sea very smooth; barometer 29·83 in., rising; temperature of air 33°, of sea 34°.

S.s. *Labrador*, Captain A. Gray; July 14, 1882, about 56° N., 60° W.—"Saw an iceberg, which collapsed with a thundering squash." Next day, "icebergs all over the place, with an occasional collapsing roar."

It may be thought, perhaps, that the thundering roars of collapsing bergs would be the explanation of the six reports heard by Captain Deuchars; but as he had many years' experience whaling and seal fishing in the ice on both sides of Greenland, he would be well acquainted with the sounds from bergs breaking up, and there must have been, therefore, something peculiar about the "guns" to lead him to suggest an electrical origin.

With reference to M. Van den Broeck's explanation of the expression *mist-poeffers* as *fog-belchings*, not *fog-dissipators*, it may be interesting to add another extract from Captain Deuchars's log: June 15, 1883, in 71° N., 11° W.—"Weather dense fog, with a white bow to south-east, known generally as a *fog-scaffer* or *demolisher*." Hy. Harries.

Meteorological Office, January 17.

With reference to the letters on this subject which have recently appeared in your pages, and more especially to the communication of my friend Rev. W. S. Smith, relative to Lough Neagh, the following extract from my notes may be of interest: "August 27, 1886.—While standing with Mr. S. A. Stewart in a recently-mown meadow, near Portmore Lough, on the eastern side of Lough Neagh, our attention was attracted by a rumbling noise. The day was very fine and warm, and dead calm, not a leaf stirring, and a few very light clouds were in the sky. The noise was like a short distant peal of thunder, but sounded faint rather than distant. While we watched, a whirlwind suddenly appeared in the direction whence the sounds had come [the north], and at a distance of about a hundred yards from us. A quantity of loose hay was instantly whirled upward to a height of about 100 feet, and, after floating about in circles, slowly settled down. A haycock at the spot was much disturbed, and presented the appearance of having endured a gale of wind. The time between the rumbling sound (which closely resembled the distant report of a cannon) and the appearance of the whirlwind was about half a minute, and the whirlwind lasted somewhat over a minute."

In W. H. Patterson's "Glossary of Words of Antrim and Down," we find the following: "Water Guns.—Sounds as of gunshots, said to be heard around the shores of Lough Neagh by persons sailing on the lake. The cause of the sounds, which are generally heard in fine weather, has not been explained." There is no doubt that the sound we heard was the mysterious "water guns," and there is also little doubt that the noise and the appearance of the whirlwind were closely connected.

R. Lloyd Praeger.

In connection with the recent correspondence upon "Remarkable Sounds," the following quotation may be interesting. It occurs as a footnote in a paper by Prof. S. A. Forbes, of Illinois, upon the "Aquatic Invertebrate Fauna of the Yellowstone National Park, &c.," published in the *Bulletin* of the U.S. Fish Commission for 1891 (Washington, 1893), p. 215.

"Here we first heard, while out on the lake [Shoshone Lake, Yellowstone National Park] in the bright still morning, the mysterious aerial sound for which this region is noted. It put me in mind of the vibrating clang of a harp, lightly and rapidly touched, high up above the tree-tops, or the sound of many telegraph wires swinging regularly and rapidly in the wind, or, more rarely, of faintly-heard voices answering each other overhead. It begins softly in the remote distance, draws rapidly near with louder and louder throbs of sound, and dies away in the opposite distance; or it may seem to wander irregularly about, the whole passage lasting from a few seconds to half a minute or more. We heard it repeatedly and very distinctly here and at Yellowstone Lake, most frequently at the latter place. It is usually noticed on still, bright mornings not long after sunrise, and it is always louder at this time of day; but I heard it clearly, though faintly, once at noon, when a stiff breeze was blowing. No scientific explanation of this really bewitching phenomenon has ever been published, although it has been several times referred to by travellers, who have ventured various crude guesses at its cause, varying from that commonest catch-all of the ignorant, 'electricity,' to the whistling of the wings of ducks and the noise of the 'steamboat geyser.' It seems to me to belong to the class of aerial echoes, but even on that supposition I cannot account for the origin of the sound." D. J. Scourfield.

Leytonstone, January 20.

It may be worth while to put on record the following statements of the distances at which the firing of guns have been heard. They were related to me by the late Prof. C. J. Harris, of Washington, and Lee University, Lexington, Virginia, who, in speaking of the distances at which sounds could be heard, said that during "The War"—the Civil War of 1861-65—he had frequently heard the firing of the guns in battles taking place many miles from Lexington; and so distinct were the reports, that it was easy to distinguish between light and heavy artillery. In particular, I remember his saying that the sound of the cannonading at the Battle of Malvern Hill was distinctly heard at Lexington. Malvern Hill is about 123 miles "as the crow flies" from Lexington. At this battle gunboats were used by the Federals, and the reports of the heavy guns on the boats could be easily distinguished.

