was fond of adopting the free discourse and chaff of school-boy days. His friendship was like that of the explorers and prairie-hunters of whom he loved to read—absolutely staunch. If you had the good fortune to be his "chum," he would stand by you through thick and thin, and share all he had with you. I do not think there was any limit to what he would have done for his friend. We took our degrees together in 1868; and in the following spring—he having been elected Radcliffe Travelling Fellow, and I Burdett-Coutts Scholar—we spent six weeks in the Auvergne and the country between that and Marseilles. In the following winter (February 1870) we took up our quarters together at Vienna, and studied with Stricker, and in Rokitanski's laboratory. He entered, on our return, at University College, London, as a student of the Medical Faculty. In 1871, after his winter medical session, he joined me at Leipzig, where his great abilities were discerned and thoroughly appreciated by Prof. Ludwig, in whose laboratory we had the privilege of working. His first scientific memoirs were published whilst he was here—one, on the nerves of the cornea of mammals, as shown by the gold method (then not so familiar as it is now), and one on the circulation in the wing of the cockroach.

In the autumn of the same year, Moseley went, as member of the Government Eclipse Expedition, to Ceylon, under Mr. Norman Lockyer, whilst I joined Anton Dohrn at Naples. Moseley made valuable spectroscopic observations of the eclipse at Trincomali, and also brought home a large booty of Land Planarians, which he at once studied by means of sections, going to Oxford for the purpose of using the laboratory and the library attached to the Museum. This admirable piece of work delighted Rolleston, who communicated it to the Royal Society; it was published in the Philosophical Transactions after Moseley had sailed on the *Challenger*, as one of the naturalists of the Expedition, in December 1872. We did not see him again until May 1876, but I had frequent letters from him, and sometimes a small parcel, or some photographs. Of the scientific staff of the Expedition, Wyville Thomson and Suhm are dead, as well as Moseley; John Murray and J. Y. Buchanan are the two survivors. Moseley, although not a botanist, undertook the collecting of plants whenever the Expedition touched land; he also made important anthropological studies on the Admiralty Islanders, and has published a wonderful mass of notes and observations, accompanied by plates and woodcuts, in his "Notes of a Naturalist on the *Challenger*." He showed the stuff he was made of very soon after the Expedition started, viz. on the arrival of the *Challenger* at the Cape. He immediately started off in quest of Peripatus—a strange, imperfectly described beast which we had discussed together over some spirit specimens of it which I had received from Roland Trimen, of Cape Town. Moseley had made up his mind before he left England to "tackle" Peripatus, and he did so. He obtained living specimens, discovered the tracheæ and the most important features in the development, showing that the "jaws" are in-turned parapodia—and sent home a memoir which was at once published in the Philosophical Transactions. In the later part of the voyage he was occupied with the corals, and especially the Millepores and Stylasterids. The wonderfully elaborate plates, and the discovery they embodied, necessitating the formation of a new group of animals, the Hydrocorallinæ, were the first-fruits of his voyage which he produced on landing in 1876. During his absence both his father and his mother had died. His old College, Exeter—where I became a Fellow in the year of the *Challenger's* departure—now was inspired through the good offices of an eminent Greek scholar, with the happy thought of offering Moseley a Fellowship and a home in the College, so that he found on landing a welcome awaiting him, and a place in which to store for a while his treasures. I do not think that a College Fellowship was ever better bestowed: that was in the good old days before Lord Selborne's Commission. In his rooms in Exeter, Moseley displayed his Japanese and Melanesian curiosities, and wrote many papers embodying the observations made during his voyage, besides the book above mentioned. He was elected F.R.S. in 1879, and after a visit to Oregon (of which he published an account) was appointed (1879) Assistant Registrar of the University of London. He took up his residence in Burlington Gardens, but not for long. In 1881 he married the youngest daughter of Mr. J. Gwyn Jeffreys, F.R.S., the distinguished conchologist, and in the same year was elected, on the death of his teacher and close friend, Prof. Rolleston, to the Linacre Professorship in the University of Oxford.

Moseley had had no previous experience in teaching, but he set to work with that unbounded energy and strength which characterized him. He spared no pains to make his lectures absolutely up to date, and arranged a thorough laboratory course extending over two years to illustrate them. The regulations of the University as to examinations and curriculum were at that time not unfavourable to the study of animal morphology, and Moseley usually had ten or a dozen serious students besides the elementary class. Lincoln, University, and New Colleges encouraged his and their efforts by offering and awarding Fellowships to students of the University distinguished in animal morphology; and after six years all was progressing as satisfactorily as possible, when he was attacked by illness which brought his work to an end. Not only was he unable to carry on his work, but his absence naturally enough was unfavourable to the interests of those studies which he would have fostered and guarded, had he been able to take part in the legislation of the University.

