18 juli 1895

HKO to Dewar 18 July 1895

From Mr. David Nutt I learn that you wish to receive 6 copies of the Communication from the Laboratory of Physics of the University of Leiden no.14, and I haste to send them to you. I also sent a complete series to the Royal Institution. The failing numbers will soon be added. I feel happy to do a little service to you. From my paper itself my admiration for your beautiful work will be evident enough.

I have many times shown the exquisite blue liquid, that we got after so much labour, to scientific friends, corning even from Russia. When I poured into one of your vacuum vessels, where the ebullition stopped and the mercury mirror appeared, this sight never failed to charm whoever loved science. I brought liquid oxygen in one of your vacuum glasses to my aged professor Rijke, living at some distance from the laboratory who was delighted to see some experiments with it performed in his reading room, and I then returned in my laboratory and there was left enough oxygen to pour it again in my permanent bath.

I am sure that your vacuum glass is the greatest advance in low temperature work since 1883 (this sentence underlined by Dewar).

I have been myself occupied in using the vacuum for lessening convection. In my experiments on the combination of the Lissajous figures with Foucault's pendulum (New demonstrations of the rotation of the earth) my pendulums oscillated in an evacuated case, to lessen the effects of viscosity. So as the problem of convection presented itself, I attacked it in the same way, as will appear from the description of some apparatus that will be given later and that ought to be compared with your high vacuum glass to show all the perfection of the latter.

The trace of some of my endeavours you will find yet in rudimentary form in rny permanent liquid oxygen bath, when the oxygen when boiling in vacuo is protected by its own vapour of loss pressure. From all this it will be clear how fully I admire the beauty of your splendid vacuum glass.

The construction of my boiling glass and case will remain especially appropriated for experiments in which the oxygen is to be enclosed between plain parallel glasses, as I hope to show shortly.

But where the plain parallel glasses are not absolutely necessary I hope to give a combination of the use of your vacuum glass. For instance f o r working with very pure oxygen i.e. permanent baths of constant temperature, when it is of importance that the necessary quantity of oxygen be as little as possible.

By the method of enclosing the ‚jet, so that one can look at it, when manipulating the cock the waste oxygen ejected from the reduced to a minimum and immediately recollected. [When the liquid oxygen is not preliminarily cooled in a liquid oxygen bath, the greater part of it evaporates immediately in cooling the rest to the temperature of the bath; this part I will call waste oxygen] Now if for the bath itself can be used one of your vacuum glasses, where evaporation by convection is less than in my glasses the oxygen to be collected on this accord is less and so the total quantity to be compressed for maintaining the bath is again reduced.

I am very sorry that my work can only advance at so slow a pace. I have in the first place to pay my attention to my Scholars working in different departments and as appears from Communi.no14 it is under no favorable circumstances that i struggle with the difficulties in my own department. If my work might prove of some interest to the foreign leaders of scientific research ---and your friendly request for copies gives me some hope in this respect--- my position as a researcher will be ameliorated of course.

I hope that afterward I will have the occasion to come to London. It will be such a great pleasure to me to make your personal acquaintance and to see you at work.

20 Juli 1895

Dewar to HKO 20 July 1895

Fleming told me about you valuable and suggestive paper and not having received a copy I told the Librarian to order some. No doubt you may have addressed the paper to Cambridge, but as I am not resident during the summer and will await my arrival in October.

It is only three persons who know the worries and troubles of low temperature research who can appreciate properly all you ave done. Such work requires a long apprenticeship of a very trying and disheartening kind. The average scientific man thinks it is all plain sailing and when Prof.Olszewski tells the world with perfect assurance that he has done everything, then for a time The world will believe it. I observed this in a footnote to page 14 you state my apparatus of 1886 was "on a small scale and was not used for collecting liquid oxygen. I presume what you intended to say was not used in the particular experiment described for collecting liquid oxygen. Surely you can' t imagine that I did not know and never attempted therefore to collect liquid oxygen from the 1886 apparatus apart and before adding the complicated arrangement for discharge into vacuum as described in the "lecture " .Then you say it was small. Well this is true, but it was as big as Olszewki.s of 1890. His cylinder as described in the Bulletin of the Cracow Academy June 1890 had a capacity of 30 to 100 cl whereas mine liquefied 22 cl of liquid oxygen at a time. The capacity of my copper coil of 1886 was between 30 and 100 cl then you may properly ask why was my apparatus of 1886, was rapidly enlarged between 1886-90 [1887-90 in the draft in Dewa r 's pocession]. The answer is very simple. In discharging the liquid oxygen into the exhausted tube immersed in [liquid in draft] ethylene (as shown in the Lecture) by some strange chance the two liquids and gases got mixed, the mixture caught fire and there was a terrible explosion. I was nearly killed and as the experiment was being performed before a number of people, several got hurt. This was sufficient to stop such work till the year 1889. Then larger apparatus was made, which worked well and the next " approach" of the Faraday Centenary lecture to ?? 1890-91 to constructing the large and complicated apparatus described and shown in the lecture on the chemical work of Faraday. On reading this lecture and looking at the arrangement ?? and chemical ?? would know that it would be impossible for me to elaborate such ,machinery and work it between June 1890 and June 1891. The fact is I never learnt anything in the way of manipulation of liquid gases from Prof. Olszewski. My residence at the Royal Institution is between the months of February and July, since I have also my Cambridge Professorial duties to discharge. If I had nothing else to do but low temperature work I like you might get on faster. In the meantime I send you a copy of a recent paper on the thermoelectric properties of the metals at low temperatures along with some old lectures giving here which you might like to peruse.

I trust we may meet some time here, but in the present condition of low temperature discussion I have felt is reasonable and proper to decline allowing even Pictet to inspect my Laboratory. The object is to prevent any further recriminations(?) seeing the details of my apparatus has not been published {described in draft}. Of all earthly deformities (deformations) scientific meanness is the most contemptible and the recent criticism on my work both in this country and america(?) has not tended to ?! or to add lustre to the dignity of science.

6 Maart 1896

HKO to Dewar 6 March 1896

I have to thank you most cordially for the Proced. Dec. last and have the pleasure to send you to day no. 23 of the communications from my laboratory. I have not been able to repeat your splendid experiments for since your last letter it was impossible for me to work at low temperatures and that for a reason you will. be astonished to hear. The municipality of Leiden has made objections as to my working with condensed gases and has not been content with asking that additional means of precaution are taken, but is gone so far to claim in August last that my cryogenic laboratory be removed from the city! Not withstanding that never any notable accident happened in all the years I have been working there, and that from my communication no.14 it is evident that avoiding danger is one of the principles of my apparatus. I would have been able to get much more results in low
temperature work, to be sure, if I had not taken always all profitable precautions, and I and my helpers might be not injured all the less. But it has been my principle to make no experiment before I had convinced myself that it would do in no manner harm to any one. Where a local authority is so extravagant, it is, of course, not possible to continue the work. But I must wait till authorization for this work according to the strict formalities
required by law is given by the Government(?). Now in the course of what is necessary to be done since August last for obtaining this authorisation there has been named last week a commission from the Royal Academy of Sciences of Amsterdam (at the request of the Government) to give evidence on the alleged dangers of my laboratory for the neighbourhood. The first question that commission will ask of me of course shall be: how is it with Prof. Dewar working in the very center of London.

Now you wish me this summer that if I came to London you would decline allowing me to inspect your laboratory. Though I did not intend at that time and though I will not have the opportunity before long to come to London. I can say that this communication was a great disappointed to me. But I do not think you go so far in secrecy that you will not assist a fellow-worker, who is assailed in such a manner as I just communicated to you . So I beg to have to ask you the following questions.

1.Has there been ever made any objection to your laboratory from the point of view of public security.
2. How far is the distance from the places where your compressed gases are stored under pressure from the rooms where people assembIe.
3. How many horsepowers has your engine.
4. How much ethylene gas have you in your apparatus under pressure, and how much in store in cylinders.
5. How much other cylinders with compressed gas have you in store or in use (carbonic acid included) .
6. Have you ethylene compressed in spirals or reservoirs immersed in nitrous oxide.
7. What are the greatest pressures in your storage cylinders and in your pumps and the greatest product of volume and pressure in your pumps and in your storage cylinders.
8. Are their (sic) any measures of safety prescribed to you for the use of your apparatus or for the case of a fire in the building.
9. Are there some special measures of precaution devised by you.

For better understanding communication no.23 when reference is made to no.14 I sent to you the photograph D.7. of the drawing of my boiling glass and boiling case. The disposition of the vessel might prove useful for collecting hydrogen also, as indicated in no.23, especially when ?? by the cupshaped vacuum ?? mentioned there. In regarding the photograph with a microscope you will see all details very clearly. I have as yet no photograph of the boiling glass with greater spiral that is mentioned. But I will send it afterwards. I join the following photographs

A.l. schematical view of the whole installation A.II.
Details of this installation.
C.6. Ethylene boiling flask described comm.?
E.10. View of the cooling apparatus (it has been taken when they were some ?? ?? so that for the particular I refer to the drawings.

