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# EXPERIMENTAL ESSAYS.

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I.—ON THE MOTIONS OF CAMPHOR ON WATER.

II.—ON THE MOTION OF CAMPHOR TOWARDS THE LIGHT.

III.—HISTORY OF THE MODERN THEORY OF DEW.



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# EXPERIMENTAL ESSAYS.

## INTRODUCTION.

If I were allowed to advise any one desirous of occupying his leisure in the study of nature, I should say—"Take one single natural object; find out all that is known respecting it, and then try and find out something new."

Any one who entered heartily on such a task, could hardly fail of being rewarded. To find out all that is known respecting the commonest object, would lead the explorer through many pleasant paths of knowledge. Of course, the choice of the object must be regulated by the student's taste and means. Objects like air or water would demand a good deal of apparatus, and even a special training for their study. A plant, a crystal, or a fossil, would require but a few books and specimens, and only such apparatus as could be cheaply and conveniently got together in a private house.

In proceeding to ascertain what is known respecting the object in question, a Cyclopædia will inform us of its chief properties. If the article has been written in a business-like manner, it will be sufficiently full to instruct the general reader, and sufficiently suggestive for the student; that is, it will tell the one all he wants to know, and the other, where he may get further information. In this respect, a Cyclopædia is most useful; it should rather be a finger-post to point out the way, than the way itself; it should place the mere visitor on a prominence from whence the prospect may be seen, but it should furnish the traveller who wishes to explore with the necessary guides.

The authorities given in the Cyclopædia may be some special work, such as a treatise on Chemistry or Physics, or they may consist of Memoirs in the Transactions of learned Societies. These should be carefully consulted, especially the latter. It is customary to slight old Memoirs, on the ground that the truths they tell have been absorbed into the body of Science; while it is useless, if not dangerous, to read the errors which often belong to them. Doubtless, a man should be fortified by scientific truth, before he voluntarily places himself in contact with scientific error; but supposing him to have a moderate acquaintance with the principles of modern Science, he will find old Memoirs to be delightful and instructive reading. They bring one into contact with the minds of the distinguished men of other times; they are often models of style, and point out modes of research which excite admiration. They have often, moreover, the great merits of earnestness and conciseness; they say what they have to say with simplicity and truth. In this respect they contrast strongly with modern Memoirs. Owing to the amazing growth of Science, we have of late years rather gained in minuteness what we have lost in breadth; facts have multiplied in a higher ratio than laws; details have become so vast, that it is more than ever difficult to master a Science. Hence, also, it is more difficult to make a discovery, and when made, to describe it concisely.

In addition to the pleasure of reading old Memoirs, there is yet another reason why they should be consulted. It does not always happen that their most valuable points have been correctly presented. A subsequent writer, who is attentive to original authorities, very properly takes from them what he wants, and no more. A less careful writer, who is content with authorities at second-hand, alters and abridges the abstract made by another hand, and in this way the original is often not only not fairly represented, but may even be misrepresented. Besides this, the old writer often treats, in a most interesting manner, of incidental points belonging to his main argument; and while that argument is made the subject of scientific teaching, the incidental matter may remain buried and neglected in the volume of Transactions to which the Memoir belongs.

I have often heard complaints as to the difficulty of finding authorities on any particular subject. It certainly is difficult, but not impossible. Some learned Societies publish collected indexes of their Transactions from time to time. Scientific Journals do the same;* but where these fail, we may often get a clue to Memoirs by the references made by one writer to the labours of his predecessors. This is often done in so loose a manner, that only the names of his fellow labourers are mentioned. By referring to such names in a biographical dictionary, we may often get the references we want, together with other particulars, which greatly enhance the interest of the particular memoir.

Of course, no one would undertake such a task as the above unless the love of his subject inspired a sufficient interest in his work. *Omnia vincit amor*, is true in science as in morals. Science must be cultivated for her own sake: fame may or may not attend the cultivation; profit seldom or never. This country has but few rewards to bestow on her men of science; such as exist are strenuously competed for,

* The Royal Society has two or three indexes of its Transactions, but not to any very recent date. The French Academy of Sciences has also an index to a great part at least of its *Histoire* and *Mémoires*. There is also a collected index of the contents of the first thirty-one volumes of the *Comptes Rendus*. The *Philosophical Magazine* has also separate indexes. Scientific students are looking forward with anxiety to the long-promised indexes of English and foreign scientific Journals by the Royal Society; and although these do not go farther back than the year 1800, and are under the names of authors instead of subjects, they will be of great use. Indeed, for want of such indexes, not only are men liable to waste time in repeating what has already been accomplished, but an injustice is done to the fame of older scientific writers. One remedy, however, in the absence of these collected indexes, remains, and that is, to consult the index to each volume for a series of years. This is not so formidable a task as it looks.

and the claims of every competitor are closely scrutinised. It is best, therefore, for those of scientific tastes to whom Science is not a profession, and who have a vocation as a means of livelihood, to look for their reward in the intellectual, and even moral, advantages which attend their avocation.

Special treatises have been written on these advantages, and to them I refer. My present object is to show, by example, how a scientific taste may be gratified in an inexpensive manner in one's own house, without any very costly apparatus. I may, however, be permitted to say a few words as to the mode of inquiry here recommended.

A library such as that of the British Museum will supply the means for ascertaining the history of the particular object of research. As the materials accumulate, the student will probably be surprised that so much is known respecting it. He may also have occasion to notice the curious way in which knowledge advances, first by indications of broad facts, and then by filling up the connecting details; just as in the first view of an undulating country the hill-tops are prominent, and conceal from view the valleys which must be laboriously traversed before the real connection between the summits can be known. So in science broad facts may be repeated from book to book during a series of years, in which no advance is apparently made. Ten or twenty years or even more, may thus be unproductive, as far as the object of inquiry is concerned: then a new crop of properties is added to its description. Discussion arises, in the midst of which old properties are re-stated, or differently explained, and numerical results corrected. The object may then be again left to slumber; unless, indeed, it be connected with some larger inquiry, which is exciting general attention.

Having ascertained all that is known respecting the object in question, and having repeated all the practicable experiments, we feel a sort of affection for it, and a lively interest in it. It occupies so many of our thoughts, that we soon begin to try experiments of our own, and after many failures discover new properties respecting it. Our interest in it is then increased a hundredfold.

It is not until we have proceeded some way on the path of original inquiry that we become impressed with the truth that, however much is known respecting the object of our pursuit, quite as much remains to be discovered. It is this that gives so great a charm to inquiries into nature; they bring the finite mind into contact with the infinite; they reward the labour by satisfying the intellect.

I have elsewhere had occasion to contrast the study of a natural object with that of an artificial one. An artificial object may, indeed, excite our delight and admiration, but we soon comprehend all that it was intended to teach. It was designed and executed by the wit and skill of one man, and may be understood by another. It partakes of many, or at least some, of his imperfections: it may represent the existing state of knowledge, which, it may be, halts because the instruments of observation are imperfect; it admits, in fact, of improvement, and will probably be improved by advancing Science—unless, indeed, it be set aside altogether by a more perfect instrument constructed on a different type.

Such remarks are altogether out of place when applied to a natural object. We shall never learn all that it can teach. Its place in creation was assigned to it by Supreme Wisdom, and as Science approaches nearer to that wisdom, new properties will be discovered in it; it will appear to us more in harmony with surrounding objects; it will never be superseded or set aside, but will more eloquently admonish us of the laws of its being. Göthe has a thought on this subject which deserves attention:—

> "Wouldst thou the highest, the greatest attain? The plant may instruct thee:
> What it unwittingly is, wittingly strive thou to be." *

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* "*Suchst du das Höchste, das Grösste? Die Pflanze kann es dich lehren:*
*Was sie wissenlos ist, sey du es wollend,—das ist's.*"

Of course, for "plant" we may substitute any other natural object, and the elegiac verse will remain equally true.

In illustrating by example the practice here recommended, the object selected for examination is a piece of *camphor*. The choice of this substance was determined by the curiosity two of its best known properties had excited within me almost from a boy. Why do small fragments of camphor spin rapidly on the surface of clean water? Why do crystals of camphor in the druggists' windows move towards the light? These two questions (the answer to which will be found in the following pages) frequently recurred. The duties of my profession have prevented me from attempting to answer them during my life's prime. I am thankful that comparative leisure has been granted to me for original research in my life's decline.

In treating these subjects I have described a large number of original experiments, and, where necessary, the precautions required to conduct them successfully. Most of them may be performed by young persons without danger; and I should be indeed gratified to know that this little book had found its way into their hands. Nature speaks so lovingly to the young, and they respond so willingly, that we may well take advantage of this circumstance to gratify a taste which encourages the order, method, and exactness of Science.

These papers then are intended to illustrate a form of conducting scientific inquiry cheaply; and in order to carry out my own principle of economy I have arranged with my Publisher to sell this little volume for one shilling.

*King's College, London.*
*Michaelmas Vacation*, 1862.

# ESSAY I.

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## ON THE MOTIONS OF CAMPHOR ON THE SURFACE OF WATER.

1. Every one knows that when camphor in small fragments is thrown upon the surface of clean water it moves about and rotates with considerable rapidity. This fact has long been one of the waifs of science. It has often been picked up and examined by good observers, and as often thrown aside; and as there is no scientific lord of the manor entitled to waif, it still remains unclaimed, and wanders about the regions of knowledge as a scientific outlaw. The following is one of the many attempts to reverse the outlawry, and to get this curious and suggestive phenomenon restored to good society. My plan will embrace both history and inquiry. I shall not only write the history of this fact from its birth, but shall endeavour to verify every incident in its career. I shall also have to relate new facts, and to give new views respecting old ones; the final result being, according to my view, to settle a question which has disturbed science during a century and a half.

### SECTION I.—HISTORY OF THE SUBJECT.

2. About the year 1686 Dr. Antonio de Heide,* a Dutch physician, was observing under the microscope the behaviour of camphor while undergoing solution. It appears from his description that when a small lump of camphor is placed

* *Centuria Observationum Medicorum*: Amsterdam, 1686. *Observatio* LVII is entitled, "*Solutio Camphoræ diversis menstruis instituta et microscopio observata.*"

in oil of olives, it emits particles which move about in the oil and return to the piece which had just before apparently repelled them. This circuit is kept up until the whole of the camphor is dissolved, or the oil is saturated. It was inferred that the camphor escapes from the oil by evaporation in the course of twenty-four hours, since after that time the oil loses its acrid taste.

3. Dr. Heide also found that camphor dissolves in aqua-fortis, with the emission of particles, and an action so violent as to produce a kind of vortex. It was also noticed that the camphor dissolved in one portion of the aqua-fortis remains distinct from the other portion. Similar effects were noticed when camphor was placed in aqua-regia (consisting of aqua-fortis and sal-ammoniac), also in oil of sulphur (*oleo sulphuris*); but it is stated that in spirits of nitre, spirits of salt, in oil of vitriol, and in rain water, camphor is not soluble. Camphor was found to dissolve more freely in turpentine than in oil of olives, and the beautiful, feathery crystals of camphor were noticed when the turpentine had evaporated.

4. The experiments were made on pieces of flat glass, and the description is illustrated by two rude but graphic woodcuts.

5. So far Dr. Heide. It may be remarked that the notion that camphor is not soluble in water was held by several subsequent observers; and that in trying the experiment the motions of camphor on the surface of water escaped the notice of Dr. Heide, doubtless because his apparatus was not quite clean.

6. This observation was reserved for Romieu, after an interval of sixty-two years. The fact appears to have been communicated to the Royal Society of Montpellier in 1748, but it was not published until 1762, which is the date of the *Histoire de l'Academie Royale des Sciences* of Paris for 1756, in which year M. Romieu's paper appears to have been sent from Montpellier to Paris. It is entitled, " On a new chemical vegetation made with camphor, and on some properties of this substance."* After devoting six pages and some beautiful copper-plate engravings to the first part of his inquiry, he

* *Sur une nouvelle végétation chymique faite avec le Camphre, et sur quelques propriétés de cette substance.*

relates "another very singular property belonging to this substance." If camphor in small pieces about a line in diameter or less be thrown on the surface of pure water contained in a glass, they will keep up a continual agitation until they are dissolved, which may be in about half an hour in summer, provided the pieces be not more than one-third or one-fourth of a line in diameter. There should not be more than twelve or fifteen bits of camphor on the surface, for if it be entirely covered with the fragments there is no motion. If a fragment be four or five lines in diameter and upwards there is no motion unless it be set on fire, but in that case the motion ceases when the flame is extinguished. The most rapid motion pertains to the smallest fragments, or spiculæ, which often move in spirals; agitation of the water, and also warmth, favour these motions. The motions of the fragments are said to resemble those of attraction and repulsion. When examined with a lens, a slight depression is seen on the water under each fragment, such as would be produced by the escape of a current of air from the camphor.

7. The above observations may be endorsed as tolerably accurate; except, perhaps, that one which denies motion to a fragment of camphor four or five lines and upwards in diameter, except when set on fire. This is a mistake. But a far more serious mistake is the electrical theory of the author, by which he sought to account for these motions. He states that camphor does not rotate on the surface of water contained in vessels of iron or copper, but that the experiment succeeds very well in vessels of glass, sulphur, or resin; that while the camphor is spinning in these vessels, it is arrested if the surface of the water be touched with the finger, with iron wire, or brass, or with a rod of wood; but that the contact of glass, sealing-wax, or sulphur, has no effect in arresting the motions. Hence the motions of camphor on the surface of water is declared to be an electrical phenomenon.

8. Now Romieu does not seem to have been aware that if, while the camphor fragments are performing their wild dance, the surface of the water be touched with a minute globule of oil, fixed or essential, such as can be taken up on a pin's point, the motion is generally arrested, the particles becoming suddenly paralysed (23). Moreover, if the glass be at all greasy, or the water not clean, the camphor will not rotate. Hence

there can be no doubt that Romieu's metallic vessels were not clean, any more than the iron and brass wire, and rod of wood; while the act of dipping a finger in the water is sufficient to impart an organic film to the surface, and thus prevent that adhesion between the camphor and the water which, as we shall afterwards show, is necessary to the success of the experiment.

9. But it may be said that Romieu's experiment succeeded in vessels of glass, resin, and sulphur, and that touching the water with rods of those substances did not make it fail. We may answer that glass, resin, and sulphur, are more easily cleaned, and are more likely to be clean than metal. A metal cup or rod is constantly handled without undergoing the daily cleansings that are bestowed on glass, while the fragile nature of vessels of resin and sulphur would cause them to be handled with care, and hence they are more likely to be clean than articles of metal.

10. But some allowance must be made for the misguiding influence of a foregone conclusion. The world, at the time of Romieu's experiment, was in a state of great expectation on the subject of electricity; its identity with lightning was more than suspected, and it was just about to be proved both in France and America. Men were accustomed to refer every strange phenomenon to electricity, and what seemed to be otherwise unaccountable was thus thought to be sufficiently explained.

11. But the best answer to Romieu's electrical theory is that camphor rotates perfectly well (as I have repeatedly proved) in vessels of metal, and that its motions are not arrested by touching the water with a good conductor— always provided the metallic vessels, rods, &c., be chemically clean. I have connected the metal vessels with the ground, but still the camphor rotated; and have touched the water with rods of glass and of metal, and still the camphor rotated; but if the rod be left in the water, the camphor will be attracted by it, attach itself to it, and so be brought to rest.

12. We do not hear any more on the subject of the motions of camphor until the year 1785, when one Kosegarten read before the University of Göttingen one of those learned Latin theses which formed the delight of old academies. It is entitled " Concerning Camphor, and the Parts

of which it consists." * It was published at Göttingen in the year last mentioned, and was included by Dr. Frank in his collection of theses.†

13. In this work the author collects together all that was known or conjectured on the subject of camphor. He repeats the notion that camphor is not soluble in water, nor in alkaline liquors (p. 33), but admits (p. 70) that if camphor be digested in water, the latter retains its odour. This thesis, however, derives its chief value from a letter on the motions of camphor, addressed to the author at his request by the celebrated Lichtenberg, so well known to the electrician for his figures, and to the literary man for his "Explanation of Hogarth's Engravings," ‡ said to be the best exposition of our great satirist's works. The reprint of this thesis by Frank has also an especial value on account of a Latin letter by no less a man than Volta (19), relating a series of experiments and observations on the motions of camphor.

14. But first in the order of date is Lichtenberg's letter, the German of which reads oddly in the midst of the Latin, and no less so from the running Italian translation which Frank has appended to it. This shows how little German was read in Italy at the time. The letter is dated Göttingen, 6th May, 1785. It begins by exposing the erroneous electrical theory of Romieu, and substituting another, which is almost as objectionable, viz., that a high temperature, if not exactly the cause of the phenomenon, is necessary to its success. He expresses some surprise that, on introducing the bulb of a thermometer into the water, the motions of the camphor should have ceased, *although* the temperature indicated was 130° F. He suggests that the introduction of the large bulb of a thermometer may have altered the surface of the water in some way; or what is more to the purpose, he suggests that the bulb may not have been quite clean, and so have communicated to the water a slight film.

15. The probable cause of these motions he thinks to be this:—"The particles of camphor attract each other, after the manner of all light bodies floating on the surface of

* *De Camphora et partibus, quæ eam constituunt.*

† *Delectus Opusculorum Medicorum: Ticini*, 1787. Kosegarten's thesis is in the third volume, in which it occupies upwards of a hundred pages.

‡ *Erklärung der Hogarthischen Kupferstiche.*

water; but they cannot remain collected together, since they are constantly changing their figure and mass by means of evaporation. A system of bodies which attract each other until they come to rest, cannot easily rest so long as these bodies are undergoing a change of figure, mass, and volume. If two pointed bits of camphor are in contact by their points, and these points evaporate, the fragments will be still more widely separated by the attraction of other fragments, and so on. Suppose small pieces of paper, thrown on the surface of water, to be collected together and at rest; if it were possible gradually to change their forms, some into circles, others into parallelograms, others again into various kinds of triangles, motions similar to those of the camphor on the surface of warm water would be observed. Camphor is indeed such a body as we have imagined in the case of paper. It is not to be supposed that evaporation alone is sufficient to impart motion to the fragments of camphor, although this is certainly the cause of the motion of a single piece of camphor on water. But enough of this. You will see that this explanation is more natural than an electrical one."*

16. It is surprising that so clever a man as Lichtenberg should not have been struck by the fact, that if his theory were worth anything, it should explain the motions of a single fragment of camphor, as well as of many fragments, on the surface of water. But he has one theory for a single fragment, and another theory for many fragments. That attraction has something to do with the phenomenon must be admitted; but this will be discussed further on.

17. Before introducing his Italian translation of Lichtenberg's letter, Dr. Frank inserts a note in Latin, in which he states that the celebrated Volta performed in his presence some experiments, which proved that electricity had nothing

* Lichtenberg says that he might speak of other motions in liquids which could more plausibly be referred to electricity, and yet have nothing to do with it. "They more nearly resemble animal motions, and although they have not converted me to the theory of Buffon, yet the opinion of that illustrious man is not so ridiculous as many persons suppose." This refers to Romieu's remark, that the motions of *infusoria* in water, described by Buffon, were merely the motions of vegetable particles acted on in the same manner as the fragments of camphor.

to do with the motions of camphor on water. His opinion was, that these motions are due to an effluvium which escapes from the camphor explosively, after the manner of a fire-work, and thus, like it, produces rotation. The motions are less remarkable in cold than in hot water, because the latter causes the elastic fluid of the camphor to escape more abundantly. At the same time Volta states that camphor is not soluble in water:—"*Aqua, cujus in camphoram solvendam nulla est potentia.*"

18. This theory of Volta, as given in the words of Dr. Frank, is referred to by M. Dutrochet.* Apparently conscious that any theory from so distinguished an observer as the founder of Voltaic electricity must be received with respect, he infers that Dr. Frank has not reported Volta correctly, or if correctly, that Volta himself did not think his own views sufficiently important to commit them to writing; for he says, "It will be remembered that Volta has not written anything himself on this subject; we know his opinions only from the report of Frank and of Brugnatelli."† And yet, had M. Dutrochet only turned over the pages of Dr. Frank's volume, and looked to the end of Kosegarten's thesis, he would have found the following note, introducing a Latin letter of five closely-printed pages, from the pen of Volta himself:—"At the end of this dissertation, after this edition had been in type, the illustrious Alexander de Volta, at my request, communicated to me an account of his experiments, which my readers will receive with pleasure, corroborating as they do that highly intellectual man's opinion, already given, as to the phenomena in question."‡

19. Volta's letter, which seems to have escaped the notice of his biographers, is sufficiently interesting to be inserted in this place. He begins by referring to his explanation of the motions of camphor as given by Dr. Frank.

* *Recherches Physiques sur la Force Epipolique.* Part I. Paris: 1842.

† "*On remarquera que Volta n' a rien écrit lui-même sur ce sujet; on ne connait ses opinions que par le recit de Frank et de Brugnatelli.*"—Page 6.

‡ "*Sub finem dissertationis, postquam typis hæc editio iam fuisset, illustris Alexander de Volta sequentia mecum communicavit experimenta, quæ lectoribus grata esse, et indicatam supra viri hujus perspicacissimi sententiam de camphoræ miro illo phenomeno, corroborare potuerint.*"—Page 127.

"The above explanation," he says, "is wonderfully supported by the discoveries I have lately made with the assistance of Dr. Brugnatelli, who in all these experiments has acted with me. Not only small fragments of camphor, but also pieces of the thickness of the thumb and upwards, are carried about by a certain impulse, and revolve circularly when placed on the surface of water, and set on fire; for it is very evident that all this takes place by the eruption of a wind or torrent of vapour. The reason is, that the powerful body of effluvia, which forms the flames, drives backwards the mass which lies in the most mobile state, and is capable of being turned, as I have noticed in the case of *pyroboli*. But I was unwilling to conclude my experiments here, and therefore proceeded with the inquiry; for scarcely had I made up my mind as to the nature of the phenomena, when a suspicion arose, which soon grew into a certainty, which I was bold enough to predict to you and to Brugnatelli, namely, that flowers of benzoin, having about the same volatility as camphor, would display similar effects; and also that similar effects would be produced by volatile concrete alkali, at least so far as its extreme solubility in water permitted. When I came to submit this opinion to the test of experiment, my expectation was realised, and I saw, not without pleasure, the glittering, feathery little laminæ of flowers of benzoin, when thrown on the surface of water, divide, repel each other, and rotate even more perfectly than fragments of camphor. Indeed, this rushing to and fro, and the rotatory motion, are more marked than in the case of camphor, although the phenomena do not last nearly so long. This rotatory motion becomes more and more rapid, and in the case of the more minute particles incredibly rapid, as was noticed with camphor. This experiment with gum benzoin was first made at my suggestion by Brugnatelli, and soon afterwards by myself, assisted by him. At length, after a second and third repetition, I showed it to you, who viewed it with friendly approval. In the case of volatile concrete alkali, we found that a particle of it put into water not only rotated on its own axis, but also moved up and down with a sub-saltatory kind of motion (*subsultus*), so long as the fragments remained suspended in the water, and not dissolved; but they soon disappeared by solution, or sank to the bottom. Hence it was only during some brief seconds that it was possible to watch these remarkable rotations—remarkable, I say, from their extreme rapidity, even beyond conception, from their first contact with the water. This is conformable to the nature of the alkali, seeing that it is more volatile than camphor and benzoin under similar circumstances.

"While lately engaged in these experiments in a pharmaceutical laboratory (*officina pharmaceutica*), there stood at my side a young apothecary, who, seeing the success of my trials with gum benzoin, suggested to me the trial of salt of amber, on the ground that it is of an acid natnre, concrete, and volatile, as in the case of benzoin. Adopting the suggestion, we were gratified with a novel and most beautiful sight; for the particles of the salt, when sprinkled on the water, had scarcely touched it when the surface became variegated with widely-extending circles, displaying the colours of the rainbow in the brightest hues, the whole surface being covered with a variegated coating of many colours. Each circle does not, however, retain the same colours, but gradually changes as it widens, doubtless in the same manner as the soap bubble, which shows a succession of various colours while being blown, from its becoming thinner and thinner. This phenomenon of an effluvium given off by salt of amber spreading over the surface of the water with a display of various colours (it is, I assure you, a pretty sight), while accompanying the multiform wanderings and rotations of the particles of this salt as they float on the water, makes manifest to the eye the cause of those same motions, namely, that the small masses are repelled or driven back from that point where the substance I have spoken of is poured forth most abundantly, covering the water and suffusing it with colours; and this efflux, so to speak, is visible to the eye. When a fresh portion of the salt is thrown on the water its whole surface is well-nigh disturbed, and the coloured rings previously formed are broken up. Who then can doubt that this is the result of the effluvium spreading itself in circles? We see also a number of small masses of salt rotating on their axes, some hurrying to and fro, and dragging behind them a long broad tail (reminding one of the drawings of tailed comets), an effect which evidently proceeds from the same torrent of effluvia that happens to be projected to a greater distance, which torrent is left behind by the mass of salt being repelled from that spot backwards. But how is it that a tail of this kind becomes twisted into a spiral, whilst the body from which the tail is projected rotates on its axis? It may be, that from this very circumstance a greater reaction is produced, namely, that it is against the water itself that the matter given off by the salt strikes, which is different from what happens with the effluvium of camphor, for this only strikes against the air. But what is there to prevent our admitting at least a partial impact of these effluvia also against the surface of the water, which in some manner they invade? But it may be urged that they leave nothing on it. Nothing, it may be, that does not evaporate quickly; nothing visible: but why not something invisible? This surmise will not appear altogether improbable if we weigh all the circumstances, and remark also that when some fragments of camphor have been left for some time on the water, they no longer rotate, and that fresh fragments thrown on the same water remain quite at rest. Why then may we not say, when the motion of the camphor becomes languid and ceases, that the water is impregnated or covered with the adhering exhalations of the camphor, although it be not visible? If this be admitted, the same explanation will apply to the motion of the particles of salt of amber, benzoin, camphor, and volatile alkali; if not, then the salt of amber and the benzoin act in a manner peculiar to themselves. Still, however, our opinion remains unassailed, the one point for which we contend being this—that the immediate cause of the rotations of the fragments of these volatile bodies on the surface of water is a copious and strong reflux of vapour from each fragment, or to speak more accurately, the reaction of the surrounding fluid, whether it be air or water, against this torrent produces the motion.

"A number of other substances, besides those named, were tried by us. A bystander suggested that similar motions might be obtained from bodies remarkable for their odour, such as musk, castor, and ambergris. But although such bodies have a powerful odour, the odorous material has not much force, as indeed is evident from the little or no loss in weight which such bodies experience on exposure to the air. Hence when thrown on water they pour out no torrent, as in the former cases, and hence do not rotate—a result I had predicted before the experiment was made.

"I have no time to name the other bodies which also yielded no result; but many yet remain to be tried, and they may throw new light on the subject. For the advantage of those who wish to repeat these admirable experiments, it may be observed that the water should not be of the coldest,—in fact, the warmer both it and the air are the better,—but the purity of the water and of the containing vessel is of the highest importance. If the water be defiled with any foreign substance, or its surface only slightly fouled with oily matter—if only the dust of the room or of one's clothes be upon it, the looked-for motions of camphor and of benzoin will not take place, or will be so feeble as to be scarcely sensible.

"If no impurity be visible on the water, and still no rotations of the camphor occur, an agitation caused by striking the vessel will be useful; or spilling a portion of the water from the surface, so as to leave the rest pure. Less care is required in the case of salt of amber, fragments of which will move about with rapidity even when the surface is covered with a variegated coating. Specimens of water from different fountains show great differences in respect of these motions; some, notwithstanding their clearness, will scarcely, with the utmost care, allow the fragments of camphor or of benzoic acid to rotate. Hence, would it not be possible in this way to detect the presence of impurities in spring water?

"I may further remark that water is not the only liquid adapted to these experiments, for I have produced on the surface of wine the motions of camphor and benzoin, and that about as briskly; but the experiment altogether fails on spirits of wine, in which the particles sink at once. Nor is the experiment very successful with olive oil, &c."

20. This letter of Volta, though quite overlooked by subsequent writers, nevertheless forms an era in the history of our subject. It establishes no less than seven important points, and gives all but the true theory of the phenomena. He proves —1. That other substances besides camphor rotate on the surface of water. 2. That when the water becomes impregnated with the camphor, the motions cease. 3. That warm water and fine weather are favourable to these motions. 4. That the purity of the water and of the containing vessel is essential to the success of the experiment. 5. That the

success or failure of the experiment is a sort of indicator of the purity of the water. 6. That agitation of the water promotes the success of the experiment. 7. That these rotations take place on wine, but not on spirits of wine, and not very successfully on olive oil, &c. And, lastly, the reaction of the vapour of the camphor, &c., on the air or on the water, produces the motion of the fragment in the opposite direction.

21. Professor Brugnatelli, who assisted Volta in his experiments on this subject, afterwards carried on the inquiry alone, and increased the list of substances capable of rotating when thrown on the surface of water. According to Dutrochet,* these results were published in 1790 in the *Annali di Chimica* (tome i.); but I have not been able to meet with this memoir. Dutrochet, however, says that Brugnatelli rejects the theory of Volta, and is quite convinced that the evaporation of the camphor, &c., into the surrounding air, has nothing to do with their motions on water; but he admits that the camphor and other bodies pour out upon the surface of the water an ethereal oil, or some analogous substance, which, striking against the water, produces motion in the camphor by reaction.

22. There is a further statement of Brugnatelli's views in one or two papers by Dr. Carradori.† He says, addressing Brugnatelli, " You imagine these movements to be produced by the mechanical force of the elastic vapours on the surface of the water; that those bodies which move most rapidly are precisely those which have least attraction of surface for the water; and that if an oily liquid stops the motions of the camphor, it is from the greater surface attraction of the water for this oil, and that the oil stops the motions of the camphor by preventing the liberation of those vapours upon which the motions depend. You admit that the camphor, or the volatile oil of which it is composed, as also fixed oils, &c., have a surface attraction for water. I prove that on this surface attraction, and on no other cause, the motions of camphor on water depend." ‡ Again he says, " The mecha-

* *Recherches Physiques, &c.*, part i. p. 6.

† *Giornale di Fisica, Chimica, e Storia Naturale*, tome i.: Pavia, 1808. Carradori's paper is at p. 97, and it is entitled, *Risposta alle Obbiezioni del Prof. Brugnatelli rapporto ai movimenti Canfora sull' acqua.*

‡ " *Io ho provato gìa, e poco mi vuole a replicarlo, che da questo solo*

nical force of the elastic vapour against the water has nothing to do with the phenomenon; it depends entirely on surface attraction." He also affirms that bodies that have not this surface attraction are incapable of rotating on water. If, however, an indifferent substance, such as sulphur, glass, earth, sugar, paper, &c., be smeared with a fixed oil, it will rotate on the surface of water. If, then, the motion of camphor be due to its odorous vapour striking against the air, how does M. Prevost account for the similar motion of an inodorous substance smeared with an inodorous non-volatile oil?

