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WHO OF THE TWO: DEWAR OR OLSZEWSKI? A POLEMIC IN ENGLAND ABOUT THE PRIORITY IN LIQUEFACTION OF PERMANENT GASES

The present work is related to a monography that is being prepared and bears a temporary title: *The attainments of Polish scientists in the field of low temperatures.*

The contribution of our research workers to this branch of science is, as we know, considerable. The 287 bibliographic items and 45 names of authors (and the list is far from complete) who published original works dealing with cryogenic research bears witness to this fact.

The present publication has in view to clarify one of the most important events in the history of cryogenics. It is based on hitherto unpublished letters received by Olszewski from English scientists, chiefly from Ramsay and Pattison Muir. The event in question is a controversy which in the nineties of the last century inflamed the minds and has been fought out around the person of Olszewski and his attainments.

It may be worth noting that this controversy has not been initiated by Olszewski himself and his part in it has been quite negligible. Swords were crossed by two camps of English scientists. This controversy and the liquefaction of argon were of great importance and have been a deciding factor in the shaping of English public opinion with regard to the Polish scientist.

Before entering in a discussion on the principal point it is desirable to recall some facts of importance connected with the studies of the history of research on low temperatures. It will help us a clear presentation of the whole story and will permit to avoid mispresen-

tation. In the first place it is necessary to stress that in the whole history of science probably no other branch of research has aroused so many stormy controversies as is the case with cryogenics. Some of the discoveries had several pretenders, some polemics were of long duration, some priorities were fiercely fought out.

At the end of the XIX century the problem of liquefaction of permanent gases was attacked from many sides. A feverish race lasting a score of years followed and aroused excitement among scientists throughout the world. It is only natural that in the heat of an exciting rivalry nerves sometimes gave way and the rush connected with such a rivalry led often to errors and misjudgement. The nature of the research favoured such misunderstandings. Let us take as an example the liquefaction of gases to the so called dynamic state. It was necessary to see only for a fraction of a second a thin haze of liquefied gas. How easy it was to commit an error, to be the victim of an illusion, the more so as the impurities could have a deciding influence on such a phenomenon and it was easy to take the impurities as the liquefied gas. Another source of misjudgement was the fact that in the beginning there was no reliable method to measure such low temperatures necessary for liquefaction of gases.

These and still other causes had the effect that controversial announcements were made and aroused lively discussions and polemics. Pictet publications were strongly critisized, Jamin attacked Wróblewski and Olszewski, both the last named quarelled frequently with each other, then came the polemics we are going to present here of Dewar vs. Olszewski and his English adherents. Another very sharp controversy arose between the same Dewar and Hampson. All these disputes caused such chaos that even to-day the effects are felt when dealing with the history of gases liquefaction and in many textbooks informations are still not always reliable.

In studying the history of low temperatures another point should be stressed. Many of these misunderstandings were due to the fact that no clear distinction existed between the so called dynamic and static states of liquefied gases. This difference has been established and defined by Berthelot. According to his definition the dynamic state of a liquid is such a state when the liquid exists only as a hazy emulsion in a surplus of gas still not liquefied. A static state he called such state when liquid is in the form of clearly visible drops or in the form of a liquid that can be poured down. From what has

been said we may draw the conclusion that when speaking of a gas liquefaction it is necessary to establish clearly have we in mind the liquefaction in a static or in a dynamic state. This is not always observed and leads often to misunderstandings.

In order to avoid such misunderstandings we are going to make clear right now our standpoint in the matter of priority concerning the liquefaction of permanent gases. We are sure that our standpoint is best founded and is in accordance with the opinion of the majority of authors. In our monography this problem is going to be discussed and motivated in detail.

In our opinion the most sound judgement is that air components were liquefied to a dynamic state by Cailletet and Pictet (1877). The work of the last named became widely known due to an extensive publicity. Later it became obvious that in this whole affair there has been some amount of the so called bluff, nevertheless the position of Pictet with regard to the dynamic liquefaction of air remained unshaken.

The priority to a static liquefaction of air components — oxygen and nitrogen and other gases of a similar critical point belongs undoubtedly to Olszewski and Wróblewski (1883). The priority of liquefaction of hydrogen to a dynamic state has three pretenders — Wróblewski, Olszewski and Pictet. Pictet has never been seriously considered. Famous was his stupefying assertion that he heard the click of metallic hydrogen. What concerns the two others the matter has been closely analysed by Kurzyniec [1] who investigated the Pictet claim also. After many calculations he came to the conclusion that Olszewski ought to be justly regarded as the first one who did liquefy hydrogen to a dynamic state (1884). The static liquefaction of hydrogen is unanimously adjudged in favour of Dewar (1898) and liquefaction of helium to Kamerlingh Onnes (1908).

