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L. On the motion of vapours toward the cold

Charles Tomlinson

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one who has so eminently distinguished himself in original investigation should have chosen to superadd to his functions as a discoverer those of a severe and hostile critic upon the labours of those men who have worked at the same subject with himself, and by all of whom he has been treated with the utmost possible consideration.

I remain,
My dear Sir David Brewster,
Yours very truly,
B. STEWART.

5 Alva Street, Edinburgh,
April 10, 1863.

L. *On the Motion of Vapours toward the Cold.* *By* CHARLES TOMLINSON, *Lecturer on Physical Science, King's College School, London.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE just been arranging for repetition Dr. Woods's ingenious and beautiful experiment on the motion of vapours toward the cold, as described by him in the current Number of your Magazine.

While engaged last summer in investigating the law of the motions of camphor, &c. toward the light, I performed a number of experiments in the same direction as that by Dr. Woods. They were not described in my paper inserted in your Magazine for November last, because in it, as at the British Association Meeting in October, I wished to be as concise as possible; but you will remember that in my published paper I state (p. 361) that "I omit many subsidiary facts observed during the inquiry." The following are a few of them.

On the 21st of May, 1862, a solution of iodine in bisulphide of carbon was put into a stoppled eight-ounce phial in quantity sufficient to form a ring at the bottom and to surround a central convex island. The bottle was placed in a west window. The day was cloudy and wet. In the course of an hour a violet dew was formed a little above the solution. In another half hour (*i. e.* at noon) the dew became more copious, and extended over nearly the whole surface of the bottle, but was densest on the side nearest the light. In another half hour (12.30 P.M.) the beads of dew were smaller and paler. At 1 P.M. they had become almost colourless, and solid iodine had condensed on the convex island at the bottom of the phial. At 3 and also at 4 P.M. there was a gleam of sunshine, when the bottle became filled with a yellowish vapour and the dew again became coloured, though faintly. At 7 P.M. the dew formed a violet arc on the side nearest the window.

On the morning of the 22nd the arc was pale, and there was a dense deposit of thick purple matter on the furthest side just above the liquid ring. At 5 P.M., the sun being on the window, the dark-coloured deposit was on the nearest side, but equally low down. These are effects of capillarity evidently similar to those so well described by Dr. Woods; but I paid no particular attention to them at the time, as my mind was occupied with other phenomena.

As the season advanced and became more settled, the phenomena varied. A portion of the bisulphide was decomposed, and another portion forced its way out of the bottle. During these events the motions of the vapour went on with the regularity of a clock. When the sun was on the window, a current apparently rose on the nearest side and descended on the furthest, where the deposit was made. When the sun was off the window, the current set in in the opposite direction; and the nearest side being the colder, the deposit was transferred to it. These motions were marked in an interesting manner at the sides of the bottle; at first by well-shaped parabolic spaces, marking the temperate regions between the hot and the cold ones; afterwards by lines as straight as if ruled, diverging above and below from a centre placed a little above the liquid. The lines above this centre were several inches in length; those below it about a third of an inch. These lines, lastly, changed into a minute sea-weed pattern which remained fixed for some weeks; but I find by my note-book that the bottle had become dry early in June.

A similar experiment was also tried with phosphorus in bisulphide of carbon, &c.

I also performed a number of experiments on the motion of the vapour of water towards the light. During many years I had often noticed, when preparing gases for the purpose of a lecture, that the moisture condensed on the sides of the gas-jars and bottles was of a different pattern with different gases. I speculated a good deal as to why this should be so, but now determined to bring the fact under the ordeal of exact experiment. Accordingly in May last I filled six quart stoppled bottles, No. 1 with oxygen, No. 2 with nitrogen, No. 3 with hydrogen, No. 4 with nitric oxide, No. 5 with carbonic oxide, and No. 6 with carbonic acid. The gases were collected over water at the pneumatic trough, and about half an ounce of water was left in each bottle, which was tied over with soft leather wetted in a solution of gum, which was also run round the stopple. The bottles were placed on a table near a west window, and a journal was kept of their proceedings, which were recorded twice a day, together with the temperature and the state of the weather. The dew was of the most varied character in the different bottles: in some it was fine, in others coarse; and the deposit was also frequently ploughed into furrows by the weeping tears which trickled down as the moisture on the inner side of the shoulder of the bottle became overcharged. It was beautiful to see the almost clocklike regularity with which the deposits oscillated between the front and back of the bottles. At 9 A.M., when the first observation was generally made, the deposits were on the side nearest the window, often occupying exactly one-half of the bottle; at 3, 4, or 5 P.M., when the second observation was taken and the bottles had been exposed to the sun some time, the deposit was on the furthest side. When the afternoon was cloudless, the bottles were filled with vapour; but as the evening advanced, the deposits were made on the furthest side, again to pass over to the nearest side as the cold of night was felt.

