Chapter 3
James Dewar and His Route to the Liquefaction of Hydrogen

J.S. Rowlinson

In the 1840s Kincardine was a small port on the river Forth above Edinburgh where the local inn, the *Unicorn*, was kept by Thomas Dewar and his wife Ann. They had seven sons, of whom six survived infancy, and the last of whom, James, was born in 1842. He was educated first at the local school and then, after the death of both parents, at the nearby Dollar Institution (now Dollar Academy). In 1859 he went to Edinburgh University where he studied under the physicist Guthrie Tait and the chemist Lyon Playfair. As was usual for students from financially modest backgrounds, he took no degree but served as an assistant to Playfair and later to his successor, Crum Brown. His earliest research was in organic chemistry and physiology but he spread himself widely and published steadily, so creating a reputation as an active and coming young man. He applied unsuccessfully for the Regius Chair of Chemistry at Glasgow in 1874, but was then a candidate for the Jacksonian Professorship of Natural Experimental Philosophy at Cambridge where, in 1875, the electors were looking primarily for someone who could teach chemistry to medical students, but where the wide but obsolete conditions of the endowment allowed the occupant considerable freedom to choose his own field of teaching and research. The resources immediately available to the new professor were, however, even less than Dewar had been accustomed to in Edinburgh, so when the Fullerian Professorship of Chemistry at the Royal Institution in London was advertised in March 1877 he applied for that also. He was again successful and held both chairs for the rest of his life. The resources of the Royal Institution, or the RI as it was commonly known, proved over the years to be greater than anything that he could probably have mustered in Cambridge in the last quarter of the nineteenth century.

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He gave his statutory lectures at Cambridge, and cooperated with George Liveing, the Professor of Chemistry, in a long series of papers on visible and ultraviolet spectroscopy. He was assisted there also by a series of Demonstrators with whom he maintained his interest in organic chemistry but, from 1877, the centre of his research was firmly established in London at the Royal Institution.

Dewar had a strong interest in the history of chemistry and knew of the reputation that Davy and Faraday had acquired at the Royal Institution for research on the liquefaction of gases. Faraday claimed that he had been the first to liquefy six different gases but he never succeeded with the simplest, the so-called ‘permanent gases’, the elements oxygen, nitrogen and hydrogen. Many gases such as ammonia or chlorine could be liquefied by pressure alone, or by pressure with the modest degree of cooling provided by such refrigerants as ‘Thilorier’s mixture’ – solid carbon dioxide partially dissolved in ethyl ether. Faraday recognised that the liquefaction of the ‘permanent gases’ would require preliminary cooling to temperatures beyond his reach. It was commonly held that the implication of Boyle’s and Charles’s laws was that the zero of temperature was at about −273 °C so that there was much thermally unknown territory to be explored. Thomas Andrews in Belfast clarified the position in the 1860s by showing that for each gas there was a ‘critical temperature’ below which a gas must be cooled before it could be liquefied by pressure. There the matter rested until, a few months after Dewar’s appointment to the Royal Institution, the field suddenly sprang to life with a report from a meeting of the Académie des Sciences in Paris on 24 December 1877 that two men had simultaneously and independently liquefied oxygen. They were Louis Cailletet, in the family iron-works at Chatillon-sur-Seine in Burgundy, and Raoul Pictet, a physicist in Geneva. Their methods differed but both depended on compressing a gas to about 300 atm pressure, cooling it with an external refrigerant and then cooling it further by letting a first part of the sample of compressed gas do work in expelling the second part through a narrowly opened valve. This work could come only from the energy of the first part which was thereby cooled and partially liquefied. The liquefaction was visible as a mist either in the gas remaining in the cylinder (Cailletet) or in the jet of gas coming out through the valve (Pictet). So far, so good – the so-called permanent gases *could* be liquefied, but neither man was able to collect the liquid as a static sample of a size large enough for it to be studied further in the laboratory, or for it to be used to cool other bodies down to the temperature of the new liquids.