Coming back to our proper theme, that is to the presentation of the polemic, let us describe the background against which the polemic has been fought out. It is 1894. 11 years have gone since Olszewski and Wróblewski in Cracow have liquefied air in static state. Wróblewski died 6 years ago. After 1883 interest in cryogenic problems did not abate, it was still a domain full of life, fashionable, attracting the interest of the scientific world. Despite the attain-

[1] E. Kurzyniec, *About the Priority of the Liquefaction of Hydrogen.* P.A.U., Prace Kom. Hist. Med. i Nauk Mat.-przyr., t. III, nr 3 (1953).

ments of Polish scientists that have been mentioned above there was still much work left. On one side one of the then known gases — hydrogen remained still unliquefied on the other the liquefying technique was still very primitive. With the greatest efforts, with the aid of very complicated installations, being exposed to accidents when working with the easily inflammable ethylene it was possible to obtain small quantities of a fluid enclosed in a hardly accessible tubule. To make any measurements was quite a problem.

So after 1883 the efforts of low temperature workers went in two directions: to liquefy hydrogen and to improve the method of liquefaction the other gases. With this in view were working: in Cracow Olszewski and to the end of his days Wróblewski. Abroad work was going on too. Cailletet and Pictet were working hard. New cryogenic centers were established in England where the foremost position was held by Dewar, and in Leyden in Holland by Kamerlingh Onnes.

Such was the state of affairs when on Dec. 2d 1894 Ramsay addressed to Natanson in Cracow a letter of the following content:

Dec. 2-nd 1894.

Dear Professor Natanson,

First of all let me congratulate You on Your appointment to the Chair of Physics at Cracow. You well deserve it, and I am delighted that You have got a sphere of action which will so well emit Your inclination and ability. I have written to Prof. Olszewski, but as I have not the pleasure of personally knowing him, may I ask You to be so good as to act in some way as an introducer, and tell him who I am, and that I am not likely to abuse his confidence. I may as well tell You the state of things, and I can do so more openly than I am to him, a perfect stranger. Professor Dewar, for several years past, has been repeating some of Olszewski's experiments, and lecturing publicly on the matter; I have, I think heard all his lectures of the Royal Institution and he has never mentioned Olszewski's name. These lectures are not published in full, but abstracts appear in the "Times" and in other papers; and very short abstracts appear in the "Proceedings" of the Royal Institution, a publication which has practically no circulation beyond the members who, as a rule are amateurs of science, and not scientific men. The results of all this is that Dewar has attributed to him the liquefaction of oxygen, of nitrogen and of air, and that these statements pass without contradiction, which they would receive were they published in any scientific journal. And the Royal Society Council, on whom were only two chemists (both of whom objected) have this year given him the Rumford Medal for his researches on the states of matter at low temperatures. I am certain that he has obtained no

new results (one which Fleming obtained, and which Dewar insisted on putting his name to alone excepted). I strongly object to see a man claims ignored, and I am sure that Professor Olszewski owes it to himself to restate his results in English. This might be done in the "Philosophical Magazine" in the form of an article or of letter. If in the form of an article, it will need no excuse: if in the form of a letter it might have for its text the award of the Rumford Medal. Had I been on the Council of the Royal Society this year, I should certainly have protested, and urged that it should have been given to Prof. Olszewski. I hope therefore that You will urge on Prof. Olszewski the necessity of following the course which I have suggested.

Lord Rayleigh and I are still working away at our gas, and we hope to publish in January. I do not think that our results will affect Stas's number, but will bring the density result in accord with it (14,04). Many thanks for Your paper on Maxwell's law. I send You a reprint on Surface Energy from the "Zeitschrift f. phys. Chemie". I am glad, to hear Your criticism of Van der Waals.

Yours very truly
W. Ramsay

The two letters of Ramsay, one quoted above to Natanson and the other to Olszewski mentioned in the foregoing correspondence have initiated a chain of events which followed one after another like an avalanche. These events were centred around two problems the one is the improper attitude taken by Dewar in relation to Olszewski's attainments and has been discussed in a broad way in the foregoing letter, the other was the "work at our gas" done by Ramsay in collaboration with Rayleigh, spoken of in the said correspondence. The problem consists in indicating precisely the density of nitrogen so as to check the atomic weight established by Stas. This research as we know resulted in the discovery of argon.

We do not know the exact date of Ramsay's letter to Olszewski and what has been its content; it has not been preserved to our days. In any case it has been received by the addressee at least a few days before Natanson got his, as already on the 4th December 1894 Ramsay in a letter thanked Olszewski for his answer.