He also said that during the Battle of Manassas—or, as it is also called, Bull Run—the cannonading was heard at Lexington. The battle-field is about 125 miles from Lexington. These distances have been furnished me by the Assistant Superintendent of the U.S. Coast and Geodetic Survey, and are accurate within a mile or two. W. G. Brown.

Washington, D.C., U.S.A., January 3.

The Place of "Pithecanthropus" on the Genealogical Tree.

Writing to Nature (January 16), under the above heading, Dr. Eugene Dubois makes the following statement: "In Prof. Cunningham's tree, figured in Nature of December 5, p. 116, he regards the left branch as all human, the right one as entirely simian, and he placed Pithecanthropus midway between recent Man and the point of divarication." In this assertion there are two inaccuracies. I do not regard the left branch as being entirely human, but merely as representing a hypothetical line of human descent. During the debate which took place at the Royal Dublin Society, I was most careful to insist that at a certain point on such a line (marked on the diagram by a ×, Nature, December 5, p. 116), we might expect to meet with an individual possessing ape-like and human characters in equal degree; whilst below that point ape-like characters would predominate, and the human characters diminish until, probably, before we came to the junction of the line with the main stem, the latter had reached a vanishing point. But, again, I did not place Pithecanthropus on the mid-point of the line, but much lower down, as may be seen by a refeience to the diagram itself, where the upper mark of interrogation (?) indicates the place which I assigned to the fossil cranium.

I would wish to add that my diagram was not drawn with the view of elaborating in any detail a genealogical tree of Man and the Anthropoid apes, but simply for the purpose of eliciting from Dr. Dubois his views regarding the place he wished to assign to Pithecanthropus in relation to Man on the one hand, and the existing Anthropoid apes on the other.

It seemed to me that a definite statement from Dr. Dubois on this point was desirable, seeing that I considered that the title he had given to his memoir was apt to lead to misconception. D. J. Cunningham.

THE CHEMICAL SOCIETY'S HELMHOLTZ MEMORIAL LECTURE.

In his Helmholtz memorial lecture, delivered last Thursday, Prof. G. F. Fitzgerald gave an able exposition and development of those branches of the work of the late Prof. Helmholtz which intimately affect chemistry, and at the same time made an important contribution to several much-vexed questions of higher chemical physics. A brief account of the chief points of the lecture is given in the following abstract.

Helmholtz made the great discovery that, by virtue of their vorticity, vortex rings floating in a perfect fluid are unable to destroy or create one another; although these vortices may distort each other, becoming drawn out into thin threads or rolled into spherical balls, one cannot destroy another. This discovery it was that afforded a basis for those speculations of Lord Kelvin which would identify atoms with vortex rings moving in a perfect fluid; the indestructibility of atoms finds a parallel in the permanency of vortex rings, and the two have many properties in common. As, however, our knowledge of vortices has increased, so obstacles to the acceptance of the atomic vortex hypothesis have arisen. Thus the energy and the inertia of vortex rings increase together whilst their rate of motion decreases, so that on raising the temperature of a gas composed of vortex atoms, and therefore increasing the rate of motion of its particles, it would seem that, in some mysterious way, more energy leaves the gas than enters it. Similarly, unless the weight of a body alters appreciably as its temperature changes, it is not easy to see how the simple vortex theory of matter can be true; the difficulty of determining weights at different temperatures of course stands in the way of an experimental examination of this point. Many modifications of the vortex theory have been proposed, but the only statement that can be made with certainty is that the space between the atoms, whatever their nature may be, must be filled with some complicated structure, the postulation of which is essential for the explanation of electro-magnetic actions. It is therefore impossible to believe that atoms are simply thin vortices floating in an otherwise motionless and structureless medium.

A curious analogy is noticeable between the stability of vortex systems and chemical valency. A system of two vortex rings, both rotating in the same direction, assumes a state of fairly stable equilibrium in which the two rotate round one another, whilst a system of three vortex rings is stable in a state in which the vortices are situate at the apices of a triangle. Similarly, a condition of stable equilibrium is possible for systems of four, five, or six rings; a system of seven vortex rings, however, is unstable, and vortex systems generally become unstable when composed of more than six rings. The curious analogy between this result and the fact that the atom of no chemical element requires to combine with more than six monovalent atoms, should be kept in view in default of a sounder dynamical conception respecting the limitation of chemical bonds.