The next step was taken by Zygmunt Wróblewski and Karol Olszewski in Krakow. They cooled the compressed oxygen in a stout glass tube cooled by liquid ethene. At atmospheric pressure this boils at 169 K which is above the critical temperature of oxygen at 155 K. Their innovation, in 1883, was to cool the ethene further by pumping away its vapour until the pressure was reduced to about 0.04 atm when its temperature is 130 K and so at a temperature at which the oxygen can be liquefied by a modest pressure of about 20 atm. They prepared liquid oxygen in small samples but had still to solve the problem of preserving it from rapid evaporation by the heat conducted from the surroundings. Dewar repeated their process at the Royal Institution and, in 1892, solved the last problem with his invention of the silvered vacuum flask, now known by his name in laboratories throughout the world, or in households by the patented name of ‘Thermos flask’. Liquid oxygen, and the physically similar liquid nitrogen, and so also liquid air, were now available, albeit at some trouble and expense, in any laboratory that chose to follow the route mapped out by Cailletet, Pictet, Wróblewski, Olszewski and Dewar. Liquid oxygen was first exhibited in Britain by Dewar in a lecture at the Royal Institution in June 1884, and by 1892 he was making it ‘by the pint’ (0.6 l) (Dewar 1884, 1886; Brock 2002; Rowlinson 2009).

Hydrogen alone remained as a gas whose ‘permanency’ was a challenge to the chemists and physicists. Its critical temperature, now known to be at 33 K, was correctly estimated by Wróblewski by a comparison of the behaviour of hydrogen at high pressures with that of the gases that could be liquefied. Such a common family resemblance of different simple gases had been established by J.D. van der Waals in his ‘law of corresponding states’ in 1880 and was the guiding principle of those seeking to liquefy hydrogen, and later helium, which had been found on Earth in 1895. Dewar was later to suggest that van der Waals’s law, or principle, as it is now more correctly described, was the greatest advance in thermodynamics since the time of Carnot, an assessment that overlooks the contributions of Thomson, Clausius and Gibbs (Dewar 1902). But then Dewar was exclusively an experimentalist and so valued highly any generalisation that helped him plan his experiments. Many of his papers and lectures contain such phrases as ‘Now what are the facts?’, and he often seemed to think of theory as nothing better than speculation.

A temperature of 33 K is too low to be reached by the evaporation of liquid oxygen or nitrogen which solidify at 54 and 63 K respectively. In principle there was no limit to the cooling that could be achieved by making the hydrogen perform external work by, for example, driving a piston against a gas under pressure. This principle was used and well-understood by engineers who built commercial refrigeration plant but difficulties of thermal insulation and of lubrication limited the lowest temperature that could be reached in this way to about 178 K. This temperature was achieved by Ernest Solvay in Belgium in 1885–1887 and the method was patented in several countries. He recalled this work to public notice 10 years later (Solvay 1895) after Linde’s success with a radically different method of cooling.

Carl Linde was a German engineer who had learned his thermodynamics from Rudolf Clausius in Zürich and who had appreciated that there was a method of cooling a gas that required no moving parts in the low-temperature section of the machinery and so which avoided the mechanical difficulties of lubrication and minimised the thermal difficulties of insulation. The method depended on expanding a compressed gas in a steady flow through a valve or throttle when the increased mean separation of the molecules in the expanded gas reduced the small attractive potential energy of pairs of molecules at small separations. It is the force of this attractive potential energy (now called the van der Waals force) that is responsible for the condensation of a gas into a liquid. Its origin and molecular magnitude were unknown in the 1880s but its existence and its macroscopic consequences had been established from measurements made in the 1850s by James Joule and William Thomson. They were not concerned with gas liquefaction but needed to know the thermodynamic consequences of such a potential energy in order to validate their calculations of the absolute scale of temperature and to secure the foundations of the two laws of thermodynamics (Chang 2004; Rowlinson 2009). The effect is small, a cooling of about 0.25 K for a fall of pressure of one atmosphere for oxygen and 0.20 K for nitrogen. For hydrogen, for which the molecular attraction is small, and for which the mutual repulsion on molecular collisions is more important, the Joule-Thomson effect, as it is now called, is negative at ambient temperatures; that is a reduction of pressure leads to a rise of temperature, or the gas warms slightly on expansion. It was expected, on the good grounds established by Andrews, that hydrogen could also be cooled by 'Joule-Thomson' expansion if the starting temperature was sufficiently low. The 'inversion point' at which this change of sign occurs is now known to be about 190 K. A so-called perfect gas, which conforms to the Boyle's and Charles's laws, would neither warm nor cool; it has a Joule-Thomson coefficient of zero.