During the happy and busy six years which Moseley spent as Linacre Professor at Oxford, he trained Bourne, Hickson, and Fowler to carry on his coral work; with Baldwin Spencer he investigated the pineal eye of Lacertilia, and himself published his remarkable discovery of eyes and other sense-organs in the shells of Chitonidæ. He was largely instrumental in securing the Pitt-Rivers collection of anthropological objects for the University, and superintended the preliminary arrangement of the collection in the building erected for it. He served twice on the Council of the Royal Society, was a founder and member of Council of the Marine Biological Association, and was President of Section D of the British Association at the Montreal meeting.

His love of travel was shared by his wife, who went with him from Montreal to Arizona to visit the town-building Indians of that remote region, and who, only a year before his illness, accompanied him on an Easter holiday trip to Tangier and Fez. During his illness she has been his constant companion. He leaves, besides her, two daughters and a son.

E. RAY LANKESTER.

ON THE VIRIAL OF A SYSTEM OF HARD COLLIDING BODIES.

A RECENT correspondence has led me to examine the manner in which various authors have treated the influence of the finite size of molecules in the virial equation, and I should like to lay a few remarks upon the subject before the readers of NATURE.

To fix the ideas, we may begin by supposing that the molecules are equal hard elastic spheres, which exert no force upon one another except at the instant of collision. By calling the molecules hard, it is implied that the collisions are instantaneous, and it follows that at any moment the potential energy of the system is negligible in comparison with the kinetic energy.

If the volume of the molecules be very small in comparison with the space they occupy, the virial of the impulsive forces may be neglected, and the equation may be written

$$pv = \tfrac{1}{3}\Sigma m V^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1$$

where p is the pressure exerted upon the walls of the inclosure, v the volume, m the mass, and V the velocity of a molecule.

In his essay of 1873 Van der Waals took approximate account of the finite size of the molecules, using a peculiar process to which exception has been taken by Maxwell and other subsequent writers. It must be said, however, that this method has not been proved to be illegitimate, and that at any rate it led Van der Waals to the correct conclusion—

$$p(v - b) = \tfrac{1}{3}\Sigma m V^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which b denotes four times the total volume of the spheres. In calling (2) correct, I have regard to its character as an approximation, which was sufficiently indicated by Van der Waals in the original investigation, though perhaps a little overlooked in some of the applications.

In his (upon the whole highly appreciative) review of Van der Waals's essay, Maxwell (NATURE, vol. x. p. 477, 1874) comments unfavourably upon the above equation, remarking that in the virial equation v is the volume of the vessel and is not subject to correction.[1] "The effect of the repulsion of the molecules causing them to act like elastic spheres is therefore to be found by calculating the virial of this repulsion." As the result of the calculation he gives

$$pv = \tfrac{1}{3}\Sigma m V^2\left\{1 - 2\log\left(1 - 8\frac{\rho}{\sigma} + \frac{17\rho^2}{\sigma^2} - \ . \ . \right)\right\}, \ . \ . \ (3)$$

where σ is the density of the molecules, and ρ the mean density of the medium, so that $\rho/\sigma = b/4v$. If we expand the logarithm in (3), we obtain as the approximate expression, when ρ/σ is small,

$$pv = \tfrac{1}{3}\Sigma m V^2(1 + 4b/v), \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or, as equally approximate,

$$p(v - 4b) = \tfrac{1}{3}\Sigma m V^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which does *not* agree with (2).

The details of the calculation of (3) have not been published, but there can be no doubt that the equation itself is erroneous. In his paper of 1881 (*Wied. Ann.*, xii. p. 127), Lorentz, adopting Maxwell's suggestion, investigated afresh the virial of the impulsive forces, and arrived at a conclusion which, to the order of approximation in question, is identical with (2). A like result has been obtained by Prof. Tait (Edin. Trans., xxxiii. p. 90, 1886).

It appears that, while the method has been improved, no one has succeeded in carrying the approximation beyond the point already attained by Van der Waals in 1873. But a suggestion of great importance is contained in Maxwell's equation (3), numerically erroneous though it certainly is. For, apart from all details, it is there implied that the virial of the impacts is represented by $\frac{1}{3}\Sigma m V^2$, multiplied by some function of ρ/σ, so that, if the volume be maintained constant, the pressure as a function of V is proportional to $\Sigma m V^2$. The truth of this proposition is evident, because we may suppose the velocities of all the spheres altered in any constant ratio, without altering the motion in any respect except the scale of time, and then the pressure will necessarily be altered in the square of that ratio.