Accept dear Sir my most cordial congratulations with your splendid researches in the proceed. of Dec. last and believe me yours...

8 Maart1896

Dewar to HKO March 8, 1896

It would be a great disaster to science in your country (and universal science) if the municipality of Leiden succeeded in carrying out any restrictions on your splendid cryogenic laboratory and the fine work you are doing. I cannot understand such a position. Surely the scientific man is certain to do all in his power to avoid accidents and therefore the municipality can have full confidence in him. I have answered your questions in a hurry as I have to leave for London for a few days and have just time to catch the post. If I can do anything to help you it wil1 be a pleasure to me to at(sic). In the meantime I may say that I have made all my experiments with high pressure apparatus before the Prince of Wales and the Sister of your Queen Dowager the Duchess of Albany without the slightest hesitation and no suggestions of danger were even suggested. With kind regards.

Answers

1.No.
2.Immediately below and in the actual lecture room.
3.Gas engine 100 horsepower.
4. The cylinders of $C2H_4$ 60 and 70 Ibs.
5.Large quantities 20 or 30 bottles.
6.No spiral at all used in $N_2O$.
7.Greatest pressure 150 to 300 Atm.
8.No.
9.No, ordinary care of scientific man.

12 Mei 1896

to Dewar 12 May 1896

I thank you very much for your kind letter of March 8. As you offered to act when you could do anything to help me in defending my laboratory I now beg leave to ask you the permission of printing your letter with the report , that will be presented the Ministry of internal affairs here. The commission of the Royal Academy of Sciences appointed for this case, coming to the same conclusion as you, considered your letter as one of great importance for the question and wishes therefore to print it as an appendix. I suppose I have well read your abridged answers to questions (4) and (6). I rad them (4) Cylinders of $C_2 H_4$ 60 and 70 lbs. .(6)No spiral at all used in $N_2O$.
It was a great pleasure for me to send you 6 copies of my no.23 and I will be very glad to send directly the communications to the addresses you may point out.

10 Februari 1897

HKO to Dewar 10 February 1897

Comm. no33 will have reached you. It contains a research of Dr.Zeeman who has been appointed lecturer of experimental physics at the University of Amsterdam and therefore takes leave from me as a pupil.
The reason why I wish to draw particularly your attention to his work is this: Dr. Zeeman has been working in a line that has occupied your illustrious predecessor during the last time
(sic) of his life. I hope the results obtained by Zeeman my claim your full sympathy and that you will have the occasion to repeat the experiment at some time in the laboratory of Faraday.

This would be a great honour for Dr. Zeeman. Different expressions and ideas having more or less analogy with those of Zeeman, but unknown to him, when their communications were printed will be treated in an appreciation.
I have not been able to continue researches with liquid or compressed gases, otherwise than a very limited scale, the use of extremely low temperatures being excluded by the opposition against the cryogenic laboratory, lasting now for almost two years. I can tell you to my great satisfaction that the Supreme Court has just given the verdict that when sufficient precautions are taken I may be allowed to go on. So I hope to recomance work after some months.

12 Februari 1897

Dewar to HKO 12 February 1897

I will take every(?) opportunity of reading the most interesting and remarkable paper of your pupil and I congratulate you on producing such fine work. You must come over and give us a lecture some day on your work. We are always glad to have distinguished continental scholars. You would require to deliver the Lecture in English or French and show experiments connected with your work.
Like you, my low temp. work has been delayed from one reason or another. Dr. Fleming and I are publishing our old observations: they have been accumulating for some considerable time. After my Cambridge course of lectures end in April I hope to start some new work. I am very glad to learn you have been successfully(sic) before the Supreme Court.
With kindest regards.

11 Juni 1898

Dewar to HKO 11 June 1898

I am very much much obliged for the(?) reminded about the Jubilee of van der Waals and will have much pleasure in congratulating him. In England Corporate Bodies do not as a rule take action in such matters so they are left to the individual.
It is very kind of you to speak so kindly about hydrogen. You know something about the difficulties of the problem.

My troubles I can see are only beginning. It will be a long time before hydrogen is on tap. No doubt it will come same day. In the meantime progress must be very slow. I enclose an account of the Boiling point and the Density determinations so far as I have been able to do this. Later on I shall get more accurate values.

The ?? of the ?? is crypton. What scientific exaggeration and ?? we have in our time. Liveing and I saw the same spectrum lines in liquid oxygen and air in 1894 (Phil. Mag.) and thought the green line was the auroral(?)line. With kindest regards.

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NO LETTERS OR TELEGRAMS OF CONGRGULATIONS FOR THE LIQUEFACTION OF HYDROGEN ARE FOUND IN DEWAR'S OR HKO'S PAPERS

7 Mei 1903

Dewar to HKO May 7, 1903

It will give me the greatest possible pleasure to take the residence in London of ?? pleasant and interesting. In fact if at any time any friend of yours can in any way be aided by me you say so.
For some time I have been in a very feeble condition of health and have not been able to do my work(?) in the way of research. I have a quantity of helium ready and intend soon to circulate and to do it starting with liquid hydrogen. It is however a very complicated and risky business as you well know. I have already lost L cubic foot of helium by the ?? of vacuum vessels during the course of its circulation at liquid air temperatures and I dread any repetition of the disaster.
I feel I ought to congratulate you on the splendid work that you and your pupils are carrying(?) out from the Leiden Laboratory. I only wish that I had again the gift of growth so that I might begin my scientific career after a training in your Dutch school of science. I know no country that has done so much for science (considering its size) during the last quarter of a century. In my BA address I made a feeble attempt to show what the(?) great work of van der Waals had done for Physical enquiry, I and I am very disgusted that he has not received(?) scientific honours from England. I am determined it must come soon.
With kindest regards.

29 December 1903

HKO to Dewar 29 December 1903

I your last letter you had the kindness to tell me that you would be of any aid to any friend of mine. I beg leave to introduce now to you Mr,Crommelin. ?? ?? pupi1s, a very nice young man who comes to London to profit from the aspect of scientific life going on there, and who worked to see the man of whose triumph I have told him so much and the laboratory where these splendid researches are made. He is one of my assistants and so has understanding(?) for what you will show him. Besides this he has a keen(?) idea of scientific fairness which to give along with enthusiasm, is one of the aims of my teaching.
I have read with the greatest pleasure your brilliant address to the BA [he means the 1902 meeting] I have seen that it escaped your notice ---as it seems to be the case generally--- that the systematic introduction of regeneration in liquefying gas before Linde has been one of the characteristics of my work.

If not it must not have been possible for me to reach the result with my small means.

You have told me of your disaster with helium. It has been a fearful one. Could be that you would not have had it by the use of my compressor for pure and costly gases described in Comm.no.54.? I wished it could be of any use in your splendid attack on helium, the boldest attack that can be dreamt of in low temperatures worked (?) even by yourself.

5 Januari 1904 (?)

Dewar to HKO Jan [or May or June?] 5, 1904

I was very pleased to receive Dr.Crommelin with your very interesting letter this morning and to show him around(?) the laboratory. He will tell you all the new things of any importance I had to show. The fact is for some time I have not been in good health and little or no work has been done. A few months ago Curie was here and I made for him(?) her(?) a number of determinations of the heat evolved by $RdBr_2$ at 20 abs. It is
?? the heat evolved is not diminished, but I believe firmly is increased if anything. In any case it is strange enough to find that it is not less than ?? 273 abs. I got Curie to allow me to transform the $RdBr_2$ to a small quartz tube and to ?? it at a bed(?) but under high exhaustion. Thus it was lef t in splendid condition for spectroscopic examination?? ?? ??any gaseous ??. like helium. As Curie naturally took the tube away with him I requested him to hand it ?? to Duslinderes(?) and I shall be interested to see what makes of it.

I did not intend in my BA address to take the details of modern history re our own special field of work. In papers I published in 1896 I pointed out that you had used the "regenerative principle" long before Linde, and I wou1d be sorry if you thought that I had any desire to omit the fact in an address in 1902.

I had hoped by this time to settle the helium question but(?) intermitant(?) bad health has been a great(?) obstacle. In my work I have never been able to do anything unless substantially with my own hands and presumed(?) jurisdiction of what is going on. In pioneering work assistants are a waste. I am ready to start the helium again, but am interrupted with a hydrogen apparatus that is being tested for the St. Louis Exhibition. If my health breaks down. Then I would hand the helium ??? to you. This is the best thing I could do for Science. With kindest regards.

30 Mei 1904

HKO to Henry Young (assistant secretary of the Royal Institution) 30 May 1904.

I had the honour to receive your letter informing me that the General Meeting of the Members of the Royal Institution of Great Britain on Monday May 9th I have been elected an honorary member of the Institution..
From the beginning of my career I have followed the brilliant work of the professors of the Royal Institution with the keenest interest, and admired an institution which can boast such a rich harvest of original research and pregnant discovery. The great honour conferred on me by this institution is therefore valued by me at the highest degree and will stimulate my efforts for the advancement of the experimental science, to which 1 have devoted my life.
It will be a great honour for me to receive a diploma of my honorary membership, signed by His Grace the Duke of Northcumberland.

