, is in reality maintained in working order only by an enormous expenditure of external power, which would be nothing less than ruinous if the supply were drawn from anywhere else than from the infinitude of space."

It will be seen that this objection vanishes by regarding the gravific æther as simply a stationary gas, within the limits of mean path of whose particles the gravitating parts of the universe are immersed; as then no *supply* of matter or expenditure of external power is required. Also, it may be added, that a difficulty (mentioned p. 47 of same article "Atom") in regard to the supposed excessive heating of gross matter that would occur under the impacts of the gravific particles, was considered by the present writer (*Phil. Mag.*, November, 1877), and a means suggested for removing it without the necessity for admitting any conditions which could be regarded as in themselves improbable.

[2] It is said that Faraday was the first to use the expression "radiant matter."

[3] To my mind, I must confess, it seems difficult to understand why "potential" energy (in the sense of an energy which is *not* kinetic) appears to be (comparatively speaking) so much brought to the fore-ground, to the exclusion of the intelligible view of *motion transferred from matter in space*. Is it not in general considered a right principle to give preference to the intelligible or conceivable, in place of that which cannot appeal to our reason? Evidently the term "*kinetic*" (applied to energy) would be a redundant and superfluous prefix, unless it were thereby implied that some *other* energy than "kinetic" energy, viz., an energy *without motion*, existed.

[1] I quote this dimension from a former paper, "On the Nature of what is commonly called a 'Vacuum'" (*Phil. Mag.*, August, 1877), a few of the data of which it is convenient to use here as a commencement. It should be remarked that Mr. Johnstone Stoney appears to have been the first to carry out calculations regarding molecular dimensions and distances, and to deduce therefrom conclusions regarding the number of molecules in unit of volume of a so-called "vacuum"—which tended to upset preconceived ideas.

[2] It is evident that the "radiant" form of motion (or motion of the molecules *equally in all directions*) is the sole condition for equilibrium of pressure in all directions in a gas, or for an exact balance of momenta in every direction. It is an obvious corollary from this—expressing a known fact—that if any imaginary straight line be taken anywhere in a gas, as many molecules at any instant are moving towards one extremity of the line as are moving towards the opposite extremity—the resolved components of the motions along the line being taken when the motions are oblique. It appears therefore that in order to bring gas rarefied to one millionth under the normal conditions for correcting the motions of its molecules (so as to move in the normal "radiant" manner); it would be necessary to employ a containing vessel of such size that the molecules can adjust their motions freely by mutual encounters. Hence a containing vessel whose diameter was a considerable multiple of the mean path (four inches in this case) would be required—say some feet in diameter at least.