The letter Ramsay sent to Olszewski begins as follows:

Dear Colleague,

Best thanks for your valued letter. I hope you will be willing to write a s h o r t article as it would be advisable to have it sent to daily papers and journals. If I am allowed to offer my advice I would suggest to have it written in a popular way and make it intelligible to all half scientific people. (.....)

Further in his letter Ramsay asks Olszewski to make some attempts to liquefy argon, a gas recently discovered by himself and Lord Rayleigh. He writes:

My accessories are insufficient to make such determinations at low temperatures and I have no confidence at all in Dewar's works. I even know him to determine temperatures by means of a silver ball as he takes for granted the specific heat of silver to remain constant, which of course is not true.....

On the 26th of December 1894 (as can be seen from a letter of Dec. 28th 1894) Ramsay dispatched a sample (300 cu. cm.) of argon, but still previously in a letter of 21st Dec. 1894 he was asking Olszewski to rush the work on argon. He wrote:

(.....) I have been advised Dewar has discovered that when a mixture of this substance (argon — *auth)* and nitrogen is frozen to a temperature — 210° then crystals are being isolated. He did not purify the gas as much as I know but applied merely the mixture. As I know him being not capable to define correctly the temperature, he did only some rough qualitative experiments and it would be of importance to have your observations done as quickly as possible (...)

I met the editor of "Nature" the other day. He told me he is ready to receive from you a letter (a claim if you wish). He knows the Dewar's affair pretty well. He advised me however to ask you to have the letter written in such a way that it will be possible to him to have it published. Fitzgerald, the editor of "Phil. Mag." assured me also he would like to have an article about your discoveries to publish it in February, including all necessary tables a. s. f. In case I may be still of some help to you please put me any questions to answer. It would be well not to mention my name in your letter.

Yours truly
W. Ramsay

The next letter of Dec. 28th 1894 is partly devoted to an article Olszewski was preparing for the English press, but the chief theme was the work on argon. It contains the following paragraph:

(.....) Lockyer is waiting for your letter concerning the Rumford medal, and Fitzgerald expects to be able to publish your article in the February issue of "Phil. Mag." You may send him your letter directly, 29 Bedford St, Covent Garden, London, or if you wish through me. It is important however that it appears shortly.

With best wishes for the New Year
Yours truly
W. Ramsay

Shortly after Olszewski sends his manuscript and Ramsay acknowledges its receipt on Jan. 6th 1895.

Dear Colleague,

Your manuscript arrived to-day. Yesterday[2] in the evening I read it all through very carefully and it gives me much pleasure to congratulate you and your coworker on your English. There are almost no errors and both of them are fit to be published without any alterations. To-morrow in the morning I shall forward them to Lockyer and Fitzgerald; I think however the second copy of your letter should be published in the "Times" as this paper has written in detail about all Dewar's works. I know personally the editor and wrote him a letter asking to have your answer published. In such a way it will reach the public still better than through the medium of "Nature"; I shall arrange everything in such a way of course that your letter will appear in both papers on the same day...

The above letter indicates that two kinds of manuscripts were making the trip from Cracow to London. Both appeared soon after in print. In "Nature" of Jan. 10th 1895 in the column *Letters to the Editor* Lockyer inserts a letter by Olszewski under the title: *On the Liquefaction of Gases. A Claim for Priority,* and "Philosophical Magazine" edited by Fitzgerald contains in the February number of 1895 an extensive report by Olszewski *On the liquefaction of gases.*

The content of both articles was similar, the theme in the "Philosophical Magazine" being discussed more fully. The beginning of this article is as follows:

My researches concerning the liquefaction of gases, with which I have been occupied ever since the year 1883, have been published in various scientific periodicals in Polish, French, and German Languages, viz., in the publications of the Academy of Sciences of Cracow (in Polish), in the "Bulletin International" of the same Academy (in French and German), in the "Annals of the Academy of Sciences of Vienna", and in Wiedemann's "Annalen der Physik und Chemie" and in his "Beiblätter", as well as in the "Comptes Rendus". Though I suppose that my labours are sufficiently known to the scientific world, yet there are motives which lead me to ask the Editors of the "Philosophical Magazine" to insert following summary of the more important results of my experiments.

[2] An example of Ramsay's absence of mind — a contradiction in comparisan with the preceding sentence.

