

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. Wóblewski; 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 Gessen 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 result. 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. Wóblewski 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