6 Juni 1904
HKO to Henry Young June 6, 1904.
Informs him that he had received the diploma of honorary membership of the RI.

17 Januari 1905

HKO to Dewar 17 January 1905 ,

I regret that I have not congratulated you on occasion of your well won baronship, having ?? your nomination. Now I beg leave to address you my most cordial congratulations with the Lavoisier medal. The French Academy is honouring your work in this manner had given expression to the admiration with which the whole scientific world regards your work. I hope to send you in a short time an address delivered by me last February in which 1 have tried to point out the great intrinsic scientific value of your work. The translation into english has been delayed by different reasons but now it is nearly in print. In correcting the proof sheets and looking up quotations I hit on a curious discrepancy in two separate copies of your Belfast address. In one of them I find p.31 the critical point of hydrogen given as from 30 to 32 degrees. In the other about 29. You would oblige me

by telling me what is the number accepted by you.

18 Januari 1905

Dewar to HKO 18 January 1905

You are labouring under a mistake. I am not a Baronet or member of any ?? of knighthood. Only the plainest and commonest kt such as was held by Newton and Brewster. The fact is I declined so called honours of this kind years ago from Queen Victoria and but for(?)external pressure (much against my own feelings) would have continued the same attitude in the present reign. What I said in the last paragraph of my British
Association address as the real decoration of a scientific man his(?) successful scientific endeavour is after all the ?? all and end all here. I thank you all the same for kind congratulations.

With regard the Tc of hydrogen you will find all I can say in the enclosed. Formerly I said it might range from 29° to 33° . The exact determination is more in your special province than mine. Exact physical measurements and pioneering work do not go well together. Such refined matters I must leave you to settle.

I am lecturing here on the 20th on new Low Temperature Experiments, the chief form(?) being the application of charcoal. I must put all the notes together and send you a copy. ~

8 Juni 1905

HKO to Dewar 8 June 1905

I did not wish to trouble you about providing myself helium from the Bath wells, but a letter from the Board of the hot(?) springs refers me to you for the same.
I thought you had occasionally helium containing gas from them but now I learn from the letter that you have put up a plant of machinery there for extracting the gases regularly. Of course, if I had been aware of this, knowing your kindness, I would have addressed myself immediately to you in the hope that you will let me share in the costs as well as in the product.
The question is that I have advanced so far that I can take seriously to show the determination of the isotherms of helium at low temperatures as well as the magnetic dispersion of the plane of polarization in my large apparatus for compressed gas. I want many liters of pure helium and to get this I will be obliged to distill it from yet larger quantities of impure helium. It seemed most appropriated[sic] to me to prepare them out of a great
number of cubic meters of the helium containing gas in which you first found this precious element.
I realize my will that getting the pure helium in sufficient quantities will take some two years of preparatory work and that I will have to sustain many losses before all is arranged in an unobjectionable way. But the more it is necessary that I make a beginning with it. So I have to seek for a copiuous supply of helium containing gases .
I am sure you will sympathize with my attacking the problem of the isotherms. The determination of the isotherms is the rational way to get the data for calculating the critical points, of which you have already made estimations and exact determinations of isotherms is just in my line of accurate measuring work.
I will be very happy if you will be able to assist me in getting the material for preparing sufficient quantities of pure helium.

12 Juli 1905

Dewar to HKO July 12, 1905

It is needless to say that I sympathize with everything you say about your projected work on helium. I have in my own way been engaged on this subject for years and after many misfortunes and no little expenditure I have been unable to

accomplish my specific object. We both want the same material in quantity from the same place at the same time and the supply is not sufficient to meet our great demands. It is a mistake to suppose the Bath supp1y is so great. I have not been able so far to accumulate sufficient for my liquefaction experiments.* If I could make some progress with my own work the time might come when I could give a helping hand which would give me great pleasure. For the present things are in a sad way with me. For the last 4 months I have

been seriously ill and quite unable to do any work, so that all research has been ?? ?? ??** As soon as I get medical leave I can stay away for a rest. It is clear therefore that I can make
no definite promises at the present time further than that I hope on returning to make some real progress and in this event your request will be kept in mind.

These passages are only in the draft of the letter found among Dewar's papers.

* No doubt other ?? ?? helium as Bath if they were properly examined . I collect all the gas I can get at Bath and bring it to London to ?? ??

**It is therefore impossible for me to promise when I shall be in a position to say that I can entertain such proposals as you put forward in your letter regarding ?? of the Bath supply. If on the other hand my condition of health shall render it impossible for me to attempt further prosecution of the Helium work then I shall have to consider what ought to be done with ?? there and you shall not be forgotten.

20 Juli 1905

Dewar to HKO July 20, 1905

If you will look at my Bakerian Lecture Proc. Royal Society vol.68, 1901, in the opening paragraph you will find the following sentence. Until the experiments are repeated with a helium thermometer filled with helium previously purified by cooling to the lowest temperature that can be reached by the use of solid hydrogen, the gas being under compression ?? accurate values can be reached. This makes it clear that I did not overlook the means of ?? your suggest ? I made ?? ?? allusion to the method in 1901.
I must confess not to understand my having done anything in the discussion of your title in my last letter; at least in any conscious way.

22 Juli 1905

HKO to Dewar July 22, 1905

I thank you very much for your ?? of your Bakerian Lecture p.301 (or 361) and I am glad that I am enabled to refer to our (?) previous ?? ?? ?? ?? for purifying helium, in the English pamplet of my Dutch communication. You will have remarked in my writings that I ?? as a pleasure to state what has been ?? in the field before me. To Prof. van der Waals I often send the dutch proofsheets of my articles to be sure that I have nothing forgotten in quoting from his numerous writings. I do not well understand how your (?) ?? has escaped my notice, but it has, and so I came in the curious position to tell you something you have published many years before. I am sure you will excuse me!

As to my remark about your dropping my title, there again comes(?) to coincidence how difficult it is well to understand foreign uses and manners. Here in Holland when I am ?? of a ?? maan(?) Then I drop his title in addressing him and he considers it as a mark of friendship and kindness. In your letters you address me "Dear Onnes" and I see now that you do it unconsciously, but I interpreted it in my manner, being happy that you added this mark of kindness to the other one you gave me. Now you had written in your last letter again addressing me with my title I feared that in any way I had not responded to your kindness. It was therefore that I asked you to go on with dropping my title in addressing me, this being quite in harmony with my feelings towards you with my admiration of your work, and with the difference of our age. I hope I have elucidated also this point to your perfect satisfaction. With kindest regards.

24 Juli 1905

Dewar to HKO July 24. 1905

We have both independently originated the method of helium purification by means of compression at the lowest attainable temperature so that the idea is yours as well as mine. I have no grievance on this subject or any other against you so that you have no need to make even the ??? of being excused by me.
If you agree to drop al1 the titles like Sir or Doctor or Professor I shall be very pleased to negniesee. I certainly never intended to make any gratification ?? in warm friedship towards yourself.