The atomic vortex theory again meets with difficulties in connection with homologous series of organic compounds and with the atomic weights; the atomic weight of mercury is 200, that of hydrogen being unity, and it can be shown that the volume occupied by the mercury atom should be some 2800 times that occupied by the atom of hydrogen, a result hardly reconcilable with the known properties of these elements. Valency also presents obstacles to the theory; thus nitrogen and carbon should be respectively mono- and di-valent unless the vortex rings are doubled on to themselves, and even then the doubling indicates the existence of two allotropic modifications of carbon, a right- and a left-handed form, for which no evidence exists. The vortex theory of atoms and the experimental facts regarding atoms are thus sadly at variance, and much still remains to be done in clearing up the questions at issue.

The theory of semipermeable membranes, which is of such importance in certain branches of physical chemistry, is as yet in a very unsatisfactory state. The absolute disregard of any possible heating effects occurring during osmosis may lead to serious errors, corresponding to those which crept into the theory of galvanic cells by neglect of the thermal effects which arise when electrical currents enter or leave a liquid; possible causes of error, such as these, should be well borne in mind until the theory and practice of semipermeable cells are in better agreement than at present. These semipermeable membranes are frequently regarded as being only some kind of molecular sieves, although they are really much more analogous to Graham's second class of membranes, which only allow the passage of gases soluble in the membrane itself; the laws governing the two kinds of membranes are quite dissimilar. It is not easy to sharply distinguish between physical and chemical permeability when molecular magnitudes are dealt with, and one molecule may pass amongst others not so much by reason of possessing the right size as the right shape. There seems some hope of extending our methods of "chemical filtration" by means of sets of properly constituted diaphragms, each of which is penetrable by certain classes of molecular groups.

The application of thermodynamics to chemical investigations is full of pitfalls; the law of conservation of energy has been often misapplied, and it is not sufficiently realised that the second law of thermodynamics is not strictly applicable to irreversible chemical changes, such as explosions, &c.

The tendency to regard chemical forces as electrical ones is not altogether justifiable; too many instances of irreversible chemical changes exist to permit a parallel between chemical actions and simple reversible electrolysis. Chemical actions are of a far more complex nature than simple electrolysis, and that other than purely electrical forces are operative in solution is indicated by Helmholtz's investigations of electrical diffusion through fine tubes. No statical theory of solid or liquid media which supposes the action of none but electrical forces is possible, for such media would be essentially unstable; as far, then, as solids and liquids can be conceived as statical systems, the postulation of other than purely electrical forces is imperative. The success which has attended the accepted theories of crystal structure and of the asymmetric carbon atom, makes it pretty safe to conclude that many properties of molecules are deducible from purely statical theories of structure.

The enormous increase of knowledge which has attended the assumption that a substance in liquid solution behaves in some important respects like the same substance in a pure gaseous state, has led to the grave error of supposing that the physical conditions of molecules of a substance when gaseous and when dissolved are similar. A dissolved molecule is always within the spheres of action of countless neighbours; its path is of the order of one-hundredth of its diameter, and it receives, perhaps, 10^{14} blows per second, so that its vibrations are comparable with those of radiant heat; in the gaseous state, however, the molecule has a free path thousands of times its diameter in length. The dynamical conditions of gaseous and dissolved molecules are thus absolutely dissimilar. Although it is curious that the osmotic pressure of a dissolved substance should be even roughly identical with the vapour pressure of the same quantity of the substance as a gas under similar conditions of volume and temperature, it is wholly erroneous to attribute this coincidence to a similarity between the dynamical states in the two cases. Osmotic pressure is more nearly related to Laplace's internal pressure in a liquid, which depends on intramolecular forces, than to a gaseous pressure which is practically independent of the forces operating between the molecules. Considerations respecting the capillarity and vapour pressure of solutions and solvents shows that some very close connection exists between osmotic pressure and capillarity, and afford a trustworthy method of applying thermodynamics to the calculation of osmotic pressure.

It is almost impossible to explain dynamically the assumption that free electrically charged ions wander about in a liquid in a condition at all rightly described as one of dissociation. The term "dissociation" should be restricted to a condition in which the components of a molecule are in no way connected by chemical bonds; the possibility of the independent diffusion of the molecular components through porous membranes would afford a simple test as to whether molecules were really dissociated or not. The term dissociation as applied to electrolytes, in which this independence of the ions does not exist, is obviously a misnomer. There is said to be an electrical force acting between the various oppositely charged ions into which a dissolved molecule separates, which in some way still binds them. Even in dilute solutions this force is very considerable, and must make the condition of charged ions moving independently in the liquid so unstable as to be dynamically impossible unless other important forces operate at the same time. Although the present theory of free ions affords a rough working analogy, yet it is illusory and misleading, and threatens to prevent important advances by its illusive appearance of explanation. It must not be forgotten that the older theories of light and the caloric theory of heat constituted stumbling-blocks long after their inadequacy had been conclusively demonstrated.