23. In the year 1797, M. Benedict Prevost, of Geneva, wrote a memoir, entitled *Sur les Emanations des Corps Odorans*, an analysis of which was made by Fourcroy, and inserted in the *Annales de Chimie.** Prevost (without any reference to Volta) (19) attributes the motion of camphor and of other volatile bodies to the formation of an atmosphere of elastic fluid round them, and the impact of such fluid on the air. He compares the escape of the elastic fluid from the odorous body to the discharge of a gun; and he believes that the point most favourable to the disengagement of such fluid is just where the air and the water meet. He even affirms that a piece of camphor of the size of a pea, placed on a metallic disc four or five lines in diameter, will give motion to that disc on the surface of the water. If, while camphor is rotating on water, the vessel be covered up, the motions will slacken and cease; or, if the experiment be continued too long, the surface of the water will be surcharged with the odorous matter, and prevent the emission of the gaseous jet which is said to be the cause of the motion. If, while the camphor is rotating, the surface of the water be touched with the point of a pin dipped in oil, the motions suddenly cease, as if the camphor had been struck by lightning. This effect is attributed to the formation of a film, which prevents the water from entering the pores of the camphor.

24. Fourcroy, in reporting the above paper, expresses his own opinion that these motions are due to the attraction of odorous matter for air and for water, and their solution in one or both.

*principio dependono i movimenti della canfora sull' acqua, e che non vi ha parte nessun' altra cagione.*" P. 99.

* Tome xxi. p. 254. The volume is dated 30th January, 1799. See also tome xxiv. p. 31.

25. The same volume that contains Fourcroy's notice of Prevost's paper has also a notice of some experiments by M. Venturi, professor of physics at Modena.* He showed that when a column of camphor is fixed vertically in water, it wastes away at the surface: a horizontal groove is cut in it, and in the course of twenty-four hours the column is cut through. The chief wasting action is at the junction of the air and of the water, the portion in the air and that in the water losing only an insensible amount of their weight. The camphor then is dissolved at the surface, and evaporates there, and the amount of action is proportioned to the extent of surface. The oily matter of the camphor covers the surface of the water and evaporates; and this explains the motions of camphor free to move on water. This motion is nothing more than the mechanical effect of the reaction which the oily substance, in spreading on the water, exerts on the camphor itself. "If the retroactive centres of percussion of all the jets do not coincide with the centre of gravity, a motion of rotation must ensue, combined with one of progression. As the jets of oily liquid only take place on the surface, the rotation can only be round an axis perpendicular to the horizon; and since, in similar bodies of different magnitudes, the algebraic ratio of the sides to the mass increases in the duplicate inverse ratio of the sides themselves, the smaller fragments, containing more jets than the large ones, ought to rotate much more quickly."

26. Dr. Carradori again appears,† and, while approving of Venturi's explanation, claims the merit of having originated it. "Camphor," he says, "owes its motions to the expansion of an oil which is drawn from it by the force of attraction of the surface of the water." He refers to papers in Italian journals, published before the year 1794, in which this view is maintained.‡ He then combats Prevost's theory, and denies that camphor on a piece of cork or other substance, floating on water, has any motion.

27. M. Prevost replies to this paper.§ He denies that

* *Annales de Chimie*, xxi. 262.

† *Annales de Chimie*, xxxvii. p. 38.

‡ *Opusc. Scelti di Milano*, tome xx. *Ann. di Chim. et Giorn. Fisico medico di Brugnatelli*, especially tome xvii. Also *Journal de Physique Sull' attrazion di superficie.*

§ *Annales de Chimie,* xl. 30. *Vindemaire, An. X*e.

the motions of camphor are due to "the elective affinity of the surface of the water for a kind of oil which issues from the camphor on contact with water." He denies the existence of any such oil; he has sought for it in vain; and endeavours to prove by experiment that its existence is mere fancy. He reiterates the statement that camphor on a slice of cork, &c., rotates on water, and attributes Carradori's failure in repeating this experiment to some defect in the purity of his apparatus.

28. But in order to prove his theory that the motions of camphor are due to the escape of an elastic fluid striking against the air, he asserts that minute fragments of camphor will rotate on the surface of pure dry mercury as well as on water. He even goes so far as to say, that camphor will rotate on any clean dry surface, and that he has actually seen fragments of camphor, too minute to be watched by the naked eye, rotate on various supports under the microscope. He further states that camphor on a very small disc of mica placed on mercury will cause the mica to rotate; that benzoic acid and dry musk rotate on mercury; but that all motion ceases when the surface becomes tarnished.

29. Dr. Carradori replied* to this paper with a series of new experiments and old arguments, which do not require further notice here. He very properly stated that, so far from oil of camphor not existing, camphor is itself an oil —a vegetable concrete oil.

30. M. Biot reviewed the statement of Prevost, and the objections of Venturi and Carradori.† He has repeated many times the experiment of the rotation of camphor on mall discs placed on water, which Dr. Carradori could not perform (26). Small bits of camphor and of benzoic acid rotated on very dry mercury as on water; camphor also rotated on discs of very thin mica on mercury. The following experiment, he says, seems to decide the question as to the motions of camphor on water in favour of Prevost's theory: If a very small pointed cone of camphor be presented to a thin film of water, covering a glass plate made

* *Annales de Chimie*, tom. xlviii. p. 197.

† *Extrait des Recherches du C. Benedict Prevost, et de quelques autres Physiciens, sur les Mouvemens des Substances Odorantes placées sur l'Eau*, par le C. Biot. *Bulletin des Sciences par la Société Philomatique*, No. 54, p. 42. Paris: *Fructidor. An* 9 *de la Republique.*

chemically clean, it will repel the water, and leave a dry space round it. Hence he concludes that camphor acts on water at a distance, and that its movements in this liquid are due to the mechanical reaction produced on itself by the resistance which its vapour experiences in darting against the liquid which surrounds it, and that this emission of vapour is most abundant where the air and the water meet.

31. The camphor cone will also repel fragments of gold-leaf floating in water, without touching either it or them. A morsel of sponge wet with ether will move for an instant on water just like camphor, and jets will be seen issuing from the sponge, and imparting motion to it. In like manner, the vapour of camphor, darting out from every part of the surface of the fragment, produces motion. This emission of vapour, however, is most abundant at the surface, because there the vapour spreading over the surface is more quickly dissolved in the air. The resultant of these different impulses not passing through the centre of gravity of the fragment, the camphor has a progressive movement, and a motion of revolution about itself. The figure of the camphor changing each instant, the motion of its centre of gravity is never uniform nor rectilinear; it varies incessantly as well as the angular velocity of rotation. Evaporation taking place chiefly at the surface of the water, the motion of rotation is established round an axis perpendicular to this surface, and which passes through the centre of gravity of the body. As the emanation of the particles of camphor is (*cæteris paribus*) proportionate to the extent of its surface, and as the surface increases only as the squares, while the masses increase as the cubes of the homologous dimensions, the rapidity of rotation of a fragment must be great in proportion as its volume is small.*

32. About the year 1812, we meet with Dr. Carradori again † in some papers on the surface attraction of mercury

* Writing forty years later (*Comptes Rendus*, xii. 624), M. Biot admits that the above mechanical theory is essentially the same as that given by Venturi (25), but it suffices, he thinks, in conjunction with his experiments, to establish the fact that camphor acts on water at a distance, and that its motions on that fluid are due to the mechanical reaction on itself of its vapour darting against the water.

† *Giornale di Fisica, Chimica e Storia Naturale, di L. Brugnatelli*, tom. iii. pp. 261, 373. Also iv. p. 297.

for phosphorus. His experiments are based on an observation by Accum, that bits of phosphorus on mercury rotate after the manner of camphor on water. A piece of phosphorus about the size of a millet seed moved about until it had covered the mercury with a thin film, and then the motion ceased. A second bit of phosphorus did not rotate until the mercury had been cleaned. If the mercury be covered with a fine powder, such as flowers of sulphur, the phosphorus will sweep it aside. In a dark room the phosphorus leaves behind it a luminous trail. Phosphorus also rotates on the surface of tepid water.

33. In 1820, M. Serullas published* some experiments on the rotations of the alloys of potassium, sodium, &c., on a very shallow surface of water resting on mercury. When a pellet of an alloy of potassium and antimony was thrown on the water it rotated, disengaging hydrogen gas, especially from one point; and it was noticed that the motion of the pellet was in a direction the opposite of this jet. Admitting that camphor rotates in consequence of the emission of an effluvium of its own substance striking against the water or the air, and reacting on the fragment, so the alloy rotates in consequence of the emission of a jet of hydrogen. Alloys of potassium and bismuth, potassium and lead, and potassium and tin rotated on mercury alone, but more rapidly when water was added. The potassium may be replaced with sodium.

34. In the year 1825, the Brothers Weber published their celebrated work on Waves,† in which, in the section devoted to the influence of oil in stilling the waves, they refer incidentally to the motions of camphor. They give the theories of Prevost, Carradori, and others, and show, in illustration of Carradori's attraction of surface theory, that a downy feather, smeared with oil on the two opposite sides near the ends, will rotate on water. They do not, however, settle anything, but on the contrary declare that "the varied phenomena connected with the subject still remain unexplained."‡ They are even inclined to fall back upon an electrical theory in order to explain the rapidity with which a drop of oil spreads out over the surface of water.

* *Journal de Physique*, xci.

† *Wellenlehre. Von den Brüdern Weber.* Leipzig, 1825.

‡ "*Die ganze Erscheinung ist noch gar nicht erklärt.*"

35. In 1833, M. Matteucci * described some experiments on the subject of the camphor motions. He states that raspings of cork steeped in ether rotate for a long time, provided a thread conducted from the ether bottle to the surface of the water be made to act as a syphon: that a large piece of camphor on water did not rotate at first; but, when rotating slowly, it was put under the receiver of an air-pump, the motions increased with the rarefaction of the air, but were arrested when the working of the pump was stopped. He concludes that it is to the currents of the vapours of volatile substances that their rotation is due.

36. In 1840, M. Dutrochet laid before the Academy of Sciences, at Paris, a long memoir entitled, *Recherches sur la Cause des Mouvements que presente le Camphre placé à la Surface de l'Eau, et sur la Cause de la Circulation chez le Chare.* This memoir is printed in the *Comptes Rendus* for 1841, and occupies sixty-nine pages. It formed the subject of many discussions at the meetings of the Academie. M. Dutrochet affirmed at starting that the motions of camphor constituted a *phenomène inexpliquée,* but M. Biot replied by bringing forward his own experiments of 1801 (30), and those of MM. Prevost (23) (27), and Venturi (25).

37. It will not be necessary, however, to give M. Dutrochet's views, as developed in this memoir, since, in a later work already cited † he recalls them, and gives, in this separate publication, "everything that seemed fit to be preserved of this first inquiry, published with too much precipitation." ‡

38. But before this work was issued, MM. Joly and Boisgiraud laid before the Academie a memoir entitled, "New Researches on the Motions of Camphor, and of some other Bodies placed on the Surface of Water and of Mercury." § This memoir was referred to a commission composed of MM. Gay-Lussac, Pouillet, and Regnault, and their report on it is given in the *Comptes Rendus* for 1841, p. 690. It consists of a few lines only, and states, among other points which were not new—if indeed the following be new—that

* *Annales de Chimie et de Physique,* liii.: 1833.

† *Recherches Physiques sur la Force Epipolique,* p. 16. Paris: 1842.

‡ " *Tout ce que j'ai jugé à propos de conserver de ce premier travail, publié avec trop de precipitation.*"

§ *Nouvelles Recherches sur les Mouvements du Camphre, et de quelques autres Corps placés à la Surface de l'Eau et du Mercure.*

the authors find that an elevation of temperature, and whatever favours evaporation, accelerates the movement of the camphor; that benzoic acid, thin slices of cloves, of pepper, of orange peel, &c., present motions on water similar to those of camphor; * that naphthaline is motionless on the surface of water, but moves briskly on mercury; and that mercury has the advantage of rendering visible by the condensation of the breath effects which cannot be seen in operating with water.

39. In the same volume of the *Comptes Rendus* (p. 662) is a note from M. Bagé to the effect that fragments of crystallised citric acid behave like camphor on the surface of water.

40. But to return to M. Dutrochet's work. Denying the truth of all former theories and opinions as to the cause of the motions of camphor on water, he refers those motions, together with a great variety of other phenomena, to a particular force named the *epipolic* (from επιπολη, surface), because it resides in the mutual relations of the surfaces of bodies. He does not pretend to say what this force is. At one time he inclines to the opinion that it is due to the expansive force of heat (p. 62); that it also resembles electricity; but differs both from heat and electricity, although it has greater analogy with the former (p. 72). At page 85, referring to the currents produced in water by camphor, he says, "Their cause escapes us, and belongs, as far as it appears, to the very nature of the mysterious force which produces these movements." † At page 100 he states that "whatever produces a disengagement of heat at a point in the surface of water, produces at the same time in this point a development of the epipolic force." In the second part of his treatise, published in March, 1843, M. Dutrochet says (p. 159)—"The motion of camphor on water is an effect of reaction produced by heat repelling epipolic currents, which are formed near the small fragment of this volatile substance, especially near its points, or angular parts. These heat-repelling epipolic currents are two in number." ‡ And again (p. 160)—"Every-

* Volta noticed the motions of benzoic acid on water, Prevost of slices of cloves, and Brugnatelli and Carradori those of lemon-peel, &c.

† "*Leur cause nous echappe, et tient, à ce qu'il parait, à la nature même de la force mysterieuse qui produit ces mouvements.*"

‡ "*Le mouvement du camphre sur l'eau est un effet de réaction produit par des courants épipoliques calorifuges qui naissent au pics du petit*

thing then concurs to prove that these epipolic currents, produced on water by a morsel of camphor placed on the surface of that liquid, are due to the local heat developed on such surface by the vapour of the morsel of camphor, and probably also by its immediate contact." *

41. I cannot help thinking that M. Dutrochet's ideas as to the cause of the motions of camphor on water were much more distinct at the commencement of his long and elaborate inquiry than at its close. I have just quoted a passage from near the end of the second part of his essay. At p. 74 of the first part, he says, " When a piece of camphor is placed on the surface of water, there forms around it a portion of camphorated water [camphor julep], which immediately becomes endowed with a rapid centrifugal extension, due to the development of the epipolic force. The morsel of camphor, surrounded by camphorated water incessantly renewed and incessantly projected circularly on the surface of the surrounding water by a kind of intermittent explosion, must necessarily partake by reaction of the motions of the liquid which surrounds it, and receives from it those motions of progression which we see it execute on the surface of the water. Such is, in short, the cause of this phenomenon, which we are now about to study in all its details."

42. I think the above extracts fairly represent M. Dutrochet's views. They are selected from different parts of his long essay, but are so mixed up with a multitude of details relating to other phenomena, that it has been difficult to separate them. I am not aware that M. Dutrochet's views have been combated, or that any subsequent writer has taken up the subject. My object is not to disprove the epipolic theory (which seems to be an elaboration of Dr. Carradori's views on the attraction of surface), but rather to endeavour to account for the motions of camphor on water by means of new experiments involving, to a certain extent,

*fragment de cette substance volatile, specialement auprès des pointes ou des parties anguleuses qu'elle possède. Ces courants épipoliques calorifuges sont au nombre de deux,"* &c.

* " *Tout concourt donc pour prouver que les courants épipoliques, produits sur l'eau par une parcelle de camphre placée sur la surface de ce liquide, doivent naissance à la chaleur locale développée sur cette surface par la vapeur de la parcelle de camphre, et probablement aussi par son contact immédiat.*"

the views of such eminent observers as Volta, Venturi, Carradori, and Matteucci. These four observers have explained the motions of camphor on water; but their explanations, although correct as far as they go, do not, I think, go far enough. I will, therefore, describe some of my own experiments, and wind up with a series of propositions to which those experiments lead, and which represent succinctly most, if not all, the conditions of the case (112).

## Section II.—Further Investigation of the Subject.

### 1. *The substances that rotate.*

43. The general fact, then, is that small fragments of camphor, when thrown on the surface of clean water, become endowed with motions of rotation and progression; and that if the surface be touched with oil, the motions are instantly arrested. In repeating this experiment, it will be found that the fragments rotate in different directions, some quickly, others slowly; some remain at rest, others move round close to the glass, without at the same time spinning. A fragment, while pursuing one direction, will suddenly change its path, as if scared from its purpose, like a live thing; or, while moving round near the edge, it will occasionally stop and play about within a small circuit. A fragment, while rotating, will sometimes throw off smaller fragments, which rotate, it may be, in opposite directions to the parent piece, but much more quickly. Or a small piece rotating in one direction may suddenly change its direction, and rotate in the opposite one.

44. The quantity of water does not greatly influence the phenomena, except as to time (67). If water be dropped on a plate of glass, so as to form a flat shallow lens, fragments of camphor will rotate on it with great rapidity, often sliding over the convex edge, and rotating nearly in a vertical position; they then dart from this to the horizontal surface, move rapidly about with a spinning motion, again to go over the edge, to be again driven upon the surface, and so on, many times.

45. The fragments of camphor sometimes remain quite inert, but a sudden jerk given to the glass, or pouring its

contents into a second empty glass, may set them spinning. In such case, there is a thin film of organic matter on the water, which prevents adhesion between the water and the camphor, and the mechanical actions described break up this film, and promote adhesion. Volta's idea (19) was so to agitate the vessel as to spill some of its contents, and thus expose a fresh surface. Washing the glass vessel in sulphuric acid or caustic alkali may be necessary to make it chemically clean (52, note).

46. The experiments also succeed best in bright, warm weather, and often fail altogether in dull, wet, or foggy weather, unless hot water be used—but this must be perfectly clean. Volta noticed the importance of a rather high temperature, both of the water and the surrounding air (19).

47. The above remarks refer to the camphor of the *Cinnamonum camphora*, which produces the ordinary camphor of the shops. What is known as "Borneo camphor," from the *Dipterocarpus C.*, or *Dryobalanops C.*, is more crystalline and less volatile and fragrant than the former. It rotates on cold water, but its motions are more rapid on hot.*

48. Artificial camphor from oil of turpentine ($C_{20}$ $H_{16}$ H Cl.) rotates well on tepid water. The crystalline plates of camphoric acid are also admirably adapted for showing the experiment, even in damp weather, when it is difficult to perform it with ordinary camphor. A single drop of a solution of camphor in sulphuric acid, gently delivered to the surface of water from the end of a glass rod, will form into a perfectly-shaped double convex lens, or button, Fig. 1, with a sharp projecting ring coinciding with the surface of the water, and separating the upper half lens from the lower. A button of this kind will move about on the surface of the water for a long time in a playful, eccentric manner, sometimes describing the curve shown in Fig. 2. Its red back, projecting just out of the water, and its life-like motions,

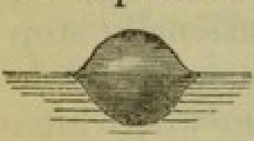


*Fig.* 1.

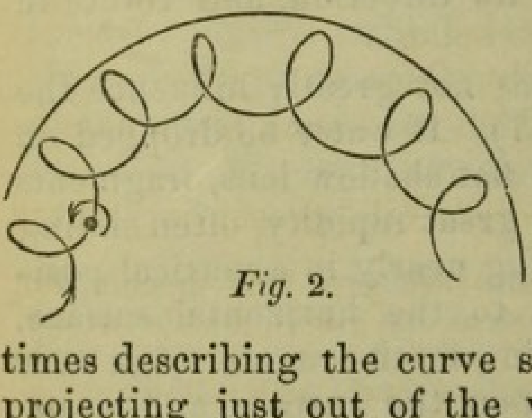


*Fig.* 2.

* There appears to be a difference in the chemical composition of these two camphors of only two atoms of hydrogen: laurel camphor consists of $C_{20}$ $H_{16}$ $O_2$, and Borneo camphor of $C_{20}$ $H_{18}$ $O_2$.

give it the appearance of a water insect. The resemblance is more striking when several drops are deposited on the surface of the water: they move about briskly, and avoid each other as if endowed with intelligence. When these buttons have come to rest, they will renew their gambols if transferred to fresh water.* A drop of a solution of camphor in benzole, gently delivered to the surface of water, is well adapted to show the surface attraction so much insisted on by Carradori (22). We shall recur to this subject (95).

49. Essential oils consist of two portions, the *stearopten*, or solid portion, and the *elaiopten*, or fluid portion.† Camphor is the stearopten, or solid portion of a volatile oil, the fluid portion of which is not well known in this country. A small quantity of it is contained in sublimed camphor, and a larger quantity about crude camphor. It may be separated by distillation, if care be taken to keep the temperature below the boiling point of the stearopten. I obtained a portion of the Borneo camphor oil from a wholesale druggist; it was of a dark amber colour, with an odour between that of turpentine and camphor, and containing feathery crystals of camphor. A small portion of this oil exposed to the air formed plates of solid camphor. One of these, wet with the oil, was placed on the surface of water. It shot out a beautifully variegated tail, curved like a comet, and immediately began to move in the opposite direction, like a Barker's mill (see Fig. 3). After twenty or thirty rotations the oil was expended, and the plate rushed up to the side of the vessel. Small fragments separated from it continued to spin rapidly, evidently from the oil being drawn off at one of the angles by the adhesion of the water-surface, which used such fragments as a point of resistance.

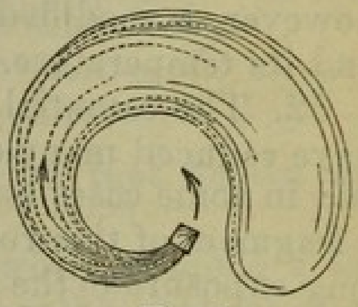


*Fig.* 3.

50. Camphor, a volatile concrete oil, presenting these

* A small quantity of sulphuric acid, such as a fluid drachm, will liquefy a large quantity of camphor, such as 250 grains, if added in portions of 50 grains at a time after intervals of two days. The acid is not decomposed, but the colour of the solution passes through yellow, orange, and ruby into black. When a drop is put on water, the acid dialyses off, and the camphor resumes its usual colour.

† *Stearopten* from στεαρ, fat } and πτηνος, volatile.
*Elaiopten* from ελαιον, oil }

phenomena of rotation, &c., on water, it occurred to me that a volatile oil rendered concrete by cold would behave in a similar manner. For this purpose no oil is more convenient than oil of aniseed, which is concrete at the usual temperature of the air during the winter months of England. Accordingly, a small lump of this concrete oil was placed on water some degrees warmer than itself, when it immediately began to rotate, the oil from it forming a trail which was diffused in widening spirals over the surface of the water, and was evidently the cause of the motion.

51. A similar effect was produced by a fragment of carbolic acid solidified by cold. The fragment moves about rapidly, liquefies, and continues in motion under the form of its cohesion-figure. A drop of creosote gently delivered to the surface of water becomes greatly agitated, and sails about during some minutes (91). A drop of oil of cloves, or of oil of pepper-leaves, will do the same. A drop of oil of camphor on the surface of acetic acid exhibits similar phenomena. A fragment of camphor on the surface of nitric acid instantly liquefies, and in the act of doing so forms a very perfect cohesion-figure, Fig. 4, every part of which is greatly agitated. The character of the figure, however, is modified by the strength and purity of the acid and its temperature.

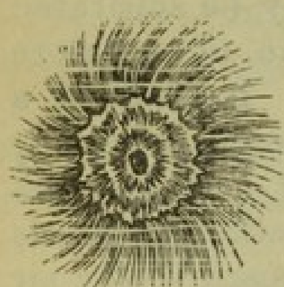


*Fig. 4.*

52. The essential oils do not readily congeal by cold. I have reduced many of them to the temperature of zero, Fahr., and in some cases have caused a separation of the stearopten: a fragment of this rotates on the surface of water. Others, by long exposure to the air, form a viscid mass, a portion of which rotates when placed on the surface of clean tepid water.*

53. Succinic and benzoic acids rotate well on water (19.)

* Distilled water is not necessary in these experiments. New River Company's water was used in glasses which were occasionally cleaned by being filled with cold sulphuric acid, and rinsed with water, and after every experiment washed out with solution of caustic potash and rinsed. The glasses used were conical in shape, and the opening at the mouth was about 4 inches in diameter (see Fig. 10). In general, the larger the surface of water, the more favourable the experiment. Tepid water should not be had out of a boiler or kettle, nor be poured into a jug used for domestic purposes, none of these articles being chemically clean, but should be prepared in a clean flask by means of a spirit-lamp.

The succinic acid should be in small fragments, but these do not rotate exactly like camphor. In one experiment with this salt, a piece darted across the surface two inches or more, in a straight line; other pieces, instead of spinning in one spot, described circles of one or two tenths of an inch in diameter. Sublimed benzoic acid requires the water to be heated to 80° or 90°, and then the bundles of needles open suddenly, cover the surface, rotate with great rapidity, and quickly disappear. Citric acid (39), in small slices, spins with great rapidity, but not nearly so long as camphor. Tartaric acid, which is sometimes substituted for citric, does not rotate on water; and hence a rapid mode of discriminating between these two acids in powder, might, under certain circumstances, be adopted. Indeed, such salts as are very soluble do not rotate, but a stream of solution descends from the fragment. A crystal of a soluble salt may, however, be made to rotate slowly, if it be kept to the surface by being attached to a piece of cork, by means of an india-rubber ring, as in Fig. 5. If gently deposited on the surface of still water, it will describe a curve similar to that shown in Fig. 6.

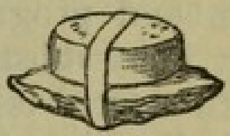


*Fig.* 5.

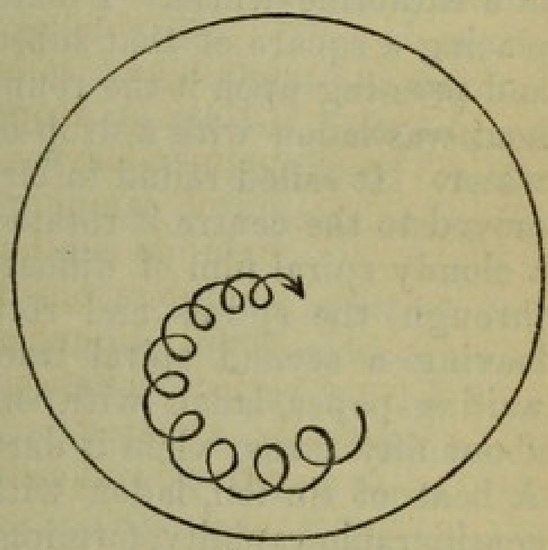


*Fig.* 6.

54. It is remarkable, however, that while succinic acid rotates briskly on the surface of water, a soluble salt of that acid, such as succinate of soda, will not rotate except by the artifice shown in Fig. 5; while some of the butyrates, such as the butyrate of baryta, of lime, and of magnesia, rotate admirably on the surface of water. The reason for this will probably be found in the molecular condition of the two acids. Succinic acid is a crystalline body, and it slowly yields to solution on the surface of water; butyric acid is a liquid body lighter than water, and strongly attracted by its surface. A single drop of this acid delivered to the surface of water four inches in diameter, instantly flashes out into a

film. I have also found that fragments of caustic potash rotate on the surface of butyric acid.

### 2. *Imitations of the above Rotations by inert Substances.*

55. Carradori pointed out that these phenomena of rotation and progression may be imitated by placing on water any inert substance, such as tin-foil, talc, paper, &c., smeared with a fixed oil. Better results are produced by the use of volatile oils. The experiment may even be extended to any volatile fluid, such as ether, alcohol, chloroform, &c. A piece of talc touched with a glass rod dipped in oil of camphor, and placed on water, spun round with the rapidity of a catherine-wheel. I made a small coracle of tin-foil by placing a square of that substance in the palm of the hand, and pressing upon it the rounded end of a glass rod. This boat was laden with a drop of oil of camphor and placed on water. It sailed round in circles close to the glass. When moved to the centre it rotated in a re-entering spiral, leaving a cloudy spiral film of diffused oil. It then crossed its path through the spirals and rotated in an opposite direction, leaving a second spiral track behind. A similar boat of writing-paper, laden with oil of camphor, spun admirably about fifty turns, when it darted up to the side of the glass. A boat of tin-foil, laden with turpentine, spun round with considerable rapidity, forming a very regular and beautifully coloured spiral of twelve or fourteen turns. It then stopped, and slowly sailed up to the side of the glass. Creosote in a paper boat rotated wildly for about half a minute, and then darted up to the side. A small flat square of paper, carrying a spot of creosote, rotated forty or fifty times in widening spirals, when it reached the side and sailed round it many times, until the creosote was nearly expended. Bisulphide of carbon, in a paper boat, rotated admirably, but was quickly influenced by the capillary action of the side. Chloroform in a boat gave a few wild turns, and then rushed up to the side. Absinthe moved in curved lines, marking its track in gorgeous colours. Oil of wintergreen, on a flat piece of paper, rotated admirably ; rapidly at first, then more slowly, so slowly indeed, that the motion of the raft was clearly opposite to that of the jet, as in Fig. 3.

56. Now in these cases the motion is due to a jet of an oil, &c., diffusing itself over the surface of the water. The jet is visible, and the motion of the coracle, or raft, can easily be accounted for on mechanical principles, as Volta was, indeed, the first to do (19). Prevost states (23) that a piece of camphor on a flat piece of tin-foil, on water, will cause the foil to rotate, in consequence of the effluvium of the camphor striking against the air, and reacting on the camphor and raft. I have tried many times, but in vain, to repeat this experiment. Prevost says (27) that Carradori failed for want of proper precaution as to the purity of his apparatus. I therefore washed out a glass with a solution of caustic potash, formed a little boat of tin-foil, and washed this in the same manner; turned up the corners, and loaded it with a piece of camphor about the size of a pea. The glass was filled with water, and left undisturbed for about ten minutes, to allow it to come to rest. The boat-load of camphor was then taken up with pliers, and gently deposited on the centre of the surface of the still water. It remained quite motionless.