7 April 1908

HKO to Dewar 7 April 1908

I have yet to thank you for your cordial gratulations and in the mean time I have to tell you that the object of these proved to be not the solidification of helium but curious solutions, phenomena of solid hydrogen in gaseous helium. I made the expansion experiment according to my determination of the isotherms of helium at different temperatures ea also at -252° and -259° from which I could calculate the critical temperature of helium and found nearly $5^{o}$ K. Thence followed the possibility to come below the critical temperature and to cause a cloud to appear in the gas by expansion at -259° of helium compressed at 100atm. The new in my method are the large quantity of gas, a stopcock to let flow the gas in a gasholder a {quiballon?} or the vacuum, and an inner beaker in the thickwalled tube. There was coming a dense cloud from which deposited solid masses, floating
in the gaseous helium and cottonwool like and partly also more consistent(?) masses in a syropy liquid, adhering to the walls and tumbling downward in the meantime that they evaporated rapidly (20 sec). There was no melting to be seen. As far as I could gather(?) from the experiments there I took it for probable(?) that the solid substance was helium, and as the helium had been mixed with CuO and passed over charcoal at the temperature of liquid hydrogen, I wished to have a gas where there could be only my small admixtures. If helium passed immediately to the solid state, then the position of the vapour line to the adiabatic could be non favourable for condensation then, if it passed at the liquid state, and the voluminous aspect of the solid substance was in harmony with it. So I have been --- also by same observations that gave afterwards reason to (???) or proved false ---under the convinction that I have seen solid helium rapidly giving off vapour of the pressure shown by the gas (once more than 15 atms. was observed, the critical pressures having been calculated by me at 7 atms. I thought the liquid state jumped over).
The continuation of my experiments has shown that they are to be interpreted in a quite different way.
The gas that was used proved to be by a reason not sufficiently found out not as much as was to be expected from the method of (????). By analysing what was eliminated from it by renewed purifying with charcoal at the temperatures of liquid till the charcoal took no more hydrogen from the gas (so that the gas could only contain traces of hydrogen) it could be ascertained that the gas before contained once at most 0.45 and at another time at most 0.37 volume percent of hydrogen. But this small admixture has had a great influence.
In repeating the experiments with gas subjected to the renewed ???nent, there was, though the difference of the new gas with the former was so small, in one experiment nothing to see. The velocity of expansion had been smaller in this experiment, but it seemed difficult to ascribe to the difference in velocity of expansion that the tube remained perfectly clear. At a second repetition with the same gas and greater velocity of expansion a thin cloud appeared and vanished rapidly (l sec.). The mist had another appearance as before. The application of what was observed at first therefore has to be thought in solution phenomena of solid hydrogen in gaseous helium. What made the impression of giving off vapour has been the solution of deposited solid hydrogen in the gaseous helium which returned rapidly from the lower temperatures to that of melting hydrogen, the pressure increasing at the same time. Helium at the temperatures which come here is to account according to the theory of mixtures can absorb hydrogen up to a proportion fixed for every temperature, so that it remains in it at all pressures. With acceptable suppositions one can deduce that this proportion may be very great above the melting point of hydrogen and that at this melting point itself it is certainly more that a percent.
From mixtures of small percentage the hydrogen will only be deposited at lower temperatures eg. by expansion. That the hydrogen did not remain as solid hydrogen after the expansion ???? off of the helium is also to be explained because there was only a small percentage in the gas, the small quantity that remained could evaporate in the space that was at its disposition. It remains remarkable that such a small quantity of admixtures could give the whole aspect of a condensing and reevaporating solid substance, though the rapid evaporation, even more constituent (?) parts being as blown away, is in harmony with this small quantity of substance. There could have been much more than 1 milligr or 15 cub. millimeters (in round numbers) of solid hydrogen in the tube and probably there was less. And yet a great part of the tube of 7 cub. centimetres nearly was filled with a ???? substance.
As far as the experiments on the expansion of helium are advanced now they learn the curious form that take the solution phenomena of a solid as a gas in the case of helium and hydrogen. They further ??? to the possibility to realize in mixtures of helium and hydrogen the rising or falling of the solid substance according to the pressure exerted on this

gas ---the barotropic (?) phenomenon of a solid in a gas --- first as I showed 1906 the sinking of gaseous helium in liquid hydrogen. But the question of condensing helium remains an open one.
Let me add a few words as to the mist observed in a repetition of the experiments of expansion with the 'coalpure" gas. It is seen that it contains only very small quantities of hydrogen. The spectroscopic term also gives traces. The cloud can of course be attributed to the traces of hydrogen that are left in the gas and of which the just amount is not known yet.
But it is also possible that it has been a liquid cloud. If this might prove to be the case that the critical point would be nearly as I calculated it from the isotherms and helium would obey tolerably well the laws of Van der Waals. The tube broke and so I could not more certainty about the nature of the cloud. The preceding experiments have well taught how careful (sic) one has to be in reaching conclusions from the appearing or not appearing of a cloud by expansion. A decision about the critical point of helium is therefore only to be obtained by a prolonged systematic investigation which will take much time.
I am very sorry to hear that you are suffering of bad health and that it has hampered you in your scientific work, that has led to the brilliant achievements of which you know that I am one of the most sincere admirers. Please accept my best wishes.

15 April 1908

Dewar to HKO April 15, 1908

I received your ?? interesting letter about the helium experiments before leaving London for the seaside where I am ordered by the Medical Faculty.
Having said so much about helium in my Presidential address to BA in 1902 and in subsequent lectures at the Royal Institution I felt it a duty to inform the world through the “Times” that you
had succeeded where I had failed. Considering the enormous difficulty of such experiment, we can all be mislead and your rapid discovery of the trouble puts the matters all right. I have sent a letter to the Times explaining the general cause of presence of solid matter in your experiments, and that put the matter all right with the public. I wish all scientific men were as magnanimous as yourself in making immediate correction of faulty inferences from experimental data they had reason to believe at that time was correct. Your splendid work speaks for itself to all scientific minds.
The original idea of the attempt on helium was based on in taking about $8^o$ Abs. and I said it was as low as $2^o$ Abs. Then it would be hopeless to expect success in the refrigerating machine this succeeds as well with hydrogen. If your value of $5^o$ Abs of $T_c$ is correct, then we are a long way from resulting static helium liquid or solid. In any case the Royal Institution has no money to ??? such very expensive ??? seeing it has no endownments to draw upon.
My health is improving but at my age one must anticipate a gradual or sudden sunset(?).
With all best wishes.

20 Juli 1908

HKO to Dewar 20 July 1908

You know by different publications of mine how I admire you , splendid low temperature work and so it is not more than natural that I desire to give as soon as I find the opportunity some further details of my helium experiments to you. I begin by sending you a photograph of the apparatus. Please excuse me if I beg leave to ask you to consider it as communicated only for your personal information. To the last Dutch publication of the
Proceedings of the Amsterdam Academy is only added a note that just before expecting them the news arrived that helium was liquefied at somewhere more than 4°. To the English translation of the Proceedings that is to be published these days I am allowed to add a more extended note on the experiments. Of course this should remain the first publication in existence(?). On the photograph I have put different indications that will show you the way the helium circulation built “correspondingly” (in applylng the ideas of my comm.no23 at least) to my hydrogen circulation of comm no. 94, based on your liquefaction of hydrogen, and circulating 200 liters of helium purified by absorption in your way in coal through hydrogen, evaporating in vacuo during some pressures(?) up to 100 atmospheres by my mercury and auxiliary compounds of pure and costly gas of comm. no.54. All the time the helium apparatus remained perfectly clear! Nobody but you and myself what this means. As you know my means and only those of a teaching laboratory. But working in the same directions I have in the course of years got on last different your appliances for this research. All --- the human force especially--- was now strained to the utmost and the result only just got at the end. It was a good thing to have trusted to the

utmost the theory of Van der Waals and to my isotherms, which would only be obtained after many years of preliminary work, but have proved efficient. A second time when we have ??????? to find the way out of false tracks, it will go easier, but working with liquid helium will remain a difficult thing. Expense and cost of plant is small, but labour is excessive.

21 Juli 1908

HKO July 1908
Notes on the work leading to the liquefaction of helium
Group A leads to I. the course of refrigerators giving liquid air.
II. the cycle for continuous liquefying of hydrogen.
Group 8 leads to the isotherms of helium.

Group A I. The object was to arrange permanent baths for accurate measurements by the cycle method.. From different reasons high purity of the gas in the cycle giving the bath of liquefied gas was necessary. There was special attention paid to make only a minimum of additional gas circulate in the cycle, and to ensure that the gas would not become contaminated in prolonging the work. The liquefied gas of one cycle being also available for cooling the compressed gas in a cycle with a less cercible gas than was obtained in a cascade. Regeneration makes that there is obtained a very great economical effect even at the lowest temperature.

From the regeneration cascade there was in work in 1892 the chloromethyl and ethylene cycle, the oxygen cycle only so far that succeeded in pouring off some liquid oxygen at ordinary pressure. In 1894 this cycle was in good working order and the contours of the permanent liquid oxygen bath brought to more than 1/4 of a liter. This without Dewar's glasses. The third step cascade was ameliorated by the by, also there was taken a growing ?? of Dewar's glasses and accordingly by a fourth (open) cycle for liquefying and evaporating air was added. According to the great economical effect mentioned above this cycle gives 9 liters of liquid air per hour, so the 75 liters used in the Helium experiment could be obtained without difficulty. {Last sentence underlined by Dewar}

II. As soon as the oxygen cycle was completed the hydrogen cycle was taken to hand. The work was done (as published in 1896) according to the same theorem that has now been laid at the foundations of the method of liquefying helium. Then extreme purity was still more required work should be arrived at, and continuous work was of the utmost importance for the liquefaction of helium. The cycle worked well in 1906. It gives 4 liters of liquid hydrogen per hour.. The apparatus has been provided since 1906 with an arrangement to easily prepare a store of exceedingly pure hydrogen gas. This arrangement is not yet described but it is very simple in principle as it depends on freezing out the impurities by evaporating pure hydrogen in circulation. This make it possible to take off 20 liters of liquid hydrogen as well used in the helium experiment, though the apparatus is relatively small. {Last two sentences underlined by Dewar}.

Group B. All the importance of having at disposal such an efficient cooling at -259° appeared from the determination of the isothermals as they put the Boyle point at -259°. Long before the helium was known there was worked out isotherm determinations at very low temperatures. Object was then hydrogen. There were
arranged cryostats, manometers, piezometers expressly for this and an elaborate series of investigations of low temperature determinations was ??? taken. For only by very accurate work in comparing (???) the deviations of the law of Boyle Charles for this nearly permanent gas can be derived its critical temperature.

10 Augustus 1908

Mrs. HKO to Dewar 8 August 1908.