There is little mystery about how Linde came to consider the use of the Joule-Thomson effect for cooling a compressed gas – he had learnt his thermodynamics from its leading exponent, Rudolf Clausius. There is, however, a mystery about how it came to be exploited by British chemists and engineers. By the 1890s Joule was dead and Thomson's many interests now lay outside thermodynamics. But a most unlikely candidate came forward and hit on the cooling of a flowing gas by expansion, without at first any knowledge of the Joule-Thomson effect. William Hampson was an Oxford graduate in classics and philosophy who seems to have never studied physics or engineering (Davies 1989). Nevertheless he conceived in 1894 of a new apparatus that could be used for cooling and so liquefying gases. Its essential component was an 'interchanger' in which a stream of gas at ambient temperature and high pressure is cooled by passing it down a long tube which is at each point in thermal contact with a second tube containing a colder gas flowing in the opposite direction, at a lower pressure but with the same rate of mass flow. The simplest way of ensuring this thermal contact, which was chosen by Linde, is by threading one tube inside the other. In this way the first (or high- pressure) stream is cooled on exit to the temperature of the second (low-pressure) stream at the point at which it enters the combination of tubes; the temperature of the two streams is interchanged. Hampson proposed to take gas at room temperature and pressure, compress it to, say, 200 atm. so warming it by the work done on it, cooling it back to ambient temperature with cold water, and then passing it through an 'interchanger' in which it was cooled further by the contact with the stream of colder gas flowing in the opposite direction. It next went through a partially-open valve at which the pressure fell and, for oxygen and nitrogen, the temperature fell also as a consequence of the Joule-Thomson effect – perhaps by 22 K for oxygen falling from 100 to 10 atm. This cooled and expanded gas now becomes the second stream in the interchanger and so gives up its 'cooling' to the incoming high-pressure gas. The returning gas is re-compressed and starts the cycle again but now already a little cooler than on its first pass. This recycling is continued, with the gas getting cooler in the interchanger on each cycle until it liquefies in part on the low-pressure side of the expansion valve. Hampson conceived such a device but had not the resources to build it, so in

November 1894 he went round to the Royal Institution, where he had had an introduction to Robert Lennox, Dewar's principal technical assistant, to propose collaboration. There is no evidence that Hampson knew then of the Joule-Thomson effect and so we cannot know why he thought that the apparatus would work. From the wording of a patent that he took out the next year (Hampson 1895) it seems that he supposed that the combination of an interchanger (a device already known to refrigeration engineers), expansion through a valve, and the repeated recycling of the gas, constituted a novel and so patentable process. Lennox had learnt some engineering and maybe some thermodynamics from his apparently casual attendance at lectures in Glasgow by James Thomson, the elder brother of William, but it seems that he too had not then heard of the Joule-Thomson effect. Lennox thought about Hampson's proposal and they discussed it more than once. Lennox claimed later to have told him that, on reflection, he thought that it was 'nebulous' and would not work. What seems certain, however, and is critical to our story, is that Lennox told Dewar of Hampson's visit. Dewar was obsessively secretive about his research and within a few days of Hampson's visits he had notified the Managers of the Royal Institution that he had repelled 'endeavours on the part of certain strangers to obtain access to the laboratory.' This report is apparently the only communication of this kind that he ever made to the Managers, and is to be found in their Minutes at the Royal Institution for 3 December 1894. Hampson is not named but there can be no doubt that Lennox had told Dewar of the visit and it is most unlikely that he did not also tell him the name of the visitor and of his proposal for collaboration.

When Hampson got no reply from Lennox to further letters early in 1895 he submitted his application for a patent and started discussions with Brin's Oxygen Company (later British Oxygen Company, or BOC) who were interested in any new way of preparing oxygen more cheaply than by the then usual chemical route via barium peroxide. Meanwhile Dewar and Lennox had not been idle but had decided to follow up Hampson's proposal. It seems that Dewar, although he knew of the Joule-Thomson effect, did not then associate it with this possible new route to the liquefaction of gases. He had delivered a commemorative lecture after Joule's death in 1899 (Dewar 1890) but his remarks there show that he did not attach much importance to the effect and that he had not fully appreciated their results, saying, for example, that Joule and Thomson had found that hydrogen was cooled on expansion at room temperature, whereas they had shown that it was heated.