57. In order to test the purity of the water, a bit of camphor of about the size and shape of a canary-seed was thrown upon the surface. It spun round, darted up to the boat, seized it as if by means of teeth, and began whirling it round, first in one direction for twenty or thirty turns, then in another direction for as many: at the same time causing the whole to sail about slowly. It then moved gently round up to the side. The whole thing was like an insect attacking an object in the water, it was so life-like. It lasted about twenty minutes, when I detached the small fragment of camphor from contact with the raft, and it then began to sport about on its own account, just like an aquatic insect. It occasionally described the figure 8 of the skater. (See Fig. 7.) But it soon felt the influence of the boat, darted up to it, and whirled it round as before. I withdrew this small frisky piece of camphor, and left the raft laden as before; it sailed up to the sides without rotating; I removed it again to the centre where it remained at rest. I knocked the cargo out of the boat—it soon attached itself to it, and turned round slowly with it.

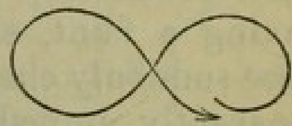


*Fig.* 7.

58. I now took a clean glass of water, and put on it a slice of cork, and on this a bit of camphor; there was no motion whatever, except such as could be clearly traced to the capillarity of the sides of the glass. A piece of camphor put into the water was very lively; it soon found out the cork, which was sticking to the side, and kept playing about it for a long time, just as a fish plays with a bait.

59. I am satisfied that unless the camphor touch the water there is no motion imparted to the raft or boat by the camphor as such; but an observer may be deceived by the capillarity of the sides of the glass, and also by the rotation of the water consequent on filling the glass. This rotatory movement keeps up a long time, and may readily deceive.

### 3. *The Camphor Film.*

60. The rotation of inert substances smeared with an essential oil, is clearly due to the formation and diffusion of a film in consequence of the adhesion of the water. That such a film is formed when camphor is thrown on water, and that it owes its motion to the diffusion of such film, is the next point to be proved. The camphor deposited by the oil of camphor, by exposure to the air, is well adapted to this proof from the circumstance that the film is visible. The following experiment may be cited in illustration of this. On the 18th December a small quantity of oil of camphor was exposed to the air. Next day camphor was found deposited in plates and granular masses. On the 20th, the oil was set to drain off from the solid during about two hours, and the solid was then dried on filtering paper. A slice of this on water broke into three portions, each of which rotated well, leaving a faint, shadowy, but still very distinct trail; one piece suddenly changed its direction of rotation, then stopped, apparently impeded by the film, then got clear of it and rotated again, and again stopped. One of these pieces of camphor was now transferred to a fresh glass of water. It instantly broke up into numerous fragments, which rotated with amazing rapidity; one piece rotating rapidly in one direction was joined by a second piece, when it stopped for a few seconds, and suddenly changed its direction of rotation.

The day was cold but dry, with a high barometer (30·33 in.) and rising. The experiments were performed in a room without a fire.

61. A piece of the filtering paper used in drying the crystals was put on the surface of water; it rotated admirably for a few turns, then went to the side and sailed all round the glass, and finished with two or three pirouettes in the centre, diffusing a film during the whole performance.

62. Now the ordinary sublimed camphor does not produce any visible film on water—a circumstance which has occasioned so much discussion as to the cause of the motion; common camphor having been used in these observations from the time of Romieu (6) to that of Dutrochet (36). Had the film been visible, as it is in a large number of the allied experiments described in this Essay, the motions of camphor on the surface of water would scarcely have led to any discussion. When a fragment of camphor is thrown on the surface of nitric acid, a well-defined film or cohesion-figure is visible (51, Fig. 4). That a film is produced on the surface of water by common camphor cannot be doubted by any one who will take the trouble to render its effects visible. For example—camphor will not rotate, that is, will not form a film on water, unless the glass and the water be quite clean. In like manner, a drop of benzole gently deposited on water will not form a film or cohesion-figure on its surface, unless the water be quite clean. If the water or the glass be contaminated with organic matter, the drop of benzole will flatten into a lens, but will not spread out into a film. That a piece of camphor on water is surrounded by a film, may be proved by gently depositing a drop of benzole on the water on which camphor is spinning. The benzole forms a very perfect double convex lens, only a little larger in diameter than the drop. A drop of Persian naphtha, under similar circumstances, forms a lens which slowly flattens and spreads. The camphor, in sweeping round, repels the flattened disc of naphtha, as in Fig. 8, the naphtha (N) being circular, except when the influence of the camphor (C) is felt, and this influence is exerted, according to my view, by means of an invisible camphor film (83). This film does not repel the naphtha bodily, on account of its superior adhe-

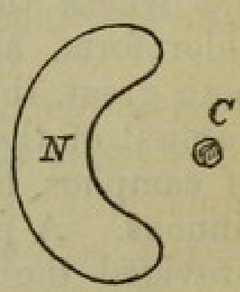


*Fig. 8.*

sion to the water on the side farthest from the camphor—hence the kidney-shaped figure.

63. So also creosote on water, while its cohesion-figure is displaying its remarkable phenomena (51), if the disc be touched with a point of camphor that passes through it to the water, the creosote will split into numerous lenticules, in consequence, as I believe, of the extension of a camphor film under it. In some cases, as when the water is nearly saturated with creosote—a third drop, for example, on the surface of two ounces of water—the camphor, in splitting up the disc, will stop all motion of the creosote. After some minutes, however, the small discs of creosote will recover their motion in consequence of the evaporation or solution of the camphor film (92).

64. The existence of the camphor film may also be beautifully shown, by slightly dusting the surface of the water with lycopodium. If a piece of camphor be gently lowered into the centre of the surface, the moment the camphor touches the water the powder will open into a large pupil, revealing a clear space of water about two inches in diameter, depending on the size of the camphor and the amount of surface (69). Other proofs of the existence of this camphor film will be given presently.

## 4. *Solution and Saturation.*

65. The motions of the boats and rafts of tin-foil, paper, &c., smeared with oil, &c. (55), do not last long, on account of the small degree of solubility of the oils, or their comparatively slow rate of evaporation. In the case of the oils, the film may clog the motion before the whole cargo is diffused; but where the cargo is volatile, as in the case of ether, chloroform, alcohol, &c. (55), solution and volatility being both great, the motions are rapid but of short duration, on account of the rapid expenditure of the cargo. In the case of camphor, however, the motions are both rapid and continuous. A piece of camphor will go on spinning for hours, provided the water be exposed to the air, and the air be tolerably dry. If evaporation be arrested, the motions of the camphor will cease as soon as the water is saturated; and camphor requires about 500 (some say 1,000) times its bulk of water for solution. In warm, dry weather, the covering

of the vessel with a glass plate, unless it fit very accurately, will not be sufficient to arrest the motions of the camphor; but if the camphor be put into a stoppled bottle nearly full of water, the rotation will cease after a time, depending on the quantity of water engaged. In an experiment of this kind the camphor rotated briskly for an hour, when the bottle was left until next morning : by that time all the lumps had disappeared, and the surface was covered with a dotted crust. Some of the julep was poured into a wine-glass, and fresh fragments of camphor added ; they remained perfectly passive, as in Volta's experiment (19), but when some of them were removed to a glass of clean water, they rotated briskly ; and on filling up the wine-glass of julep with fresh water, the fragments left in the julep rotated.

66. This experiment was repeated in the following manner : —The camphor was placed in a well-stoppled bottle, and watched during several hours. The motions of the fragments were at first brisk, then declined, and at length ceased. The bottle was next shaken up many times, and left until the following morning. About one ounce of julep was poured into a large glass, and fresh pieces of camphor were cut into it; they did not move, but on filling up the glass with fresh water, the rotations set in vigorously.

67. Similar quantities of camphor were placed in very unequal quantities of water, and carefully excluded from the air ; the camphor in the larger vessel went on spinning for hours, that in the small one soon ceased, on account of the more rapid saturation of the smaller quantity of water. Unequal quantities of camphor were set spinning on small but equal quantities of water ; the motions ceased first in the vessel that contained the largest supply of camphor. Increase the quantity of water, and the fragments resume their motion.

68. It may also be remarked, that the motions of creosote on water cease when communication with the air is cut off.

### 5. *Evaporation.—The Camphor Film in motion.—Camphor Currents.*

69. It appears, then, that the continuous motion of camphor on the surface of water, depends on evaporation, as well as solution. To show the great importance of evaporation

in producing continuous rotations, I lowered a cylinder of camphor into the centre of a glass of water, the surface of which was about four inches in diameter. The camphor was fixed in forceps, Fig. 9, moving vertically in an arm attached to the rod of a retort stand, as shown in Fig. 10, left hand. In order to see what took place under these circumstances, the surface of the water was slightly dusted with lycopodium. Other powders were tried, but none answered so well;* only this precaution must be adopted—as small a portion as possible is to be used, only just sufficient to cover the surface with a single layer of particles, so that evaporation may scarcely be checked. The particles when thoroughly wet, will sink just below the

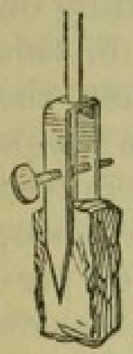
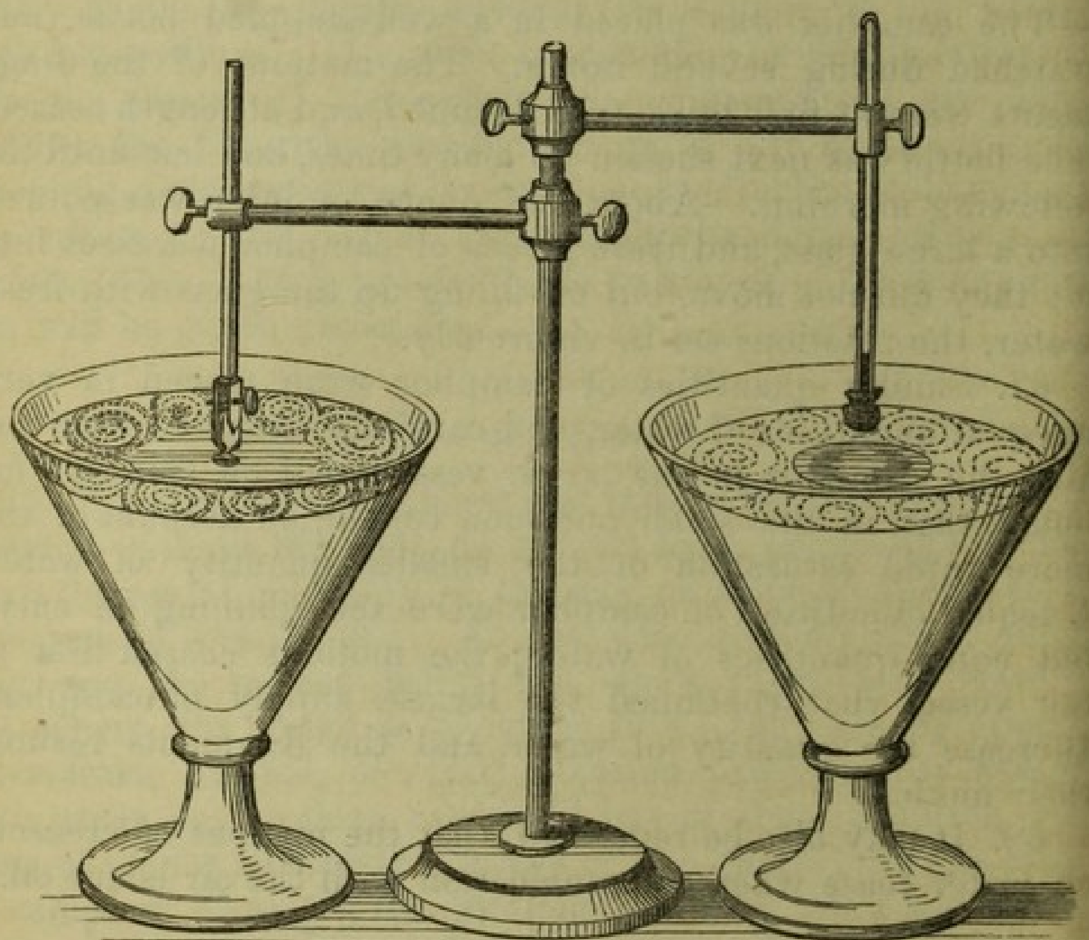


*Fig.* 9.



CAMPHOR CURRENTS. *Fig.* 10. ETHER CURRENTS.

surface, and form a continuous layer admirably adapted to

* Dutrochet used kaolin in fine powder dispersed through the liquid for the purpose of testing the presence of vertical as well as horizontal

show the phenomena. The best method of applying the powder, is to tie a portion of it up in a piece of cambric, and shake it gently over the surface.

70. Matters being thus arranged, the water covered with its layer of lycopodium perfectly at rest, the forceps rod holding the lump of camphor, which may be from a quarter to a third of an inch in diameter, is to be gently brought down to the centre of the surface. If the experiment has been well arranged, and the day is favourable, a very remarkable effect will be produced. The very moment the camphor touches the water, there is a strong repulsion of the lycopodium powder, and a clear circular space of water is opened, a couple of inches in diameter. Then there is a pause for a moment, when, all at once, the lycopodium is formed into a number of revolving wheels, arranged in pairs; the two members of each pair revolving in opposite directions; the whole forming a wonderfully beautiful and striking figure. (See Fig. 11.) The number of wheels in a glass four inches in diameter (Fig. 10), may be 4, 6, 8, or 10, according to the size and shape of the piece of camphor, and the vigour with which the process is carried on. In Fig. 11 it will be seen that a triangular piece of camphor sends forth three currents, one from each face, producing three pairs of wheels. An experiment of this kind was set going at 12·40 p.m., on a bright day in October, 1861. It was watched occasionally during eleven hours, and the currents continued to flow all that time with unabated vigour. The apparatus had not been touched, and no more water had been added; the number of wheels, however, had diminished, which was explained by the circumstance that the camphor at the surface of the water was

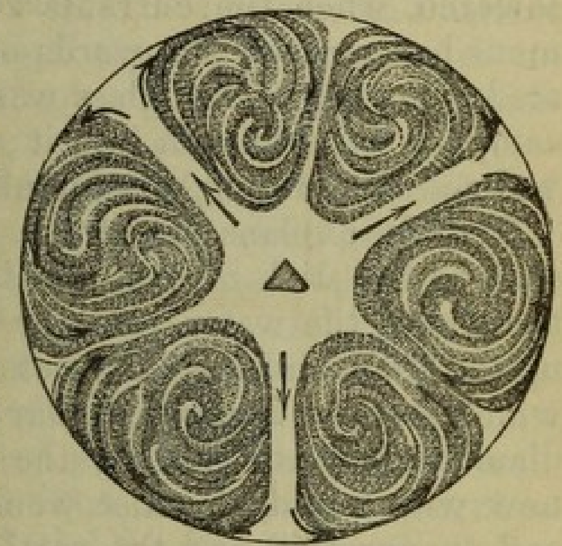


*Fig.* 11. CAMPHOR CURRENTS.

currents; but he worked on so small a scale, with a watch-glass, for example, requiring the aid of a microscope, that he seems to have missed the real nature of the phenomena.

nearly cut through, as in Venturi's experiment* (25). On examining the apparatus early next morning, the camphor was found to be quite cut through, and the severed piece was, by its rotations, making a great disturbance among the lycopodium particles. The camphor in the forceps was quite out of the water. It was lowered just below the surface, and the currents set in again. This time, also, three pairs of wheels were in active rotation. The experiment was watched during the day, when the currents continued to be active, and a second incision was made at the surface. The water smelt strongly of camphor, and, indeed, scented the whole room. The cut went on until a second piece was separated, which behaved like the first. The forceps were again lowered, when the currents set in as before. The experiment having lasted upwards of fifty hours was now terminated; but before the glass was emptied, fresh fragments of camphor were thrown into it; these gyrated vigorously, and waltzed with the old fragments.

71. My explanation of the above phenomena is this: as soon as the stick of camphor is lowered below the surface of the water, the water rises by capillary attraction some way up the stick, and instantly detaches a portion of its substance, which forms a film or circular disc about an inch or more in diameter, the stick being the centre. If the camphor were now withdrawn, the disc would be disposed of by solution and evaporation, and the particles of lycopodium would close in as the film disappeared. But as the camphor remains in the water, there is a perpetual renewal of camphor films, so that before one is disposed of by solution and evaporation another is formed upon it, and the boundaries of the first film, which are accurately defined by the cohesion of the film and the adhesion of the water, are broken through by the excess of matter, which excess is carried by the adhesion of the water in radial lines to the sides. If this excess of matter were sufficient for the purpose, the adhesion of the water would form it into outer boundary discs or films, as happens in the cohesion-figures of sulphuric ether, castor oil,

* Venturi mounted some cylinders of camphor on pellets of lead, placed them upright in a saucer, and poured in water up to about half their height. He noticed that they were cut through at the surface of the water, but he did not suspect the existence of these currents.

and some of the volatile oils;* but the excess not being sufficient to form rings, is projected in radial lines, which, striking against the curved side of the glass, each line becomes divided into two portions, one of which passes to the right, and the other to the left; and as all the other lines are behaving in a similar manner, the reflected curves influence each other, and constrain every curve to be a re-entering one. The motions of the lycopodium render this process perfectly distinct, and enable the eye to follow all the details and variations of the phenomena.

72. The films thus formed are disposed of by solution and evaporation, but chiefly by the latter force, nearly as fast as they are formed. They are supplied by that portion of the camphor that is cut by the surface of the water, and where the water rises above it, by capillary attraction; hence an incision is made about the tenth of an inch in width, which goes on deepening until the section is complete.

73. In order to try the effect of stopping evaporation, I conducted the above experiment in a pint white glass bottle. It was carefully washed first with sulphuric acid, then with water, next with solution of caustic potash, and again with water, after which it was filled two-thirds with water. The surface was dusted with lycopodium; a cork was next perforated, and the rod of the forceps passed through it; a fresh piece of camphor was also put into the forceps; and thus arranged, the cork was rammed tightly into the neck of the bottle. On lowering the camphor into the water, the action was so feeble that I suspected some part of the apparatus to be not quite clean. I therefore prepared another bottle with great care. It was a white glass quart bottle, arranged as before. On lowering the camphor, there was still little or no action. Feeble currents were indeed visible, but after some hours they ceased altogether. There was a circular space of about half an inch in diameter clear of dust all round the camphor, while a gradually-increasing incision in the

* This refers to the author's discovery of the cohesion-figures of liquids. That is, if a drop of an independent liquid (*i. e.* not a solution) be gently delivered to the surface of clear water, the drop flattens out into a figure characteristic of the substance employed, which figure is the resultant of the cohesive force of the liquid, its density, and the adhesion of the surface. Many of these figures are very beautiful, and their variety is all but endless. See two papers on this subject in the *London and Edinburgh Philosophical Magazine* for October, 1861, and March, 1862.

camphor at and about the level of the water showed that solution was going on. It was watched during many hours, but no descending current could be detected; and indeed, no motion whatever was perceptible, although examined by a strong artificial as well as natural light. Next morning the incision was complete—the piece was lying quite inert about an inch from the central piece: it had wasted a good deal from solution. The cut from the main piece had not been made at the surface of the water, but at varying distances above it, the parent piece presenting to the water an obtuse angle. The stump was now lowered into the water, when it immediately repelled the lycopodium for a short distance—an effect due to capillarity. A camel's-hair pencil brought down to the surface of the water under similar circumstances will in like manner repel the powder to a short distance all round it. During the day a second incision was made, but no currents could be detected. The piece below the surface of the water had a vitreous transparent appearance, and had considerably wasted. On the third day the following (Fig. 12) was the appearance of the camphor—the portion immersed being transparent, that out of the water of the usual white translucent character.

*Fig.* 12.

74. Some julep from this large bottle was now poured into a wide-mouthed conical glass, and when at rest a little lycopodium was dusted on the surface. A fresh piece of camphor was brought down to the surface, when the currents set in with great vigour. It is clear, therefore, that exposure to the air, or evaporation, is as necessary as solution in the production of these currents. Now, here is water which has been exposed to the influence of camphor during upwards of three days without exhibiting any currents, but is no sooner exposed to the air than it displays these currents very finely, producing six actively-revolving wheels.

75. To show how slight a cause interferes with evaporation, I dusted a little more lycopodium upon this active surface. This had the effect of almost entirely destroying the currents. The surplus lycopodium was next removed by dipping into the water a dry glass rod several times, after which the current set in as vigorously as before.

76. The apparatus was moved out into the sunshine when the currents soon increased in vigour.

77. On the other hand, the experiment was tried on a wet ›ggy day in November, when the air was nearly saturated ·ith moisture, and it almost entirely failed. The experiment ·as conducted in a small room where a fire is never lighted. ·he experiment was repeated in the same room on a cold, ut dry December day; the currents and wheels were pro-uced very fairly, but they soon ceased.

78. To show the importance of the kind of powder used s the exponent of these phenomena, I have dusted the sur-ace with flowers of sulphur, and also with fine tripoli, and ailed to produce the currents, or they were very feeble. )n removing such powder with a dry glass rod, and dusting n lycopodium very faintly, the currents were produced with igour.

79. Comparative experiments have also been made with qual quantities of camphor julep, and fresh water in diffe-ent glasses, with the same piece of camphor. The currents ere much more vigorous on the fresh water than on the ılep; the latter producing on one occasion only six wheels, ıe former twelve, and these much more vigorous than those.

## . *The Camphor Film continued.—Imitations of the Camphor Currents.*

80. In my explanation of the motions of camphor on ater, I endeavour to prove the existence of a camphor lm, or disc, continually given off by the camphor, and so roducing the motion. This film, or disc, is not visible, but s presence is proved by lycopodium powder (70, Figs. 10, 1), naphtha (62, Fig. 6), &c.; and it may be further proved y analogy with bodies which rotate on water, and give off lms which are visible.

81. The camphor film becomes visible if fragments of amphor be thrown on nitric acid instead of water. In such ase they rotate with great rapidity, and are at the same me surrounded by visible films, or cohesion-figures, each eing well marked and distinct. It is raised above the eneral level of the acid, and throws off lines of action hich extend to some distance, and produce rotation (51, ig. 4).

82. So also the well-shaped buttons of camphor formed by

gently delivering successive drops of sulphate of camph oil to the surface of water (48, Figs. 1, 2), show the existen of a camphor film around each. They play about in ar among each other, and avoid each other, in the most life-lil manner. And this they do, according to my view, in cons quence of the film that is detached from each button (ar surrounds it) by the strong adhesion of the water. It is, fact, as with pure camphor, a case of very gradual solutio in which each film, as it is detached by the adhesion of tl water, becomes diffused over the surface by the same for of adhesion, and in doing so not only gives motion to i parent button, but encounters other films similarly in the a of diffusion. These films, in seeking to occupy each its du share of space, repel each other, and thus give the appea ance of the parent globules avoiding each other in the lif like manner described. The films, almost as fast as they a spread, disappear by evaporation and solution, and allo others to take their place, until the water becomes saturate and the buttons cease to move.

83. This refers to the lenses of sulphate of camphor o (Fig. 1), or rather to lenses of camphor, long after the su phuric acid has dialysed off. The film surrounding eac button is often faintly visible, and the force which diffuses produces the motions in the manner described. The fil too, in the case of pure camphor, must exist, though invisibl (except by means of a powder (70) present on the surface), an must in like manner produce the wonderful rotations abou which so many men of science have disputed.

84. But it is contended that the motions of camphor ar due to the formation of camphor julep in the first instanc that in fact the actual solution of the camphor in water is th cause of the camphor's motion. Any one who had seen th camphor lowered into water dusted with lycopodium, an had noticed the rapid flash with which the clear circula space opens (70), could hardly fail to admit that something i detached from the camphor which produces this repulsion and maintains it—that something is not a solution of cam phor, there is not time for it; but a portion of the soli camphor rendered fluid by contact with water—a campho film, as I have called it, which surrounds the solid, and i disposed of by solution and evaporation. It is not denie that a solution of camphor is made, but it is not made in th

rst instance. The solution is made from the films, not from le solid camphor.

85. The camphor on water may be fairly represented by le solid carbolic acid (51). When a bit of this is placed 1 water, it becomes very active, moving about over the rface, darting out cilia, and quickly disappearing. In this se the solid is surrounded by a well-defined disc of liquid, id from which rays spread in all directions, producing eat, but well-defined disturbance on the surface, as indi-ted by a very thin layer of lycopodium powder. These ys are sharp and well defined, and throw the powder into number of rapidly-revolving wheels of the same character those produced by camphor, but quicker in motion. The pidity with which the fragment disappears shows how iportant is the adhesion of the surface in favouring solu-n, and the presence of air in assisting the evaporation of e expanded film; for when a piece of the solid acid is owed to fall on the surface of the water instead of being ntly delivered to it, it sinks to the bottom, slowly liquefies to a globule, and *remains permanent*, just as a crystal of salt would do at the bottom of a nearly saturated solution a salt.

86. Another strong argument in favour of the existence a camphor film is that the currents and wheels produced camphor, as above described, can be perfectly imitated by rious volatile substances, e vapour of which forms hesion discs on the surface water. Sulphuric ether, example, is well adapted the purpose, and my me-od of arranging the expe-nent is to tie a bit of sponge the end of a thick tube th a narrow bore, and ving arranged it in a ver-al position (as shown in g. 10, right hand, or by ng it to the ring of a retort nd), the sponge is to be

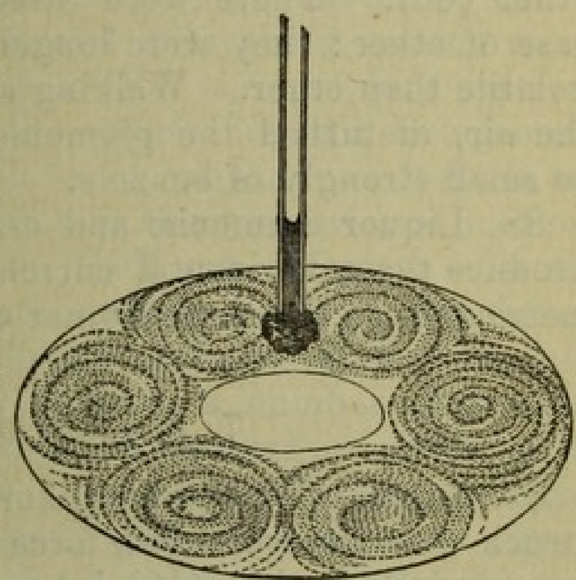


*Fig.* 13. ETHER CURRENTS.

pped into sulphuric ether, and so be brought down to thin a quarter or half an inch of the centre of the surface of the water, previously dusted with lycopodium. As soon as the vapour reaches the water, the powder is powerfully repelled, cohesion reasserts itself on the part of the ether vapour and an ether disc is formed, with a sharp, well-defined edge (See Fig. 13.) As this disc continues to be supplied with ether faster than it can be accounted for either by solution or evaporation, the surplus quantity streams off in radial lines, which, meeting the curved surface of the glass, divide and form re-entering curves, producing pairs of discs rotating in opposite directions (70). The advantage of using a tube in the above experiment is that a small reservoir of liquid can be maintained above the sponge, so as to continue the action for some time.

87. Benzole produces these surface currents in great perfection; but it is necessary to the success of this and similar experiments, that in each case a clean sponge be used upon a clean surface of water, dusted with fresh lycopodium. As soon as the benzole sponge is brought down to within half an inch of the surface of the water, there is a powerful repulsion of the powder, and a large benzole disc is formed well defined, but not so sharply so, as in the case of ether (Fig. 13.) Outside this disc currents set in irregularly at first; but, by the observer remaining still, and holding the hand before the mouth, the wheels gradually become regular almost as much so as in the cases of camphor (70), and of ether (86). There were three pairs of wheels, as in the case of ether: they were longer in motion, as benzole is less volatile than ether. Walking about, or any disturbance of the air, disturbed the phenomena. The water soon began to smell strongly of benzole.

88. Liquor ammoniæ and essential oil of turpentine also produce these horizontal currents, but they are not so symmetrical as in the three former cases.

### 7. *Surface Attraction.*

89. In studying the camphor motions Carradori (22) was struck with the wonderful force of attraction of a liquid surface, and Dutrochet (40) in an elaborate treatise extended this force to solids and liquids, under the name of the *epipolic.* It was scarcely necessary to invent a new name for phenomena, many of which range themselves under the old

name of *adhesion*, a word which in most cases is expressive of the effects which are ranged under it. In modern science solution is regarded as a case of adhesion overcoming cohesion, as when water adheres to and dissolves a salt, or mercury a metal. When the adhesion of the water and the cohesion of the solid balance each other, we have *saturation*.

90. The motions of creosote on the surface of water (51) illustrate these views in a striking manner. Creosote is a liquid of nearly the same density as water, with but slight adhesion to it (that is, very sparingly soluble in it); but one that readily saturates a moderate quantity of water (that is, a liquid whose cohesion soon balances the adhesion of the water), so that whatever visible action may take place between the two, admits of being renewed from time to time by increasing the quantity of the water. Creosote is slightly heavier than water (sp. gr. 1·059), but by carefully delivering a drop to the surface of water, from the end of a glass rod, it will not sink; but the under surface of the drop will be convex below the general surface of the water.

91. The motions of creosote on water were first pointed out by me in a paper published in the *Philosophical Magazine* for August, 1861, in the following terms:—

"I wonder whether it ever occurred to a chemist to place a drop of creosote on the surface of water. It presents a most singular appearance: it flattens out into a disc with a silvery reflexion of the light, and sails about on the water with some speed, while it is all the time rapidly agitated with a motion that gives it the appearance of a living creature. Its edge vibrates with rapid crispations; it darts out small globules, which immediately begin a series of motions of rotation and translation on their own account. In the meantime a silvery film of creosote spreads over the surface of the water: the parent globule and the smaller globules become less energetic; they perform a number of motions among themselves, moving about in circular or curved paths, carefully avoiding each other, and reminding one of the water insects which may be seen sporting on the surface of a pond in summer. Sometimes the larger globules will remain still, and the smaller ones will rotate in little lakes, which they seem to clear out for themselves in the film to disport in. After some time they all come to rest; but again begin to move for a time, once more to come to rest, and, it may be, again to rotate.

"Now there is evidently a struggle going on between the cohesion of the drop and the adhesion of the water. These two forces are so nicely balanced that it seems doubtful for a time which will prevail. The water tends to adhere to and diffuse the creosote; the cohesion of the creosote tends to prevent this action, and the struggle between the two is manifested by a series of vibrations which take place at the edge of the disc; the creosote tends to spread, its cohesive force struggles to prevent the spreading. Small globules, however, are constantly being torn away from the parent drop, and as these spin round and disappear, they leave a film which gradually covers more or less of the surface of the water. The motions of the parent disc and of the globules cease; but as the film becomes dissolved by the water, the motions (now very slow ones) set in again with the formation of another film, which in its turn is dissolved. But if the quantity of water be small, the globules soon cease to be disturbed, since the water has become saturated, or the adhesion of the water balances the cohesion of the globule, and hence the quiescence."