Mrs. HKO to Dewar 10 August 1908

Dewar to Mrs.HKO Aug 10, 1908 Dear Madam Kamerlingh Onnes

I was indeed very sorry and upset at the sad news that your husband was ill. After all one had not (?) wonder all the anxiety and worry he must have undergone. Let us hope that a good rest will see him as bright and strong as ever. Now that I am old and dicrepid(?), it will be too uphelding to think the progress of low temperature work showed he arrested

(?) by ?? a temporary illness of your husband. He has got youth on his side and this i everything.
After next week I intend going on to Scotland, but it has just occurred to me that Prof.Onnes might like his paper to be given to the British Association Meeting at Dublin early in Sept In this case I shall be prepared to do what I can to meet his wishes. Naturally I would simply have to read and explain the ?? of his apparatus used and the results. The reason why I mention the Dublin meeting is because this is the only great gathering of scientific people we shall have for some months and is appropriate that the most recent low temperature work should be brought before it seeing that it was at the Belfast meeting that I discussed the helium problem in my address. Pray convey to your husband my kindest regards and tell him to be in good cheer.

10 Augustus 1908

HKO to Dewar 10 August 1908

Many thanks for your kind telegram to my wife. I ??? defense of writing by translating last #, that will interest you very much for you.
#6 Properties of helium. With important points(?) of difference, the properties of helitum show striking points(?) of resemblance with the picture Dewar gave of them in 1902 in his presidential address. Mention is al ready made of the extremely small capillarity. The boiling point was found 4.3° on the helium thermometer of l atm at 20° K. The temperature must be corrected with the aid of the equation of state of helium to the absolute scale. If a increases at low temperatures this correction may amount to some tenths of a degree so that the boiling point is perhaps best rounded off to 4.5° K.
The triple point pressure is certainly below 1 cm, perhaps also below 7 mill. Temperature at this pressure according to corresponding states can be estimated nearly 30K .The liquid is very mobile at this temperarure. If helium might behave like pentane then we could go down to near 1°,5 K (-??? this is better than 1° K) before it becoming syropy and then solid. But how great the realm of low temp. {and high vacua) is, that has now been opened must yet be investigated.
Liquid helium has a very small density 0,154. It shows a much greater value of b, than could be derived{?).
Additional points on the isotherms of 252.7(?) and 252.8(?) have been calculated from them ---viz. nearly 0,0007.
Indeed the value of b derived from the liquid state is nearly two times the value of b, that was supposed and had been accepted in the calculation of P.Keesom and myself on mixtures of helium and hydrogen. From the great value of b follows a small value for the critical pressure, in the neighbourhood of 2 or 3 atm. probably, exceedingly small in comparison with that for other substances. If helium is subjected to high pressures, "reduced" pressures are attained much higher than can be realized with any other substance. What is reached in pressure (?) with 5000 atm on helium, surpasses what is attained in this respect by 100000 atm. on carbon dioxide.
The proportion of vapour density to liquid density at boiling point is 1 to 11 nearly. This points to a critical temperature not far above 5 K and a critical pressure not far above 2.3 atms. But all these quantities have to made the subject of new measurements and calculations before they can be accepted as definite and conclusions can be drawn. There may be given a provisional value of a viz. 0,00005. When van der Waals in 1870
in his dissertation considered the question if hydrogen had an a it will not have been but after long hesitation that he came to the conclusion that if it was even small, it must exist. "Matter will, it must be accepted, always attraction (sic)" was his argument, and fate(?) will that he repeated these words just some days before the liquefaction of helium in connection with helium.
The a now found gives the smallest value of this attraction of matter that amnifests itself just(?) so clearly so that liquid is formed out of helium.

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From &4 will appear that came only to observation as there was already some quantity. It held the helium thermally (?) at a constant temperature before it was seen. There was seen(?) curving of the surface by the flowing off, and accumulating of liquid. The surface was seen by reflection of light from below.
One time seen it was not lost from sight. regarding trough the three ?? you saw, the three surfaces 1iquid air, liquid hydrogen, 1iquid helium the difference was most striking. The edge(?) of the helium stood sharp as a knife against the walls. In fact it was a critical miniscus in a Cagniard ??? .

1 September 1908?

Dewar to HKO undated,but most probably here

Your most interesting and fascinating letter of the 10th has been sent on here where I am paying a visit to Dr.Mond for a few days before leaving London next week. I sent on your paper to the editor of the Times requesting them some general abstract (?) of the work on helium should be made in continual (?) of the early telegrams and I am glad to think that we took the ?? and (however imperfect as a general paper must be) and produced a notice on the 12th August. If I go on from Dublin to the North if you can send me a translation of your paper I could give a detailed account to the British Association unless you have delegated this duty to someone else.
I am glad to think that you must be rapidly improving in health and trust soon all will be well.
Pray give my kindest regard to Madam Onnes.

P.S. The next Proc. of the Royal Society will contain a short paper of mine dealing with rate of production of He from Rd. I found Sir William Ramsay has made some extraordinary mistake in getting a rate of production 10 times greater than I get. My results agree only too remarkably with the calculation of Rutherford.

3 September 1908

HKO to Dewar 3 September 1908

It is as if fate had to counterbalance by some misfortune my prosperity in the research work of this year. First my illness and now are an outcome of it. I perhaps lose what have given such a great honour and pleasure to me, i.e. your reading my paper at Dublin conjointly with remembering your Belfast address. In any way, I do not know how, I have been in the meaning that the B.A meeting was somewhat 10 Sept. and being unwell (sic) and the
laboratory having its holidays it has not come to my notice then beginning of the Meeting was at an earlier date. My mistake has not been caused by a retardation in my reconvalecence. I have been recovering very well and though bound to my room and terrace have been allowed to take up some work again. it is a pure blunder
only to be explained by unfavourable circumstances that I have not earlier asked some of my friends the true date of the BA meeting.
This morning I had just written a telegram with F.S. to ask you where to send the translation as my wife read from a Dutch newspaper, which took it from Nature, which I do not receive directly, that I will read a paper at the B.A. and that the meeting begins today!! I will get now precise information as to the day for which the paper had been announced, but I fear that the translation will not reach you in time. I am quite in despair the more(?) as otherwise I had arranged things well.
To make it easier for you I had put the translation in print. All was ready Saturday but by some retardation again by the holidays, I did not receive the corrected proof Monday nor Tuesday. So I wrote yesterday that it was impossible for me to wait longer. Now it has arrived and I send it herewith, making apologies for the form. It was to have the address where to send it that I wrote this morning the telegram mentioned before. As
soon as learning the perplexing news from my wife I immediately telegraphed to Dublin thinking you will be there, and understand that there has been some thing gone wrong with the exposition of the translation. I had further ordered lantern slides but these will be ready only Saturday. It is quite a misfortune!!

#for the English translation of the Amsterdam meeting reports, the Proceed. of the Royal Academy at Amsterdam that are going to press.

3 September 1908

HKO to Dewar 3 September 1908 II

I have not yet succeeded in getting the program of the meeting of Nature. But I see that you will give also a paper. I suppose that it will treat your work on the Joule Kelvin effect you had the kindness to mention to me with photograph. I am very interested in the quantitative result of your measurement at $20^{o}$ K. As soon as I have the figures I will calculate in how far they are confirming my isotherms. The determinations of the Joule Kelvin effect here at hand has not advanced so far.
It will be a great satisfaction to you to refer to your foreshadowing by your beautiful ??? of helium some of the properties of the exceptional liquid. Allow me some remarks as to your suppositions, because it will interest you what where(sic) my doubts left.

1. Your supposition of density 2 times that of hydrogen has been a most happy one. But according to what was known from helium it had ---as has been calculated by Dr. Keesom and myself from viscosity as well as from index of refraction ---to be put at least 4 times that of hydrogen as is quoted in my paper [b = 0,0005].
2. you make the difference of boiling point and critical point proportional to the critical temperature. According to the theory of corresponding states this is true if you refer to corresponding pressures. But the critical pressure of helium being very small the normal boiling point is relatively much nearer to the critical temperature by He then by Hydr.
In my paper I have even considered the possibility that the critical pressure was smaller than one atmosphere, i.e. that helium had no boiling point. I had arranged my expansion experiments in Febr. March and April also for vacuum.
It seems not to be so evident at once what I say here. e.g. Olszewski who just writes me to tell me that I have wrongly quoted him as giving 2° as crit. temperature, because he gave the boiling point seems not to realize that the preliminary question is if there is a boiling point (normal b.p. of course). Indeed if the critical point had been 2° and b the same as it is now we would have the curious case advanced in my paper and ??? elucidated now. It is a remark that you will perhaps be so kind to add in reading the paper, without remembering of course that it is an objection against your supposition made by me.
3. In your Belfast as I understand, it will the supposition of crit. temp. 8 $^{o}$ K was simply taken because 8° is somewhat lower than the temperature at which you were sure(?) that helium did not liquefy.
Your important measurements on the absorption of helium by coal have then pointed to the boiling point 6° or the crit temp 8° .
Allow me a remark with respect to this beautiful but not decisive argument. It is not to be overlooked that the cohesion of He what it might be was at all events small and much smaller than other cohesions which come more in comparing with the force coal directly in the respective gases. We have here an analogy with molecular pressure in mixtures according to the theory of v.d.Waals where we have to consider
$a = a_{11}x + 2a_{12}X(I-x) + a_{22}(I-x)2$ and where $a_{12}$ sometimes can be put $\{a_{11}a_{22}$ nearly, x being the molecular concentration of the mixture.