Firstly, because my researches appeared irregularly in different scientific papers, as they proceeded; such as wished to become acquainted with them being obliged to look them up in all the papers I have mentioned. Secondly, because of the experiments and public lectures of Prof. James Dewar, concerning the liquefaction of large quantities of oxygen and air. In several cases Prof. Dewar merely repeated my experiments: for instance, as regards the absorption spectrum and the colour of liquefied oxygen. In these cases he confirmed the observations I have made, and mentioned the results of my work in the manner usually received in the scientific world. But in his last experiments and lectures respecting the liquefaction of considerable quantities of oxygen and air and their employment as cooling agents, Prof. Dewar has thought fit not to make any mention of my labours in the same field, which had been published several years before Prof. Dewar went over them again. In the number for June 1890 of the "Bulletin International de l'Académie de Cracovie , I have described an apparatus serwing to liquefy a greater quantity of oxygen or air in a steel cylinder, from which it can be poured out into an open glass vessel, and used as a frigorific agent. It is entitled *K. Olszewski. Transvasement de l'oxygène liquide;* and a brief report on the subject is contained in the "Beiblätter of Wiedemann", vol. XV. p. 29, under the title *K. Olszewski. Ueber das Giessen des flüssigen Sauerstoffs.* That my labours should have thus been passed over in silence is all the more astonishing, because as soon as the above-mentioned "Bulletin" was printed I sent a proof of it to Prof. Dewar; I also forwarded him proofs of my other researches, knowing that they interest him.

The apparatus I constructed and described works very well and can be used without danger, so that in October of the same year (1890) I was enabled to obtain 100 cub. centim. of liquid oxygen in the presence of an audience consisting of over 100 students. In the following year, during the Congress of Polish naturalists and physicians in Cracow (July 1891) I obtained 200 cub. centim. of liquid oxygen in the presence of a good many physicists, and showed its peculiar properties; as, e. g., its bluish colour and its absorption spectrum. Subsequently, without having altered my apparatus in any way, I got about 200 cub. centim, of liquid air and used it as a frigorific agent in order to liquefy hydrogen. The construction of my apparatus is very simple, and it can easily be enlarged by using a steel cylinder of the capacity of 300, 400, 500, or more cubic centimetres. The only reason that I have never hitherto employed a steel cylinder of greater capacity than 200 cub. centim, is the circumstance that the quantity of oxygen or air which can be liquefied in this cylinder was quite sufficient for my experiments.

Then follows an enumeration of his attainments beginning with the earliest, ending with the most recent.

Concluding the author writes:

From this summary of researches, as well as of dates, it follows that the first apparatus serving to produce large quantities of the liquefied so-called permanent gases, with the solitary exception of hydrogen, was constructed by me. This apparatus can be enlarged at will by increasing its parts, but

without changing anything in its construction, so that it might be used to obtain liquefied gases in factories should they at any time prove of practical utility. By means of this apparatus I obtained as large quantities of liquid gases as I wanted; and they were used for the first time on a large scale as cooling agents (for instance, in my attempts to liquefy hydrogen), or as an object of scientific researches (the absorption spectrum of liquefied oxygen, its coefficient of refraction, &c.).

The experiments of Prof. Dewar are merely the repetition and confirmation of these researches, most of which were published several years before his corresponding investigations. His work is really original only as to the magnetic properties of liquid oxygen: that which is not borrowed from my researches is a development of ideas struck out by another — as, for instance, the experiments on electrical resistance at low temperatures, which were begun by Clausius, continued by Cailletet and Bouty, and brought ten years ago by my former fellow-worker, the late Prof. Wróblewski, to the temperature of the freezing-point of nitrogen, then several degrees below the temperature attained in the experiments of Messrs. Dewar and Fleming, who, it is true, extended their examination to various metals, alloys, and non-metals. But the execution of these labours meets with no difficulty: for the method of getting large quantities of liquefied gases is now generally known.

Dewar replies immediately. His reply has been inserted in "Nature" of the 10th Jan. 1895 next to Olszewski's letter.

This reply kept in a rather improper tone reads:

I have read the letter of Charles Olszewski, and but for your (editor-*auth.*) 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 Cailletet 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. 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.

Putting aside for a while the description of the polemic with Dewar let us take up some matter having with it a certain connection.

At the beginning we are going to quote two letters to show how much interest has been aroused in England first by the announcement the article by Olszewski is to appear, and then by its publication.

Oliver J. Lodge of the University College in Liverpool writes: (the letter has no date; according to Estreicher it has been written in December 1894 or maybe in January 1895).

University College, Liverpool
Monsieur le Professeur Olszewski
Cracow
Dear Sir

Permit me to express the pleasure with which I hear that You are going perhaps to describe your research in the "Philosophical Magazine" or at any rate in English.

I had heard of them through a Russian gentleman but we in England ought to know them better. I am sure your statement will be welcomed by and interesting to all Physicists.