92. In a subsequent paper (*Phil. Mag.*, October 1861) the phenomena which accompany saturation are pointed out:—"If we deliver a single drop of creosote to the surface of one ounce of water in a glass dish, we may witness a struggle between cohesion and adhesion that will last about five minutes. The creosote sails about on the surface of the water, in a state of considerable agitation discharging a number of small globules on all sides, which in their turn are greatly agitated: they rotate and disappear, leaving behind them a thin silvery film. Meanwhile the parent globule diminishes in size, but preserves all the characteristics of its cohesion figure, until at length it disappears. If a second drop of creosote be now placed on the water, its behaviour will resemble that of the first in a mitigated form; it will be much less energetic, and will last a much longer time before it disappears. A third drop will remain on the surface in the form of a double convex lens sharply defined, showing that the cohesion of creosote exactly balances the adhesion of the water; or in other words, that saturation has been attained. If we now add more water, or increase its solvent

power by the addition of a drop of acetic acid, the action will set in again, and the lens will change into the cohesion figure.

"The following comparative experiments were made with fresh colourless creosote in two exactly similar shallow glasses, one containing one ounce, and the other two ounces of New River water. In such water adhesion is diminished by its mineral contents. In distilled water the phenomena are the same, but the time is diminished, a drop of creosote disappearing in five minutes instead of seven:—

| Glass No. 1.—1 oz. Water. | Min. | Glass No. 2.—2 ozs. Water. | Min. |
|---|---|---|---|
| First drop of creosote disappeared in ............... | 7 | First drop of creosote disappeared in.................. | 7 |
| Second drop disappeared in | 20 | Second drop disappeared in | $12\frac{1}{2}$ |
| Two or three small specks of creosote remained moving in circular orbits in the film. | | The disc was flatter and more vigorous than in No. 1. Towards the end the disc broke up into separate portions, which rotated with immense rapidity, and disappeared. | |
| Third drop had not disappeared after ............... | 135 | Third drop disappeared in about ......................... | 25 |
| When this drop was first placed on the water it repelled the film, and the crispations set in, although sluggishly. No volleys of small globules: the edge like that of window glass. After 1 minute it became still; after 2 minutes lenticular, sailing slowly about with occasional jerking of the edge. After 7 minutes, slowly revolving on vertical axis. After 13 minutes, slowly sailing about. After 25 minutes, a very convex lenticule. After 60 minutes, at rest and slowly evaporating. | | The disc was active and vigorous, and of good figure, shooting out volleys of minute globules. After 20 minutes it broke up into three portions, two of which were active: then one split into three or four, which were scattered to a distance; then all were still; the crispations were next slowly resumed, and after 25 minutes only a few scarcely visible globules remained. | |
| | | Fourth drop haa not disappeared after ............... | 110 |
| | | The drop was active for a few minutes, then subsided into a well shaped lenticule, which slowly disappeared by evaporation. | |

93. The varied phenomena of creosote, described above, may fairly represent and explain the motions of camphor on water. In such cases as these, and also in the cases of the oils of aniseed (50), of cloves and peppermint (51), of succinic and benzoic acids (53), and some others, there is a certain amount of solubility which assists the display of the phenomena. But there are cases where there is little or no solubility, and the surface attraction alone or the pure mechanical force of adhesion comes into play. Cases of this sort may be multiplied to almost any extent; but we give only two, namely, the behaviour of paraffine on warm water, and of benzole on cold.

94. If a small fragment of solid paraffine be placed on the surface of very hot water, it melts into a perfectly circular disc, which moves up to the sides of the vessel, from which it diffuses all over the surface of the water with remarkable rotations in different parts, producing an effect very much like the *curl pattern* in marbled paper, only the streams forming the curls are in constant motion. In fact horizontal currents are formed, which to a great extent depend on the capillarity of the sides, and the adhesion of the water, together with a tendency on the part of the paraffine to reassert its cohesion. As the water cools the currents cease: the films open into holes, and the connecting parts are shrivelled by contraction into wrinkles with a singular effect.

When the water was not so hot as in the above experiment, the fragment of paraffine melted into a disc which moved to the sides; then a portion floated off and formed a separate disc, when the first one, apparently attracted by the second, left the side, and the two discs kept playing about each other until the water became too cold to allow them to move.

95. But the most remarkable instances of vigour of surface attraction, and of floating substances for each other are afforded by solutions of camphor in benzole, chloroform, &c. I furnished a member of my family with a portion of the benzole solution, requesting to be furnished with a description of the observed phenomena, when the following graphic account was placed in my hands. The experiments have been repeated so many times since, and always with so much amusement, that they have become familiar to a number of persons,

who, with me, can bear witness to the accuracy of the following statement:—

A large drop of solution of camphor in benzole delivered on water has the appearance of a convex lens, and maintains that appearance for a considerable time. It sails slowly about, or remains nearly stationary, gradually becoming larger and flatter, until at last it is a thin clear flat disc. From the first it has had the form of a perfect circle, with a clean, well-defined edge. At last, when it is at the largest and thinnest, a remarkable change comes over this disc. It has sudden contractions, like a creature shrinking from the touch: these increase in extent and rapidity, the form continually changes, the contractions multiply, the circle becomes balloon shaped, turbot shaped, sole shaped, kidney shaped. The contractions and contortions at last become so violent as to throw off portions of the disc, or to split it up altogether into separate portions, which form smaller discs; but all of these exhibit the same symptoms, and dart about with even more than the parent agony, twisting and doubling up their little forms in a variety of ways, until the result of the whole matter comes to pass. This is the throwing out, from each disc, of a tiny film of camphor, which lies passively on the water, and dots it in an irregular manner.

A curious result ensues if a second drop be now placed on the water in the midst of these films. Every one of them immediately feels its influence, and attests it by slight spasms. The drop or lens as it appears to be, creeps gradually near a little cluster of these films, which slightly shrink, but as they come within a certain distance, become suddenly attracted, are gathered up in an instantaneous manner, and disappear underneath the lens. Thus creeping and gathering, the lens imbibes all, or nearly all the camphor films on the surface of the water. In one instance we saw it pursue and absorb, in a quiet methodical manner, every individual film, and it was not until this business was accomplished, that it began to alter in character and attend to its own affairs. Spreading out and flattening as before, it then went through similar evolutions, though scarcely so energetic as in the first drop, and when at last it split up into pieces, and these pieces threw out their tiny opaque films of camphor, these coalescing formed a cloud, or rather a beautiful dense silvery film with edges like sea-weed: this covered nearly one-third of the surface of the water. There was twice as much camphor as before, and this had all been contained in the second drop, yet without increasing its apparent size. On placing a third drop on the surface of the water, it made a rapid transit across the glass, passing through the middle of the film, absorbing what came in its way, and making a lane of clear water in the film. Soon it repeated this process, making another lane as it returned. Then it crept along, gradually absorbing the camphor. When the greater part of the film had been appropriated, the drop flattened into a thin disc, and became agitated. And here an effect became conspicuous, which had occurred in a less marked manner in the second drop. A rapid movement of the edge of the drop set in, very much resembling the movement of the creosote cohesion figure, and sending off a shower of minute globules. These poured out equally on all sides, from the circular film, and had an exceedingly pretty effect. In the midst of this the film, without any apparent cause, began to change its form, to lose its coherence, and to split up into fragments, some of which began to pour out globules in the same way, but so minute as only to be detected by very close observation.

When these motions had ceased, and the camphor film had spread again over the water, it was so large as nearly to cover the surface of the glass. A fourth drop was tried, and the effects watched, then a fifth, but the results became weaker and less marked. In all the above cases a large heavy drop of benzole was poured from a bottle; we next tried a small drop from a glass rod. When a drop was gently delivered to the surface of pure water from the end of a glass rod it spread into a thin film without assuming the lens shape; and this film had no time to assume a definite form, for it was immediately attacked by the contortions and contractions already described. These however were far more rapid and violent than before. The film split into a number of pieces, which rushed about with extraordinary velocity, dashing through each other, twisting into thread-like snaky forms and figures very much like Chinese dragons, again uniting again to separate, sometimes circular, oftener irregular, and frequently when one portion, as if tired of its vagaries, went sailing across the surface at a sober pace, two or three others dashed from the side of the glass with extreme rapidity, breaking it up, and making it as lively as themselves.

96. It is difficult to convey an idea of the strength of the solution required to produce the above effects, on account of the very varying quality of the benzole of the shops. It is never sold pure unless specially prepared so by a scientific chemist. But with ordinary benzole, some very good effects may be produced, and these vary with the proportions of camphor. A weak solution may consist of five grains of camphor in two drachms of benzole; a strong one, sixty grains of camphor in the same quantity. A drop should be placed gently on the water from the end of a glass rod. In making the solution in a glass vessel, a very beautiful phenomenon may be observed by holding the vessel containing the camphor up to the light, and pouring on the benzole. The lump of camphor will display the most charming tints of blue and orange, which will continue while it is being dissolved. This effect appears to be due to the formation of thin plates of benzole in the cracks of the camphor.

97. A solution of camphor in chloroform furnishes an admirable exaggeration of the effects due to a drop of a solution of camphor in benzole on water. It darts about with amazing rapidity, the chloroform soon disappears, and the camphor left behind commences its usual gyrations.

98. A drop of a solution of camphor in bisulphide of carbon on the surface of water formed a lens, which moved about as if uncertain what to do, when it suddenly darted across the water, and disappeared by evaporation, discharging the camphor in the form of a beautiful but peculiar film.

99. A drop of a solution of camphor in oil of bitter almonds spread out into a large film on the surface of water, but the film broke up rapidly into discs, and each disc threw out waving cilia, which set it sailing about, and it soon disappeared with change of form.

### 8. *Action of Oils, Acids, &c., on rotating Camphor.*

100. It is commonly stated that if while camphor is rotating on water, the surface be touched with an oily or greasy substance, the motion will instantly be arrested (23). It is further stated by Dutrochet that acids and alkalies have the same effect. It is worth while to inquire whether these

statements are not too general. In the first place, the fixed oils which form films on the surface of water, arrest the motions of camphor by preventing adhesion between the camphor film and the water and its evaporation in the air, two of the conditions which are indispensable to the motions of camphor on water (71—73). In the second place, an essential oil which forms a film on water may arrest the motions of camphor during its solution and evaporation, but these processes being complete, the gyratory motions of the camphor may proceed as before. If the essential oil be not sufficient to form a film over the whole surface, the motions of the camphor may not be arrested at all. For example, a piece of camphor the size of a pea was touched with a drop of oil of camphor. When placed on water the camphor moved round near the side of the glass many times, diffusing and dispersing the globules of the oil. A similar effect may be observed by touching the camphor with oil of turpentine. A large fragment of camphor was dipped into acetic acid, then placed on water, when it rotated rapidly while delivering the acid to the water. It then stopped, then made several more rapid turns as if delivering more acid, again stopped, and so on many times.

101. Camphor rotates almost as actively on the surface of acetic acid as on that of water, but there is an agitated kind of action between the acid and the camphor, and the latter soon becomes transparent and rounded by rapid solution.

102. While about six fragments of camphor were very lively on water, I touched the surface with a drop of butyric acid from the end of a glass rod. The camphor ceased to spin, and five of the fragments collected together. After about two minutes they recovered themselves, and commenced spinning with as much life as before. I now touched the water with a second drop of the acid: the camphor ceased to spin, but the fragments moved slowly for a short time in right lines, as if uncertain what to do. They then collected into five separate pieces or groups, but soon began to spin again in a lively manner. A third drop of acid stopped the camphor, but after a few minutes the rotations set in, although more sluggishly than before. By this time the solvent powers of the acid had become manifest, only three pieces of the camphor being left. The water was now left for six hours, fresh camphor was thrown in, it rotated sluggishly,

but after some time it recovered itself, and spun with tolerable vigour. A number of fresh pieces were thrown on the surface late at night. The next morning only one piece remained, and that was about the size of a pea; it was rounded and transparent from the effects of solution, and was rolling about, or rather floundering in all directions. The water was still smelling strongly of butyric acid. Hence it will be seen that this fatty volatile acid does not stop the rotations of the camphor, nor prevent evaporation from taking place fully and freely.

103. After the details related with sulphate of camphor oil (48) (82), it is scarcely necessary to remark that sulphuric acid does not arrest the motions of camphor on water. The same may be noted of hydrochloric acid, spirits of wine, and sulphuric ether; but in such cases, if too large a quantity be added to the water, the motions of the camphor cease.

104. The nitrate of camphor oil (94) when poured off from the acid smells strongly of camphor: a drop of it on water spreads out into an irregular star of solid camphor (two examples of which are given in Figs. 14 and 15), as

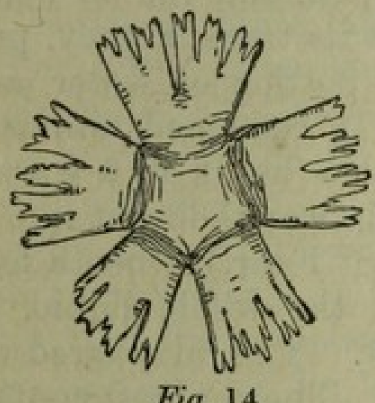


*Fig.* 14.

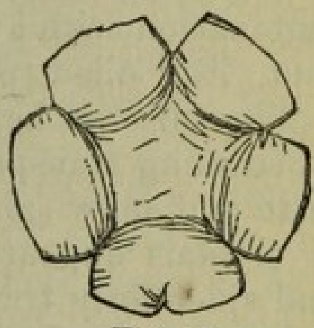


*Fig.* 15.

camphorated spirit does under like circumstances. The camphor appears to owe its liquid condition to the presence of a small quantity of nitric acid, and when the oil is brought into contact with water, the latter combines with the acid, and liberates solid camphor, as it does with the alcohol in the case of camphorated spirit. The solid camphor thus deposited disappears by solution and evaporation, discharging from its edge whole colonies of fragments, which rotate rapidly. Then the larger fragments begin to split up and sail about, throwing off from their edges minute fragments.

105. An attempt was made to separate the nitric acid

from the oil by Mr. Graham's method of dialysis. Two hundred grains of camphor in four portions were added at intervals to one drachm of nitric acid. The oil was perfectly fluid, and was poured into a cone of parchment paper, dipping into distilled water. A stream of acid descended through the water, but the camphor on the dialyser began to revive in small rounded cauliflower heads. This revived camphor sank when placed on the surface of water, but minute fragments of it rotated rapidly on the surface, each fragment being surrounded by a thin film. The oil remaining in the dialyser was from time to time tested by placing a drop on the surface of water. At first each drop flashed out into a thin sheet of solid camphor (Figs. 14, 15); but after an hour's action the oil remained as a lens on the water, and became solid more slowly. It did so at first at the edge, which was raised as if a stout thread had been run round it: then the solid camphor crept slowly inwards until the whole became solid. The lens then slowly rotated, broke up, and the parts revolved with increasing rapidity as they became smaller. When the descending stream of acid was scarcely perceptible, the process was stopped, and the oil was transferred to a test tube. It had lost its transparency, probably from combination with a minute portion of water, and was translucent, like oiled paper. It reddened litmus paper feebly. The oil was kept for two hours in a warm room without becoming transparent. It was then put into a mixture of salt and snow (about + 8° Fahr.), when a few stars of snow crystals appeared, and the whole liquid became bright and splendidly transparent. The oil poured off from the crystals remained transparent. The slightest contact with moisture produced heavy opaque camphor. So also by gently heating the oil at the flame of a spirit-lamp, it became opaque from the liberation of camphor, which quickly sublimed.

106. The effect of a minute quantity of acid on a large lump of camphor is remarkable. A lump of opaque refined camphor, weighing twenty-three grains, was touched with a single drop of nitric acid. It immediately became transparent, and had the appearance of a lump of Iceland spar, showing the cleavage planes beautifully. In a minute or two the whole surface was wet, and the lump was standing in a little pool of oil. Such a lump as this would require another drop or two of acid to liquefy it entirely.

107. Experiments of this kind are best conducted in shallow glass vessels: if in opaque ones some of the effects will not be seen. The vessels should be covered with glass plates. If left for some days a very pretty camphor vegetation, consisting of rounded masses, will creep up the sides of the vessel.

108. It will be seen that M. Dutrochet must have been labouring under some singular misapprehension when he stated that acids arrest the motions of camphor on water. There are indeed other liquids which have that effect in common with fatty oils, although they do not act quite in the same manner. Such are benzole, chloroform, naphtha, and creosote; and these by forming films of their own repel the camphor film, and arrest the camphor motions. Whereas a fatty oil will diffuse over the whole surface, and prevent the adhesion of the camphor film. On one occasion I placed a drop of naphtha on the surface of water where a fragment of camphor was spinning: the camphor flew hastily to the side of the glass, the naphtha pursued it, seized it, and rushed wildly about with it for some minutes, then all became quiet for a time, colour was developed in the naphtha film, and the camphor began to rotate rapidly in it.

109. The action of vapours upon the rotating camphor is often remarkable. If a small pellet of sponge dipped in benzole be held over the fragment of camphor, the benzole vapour will condense upon it, and streams of benzole, being seized on by the water, will react upon the camphor and produce a great increase in its rate of rotation, so much so, that the form of the camphor sometimes ceases to be visible. If the benzole sponge be held a little on one side of the camphor, the latter will dart away as if scalded. During the action of the benzole vapour the camphor exhibits vitreous lustrous points, showing that solution is proceeding rapidly under the action of the vapour. Some other vapours present phenomena similar to the above.

### 9. *Camphor on Different Surfaces.*

110. It has been remarked (94) that camphor rotates on the surface of acetic acid. Camphor sinks in most of the essential oils, but before doing so the smaller fragments rotate on the surface of some of them, although the action is

feeble. Prevost states (28) that minute fragments of camphor rotate on the surface of pure and dry mercury. I have not been able to obtain this result. Some mercury was purified by washing with dilute nitric acid during some weeks. It was drained off from the acid, shaken up with sulphuric acid, washed and shaken up with caustic potash solution, then drained off and dried, and filtered through a pin-hole into a glass that had been washed with solution of potash. The mercury was very convex with the black reflexion of highly-polished silver or speculum metal. Some minute fragments of camphor were scraped upon this surface, but there was no motion in them, except such as could be fairly traced to the attraction of the sides of the vessel, and the agitation produced by a carriage or omnibus passing the house. So sensitive is the mercury to any tremor of this kind that it is scarcely possible to preserve an unruffled surface; the house where the observation was made having a railway at the back, and a crowded thoroughfare in the front. The camphor partook of the tremor of the mercury, but it had no other motion whatever. The day was bright and fine, and the camphor currents (70) were very vigorous. I tried the experiment on another day with mercury that had been cleaned by agitation with pounded white sugar, and filtration through a pin-hole in writing-paper. The mercury was warmed by means of a spirit-lamp, but the fragments of camphor showed no signs of rotation. They evaporated freely under the influence of the higher temperature, but they did not rotate.

111. Prevost also states (28) that minute fragments of camphor rotate on any smooth hard dry surface, and that their motions can be seen under the microscope. I believe that in this case, as in the former, the observer mistook the results of some agitation or tremor of his apparatus for phenomena, which, if true, would lend great support to his theory. The reputed motions of camphor on a raft have already been examined (56—59).

## Section III.—Summary.

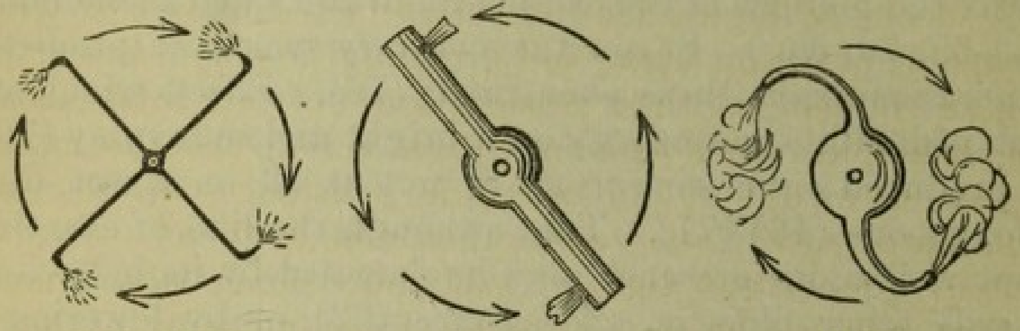
112. It will be seen from the foregoing details that the various kinds of camphor (43—47), camphoric acid, solutions of camphor in sulphuric acid and benzole (48), oil of

aniseed (50), and carbolic acid solidified by cold (51), rotate and move about on the surface of water. That creosote, oil of cloves, &c. (51), do the same, and that camphor rotates on nitric and acetic acids, &c. (51). That succinic and benzoic acids (53), the butyrates of baryta, lime and magnesia (54) rotate on water, and that inert substances such as paper, tin-foil, &c., smeared with an essential oil also rotate on the surface of water (55).

That the substances which rotate being more or less soluble, are gradually spread out in the form of films, by the adhesion of the liquid surface, such films reacting on the substances which form them produce motion (49). That the films thus formed are disposed of by evaporation and solution (65—79). That whatever interferes with evaporation lowers or arrests the motions of the camphor and the allied phenomena (65—68, 73, &c.) So on the contrary whatever promotes evaporation exalts these phenomena (76). Effects which are displayed with great energy on a bright and sunny day (76), are produced either sluggishly or not at all on a wet, dull, or foggy one (46) (77). That although the film of camphor is not visible, its presence may be detected by its behaviour towards other films on the surface (62), or by lowering a stick of camphor into water, the surface of which had previously been dusted with lycopodium powder (69). A series of horizontal currents produced by the films will in such case be made visible (70), which films or jets cause the camphor, when free to move, to rotate on a vertical axis. That the production and diffusion of these films over the surface of the water, make an incision in the stick of camphor, at the part where the water and the air meet (70—73). That the symmetrical motions of the lycopodium powder produced by the camphor can be exactly imitated by ether and other volatile substances that form films on the surface of water (86—88). That the surface attraction which spreads out the films is a powerful force of adhesion overcoming cohesion (89), and may be well illustrated by the phenomena of a drop of creosote on water (90—92), of oil of cloves, &c. (93), and of a fragment of paraffine (94), and of a solution of camphor in benzole (95), or in chloroform (97), or in bisulphide of carbon (98), or in oil of bitter almonds (99). It is further shown (100) that the motions of camphor on water, are permanently arrested by a fixed oil (107), but not by a volatile one, nor by acids (101). Further details respecting the action of sulphuric acid on camphor are given (103), and of nitric acid (104—106). The action of vapours of benzole, chloroform, &c., on rotating camphor is stated (107), and also the action of a drop of naphtha on the same surface with a fragment of camphor. The behaviour of camphor on different surfaces, including that of mercury, is stated (109).

To the question, Why does a fragment of camphor rotate on the surface of water? the short answer is, that the adhesion of the surface constantly detaches from the fragment a portion of its substance, and spreads it out into a film, which film, reacting on the fragment, produces motion after the manner of a Barker's mill.

*Fig.* 16.



ELECTRICITY. WATER. STEAM.

In Fig. 16, the principle of Barker's mill is illustrated in three forms. In the first the prime mover is electricity, in the second it is water, and in the third steam. In the first case two wires, bent at right angles, are supported by a cap upon a point on the prime conductor of an electrical machine. Each of the four arms of the wires are terminated by a point in a horizontal direction at right angles to the wire, all the points being turned the same way. When the wires are electrified, the electricity escapes from the points with such force that the recoil causes the star to spin rapidly on its centre. In the second case, water flowing down a vertical tube into a horizontal tube, supported on a vertical axis in the centre, and furnished with two holes on opposite sides, near the extremities, the recoil produced by the two issuing jets will cause the two arms to rotate. In fact, the holes being in opposite directions, the water, in escaping from them will, by its reaction, throw the pipe, with its arms and axis, into rapid rotatory motion. In the third case a similar result is brought about by steam.

# ESSAY II.

## ON THE MOTION OF CAMPHOR TOWARDS THE LIGHT.

### Section I.—History of the Subject.

1. Books of high scientific authority teach us that "light is capable of acting upon, and directing homogeneous attraction, and influencing the crystallisation of certain substances."* The examples usually given of this action are the crystallisation of camphor, and of saline substances on the most illuminated side of the vessels that contain them.

2. A property of light of such importance must have a history, whether written or not. I am not sure of having been able to trace all the steps in its career. As early, however, as 1720 M. Petit pointed out the fact† that saline solutions do not readily crystallise when shut up in the dark. In 1788 M. Chaptal published‡ a memoir concerning the action of light on the crystallisation of saline solutions. He had observed in his chemical factory at Toulouse that salts in crystallising formed creeping deposits (*vegetation saline*), on the side nearest the light, and that by covering over certain parts of the evaporating dishes, both above and below, with black taffeta, so as entirely to exclude the light,

* "Introduction to the Study of Chemical Philosophy," by Professor Daniell, 2nd Edit., p. 452. See also Professor Brande's "Manual of Chemistry," 1848, p. 11. Also Professor Miller's "Elements of Chemistry" (Vol. iii., 2nd Edition, 1862), in which it is stated (p. 540) that camphor "becomes slowly volatilised at common temperatures; and if kept in glass bottles, is gradually sublimed and condensed in octohedral crystals on the side of the vessel which is exposed to the light."

† *Histoire de l'Academie*, quoted in Hunt's "Researches on Light," 2nd edition, 1854.

‡ *Histoire de l'Academie Royale des Sciences de Toulouse.* See also *Elemens de Chimie.* Montpellier, 1790, tom. i. p. 36.

he could determine the deposits to any part of the vessel at pleasure by admitting the light to it. He found, however, that when a large number of experiments were being conducted in a small close room, they did not succeed unless provision were made for a free circulation of air. He, therefore, concluded that the concurrence of air and light were necessary to the result.

3. These facts were confirmed by one of Chaptal's laboratory pupils, M. Dorthes, who further observed that camphor, spirits of wine, water, &c., "which rise by insensible evaporation in the bottles partly filled with these substances, always condense on the most illuminated sides of the vessels."* From this time the motion of camphor, &c., towards the light was received into science as an admitted fact. No one, however, seems to have investigated the subject with a view to ascertain what rays of the spectrum were most concerned in determining the result. Count Rumford noticed that an aqueous solution of chloride of gold is decomposed by the action of light, and that small spangles of gold are collected on the side of the vessel nearest the sun. It is not necessary to refer to the decompositions by means of light, of nitrate of silver, peroxalate of iron, tartar emetic, &c., which preceded the splendid discovery of photography. No sooner was this discovery made, than, with a few exceptions, men of science had eyes only for the chemical action of light. One of these exceptions, however, requires to be noticed somewhat in detail from this circumstance, that the motion of camphor, &c., towards the light became in his hands a complicated question. About the time when Daguerre and Fox Talbot were astonishing the world with the details of their beautiful discoveries, Dr. Draper, Professor of Chemistry at New York, was investigating these camphor motions, and including them in a much wider inquiry, namely, "The Chemical Action of Solar Light."†

* Chaptal, *Elemens de Chimie.* See also *Eloge historique de M. Dorthes*, by C. L. Dumas, Director of the School of Medicine at Montpellier, contained in the *Bulletin de la Société libre des Sciences de Montpellier*, No. xl. tom. 3e, where the motion of camphor, &c., towards the light is referred to the same class of phenomena as the motion of plants confined in the dark to a small hole that admits the light.

† See Appendix to a "Treatise on the Forces which produce the Organisation of Plants." New York, 1844. The papers on the Che-

4. Dr. Draper remarks with reference to camphor, that "the action is so slow, and requires such a length of time for its completion, that no successful investigation has been made as to the nature of the forces in operation." He, therefore, contrived a method for hastening the phenomenon which was simply to exhaust the vessel containing the camphor of air, and then to place it in the sunshine. In the course of five minutes small crystalline specks appeared on the side nearest to the sun; these increased in size, and became in about two hours beautifully stellated crystals, from one-eighth to one-half of an inch in diameter. Only a few stragglers were found on other parts of the glass, and sometimes the whole side next the sun was covered with a deposit of camphor. It was also found that water in a close vessel deposited its vapour on the side of the glass nearest to the sun; that mercury in a barometer tube did the same; that iodine heated in a vessel, and then exposed to the sun, formed a deposit on the sunny side. It was further found that a ring of tin-foil, one and a-half inch in internal diameter, and half an inch wide, attached to the vessel, or even suspended half an inch from it, so as not to touch it, prevents the deposit of camphor from forming in and about the limits of such ring. Indeed, a ring attached to the glass will even remove the crystals already deposited on it. A ring of resin, on the contrary, does not prevent the formation of the camphor deposit.

5. It is also stated that camphor may be kept in *vacuo* in the dark for any length of time, and no deposit be made; but that if the vessel be brought from the dark into the sun, a deposit will be made towards the light in a very few minutes.

6. If the inside of a receiver be marked with a glass rod, the camphor will be deposited along the marks.

7. Artificial light, such as that of an Argand lamp, was not found to have any action in producing the deposits.

8. It was further found that the deposits were sometimes farthest from the sun, and at other times nearest; these are termed *aphelion* and *perihelion* movements. Dr. Draper

mical Action of Solar Light had already appeared in the *American Journal of Science*, and a summary of the experiments on camphor, under the title "An Account of some Experiments made in the South of Virginia on the Light of the Sun," was published in the *London and Edinburgh Philosophical Magazine* for February, 1840.

says :—" The sun's rays have the power of causing vapours to pass to the perihelion side of vessels in which they are confined; but as it would appear, not at all seasons of the year. For example, I have a certain glass fitted up for making these observations, and in this vessel, during December, January, and part of February 1836-7, a deposit was uniformly made towards the sun; during March, April, and part of May next following, although every part of the arrangement remained to all appearance the same, yet the camphor was deposited on the side farthest from the sun. It does not appear that any immediate cause can be assigned for this waywardness."

9. It was further found that when the sun's light was passed through water, and solutions of ammonio-sulphate of copper, and of bichromate of potash, the crystallisation was on the aphelion side.

10. The following statement is also made :—" Light which has suffered reflexion at certain angles seems to have undergone a remarkable modification, being no longer able to put the glass into such a condition that it can cause motion towards the sun." Under such circumstances crystallisation proceeds with rapidity, not on the perihelion side of the vessel, but on the opposite side. This result is not supposed to be due to polarisation as it takes place at all angles.

