

The quite independent confirmation of my discovery of the limit of the liquid state given in a letter in NATURE, vol. xxii. p. 435, by my old colleague, Dr. Carnelley, helps to dispel the idea of an intermediate state above the critical point, and confirms me in the use of the term "gas" for all fluids above their critical temperatures in speaking of the "solubility of solids in gases." The term vapour should only be applied to an aeriform fluid which by pressure alone can be reduced to the solid or liquid state, and above the critical temperature this cannot be done. As yet I have no evidence that vapours so defined are capable of dissolving solids, and this negative property may help to form a definition of that division of matter. As Dr. Carnelley does not mention the coincidence of our researches, perhaps you will permit me to quote from our respective papers. Dr. Carnelley says:—1. "In order to convert a gas into a liquid, the temperature must be below a certain point (termed by Andrews the critical temperature of the substance), otherwise no amount of pressure is capable of liquefying the gas."

As far back as May 24 I wrote ("On the State of Fluids at their Critical Temperatures," *Proc. Roy. Soc. No. 205*, 1880):—"The same results were obtained as before. When the temperature was below the critical point, the contents of the tube were liquid, and when over that temperature the reaction was always gaseous, notwithstanding the variations of pressure."

"I think we have in these experiments evidence that the liquid state ceases at the critical temperature, and that pressure will not materially alter the temperature at which the cohesion limit occurs." Dr. Carnelley will find the whole of my paper devoted to an experimental demonstration of what he has now deduced from his experiments. The paper was written with the title, "On the Cohesion Limit," but by the advice of Sir William Thomson, to whose great kindness in helping me with advice and information I am much indebted, I altered the title until I had the whole field explored. This I have since done, and have completely established the "cohesion limit" for all liquids—that for homogeneous liquids being an isotherm starting from the critical point. My paper being a very full one has taken much time and work, and the corrections for over a thousand experiments will take me some time yet. Prof. Stokes (whose kindly interest and encouragement have greatly lightened my labours) has been kept informed of my progress, and is cognisant of the work I have done in this direction. Dr. Carnelley's second conclusion is also very interesting, especially when applied to water; but surely we are not to understand that the solid ice was hot throughout, or that, if a thermometer had been imbedded in the ice, it would have risen. Although the vessel be red hot, the ice need never be allowed to melt, but made to pass directly into vapour, and yet its temperature remain 0° till it has been entirely volatilised.

I notice from your report of the British Association that Sir William Thomson calls attention to Cagniard de Latour's method of showing the critical state of a liquid by sealing the requisite proportion of liquid in a stout tube and heating it in a bath. It should not be forgotten that, although to Dr. Andrews undoubtedly belongs the credit of establishing the definite finish of the boiling-line and the apparent continuity of the liquid and gaseous states, to Baron Cagniard de Latour belongs the discovery of "l'état particulier" where the liquid state ends. Latour's method, although often used by Mr. Hogarth and myself, is not convenient for purposes of research. The method, your report goes on to say, was criticised by Prof. W. Ramsay, in what spirit we are not informed, but Dr. Ramsay added that he had found an apparatus in which a screw was employed to produce increase of pressure instead of using the expansion of the liquid itself. Dr. Ramsay, however, did not say whether the apparatus he had found was that invented by Mr. Hogarth and myself, and described by us in *Proc. Roy. Soc.*, No. 201, 1880, in which india-rubber in a hollow cap is made by compression to yield a perfectly tight joint and to answer also for a screw when protected by a facing of leather. Dr. Ramsay visited my laboratory, and had the apparatus taken down and fitted up before his eyes, and with my permission had an apparatus made. The use of the compressed india-rubber for obtaining the requisite close fitting constitutes the important feature of my apparatus; the employment of iron for constructing the vessel enabling experimenters to dispense with the use of two liquids as in Andrews' apparatus—mercury being used alone.

I have made some little progress with the construction of vessels to withstand pressure at high temperatures, and I expect in a few weeks (when I have prospect of leisure) to carry my

crystallisation experiments to a scientific if not commercial success.

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### Fascination

I EXPECTED some of your readers to refute the explanation of Mr. Stebbing on "Fascination." I see in NATURE, vol. xxii. p. 383 another paragraph which is not more to the purpose. Want of presence of mind and stupefaction are not fascination. In 1859 (twenty-one years ago) I followed in the rocks of Avon, close by the park of Fontainebleau, the fairy paths of Denecourt, when the approach of a storm induced me to leave the blue arrows, indicating the right path, for a short cut. I soon lost my way, and found myself in a maze of brambles and rocks, when I was startled by seeing on my left hand, at a distance of about ten yards, a snake, whose body lifted up from the ground at a height of about a yard, was swinging to and fro. I remained motionless, hesitating whether to advance or to retreat, but soon perceived that the snake did not mind me, but kept on maintaining its swinging motion, and some plaintive shrieks attracted my attention to a greenfinch perched on a branch of a young pine overhanging the snake, with his feathers ruffled, following by a nod of his head on each side of the branch the motions of the snake. He tottered, spread his wings, alighted on a lower branch, and so on until the last branch was reached. I then flung my stick at the snake, but the point of a rock broke it and the snake disappeared with the rapidity of an arrow. On approaching the spot, a real abode of vipers, which I did with the greatest precaution, knowing by observation that death may be the result of the bite of a viper, I saw the greenfinch on the ground agitated by convulsive and spasmodic motion, opening and shutting his eyes. I put him in my bosom to try the effect of heat, and hastened to reach the park of Fontainebleau. The little claws of the bird opening and shutting, perhaps as an effect of heat, made me think that he might perhaps be able to stand on my finger, and he did clutch it, and held on with spasmodic squeezes. In the park I got some water, and made him drink it. In short, he revived and finally flew off in the lime-trees of the park.

Now whilst following the motions of the snake and bird I experienced a singular sensation. I felt giddy; a squeezing like an iron hoop pressed in my temples, and the ground seemed to me to be heaving up and down. In fact the sensation was quite analogous to that experienced on a beginning of sea-sickness.

From these facts would it not seem probable that fascination is nothing more nor less than an extreme fatigue of the optic nerve, produced by a rapid gyratory motion of a shining object and resulting in a nervous attack and a coma? Curiosity rivets at first the attention of the bird, unconscious of any danger, and when giddiness warns him of his peril it is too late. The snake is as well aware of this as the *Lophius piscatorius* is of the effect of his membrane.

In this system the fact of the bird coming down from a higher to a lower branch would be explained by the supposition that, giddiness overtaking him, he opened instinctively his wings and clung to the next support that he found, the motion having partially removed the giddiness so as to enable him to hold fast.

Observe, that nothing hindered the bird from flying away, and that the snake being at most five feet long, could never have reached even the lowest branch.

Besides he could have no nest to protect, for in the rocks of Avon there is no water save rain-water in the hollows of the rocks, and this is not potable on account of microscopic leeches which people it, the instinct of birds teaching them to avoid it.

Jersey, August 29

CHATILL

P.S.—I inquired of Mr. Denecourt, "the sylvan of the forest," if he were aware of the existence of such large snakes in the forest, and he told me that he had only seen, in the "rocher Cuvier Chatillon," a snake about four and a half feet long, which he killed, but that even larger snakes had been seen in this very "rocher d'Avon" and in the "rocher St. Germain," but he thought that they were only "couleuvres" of a large size and quite inoffensive.

### Meteor

ON the 19th inst., at 11.34 p.m. (within a minute of G.M.T.), I observed a large meteor in the east, towards which I happened to be looking, the sky being quite free from clouds, and clear.