Yours faithfull
Oliver J. Lodge

The second letter from William Gaunon (Owens College, Manchester) of the 28th Jan. 1895 reads:

The Owens College,
Manchester, 28 January 1895
Sir,

I had intended shortly to read a paper before "The Physical Colloquium" of this College on *Recent Work at Low Temperatures*. I am much interested in your letter to "Nature" of Jan. 10th. I would be much obliged if you could let me have any papers You are publishing. I have those published in "Comptes Rendus" 1883. I can also get at any published in "Wiedemanns Annalen" or "Beiblätter".

Excusing myself for the trouble I give You
I am Yours truly

William Gaunon
Lecturer in Physics

There is finally a letter sent by Ramsay to Olszewski on Feb. 1st. 1895. It is concerned with the second object of interest to both scientists — argon. A thorough research on this gas shared by Lord Rayleigh, Ramsay, Crookes and Olszewski done at a speedy pace has been concluded and Ramsay was able to publish a report at a meeting of the Royal Society on the 31st January 1895. The letter of Feb. 1st is namely an account of the said meeting. An excerpt of this letter reads:

Dear Colleague,

Yesterday I delivered at the "Royal Society a lecture on argon, describing the work of Lord Rayleigh and myself, as well as your valuable determination of the constants at low temperatures. The attendance was so great that the meeting had to take place not in the usual hall of the R. S. but in the bigger hall of the London University, where at least 1000 people were present. Your name was met with great applause; in any case the scientific public has doubt no more of your priority.

Going back to the polemic it must be stated that Dewar's announcement of Jan. 10th was not left without answer. The first to react was Pattison Muir (Gonville and Caius College-Cambridge) who from that time on joins the polemic and takes a lively part in the discussion. On 22d Febr. he wrote to Cracow:

Professor Olszewski
Dear Sir,

I send You by the post copies of "Nature" for Feb. 14th and 12th, which contain letters from Prof. Dewar and myself, regarding the liquefaction of the gases.

I do not know what Prof. Dewar will do now. Probably he will reply in next week's "Nature". I hope some one else will take up the matter. It is important that the discussion be continued until Prof. Dewar is forced to withdraw all claims to originality.

I want some fellow of the Royal Society to refer to the Rumford medal; the giving of that medal has stamped Dewars work as very important, whereas it is merely crude qualitative lecture illustrations.

I am Your sincerely
M. M. Pattison Muir

And here are some excerpts from the polemic between Pattison Muir and Dewar that has taken place on the columns of „Nature" and which has been already spoken of.

The first to speak was Pattison Muir in the number of the 14th February 1895. He states in the beginning that Olszewski has a just claim for priority in various branches of cryogenics especially where large quantities gas are in question. Basing his arguments on what Dewar published at various times in the "Proceedings of the Royal Society" Pattison Muir asserts the works of Dewar are devoid of any signs of originality and frequently are no more than a repetition of Olszewski work. He states quite clearly:

A reference to the "Proceedings" of the Royal Institution is then sufficient, not to remove, but to strenghten, "the suggestion that the apparatus I (Prof. Dewar) use, has been copied from the "Cracovie Bulletin" of 1890, or at least that it has been borrowed from descriptions of apparatus devised by Prof. Olszewski.

In the same number of „Nature" Dewar replies. Such is his answer that, though the column *Letters to the Editor* is provided with a notice: "The Editor does not hold himself responsible for opinions expressed by his correspondents", he felt obliged to add at the end of Dewar's letter : "A few personal remarks in Prof. Dewar's letter have been omitted, as they do not affect the points at issue".

Dewar does not reply directly to charges made against him. Instead he endeavours to oppose Olszewski to other cryogenists suggesting that Olszewski speaking of his priority in certain branches of science tried to abase the merits of others.

He wrote:

. . . . . .

The object of the communications on the liquefaction of gases, which have recently appeared in "Nature" and the "Philosophical Magazine", is to depreciate the work of Cailletet and Pictet, to smother away the first-class work of the deceased Wróblewski; to annihilate myself, and thereby to magnify the claims for originality of Prof. Olszewski.

Using quotations from the "Proceedings of the Royal Institution" he tries to reduce the value of charges made against him by Pattison Muir. It is impossible to enumerate all arguments advanced by Dewar. In most cases they have nothing to do with the charges and are merely a description of some of his own works, done by him in the preceeding years.