11. A very few words will explain Dr. Draper's theoretical views on these highly curious and interesting phenomena. In his letter to the editor of the *Philosophical Magazine*, in 1840, he endeavours to account for the perihelion deposits by a timid reference to the action of heat. A more mature opinion is given in 1844, in his quarto volume, based upon an experiment in which a vessel containing a differential thermometer was exposed to the sun, when the side nearest the sun was found to be the hottest. He says :—" We cannot admit that the rays of heat have any active part in bringing about the phenomenon." He supposes that electricity may be the efficient cause of these deposits. Let it be granted that electricity is emitted by the sun, and that when light falls on any surface, that surface becomes electrified, these effects follow: the particles of camphor are attracted by the surface, except where the tinfoil ring robs the glass of electricity, or its shadow prevents

the glass from becoming electrified. Attempts were made with very delicate instruments to detect the presence of electricity, but not the slightest indication of that force was obtained. His ultimate conclusion seems to have been that these deposits are due to a mechanical action of light. In short, the results of Dr. Draper's elaborate inquiry was to multiplv phenomena, and to leave the theory as it was.

## Section II.—Examination of Dr. Draper's Results.

12. The above is, I believe, a fair statement of Dr. Draper's experiments and views. In proceeding to verify his experiments, no use was made of the air-pump and its accompanying apparatus, but the camphor was put into common phials of white and coloured glass. In some preliminary trials made by the sea-side in the Easter vacation of 1862, an ounce of camphor was distributed among half a dozen phials, white, light green, and dark purple. They were well corked, and placed at night on the ledge of a window frame, on the second floor, facing the sea with a south aspect. An arrow (*a*, Fig. 1) was marked on the top of each cork, the point of which was towards the light.* The next day (24th April) was cloudy: there was an abundance of light, but no sun. The result was in all the bottles a faint nebulous mass of small crystals nearest to the light. This effect was noticed at 10 o'clock A.M., and the deposit did not increase during the day. The 25th was a bright sunny day, and the result was an abundant deposit of crystals in all the bottles; in the white glass phials the crystals were distributed over nearly the whole of the interior surface, front and back, and at the sides; in the green glass the deposit was mostly at the back and sides, scarcely any being in front nearest the light; while in the purple bottle the deposit was farthest from the light. The morning of the 26th was wet: the green glass bottle had a deposit in the front as well as at the back: the white glass had also

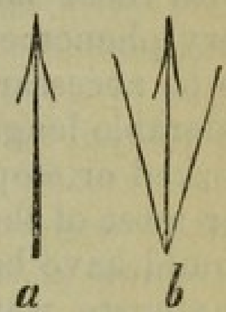


*Fig.* 1.

* The double arrow (*b*, Fig. 1) refers to a subsequent experiment (60).

front and back deposits: and in the purple the crystals were creeping round to the front.

13. On returning to town some camphor bottles were examined, which, nine days before, had been tied up in brown paper with a piece of stained glass in each wrapper, the paper being cut away in the place where the coloured glass was situated. The bottles had been left on a desk, near a window on which the morning sun shone. The colours were red, yellow, blue, and green, and in all four cases, there was an abundant deposit farthest from the light. To see what time was required for the formation of the deposit, a bottle containing camphor, covered with an opaque screen, except where a piece of green glass formed a window, was placed in the sun one afternoon towards the end of April. In less than two hours the screen was removed, and an abundant crop of crystals was found on the side farthest from the light.

14. Bottles containing camphor, which had been planted in windows facing the east, did not agree in their results with those facing the west. Indeed a variety of contradictory phenomena soon appeared, to reconcile which it seemed to be necessary to make repeated observations over a considerable length of time. Common refined camphor, in well-corked or stoppled bottles, seemed to be sufficiently delicate for most of the purposes of such an inquiry. I dare say it would have been more sensitive in exhausted receivers, but having to watch no less than twenty bottles every day, it was desirable to avoid the expense and inconvenience of so many air-pump receivers, or capped vessels, and the use of the air-pump or exhausting syringe (4). It occurred to me that crude camphor might be more sensitive than refined. I had already, before leaving town, put about 50 grains of crude camphor at the bottom of a tall glass, 16 inches in height, and 2 inches in diameter, and left it uncovered. My idea was that as the vapour of camphor has a low tension, a deposit would probably be made in so deep a vessel. It was left near an east window during nine days, and then examined, but not the faintest trace of a deposit was visible. It was then covered with a glass plate, and in the course of a few hours there was an abundant deposit of delicate needle-shaped crystals, extending all up the glass on the side farthest from the light.

15. This experiment shows that although the elastic force of camphor vapour may be small,* its diffusive power is considerable. It requires, therefore, to be confined in the vessel in order to produce a deposit, but this confinement does not necessarily imply close corking or stoppling; for if the bottle have a very narrow neck it may be left open, and good deposits will be produced. For example, a square bottle (an air-pump "breaking square"), containing crude camphor has been upwards of three months in an east window exposed to the morning sun, and although uncorked all this time, it has good deposits on three of its sides. The diameter of the neck is three-tenths of an inch.

16. Several comparative experiments, made with crude camphor and refined camphor, resulted in favour of the greater sensitiveness of the former. A six-ounce or eight-ounce white glass phial, containing a few grains of crude camphor placed in the sunshine, will show a deposit in the course of a few minutes. Refined camphor will be longer in producing a deposit, and when produced, it will be less decided. If time, however, be allowed, the refined camphor produces equally beautiful and dense deposits, and as great a variety in the patterns of the figures.

17. In order to produce reliable results in an inquiry of this kind, I thought it necessary to extend the observations over a considerable length of time, so as to be able to watch the effects of sunshine and shade, and the varying temperatures of this variable climate. For this purpose a number of bottles and glass vessels were arranged in the windows of some of the rooms and the staircase of my house, which has an aspect nearly east and west. I found amusement in watching the growth of the very beautiful figures produced, varied in character and detail as they are by minute circumstances, and changing as they do from day to day, with every change of weather. It is necessary, therefore, I repeat, to watch the phenomena over a considerable length of time to bring them within the influence of known laws; otherwise, we shall obtain merely such startling results as surprised Dr. Draper, and left him apparently more be-

* According to Saussure (*Annales de Chimie et de Physique*, tom. xiii. 1820) the elastic force of camphor vapour at $15\frac{1}{2}°$ C. = 4 millimètres of mercury, or 59·9° F. = ·15748 inch.

wildered at the close of his inquiry than he was at the beginning.

18. I must confess that in repeating Dr. Draper's experiments, the whole subject of these camphor deposits seemed to me to be beset with difficulties. I not only obtained his results, which seemed at first very surprising, but others that were equally so. For example, during about a week of fine weather, early in May, the bottles in the east windows showed deposits farthest from the light, while those in the west windows were nearest to the light. The mind is so apt to generalise, and is so pleased with a neat result, that it required some courage to resist such a conclusion as this, namely, that the rising sun produces aphelion deposits, and the declining sun perihelion deposits. But on testing the result more closely, its unsoundness was detected. For this purpose four quart stoppled bottles, A, B, C, D, were supplied each with 100 grains of crude camphor in coarse powder, and placed as follows :—A in the west window on the inside, B in the west window on the balcony outside, C in the east window, and D outside the east window. The bottles were placed in position at noon, on the 7th May. In about twenty minutes D showed a deposit of very minute stars nearest to the light; but rain coming on, water penetrated into the two outer bottles. They were, therefore, cleaned, re-charged, and tied over with india-rubber cloth. Deposits were obtained in all four bottles, varying in position and character, as will be hereafter noticed; but this fact was clearly established, that while one of the indoor bottles was forming a deposit farthest from the light, the outside bottle had its deposit nearest to the light, and *vice versâ*. In fact, the two bottles, within a few feet of each other, bore contradictory evidence. They were both placed under the same circumstances as to light, but with this important difference as to weather—that one was protected from it, and the other exposed to it. In fact there was no escaping from the conclusion, notwithstanding Dr. Draper's protest (11), that heat is largely concerned in the production of the phenomena. It appeared in these early trials to be clearly made out, that the heat which raised the camphor in vapour would, if beyond a certain temperature, so far overcome the attractive energy of the light as to drive the vapour to the coldest surface of the glass, or that farthest from the source which

supplied both the light and the heat, where it would condense after the manner of dew.

19. To test this idea all that seemed necessary was to expose the vessel containing the camphor to the heating action of the sun apart from its light. Accordingly, two cylindrical glass jars, A and B, 7 inches high, and 2¼ inches in diameter, were charged with about 100 grains of crude camphor; their mouths were closed with bungs covered with tin-foil, and they were so enclosed in well-fitting canisters of tinned iron. The top of each bung was marked with an arrow, the point of which was made to coincide with a notch at the top of the tinned iron case, and a corresponding arrow was marked on the top of the cover, so that it was easy to place the canisters and to examine the results. A was put in the west, and B in the east window on the morning of the 8th May. They were examined at 3 P.M., the temperature in the west window being 70°. A exhibited a faint deposit of crystals on the side of the glass farthest from the light, that is on the coldest part of the jar; B gave no result, the sun having left the window before this canister was placed. The next morning at nine o'clock, B was again examined, but again no result. Between this hour and 10.30, there was some sunshine, and on again examining the glass a faint deposit was found farthest from the light. The two jars were examined from day to day during some weeks. The results were very variable. The deposits formed one day would generally disappear during the night, and not be formed again for some days, unless there had been sufficient sun (*i. e.* sufficient warmth) to raise enough vapour to produce a deposit. In no case where the light was excluded was a deposit formed on any other part of the glass except that farthest from the source of heat.

20. In these early experiments I could not account for the scantiness of the deposits on any other ground than the removal of the stimulus of light. I scarcely ventured to suppose that the statement made by so many distinguished men, that light determines the motion of these crystals towards itself, had no foundation. I rather proceeded to multiply and record facts, being quite sure that these would speak far more eloquently than any theory. I learnt from my canister experiments that the heat of the sun, apart from its light, will produce these deposits. Why not then the

heat of a fire? Accordingly a small bottle, containing a few grains of crude camphor, was placed before the fire on a very dark foggy morning (12th May). In less than an hour small bright crystalline dots appeared in great number on the side of the bottle farthest from the fire.

21. Four eight-ounce phials, A, B, C, D, were charged; A with refined, and the others with crude camphor. A was covered with tin-foil, B was enclosed in a tin canister, C was covered with brown paper, and D was left naked. They were all arranged around a heated cannon-ball in a darkened room, at such distances as to be exposed to an initial temperature of 90°. In less than half an hour, C and D exhibited copious deposits of small crystals farthest from the source of heat. In an hour and a half A had a very faint farthest deposit, B no deposit, the heat not having been maintained sufficiently long to penetrate the metal. The apparatus remained undisturbed until the next morning, when the naked bottle D, being put on the window ledge in the sunshine, there was an almost immediate deposit nearest to the light. The bottle A, still covered with tin-foil, was also put into the sun, but no deposit was found after five hours' exposure, the heat being reflected from the surface.

22. On thinking over the results of these experiments, it became evident that the action of heat was important. The bright tin-foil reflected the heat, and prevented a deposit from forming; the brown paper absorbed the heat on the side exposed to the source, but remained comparatively cool on the opposite side where the deposit was most abundant; the glass in the tin canister, exposed to the continued action of the sun, had a faint deposit farthest from the source, because the canister not being very bright, became by its good conducting power almost equally hot in every part, and kept the glass inside at about the same temperature. There was a *slight* difference in temperature on the farthest side, and here was a slight deposit.

23. All this seemed to be satisfactory, but how did it consist with the action of reflected light in producing farthest deposits? I had already obtained results which seemed to confirm Dr. Draper's statement (10). For example, four flat-sided bottles were placed in a bookcase, 19 feet from the window of a room which received the reflected light of some light-coloured houses on the opposite side of the road.

All these bottles had farthest deposits. As the season advanced, and the foliage of some trees cut off the view of these houses, other bottles of camphor similarly placed, had no deposits at all, even after two months' exposure. This result was at first attributed to the cutting off of the reflected light. The real explanation will be given further on (51).

24. In other cases where farthest deposits had been obtained, the bottles stood on the window ledge on white paper. This was replaced by dark paper, and still the farthest deposits were obtained.

25. Then came the question as to whether the quantity of camphor had anything to do with the production of deposits farthest from the light? My general plan was to cover or nearly cover the bottom of the bottle with camphor. A much smaller quantity was now used. A six-ounce phial, charged with only three grains of camphor, was placed in the sun. In about ten or fifteen minutes this had a copious farthest deposit. Then it occurred to me that the form of the bottle might influence the result by the reflection of the light from the convex inner surface of the bottom. To determine this, two grains of crude camphor were put into a six-ounce phial, and this into a well-fitting wooden box which concealed from the light a length of two inches of the lower part of the bottle. After a few minutes' exposure to the sun, a deposit was obtained farthest from the light. In two hours the deposit had become copious: it was high up at the back of the bottle, there being no deposit in the part within the box or an inch above it.

26. In such cases as these it again became evident that heat is largely concerned in producing the farthest deposit; that in fact, where the sunshine is fierce, it overrides the attraction of the light for the vapour of camphor. The vapour under such circumstances obeys the ordinary laws of cooling, and is condensed on the coldest part of the glass.

27. Still, however, it remained to be proved that reflected light, when not accompanied by too high a temperature, will, contrary to Dr. Draper's experience (10), produce a deposit nearest to the light. On the night of the 8th May a white glass bottle, containing camphor, was furnished with a brown paper hood so as to cover it completely. A slit was then cut out, $3\frac{1}{2}$ inches in length, and $\frac{3}{4}$ inch in width, extending from near the top to the bottom of the bottle. A

looking-glass was placed near to, and facing an east window, and a few inches off, the covered bottle with the slit opposite the looking-glass. The next morning was wet and cloudy with occasional bursts of sunshine, but in the course of two hours, a deposit was formed nearest the slit, and consequently nearest the reflected light. The experiment was repeated over and over again with the same result, modified in this way, that if the morning sun were unusually hot, the deposit was scattered over a considerable portion of the interior of the bottle, but in such cases the largest crystals were opposite the slit. A bottle was wrapped up in tin-foil, and a slit, only one-tenth of an inch wide, cut out. The deposit was in this case confined to the slit and its vicinity. The results of these and many other experiments satisfied me that the "remarkable modification" which Dr. Draper supposes reflected light to have undergone, whereby it produces only aphelion deposits, is really an effect of heat capable of easy explanation.

28. The experiments on the protecting action of tin-foil rings (4), though not very original in their conception, produce admirable results. Had Dr. Draper been acquainted with Prevost's experiments on dew,* and those of Carena on hoar frost,† he would have seen how tin-foil favours or prevents the deposition of moisture on glass, according as its position favours or obstructs the radiation of heat. He would, moreover, have seen that his electrical theory (11) is of no value in explaining phenomena which, whether the action of light be admitted or excluded, fall under the operation of those great laws which regulate the formation of dew.

29. It is true also that the vapour of water and of iodine move towards the light, but only under certain circumstances. Dr. Draper's method of performing the iodine experiment (4) is, I think, characteristic of his general mode of inquiry. He raises the iodine in vapour, and then places the vessel containing it in the sunshine. That is, he hastens the result, and is satisfied when it is attained. Had the iodine been exposed to the varying influences of heat and cold, sunshine and shade, it would have been found that the deposit, like that of camphor, and also of water, &c., is sometimes made towards the light, and at other times away from

* *Annales de Chimie.* An. xi. 1802-3.

† *Memoires de l'Academie Royale des Sciences de Turin.* 1813-14.

it. An experiment should speak with many tongues, and in order to enable it to do so, it must be repeated many times under circumstances sufficiently varied to detect the regulating law, and to eliminate disturbing causes.

## Section III.—A New Theory Proposed.

30. In the above examination of Dr. Draper's results, the inquiry became divested of some of its marvellous features, such as the action of the sun in producing deposits nearest to the light at one season, and farthest from the light at another (8). The action of reflected light in producing farthest deposits only (10), was disproved, and the function of heat in promoting some of the results was also established. I made many experiments with the view to reconcile the mutual action of heat and light in producing these deposits; but they were all failures. Now, as failures are more eloquent to him who fails than to him who reads, it is not necessary to describe them here. So long as I looked for the action of light and heat, the results were full of anomalies. No sooner did I cease to regard light as having anything to do with the matter than all became clear. Let it be granted that camphor and other substances capable of being raised in vapour at ordinary temperatures, become, on a reduction of temperature, condensed on the coldest side of the vessel, after the manner of dew, and all the varied phenomena range themselves in the most orderly manner under the two well-known laws of *radiation of heat*, whereby a surface, or a portion of a surface, becomes colder than the vapour in contact with it; and, secondly, *condensation of vapour* by the contact of the colder body. That is the theory which I have now to support by experiment and observation.

31. But first as to the substances used. The same general result of motion to or from the light, according to the temperature, belongs not only to the vapour of ordinary laurel camphor ($C_{20} H_{16} O_2$), but also to that of Borneo camphor ($C_{20} H_{18} O_2$), and of artificial camphor from oil of turpentine ($C_{20} H_{16} HCl$).* An aqueous solution of camphor (camphor

* The Borneo camphor was from a sample supplied to my friend, Professor Miller, by the late Dr. Royle. The turpentine camphor was prepared by Professor Miller.

julep) exposed in small quantities to the light, forms various figures and deposits. The figures of precipitated camphor are often taken up again by the liquid, and deposits of vapour of water only may be formed. In some cases the deposit causes exactly one half the glass to appear as if it had been ground. At other times only a long narrow oval deposit is made.

32. Perhaps the most charming effects are those produced by oil of camphor, the elaiopten of Borneo camphor. A thin layer of this oil at the bottom of a long cylindrical vessel exposed to the light, fills the confined space with its vapour, combines with oxygen, and becomes solid camphor, which forms beautiful stars and fern-like figures on the side of the glass (Fig. 2 represents groups of the stellar varieties).

*Fig.* 2. Oil of Camphor Figures.

33. The figures of the various deposits of camphor are hexagonal segments of octahedra with bevelled edges. If a small cube of soap be cut into the form of an octahedron, and a section be made through the middle of six faces parallel with the two remaining faces, hexagons are obtained with bevelled edges. The dotted lines in Fig. 3 show this. The hexagonal plates when isolated are often very perfect (as in Fig. 4), but they are frequently crowded together (Fig. 5), and become distorted.

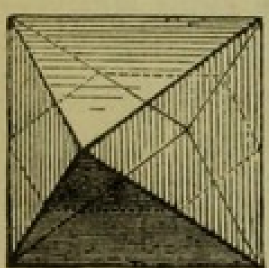


*Fig.* 3.

The presence of a little vapour of naphtha, ether, alcohol, bisulphide of carbon, nitric acid, &c., in the bottle, modifies

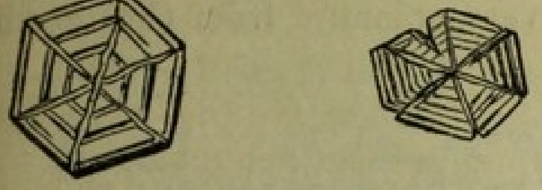


*Fig.* 4. Hexagonal Plates of refined Camphor.

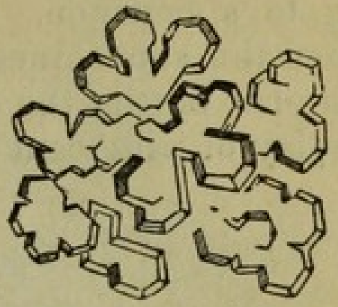


*Fig.* 5. Group of the same.

the figures in a remarkable manner, producing an endless

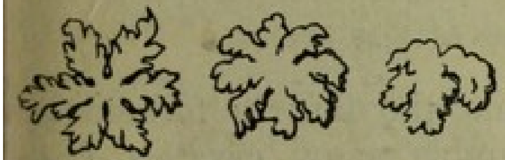


*Fig.* 6. Crude Camphor touched with Wood Spirit.

*Fig.* 7. Crude Camphor touched with Naphtha.

variety of camphor foliage. (See Figs. 6 to 9.) The general

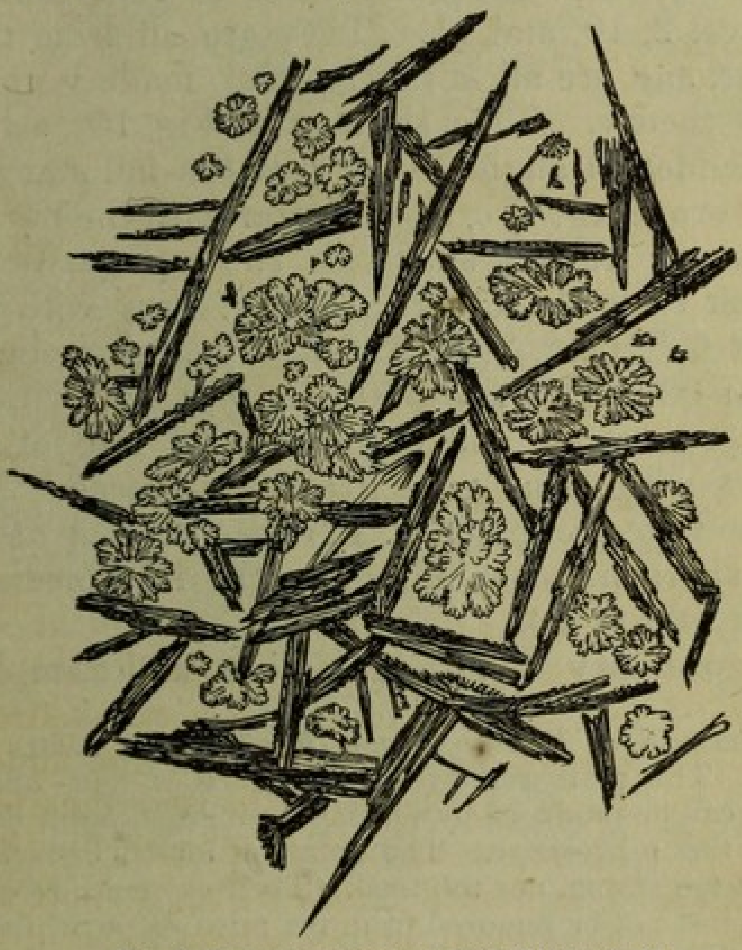


*Fig.* 8. Camphor touched with Nitric Acid.

appearance of the long crystals of Fig. 8 when magnified are shown in Fig. 9, and also of the rounded groups with a terminating plate here and there, showing a form approaching to a hexagon. The crystals are highly refractive, and when the sun shines on a window containing a number of camphor bottles the effect is very splendid from the dazzling display of iridescent light.

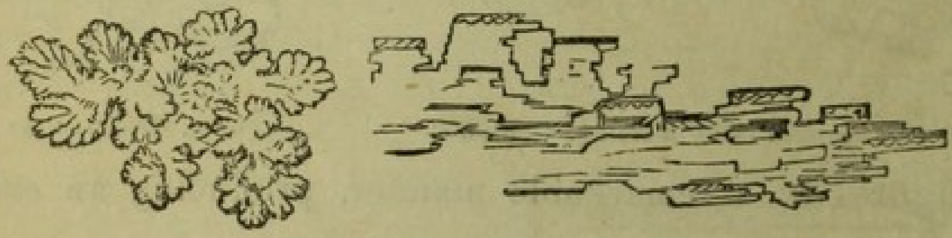


*Fig.* 9. Enlarged portions of Fig. 8.

34. It is no new observation that the figures formed by camphor are six-sided, and resemble snow crystals. (See Fig. 2.)* But I think that any one who has seen the figures produced by oil of camphor will be struck by their extreme beauty. An attempt has been made to represent some of these in Figs. 2, 10, and 11. They are all from the side of one glass jar, and are all faithful copies, made without reference to any theory. They show at *a* (Fig. 10), a full star of six rays, inclined at angles of 60°; at *b*, a full star with supplementary branches; at *c*, another star with the branches still more produced; at *d*, a star with two rays (at 0° and 180° much shorter than the others; at *e*, *f*, stars with only four rays, viz., at 60°, 120°, 240°, and 300°; at *g*, a star of three rays, viz., at 60°, 180°, and 300°; at *h*, a star with scarcely more than two rays, and indications of two other rays; at *i*, *k*, stars with some rays greatly produced. The main lines in the two groups (Fig. 11) are inclined at angles of 120°, with short branches filling up the intermediate angles of 60°, as at *m*, *n n*, *o o*.

35. The ordinary "storm-glass" is an example of the

* See Ledermüller *Microscopische Ergötzungen*, 1760. Seite 77. Tab. xxxix. The plate represents a number of four- and six-rayed stars. They can be produced by placing a single drop of camphorated spirit under the microscope. The solution must be of a particular strength—not too strong, nor too weak. Their existence is very brief, so that the eye must not be removed from the microscope while the action is going on. They are compared to snow crystals (*schnee-flocken*). A French translation of this work appeared at Nuremberg, in 1764, under the title of *Amusement Microscopique.*

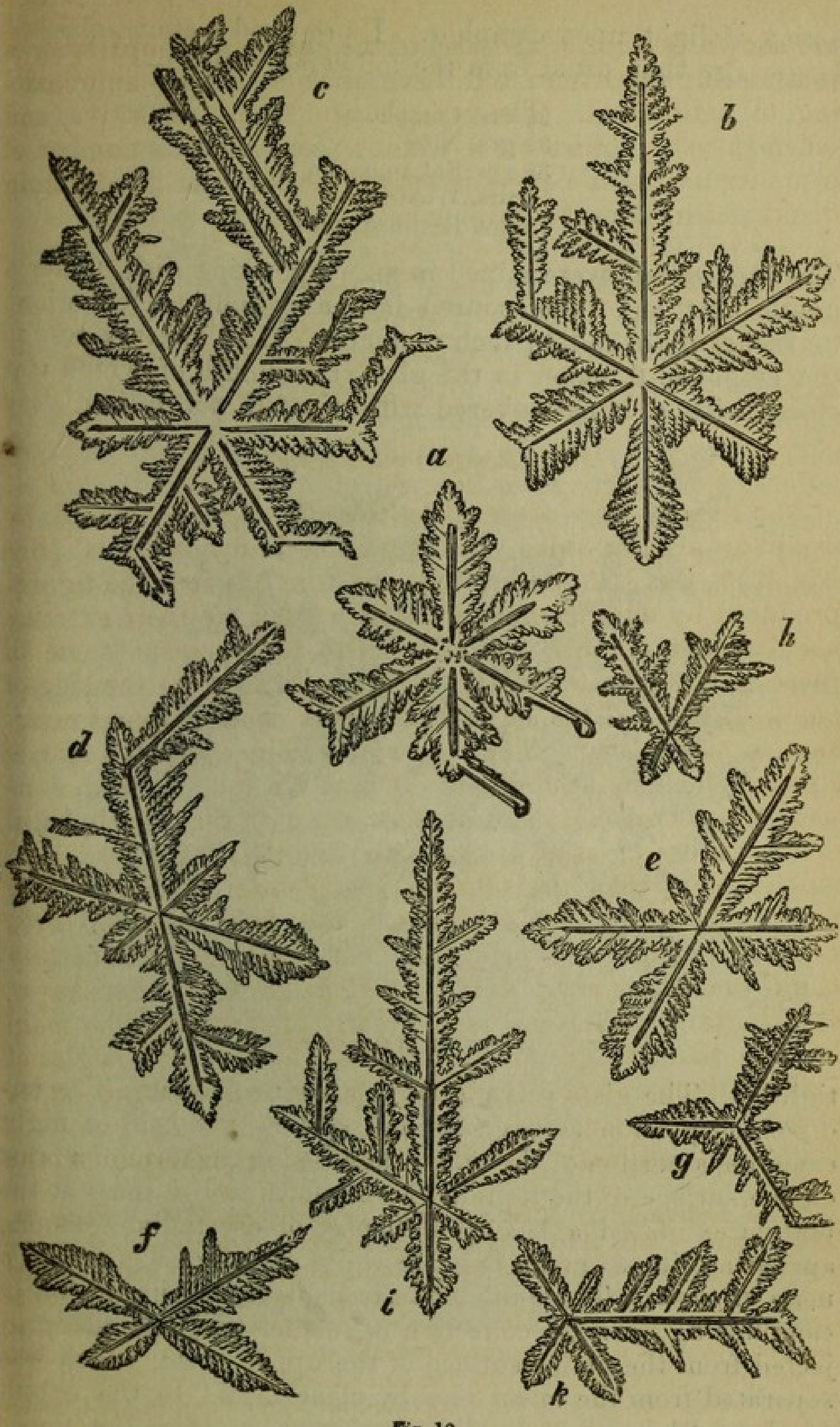



Fig. 10.

action of light upon camphor. I prepared a storm-glass of larger size than usual from the following recipe :—

$2\frac{1}{2}$ drs. Camphor.
38 grs. Nitre.
38 grs. Sal ammonia.
9 drs. Water.
11 drs. Rectified spirit.

