LETTERS TO THE EDITOR.

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On the Liquefaction of Gases—A Claim for Priority.

EVER since the year 1883, I have been almost uninterruptedly engaged in the examination of the behaviour of the so-called permanent gases at very low temperatures. During the first few months I performed my experiments together with the late Prof. Wólcowski; afterwards, during a series of years, I was alone; and more lately, I went through several investigations with Prof. Witkowski. The results of my researches I published in the Polish, French, and German languages, whilst they were going on; in the *Reports of the Cracow Academy*, of the Vienna Academy, in *Wiedemann's Annalen*, and in the *Comptes rendus*. My researches are thus well known to the scientific world, and I may add, without boasting, that they have been acknowledged by learned men of different nationalities; they were also known to Prof. Dewar, who repeated them several times, and always confirmed my results—those, for instance, on the absorption spectrum and the bluish colour of liquid oxygen, and on the liquefaction of ozone.

Prof. Dewar at first duly acknowledged those of my experiments which he repeated, but afterwards he changed his behaviour, and in the lectures which he gave in the Royal Institution, and during which he liquefied large quantities of oxygen and air, he never again mentioned that his experiments were merely repetitions of mine, performed and published several years before. This is, perhaps, the reason why the English public, which attended those lectures, grew convinced that the liquefaction of oxygen, and other so-called permanent gases, has been achieved for the first time by Prof. Dewar; and it may be that the Rumford medal awarded by the Royal Society to Prof. Dewar, for the labours which I was the first both to perform and to publish, is due to those very lectures. That my labours should thus have been passed over in silence, is all the more astonishing, because as soon as the description of my apparatus, serving to liquefy large quantities of oxygen and air, was published in 1890, I sent him a reprint of it from the *Bulletin International de l'Académie de Cracovie*. A brief report of the apparatus is also contained in the *Beiblätter of Wiedemann* (vol. xv. p. 29), under the title, "K. Olszewski: Über das Giessen des flüssigen Sauerstoffs."

There is here no space for me to enumerate all my investigations as regards the liquefaction and solidification of the gases in question; but I intend shortly to publish in the English language a more complete summary of my works, by which the English public will be enabled to see that only a small part of the researches which were performed by Prof. Dewar ought to be attributed to him. For the present, I will only state that all the so-called permanent gases (hydrogen alone excepted) were liquefied in quantity for the first time by me, and that I determined their critical points and boiling points; that nitrogen, carbon monoxide, nitric oxide, and methane were also solidified, and their freezing points determined. By means of solid nitrogen I obtained the *lowest temperature* that ever has been both obtained and measured, viz. -225° . Many other gases and liquids were frozen, and their freezing points determined for the first time by me. I must finally remark that I also gave public lectures on the subject in Cracow; the first in 1890, during which I obtained, in the presence of over a hundred students, 100 c.cm. of liquid oxygen; the second in July 1891, during the Congress of Polish Naturalists and Physicians, and then I obtained 200 c.cm. of liquid oxygen in the presence of a good many naturalists, and showed its bluish colour and its absorption spectrum. The only reason that I have never hitherto employed a larger quantity of liquid oxygen or air than 200 c.cm. was the circumstance that this quantity was quite sufficient for my experiments; for my apparatus can be enlarged at will without changing anything in its construction. I have very often used large quantities of liquid oxygen and air whilst attempting to liquefy hydrogen, and to determine its critical pressure, as well as to inquire into the optical properties of liquid oxygen, as is proved by the whole series of researches, performed together with Prof. Witkowski.

CHARLES OLSZEWSKI.

University of Cracow, Austria-Hungary, December, 1894.

I HAVE read the letter of Charles Olszewski, and but for your courtesy in drawing my attention to it would have allowed it to pass without notice. Considering the Royal Society, in the year 1878, awarded the Davy medal to Cailliet and Pictet for their achievements of the liquefaction of the so-called permanent gases, it is hardly likely I could put forward in England any claim for such a re-ult. A reference to the *Proceedings of the Royal Institution* between the years 1878 and 1893 will be sufficient to remove the suggestion that the apparatus I use has been copied from the *Cracovie Bulletin* of 1890. The work of the late Prof. Wróblewski has been fully acknowledged in England, and I am not aware of any injustice done to Charles Olszewski on account of the alleged omission of his subsequent investigations from public notice.

JAMES DEWAR.

The Term "Acquired Characters."

I AM afraid that as Sir Edward Fry has endeavoured to show that the explanation, given by Mr. Galton and accepted by me, of the term "acquired characters" is an absurdity when applied to the consideration of the question as to whether those characters can be transmitted by generation, I must proceed to convict Sir Edward of a loose and unwarranted use of language whilst availing himself of the plausible form of strict logical statements. I am a little disappointed with the value of the results hitherto accruing from the intervention of high judicial authority in a scientific discussion.

Sir Edward Fry asked for a definition of the term "acquired characters." From the observations which accompanied his request, it was evident that he wished for a statement of the meaning attached to the term when it is either asserted or denied that the acquired characters of a parent may be inherited by its offspring.