But Pattison Muir has no intention to resign; he segregated the charges and in "Nature" of 21st Febr. 1895 continues to prove the claim of Olszewski to be justified. He wrote among others:

Prof. Dewar hides the essential questions in a mist of words. If he has made marked improvements in the methods of liquefying and manipulating the more permanent gases (besides his invention of vacuum receivers); if he has conducted original, accurate, and thorough investigations into the properties of the liquefied gases, where are the accounts of this work to be found? Every student of the subject knows he can lay his finger on the work of Olszewski, and also on that of his deceased colleague Wróblewski; and he knows that work to be thorough, accurate, and important.

The relation of Olszewski to the polemic between Dewar and Pattison Muir in which he himself took part in a certain degree may be seen in the next letter sent to Olszewski by Pattison Muir on the 25th Febr. 1895. It reads:

Dear Sir,

I received Your letter today, for which I thank You much. I am glad You approve of what I have done in "Nature". It was impossible for me to allow Prof. Dewar to go on hold-by stealing another man work. By this time I hope You have received two copies of "Nature" I sent You ten days ago; stupidly I put Poland on the address, instead of Austria-Hungary; I hope the parcel, and also a letter I wrote, will reach You.

You will see from my second letter in "Nature" that I have replied to Dewar much in the way You suggest in Your letter to me; I have not however said anything about the use of liquid N_2O by Pictet. Of course no one supposes that You have shown any wish to depreciate the work of Cailletet, Pictet and Wróblewski; I thought it best to pass over that part of Dewars letter in silence.

Everyone I see is speaking against Dewar; but it is difficult to get anyone to take the matter up in a decisive wey. I hope some one in authority will ask publicly; "why did Prof. Dewar receive the Rumford medal?" I consider the giving of that medal to him to be a disgrace of the Royal Society which undoubtedly represents English scientific men.

I am Your sincerely
M. M. Pattison Muir

And what is happening to the discussion? Pattison Muir had the last word, it is now the turn of Dewar who in his answer printed in "Nature" of the 28th Febr. 1895 replies in an as usual, rather unpleasant tone.

As soon as Dewar's answer appeared in print Pattison Muir reacts, and on the 1st of March 1895 sends a letter to Olszewski and it is evident he is loosing his patience.

Dear Professor Olszewski

I send You a copy of this weeks "Nature". I must send a short answer to this letter of Prof. Dewar; but it is almost impossible to fight with a man so utterly unscrupulous, and one who refuses to meet any direct challenge but always goes off on side issues. I am afraid Dewar will succeed in giving many people an impression that he has done some scientific work. You notice in this letter he claims to have liquefied hydrogen, and obtained solid argon from the air.

I am Your sincerely
M. M. Pattison Muir

* * *

In March 1895 a new conflict joins the preceeding ones: the liquefaction of hydrogen. An introduction to this dispute is a letter by Ramsay and a notice in "Nature" on 21st March 1895.

The letter of March 24th 1895 beside an important information announcing the discovery of helium in cleveite contains the following paragraph:

(.....) Immediately upon receiving your letter I translated the first two sentences and sent them to "Nature"; this week it appeared on the first line of the "News". So we took care of your priority. If Dewar writes roughly shall I make a citation of your second sentence? It reads: "give him no opportunity to break the eighth[3] commandment" or something of this kind. I feel called to do it but I pray not be let into temptation.

A letter by Ramsay of May 12th 1898 indicates that the letter mentioned above has been written by Olszewski on the 17th March 1895.

A note in "Nature" mentioned by Ramsay in the letter dated March 24th 1895 appeared on the 21st March 1895:

Prof. Ramsay has been good enough to forward to us the following translation of a passage in a letter he has recently received from Prof. Olszewski: "I have at last succeeded in determining the critical temperature and the boiling-point of hydrogen. I have found for the former — 233° and for the latter — 243°. I have used the dynamical method, which I described in the "Philosophical Magazine". A thermal couple proved of no use, and I was obliged to avail myself of a platinum-wire thermometer, measuring the temperatures by the alteration in resistance of the wire. I have obtained satisfactory results, and intend to publish an account of them in English".

In spite of apprehension felt by Ramsay, armistice reigned for the time being on the battlefront. Dewar did not react to Olszewski notice. He was delivering three lectures dealing with the liquefaction of gases. Olszewski got informed by his "adherents" — Ramsay and Pattison Muir; they were not accquainted however with the content of these lectures.

An earlier letter of Ramsay (dated April 27th 1895) began as follows:

Dear Colleague,

Your interesting letter concerning the liquefaction of hydrogen is still, if I am right, unanswered. My excuse must be that never in my life I was so busy as was the case lately. I have done immediately what you have

[3] Another example of Ramsay's absentmindedness — it should be "seventh"

charged me to do and have sent to "Nature" a translation of these sentences which announce the liquefaction. It arrived somewhat late but obtained nevertheless the first place among the small notices. Had Dewar written something about it I was prepared to translate the next sentence or maybe it were even better to let it be printed in German. Fortunately there was no opportunity to do it. D. had made known he is going to deliver in the next future three lectures on the liquefaction of gases. Unfortunately I shall not be able to be present. I shall however send you notices that will appear in the daily papers if they contain anything of interest.