The first mention of a radically new apparatus is in an undated entry in Dewar's laboratory notebook which, from its position in the book, must have been written in about June 1895. It was called the 'direct liquid air apparatus'. Meanwhile a tight security was maintained; that summer he refused to let either Raoul Pictet, now in Berlin, nor Heike Kamerlingh Onnes from Leiden, visit his laboratory. It was unusual for the monthly meetings of the Managers to discuss anything other than the business side of running the Royal Institution but on 2 December 1895 the new 'direct' apparatus was revealed: 'The Managers after the Meeting went to the Laboratory where Professor Dewar showed the making of liquid air by a simple method.' (Managers' Minutes 2/12/1895) The demonstration was described in the *Times* the next day, when the reporter was more impressed by the simplicity and cheapness of the new process rather by any new principle that had been exploited. Indeed, he said explicitly ‘The principle is a familiar one, and has long been practically applied upon a large scale in the laboratory of the Royal Institution’ (*Times* 3/12/1895). It is hard to attach any meaning to this sentence, unless the writer had in mind Dewar’s invention of the vacuum flask 3 years previously.

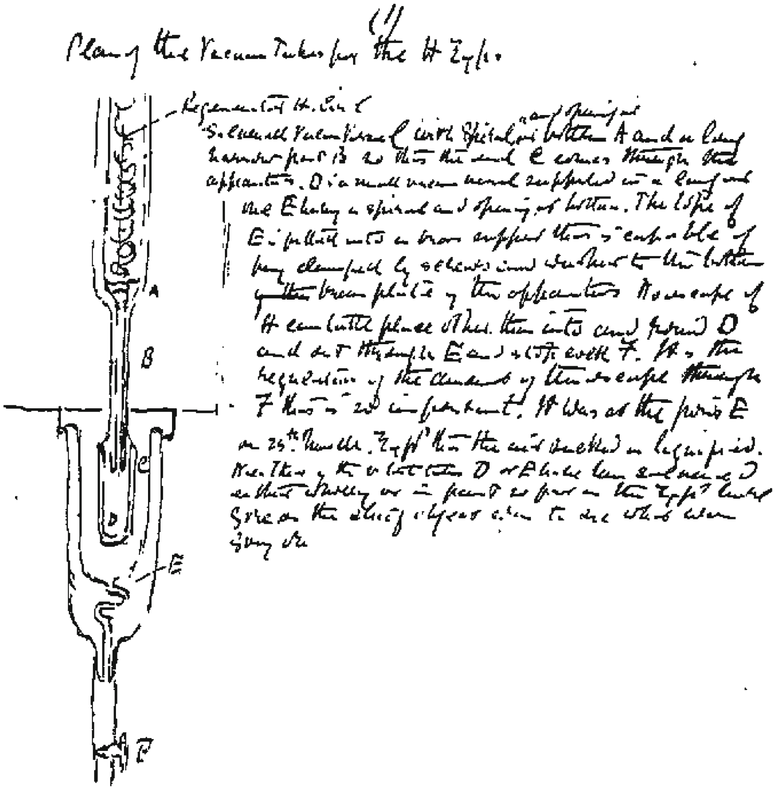
Linde, knowing nothing of the discussions and work in London, had built his own apparatus, which he had demonstrated to scientists and engineers in Munich in May 1895. There was an account of his success in a German technical journal in August but this was probably not seen by Dewar or by Hampson since the journal was in neither the British Library nor the library of the Royal Institution. A second account Linde’s work was given before a meeting in Aachen and a translation of this report appeared in Britain on 4 October in the *Engineer*, a journal that was taken by the library at the Royal Institution (Schröter 1895). The dependence of the new process on the cooling provided by the Joule-Thomson effect was now made clear, with direct quotation of the quantitative results from Joule and Thomson’s papers. The appearance of Schröter’s report put Dewar in a tricky position. He was clearly proud of his new ‘direct’ route to liquid air, which he had not yet published, and he was keen to claim credit for it. Was he to try to acquire as quickly as he could information about this novel use of the Joule-Thomson effect, or was he to claim that the effect was already well-known and it was his ingenious combination of its use with the interchanger that was his claim to novelty? In the event, he privately followed the first line, while in public he followed the second. Within days of the publication of Schröter’s paper he had written to Thomson (now Lord Kelvin) to tell him of his success with liquid air (Dewar 1895a):

> Private 29th October 95
> Dear Lord Kelvin,
> I have just received the results of some experiments which I left going this morning. And I think that they will interest you. For long I have been at the Hydrogen problem and have almost given it up in despair, as beyond my resources. The result has been that I have had to invert the problem and ask myself how can air be liquefied from temperatures far above the critical point.
>
> This I find is easy if I repeat the old experiment you made with Joule. - - - Thus I feel almost certain that if hydrogen in the liquid state can be collected I will do it through and on account of the work of Kelvin and Joule
>
> Ever Yours Truly
> James Dewar