Mr. Francis Galton gave (and I accepted) as a brief explanation of the term the following: "Characters are said to be acquired when they are regularly found in those individuals only who have been subjected to certain special and abnormal conditions." I took the trouble to expand this explanation of the term at considerable length. Whether Sir Edward Fry has understood what was said, or not, is uncertain. Whether he has, or has not, he proceeds to state that this definition excludes the possibility of the inheritance of acquired characters, and renders the inquiry as to whether characters acquired in one generation may be handed on to the next by inheritance impossible! And therefore, according to Sir Edward, the definition is a worthless one for the present purpose. Sir Edward's argument runs: "Characters can only be found regularly either in individuals exposed to conditions which induce them, or in individuals which have inherited them. If then a character appears *only* in those individuals exposed to certain conditions, it does not appear in individuals by inheritance." That is perfectly correct; but where Sir Edward Fry is entirely wrong, is in his illogical assumption that the words "does not appear by inheritance" are equivalent to "is not transmissible by inheritance"; in fact, that "does not" means "never will or can." Surely when Sir Edward takes pains to use such a technical term as "identical proposition," he should remember the difference between "particular" and "universal." Mr. Galton's definition enables the observer to recognise and select for inquiry an acquired character, viz. one which is found in those individuals only which have been subjected to certain special conditions—that is to say, one which is at a given time and place so found. Nothing is said or implied as to future possibilities. It is the purpose of the inquirer to ascertain whether this acquired character can appear in a later generation as a transmitted character. In the specimens examined it *has not* yet so appeared. As Sir Edward justly observes, since it appears *only* in those individuals exposed to certain conditions, it *does not* appear in individuals by inheritance. But that has nothing to do with the question as to whether it *will or can* appear in individuals by inheritance. Accordingly the conclusion reached by Sir Edward Fry, that Mr. Galton's definition of the term "acquired character" reduces the proposition that acquired characters are not transmissible to an identical one, is erroneous, and due to a confusion by Sir Edward of a statement of what is observed at a particular moment with a statement of what must be for all time.

It should be noted that Mr. Galton's words do not furnish, or profess to furnish, a definition by which any character may be assigned to its class as either acquired or inherited. It may

well be doubted whether such a definition can be framed in the present state of knowledge. What Mr. Galton's definition or phrase does accomplish, is to point out *some* characters which may certainly be classed as "acquired" and not "inherited," and from the study of which we may accordingly start in the inquiry as to whether or not the acquired characters of one generation may become inherited in a subsequent generation. "Characters," writes Mr. Galton, "are said to be acquired when, &c." This by no means asserts that there are not other characters which should be regarded as acquired, if we knew fully about their history; for instance, at the very moment when our observation is being made on a group of individuals, some might conceivably be exhibiting a character inherited from the last generation, and other specimens might be exhibiting exactly the same character acquired *de novo*. Such cases (supposing that they ever occur) would not help us at all in the attempt to determine whether acquired characters are transmissible; and the fact that they are not included in Mr. Galton's definition (though their existence is not expressly denied) renders that definition a more practical one, and more useful to the experimental naturalist than a more comprehensive definition which could not be brought to a practical issue.

Lastly, it seems to me that Mr. Galton's definition is precisely what Lamarck pointed to in his "Première loi" and the first sentence of his "Deuxième loi." The reciprocally destructive nature of the propositions contained in those two laws I pointed out, in a former letter, and have not yet had the pleasure of seeing, in reply, any defence of Lamarck's position from one of his adherents. E. RAY LANKESTER.

Oxford, January 4.

Boltzmann's Minimum Theorem.

THE remarkable differences of opinion as to what the H-theorem is, and how it can be proved, show how necessary is the discussion elicited by my letter on the oversight in Dr. Watson's proof. Each of the four authorities who have replied takes a different view.

Dr. Larmor enforces the view I put forward at the close of my letter, and says that the theorem is what I said appeared an *à priori* possibility; and I may here point out that his letter is a complete answer to the argument I used in the *Phil. Mag.* 1890, p. 95, urging that, as there were as many configurations which receded from the permanent state as approached it, there was an *à priori* improbability that a permanent state would ever be reached. This argument was criticised at some length, not really answered, in Messrs. Larmor and Bryan's Report on Thermodynamics (British Association Report, 1891), but the suggestive remarks there given helped me, I think, to arrive (independently) at the complete answer given in Dr. Larmor's recent letter. But my present use of the argument is not that which Dr. Larmor criticises; I now use it as a test of a particular proof of the H-theorem. I say that if that proof does not somewhere or other introduce some assumption about averages, probability, or irreversibility, it cannot be valid.