The latter part of Ramsay's letter is devoted to the research on argon and helium and talks over the possibility of a collaboration of the two scientists in this field of action.

The letter of Pattison Muir is dated May 16th 1895:

Dear Professor Olszewski,

I am much obliged for Your letter of May 3-d. Dewar has delivered his three lectures at the Royal Institution; but as only brief newspaper reports have appeared, it is impossible to judge accurately how Dewar treated the question of Your work and that of Wróblewski.

"The Journal of the Royal Institution" containing the report of Dewar's lectures will not apeear, I suppose, til next year.

I do not think it would be advisable for me to publish anything regarding Dewar's lectures, at least not at present, when there is only a newspaper report to go on.

I am Your sincerely
M. M. Pattison Muir

The next information of the revival of the polemic Dewar-Ramsay appeared in the beginning of 1896. The "Chemical News" of January 24th 1896 contained a report of a meeting at the Chemical Society which took place on the 19th December 1895. Dewar delivered a lecture *The Liquefaction of Air and Research at Low Temperatures.* The publication contains a résumé of the discussion which followed. In a few places of his report Dewar makes some polemic excursions in Olszewski direction. He goes back to an article published by our scientist in the "Philosophical Magazine" of February 1895.

The "Phil. Mag." of February, 1895, contains a fantastic claim put forward by Professor Olszewski of Cracow, that because he used in 1890 a steel tube combined with a stopcock to draw off liquid oxygen, he had taught the world, to use his own language, 'the method of getting large quantities of liquid gases".

Dewar in his polemic takes advantage of some misunderstandings that existed between Olszewski and Wróblewski using them as an argument against Olszewski. In one place he refers to a "pamphlet" by Wróblewski entitled *Comment l'air a été liquéfié*. At some other place in a chapter about research on hydrogen he writes:

In Professor Olszewski's paper "On the Liquefaction of Gas" (Phil. Mag., 1895), after detailing the results of his hydrogen experiments, he says: "The reason for which it has not been hitherto possible to liquefy hydrogen in a static state, is that there exists no gas having a density between those of hydrogen and of nitrogen, and which might be for instance 7—10 (H = 1). Such a gas could be liquefied by means of liquid oxygen or air as cooling agent, and be afterwards used as frigorific menstruum in the liquefaction of hydrogen". Science will probably have to wait a very long time before this suggestion of how to get "static" liquid hydrogen is realised. The proposal Wróblewski made in 1884 of using the expansion of hydrogen as a cooling agent to effect the change of state is far more direct and practicable.

The last sentence has a connection with the rapid development of the countercurrent method in 1895. This method was a great step forward in the technique of gases liquefaction. In its elaboration Dewar was much advanced and hence his feeling of superiority over Olszewski. (In this domain also a sharp dispute is going to take place between Dewar and his new opponent Hampson. Dewar admits the basic idea to have originated with Wróblewski but he awards himself the credit of having done the greatest work in elaborating the countercurrent method. Not going into the depth of this discussion it may be stated that at present Dewar arguments are not considered valid and the method whose priority he fought for is generally admitted to be the work of his opponent Hampson).

The discussion on the report delivered by Dewar is in so far of interest as it is concerned almost entirely with the polemic Dewar vs. Olszewski and his "advocates".

At the same meeting Ramsay broke some sensational news. A report of it says:

Professor Ramsay remarked that Professor Olszewski had succeeded in liquefying hydrogen, and from unpublished information received from Cracow, he was able to state that a fair amount of liquid had been obtained, not as a froth, but in a state of quiet ebullition, by surrounding a tube containing compressed hydrogen at the temperature of oxygen boiling at a very low pressure. On allowing the hydrogen in the middle jacket suddenly

to expand, the hydrogen in the inner most tube liquefied and was seen to have a meniscus. Its critical point and its boiling-point, under atmospheric pressure, were determined by means of a resistance thermometer.

According to an account of the meeting Dewar answer was:

Professor Dewar, in reply, stated that he could have no knowledge of unpublished work on the liquefaction of hydrogen. The mere fact of liquefaction was first definitely given by Wróblewski, although Cailletet had made an earlier experiment of the same kind. His paper contained a quotation from Prof. Olszewski's communication made to the "Philosophical Magazine" in Feb., 1895, in which Olszewski distinctly says that he had not succeeded in getting liquid hydrogen in the "static" condition. Further, in a later paper, published in the same journal, for August, 1895, no mention is made of getting a "fair amount of liquid in a state of quiet ebullition" or of seeing a "meniscus".