To find out more about the Joule-Thomson effect he went to his usual source of advice on problems in theoretical physics, his brother-in-law, Hamilton Dickson. He was a mathematician and physicist who had trained under William Thomson at Glasgow and was now a Fellow and the mathematical tutor at Peterhouse, Cambridge. He had been giving such advice to Dewar from the time of his marriage to the younger sister of Dewar’s wife in about 1879. He now wrote on 11 December 1895 to explain, not very clearly, how the Joule-Thomson coefficient could be calculated, and how the lowering of the temperature could be calculated from Joule and Thomson’s experimental results (Dickson 1895). So Dewar was well-prepared when he described and demonstrated his new apparatus before the Chemical Society on 19 December (Dewar 1895b, 1896). He did not, however, mention explicitly his use of the Joule-Thomson effect, nor the work of Linde and Hampson, saying only that 'The experiments of Joule and Thomson and Regnault on the temperature of gas jets issuing under low pressures are well known'. He went further with the extraordinary claim that: 'Apart, therefore, from important mechanical details, and the conduct of the general working, nothing new has been added by any investigator to the principles involved in the construction and use of low-temperature apparatus since the year 1878'. In the discussion of the paper, Bertram Blount, an 'engineering chemist' known for his plain speaking, asked why Dewar had not acknowledged the priority of Linde's work, as reported in Schröter's paper. Dewar could only reply that if his own apparatus had any 'resemblance to the Linde apparatus described by Mr Blount, chemists, for once, may be congratulated that a small laboratory apparatus works in some respects better than a large industrial plant.' He was on safe ground in denying any debt to Linde since there was too little time between the appearance of Schröter's paper in Britain on 4 October and his own demonstration before the Chemical Society on 19 December for the design, building and testing of a new apparatus.

Hampson enlisted the help of Brin's Oxygen Company to build an apparatus that was close in design to that of Dewar, a resemblance that argues again for a common source in Hampson's original drawings of November 1894 – Linde's, although working on the same principles, was of quite different design. Hampson demonstrated his apparatus publicly in March 1896, but it seems that he still did not appreciate the crucial role of the Joule-Thomson effect. A pseudonymous correspondent in *Nature* wrote that there is no evidence that Hampson understood the principle of his apparatus. His patent application and his demonstration contained nothing on the subject, a point to which Linde himself later drew attention ('Zero' 1896; Linde 1899). The almost simultaneous exhibition of this new way of liquefying gases by Dewar and Hampson led to long and acrimonious correspondence about plagiarism and discourtesy in the scientific and technical press, mainly between the two principals but which involved Lennox also, writing under the pseudonym 'Arenal' – his initials were R.N.L. This correspondence led nowhere and added nothing to the science of the field.

The next step was to try to liquefy hydrogen in bulk. This was clearly going to be difficult since its critical temperature had been estimated correctly by Wróblewski at −240 °C (or 33 K). The expansion of hydrogen, pre-cooled by liquid air, in Dewar's new apparatus had led to a jet of a mixture of gaseous and liquid hydrogen but there was no way of collecting the small amount of liquid from the rapidly moving jet. His first idea was to liquefy a mixture of nitrogen and hydrogen and to use this as a first step in cooling pure hydrogen to a temperature at which the Joule-Thomson coefficient was positive. This attempt failed, probably because hydrogen is not sufficiently soluble in liquid nitrogen. He had therefore to embark on building an improved liquid air apparatus with which to pre-cool his compressed hydrogen. There was first a small diversion while he combined forces with Henri Moissan to liquefy fluorine, the only elementary gas other than hydrogen and the recently discovered helium that was still to be liquefied. This problem was soon solved (Moissan and Dewar 1897) and he was then able to turn his attention again to hydrogen. There is no full description of the apparatus he built. None was published and so the apparatus that was subsequently acquired by the Science Museum in London was that designed by Hampson 2 years after Dewar's success.



