LETTERS TO THE EDITOR.

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The Condensation of Helium.

In addition to my short note printed in last week's NATURE (p. 559), let me begin by remarking that as recently as last year, in an address to the Dutch Congress of Natural Science and Medicine, I expressed the opinion that it would be scarcely possible to liquefy helium. Olszewski, from his expansion experiments, had deduced that the critical temperature of helium was lower than 2° K. Dewar had no more succeeded in liquefying it by expansion, and some experiences of my own on helium gas sinking in liquid hydrogen seemed to indicate that helium was nearly a perfect gas. At the same meeting I indicated the determination of the isothermals of helium, an investigation with which I was occupied, and which I had prepared by a series of researches, as the direct way to the calculation of the critical temperature.

The first results I obtained with the isothermals changed totally my views on the liquefaction of helium. From the isothermals down to −217°, it followed that the critical point of helium is at nearly 5° K., more in harmony with the estimate of the boiling point at 5° or 6° K. by Dewar, according to the helium absorbed in charcoal, and the determinations at −252° C. and −259° C. confirmed the result. It thence followed that it would be possible, by rapid expansion of helium compressed at 100 atmospheres at the melting point of hydrogen, to pass below the critical temperature, and to cause a mist to appear in the gas. Also liquefaction by the Joule-Kelvin effect seemed possible. It was to put the first conclusion to the test that I made my recent experiments.

The new features of my application of the expansion method to helium were:—(1) the great quantity of gas; (2) the application of a stop-cock on the tube to let off the gas from the tube into a gas-holder, a gas-bag, or a vacuum; (3) an extremely thin-walled beaker, placed in the thick-walled tube to protect the cooled gas against heat conduction. These devices had been used by Olszewski in his experiments on the expansion of hydrogen.

At the expansion a dense cloud appeared, from which solid masses separated out, floating in the gaseous helium, resembling partly cotton-wool, partly also denser masses, as if floating in a syrupy liquid, adhering to the walls and sliding downward, while at the same time vanishing rapidly (20 seconds). There was no trace of melting.

So far as I could judge, then, from my experiments, I considered it probable that this solid substance was helium. The helium had been burned with copper oxide and passed over charcoal at the temperature of boiling hydrogen, and I trusted to have a gas with only very small admixtures. If helium passed immediately to the solid state, then the position of the vapour-line to the adiabatics would be more favourable for condensation than if it passed into the liquid state, and the voluminous aspect of the solid mass was in harmony with this. By the above, and also by other observations, which afterwards gave rise to doubt or proved incorrect, I was for some time under the impression that I had seen solid helium rapidly giving off vapours of the pressure shown by the gas (once more than 15 atmospheres was shown). The continuation of my experiments has shown that they must be explained in quite a different way. By a not sufficiently explained cause, the gas proved to be not so pure as was supposed, considering the method of purification. In analysing what was absorbed by charcoal at the temperature of boiling hydrogen until the charcoal removed no more hydrogen, so that the gas could only contain traces of hydrogen, it could be proved that in one case the gas had contained only 0·45 and in another only 0·37 volume per cent. of hydrogen at most. (About a small possible quantity of neon I could not yet be certain.) But this small admixture must have had a very great influence; for at a first repetition of the experiment with the helium subjected to this new treatment no cloud at all was observed. In this experiment the velocity of expansion had been too small. At a second repetition with the same gas, but with greater velocity of expansion, a thin cloud appeared and vanished extremely rapidly (1 second). The mist now had a different aspect.

The explanation of the previous observation is to be found in solution phenomena of solid hydrogen in gaseous helium. The phenomena which made the impression of being the giving off of vapour had been the solution of deposited solid hydrogen in the gaseous helium, the latter rapidly returning from the lower temperature to that of melting hydrogen, and the pressure increasing in consequence. Helium at the temperatures that come into account here can, according to the theory of mixtures, take up at every temperature a percentage of hydrogen, determined by that temperature in such a way that it is not deposited at any pressure. With acceptable suppositions one can deduce that at temperatures above the melting point of hydrogen this percentage can be considerable, and that at this melting point itself it can be more than 1 per cent. From mixtures with smaller percentage, the hydrogen is only deposited at lower temperatures, *e.g.* by expansion. By the smallness of the quantity of hydrogen present it is also explained that, after prolonged blowing off of the helium, no solid hydrogen was left, for the quantity was so small that it could evaporate in the space which it found at its disposal. It is remarkable that so small a quantity of admixture as the gas contained has been able to give the total phenomenon of a substance condensing to a solid and re-evaporating, though the rapid evaporation is in harmony with the smallness of this quantity of substance, considering that even denser masses were seen to be blown away sometimes. There cannot have been much more than 1 milligram or 15 cubic millimetres of solid hydrogen in round numbers in the tube—probably there was less in it—and yet the tube of nearly 7 cubic centimetres was over its whole length for almost a quarter filled with dense, flaky substance.

So far as the experiments on the expansion of helium are now advanced, they show the curious forms that the solution phenomena of a solid in a gas take in the case of helium and hydrogen. They further point to the possibility of realising with mixtures of hydrogen and helium the rising or falling of the solid substance according to the pressure exerted on the gas, the barotropic phenomenon for a solid and a gas. But the question of condensing helium is to be considered yet as an open one.

Let me add a few words as to the mist observed in the repetition of the expansion experiment with the "coal-pure" gas. It is certain that this gas only contains very small quantities of hydrogen. The spectroscopic test also gives traces only. It is possible that the amount of the traces will prove sufficient to attribute the mist to the traces of hydrogen left in the gas. But it is also possible that the mist has been a liquid cloud, and the changed aspect seemed to point to this. If this might prove to be the case, then the critical point would be nearly what I calculated from the isotherms, and helium would obey tolerably well the laws of van der Waals. The tube broke, and so I could not attain more certainty about the nature of the cloud.

The preceding experiments show very strikingly how careful one has to be in arriving at conclusions from the appearance or non-appearance 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.

April 14. H. KAMERLINGH ONNES.

Satellites of Yellow and Green Lines of Mercury.

Being engaged with the investigation of the Zeeman effect by using a 35-plate Echelon spectroscope constructed by Hilger, I made an experimental test of the resolving power of the instrument on the yellow and green lines of mercury. With a lamp of the Aron type (30 volts, 6 amperes), and by eye observation with a micrometer, I found the following satellites, some of which seem to be new. $\delta\lambda$ is given in Ångström units. The measurements by Janicki with an Echelon spectroscope, and by Baeyer