It will be interesting to inquire how far this conclusion is limited to the suppositions laid down at the commencement. It is necessary that the collisions be instantaneous, in relation, of course, to the free time. Otherwise, the similarity of the motion could not be preserved, the duration of a collision, for example, bearing a variable ratio to the free time. On the same ground, vibrations within a molecule are not admissible. On the other hand, the limitation to the spherical form is unnecessary, and the theorem remains true whatever be the shape of the colliding bodies. Again, it is not necessary that the shapes and sizes of the bodies be the same, so that application may be made to mixtures.

In the theory of gases, $\Sigma m V^2$ is proportional to the absolute temperature; and whatever doubts may be felt in the general theory can scarcely apply here, where the potential energy does not come into question. So far, then, as a gas may be compared to our colliding bodies, the relation between pressure, volume, and temperature is

$$p = T\phi(v), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where $\phi(v)$ is some function of the volume. When v is large, the first approximation to the form of ϕ is

$$\phi(v) = \frac{A}{v}.$$

In the case of spheres, the second approximation is

$$\phi(v) = \frac{A}{v} + \frac{Ab}{v^2},$$

where b is four times the volume of the spheres.

Thus far we have supposed that there are no forces between the bodies but the impulses on collision. Many and various phenomena require us to attribute to actual molecules an attractive force operative to much greater distances than the forces of collision, and the simplest supposition is a cohesive force such as was imagined by Young and Laplace to explain capillarity. We are thus led to examine the effect of forces whose range, though small in comparison with the dimensions of sensible bodies, is large in comparison with molecular distances. In the extreme case, the influence of the discontinuous distribution of the attractive centres disappears, and the problem may be treated by the methods of Laplace. The modification then required in the virial equation is simply to add[1] to p a term inversely proportional to v,[2] as was proved by Van der Waals; so that (6) becomes

$$p = T\phi(v) - av^{-2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

According to (7), the relation between pressure and temperature is *linear*—a law verified by comparison with observations by Van der Waals, and more recently and extensively by Ramsay and Young. It is not probable, however, that it is more than an approximation. To such cases as the behaviour of water in the neighbourhood of the freezing-point it is obviously inapplicable.

In their discussions, Ramsay and Young employ the more general form—

$$p = T\phi(v) + \chi(v); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and the question arises, whether we can specify any generalization of the theoretical conditions which shall correspond to the substitution of $\chi(v)$ for av^{-2}. It would seem that, as long as the only forces in operation are of the kinds, impulsive and cohesive, above defined, the result is expressed by (7); and that if we attempt to include forces of an intermediate character, such as may very probably exist in real liquids, and must certainly exist in solids, we travel beyond the field of (8), as well as of (7). It may be remarked that the equation suggested by Clausius, as an improvement on that of Van der Waals, is not included in (8).

[1] In connection with this it may be worth notice that for motion *in one dimension* the form (2) is *exact*.

[1] It thus appears that, contrary to the assertion of Maxwell, p *is* subject to correction. It is pretty clear that he had in view an attraction of much smaller range than that considered by Van der Waals.

Returning to the suppositions upon which (7) was founded, we see that, if the bodies be all of one *shape*, *e.g.* spherical, the formula contains only two constants—one determining the size of the bodies, and the second the intensity of the cohesive force; for the mean kinetic energy is supposed to represent the temperature in all cases. From this follows the theorem of Van der Waals respecting the identity of the equation for various substances, provided pressure, temperature, and volume be expressed as fractions of the critical pressure, temperature, and volume respectively. If, however, the *shape* of the bodies vary in different cases, no such conclusion can be drawn, except as a rough approximation applicable to large volumes. RAYLEIGH.

Terling Place, Witham, November 18.

THE IMPLICATIONS OF SCIENCE.[1]

II.

I MIGHT now at once return to further consider those implications of science to which I have called your attention, but I think it will be better to first briefly pass two important matters in review.

The first concerns our means of investigation as to such fundamental questions.

The second relates to our ultimate grounds for forming judgments about them. We have to consider how fundamental truth can be acquired and tested.