The less important the cohesion constants of the gas becomes in comparison with the adhesion constant of the gas for the coal the more conclusions about the boiling point (it is a of the gas) are uncertain.
Helium could be a permanent gas and yet absorbed greatly at law temperatures by coal. This whole question of attraction of helium by coal is a very interesting one .
I hope to give calculations on it later.

4 September 1908

HKO to Dewar 4 September 1908

Just as I had yesterday got the programme showing that there will be time for our papers Monday, I received also your kind telegram announcing that indeed all will go right on Monday.
I calculate that you will have received my letter Friday evening or Saturday early in the morning. as to lantern slides it is not possible to have them ready before Saturday afternoon. so that they can be in Dublin Monday early in the morning. I do not know what is the meaning of working in the sections but at all events I will send them.
It will cause you perhaps some difficulty that in my paper all my former papers are supposed known. of course you have taken notice of some of them. But it can be perhaps a help to your memory when I sent you
1. Some notes about the work leading to the helium experiment.
2. Some of the communications in which I have for this end indicated by black pencil what comes in a ,"? now and with red what deserves especially attention in relation to the notes.
In ?? most of the pages could have been torn off. It seemed preferable to me to leave the communic. as it is and to use only black and red pencil for your references.
I have written somewhat in a hurry yesterday. I have not much to add, but allow me to emphasize that I wished that you did personally know the objections which were the cause that in some points in my paper I express myself with some caution. Though I did not find it necessary to put the reasons of this in my paper, the paper being not occupied with these questions in particular. The reader who is on the height of the questions will be advised by my catious expressions that there is left room for objections to the possibi1ity of which he wi11 ??? himself by deeper study and the general reader does not want them.

6 September 1908

Dewar to HKO Sept 6, 1908

Sunday, PS

Hope your health is improving.

BA Dublin
Dear Professor Onnes,
I was very glad to receive you r paper yesterday and wil1 have the honour of reading it to the chemical rneeting tomorrow. Before I got your telegram I had put slides in hand and naturally if yours arrive in time I will use them.
'I not (maybe wrote) your remarks *re* the anticipated properties and possibility of liquefaction of helium and by myself. It is quite true that in 1902 (BA address) I had some doubts on this subject (for reasons expressed) but I had no doubt by the year 1904-5. By this time I had made the ???? experiment of the following nature.

(1) absorption of He at corresponding (?) pressure and temperature corresponding (?) to 12$^{o}$ Abs.
(2) the hypothetical density of the He in charcoal at corresponding temps as compared with H .
Thus I got

| | Vol. H absorbed | Vol. He absorbed | Density |
|---|---|---|---|
| 63$^{o}$ | 180 | 5 | 0.08 |
| 15$^{o}$ | -- | 195 | 0.17 |

This confirmed my anticipation about the density of the liquid while (1) made it certain (?) that as compared with other gases the BP is about 5$^{o}$ abs.
(3) The determination of the thermal evolution (?) in the absorption of He by charcoal at the boiling point of H as compared with that of O or N or Argon under corresponding states and naturally (?) sufficient concentration. Thus I found

| | | | |
|---|---|---|---|
| Mol. Latent (?) Heat(?) | He | 483 | 18$^{o}$ |
| | O | 3146 | 82$^{o}$ |
| | N | 3059 | -- |

Thus combining (2) and (3) and keeping to Van der Waals Laws Tc/Pc from fluid density is about 2.2 and as the thermal evolution (?) in charcoal is at corresponding temp. is about 2 times the ?? of evaporation of the Liquid Gas ??? ??? in Van der Waals Log p = A –B/T is ??
This gives ?? BP of 5$^{o}$ Abs and ?? B = 11 log $P_{He}$ = 5.08- 11/T . This for 3atmospheres would give T 6°.3 and this would be of the order of the Tc (?). That was the kind of reasons I put forward in my latest lectures so that upon (1) alone. Naturally if the BP = 4°.5 then the first term A of the equation would be higher(?).
(4) As to the value of the Real Joule Kelvin any experiment I do consider reliable. My object was to find if any cooling took place at the boiling point of hydrogen with the same He then placed in Jet and Regenerating coil. The lag (?) of the mass of the He thus in (?) the coil is a nuisance. With my 30 atm. of He expanding at 20$^{o}$ abs the rate of fall per minute about 0°.1. That shows at once that whole (?) the integrations could be done from 20° abs. Thus ?? is the proper agent to use.
As I will be leaving here any letter sent to London will reach me.

25 September 1908

HKO to Dewar 25 September 1908

My health has now been well improving. I was some days at the seaside. I will be allowed to go to the Paris congress, for which I have been appointed as H.Ms first delegate. Of course I cannot take part in festival or anything of that kind, but the work in the sections will be possible. I had some idea to see you then. But from the summaries I learn that Prof. Ramsay is the English rapporteur and I derive from it that you will not come to Paris. Can I do anything for you there? I would be of course only a small reciprocity of the kindness you had for me in Dublin, but if I could be of any service to you , it would be a great honour and pleasure for me. Please accept my repeated hearty thoughts for your great kindness at Dublin, especially for the sympathetic way in which you have presented my work. They were under your audience who were so much impressed by your speech, that they wrote to me to congratulate me on the occasion. A curious instance was the wife of professor Kossel(?) at Heidelberg, whom I did not see for years.
As to your observation (?) of the properties of helium from absorption by charcoal your last letter has not taken away my difficulties, though I acknowledge the strength of your new (?) arguments. E.g. the compared quantities are not

absorbed under corresponding pressures. But as! told you I will give you later on my calculations.
I will still have some months to take ??? care of my health and only the more necessary work will be allowed. But one of my first wishes is to invite you to come to see the helium.

29 September 1908

Dewar to HKO September 29, 1908

Dear Professor Onnes,
I am very delighted to learn that you are steadily improving in health and that the progress has been so satisfactory that you are able to undertake a journey to Paris. The Dublin meeting of the British Association will be memorable for two things. (1) The liquefaction of Helium by Prof. Dr. Onnes (2) The Bursting of
the Ramsay Bubble of Transmutation(?) of the Elements and other Unreliabilities. The first will be a land mark forever in the history of scientific achievements. The second is the newest exponent of "Modern Alchemy" as nearly (or keenly) associated like the old with men who are in essence arrant humbugs and notoriety hunters. Yet it is degrading to think thus men like Ostwald, Randall(?) and Co(?) could be taking in with such unreliable work: or in any case instead of acclaiming it as the greatest discovery since Lavoisier should not have waited until other people had confirmed it~
Instead of remaining in Ireland I come slowly in here to be near Cambridge where my work begins next week. The weather has been awful but the Sea ??? has done my throat(?) good which has been giving me great trouble for some time. I am not able to stand another congress(?) even if I was free to go. From the beginning I told the English Committee that they had better not depend on my being able to be president as my health was ?? "Sir William" had no difficulty in promising to take up any duties where he can be ?? and now they write to me again stating that the great in low temperature work will be unable to be present. But you (?) ?? can tell us the chance or promise (?) of a decoration (?) would bring him by ?? train (?). I have nothing to say to the Paris Congress; and I go further and remark that at the present juncture you are the man to say everything to such a gathering.
With regard to my charcoal expts. the compression (?) of the ??? is at (?) nearly the same pressures so far as I remember. Naturally I made a careful study of the Hydrogen values with different concentrations and this (?) giving ?? my partial pressures in order to infer the corresponding values (?) of Helium. In the same way N and O were examined.
It is needless to say I wish you and madam Onnes a great reception in Paris and that it will go merry as a ?? Bull.
With kindest regards.

2 November 1908

HKO to Dewar 22 November 1908 ,

I thank you very much for your kind telegram and letter. Wednesday was a beautiful day. Prof. van der Waals spoke splendidly. The more I was happy to be allowed to quote your words. They did give him great satisfaction and I will not forget his beautiful look, as I quoted them. You know Van der Waals is
not only a master genius, but also a really great man. What you say about there not being given enough honour to him by other countries, is quite my idea. The sole explication is that his work wants time to be seen in its full greatness by the general scientists. How long took it before it came to general notice even. There was only your Maxwell who appreciated well. Clausius did not grasp the scope of the work at all. When people like Clausius fail who will you think of other one hurrying after the nearest of the new. Bo1tzman had an adequate idea of Van der Waals work. But he himself has not found the appreciation he deserved by his revolutionizing views. It is like you say one have to await the work of time, which sifts ---as v.d.Waals once said to me --- admirably well that which has real value from the other things, and I trust one will see ere long the apotheosis, and with you and I both with the same fervour.
My health, that has been up and down just allowing me to do my work in Paris, but not to take part in any other thing, is now improving, so that ere long I hope to be working as before.