With respect, however, to the texture of the dew, the results were anomalous. After several weeks' observation I was not in a condition to place my finger on any one bottle and name its contents from the character of the aqueous deposit—except perhaps in the case of carbonic acid, which declared itself by its superior solubility. Suspecting that the bottles, although new from the glass-warehouse, were not clean, I washed out other similar bottles with strong sulphuric acid, rinsed them with clean water, and filled No. I. with air, No. II. with hydrogen, and No. III. with carbonic acid. On placing these near the window, the results were certainly such as to justify the conclusion that the deposits of moisture are similar in various gases if the receiving surface be chemically clean. In such case, however, we get, not dew, but a sheet of water marked by weeping tears, but without furrows; and it is often difficult to distinguish on which side the deposit is made, so regular is the watery film. There can be no doubt, however, as to its being deposited on the colder side.

But if vapour of water in different media presents similar phenomena, different vapours in the same medium give varied phenomena. I have arranged in the window, as nearly as possible under similar circumstances, clean 8-ounce phials containing small quantities of alcohol, ether, benzole, belmontine, bisulphide of carbon, &c. The deposits varied in texture according to some law depending partly on the varying cohesive force of the liquids in question, by which they form drops of different sizes from the same aperture, and ascend to different heights in capillary tubes of the same bore.

I may here be allowed to refer to one of Dr. Draper's experiments, in which the inside of a glass bottle being marked with a blunt point, such as that of a glass rod, the camphor vapour is afterwards deposited along the marks. A fact of this kind was pointed out more than a century ago by De Mairan*, who noticed that the hoar-frost was deposited on his window-panes in spiral lines, produced, as he supposed, by the fine sand or ashes used in cleaning the windows; and although these lines were not visible to the eye, they nevertheless formed ridges sufficiently prominent to catch the condensing vapour. To test this idea, Carena†, in the severe winter of 1814, cleaned four panes of his window with fine sand, rubbing two of them with a circular movement, a third in straight lines from top to bottom, and a fourth in diagonal lines. On the next day and on several succeeding days the hoar-frost was deposited more or less on the lines or furrows produced by the friction.

In the older memoirs and treatises on dew, the writers are very fond of ornamenting their work with pleasing figures of dew and hoar-frost as it appears on various objects; and they attribute the differences in the patterns of the watery particles to differences in the radiating powers of the objects in question. It does not seem to have been suspected that the varied patterns also depend on the want of adhesion to the surface. When this is chemically clean we get, not dew, but a sheet of water. All objects exposed to the air contract an organic film; and when the inner surface of a bottle, for example, as in Dr. Draper's experiment, is rubbed with a blunt point, the film is raised into ridges which catch the camphor vapour more readily than the other parts of the surface‡. Indeed so sensitive is this vapour in detecting prominences, that it will attach itself to, and render visible minute objects on the inner surface of the bottle which before were quite invisible. For example, I have many times washed out a bottle and wiped it dry with a clean cloth. The bottle appeared to be perfectly clean to the eye; yet after a few minutes' exposure to the sun, with its charge of crude camphor, innumerable filaments, evidently derived from the duster, would start into view from being coated with a very thin layer of camphor.

In a small volume recently published, entitled "Experimental Essays," I give some specimens of camphor-figures modified by the presence of vapour of benzole, wood-spirit, naphtha, chloroform, nitric acid, &c. One of the actions of these liquids is apparently to modify the rate of evaporation of the camphor.

* *Dissertation sur la Glace.* Paris, 1749.

† *Mémoires de l'Académie Royale des Sciences de Turin pour les années* 1813–14.

‡ Most of the phenomena of breath-figures (the *figures roriques* of the French, and the *Hauch-Figuren* of the Germans) depend on the existence of this film.

Equal quantities of camphor were placed in glass capsules before a window, and were wetted with the same number of drops from the end of a glass rod. The liquids were water, benzole, ether, chloroform, bisulphide of carbon, and alcohol. A similar weight of camphor was left untouched for comparison. The camphor disappeared by evaporation at very different rates; but the experiment requires to be repeated with more care before numerical results can be given. It illustrates probably only a case of adhesion. The liquids partly dissolve, spread out the camphor, and attach it with more or less force to the capsules. I have also observed that where camphor has been touched with bisulphide of carbon, &c. and then exposed to the light in stoppled bottles, a creeping kind of vegetation is formed; and unless the temperature be higher than that of the air in the shade, the deposits are very different from those obtained from camphor not embarrassed by the presence of another vapour.

The whole subject of the action of vapours on camphor is a curious one and might repay further inquiry; but as far as my notes at present extend I should, by quoting them, merely multiply facts without leading to any useful principle. I fear that this communication requires an apology on that ground.

I remain, Gentlemen,

Your obedient Servant,

C. TOMLINSON.

King's College, London,
April 2, 1863.

LI. *On the Absorption of Gases by Charcoal.* *By* JOHN HUNTER, *Scholar, Queen's College, Belfast**.

I AM induced to publish rather sooner than I intended the following preliminary results of a series of experiments with which I have been engaged for some time on the absorption of gases by charcoal, in consequence of observing that a paper on the same subject has lately been read before the Royal Society.

In this inquiry I have followed nearly the same method which was employed by Th. de Saussure in his well-known memoirs on the absorption of gases by charcoal and other bodies. The gases, carefully dried, were collected over mercury, and after their volume had been observed the charcoal was introduced. It was heated to redness before each experiment, and while in a state of incandescence immersed under mercury. My first object was to ascertain the absorbing power of different kinds of charcoal; and

* Communicated by the Author.