Prof. Fitzgerald thus contends that the fundamental conceptions underlying many of the current physico-chemical theories, such as those of osmotic pressure and electrolytic dissociation, are dynamically unsound, so that all attempts to gain an insight of what occurs in solution by their aid are necessarily unsuccessful. He seems to consider that an unyielding adhesion to these theories has led to an illogical habit of thought upon such matters, and has made possible the inaccurate application of thermo-dynamical reasoning. W. J. P.

NEW EXPERIMENTS ON THE KATHODE RAYS.[1]

(1) TWO hypotheses have been propounded to explain the properties of the kathode rays.

Some physicists think with Goldstein, Hertz, and Lenard, that this phenomenon is like light, due to vibrations of the ether,[2] or even that it is light of short wavelength. It is easily understood that such rays may have a rectilinear path, excite phosphorescence, and affect photographic plates.

Others think, with Crookes and J. J. Thomson, that these rays are formed by matter which is negatively charged and moving with great velocity, and on this hypothesis their mechanical properties, as well as the manner in which they become curved in a magnetic field, are readily explicable.

This latter hypothesis has suggested to me some experiments which I will now briefly describe, without for the moment pausing to inquire whether the hypothesis suffices to explain all the facts at present known, and whether it is the only hypothesis that can do so. Its adherents suppose that the kathode rays are negatively charged; so far as I know, this electrification has not been established, and I first attempted to determine whether it exists or not.

(2) For that purpose I had recourse to the laws of induction, by means of which it is possible to detect the introduction of electric charges into the interior of a closed electric conductor, and to measure them. I therefore caused the kathode rays to pass into a Faraday's cylinder. For this purpose I employed the vacuum tube represented in Fig. 1. ABCD is a tube with an opening a in the centre of the face BC. It is this tube which plays the part of a Faraday's cylinder. A metal thread soldered at S to the wall of the tube connects this cylinder with an electroscope.

EFGH is a second cylinder in permanent communication with the earth, and pierced by two small openings at β and γ; it protects the Faraday's cylinder from all external influence. Finally, at a distance of about 0·10 m. in front of FG, was placed an electrode N. The electrode N served as kathode; the anode was formed by the protecting cylinder EFGH; thus a pencil of kathode rays passed into the Faraday's cylinder. This cylinder invariably became charged with negative electricity.

The vacuum tube could be placed between the poles of an electro-magnet. When this was excited, the kathode rays, becoming deflected, no longer passed into the Faraday's cylinder, and this cylinder was then not charged; it, however, became charged immediately the electro-magnet ceased to be excited.

In short, the Faraday's cylinder became negatively charged when the kathode rays entered it, and only when they entered it; *the kathode rays are then charged with negative electricity.*

The quantity of electricity which these rays carry can be measured. I have not finished this investigation, but I shall give an idea of the order of magnitude of the charges obtained when I say that for one of my tubes, at a pressure of 20 microns of mercury, and for a single interruption of the primary of the coil, the Faraday's cylinder received a charge of electricity sufficient to raise a capacity of 600 C.G.S. units to 300 volts.

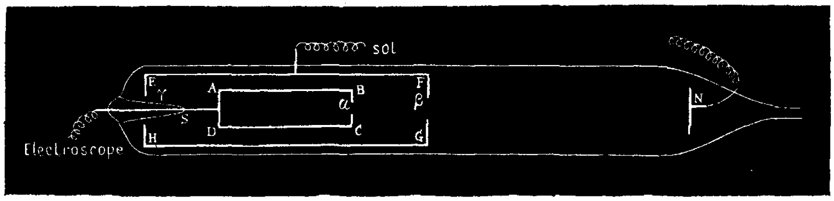

FIG. 1.

(3) The kathode rays being negatively charged, the principle of the conservation of electricity drives us to seek somewhere the corresponding positive charges. I believe that I have found them in the very region where the kathode rays are formed, and that I have established the fact that they travel in the opposite direction, and fall upon the kathode. In order to verify this hypothesis, it is sufficient to use a hollow kathode pierced with a small opening by which a portion of the attracted positive electricity might enter. This electricity could then act upon a Faraday's cylinder inside the kathode.

The protecting cylinder EFGH with its opening β fulfilled these conditions, and this time I therefore employed it as the kathode, the electrode N being the anode. The Faraday's cylinder is then invariably charged with *positive electricity*. The positive charges

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[1] Translation of a paper by M. Jean Perrin, read before the Paris Academy of Sciences on December 30, 1895.

[2] These vibrations might be something different from light; recently M. Jaumann, whose hypotheses have since been criticised by M. H. Poincaré, supposed them to be longitudinal.