15 November 1908

HKO to Dewar 15 November 1908

There will be given by the Amsterdam Society for physics etc. Wednesday a medal to Prof. v.d.Waals and a medal to me. At this solemnity an address in honouring and the medal, will be made to v.d.Waals by the president of the society prof. Zeeman, my pupil as you know, and to me by prof. v.d.Waals. In my short reply I hope to have occasion to draw the attention to the prominent place you give in your papers to vdWaals theory. What is to be found in print has been repeated by you very strikingly I in your kind letter of 25 July to me as follows "the master of us all. He is after all the creator of all our ideas and cannot be too highly honoured". These words expressed very beautifully my meaning. So I come to ask you if you would like it that I quote this passage of your letter in public.

17 November 1908

Dewar to HKO 17 November 1908

Dear Professor Onnes,

You are at full liberty to use the quotation from my letter. It is needless to say that I am delighted that Holland is about to honour the Master and the Pupil. I must confess, however, to some disappointment that far more honours have not been accorded to Prof. van der Waals by other countries. In saying this I don't mean to convey any want appreciation of your own work; seeing you can afford to wait. I am convinced as the great (?) progress that the great scientific work of Van der Waals will be more thoroughly appreciated and rewarded, and I can only hope to live long enough to witness the apotheosis of the Master shining(?).
My Cambridge lectures are on so I am running backwards and fowards between London and the University. With kindest regards.

11 Maart 1909

HKO to Dewar 11 March 1909

From Nature I see the honour you received Febr 11 and I most hartily congratulate you with it.
It is with the greatest sympathy and satisfaction that I see the growing appreciation of your splendid work and the great advances science owes to you, and I hope you may enjoy in health further appreciation. My health is restoring itself but I am still obliged to take all care and work suffers much from it.

13 Maart 1909

Dewar to HKO March 13, 1909

I always associated decorations a sane sign of rapidly approaching decay and dissolution; unless such things are reached early in life. If one only likes long enough having done creditable scientific work such things are ?? to arrive , so that in reality their value should not be exaggerated. I thank you all the same for your kind letter.

I am very pleased to learn your health has improved and that soon you will be able to return to your old labours. I cannot give any good account of my own condition. I managed to lecture in Cambridge between October and December and then broke down again; so that I have done nothing but undergo medical treatment. My working days are I fear doomed. However, one still has a kind of 1anguid hope.
With kindest regards .

30 Januari 1910

HKO to Dewar 30 January 1910

I have yet to congratulate you on the occasion of your receiving the Davy medal --- one of the greatest honours given in your country ---and your nomination as a foreign member of the Academy in Rome proving that the continent shares the admiration of England for your work. I know your wish not to be honoured.
You have the right to have the conviction of having some work of the highest value for science, and such conviction gives greater satisfaction and much greater happiness than honour can afford. But you will permit to your admirers and friends to rejoice their ??? when the importance or your work is generally understood and when justice is done in paging to it the tribute of honour that deserves.
With much compassion I heard the death of the late W.L.Mond (?), knowing that you lost an excellent friend in him.
My present letter has the object to entertain you for a moment on this Association Internationale de Froid, for which I beg you to give me leave. I have been very content to hear from M.Leonard that you had taken a kind interest in its object and had been elected a member of the first committee, that of liquefied gases and ??? . You have seen I suppose that M .d'Arsonval and I myself have been charged to organize the work of this committee. The first thing we have taken to hand is to make a project for the units to be used in international intercourse (and so to be introduced in national use) in the matter of cold industry. M. d'Arsonval and I have asked M.Guillaume who devotes all his life to the metric system to take the leading and to ask a small group of representative scientists to form a committee which would draw a report that could be submitted as a subject of discussion to the conference of all nationalities to be gathered for the occasion on the units(?) to be accepted by the second congress at Vienna. We want representative men to give a moral value to our report and we
want a small select group only to come to a practical result. It was in this quality as our delegate that M.Guillaume asked you to take part in this small commission, in which I myself on his request have also accepted a seat. On my inquiry he informs me that you have not taken a decision till now, and so I come to ask you not only in the interest of the scientific subject we wish to promote but also as a personal favour for me to accept.
The great meaning of our report of course is to found the units for the ??? of cold on the metric system and to develop to thermodynamics the CGS that has conquered al ready the electric trade. The work will not ask from you to make any journey, which would not suit the state of your health, it can all be done by correspondence. M.Guillaume will first make a preliminary report on which he has been already consulting myself, then critics will be asked and then views of the different members worked out till we come to a report with which we all can harmonize. We have to put the scientific opinion before the congress and scientific opinion in the many points of course agrees ??? So I hope that it can not take much of your time, and the might of your name could be of great use for the good object we have in mind. In coming to you with my request I accept that you take just as I do myself a keen interest in the propagation of the metric system and the CGS as being profitable to the solidarity of mankind and that therefore you have sympathy for all that can favour the further, introduction of the metric system and the CGS in the trade.
Helium work is proceeding, but as you will understand, especially because I have yet to take much care this winter as to my health, very slowly. Happily I am allowed to trust that after having taken yet this winter the prescribed care I can resume my work with the intensity of before. This summer in Switzerland did me a lot of good so that I have been regular at my work without any disturbance since a much better position than last winter, when I was repeatedly absent from the laboratory for many weeks.

23 October 1910

Dewar to HKO October 23, 1910

I wi11 read you r Report as soon as I can manage to devote my mind to ?? difficult scientific questions. In any case I feel sure that I am not likely to improve on anything that has passed through your hands.
I am very grieved to learn that you are not as strong as we all would like. After all we are only mortals; but you have been putting a strain on yourself. This even an immortal could hardly stand. Let us hope a quiet reach (?) on Olympus will effect a thorough cure.
I have been chronically ill for some years in many ways that I need not go into. Last summer I had to undergo an operation for the removal of a tumour from "?? of my vocal cords; and I have just returned and settled; where I was send; doomed to silence for more than two months. My voice is gradually improving and the medical faculty regard the tumour as non malignant. Naturally at my age only time can tell.
I have not been allowed to read any scientific memoirs for some time. What you tell me about your helium and oxygen work is full of interest and shows great strides in investigation (?).
Pray convey our ?? kindest regards to Madam Onnes and accept the same for yourself.
I have to be in residence in Cambridge this term and am only in London intermittently.

12 November 1910

HKO to Dewar 12 November 1910

We were very sorry to hear that you have had to pass through a serious operation, but am happy to hear that all did go well and that the medical faculty has the but(?) hope for further progress. We all have rejoiced very much in the Nobel prize being given to prof. Van der Waals. Time sets all right! You have done a very great pleasure to prof. van der Waals by your kind telegram. I visited prof. van der Waals soon after he received it and I am sure you will hear with pleasure that I saw how he was very much touched by your kind gradulations. You know that you are considered very highly by him and he did show me immediately your telegram. I told to prof. van der Waals how you have given the opinion expressed in your telegram long before in your letters to me.

May I be allowed to ask again about the Rapport de la Commission preparatoire pour les huiles frigorifiques. ?? prefer for its publication. Would you allow to print your name as a member of the commission on the title page as is done in the proof ? And if so would you have the kindness to say it with a word on a card. I hope professor Dewar that your health will go improving.

14 November 1910

Dewar to HKO November 14, 1910

You are at liberty to use my name as you suggest. The enclosed notes were made merely as personal criticism from the strictly scientific point of view. All these have no doubt passed through your mind long ago. Do not interrupt the Report because of anything I have said.
The triumph of Van der Waals was bound to come and I am only too delighted to have liked long enough to see it take place. But more must come yet.
I am slowly improving but my wife is ill again,
Kind regards.

17 November 1910

HKO to Dewar 17 November 1910

My best thanks for your kind letter and interesting remarks. Some indeed had passed through my mind but they gain a very great weight by being also advanced by you, and for a part can be met by changes in the redaction (???). There remain difficulties which M. Guillaume and I myself have felt as a matter where in the first place the manufacturers must be contented, so that there can only be question to do the propositions is a way that opens a chance for coming afterwards to the introduction of the purely scientific procuring that has been followed in electricity. One of the great difficulties is that frigorotechnics is only a small part of applied thermodynamics and mechanics. I sent you notes with my remarks to M.Guillaume and after his answer will write you again.

1 Februari 1912

Dewar to HKO February 1, 1912

Whatever you tell me I ought to do depend upon it, I will acquiesce in. I am very conscious however that I can't give you any ideas or real aid in the great work you have undertaken, but all the same I will be able in any case to support the cause you represent. Pray use my name in any way you think proper in the committee.
If it even had been possible at my age to continue difficult and exacting low temperature work, the death of my patron Mond would have brought it to an end, seeing that the Royal Institution has no friends of its own to ?? Nowadays when I am able I have to keep to simple experiments that give little anxiety. I enclose a notice of my last public lecture chiefly dealing with the question of life at low temperatures.