The ingredients were mixed in an evaporating dish, under a gentle heat, and then poured into a cylindrical foot-glass, $9\frac{1}{2}$ inches high, and $1\frac{1}{4}$ inch diameter. The mixture rose to the height of 6 inches in the glass, the mouth of which was closed with a bung covered with tin-foil, not fitting very

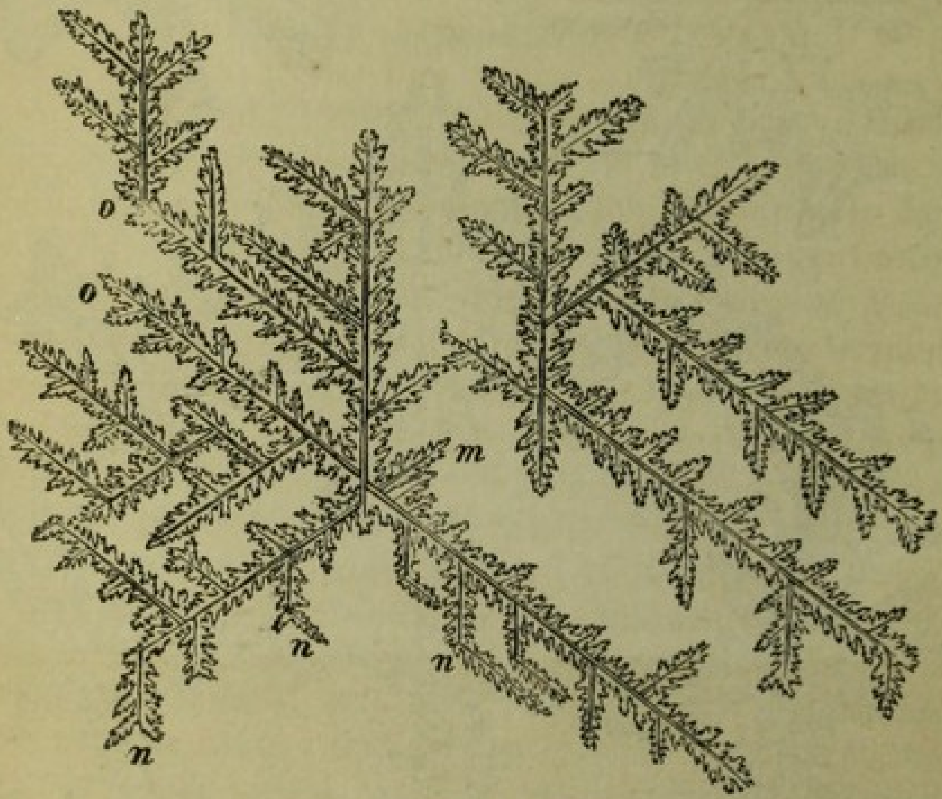



*Fig.* 11. Oil of Camphor.

tightly. The glass was placed in a west window. The undissolved portion slowly subsided, and as the cold of night came on, a portion of the solid condensed in the form of a crust on the surface of the liquid. Next morning the mass at the bottom exhibited a beautiful display of crystalline feathers, appearing like so many delicate white ferns growing from a mass of snow. The quills were transparent as in the oil of camphor figures. A collection of smaller feathers also projected from the under surface of the upper crust, which was separated from the lower one by clear liquid, to the extent of about $2\frac{1}{2}$ inches. Sublimation went on from the upper crust, and covered the dry upper surface of the glass nearest the light with camphor figures, consisting of minute stars and lines. When the sun shone upon the window the feathers at the bottom, the crust at the top, and the dry camphor deposit, all disappeared by solution and evaporation. The next morning was cold and cloudy, large woolly masses were heaped up from below, and downy masses in the space between the upper crust and the mass below. As the day improved, increased evaporation went on from the upper crust, the result of which was the formation of feathers which, when complete, were pushed down by other feathers in the act of forming, and, slowly descending, the clear space between the two deposits became heaped up at the bottom. The change from cloudy to bright and windy weather was often marked by ascending and descending currents of exquisite little stars of 4, 6, and 8 radii, variously formed, some being branched or toothed, others curved, others in pairs alternately long and short.

36. The phenomena presented by the storm-glass are singularly varied and beautiful, changing every hour with the changing aspect of the sky, the position of the sun, and the greater or less amount of cloud. As a weather indicator it does not appear to be of much value: it rather follows the weather than precedes it, and seems to depend for its action entirely upon variations in temperature.

37. Naphthaline, in an eight-ounce phial, exposed to the

*Fig.* 12. Naphthaline.

sunshine, formed a deposit nearest to the light. The figures depend somewhat upon the temperature, but a common form is that of a cluster of needles surrounded by rounded transparent plates, apparently due to the fusion of some of the needles. These figures resemble flies that have been flattened between the leaves of a book. As the season advanced, deposits farthest from the light were formed, and thin crystalline plates, with well-defined rounded edges projected inwards; they had exactly the texture and appearance of insects' wings. (See Fig. 12.)

38. Iodine produces admirable results, provided they are not hurried. Dr. Draper's plan (4) of raising the iodine in vapour by means of artificial heat, and then placing it in the sunshine, is not a good one. A small quantity of iodine should be taken, scarcely sufficient to cover the bottom of a quart white glass stoppled bottle, made clean and dry. The bottle should be placed in the window so as to be occasionally exposed to the direct rays of the sun. A bottle so placed, on the 17th May, under the influence of the afternoon's sun, had an abundant deposit farthest from the light, and a scanty one nearest to it. The next morning the deposit nearest to the light was abundant, and went on increasing during the day, which, though occasionally obscured, was warm. The deposit was made up of very perfect acute

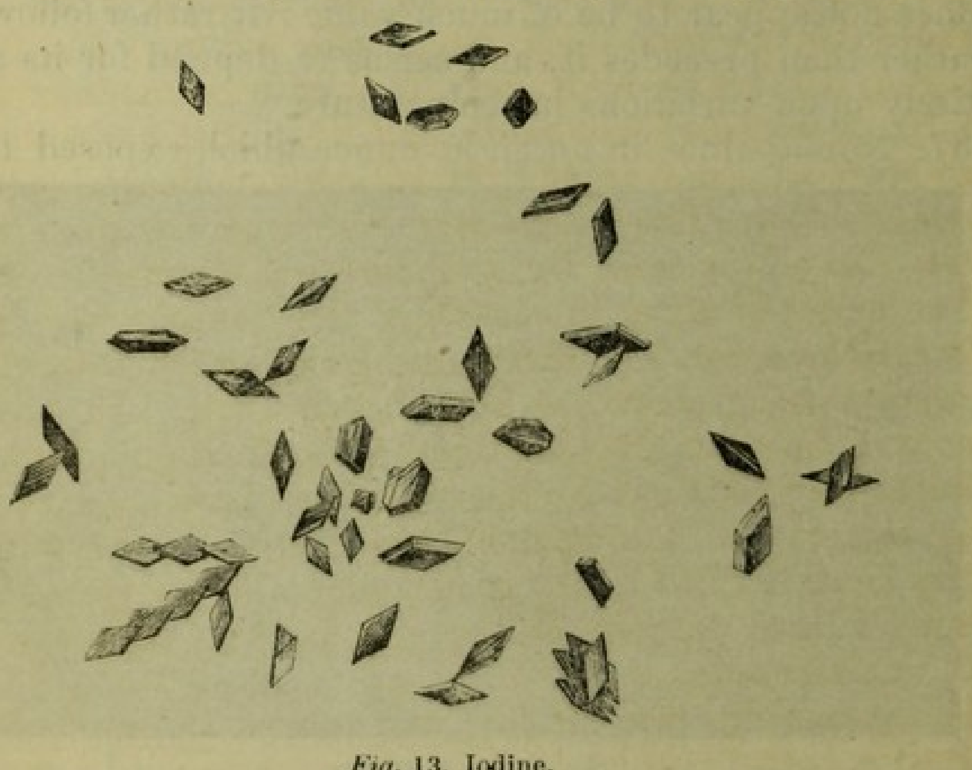


*Fig.* 13. Iodine.

rhomboids. They gradually increased in size during a few weeks, and many of the crystals were upwards of one-sixth of an inch in length. They then became deformed by accretions of other crystals, which, collected in strings by the junction of the axes, or bristled with points from the growing out of smaller crystals. (See Fig. 13.)

39. Mercury, in a tall narrow glass, was exposed to the light during many months, but no satisfactory results were obtained, for reasons which will be given hereafter (62).

40. Chloral shows the general fact very well. A specimen in Professor Miller's laboratory at the time of observation was liquid at the bottom of the bottle, with a solid deposit on the side nearest the light.

41. Sesquichloride of carbon also forms a deposit on the side nearest the light.

42. The vapour of water also forms deposits towards the light. Dr. Draper remarks, that if a closed vessel, containing a little water, be placed in the sunshine, dew will be formed in a few minutes nearest the source of light, and increase in quantity, so that drops will trickle down. Had Dr. Draper made this observation many times, he would have seen that the deposit is as often farthest from as nearest to the light.

43. I tried the experiment with water on a large scale, and under varied circumstances. For which purpose six quart stoppled bottles were filled, No. 1 with oxygen, No. 2 with nitrogen, No. 3 with hydrogen, No. 4 with binoxide of nitrogen, No. 5 with carbonic oxide, and No. 6 with carbonic acid. They were all tied over, and placed in a row on a table before a west window. The bottles were new from the glass warehouse. In all six cases the moisture was supplied by about half an ounce of water in each, and the deposits were all of different character. In the morning they were towards the light, occupying about one half of each bottle. In some the dew was fine, in others coarse, with furrows of various breadths ploughed out by the weeping tears. As the sun came round, and the heat of the afternoon was felt, the bottles became filled with vapour, which in the course of the evening formed deposits on the side farthest from the light or the coldest side.

44. Suspecting that the fine, and coarse, and mottled, and otherwise varied texture of the dew, arose from the presence of an organic film on the surface of the glass, other bottles were procured, and washed out with strong sulphuric acid,

and then rinsed with copious effusions of clean water. Three bottles were filled with air, hydrogen, and carbonic acid respectively, and after being exposed to the light for some hours of the afternoon, the effects were certainly such as to warrant the conclusion that the deposits of moisture are the same in all cases, provided the receiving surface be chemically clean. Here then was another proof of the difficulty of getting a chemically clean surface, unless specially sought for. The six bottles used in the former experiment were new. The three in the subsequent experiment were new also, only specially prepared. We see, then, that while philosophers, from the time of De Mairan,* have carefully studied and figured the deposits of dew and hoar frost,† they little suspected that imperfect adhesion was to be reckoned among the causes which produce variety in those beautiful meteors. On a chemically clean surface, not dew, but a sheet of water, not icy feathers, but a plate of ice, would be formed. Dr. Draper noticed (6) that if characters were traced with a blunt point on the inside of a bottle containing camphor, the deposit would trace out the boundaries of those characters. A similar fact was noticed by De Mairan with respect to frost on the window-pane, and he has given a drawing representing it.‡ The fact is, when we write with a blunt point on glass, we either communicate an organic trace to it, or we disturb the organic film already existing, thereby raising a ridge which catches the condensing vapour more readily than the sunken parts of the film, so that the tracing thus becomes visible. In a chemically clean bottle the deposits follow certain lines and markings existing in the glass, and often reveal its structure in a remarkable manner, detecting lines and striæ which would be otherwise quite invisible to the eye.

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

† In the plates which illustrate Mr. Harvey's article on Meteorology in the *Encyclopædia Metropolitana*, a large number of figures are given showing some of the modes in which moisture condenses on glass, metal, &c.

‡ The frost was deposited in spiral lines, produced, as De Mairan supposed, by the fine sand or ashes used in cleaning the windows. To test this notion 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 formed in the direction of the lines or furrows produced by the friction.

45. Lastly, the substances used in this inquiry were saline solutions. I had such confidence in Chaptal's results (2), indorsed as they were by the authoritative name of Professor Daniell, that I did not repeat his experiments, until I had ceased to regard the action of light as having anything to do with these deposits. I then repeated Chaptal's experiments with some variations in the form, and naturally so, because as the form of an experiment is but the expression of the thought which produced it, the one will vary with the other, as vessels cast in moulds of different pattern will vary. Chaptal knew nothing of the laws of radiant heat, and next to nothing of hygrometry. He obtained his results, as he thought, by shutting out the light, when in fact he was merely preventing cooling by radiation. I obtain results identical with his in transparent vessels in the full sunshine, by preventing radiation and evaporation in one half of each vessel by covering it with a thin plate of glass or of mica. In such cases the exposed half of the vessel had a crystalline capillary deposit running round it, and increasing from day to day, and even passing over the edge of the glass, and covering the outside, while on the covered portion there was no deposit whatever, or only a faint one, after some days' exposure.*

46. Indeed it is not necessary that the covering glass plate should be in contact with the glass; if it only overshadow it, or be suspended over it, the preventive action is equally produced. I have had a solution of bichromate of potash in one vessel, and a solution of sulphate of iron in a similar vessel at a lower level (Fig. 14). The glass, which partly covered the first, projected over the second vessel without touching it, and in both cases, that is in the covered half of one, and the overshadowed half of the other, there were no deposits, although there were abundant ones on the uncovered sides of both glasses.

* Acetate of lime (formed by saturating acetic acid with lime, and filtering) is admirably adapted for showing these experiments in the most striking manner. Large rounded cauliflower masses of great beauty form on the uncovered half of the glass, swell over and form on the outside: they are first white, but by exposure to the sun, become of a delicate yellow, touched with a deep brown on the most projecting portions, while the solid in the dish is striated after the manner of certain fibrous minerals. Chaptal points out *le sel aceteux calcaire* as producing good results.

47. Chaptal recognised the fact (2) that when a large number of evaporating dishes were arranged according to his method in a small close room, no results were produced.

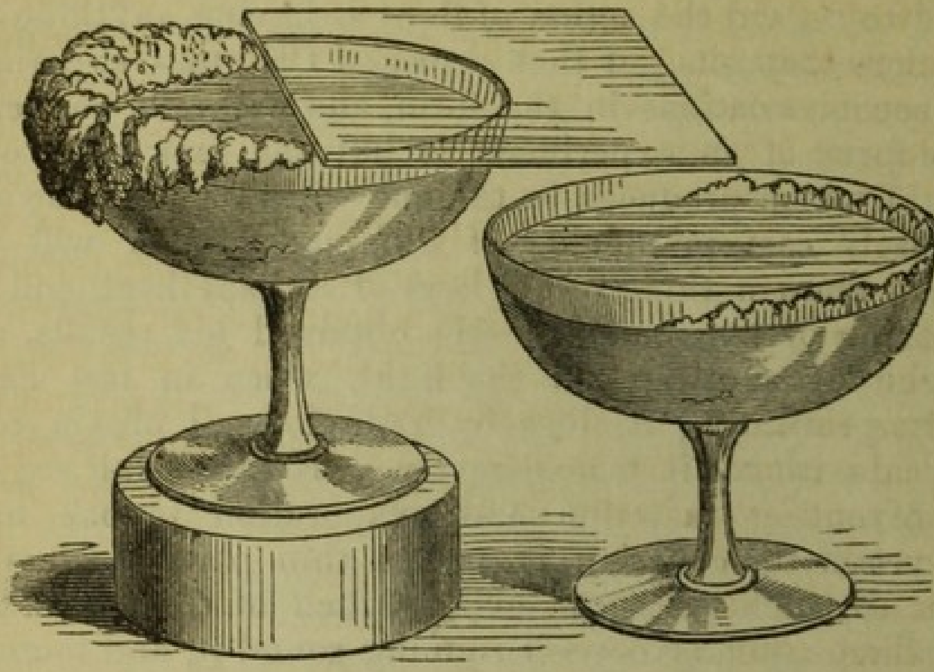


*Fig.* 14. Acetate of Lime and Sulphate of Iron Solutions.

He would have succeeded had he contrived some method of keeping the air dry. I put a solution of sulphate of copper, partly covered with a glass plate, upon a shelf in a dark cupboard. No crystals were formed after many days, on account of the enclosed space becoming saturated with moisture, and thus preventing further evaporation. To prove that the absence of light had nothing to do with the result, a capsule, containing a few lumps of caustic potash, was placed by the side of the solution, and the whole was covered with a bell glass. In the course of a few hours the creeping crystals began to form on the side of the vessel not covered with the glass plate, just as in the case of similar solutions exposed to the light. Identical results were obtained when solutions of sulphate of zinc and of sulphate of copper were placed in the dark enclosed space without being covered with the bell glass, provided the air were kept dry by means of caustic potash.

48. These experiments also serve to explain some facts which puzzled Dr. Draper. A bottle of camphor in the dark produces no deposit, even after months of exposure, but when put in the window forms a deposit immediately. One bottle

put within another forms no deposit, even though exposed to the diffused light of day. Exactly so. A bottle shut up in the dark is protected from radiation; it is equally warm all round; and though filled with vapour, there is no sufficient reason why a deposit should be made at one part of the bottle more than another. Put that bottle in the window and it becomes cooled on the side nearest the window pane, and a deposit is made. Light has nothing to do with this result. The experiment can be made in the dark as well as in the light; indeed much better, for by night there is a greater difference between the inner and the outer temperatures than by day. I have taken a bottle of camphor out of a dark cupboard and placed it in the window on which the moon was shining. A deposit was made in three minutes on the side nearest the moon. Any one satisfied with the result of one experiment would say that the light of the moon produced the deposit. I have tried the same experiment when there was no moon, and obtained a similar result. So also if a tube be passed through a cork into the centre of a bottle containing camphor (Fig. 15), there will be no deposit on exposure to the light, because the tube is equally warmed all round, and is of the same temperature as the interior of the bottle; but if a little ice be put into the tube, there will be an immediate deposit upon it. So also if a bottle containing camphor be surrounded with a solution of sulphate of copper or of bichromate of potash, there is little or no deposit, not from any refined notion of cutting off the calorific or actinic rays, as Dr. Draper supposed (9), but simply because the bottle, though exposed to strong sunshine, is kept at one temperature by the convective currents formed in the solution. On one occasion (12 July), the temperature of the sulphate of copper solution, after a few

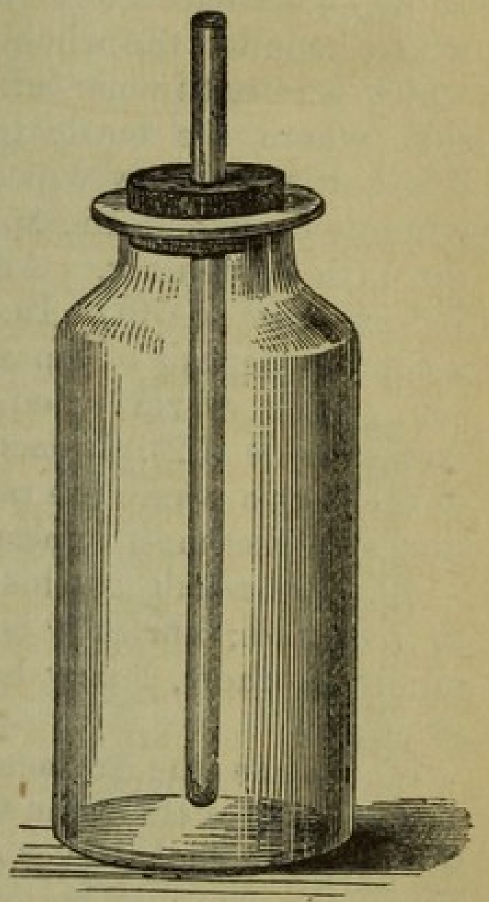


*Fig.* 15.

hours' exposure to the afternoon's sun, was read, and it was found to be 94°, and yet there was no deposit higher than one or two-tenths of an inch above the charge. These bottles were kept several months immersed in the solutions, exposed to the vicissitudes of heat and cold first in a west, and afterwards in an east, window without any increase in the low and scanty deposits. If my theory were correct, no deposit whatever would be made if special care were taken to keep the bottle of an equable temperature all round. Accordingly, a quart glass jar was three parts filled with water, at about 110° and an eight-ounce phial, that had been kept in the dark with its charge of crude camphor during some months without showing any signs of deposit, was plunged into it; the temperature was now 100°. The jar was covered with a large air-pump receiver, and so left facing the light. Next morning the bottle was taken out and carefully examined. There was no deposit whatever in the immersed portion of the bottle which included the whole of the cylinder, but near the neck which was not immersed, there was a faint deposit on one side, where the bottle tilted towards the side of the jar. The bottle was now wiped dry, and put in the angle of the window on which the sun was shining, on the morning of the 11th September. A thermometer placed near it marked 70° and rose to 78°. In less than fifteen minutes there was a copious deposit of small crystals on the side nearest the sun. The warm wood-work prevented a deposit on the farthest side (52). In fact, the deposit was formed on the side of the bottle where the radiation was most free, even though that side was most exposed to the sun.

49. The result of this experiment was quite satisfactory to my mind; but as it is my duty to try and convince the minds of others, I may be allowed to pile up a few of these proofs.

50. When the surface of the earth, and the air resting upon it, are of the same temperature, no dew is formed. If the earth be cooled ever so little there is condensation. So also if the camphor bottle be surrounded by a medium of the same temperature there is no deposit. Let that medium be of unequal temperature, and a deposit is immediately formed on the cooler side. For example: A bottle containing crude camphor, which had been kept in a cupboard during some months without showing any sign of deposit, even when

exposed to that most delicate of all tests, a lighted candle, was taken out, and a circular piece of filtering paper, of about the size of a florin, was dipped in sulphuric ether, and stuck on the outside of the bottle. In a few seconds an abundant deposit was made on the inside, exactly corresponding with this external plaister. The experiment was repeated with paper dipped in alcohol, bisulphide of carbon, &c., with the same result.

51. I could now explain a result which had puzzled me not a little (23). Bottles containing crude camphor were from time to time placed on a shelf in a glazed bookcase, close to the door, and opposite a window, and were occasionally changed during several months. They were exposed to the diffused light of the room, and formed farthest deposits during March, April, and May, and no deposits at all during June, July, and August. Now, during the first three months, the room had a fire in it. The wall formed the back of the bookcase, and glass doors its front; consequently the back part of the bottles would feel the stream of cold air from the door which passed along the wall to the fire, while the front of the bottles, protected by the glass, and looking into the warm room, would preserve a higher temperature than the back, and hence deposits were formed at the back and none in the front. When, however, fires were left off, the stream of cold air from the door would cease to flow: the inside of the bookcase would not vary in temperature, and the bottles, being protected from radiation, would not be in a condition to form deposits.

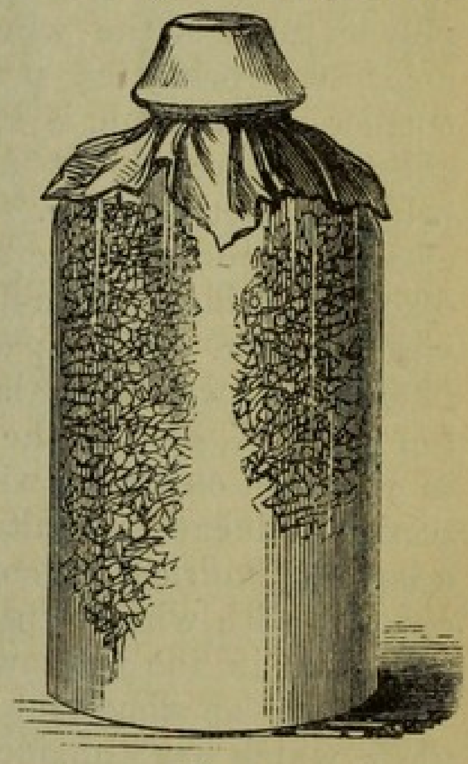


*Fig.* 16.

52. Whatever then protects the bottles from radiation, either wholly or in part, prevents the formation of deposits. The wooden scale of a thermometer, hanging a few inches from a bottle opposite the light, prevents a deposit, in a broad line exactly corresponding with the form of the scale. A bottle placed near a bar of the window frame will mark out the form of the bar by the camphor

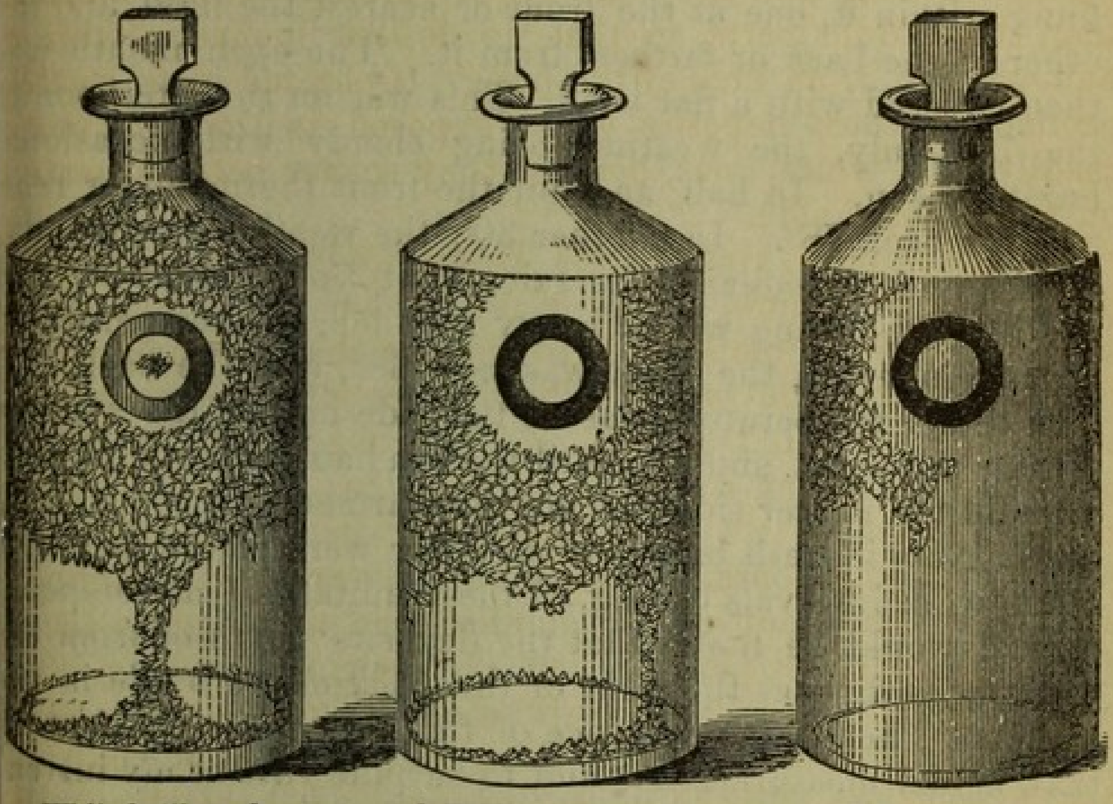
crystals coming up to within a certain distance of the bar, leaving that portion of the bottle naked, which coincides with the form of the bar (Fig. 16). In such a case as this, not only does the bar of wood prevent radiation from a portion of the bottle, but by absorbing heat from the sun it acts as a source of heat for hours afterwards. So, also, if a glass containing camphor stand on wood, the lower part is permanently warmer than the upper, as is shown by the repulsion of the deposit from the lower part, and that chiefly at the back; the front, where it is exposed to the light, being kept colder from being nearer the window. When bottles are tied over with flannel or india-rubber cloth there is no deposit under these flaps. When glasses are closed with bungs, these retain the heat, and keep the upper parts of vessels warmer than the middle parts, so that no deposit is made within a half an inch or an inch of the bung. Coloured glass bottles produce farthest deposits when exposed to the sun, more readily than white ones, because they sooner become heated on the exposed side.

53. The tin-foil rings of Dr. Draper (4) act as screens in preventing radiation. The glass, under and for a short space around the metal, is kept warmer than the uncovered portion nearest the window, and hence no deposit in and about the protected portion. To prove that this is the correct explanation, it seemed to me that black absorbent substances would act more efficiently as protectors than bright reflecting ones. Accordingly, I arranged a number of bottles containing crude camphor, and attached to them discs and rings of tin-foil, and of black, red, yellow, and white paper. The space kept clear by the black paper (Fig. 17, *b*) was much larger than that cleared by the tin-foil (Fig. 17, *a*), and the lighter papers, and these all varied in extent of clearance with their colour. To show the protecting influence of white paper on a large scale, one half of a large cylindrical jar containing crude camphor was loosely covered with white paper in the direction of its length, and so placed in the window the paper side nearest the light. There was an abundant deposit on the exposed half of the glass, which has remained some months, but no trace of a deposit has been found on the covered side.

54. I think enough has been stated to prove that the motion of camphor, &c., towards the light is really an effect

of heat. The laws which regulate the deposit of dew and hoar frost apply here. The bottles exposed in or near a window will always have one side colder than the other, and the colder surface will determine the deposit. Generally the side nearest the window is the coldest (seeing how little sun we have, and how long our nights are), and here the deposit is most copious; but when the sun shines on the window, and the side nearest the light is the hottest, a deposit is naturally made on the farthest side. This farthest deposit, however, is but transient. It disappears when the sun goes off the window, because the farthest side ceases to be the

*Fig.* 17.



*a.* White bottle and tin-foil ring. *b.* White bottle and black paper ring. *c.* Yellow bottle and black paper ring.

coldest. It goes, in fact, to augment the increasing deposit on the coldest side, or that nearest the light. I could always tell whether there had been any morning sun by inspecting the east staircase bottles on descending to breakfast. During the spring and summer of 1862, there would sometimes be sunshine and farthest deposits at five or six o'clock, A.M., while clouds or rain would come on about eight, and the farthest deposits would disappear during the day.

55. It is scarcely necessary to prove that a large bottle placed in the window, will be hottest on the side next the light when the sun is shining on the window, and coldest at other times, except perhaps during some of the warmest days of our short summer, when the external temperature is equal to, or even higher, than the internal; but as I am supporting a new theory against the united testimony of many illustrious philosophers during three quarters of a century, it is scarcely possible for me to overstate my case I will, therefore, give a few more details.

56. A glass shade with its mouth upwards, was placed on a small table, about two feet from a west window. Two thermometers, which marked the same temperature, were hung within it, one at the front or nearest the light, and the other at the back or farthest from it. The open mouth was then covered with a flat book. This was on the afternoon of the 7th July, the weather being cloudy with occasional bursts of sun. In half an hour the front thermometer read 82°, the back 78°. In another 30′, the weather was cloudy windy, and threatening for rain, front 76°, back 74°. In another 30′, during which heavy rain fell, the temperatures became inverted, the front was now 68°, and the back 74°. The colder temperature was on the side nearest the window during the night, and not until the sun had come round next day did the nearer side become the warmer.

57. Now if fresh bottles of camphor were placed near the window, at intervals during all these mutations of temperature, it would be found that the deposits go to or from the light, according as the front or the back of the bottle is the colder. There is no evidence of this fact more satisfactory than actual measurement; and to be quite sure of my instruments, I asked Messrs. Negretti and Zambra to prepare two thermometers that should range together with considerable accuracy. These instruments were received during the very warm weather, towards the end of July, and the results obtained by them struck me as remarkable. A cylindrical glass, ten and a half inches high, and nearly three inches in diameter, was washed with sulphuric acid and an abundance of water, dried, and charged with crude camphor. The thermometers were suspended in the glass front and back. The readings were taken at first every five minutes.