Mr. Burbury appears to consider that the theorem can only be proved if we assume that some element of the distribution does tend to an average (quite a different position from Dr. Larmor's), and he is as yet unable to state the appropriate assumption except for the case of hard elastic spherical particles colliding or "encountering" (for since a is constant in his last letter, it seems as if the $q_1 \dots q_{n-3}$ coordinates are really dummies). Yet Mr. Burbury has already given what purports to be a general proof of the theorem for any number of degrees of freedom.

Mr. Bryan thinks that a condition which excludes the reversed motion is implied in Dr. Watson's proof, for he says that in taking unaccented letters Ff as proportional to the number of molecules passing from one configuration to another in the reversed motion, I make a less "natural" supposition than Dr. Watson, who takes accented letters $F'f'$. I cannot see what virtue there is in putting accents on or leaving them off, and after a very careful study of Mr. Bryan's letter, I can only think that he has fallen into some confusion owing to the way in which he uses at one time *accented* and at another time *unaccented differentials*, although (as he himself remarks) there is no difference whatever between their accented and unaccented products. But even if Mr. Bryan be right, would he put us any "forrarder"? What we want is a *proof* that the collisions will make H decrease, and we can hardly be satisfied with a proof

which depends on the previous assumption that the particles do "naturally" tend to move in the desired way.

Dr. Watson meets my reversibility argument by saying that H decreases even in the reversed motion, when the system is confessedly *receding* from its permanent state. No other correspondent agrees with him in this view, which would indeed *take away all physical meaning from the H-theorem*, for the decrease of H would then be quite unconnected with the approach to a permanent state. As to the other point, Dr. Watson does not amend his proof himself, but says it is "easy" to do, and so does Mr. Bryan. Yet one has an instinctive distrust of things which are said to be "easily seen," and at all events Dr. Watson's reference to the case in which the theorem is *applied* does not help one in the *proof*, where it is necessary to express *separately* the products of the differentials expressed by the small and capital letters respectively in his "Kinetic Theory."

Mr. Burbury asks why I say the error law has been proved for the case of hard spheres without the use of Boltzmann's Minimum Theorem. I thought Tait had done so (*Trans. R.S.E.* 1886), and at all events I thought the ordinary investigation showed that there was but *one* solution, that of the error law, in that case; but perhaps I am mistaken.

Mr. Bryan says Lorenz gives the clearest account of the assumptions in Boltzmann's theorem. He would earn our gratitude if he would state them in his next letter.

EDW. P. CULVERWELL.

Trinity College, Dublin, December 29, 1894.

Aurora of November 23, 1894.

OBSERVATIONS of this aurora, by Mr. James T. Pope, at Dingwall, in the north of Scotland, have been sent to me by Mr. H. Corder, of Bridgwater, a few particulars having also been recorded here of the appearance, which, although the distance of this place from Dingwall falls but very little short of 400 miles, yet showed some very excellent agreements with Mr. Pope's description.

Beginnings of the aurora were seen by Mr. Pope between 6 and 7 p.m., as a glow which brightened gradually along the eastern, and sent up a few faint streamers from the western parts of the horizon towards the north, until 6.30, gradually fading out, after that, till nearly 7 p.m. The glow then gradually reappeared as a bright band, brighter in the east than in its western half, stretched across the sky from east to west, somewhat southward from the zenith. This band of light continued very bright for some time, but faded out gradually towards 7.30, the streamers in the north-west at the same time increasing continually in brightness.

Near Slough the display was first noticed about 7.15 p.m. as a low ill-defined white bow, stretching, at about half the altitude of those stars above the horizon, from under ζ to under ν Ursæ Majoris (altitudes 19° and 24° , azimuths 13° W. and 16° E.) from north). A little later, towards 7.30 p.m., this arc had become a bright narrow band, a degree or two in width, and about 25° long, extending from η Ursæ Majoris in the west (altitude 15° , 19° W. of north) to a few degrees under γ and β Ursæ Majoris (altitudes 16° and 19° , 2° W. and 6° E. of north) on a slightly downward slope to some degrees eastward from the latter star. It faded out partially about 7.30 p.m., leaving two bright remnants across η , and under β Ursæ Majoris, each about 8° long, while a third just similar wisp of light appeared on the same line's far leftward prolongation; this western offshoot of the band continued with the other two short segments till all had faded out at 7.45 or 7.50, marking the arc's considerable but not otherwise traceable extension westwards, across ϵ , ζ Herculis (altitudes 17° and 15° , 60° and 55° W. from north).

Dingwall is about 390 miles distant from Slough, in the direction 18° or 19° west from north; so that it appears that the strong part of the glow-band seen most brightly in the east from Dingwall, was alone observable here (if we except the light-wisp in Hercules towards the west, at last), in the vapoury sky near the horizon.

Beginning with an average altitude of between $9\frac{1}{2}^\circ$ and 12° , or of about 11° , at 7.15, the band in growing stronger reached an altitude, at Slough, of 14° or 15° towards 7.30, during about the space of time when it was most distinct, and seen most strongly in the east at Dingwall extending from east to west somewhat southward from the zenith. If its altitude there was at that time about 60° , and at Slough about 13°