the peculiar state of matter observed by Mr. Crookes does not depend on the rarefaction of the gas, but on the dimensions of the bulb (or confining envelope) relatively to the mean path—inasmuch as if it were possible to construct a bulb approximating to the mean path of the molecules of gas at (or near) normal density, analogous phenomena would inevitably occur, though of course they could not be observed by very small dimensions. [Besides the electric discharge cannot so readily take place in dense gas.] What is done therefore is to raise the mean path approximately up to the diameter of the bulb (by a high degree of rarefaction), instead of—conversely—diminishing the bulb down to the length of mean path (at a lower degree of rarefaction); when the effect would be difficult to perceive from the smallness of scale. It will be observed that it is only a question of scale (rarefaction being a mere relative thing)—only it becomes possible to use a bulb or containing vessel of larger size (to produce the conditions) in direct proportion as the rarefaction is greater; so that the whole effect becomes more magnified and distinct. The truth of this view may be more apparent by considering the case of the atmosphere when, at different heights, different degrees of rarefaction prevail Let us take the heights where the mean path of the molecules is (say) one-tenth of an inch, one inch, and ten inches respectively. Then at all these heights (as at ordinary density) the molecules of the gas move in the same normal "radiant" manner, or there is nothing peculiar about the state of the gas at any degree of rarefaction. If now a portion of gas be inclosed at each of these heights in bulbs of one-tenth of an inch, one inch, and ten inches in diameter respectively: then the gas in all these bulbs will be in an abnormal condition, or in that peculiar state where it has ceased to have the power of adjusting its pressure, and consequently the phenomena of diverting the molecules (by suitable means, electric, &c.) into any paths at desire will be possible in all the bulbs. These considerations will perhaps contribute something towards clearing up any difficulties or divergence of views as to the theoretic aspects of this question—which happens to trench on a line of inquiry pursued by the present writer for some years. Returning to our former example, it may be instructive to consider what takes place on further rarefying. Suppose the rarefaction to be carried to another millionth, by opening out our cubical room into another whose linear side is 100 times greater, viz., 10,000 feet. Here the mean distance of the molecules becomes one seven-hundredth of an inch (multiplying by a hundred)—still a very small quantity, it will be observed. It may be remarked that by this degree of rarefaction (about a million times further than a good mercurial pump could attain) there are still no less than 340 million molecules in each cubic inch of the space. The mean path however has now sprung to sixty miles—greater than the dimensions of the room (by about twenty to thirty times).[1] Our room has therefore approached the state of a confined bulb where the molecules of gas have lost control over themselves, or cannot adjust their motions so as to move in a "radiant" manner, but the molecules rebound irregularly backwards and forwards from one wall to the other, without (as a rule) colliding together, and may produce considerable irregularities of pressure. In order to restore the uniformity of pressure, and reproduce the normal "radiant" form of motion, it would be necessary to open out our room into another a considerable multiple of sixty miles in the side (the mean path)—adding fresh gas so as to leave the density unchanged. Here we should have molecules moving in streams and passing within (on an average) one seven-hundredth of an inch of each other, and "radiating" from each point of the room with perfect symmetry to a distance of many miles, like the rays of light from a luminous point. In this case we should have molecules capable of becoming virtual carriers of energy to radial distances such as might really in principle serve to some extent the practical object required in the case of light.

If we imagine (for further illustration) the rarefaction carried a million times beyond this—viz., to a millionth × a millionth × a millionth of an atmosphere—then the mean distance of the molecules would still only have risen to the small amount of one-seventh of an inch; but the mean length of path sixty million miles (about). We are thus approaching astronomical distances. It seems a curious fact to consider that a portion of matter can be projected among other portions only one-seventh of an inch apart, so as to move (on the average) sixty million miles without touching one of them. This may form an illustration of the smallness of molecules. A hydrogen molecule moving at about four times the velocity of a cannon ball (its normal rate) would take, calculably, about a year and three quarters to traverse its mean path under these conditions.

These considerations may serve to show, or facilitate the conceptions as to how particles of matter may have an extremely small mean distance and yet have an extremely long mean path. For it is readily conceivable that since (as has been mathematically proved by Clausius and others) the mean length of path of a particle increases, *cæteris paribus*, as the *square* of its diameter diminishes (a rapid rate)—particles, such as those of the æther, for instance, may have such an adequately small diameter as to admit of being in very close proximity, and yet their mean path extremely great (many millions of miles long perhaps). These conclusions, rendered more interesting by the additional light thrown on streams of molecules in the gaseous state by the experimental researches of Mr. Crookes—would therefore point, in their possible application to the æther, to a possible means for carrying energy in a "radiant" manner, producing gravity (or the general phenomena of *approach*), and capable of serving as a great source of motion, the transferences of which are illustrated and exemplified in the motions developed in gross matter on every hand, and which to the appreciative mind who will not admit the *creation* of motion, inevitably demand the presence of an agent inclosing a hidden store of motion. The above view would also have the advantage of correlating the æther with ordinary matter (as merely a body consisting of very much finer molecules—or a difference of scale). Why should we suppose the æther to be something abnormal or different from ordinary matter, without positive evidence? Would not this be a deviation from the rule of admitting *one* principle as sufficient until two are found to be necessary? This also holds in regard to energy. Why countenance at all *two* kinds of energy until we have evidence, or why deviate from *one* grand fundamental principle until we are forced to do so—hardly a probable event, especially when this deviation involves something like a rush into the inconceivable represented by an energy *without motion*?[1]

In conclusion it should be observed that there is nothing hypothetical in the above deductive results re-

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[1] It evidently follows from these considerations that if it were possible by some practical means to expand a glass bulb after rarefying; the mean path of the molecules of the inclosed gas would increase three times as fast as the diameter of the bulb.