### 26TH JULY, 1862, SUN ON WEST WINDOW.

| Remarks. | Farthest. | Time. | Nearest light. | Remarks. |
|---|---|---|---|---|
| | 74° F. | 5·40 P.M. | 74° F. | |
| | 83 | ·45 | 83 | Slight misty ap- |
| | $83\frac{1}{2}$ | ·50 | $83\frac{3}{4}$ | pearance in the |
| | 81 | ·55 | $81\frac{1}{2}$ | glass. |
| | 79 | 6·00 | $79\frac{1}{2}$ | |
| | 78 | ·5 | $78\frac{1}{2}$ | Clouds coming |
| Faint camphor | 77 | ·10 | $77\frac{1}{2}$ | over the sun. |
| deposit. Mois- | 72 | ·15 | 72 | Slightly clouded. |
| ture condensed | 72 | ·20 | 72 | |
| high up on one | 76 | ·25 | $76\frac{1}{2}$ | |
| side near the | 77 | ·30 | 76 | Faint deposit. |
| back . . . . | $76\frac{1}{4}$ | ·35 | $75\frac{3}{4}$ | Ditto increasing. |
| | 76 | ·40 | 75 | Ditto. |
| | $75\frac{1}{2}$ | ·45 | 75 | |
| | 75 | ·50 | $74\frac{1}{2}$ | Deposit much |
| | 75 | ·55 | 74 | more marked than |
| | $74\frac{1}{2}$ | 7·00 | $73\frac{1}{2}$ | on the other side, |
| | $73\frac{1}{2}$ | ·10 | $72\frac{1}{2}$ | but still faint. |
| | 73 | ·20 | 72 | |
| | $72\frac{1}{2}$ | ·30 | $71\frac{1}{2}$ | |
| | $72\frac{1}{2}$ | ·40 | $71\frac{1}{2}$ | |
| | 72 | ·50 | 71 | |
| | 71 | 8·00 | 70 | |
| | $70\frac{1}{4}$ | ·30 | $69\frac{1}{2}$ | |
| Candles intro- | 71 | 9·00 | 70 | |
| duced . . . | 69 | 10·00 | 69 | |
| Deposit all gone | 66 | 11·00 | 66 | Deposit all gone. |

58. The above results are remarkable. The camphor vapour, as also the small amount of moisture left in the glass, are condensed on the side farthest from the light, where the temperature is a little lower than in front; but the most remarkable feature is the scantiness of the camphor deposits, although there was an abundance of light and heat. The deposits were almost nothing, and at length they disappeared. The reason for this is that the front and back temperatures are about equal.

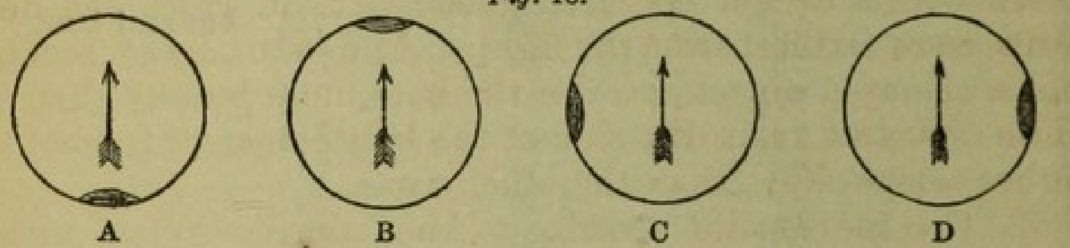
### 27TH JULY.

| Farthest. | Time. | Nearest. | Remark. |
|---|---|---|---|
| $68\frac{1}{2}$° | 8 A.M. | $67\frac{1}{2}$ | Three or four lines |
| 69 | 9 | 68 | of scattered deposit |
| 69 | 10 | 68 | nearest the light. |

Here again the fact is remarkable, that on this warm and bright morning, when the heat and light were abundant, th deposits were insignificant. This is easily explained, whe it is considered that in consequence of open doors an windows the temperature indoors and out are nearly th same. The conditions required for the production of fin deposits are warmth to raise the vapour, and cold on on side to condense it. A room warmed by a fire, and cold ai outside, are favourable to the result, so that the campho bottles in the window in cold weather are in a better positio than in this warm equable temperature.

59. It may be supposed that the position of the charge in the bottle may have something to do with the motion of the deposit. To settle this point, I prepared four eight-ounce phials of white glass, and put ten grains of crude campho into each. In bottle A (Fig. 18) the charge was in a hea

*Fig.* 18.




at the back of the bottle, in B in the front, in C at the left-hand side, and in D on the right. The bottles were placed in the window, and fine deposits were formed in all four cases on the side nearest the light; only in B, C, and D the deposit was connected with the charge in the same way as smoke may be said to be connected with the fire that produces it. In bottle A there was no such connection, the deposit being exactly in front, while the charge that supplied it was behind, and no visible connection between the two.

60. Bottles with good deposits nearest the light have also been turned round so as to place them farthest from the light. In such cases the single arrow *a*, Fig. 1, was made into a double arrow *b*, Fig. 1. In the course of a few days the deposits would gradually leave the back of the bottle and pass over to the front.

61. There is another point which may require notice in these days, when men's minds are so strongly impressed with the actinic action of light. Dr. Draper says that light passed

through a solution of bichromate of potash so as not to blacken nitrate of silver, produces an aphelion deposit in a bottle containing camphor. This is a mere effect of absorption of heat. I arranged a number of flat-sided bottles in pairs, the bottle nearest the light containing a coloured solution, and the bottle in contact with it containing camphor. The narrow sides of the camphor bottles were made opaque, so that the light which passed into them came through the coloured solutions only. These observations were extended over several months, and in every case the camphor produced farthest deposits, and when the bottle was turned round so as to make the farthest deposit a nearest one, it invariably went over to the farthest side. I also enclosed a white glass bottle containing camphor, in a wooden box, furnished with a sliding vertical door containing a hole about two and a half inches in diameter, which was accurately closed with a disc of coloured glass. In all cases the deposits were farthest from the light. And naturally so, seeing that a coloured object absorbs the heat more readily than a white one, and keeps the side of the bottle nearest to it of a higher temperature than the other parts.

62. One more point remains to be noticed. When mercury was exposed to the light in a tall narrow glass, no reliable results were obtained; that is, no deposit was formed that appeared to arise from the condensation of vapour. On two or three occasions metallic tears were seen in the vessel, but it was never clear to me that they did not arise from some shaking or disturbance of the vessel. I could not reproduce even this unsatisfactory result in a narrow vessel, even though I carefully tried for it by furnishing the vessel with a cap and stop-cock, and exhausting it with a syringe. I was also further surprised to find that a barometer tube of thick glass, charged with camphor and exhausted, produced little or no deposit, even on the warmest days, and by exposure to direct sunshine. No sooner, however, had I dismissed the action of light from this subject than the whole matter became clear. A thick glass tube by exposure to the light, does not cool unequally, but slowly varies in temperature throughout its mass, so that no deposit, either of mercury or of camphor, is possible. If, however, the tube be enlarged in diameter, and mounted so that while one part is exposed to radiation, the other part is protected, partial

cooling is possible, and a deposit is produced. This, too, furnishes an explanation of a fact that had often surprised me. In barometers of large bore there is usually a deposit of mercury in the Torricellian vacuum on the side nearest the light. I had never seen this in a tube of small bore, though I had frequently looked for it in my own instrument. Some of the barometers of large bore in the International Exhibition had very fine deposits of mercury vapour in the Torricellian vacuum; but in such cases they are mounted so that the tube is more or less exposed. Where the tube is boxed in, and protected from radiation, there is little or no deposit.

# ESSAY III.

## HISTORY OF THE MODERN THEORY OF DEW

1. Probably there never was a scientific treatise at once ɔ famous and so little known as the "Essay on Dew," by Villiam Charles Wells, M.D., published in 1814. A second dition of this treatise appeared in 1815, and a third in 1818, ɔntaining the author's autobiography written shortly before is death, which took place in September 1817. The Essay xcited some discussion during the author's lifetime. Some f the leading facts, together with an epitome of his "Theory f Dew," were at once adopted in books on Natural Philoɔphy, and these have been repeated, with little or no variaon, by every writer on Physics down to the present time.

2. What then has made Wells's "Essay on Dew" so famous, at the same time it is so little known? It is but little nown, because it has long been out of print, and, therefore, ıaccessible to the readers of popular science; it is famous, ecause innumerable books on Natural Philosophy have ·ferred to it with applause, but chiefly because one of our ıost celebrated scientific authorities has pointed out this ssay as a model of inductive experimental inquiry. Sir John [erschel, in his "Preliminary Discourse on the Study of atural Philosophy" (1830), characterises Wells's Essay as one of the most beautiful specimens we can call to mind of ıductive experimental inquiry lying within a moderate comıss;" and he earnestly recommends it to the student of atural Philosophy "as a model with which he will do well ) become familiar."*

* Dr. Lardner also, in his "Treatise on Heat" (1833), gives the sole edit to Dr. Wells. He says:—"The result of his inquiries was the scovery of the cause of the phenomena of dew, and affords one of the

3. No wonder, then, that Wells's "Essay on Dew," is famous. But among the thousands of readers of Herschel's discourse, probably not half-a-dozen have ever seen, much less read, Wells's Essay. The Library of the British Museum has a copy of the second edition, and a few of the public institutions of the metropolis have copies in their libraries; but it is seldom or never met with in private scientific libraries. It is matter of surprise that the fame of the work should not have led to its re-publication.

4. This Essay, then, is cited as a model of inductive experimental inquiry; that is, the author is supposed to have taken up a subject, which was but obscurely known, or known erroneously, and guided by one or two leading ideas, to have instituted a number of experiments, which, by their teaching, suggested others, and finally landed him on the domain of sound theory, as respects the subject in hand. Now, in a case of this kind, it is not necessary to the author's fame or originality that all his experiments should be new; it is not only right, but desirable, that he should take advantage of the labours of his predecessors and contemporaries, and enlist into his service every stray fact that is likely to assist his inquiry. But it is necessary, in adopting this course, that he carefully adhere to the law of *meum* and *tuum*; and that, when he comes to inform the world of his labours, he point out what others had done before he began his experimental inquiry; sum up honestly their results, with ample reference to books and memoirs, and show that such and such was the state of the question as he found it, and such the condition of the inquiry when he ceased to pursue it.

5. But surely Dr. Wells adopted this latter course in his famous Essay, if, indeed, there were anything to point out or

most beautiful instances of inductive reasoning which any part of the history of physical discovery has presented" (p. 328). In another treatise on Heat, published in 1855, Dr. Lardner still gives the whole credit of the "celebrated Theory of Dew" to Dr. Wells. Dr. Golding Bird in his "Elements of Natural Philosophy," 4th edit., 1854, also gives the whole credit to Wells. French and German treatises do the same. Thus Eisenlohr, *Lehrbuch der Physik*, 1860, in describing the capital experiment of two thermometers, one on the ground, and the other in the air, marking different temperatures, says, p. 367, *Dieser Versuch rührt von Wells her*. See also Mill's "System of Logic," 3rd edit., 1851, vol. i. p. 425, where the author, on Herschel's authority, gives all the credit to Wells for the discovery of the facts as well as the theory of dew.

the subject of dew except what he himself discovered! The numerous works on Natural Philosophy do indeed favour the common notion that Dr. Wells is the author of the modern theory of dew, and that he arrived at it by the induction of a series of beautiful experiments of his own contrivance. Writers on popular science are accustomed to distil their books from those of their predecessors, and to neglect that important part of the distiller's art, namely, *rectification.* Now this process of rectification can only be conducted by referring to original memoirs, and these it is not easy to find without considerable practical knowledge and acquaintance with the literature of the subject. Those writers who so constantly refer to Dr. Wells's Essay with applause, are little aware that most of its results had been published long before the author commenced his labours, and that the theory for which he has obtained so much credit, was also similarly indicated, in brief but unmistakable terms.

6. Now, if this statement be true, the very first place in which we ought to look for a confirmation of its truth is in the Essay itself. But there we find only a loose and general reference to authorities, and a very scanty acknowledgment of other men's labours. Indeed, the style of the Essay is that of a man who is announcing original discoveries. We will give one specimen, and would ask the reader whether such language as this does not entirely preclude the notion that such observations had ever been made before? He says:—"I have frequently seen, during nights that were generally clear, a thermometer lying on the grass-plot rise several degrees, upon the zenith being occupied only a few minutes by a cloud. On the other hand, I observed a very great degree of cold to occur on the ground, in addition to that of the atmosphere, during short intervals of clearness of sky, between very cloudy states of it."

7. I have inquired somewhat minutely into the history of the theory of dew, and the result of my investigation may perhaps employ half an hour of the reader's time not unprofitably. My purpose must not, however, be misunderstood. I am not anxious to detract from the real merit of Dr. Wells, nor to cast the smallest pebble against his admirers. The "Essay on Dew" is an elegant production, and proves its author to have been an earnest inquirer into nature, as well as a good observer; and, if not quite so

original as is generally supposed, I believe him to have been conscientious, but that sometimes while treading in other men's footsteps he fancied himself to be cultivating his own clearing.

8. Now, in order to investigate Wells's claim to be regarded as an original discoverer, I will credit him with the following six items, and then proceed to strike a balance by a careful inquiry into the debits. The chief points, then, which Dr. Wells is said to have established, may be thus stated :—

I. That on clear and serene nights the surface of the earth is colder than the air some feet above it.

II. That on such nights dew, or hoar-frost, is formed.

III. That in cloudy weather the temperature of the ground approaches, and is often identical with, that of the air ; and under such circumstances little or no dew is formed.

IV. That screens, even of the lightest material, interposed between the ground and the clear sky, and in general whatever interrupts the view of the sky, prevents that portion of the ground thus protected from cooling below the temperature of the air.

V. That different bodies exposed to the clear sky become colder than the air—the times and amounts of cooling being in general different in different bodies.

VI. That all these varied phenomena are to be accounted for on the principles of *radiation* and *condensation*, by the first of which the surface of the earth after sunset, provided the sky be clear, cools down below the temperature of the air ; and by the second of which the vapour suspended in the air is reduced to the liquid state by contact with a body cooler than itself. But should the sky be clouded, or the ground be protected by means of screens, the heat radiated from the earth is reflected back again, and thus maintains the surface at or about the same temperature as that of the air.

9. The history of dew is a good illustration of the vicious habit of transposing cause and effect. The ancients having noticed that dew was most abundant on moonlight nights, supposed the moon to be the cause of the dew, and the poet embalmed that belief in the term *rorifera luna* ("dew-bearing moon"). Aristotle, nevertheless, more accurately described it as a species of rain, formed in the lower

.tmosphere in consequence of the moisture which had been vaporated by day being condensed by the cold of night into ninute drops. Bacon ("Natural History," § 866) noticed hat starlight and bright moonlight nights are colder than loudy nights. Muschenbröck regarded dew as a real perpiration of plants. Du Fay considered it to be an electrical henomenon, since metals contract it but feebly, and metals eing good conductors convey away the efficient cause of the henomenon. The common notion, however, was that dew ose out of the ground.* These are mere fancies, not steps n our history, and it is surprising that they should have been onceived, seeing that a certain amount of correct observaion had already been recorded. The first point to be decided vas, whether dew rose from the ground, or whether the noisture that supplied it already existed in the air. The Florentine Academicians decided that the moisture was in he air, and they even attempted to estimate its amount by ooling the outer surface of a conical glass by filling it with ce, and then noting the moisture which condensed on the urface, and endeavouring to estimate its amount. This was lone more accurately by the Hon. Robert Boyle, who, in his "Experimental History of Cold" (published in 1665), deternined experimentally that the beautiful exhibition of frost n the window-pane is "generated of the aqueous corpuscles hat, swimming up and down in the air within the room, are y the various motion that belongs to the fluid bodies as uch, brought to pass along the window, and then by the vehement cold of the neighbouring external air, communicated through the glass, condensed into dew, and frozen nto ice." To prove this, Boyle sealed up in a phial a reezing mixture of snow and salt. "After awhile, the salt beginning to melt the snow, the dew on the outside began o congeal, and being rubbed off, the hoar-frost would quickly begin to come again. This phial, for further trial, being put nto a pair of scales with a counterpoise, after awhile, as the vapours wandered through the air in the warm room happened to be detained more and more upon the outside of the glass, and to be there frozen, the scale wherein the glass was

* In a curious volume entitled *De Rore disquisitio Physica D. Joannis Nardii, Florentini* (*Florentiæ*, 1642), it is stated : "*Rorem obseruant hi fieri exhalitu è terra eleuato, silente vento, celo sereno, anni tempore, loco, et regione temperatis.*"

began to be depressed, and to shrink lower and lower; afte which, by adding a little to the counterpoise, we reduce them again to an equilibrium, and yet after awhile the scal that held the phial subsided again more and more." In thi way Boyle collected on one occasion 18 grains of water, an on another 20 grains.

10. In this experiment Boyle distinctly recognises th fact, that *dew* and *hoar-frost* are formed by the precipitatio of the vapour of the air upon a colder body. Locking bac upon this experiment, it seems so easy for Boyle to hav transferred the scene of his operations to the surface of th earth, sparkling with dew, or bedecked with hoar-frost, an to have reasoned thus—"After sunset, the earth must b colder than the superincumbent air, or why this dew an hoar-frost?" But before the question could assume thi simple form, there was still more than a century of observa tion and experiment, doubt, difficulty, and discussion, bu nevertheless of steady progress, to be gone through.

11. A real advance was however made by Le Roi,* o Montpellier, in 1752, who (apparently without being ac quainted with Boyle's experiments) was led to doubt th truth of the received notion that dew rose from the ground by the well-known experiment of putting ice into a dr glass in summer, when dew forms on its outer surface Such moisture, he rightly thought, must be deposited b the air; and to test his opinion he sealed a bottle of whit glass containing air at the temperature of 20° Réaumu (77° Fahr.) As evening came on, and the temperature o the air declined to 15° ($65\frac{3}{4}$ F.), the interior of the bottl was bedewed in the upper part; when exposed to the col of night (which was as low as 6° R., $45\frac{1}{2}$° F.), there wa a considerable deposit of dew within the bottle. On th next day, when the bottle shared in the warmth of th returning sun, the whole of the moisture was taken u again, and the bottle became as transparent as it had bee the day before.

12. Le Roi also reasoned correctly on this experiment that the quantity of moisture in the air is dependent on th temperature. Hence, by lowering the temperature ther

* *Sur l'Elevation et la Suspension de l'Eau dans l'Air, et sur la Rosée Mémoires de l'Academie Royale des Sciences*, 1752. Paris, 1755.

must always be a certain degree of cold at which the air will deposit a portion of its moisture, and he named this temperature the *point of saturation* of the air (*degré de saturation de l'air*). "To determine this point," he says, "I take water sufficiently cooled to precipitate the moisture of the air on the exterior surface of the vessel that contains it. I pour this water into a large glass, very dry on the outside, and plunge into it the bulb of a thermometer, in order to note its temperature. I allow it to become warmer by half a degree, and then pour it into another goblet. If the outer surface of the goblet is still bedewed, I allow the water to become warmer $\frac{1}{2}°$ at a time, until I have seized the exact point above which there will be no further precipitation. This point is the point of saturation of the air. For example, on the 5th October, 1752, the temperature of the air was 13° R.; at $5\frac{1}{2}°$ the surface of a cooled glass became bedewed; above this point it remained dry; below it moisture from this air was precipitated upon it, and that in greater quantity as the temperature was reduced."

13. Le Roi noticed that the point of saturation, or the *dew-point* as we now call it, was often, during the day, very near the temperature of the air; but at night, when the air was many degrees colder than during the day, it was natural to suppose that on some nights it would fall below its point of saturation; and that, when such was the case, all the *moisture* in excess of that proper to the air temperature would be precipitated and form dew, and thus prove that dew is really formed by condensation of the moisture of the air. Accordingly on the 27th September, 1752, at sunset, when the air was at 17° R., and the degree of saturation $13\frac{1}{2}°$, a bottle of white glass was suspended in the open air; next morning, before sunrise, its exterior surface was found to be covered with dew, the reading of the thermometer being $12\frac{1}{2}°$. The result of a great many experiments of this kind was, that whenever the bottle was wet with dew, the nocturnal temperature had fallen below the point of saturation, but when the surface of the glass was dry there had been no such depression. The important observation, however, was made, that although the nocturnal temperature might fall below the point of saturation, a change of wind would sometimes prevent the formation of dew.

14. These are all capital observations, and deserve to be

rescued from a memoir which contains other less worth details; and the more so as Le Roi's method of determinin the dew-point is precisely that which many English writer on Natural Philosophy assign to Dalton, under the date 180]

15. The next step in advance was taken by M. Pictet, c Geneva, the author of some beautiful experiments on radiar heat, especially those with the conjugate mirrors. He no ticed that a thermometer suspended five feet from the groun marked a *lower* temperature on clear nights than one sus pended at the height of 75 feet.* The experiments whic led to this result originated in the method of measurin heights by the barometer; and in order to make the neces sary corrections for temperature, M. Pictet wished to ascer tain the law of cooling in a vertical column of air. For thi purpose he erected a mast in an open plain, with simpl arrangements for quickly raising and lowering thermometers He also placed a thermometer with its bulb buried in th earth near the foot of the mast, a circumstance which cause him to miss a great discovery; for had he simply placed th thermometer on the earth, instead of burying its bulb, h would have found that on clear nights the earth's surface i colder than the air above it, whereas he supposed it to b warmer. Passing over a number of interesting results, i may be stated that the readings of the two thermometers the one at five feet, and the other at 75 feet from the ground were the same at about 2 or 2½ hours after sunrise, and con tinued so during the hottest part of the day; but some tim before sunset the readings were in the opposite direction, th lower thermometer being colder than the upper one, the dif ference increasing rapidly till sunset, and averaging as mucl as 2° Réaumur, and even more, as long as twilight lasted This difference continued up to eleven o'clock at night, an apparently until sunrise, and not till some time after sunris did the readings agree, and then they began to cross eacl other as before. This result was obtained at all seasons during moderate wind and in cloudy weather; only in th last case the effect was much less appreciable. When the sk was completely clouded, or fog prevailed, or much wind, th

* The experiments which led to this result were made in 1779, and a account of them was inserted by M. de Luc in the fifth volume of hi *Lettres Physiques et Morales*. La Haye and Paris, 1779. M. Pictet' account is published in his *Essais de Physique*. Geneva, 1790.

difference disappeared, and the readings of the two thermometers were identical.

16. Pictet admits that he had always supposed the cold of evening to descend from above, and he could scarcely believe his eyes when he found the thermometer at the height of 75 feet more than 2° R. higher than one at five feet. He then begins to reason correctly. "It is then from the ground that this coldness proceeds" (" *C'est donc du sol que provient cette fraicheur* "); and indeed the thermometer hanging at four lines from the ground generally gave a lower reading than the one at five feet. This is all correct observation; but the buried thermometer led him astray, for this, naturally enough, indicating a higher temperature than any of the other instruments, he supposed that the earth retained a considerable portion of the heat it had acquired during the day, that a layer of air cooled by evaporation from the surface, produced the cold to the height of four lines from such surface, while at higher elevations the warmer air escaped this chilling influence.

17. In January, 1768, Professor Alexander Wilson of Glasgow noticed that a thermometer placed on the snow marked a temperature 8° F. lower than when suspended in the air, and that the mercury always rose a small matter when a mistiness came on, and *vice versâ*.* But the most important observations bearing on the theory of dew and hoar-frost were made by Professor Patrick Wilson, son of the above; and in reading the accounts of his experiments, which were begun in 1780,† one is struck with astonishment

* Philosophical Transactions for 1771.

† Professor Wilson's first paper is contained in the Transactions of the Royal Society of London for 1780. It is entitled " An Account of a most Extraordinary Degree of Cold at Glasgow in January last, together with some New Experiments and Observations on the Comparative Temperature of Hoar-frost and the Air near to it, made at the Macfarlane Observatory, belonging to the College. In a letter from Patrick Wilson, M.A., to the Rev. Nevil Maskeleyne, D.D., F.R.S. and Astronomer-Royal." There is a second and shorter paper in the Philosophical Transactions for 1781, entitled " Further Experiments on Cold," &c. But the author's most elaborate paper is contained in the first volume of the Transactions of the Royal Society of Edinburgh, published in 1788. It is entitled " Experiments and Observations upon a remarkable Cold which accompanied the Separation of Hoar-frost from a Clear Air." By Patrick Wilson, Professor of Astronomy in the University of Glasgow. (Communicated by Dr. Black). Read July 5, 1784. The paper is dated Feb. 14, 1784.

that he should have relinquished them just as they appeared to promise such good fruit. Ingenious in devising experiments, and honest in reporting their results, Patrick Wilson must be regarded as the most distinguished of Wells's predecessors.

18. Professor Wilson states that severe cold having set in on the 13th January, 1780, he determined to note the temperature frequently, and to keep a register, but was induced to extend his observations by meeting with "a new phenomenon, which consisted in a constant difference of temperature of the snow, which at that time covered the fields, and that of the air a few feet above,—the snow being the coldest." While taking a note of the temperature at an open window of the Observatory, at 5·30 p.m., he observed it to be at zero F. At 6 o'clock, or half an hour later, he carried the instrument into the Observatory Park, and laid it down upon the snow, when the reading was soon reduced to —13°. He supposed this to be the temperature of the air, but remembering that this was an unusual degree of cold, it occurred to him that the snow might have become cooler than the air by evaporation. To test this idea, he instituted the same evening a series of comparative experiments, and noted them down in a table; which, being the first of the kind ever published, containing the important and interesting fact, that during clear weather the ground at night is colder than the superincumbent atmosphere, cannot fail, to be examined with great interest. (The contraction gr. means *graduations* or *degrees.*)

*Thursday Evening, January* 13.

Below 0.

| | | | |
|---|---|---|---|
| 8·30 o'clock, therm. on snow pointed to | gr. — 12 | Therm. in air to gr. | 0 |
| 9 | — 14 | | — 2 |
| 10 | — 14 | | — 4 |
| 11 | — 17 | | — 6 |
| 11·30 | — 18 | | — 6 |
| 12·30 o'clock, Friday morning | — 20 | | — 8 |
| 1 | — 23 | | — 7 |
| 1·30 | — 22 | | — 8 |
| 2 | — 22 | | — 9 |
| 2·30 | — 21 | | — 8 |
| 3 | | | — 9 |
| 3·30 | | | — 10 |
| 4 | | | — 12 |

| | | |
|---|---|---|
| 4·30 o'clock, therm. on snow pointed to | gr. | Therm. in air to gr. — 12 |
| 5 | | — 12 |
| 5·30 | | — 12 |
| 6 | | — 14 |
| 6·30 | — 22 | — 13 |
| 7 | — 22 | — 13 |
| 7·30 | — 22 | — 13 |
| 8 | — 19 | — 10 |

19. Wilson soon found that the cause of these remarkable differences could not be attributed to the evaporation of the snow, seeing that hoar-frost was being abundantly deposited at the time when the readings were lowest. On the night of the 23rd January "several things were laid out at the Observatory, such as sheets of brown paper, pieces of boards, plates of metal, glasses of several kinds, &c., which all began to attract hoar-frost, seemingly as soon as they had time to cool down *to* the temperature of the air. The sheets of paper being thin and easily cooled, acquired it soonest, and when beheld by candle-light were beautifully spangled over by innumerable reflections from the minute crystals of hoar-frost which had parted from the air."

20. If, in the above passage the writer, instead of the little word *to* which I have printed in italics, could have written the word *below*, he would have given the true theory of the formation of dew and hoar-frost. It will be seen further on, that Professor Wilson more than half suspected that this cooling of bodies below the temperature of the air was necessary to the deposition of hoar-frost.

21. In the course of his first experiments in 1780, Wilson did not fail to remark, that the lowest temperatures were coincident with the clearest weather, or, as he expresses it, "the stars shining with a full and steady light, like that of the planets." A basket had been filled with snow from the surface, when the temperature was + 14°. At four o'clock a piece of thin fir plank about a foot square was placed on this snow, and on the plank a small plate of tin, and on the tin a thermometer, which at five o'clock pointed to — 16°, and at 6 to — 18°. It now occurred to him that the snow had been cooled by a descent of cold air "sweeping along its surface in so thin a sheet, as not to affect the air a little higher up." To test this idea, he placed a thermometer on the snow, and the reading was — 22°, that of the air

above being — 8°; and he then proceeded to project air on the bulb of the thermometer on the snow, by means of bellows, which had been previously cooled by lying out on the snow, the operator standing to leeward of the thermometer. After continuing to blow for two minutes, he was surprised to find that the thermometer had risen no less than 10°, the reading now being — 12. The experiment was varied on another night, by fanning the snow with a sheet of brown paper fastened to the end of a long, slender stick; and the result was, that the mercury rose to nearly the same degree as that suspended in the air.

22. These experiments sufficiently disproved the idea of the snow being cooled by a descent of cold air. The reverse process, namely, the warming of the snow by the presence of clouds, apparently suggested the following experiment, which has been often repeated by persons who seem to have had no idea that it originated in 1780 with Patrick Wilson, instead of 1812 with Wells. At 1·45, when the thermometer on the snow was at +3°, the instrument was screened by two sheets of brown paper set up on their edges, and inclined against one another like the sides of a roof, the paper having first been cooled by exposure to the snow. At 2·15 the thermometer thus sheltered pointed to + 9°, showing a rise of 6°. This capital experiment was afterwards repeated, as the author remarks, "with the same event." It may here be mentioned, that at a later period of the inquiry, Dr. Black* suggested that a gauze screen should be tried instead of the brown paper. Accordingly, Wilson says, "I fastened with packthread a piece of open gauze to a hoop of 8 inches in diameter, and an inch deep, and when the thermometers were sheltered in this manner, the quicksilver commonly rose nearly 2°."

23. That the air above the snow should be warmer than the snow itself was a fact sufficiently startling to these earnest, conscientious men, who, in a proper philosophical spirit, tried to explain it "by known principles," and yet could not avoid "a suspicion of there being something singular

* It should be stated that Drs. Irvine and Crawford, names honourably distinguished in connection with the science of heat, personally assisted Professor Wilson in this inquiry, while Doctor Black gave it the sanction of his high authority by exchanging letters on the subject with him.

and undiscovered at bottom." They not unnaturally sought to ascertain whether the higher temperature of the air extended to any great distance, and they accordingly attached a thermometer to a hook at the end of a long pole, and elevated it 24 feet. On suddenly lowering the instrument, it gave a higher reading than at the station below, where the temperature on the balustrade to the windward of the building was + 10°, and when several detached bodies were coated with hoar-frost. "The result of this trial," says Wilson, "appeared more remarkable than anything which had hitherto occurred." The thermometer was taken off the pole and laid on the hoar-frost, when the mercury fell not less than 6°; this was repeated several times. The instrument was also swung round in the air, so as to bring the bulb into frequent contact with fresh particles, and still there was an elevation in temperature.

24. After this the frost broke up, and the experiments were discontinued, under the mistaken idea, which was most unaccountably allowed to prevail, that the results depended upon an excess of cold. In February of the same year, 1780, the frost returned, and the capital observation was made during a thick fog, that there was "no difference whatever in the temperature of the hoar-frost and the air in its neighbourhood, both being + 22° at 10 a.m." At 11 the sun broke through the fog, when the temperature of the hoar-frost was observed to be as quickly affected as that of the air.