Evidently the only means of which we can make use are our *thoughts*, our reason, our intellectual activity. "Thoughts" may be, and should be, carefully examined and criticized; but however much we may do so, and whatever the results we arrive at, such results can only be reached by thoughts, and must be expressed by the aid of our thoughts. This will probably seem such a *manifest* truism that I shall be thought to have committed an absurdity in enunciating it. To suppose that by any reasoning we can come to understand what we can never think, may seem an utterly incredible folly; yet at a meeting of a Metaphysical Society, in London, a speaker, not long ago, expressly declared "thought" to be a misleading term, the use of which should be avoided.

Now I am far from denying that unconscious activities of various different orders take place in our being, yet whatever influence such activities may have they cannot affect our judgments save by and in thoughts.

If a man is convinced that thoughts are worthless tools, he can only have arrived at that conclusion by using the very tools he declares to be worthless. What, then, ought his conclusion to be worth even in his own eyes?

It is simply impossible by reason to get behind or beyond conscious thought, and our thoughts are and must be our only means of investigating problems however fundamental.

Even in investigating the properties of material bodies, it is to self-conscious reflective thought that our final appeal must be made.

For it is to our thoughts, and not to our senses only, that our ultimate appeal must be made, even with respect to the most material physical science matters.

Some persons may imagine that with respect to investigations about the properties of material bodies, it is to our sensations alone that we must ultimately appeal. But it is not so; anyone would be mad to question the extreme importance, the absolute necessity, of our sensations in such a case. Nevertheless, after we have made all the observations and experiments we can, how can we know we have obtained such results as we may have obtained, save by our self-conscious thought? By what other means are we to judge between what may seem to be the conflicting indications of different sense impressions?

Our senses are truly tests and causes of certainty, but not *the* test. Certainty belongs to thought, and self-conscious reflective thought is our ultimate, absolute criterion.

As to the ultimate grounds on which our judgments respecting such problems must repose, as Mr. Arthur Balfour has forcibly pointed out, that it is a question altogether distinct from that of the origin of our judgments, or from reasonings about their truth. Such matters are very interesting, but they are not here in point, since it is plain that no proposition capable of proof can be one the certainty of which is fundamental. For, in order to prove anything by reasoning, we must show that it necessarily follows as a consequence from other truths, which therefore must be deemed more indisputable. But the process must stop somewhere. We cannot prove everything. However long our arguments may be, we must at last come to ultimate statements, which must be taken for granted, like the validity of the process of reasoning itself, which is one of the implications of science. If we had to prove either the validity of that process or such ultimate statements, then either he must argue in a circle, or our process of proof must go on for ever without coming to a conclusion, which means there could be no such thing as "proof" at all.

Therefore the "grounds of certainty" which any fundamental proposition may possess cannot be anything *external* to it—which would imply this impossible proof. The only ground of certainty which an ultimate judgment can possess is its own *self-evidence*—its own manifest certainty *in and by itself*. All proof, all reasoning, must ultimately rest upon truths which carry with them their own evidence, and do not therefore need proof.

It is possible that some of my hearers may be startled at the suggestion of believing anything whatever on "its own evidence," fancying it is equivalent to a suggestion that they should believe anything *blindly*. This, I think, is due to the following fact of mental association. The immensely greater part of our knowledge is gained by us indirectly—by inference or testimony of some kind.

We commonly ask for some proof with regard to any new and remarkable statement, and no truths are brought more forcibly home to our minds than are those demonstrated by Euclid. Thus it is that many persons have acquired a feeling that to believe anything which cannot be proved, is to believe *blindly*. Hence arises the tendency to distrust what is above and beyond proof. We are apt to forget, what on reflection is manifest—namely, that if it is not blind credulity to believe what is evident to us by means of something else, it must be still less blind to believe that which is directly evident in and by itself.

And self-conscious reflective thought tells *me* clearly, that the law of contradiction is not only implied by all science, and necessary to the validity of all science, but that it is, as I said, an absolute, necessary truth which carries with it its own evidence. It must be a truth, then, applicable both to the deepest abyss of past time and the most distant region of space. But here, again, I think it possible that one or two of my hearers may be startled, and perhaps doubting how things in this respect may be in the Dog-star now, or how they were before the origin of the solar system. I fancy I hear someone asking: "How is it possible that we, mere insects, as it were, of a day, inhabiting an obscure corner of the universe, can know that anything is and must be true for all ages and every possible region of space?"

In the first place, I think the difficulty which may be thus felt is due to the abstract form of the law of contradiction. And yet, as I said before, it is but the summing up of all the particular instances, as to each one of which no difficulty at all is felt, but each is clearly

[1] Friday Evening Discourse delivered at the Royal Institution by Dr. St. George Mivart, on June 5, 1891. Continued from p. 62.