[1] The apparently logical plan of admitting *one* principle until *two* are shown to be necessary would appear to be reversed in the case of energy. It would seem that *two* kinds of energy are first believed in, because the existence of one kind is not (as it is said) *physically* proved yet—*i e.* proved in such a way as to be obvious to our gross senses, and not merely a deduction derived from pure reasoning based on the observed and otherwise inexplicable developments of motion taking place in gross matter everywhere around us. Some might think that the contrary procedure to the above would be the more logical—viz., to believe in one kind of energy, *because the existence of two kinds had not been proved yet.* But in the history of science there has notoriously always been a tendency to lean towards the inconceivable, rather than be contented with what our understanding can teach us, At a future day possibly the recognition that all energy is of *one* character will be thought by some a grand discovery. Some may however think it to be only the correction of an error which ought never to have been committed, for which there was no real justification—all analogy, rationality of conception, and that *oneness* of principle so characteristic of nature pointing the other way.

garding the mean distances, mean paths, &c., of molecules on rarefying gases. For the relations computed depend on known mathematical principles. The only possible ground for question would be the particular data of mean distance, &c., taken as a basis for the calculations. But it should be noticed that these rest on an experimental basis : having been deduced from observed facts by investigators of admitted competence, and by means of several *diverse* lines of argument which are found to accord in a remarkable manner as to the results,—which is therefore strong confirming evidence of their substantial accuracy. Also the above inferences regarding a mechanism for the fundamental purposes of carrying energy, storing energy in equilibrium, and producing effects of approach (such as gravity, &c.), cannot as mechanical facts admit of any question. For mechanical principles (like mathematical truths) hold independently of any inquiry as to whether they actually find practical *application* in nature or not. The best argument for their practical application in nature is the incomprehensibility of observed facts without them. We can at least say with certainty that under such conditions, effects (phenomena of approach,[1] transferences of motion, &c.) of the character observed would be produced,—and which effects have not hitherto found any explanation that appeals to our reason. The certainty of simple and automatic mechanical conditions being conceivable which are capable of producing such important effects, should lend a legitimate interest to these inquiries, and the mechanical beauty of the "radiant" adjustment of moving particles of matter which adapts them to so many noteworthy purposes at once, should surely itself be an argument in favour of the practical application of the scheme in nature,—as a simple means to great and important ends.

S. TOLVER PRESTON

DEEP-SEA OPHIURANS

IN the anniversary *Memoirs* of the Boston Society of Natural History, Prof. Theodore Lyman gives an account of a structural feature hitherto unknown among Echinodermata which he has discovered in deep-sea Ophiurans. The remarkable structures described appear under the microscope as little tufts resembling bunches of simple Hydroids on the sides of the arms of certain Ophiurans. On careful examination these tufts are found to be bunches of minute spines, each inclosed in a thick skin-bag, and in form resembling agarics, or parasols with small shades. They are arranged in two or even three parallel vertical rows, and in this respect the animals on which they occur differ from all other Ophiuridæ known, for all others possess a single row only of articulated spines. The peculiar tufts, which are apparently homologous with pedicellariæ, are attached to the outer joints of the arms, near the margins of the side arm-plates. Two new genera, Ophiotholia and Ophiohelus, closely allied to Ophiomyces, are described in which these curious appendages occur. The species of the genera are soft with imperfect calcification. Examples of Ophiotholia were dredged off Juan Fernandez, in 1825 fathoms, and of Ophiohelus off Barbadoes in 82 fathoms, and off Fiji in 1350 fathoms.