This sketch in Dewar's laboratory notebook shows only the arrangement of the unsilvered vacuum vessels (Gavroglu 1994). These were fastened below the new liquid air plant that allowed the hydrogen to be pre-cooled down to about 57 K by liquid air under a reduced pressure of about 6 in. (15 cm) of mercury. This apparatus took a year to build and test. The sketch is entitled 'Plan of the Vacuum Tubes for the H[ydrogen] Exp.' and the helical tubes at the top are labelled 'Regenerator H coil' (Dewar always used the word 'regenerator' for what is now usually called an 'interchanger'.) The legend alongside the sketch reads, after deciphering Dewar's terrible hand-writing:

> Silvered Vacuum Vessel with spiral, and opening at bottom A and a long narrow part B so that the end C comes through the apparatus. D is a small vacuum vessel supported in a longer one E having a spiral and opening at bottom. The top of E is fitted into a brass support that is capable of being clamped by screws and washer to the bottom brass plate of the apparatus. No escape of H[ydrogen] can take place other than into and round D and out through E and stop cock F. It is the regulation of the [amount?] of the escape through F that

> is so important. It was at the point E on 25th March Exp that the air sucked in liquified Neither of the v[acuum] test tubes D or E have been silvered wholly or in part as far as the Exps have gone as the chief object was to see what was going on.

The principal novelty of this apparatus was the helical capillary coil that allowed the liquid hydrogen collected in D and C to pass to the stop cock F. This gave sufficiently elasticity to the whole assembly to allow the glassware to adjust its shape to the changes produced by the severe temperature gradients. He later wrote of this coil:

> This device, developed after many unsuccessful attempts at such a vacuum vessel, was found essential for the easy production and collection of liquid hydrogen, and as all the Royal Institution designs for such vessels have been made in Germany, they have been supplied and utilized by other workers unconscious, it may be of where or how they originated (Dewar 1901).

The best published account of his liquefaction of hydrogen is in the Friday Evening Discourse that he gave at the Royal Institution on 20 January 1899, but a fuller picture can be had from the laboratory notebook cited above. Serious experiments started on 25 February 1898:

> All apparently went well until after the liquid air was put under exhaustion when attempting to open the H[ydrogen] valve nothing would come. Nothing would remove the obstructions. Thought it was solid air but the stoppage remained hours afterwards so it looks like some solid carried forward at valve. - - - It was found that the obstruction was due to solid particles of solder carried by H[ydrogen].
>
> 14th March/98.
>
> Between 25th February and this date had repeated failure in getting Silvered Vacuum. Tube to fit the Regenerator. - - - Started the apparatus as described on the 25th Feby - - - . After a few minutes a white solid along with [?] of liquid, say 5 cc, collected in the inner of the two vacuum tubes round which the H[ydrogen] coming from the Regenerator [was] passing.
>
> 25th March/98.
>
> Since the previous Experiments we placed an enlarged coil of [?] pipe of 600 cc capacity over the end of the H[ydrogen] coil in the L[iquid] H[ydrogen] chamber before it goes [through] the Regenerator in order to catch liquid air and other impurities in the H[ydrogen]. The whole arrangement seemed to work well. - - -
>
> 25th April.
>
> Repeated the above experiment
>
> 10th May/98
>
> Started Hydrogen apparatus treating CO_2, C_2H_4 and air circuits as in previous Expt. This time the liquid was collected for 2½ hours and the air pump put on for the last half hour.- - - After ½ hour the air supply was stopped and the air pump was allowed to get the pressure reduced to 6 inches of mercury. The H[ydrogen] started running through the valve at 175 atmospheres and about the rate of 15 c. ft a minute [7 litres a second]. Shortly after starting the nozzle plugged but it got free by good luck and almost immediately drops of liquid began to fall from the V[acuum] tube into the [inner] V[acuum tube] and soon accumulated to 20 cc in volume. There was a great escape of H[ydrogen] through the [stopcock] and the outer Vacuum Vessel was covered with ice. The H[ydrogen] was a clear transparent liquid with a well defined meniscus (even better than liquid air) showing no absorption spectrum and as long as the

> surroundings of the [inner] vessel were cool seemed to evaporate very slowly. I took off the [vacuum] vessel from the apparatus and placed in the liquid H[ydrogen] a narrow quill tube; sealed at the end immersed and open at the other; when immediately solid air appeared in the tube. In the same way the He[lium] tube was placed in the liquid hydrogen and I could see that a liquid was formed. This He[lium] tube gave nothing when placed in liquid air under exhaustion. The H[ydrogen] vapour above the liquid had a very marked appearance and seemed very dense with circulating currents of a greenish blue cloud. - - - The H[ydrogen] gas before use was tested for the presence of air and was found not to exceed 1 %.