25. In giving an account of his labours for this winter, Professor Wilson winds up with the following remarkable words:—"In the further prosecution of this subject, and in whatever way it may be cleared up, it is probable that we shall meet with a fine instance of the congruity of Nature in all her operations, and of the stability of those general laws which have been derived from a cautious observance of the rules of experimental philosophy. It would be going too far were we to conclude from the experiments related above *that very cold air is never disposed to deposit its contents except upon bodies as cold, or colder than itself*; and yet that this is frequently the case, seems probable from a number of common appearances." He then gives such examples as the following:—After a night of frost the slates and other thinner parts about a house will be whitened with hoar-frost,

when the walls or more solid parts of the building remain quite free. In like manner, the smaller branches and twigs of trees often acquire this frozen ornament, when the main branches and trunk remain naked for a long time; and, in general, any thin or detached body, capable of being easily cooled, attracts hoar-frost the soonest.

26. In the following year, 1781, another remarkable observation was made—namely, the greatly increased temperature of the earth beneath the snow, as compared with that of the surface, and indeed with that of the air. On the 22nd of January, from 1 till 3 in the morning, the thermometer in the air at the balustrade of the east wing of the Observatory marked + 4° to +6°, and on the snow —2° to 0°. At 1·30, the thermometer in the air, 24 feet from the ground, to windward of the house, stood at +7°. At 3, the snow in the park, nine inches below the surface, raised the thermometer to +14°, and at the depth of six inches, to +24°.

27. The following readings, taken on the 25th, show, in a most beautiful manner, the equalisation of the temperatures on the snow and in the air under the influence of increasing clouds:—

| | Thermometer in Air. | Thermometer on Snow. |
|---|---|---|
| 9·45 o'clock | +10 | +3° |
| 10·30 | +14 | +4 |
| 11·30 | +17 | +9 |
| 12·30, cloudy | +22 | +20 |
| 1 afternoon, more cloudy | +25 | +26 |
| 1·30, cloudy all over | +27 | +27 |

28. Wilson was quite aware of the importance of providing himself with good comparable instruments; for he says, "I had now ready a set of ticklish thermometers, with naked bulbs and slender ivory scales, and all very nearly agreeing as to their dimensions and distance between the fixed points." In the observations of 1783-84, he constructed his tables with especial reference to the state of the sky. These are instructive, and we quote one or two.

*December* 28, 1783, *at Night.*

| | Air. | Snow upon the Ground. |
|---|---|---|
| 11 o'clock, quite clear | +5 | —7 |
| 11·30 ditto | +4 | —7 |
| 11·50, overcast | +6 | +4 |
| 12·30, more overcast | +7 | +7 |

| *Morning.* | Air. | Snow upon the ground. |
| --- | --- | --- |
| 1·30, turning a good deal clearer | +8 | +4 |
| 1·45, still more so | +8 | +2 |
| 2·20, clear everywhere, except an ill-defined cloud in N.E. | +9 | +4 |
| 3, ditto, except a small ill-defined cloud near zenith | +11 | +3 |
| 3·45, very clear, except some better defined clouds near horizon, from N. to N.E. | +12 | +4 |

29. On the 30th December he remarks, that "this night the thermometers were affected still more remarkably by the vicissitudes of the atmosphere, the snow not only acquiring the same temperature as the air when the heavens were overcast, but thereupon becoming considerably warmer." The effects here referred to depend upon the influence of wind in tending to equalise the temperature, as upon radiation in lowering it.

| *December* 30, 1783, *at Night.* | Air. | Snow upon the ground. |
| --- | --- | --- |
| 7 o'clock, clear all over | — 1 | — 9 |
| 9 do. wind gentle E. | — 4 | — 12 |
| 10 do. do. | — 4 | — 12 |
| 11·20 Wind a little before this shifted from E. and E. by N. to S., and now some clouds in S.W. | — 1 | — 8 |
| 11·35 Cloudy all over, wind S.S.W., blowing out the candles | 0 | + 4 |
| 12 Do. do. | 0 | + 4 |
| 12·30 No intervals of sky, but a general uniform cloudiness, which hid all the stars | + 1 | + 4 |
| *Morning.* | | |
| 1·30 ditto | + 3 | + 9 |
| 2 particles of snow beginning to fall | + 6 | + 10 |
| 2·30 ditto | + 6 | + 10 |
| 3·30 more snow falling than before | + 10 | + 14 |

30. I have next to notice a capital series of experiments by Wilson, on the cooling of different bodies by exposure to a clear nocturnal sky. And in order to be able to ascertain whether these bodies gained or lost in weight by the exposure, he ingeniously arranged some of them on a balanced board, which he called a *snow-scale*, so as to be able to weigh

them after the experiment, so that, should they have gained in weight, he would thus completely get rid of the idea of the cold being the result of evaporation, which still continued to haunt him. He first exposed some fine flinty sand, free from dust, and some snow in the snow-scale, placing one thermometer on the sand, and another on the snow, and suspending a third in the air, when he was astonished to find the excess of cold at the surface of the sand, the thermometer in air indicating + 14, that on the snow + 10, and that on the sand + 8; and on weighing, it was found that the sand had gained $\frac{4}{10}$ths of an ounce, and the snow nearly $\frac{3}{10}$ths. The following are the tabulated results of this admirable experiment:—

*January* 24, 1784, *at Night.*

| | Air. | Snow scale. | Sand board. | Snow upon the ground. |
|---|---|---|---|---|
| 10·30 o'clock . . . . | + 14 | + 10 | + 8 | ... |
| 11·15 do. . . . . | + 13 | + 10 | + 8 | ... |
| 11·20 do. . . . . | + 11 | + 8½ | + 6½ | + 5 |
| 11·40 do. . . . . | + 12 | + 8½ | + 7 | ... |
| 11·55 do. . . . . | + 10 | + 7½ | + 6 | + 6 |
| 12·45 do. . . . . | + 12 | + 9 | + 7 | ... |
| 1·30 do. . . . . | + 12 | + 10 | + 8 | + 6 |

31. On this occasion another experiment was tried as to the effect of a screen in preventing the cooling of a body exposed to the sky. A little after midnight, when the temperature of the air was 12°, and that of the sand 7°, a thin disc of wood, which had been cooled by exposure to the air, was placed over the sand, at the height of about 1½ inch from its surface. After an interval of seventeen minutes it was removed, when the reading of the thermometer on the sand was only 2° lower than that suspended in the air. On the same occasion, the gauze screen already referred to was tried, with a similar result.

32. On the next night (26th January), finely powdered wood charcoal, loose shavings of brass from a turner's lathe, a friable amalgam of mercury and tin, together with sand and snow, were exposed to the sky on circular boards, two feet in diameter, mounted on a long slender frame, set at right angles to the direction of the wind. A thermometer was placed on each substance, and the reason assigned for the experiment was the very proper one, of "sand being

found to favour this refrigeration." The result of the experiment was, that all the bodies exposed, except the snow, fell full 6° below the temperature of the air, which was + 12°; the sand being rather the coldest, and the snow nearest the air's temperature. It was noticed that "the hoar-frost separated from the air most discernibly on the sand and charcoal dust, though the manner of attaching itself to those substances was very different. On the sand it showed itself by making the surface all over sparkle with an infinity of minute bright points; whilst on the charcoal dust it settled without such congruity, and formed into many broken filaments of a dull hue, which here and there lay clustered as if aiming at some stellated arrangement." This is very accurately described, and agrees with what we know as to the radiating powers of the bodies in question; but we cannot help thinking that the reduction in temperature is understated, the temperatures given being probably those of the hoar-frost rather than of the sand and charcoal.

33. In another experiment, a quantity of mercury was exposed in an oval mahogany tea-tray, and a quantity of sand in another. A thermometer was placed on each surface, and a third instrument was suspended in the air. The following are the tabulated results of the experiment :—

*January* 29, 1784, *at Night.*

| | Air. | Tray with Quick-silver. | Tray with Sand. | Excess of Cold on Quick-silver. | Excess of Cold on Sand. |
|---|---|---|---|---|---|
| 10 o'clock . . | +4 | +3 | —1 | 1 | 5 |
| 10·30 „ . . . | +3 | +2 | —1 | 1 | 4 |
| 11 „ . . . | +3 | +2 | —1½ | 1 | 4½ |
| 11·30 „ . . . | +3 | +1½ | —1 | 1½ | 4½ |
| 12 „ . . . | +4 | +2 | 0 | 2 | 4 |

34. A good deal of hoar-frost settled on the wood of the trays, and the cold was attributed to this, instead of the radiation producing the hoar-frost; indeed, the conclusion drawn from these experiments is, that "at any given time the more hoar-frost the air imparts to bodies over which it passes in a clear state, the greater is the cold then produced." This is simply mistaking effect for cause; and while expressing my

admiration at the beauty of the experiments, the ingenuity with which they were devised, and the candour with which they were reported, I cannot help thinking that in this long inquiry the professor had rather retrograded than advanced. With these important results before him, it is wonderful to me how Wilson could persist in the idea that they depended on an "excess of cold;" that our winters are so short, and the cold not usually severe enough, for the prosecution of "so dark a subject," which he therefore hands over to the philosophers of more rigorous climes. The fact is again and again brought before him, that it is "in the clearest and stillest nights only that the cold at the surface of the boards was observed to be most remarkable," and yet he allows the great discovery to escape him, that the deposit of moisture is a *consequence*, not a *cause*, of this cold. He discovers the highly important and suggestive fact, that different bodies freely exposed to the clear sky cool at different rates, and yet this great fact bears for him no fruit. He makes the equally suggestive discovery that the refrigeration is checked by screens, even where the material is so flimsy as gauze; and this is equally barren. Truly we can only wonder that a man having done so much should have stopped short,—should have ploughed the land, sown the seed, watched the growing crop, and then have left others to possess themselves of the harvest. What is so surprising is, that Wilson should not have continued his experiments and observations at different seasons, and that such men as Black, Crawford, and Irvine, should not have seen in these results something to suggest the laying out of thermometers on other nights after the frost had disappeared.

35. Another remarkable fact is, that Wilson's experiments should have been unknown to subsequent inquirers in the same field, the first of whom that has now to be mentioned is Mr. James Six, F.R.S., the inventor of a well known register-thermometer, an account of which, and of experiments made with it, was published in the "Philosophical Transactions," and also in the *Monthly Review*.

From these sources a volume was made up and published a year after his death.* The fifth chapter of this work is

* "The Construction and Use of a Thermometer for showing the extremes of temperature in the atmosphere during the observer's ab-

headed, "How far a thermometer is liable to be affected by being placed near to or at a distance from the earth; or what difference is found in the temperature of the air at different altitudes." Mr. Six was a resident of Canterbury, and took advantage of the cathedral of that place for obtaining the various altitudes required for his thermometers. He says (writing in 1783), "I took corresponding observations at different stations from the surface of the earth to an elevation of more than two hundred feet above it. On making this experiment, I was at first very much surprised to find the temperature at the upper station on some nights considerably warmer than the lower one; and, on searching for the cause of this difference, I found it to proceed from *a refrigeration which takes place on the surface of the ground in the evening and night, and more particularly so when the weather is still and clear*. For, although the earth is then more liable to be heated by the sun's rays in the daytime, and, by its superior density capable of retaining a greater degree of it than the air, yet a diminution of heat begins to take place on its surface as soon as the sun is near setting and the dew begins to appear, and increases so as considerably to cool the lower strata of the air." Here again, there is a substitution of effect for cause; for Six evidently attributes the cooling to the dew, instead of the dew to the cooling. To see whether the phenomenon varied with the seasons, he continued his observations for about a year, and arrived at the general conclusion that the most considerable differences were to be found when the weather was serene. He also noticed that the refrigeration is much less during thick fogs and cloudy skies; and he says, "I have never found the glass on the ground in the night warmer, or even so warm, as those above it, except when rain or snow has continued to fall during the whole night, be the general temperature of the air either warm or cold." He also noticed that the degree of cold varied with the kind of surface on which the thermometer was placed,—that "on green turf where the grass is not an inch long, the diminution will be less on some nights by 1°, 2°, or 3°, than where the grass is longer, and according as the cold is more or less intense." He also found the differ-

ence, together with experiments on the variations of local heat, and other meteorological observations." By James Six, Esq., F. R. S. Maidstone, 1794.

ence between the temperature on the ground and seven fe above it to be greater during the night than between seve feet and two hundred feet, except in one instance, whe they were equal.

36. Next in the order of discovery is M. Prevost, who, i a work printed at Geneva in 1792, entitled *Recherche Physico-Mécaniques sur la Chaleur,* distinctly recognise the fact that when at night the sky is clear the air is gene rally colder near the earth than at a higher elevation; tha in spring and autumn it seldom freezes under a cloudy sky and that often on a calm night, when a cloud passes over th zenith of the observer, the thermometer instantly begins t rise. He explains this on the doctrine of radiant heat which, he says, passes easily through the air, but tha clouds are opaque for heat as well as for light,—they absor both, or only allow it to pass slowly. The heat radiatin from the earth traverses the pure atmosphere with facility but is intercepted by clouds, which form a sort of vestment fo the earth; receiving the radiant heat on their lower surface they become warm there, as a garment does close to th body, and restore to the earth more heat than transparent ai can do. Meanwhile the surface of the cloud cools from th ease with which it emits heat into the rarefied air above Nor need we be surprised at the quickness with which nocturnal cloud causes its presence to be felt, seeing that th action of radiant heat, going and returning from the earth t the cloud, and from the cloud to the earth, takes place in a indivisible instant of time.

37. Referring to Pictet's observation (15) made in Januar 1777, on the night between the 4th and 5th, that the ther mometer was at 5° Fahrenheit at 10 p.m., when the sk became clouded, and at 11 p.m. it had risen to $8\frac{3}{8}$°, Prevos states that Pictet informed him that the rise in temperatur was accompanied by the appearance of a low cloud of no grea extent in the zenith. He also refers to a fact, well know to agriculturists, *viz.*, the almost instantaneous influence c clouds on the soil in preventing the formation of dew an hoar-frost: even where the temperatures are the same, thes will be found under a clear sky and not under a cloudy on All these facts, he adds, are explicable by the theory c radiant heat, and considering the clouds to act the pa of screens.

38. It is remarkable how often the fact was noted by independent observers, that in calm weather the earth at night is usually colder than the air. Thus, Mr. John Lloyd Williams, in the course of a description* of the method of making ice at Secrore, near Benares, says:—"I had a thermometer among the ice-pans, during the season of making ice, with its bulb placed on the straw, and another hung on a pole $5\frac{1}{2}$ feet above the ground, and commonly observed that when ice was formed, and the thermometer on the straw was from 37° to 42°, that on the pole would stand about 4° higher; but if there was any wind, so as to prevent freezing, both the thermometers would agree."

39. A series of experiments by M. B. Prevost belong to the year 1802-3, and are contained in the *Annales de Chimie*, an. XI. He begins by citing an experiment of Du Fay, in which a glass vase placed in a silver basin and exposed to the night air sometimes becomes covered with dew while the basin remains dry, and he shows by a number of experiments on window-panes "that a piece of metal placed on glass usually protects also the opposite side of the glass from the deposition of dew; and in general, that whenever the metal is placed on the warmer side of the glass, the humidity is deposited more copiously either on itself or on the glass near it; that when it is on the colder side it neither receives the humidity nor permits its deposition on the glass; but that the addition of a second piece of glass over the metal destroys the effect, and a second piece of metal restores it." It was also shown that when a number of drinking-glasses of the same size were about half or two-thirds filled with water, mercury, alcohol, oil, acid, small shot, and other bodies, and exposed to the night air, no dew was deposited on their lower part, but only above the level of the substances contained in the glasses, and that at distances ranging with their nature, it being greater for mercury than water, and greater for water than oil.

40. The author of these experiments attempted to account for the effects produced on the principle of conduction, for which the metals are distinguished over glass and the other substances employed. Dr. Thomas Young, however, in his "Lectures on Natural Philosophy," published in 1807, gave

* *Universal Magazine*, for June, 1793.

the true explanation. He notices the fact that metals have a very weak attraction for moisture, and then, after the short summary which we have quoted above (39), concludes in the following words:—"It appears that from its properties with respect to radiant heat, the metallic surface produces these effects by preventing the ready communication either of heat or of cold to the glass."

41. Dr. Young also, under the same date, 1807, refers to Prevost's former experiments (36), and explains the fact of a thermometer rising when a cloud passes over the place of observation, by its preventing the escape of heat radiating from the earth and reflecting it back to the surface.

42. In thus marking the chief points respecting dew which were established by the predecessors of Dr. Wells, we have seen that long before the date of his celebrated Essay five at least of the six propositions given near the commencement of this article (8) had been established by decisive experiments recorded in "Transactions" and Journals accessible to all; and this had been done many years before Dr. Wells began his labours. With respect to the sixth proposition, we have seen that M. Prevost had satisfactorily accounted for the nocturnal cooling on the principle of radiation, and had justly appreciated the influence of screens and clouds in reflecting back the heat thus emitted. Indeed we have a distinct acknowledgment by Wells of the value of Prevost's theory in explaining the phenomena of dew and hoar-frost. He says that he has adopted "the hypothesis of M. Prevost, of Geneva, on the constant radiation of heat by bodies in contact with the atmosphere, even at a time when they are exposed to the influence of bodies warmer than themselves; as it appears to agree perfectly with all the phenomena of the communication of heat which do not depend upon conduction. I shall hereafter make frequent use of this hypothesis."

43. Again, speaking of other men's labours, he says, referring to hoar-frost, "it has, I believe, from the time of Aristotle, been uniformly, and, according to my observations justly considered as frozen dew. I shall, therefore, frequently refer hereafter to the experiments of the late Mr Patrick Wilson, of Glasgow, respecting it, as if they had been actually made upon that fluid. Indeed several of my experiments upon dew were only imitations of some which ha

been previously made upon hoar-frost by that ingenious and most worthy man."

44. Dr. Wells states that he first began to think of the phenomena of dew in the autumn of 1784, when it seemed probable that the formation of dew was accompanied by the production of cold; that in 1788 the same idea occurred to Mr. Patrick Wilson, and in the same year to Mr. Six.

45. Now this is not quite ingenuous. Wilson's first paper, as we have seen (17, *Note*), is inserted in the "Philosophical Transactions" for 1780; his second paper in the "Transactions" for 1781, and it is his third paper, embodying the results of the former two, with new experiments and observations, under the date of 1784, which was published in the "Edinburgh Transactions" in 1788. Wells was aware of this, for he more than once refers in his Essay to Wilson's papers in the "Philosophical Transactions" for 1780 and 1781; but, in his anxiety to appear as the first in the field of inquiry, he post-dates Wilson's labours by as much as eight years, and Six's by five. Moreover we have only Wells's assertion that he began to study the subject of dew in 1784, whereas we have the fruits of Wilson's labours in 1780 and the following year. We have also Wells's admission that although the subject was almost constantly in his thoughts, he made no attempt to investigate it experimentally until the autumn of 1811, when happening to be in the country on a calm and clear night, he laid a thermometer on the grass, which was wet with dew, and suspended another in the air two feet above it. An hour after the thermometer on the grass was 8° lower than that in the air. Still, however, he regarded the cold as an effect of the dew; and it was not till some time afterwards, on continuing to ponder on the subject, that it occurred to him that Wilson, Six, and himself, had committed an error in regarding the cold as the effect of the dew. Might it not be the cause? This idea, which contains the gist of the whole matter, slowly dawned upon him: he resumed his experiments and arrived at the true explanation of dew and some other phenomena, his observations being "mixed with facts and opinions already published by others."

46. Here, again, we think that justice has not been rendered to Patrick Wilson, who did not altogether regard

the cold as the effect of the hoar-frost, as will be seen by the passage already quoted (25).

47. The "Essay on Dew" did not pass unchallenged during the author's lifetime. It was noticed in the *Quarterly Review* for 1814 by no less a man than Dr. Thomas Young. In this review the writer enters "a protest against the total novelty of the opinions which Dr. Wells's laborious series of experiments had so amply illustrated and confirmed." He further states, that while Dr. Wells "affords us complete information, not only respecting the sentiments of Aristotle and Theophrastus, as to the nature and causes of dew, but also of the most distinguished philosophers of modern times; some of the works, however, of the persons whom he mentions, and some of the latest, have most unaccountably escaped his attention." He then proceeds to give an account of M. Prevost's work. It is, however, but justice to Dr. Wells to state that this particular essay, published in 1792, was unknown to him; that in 1813 Dr. Marcet, a relative of M. Prevost, lent him (Dr. Wells) a copy of a work by M. Prevost, published in 1809, in which the author repeats the passages reprinted by Dr. Young from the earlier work.

48. After quoting a number of passages from Prevost's work, Dr. Young refers to his own "Lectures," published in 1807, in which he states, that "when the weather has been clear, and a cloud passes over the place of observation, the thermometer frequently rises a degree or two, almost instantaneously; this has been partly explained by considering the clouds as a vesture, preventing the escape of the heat which is always radiating from the earth, and reflecting it back to the surface." He then goes on to state that "it is true that the theory could only be completed by the application of Professor Leslie's discoveries to the circumstances of the phenomena; but it is remarkable that this very application was made, in a case confessedly *similar*, by the author of the same work which we have last quoted." This refers to the curious experiments of M. Prevost, under the date 1802-3 (39), and he might also have quoted Patrick Wilson on the different cooling properties of sand, charcoal, &c. (30). Dr. Wells remarked in his Essay, that the appearances observed by M. Prevost "may be easily accounted for;" whereupon Dr. Young retorts, "had Dr. Wells been as solicitous to attend to the labours of his contemporaries as he has been very laudably anxious to

efer to those of his predecessors, he might have said, not hat the experiments of M. Prevost *might* 'be easily accounted or,' from the properties which he mentions, but that they ctually had been explained in a similar manner by one of is own countrymen. There are, however, some modern hilosophers, who, whether from their own fault or from that f their hearers and readers, or from both, appear to be per- etually in the predicament of the celebrated prophetess of ntiquity, who always told truth, but was seldom understood, nd never believed; and the author of the lectures in question as not unfrequently reminded us of the fruitless vaticinations f the ill-fated Cassandra."

49. Dr. Wells published a reply to Dr. Young's stric- ıres in the Fifth Volume of the "Annals of Philosophy" 815); and the following are a few extracts from it. He ıys:—"My explanation of the immediate cause of dew is ounded on the simple fact, that bodies always become ılder than the neighbouring air before they are dewed, and as consequently open to the discovery of every person since e invention of thermometers. It is true, that the next step my theory could not have been taken without the assist- ıce of the late discoveries of others, and this has been amply knowledged in my essay. My health at the time of its ing drawn up was in such a state, that I scarcely hoped at I should ever finish my work, and my notes were so ritten that no person besides myself could make use of them. composed, therefore, in haste, and had neither leisure nor rength to search public libraries for all the works which I ished to consult." * Dr. Wells contends that Prevost "was

* We learn from the "Memoir of his Life," dictated by Dr. Wells in last illness to his friend Mr. Patrick, and prefixed to a work entitled Two Essays," &c., published in 1818, after the death of the author, a v particulars which throw some light on the loose manner in which the say on Dew was written. For example, he says,—"In 1800 I was ldenly seized with a slight fit of apoplexy. From this, however, I did t recover so far as to return to the exercise of my profession for several nths; and I never afterwards regained the complete possession of my mory. I became, too, much more unfit for the pursuits of any difficult in of thought which was the production of another person." He then es on to say that he was not, so far as he could ascertain, less equal to pursuits of his own train of thought.

Referring to his inquiry into the nature of dew, which he thought uld not occupy him more than a few nights, at the house of one of his nds in the country, he says,—"I commenced it in the autumn of 1812, ignorant that bodies on the surface of the earth becom much colder than the air in a clear night; this being one o the principal facts on which my theory of dew is built."

50. We must here remark, that although Prevost was igno rant of this important fact, it had been made sufficiently clea by Patrick Wilson (17, &c.) and Six (35), with whose experi ments Dr. Wells was acquainted. He states, however, tha when he wrote his Essay he had not read the explanation o Prevost's experiments in Dr. Young's works. He read th fifty-first lecture and the sixtieth, but not the intermediat one which contained the explanation in question. He con sulted Dr. Young's work in a public library, and was in haste; and as he found no reference under *Dew* in the index he searched no farther. He admits Dr. Young's conjecture a to the true explanation of Prevost's window experiments to be original. "It was most happy too, since, if admitted to be just, it completely accounted for several important cir cumstances in M. Prevost's experiments. If then, its learne and ingenious author had established its truth by facts clearly seen by himself, and had afterwards pursued the subject o dew through its various ramifications by means of the clu which would have been thus obtained, he must soon hav acquired a knowledge of the theory which has lately been

but soon found that I had greatly miscalculated the time which it woul employ me. I determined, however, to proceed, from the natura steadiness of my disposition, which would never allow me to abandon an pursuit that I had seriously undertaken. I soon found that I wa altogether unequal to it; for each night's labour fatigued me so muc that I could not undertake a second for several days after. In the mean time, my ankles began to swell in the evening, which I regarded as mark of general weakness. At length I became so infirm, about the en of 1813, that I was absolutely obliged to give up any further visits to th country.

"In the beginning of 1814, a considerable snow having fallen, I coul not resist the temptation of going for several evenings to Lincoln's In Fields, during a very severe frost, in order to repeat and extend some o Mr. Wilson's experiments on snow. I soon, however, was obliged t desist." His symptoms became so alarming, that his friend, Dr. Liste thought he could not survive more than a few months. He says,—" set about immediately composing my Essay on Dew, as my papers con taining the facts on which my theory was founded would, after my deat be altogether unintelligible to any person who should look into them I laboured in consequence for several months with the greatest eagerne and assiduity, fancying that every page I wrote was something gaine from oblivion."

submitted by myself to the consideration of the learned, and which he, as a member of that body, has pronounced to be just. But I must, on the other hand, be permitted to say, that, if Dr. Young, forgetting that Newton became a glass-grinder in the service of science, will neglect to employ, for the increase of natural knowledge, the slow and laborious method of observation and experiment, and will frequently exhibit his speculations in a manner unsuited to the capacities of ordinary men, he ought not to think it strange that opinions advanced by him on difficult points of philosophy are not, agreeably to his own remark at the end of the criticism, received as truths beyond doubt, and are often not understood."

51. This is smartly said, but cannot, we think, be admitted as a sufficient answer to Dr. Young's strictures.

52. It will naturally be asked, What then remains for Wells? If all the phenomena had been observed, and even the theory pointed out, before he began his experiments, is there any merit at all to which he can justly lay claim? We answer, that to Wells belongs the rare merit of seeing clearly where other men saw obscurely; of grasping the whole while other men held only detached parts; of bringing the scattered and somewhat incoherent labours of other inquirers to bear upon his own experiments, which were undertaken with clearer views, and consequently a more direct purpose, than those of his predecessors; and the final result of his long and patient inquiry was the establishment of a theory of extreme beauty and simplicity, the truth of which subsequent inquiry has only tended to confirm: not that Dr. Wells is to be ranked as the author of this theory, but that his Essay was, as Dr. Whewell remarks,* "one of the books which

* "History of the Inductive Sciences." 3rd edition, vol. ii., 1857. After stating that bodies, cooled down by radiation into the clear night sky to the proper temperature, will condense vapour, Dr. Whewell adds,—"The same principle will obviously explain the formation of mists over streams and lakes when the air is cooler than the water, which was put forward by Davy even in 1819, as a new doctrine, or at least as not familiar." He might have shown, that Dalton, as far back as 1794, in the "Ladies' Diary," had explained correctly the phenomena described by Davy in the 'Philosophical Transactions.' In the "Diary" for 1793, Mr. John Dalton, of Kendal, puts the following query, which he answers himself in the "Diary" for 1794:—"What is the cause of the mist which is sometimes observable in a calm evening, especially in summer, to hover

drew most attention to the true doctrine, in this country at least."

53. Still, however, it is but an act of justice to rescue from oblivion the claims of such men as Le Roi, Pictet, Patrick Wilson, Prevost, and others. Honour to them does not diminish the merit due to Wells. It only restores to its proper course the progress of discovery, where in this, as in other branches of science, and indeed in other relations of life, men are as mutually dependent on each other for intellectual progress as they are for the supply of their ever-recurring daily wants. It is not given to one man to begin, continue, and complete the journey into the undiscovered land unaided and alone; others have preceded him in the attempt, and have left the impressions of their footsteps on the virgin soil, which, however faint and uncertain, serve nevertheless as guides to subsequent explorers.

over rivers, meadows, &c.?"—"This phenomenon is only observed when the air has suffered a sudden change of temperature from heat to cold. It is found (from experience) that warm air will hold more water in solution with it than cold air; therefore, when the air is suddenly cooled, which sometimes happens in an evening, the water being then much warmer than the air, it evaporates pretty copiously at the surface, but is no sooner carried up a little into the cold air, than it is precipitated again in the form of a mist, and occasions the phenomenon." (See Dr. Henry's Life of Dalton, published, 1854, by the Cavendish Society.)

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NOTE.—The reference made by Dr. Young (48) to Sir John Leslie's discoveries in connection with radiant heat, also noticed by Dr. Wells (49) as "the late discoveries of others," being necessary to his theory, would seem to call for some account of Leslie's aid in building up the modern theory of dew. But strange to say, he always opposed that theory; his own idea being that dew is caused by pulses of cold shot down from above. In his Essay on Dew, published in the Supplement to one of the early editions of the "Encyclopædia Britannica," and included in a collection of "Treatises on various subjects of Natural and Chemical Philosophy," published in 1838, after the author's death, we meet with such passages as the following:—"The descent of chill-air, caused by superior density, explains the formation of dew;" and again—"In fine bright evenings, those cold pulses rained from the sky, are sufficient alone to depress the temperature of the ground according to the seasons, sometimes 8°, but generally about 3° Fahr. The blades of grass thus chilled from exposure, cool in their turn the damp air which touches them, and cause it to drop its moisture." It is marvellous that such statements as these should proceed from one who had done so much to advance the science of radiant heat. It is equally astonishing, that while receiving with complacency Dr. Young's reference to his discoveries, he should wind up his treatise with the following criticism on Dr. Wells's labours:—"It might, perhaps, have been judged sufficient, if Dr. Wells had contented himself with assuming the coldness induced on the ground as merely an experimental fact. At any rate, we cannot help regretting that he should have sought the explication of this primary phenomenon from the very loose, cumbrous, and visionary hypothesis of M. Prevost, of Geneva, concerning what is gratuitously called *radiant heat.* We are at a loss, indeed, to conceive how a speculation so repugnant to all the principles of sound philosophy, should at this time have procured any favour, unless it proceeds from the blind admiration which the multitude are prone to entertain for whatever lulls the reasoning faculty, and appears cloudy and mysterious."

FINIS.

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