Prof. Lyman states that among the Ophiuridæ and Astrophytidæ of the *Challenger* Expedition the entire number of new genera brought home is 20; that of species 167.

AN ELECTRICAL THERMOMETER FOR DETERMINING TEMPERATURES AT A DISTANCE

THE success of many industrial operations depends upon the steady maintenance or proper variation of certain temperatures, and it is often of the highest importance that the person in charge of these operations should be able readily to ascertain by means of the thermometer if the workmen are performing their duties correctly. It sometimes happens that thermometers have to be placed in positions which are difficult of access, or removed some distance from the centre of the manufactory, and that considerable time has to be expended in visiting the different stations. It was in order to meet the requirements of such a case as this that the electro-thermometric apparatus here described was constructed.

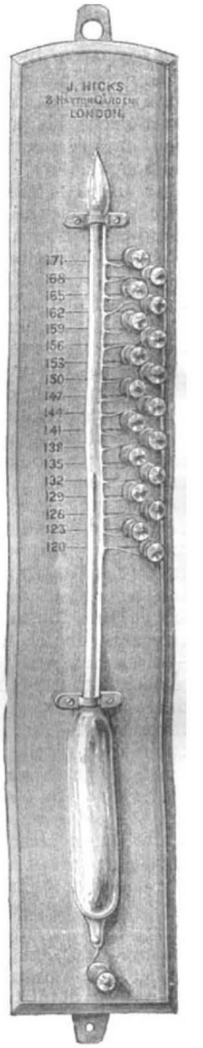

FIG. 1.

I had for some time been much in need of an instrument which would admit of the temperature of a series of malt-drying kilns being determined at a considerable distance from the kilns themselves, and, not being able to meet with a description of a suitable instrument, I was led, after several trials, to contrive this apparatus, which, although it does not embody any new principle, and is not perhaps adapted to accurate meteorological work, is nevertheless very suitable for the technical purpose for which it was originally designed, and is doubtless capable of extended application in many industries.

The apparatus consists essentially of two parts, a mercurial electro-thermometer, and a combination of apparatus which constitutes an automatic receiver and transmitter of signals from the thermometer.

The thermometer, which is shown in Fig. 1, was constructed for me by Mr. J. Hicks of Hatton Garden. It is an ordinary thermometer about nine inches in height, with a large bulb and a stem of wide bore. Through the side of the stem, and fused into the glass, are inserted a series of short platinum wires, the free end of each being connected with a binding screw. These wires, which project slightly into the bore of the thermometer, are, in my instrument, inserted at intervals of 3° F. between 120° and 171°, the range of temperature required in this case. The constructor of this part of the apparatus informs me that, if necessary, there is no practical difficulty in inserting wires at intervals of a single degree, or even less, without interfering with the calibration of the tube. The upper part of the bore of the tube is expanded

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[1] It would not be difficult substantially to imitate what occurs in gravitation (according to the dynamical theory), by cooling down the opposed faces of two metal disks freely suspended in a moderately large vessel of rarefied gas, at a less distance apart than the mean length of path of the gaseous particles,—when from known principles (already experimented on by Mr. Crookes) the two disks would approach. Here the diminished velocity of rebound of the gaseous particles from the cooled inner surfaces of the disks (which entails the approach), is imitated in gravitation by a similar diminished velocity of rebound of the gravific particles from gross matter, owing to their translatory motion being partly shivered into vibration (and rotation) at the shock of impact against gross matter (in a manner elucidated by Sir W. Thomson, *Phil. Mag.*, May, 1873). On a large scale, a similar diminution of translatory motion at impact is universally illustrated by the known retarded rebound of elastic masses at collision,—when part of the translatory motion is (in a somewhat analogous way) converted into a vibratory or rotatory motion of the colliding body at the encounter. It becomes interesting in a dynamical phenomenon of the nature of gravitation to contemplate the possibility of doing something towards illustrating it experimentally, and to acquire the certainty of the existence of the streams of particles which produce the effect,—by almost visualising them, through the means employed in the recent researches by Mr. Crookes.