It seems Ramsay went a little too far. Nowhere in later Olszewski's publications does he mention of having liquefied hydrogen to a static state. Ramsay's letters point to his absentmindedness. Being emotionally engaged in the polemics with Dewar he might have mistaken his wishful thinking for reality. A letter of Ramsay to Olszewski, the last in our possession, makes this supposition very probable. This letter bearing the date May 12th 1895 has been written after something of great importance has happened. Hydrogen has been really liquefied in static state by Dewar. The letter is as follows:

Dear Colleague,

In one of your letters addressed to me on March 17th 1895 after letting me know you have determined the boiling and the critical point of hydrogen you added the following words: "Perhaps you will be so kind and make already now an appropriate use of this here letter in order to establish in England my priority in this respect and offer Dewar no opportunity to sin against the seventh commandment". To-day Dewar made an announcement at the Royal Society and gave a description of having liquefied about 40 cu cm. of hydrogen. He says it to be a transparent liquid with a well formed meniscus. He did not determine yet the boiling point but states that a pipe inserted in it is readily filled with solid air. Your name was not mentioned in this connection.

After Crookes, Frankland and others have spoken the president, Lord Lister, called upon me to say a few words. I congratulated D. on the large scale of his experiments and told it was indeed satisfactory to hear him confirm your results and it would be of real interest to see him arriving at the same boiling and melting points as you.

Devar answered with great fervour it has never been proved you had in your hands liquid hydrogen and told I shall publicly show your letter wherein you gave a description of liquid hydrogen.

As far as I know I never noticed you had ever seen liquid hydrogen in a restless boiling state; I would like to know however did you ever had it in a visible state. I have to make rebuttal and having fought for your priority I shall consider it a friendly gesture on your part to hear some details in this matter. Was your substance in an iron pipe so you could ascertain only the liquefaction temperature from the state of thermometer, or did you have a real proof to have hydrogen in liquid state in your apparatus.

May I ask you for a speedy answer as I would like to have a lecture on this subject this coming Thursday at the Society.

Yours truly
W. Ramsay

This letter ends the correspondence between Ramsay and Olszewski, that is at our disposal. We do not know were there any more letters or maybe they have not been preserved to our days. It is certain however that the contact between Olszewski and Ramsay continues. In the same year 1898 in December an assistant of Olszewski T. Estreicher went to study in Ramsay laboratory and remained there the whole year 1899. Notices left by Olszewski show that to his last days (1912) he used to send copies of his works to Ramsay.

The polemic with Dewar vanishes. It was probably due to the fact that Olszewski did not confirm Ramsay assertion of hydrogen being liquefied by our scientist in the static state. The priority of Dewar in liquefaction of hydrogen leaves no doubt.

Such is the whole correspondence being so far in our possession dealing with the polemic in England on account of Olszewski. In concluding this article we want to present some ideas which come to mind while perusing the relevant materials.

In the first place the origin of the polemic is somewhat mysterious. Why was it that Ramsay and Pattison Muir stood so decidedly at Olszewski side? Has it been a matter of ambition or were there some deeply rooted causes?

Dewar's guilt leaves no doubt. He really did great work on the field of low temperatures but many of his claims were unfounded Hence his polemic with Olszewski in which he often used a very vigorous language. A similar character, sometimes even more violent, had his polemic with Hampson, when, what is now almost certain, he was wrong in denying priority to his opponent. He committed even blunders when for instance he announced to the world

in "Chemical News" of May 13th 1898 (page 216) his success in liquefying helium, which was obviously untrue.

The question now arises were Olszewski and his adherents always right? Most questionable is probably the matter of hydrogen static liquefaction. According to Kurzyniec Olszewski was undoubtedly the first to liquefy hydrogen in a dynamic state, he was very close to have it liquefied in static state, and got many valuable informations on critical data of the said gas. He did not succeed however to have it liquefied to a static state and nowhere in his publication did he claim to have it done. Ramsay letters suggest something else. There are two possibilities: either it was an oversight by Ramsay or it was due to a hasty conclusion by Olszewski. Taking into account the character of Olszewski as a scientist we are inclined to assume the first hypothesis to be right.

Aside from the ultimate judgement one thing is certain, The discussion before the English public did Olszewski no harm. English public came to known him well, he gained in popularity and Ramsay was certainly right in saying Olszewski gained recognition. As a result in the history of research on low temperatures the right place has been assigned by English authors to the attainments of Polish cryogenists.