I am very glad delighted to learn you are getting strong again and ? with for your complete recovery. The world of science, and ,all your admirers hopefully anticipate that for many years the usual splendid discussions will come from your laboratory.
As for congratulations, our scientific honours the less said the better when they simply result from living long enough and nothing else.
Kindest regards...
I am grinding away with my lectures here and find the large classes in organic chemistry ( ?? ) heavy work.

19 Mei 1912

HKO to Dewar 19 May 1912

I have just sent a telegram to express my most cordial wishes on the occasion that the Dutch Society of Science at Haarlem has honoured itself by adding your glorious name to the list of her foreign members.
I know you have for your work a greater reward than any honour can afford. In fact what can one wish more than to have done imperishable work and to have opened the ground on which a rich harvest of fruits is coming to science but though you have the great satisfaction of having done this I hope that it will be a pleasure to you to see that all scientists is (sic) allowed share the admiration for your work, which you know is full by those who stand nearest to your work. It is moreover in the interest of pure science that work which has truly advanced it receives in justice then appreciation which it deserves.
My wife who joins in my congratulations and I myself we hope that the health of Lady Dewar will have improved and is now ??? that you can enjoy the news being both quite well.
I beg leave to come in this same letter to you with a request , which I wished already for some time to do.
At the Vienna congress of the Association International du Froid, the l st section claimed it desirable that any research work should be subventioned by the Int. Association. Prof. v.Linde has reported this proposition ??? ???. In consequence this year a subvention of 5000 francs has been granted to this effect and I value it highly as accentuating the international character my laboratory has got.
I would be very happy if on this occasion there could be formed a committee patronizing subventions to my work. I hope that under the presidency of M. van der Waals which speaks for itself you, M.Cailletet, v.Linde and Olszewski will take part in it. I would have to submit to this committee in due time the report on the way I will have made use of this international subvention and so again in the years in which the grant will be continued. The fact the sympathy you have for my work makes that I take the liberty to come to you with this request. Immediately after the vote at Vienna I told the assembly that I hoped to have the said committee as patronizing committee. I hope you will agree to my request now it has become of(?) actuality.
I am working continually with liquid helium. but of course I can only progress very slowly as every experiment asks afresh a long preparation. I have been able to prove that resistance of mercury ??? below 10-6 of the value (solid} at $0^{o}$ C.

22 Mei 1912

Dewar to HKO May 22, 1912

I was pleased to receive your wire on Sunday and will no doubt later get a letter from the Dutch Society of Science. In the meantime I can only express my gratitude to all my friends in Holland. I consider Holland has taken the highest rank in the march of scientific progress.
You will continue to ?rate my little work. Naturally perhaps only you and I can only appreciate the difficulties of low temperature research and its awful worries: but we must not forget what we have done someone else might have done provided they possessed (?) special(?) aptitude for such labours. This view keeps us modest of wishing(?) more(?).

I agree to the necessity of the committee and the subvention(?) and would be glad to help. but I do object strongly to your being put in a position of reporting to any one.
The world of science has perfect faith that the money would be used in a proper manner by you and thus ?? the case it becomes an ?? (and I hold on indignity (?») not to be perfectly free to use the endownment in any way you think proper irrespective of publications, reports, papers, accounts etc. I have always refused to accept any condition with monetary contributions, and I feel certain that if you take the same attitude you will be better of and the more admired.
My wife managed to get through winter pretty well, but she is far from strong. As for myself I attend to the general(?) work of the Institution and my Cambridge lectures; but all initiative and research has evaporated. What can one expect when in a few months (if I like) I will be 70.
Kind regards...

20 December 1919

HKO to Dewar 20 December 1919 IT IS TYPEWRITTEN

12 October 1922

HKO to Dewar 12 October 1922.

As I accepted the invitation to come to lead off the discussions to be held before the Faraday Society on October 16, I had hoped very much to see you in London. I hoped to have been accompanied by my wife, who wished very much to be presented to Lady Dewar.
It had been a wish for many years to meet you personally and to see your beautiful and interesting land and the many good friends we have there. I regret very much that I have not been allowed to cross at this time of the year exposing myself at the same time to the fatigues of the trip. We hope now to come in a better time of the year and apart of any official invitation.
Please accept the proof of the paper Dr. Crommelin will read for me on Monday and which I had wished to hand you myself. Probably it is put to you on behalf of the F.S. But at all events my first thought was to offer you a copy of this proof.
I hope Dr. Crommelin will bring me good news of the health of yourself and Lady Dewar.

14 December 1922

HKO to Dewar 14 December 1922

It is only late that I come to answer to all the kind gratulations and marks of affection I received on the 40th anniversary of my professorate. I am obliged to do it by printed cards, but I would feel very unsatisfied if I did not make an exception with you and if I did not thank you by a letter, be it only short for all you contributed to make the 11 th of November an unforgettable day for me and my wife.
Your kind and poetical telegram [cannot be found] did not only great pleasure to myself but also to my friends to whom I was very happy to show it. And thus I have to thank you most cordially for the sympathy with which you strongly supported the commission to make her able to offer me as the day of my jubilee as professor the magnificent memorial book covering the 18 years of the Laboratory part since my silver ??? jubilee. The president of the commission was Prof. Zeeman and it was a very great satisfaction of my profesorate to be addressed by my most brilliant pupil, now one of my best friends. It was a happy, very happy day.

Just found last week that Indium also becomes supraconductive in liquid helium. Note that it fills a place indicated by the atomic table, and

| | | |
|---|---|---|
| | Ind | Sn |
| | 49 | 50 |
| Hg | Tl | Pb |
| 80 | 81 | 82 |

the other(?) supraconductors. My collaborator was Tuyn, the same with whom I found Tl.

## Inhoud

| | | |
|---|---|---|
| 1895.0718 | Brief HKO | Correspondentie Dewar |
| 1895.0720 | Brief Dewar | Correspondentie Dewar |
| 1896.0306 | Brief HKO | Correspondentie Dewar |
| 1896.0308 | Brief Dewar | Correspondentie Dewar |
| 1896.0512 | Brief HKO | Correspondentie Dewar |
| 1897.0210 | Brief HKO | Correspondentie Dewar |
| 1897.0212 | Brief Dewar | Correspondentie Dewar |
| 1898.0611 | Brief Dewar | Correspondentie Dewar |
| 1903.0507 | Brief Dewar | Correspondentie Dewar |
| 1903.1229 | Brief HKO | Correspondentie Dewar |
| 1904.0105 | Brief Dewar | Correspondentie Dewar |
| 1904.0530 | Brief HKO > Young | Correspondentie Dewar |
| 1904.0606 | Brief HKO > Young | Correspondentie Dewar |
| 1905.0117 | Brief HKO | Correspondentie Dewar |
| 1905.0118 | Brief Dewar | Correspondentie Dewar |
| 1905.0608 | Brief HKO | Correspondentie Dewar |
| 1905.0712 | Brief Dewar | Correspondentie Dewar |
| 1905.0720 | Brief Dewar | Correspondentie Dewar |
| 1905.0722 | Brief HKO | Correspondentie Dewar |
| 1905.0724 | Brief Dewar | Correspondentie Dewar |
| 1908.0407 | Brief HKO | Correspondentie Dewar |
| 1908.0415 | Brief Dewar | Correspondentie Dewar |
| 1908.0720a | Brief HKO | Correspondentie Dewar |
| 1908.0721 | Notities HKO | Correspondentie Dewar |
| 1908.0810 | Brief Dewar | Correspondentie Dewar |
| 1908.0810a | Brief HKO | Correspondentie Dewar |
| 1908.0901 | Brief Dewar | Correspondentie Dewar |
| 1908.0903a | Brief HKO | Correspondentie Dewar |
| 1908.0903b | Brief HKO | Correspondentie Dewar |
| 1908.0904a | Brief HKO | Correspondentie Dewar |
| 1908.0906 | Brief Dewat | Correspondentie Dewar |
| 1908.0925 | Brief HKO | Correspondentie Dewar |
| 1908.0929 | Brief Dewar | Correspondentie Dewar |
| 1908.1102 | Brief HKO | Correspondentie Dewar |
| 1908.1115 | Brief HKO | Correspondentie Dewar |
| 1908.1117 | Brief Dewar | Correspondentie Dewar |
| 1909.0311 | Brief HKO | Correspondentie Dewar |
| 1909.0313 | Brief Dewar | Correspondentie Dewar |
| 1910.0130 | Brief HKO | Correspondentie Dewar |
| 1910.1023 | Brief Dewar | Correspondentie Dewar |
| 1910.1112 | Brief HKO | Correspondentie Dewar |
| 1910.1114 | Brief Dewar | Correspondentie Dewar |
| 1910.1117 | Brief HKO | Correspondentie Dewar |
| 1912.0201 | Brief Dewar | Correspondentie Dewar |
| 1912.0519 | Brief HKO | Correspondentie Dewar |
| 1912.0522 | Brief Dewar | Correspondentie Dewar |
| 1919.1220 | Brief HKO (verwijzing) | Correspondentie Dewar |
| 1922.1012 | Brief HKO | Correspondentie Dewar |
| 1922.1214 | Brief HKO | Correspondentie Dewar |