By chance Dewar's colleague Lord Rayleigh was present in the Royal Institution when hydrogen was first liquefied and was witness to this success. That day, a Tuesday, Dewar asked the President, Lord Lister, if he might announce his results at the regular meeting of the Royal Society on the Thursday. This he did, but he worked in the laboratory until the last minute. The notebook continued:

12 May

> Conducted everything as on the 10th starting with the H gas at 140^{atm} and [?] collected about 60 cc and took it out of the apparatus placing the V[acuum] tube in liquid air so that the outer surface was always [?] -183°. - - - Dipping some cotton wool into the liquid and lifting into the air it ignited with a [?] giving a [?] of flame. The cotton wool saturated with L[iquid] H[ydrogen] between the poles of a magnet was attracted. Believe this due to liquid air being condensed on its surface as solid.

The claim to have liquefied helium was soon tacitly dropped; again an impurity in the hydrogen was probably responsible for the formation of this liquid. The liquefaction of helium was to prove too difficult for the skill of Dewar, for the resources of the Royal Institution, and for the problem of obtaining sufficiently pure helium. It was to be another 10 years before Kamerlingh Onnes in Leiden succeeded with the last of the 'permanent' gases.

In 1899 the solidification of hydrogen followed its liquefaction, when Dewar cooled his liquid hydrogen by evaporation. He estimated the temperature of the melting point by gas thermometry to be about 16 K, a little above the currently accepted value of 14 K. By pumping on the solid he cooled it further to perhaps 10 or 11 K, but this was still a long way from the critical the temperature of helium, now known to be at 5.2 K.

The story of the liquefaction of hydrogen had an unexpected sequel. In 1904 an international exhibition was held in St Louis, Missouri, at which all nations were invited to show what they could do in the arts, science and technology. The centrepiece of the British entry in the Chemistry Section was chosen to be an apparatus for the liquefaction of hydrogen, an operation not yet attempted in the United States. To this end a properly engineered version of Dewar's apparatus of 1898 was designed and built by the firm of Lennox, Benton and Reynolds. This was a small engineering company in the Rodebush Works in Fulham in south-west London that Robert Lennox had established, with Dewar's cognisance, for making his ever increasingly complicated apparatus. The new version, built mainly in brass and other metals, worked on the same principles as the 1898 apparatus but was on a bigger scale. It was assembled and tested at the Royal Institution before being shipped to America. Dewar never intended to go to the exhibition himself but entrusted the demonstration of the apparatus to Joseph Petavel who had worked in the Davy Faraday Research Laboratory at the Royal Institution and had become first known to Dewar when he was the assistant to Ambrose Fleming at University College, London. Dewar and Fleming had cooperated in electric and magnetic experiments at low temperature. Dewar was a difficult man to get on with, and had previously had a row with Petavel, but had now accepted that he was the best man for the job. He ended his career as Director of the National Physical Laboratory. The choice of Petavel did not prevent Dewar from raising last-minute objections to the use of the German-made vacuum tubes with the necessary helical spiral. It is not clear what the difficulty was, but fortunately Robert Hudson, one of the British engineers involved, managed to get a fresh supply directly from the glassblowers in Berlin which reached St Louis just in time for the opening of the exhibition (Hudson 1964). The apparatus proved to be perhaps the greatest hit of the Exhibition. It produced liquid hydrogen twice a week for 2 months without a single hitch. Each public demonstration by Petavel needed between 1 and 3 l of liquid hydrogen, amounts that were then quite unprecedented. Some of this was solidified and this was also exhibited. Around these spectacular demonstrations Petavel gave over 80 lectures to the public at large and to professional physicists, chemists and engineers. At the end of the exhibition the apparatus received a Grand Prize, the highest award, and Dewar and Petavel received respectively a gold and a silver medal. The equipment never returned to Britain but was bought by the US Government, presented to the National Bureau of Standards, and re-erected in Washington. If the liquefaction of hydrogen was the climax of Dewar's career then this superbly engineered apparatus was the finest achievement of his principal assistant, Robert Lennox. But, as so often with Dewar's affairs, the story did not have a happy ending and a few years later, when Dewar blamed Lennox for his part in the failure to liquefy helium at the Royal Institution, Lennox resigned and, in the words of one of his colleagues, 'the Laboratory lost the most outstanding Assistant it had ever known' (Green 1956).

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