XXII. *On the repulsive Power of Heat. By the Rev.* Baden Powell, *M.A. F.R.S. Savilian Professor of Geometry in the University of Oxford.*

Received April 10,—Read June 19, 1834.

THE expansion of bodies by heat seems to imply a mutual repulsion of their particles; and it is a question naturally suggested, whether such a power of repulsion may not generally belong to heat, or be excited by it, between particles or masses of matter, at sensible as well as insensible distances.

But however obvious the suggestion of such an inquiry, it is not of a nature easy to be pursued or decided. The subject has been partially investigated by Signor Libri and by MM. Fresnel and Saigey; but their researches do not seem to have attracted much attention, and their results have even been regarded with considerable doubt. Very recently, however, Professor Forbes, of Edinburgh, has revived the inquiry, by referring to the same principle to account for the singular phenomena presented in certain vibrations of heated metallic bars, first noticed by Mr. Trevelyan, and since fully investigated by himself*. In a different form the subject had occupied my attention before I was acquainted with Professor Forbes's investigations; but on reading his paper, a new interest attached to the inquiry, and in pursuing it, I have obtained some results which appear to me decisive on a question, at once of importance in the analogies of physical action, and which has been hitherto regarded as involved in considerable uncertainty.

Signor Libri, I believe in 1824, examined the influence of heat on capillary attraction, and found that a drop of water suspended on a wire, when the wire was heated at one part, moved away from that part, both when the wire was horizontal and even when inclined upwards from the heated part. This he inferred was due to repulsion produced by the heat between the wire and the particles of the water.

M. Fresnel† employed discs of foil and of mica fixed vertically at the extremities of a delicately suspended magnetic needle in vacuo, placed so little out of the meridian that it just produced a pressure of the disc against another fixed disc. On heating either of the two with the sun's rays, concentrated by a lens, a sensible repulsion was produced. He showed that the effect was not occasioned by any current of the little air remaining, as it was not increased on the admission of more air; that it bore no relation to magnetic or electric conditions; and did not increase, but gene-

* Transactions of the Royal Society of Edinburgh, vol. xii.

† Annales de Chimie, vol. xxix. pp. 57, 107. (1825).

rally diminished, with thicker discs. He mentions other points on which his results had not been equally decisive, but allows that the whole subject requires further examination. The completion of this interesting inquiry is doubtless one of the numerous benefits of which science was deprived by his early loss.

M. SAIGEY*, in the course of a series of experiments on the development of magnetism in certain metallic bodies, notices some effects of repulsion, which (after examining every ordinary cause likely to have occasioned them,) he concludes by referring to heat. He tried the effect by means of a needle of lead finely suspended at different distances from a bar of copper, and found the number of oscillations in a given time decrease with the distance; that is, the needle more rapidly assumed the position of parallelism to the heated bar in which repulsion would tend to place it.

Signor LIBRI's result is remarkable as contradicting the statement of LAPLACE†, who speaks of the "repulsive force of heat" as subsisting among the particles of a fluid; but observes that *experiment shows* it has no other effect on capillary attraction than what results from its diminishing the density of the fluid.

In trying to repeat LIBRI's experiment, I have never been able to succeed, except in producing a slight apparent motion in the drop, which seems explicable from the mere effect of evaporation on the side next the heat.

I have observed a drop of oil, contained in a glass tube of about one tenth of an inch bore, move away from the part where heat is applied, evidently from the expansion of the glass, which renders the tube slightly conical, when the drop moves towards the narrower end. I have applied heat to capillary tubes till the suspended liquid has boiled, without producing any effect; to inclined glasses between which a drop of oil was advancing, without in the least affecting its motion; and to a plate of glass from the under side of which a globule of mercury remained suspended, without overcoming the attraction.

With regard to repulsion at greater distances, on employing an arrangement somewhat similar to FRESNEL's, when the discs were two small plates of glass with truly plane surfaces, I found that if in the first instance they were pressed together, so as to adhere, heat always overcame the attraction, and the moveable disc sometimes receded to a sensible distance. But this effect (and perhaps also that in FRESNEL's experiment) appeared to me in a great measure due to another cause than repulsion, viz. the slight curvature which will be given to the plate of glass by the greater expansion of the more heated surface, producing a convexity towards the heat.

The amount to which this takes place may be easily calculated from the known dilatation of glass, the difference of temperatures of the two surfaces, and the thickness.

In some cases, the two glasses were pressed so hard together that the *colours of thin plates* appeared between them. On the application of heat, these colours in-

* Bulletin Mathématique, tom. xi. No. 167.

† Mécanique Céleste, Supp., livr. x. p. 75.

stantly descended in the scale, and soon vanished. These tints, then, may be employed to furnish an exact indication of the most minute changes of distance between the surfaces, by whatever cause they may be produced; and the effect due to curvature by heat (or rather, in this case, the restoration of the bent glass to a plane figure,) might be calculated, and compared with the effect observed. I made many experiments in this way, and satisfied myself that the change of figure was *insufficient* to account for the *whole* observed effect, and that the separation indicated by the descent of the tints in the scale was therefore, in part, due *to a real repulsion.*

But I do not detail these experiments, because it is immediately evident that the use of *lenses* would afford a simple mode of deciding the question, divested of all influence of change of figure, without any calculation. It is evident, that if the rings be formed between a convex surface and one which is either convex, plane, or even concave of less curvature, heat applied outside of either glass will tend, by the change of figure in every case in the first instance, to *diminish the angle of contact*; that is, (if no other cause interfere,) to make the rings *enlarge,* without altering the central tint, until the curvature become equal to that of the convex surface.

In this form of the experiment I have invariably found that, *from the first moment, the rings regularly* CONTRACT, *and the central tint descends in the scale, till the whole vanishes.*

There are, however, several precautions necessary to be attended to. If the glasses be more than very slightly convex, the portion of the surface, throughout which they approach sufficiently close for the repulsion to act, is very small: this may render the total effect of the repulsive force too weak to overcome the weight of the upper glass, or even its inertia, though placed vertically. This difficulty I found with surfaces which gave the first bright ring, when the centre was a point of maximum brightness, about 0·1 inch diameter. Even here the rings never enlarged. But with surfaces of less curvature, which gave a diameter of 0·2 or 0·3 inch, the effect never failed to be exhibited, most decidedly, on bringing a red hot iron over the glasses when laid one on the other, without pressure.

The experiments, though simple in principle, certainly require some care: but with all precautions, and after the most careful consideration of all causes which can have tended to produce or affect the result, it appears to me that the separation of the glasses through the extremely small but finite and known spaces, whose changes are indicated by the degradation of the tints, can only be due to *the real action of a repulsive power, produced or excited between the surfaces of the glasses by the action of heat.*

There are many questions relating to the nature and properties of this repulsive power which are immediately suggested, and some of which appear capable of solution by variations of the same method.

The *distance* at which the repulsive power can act is shown by these experiments to extend beyond that at which the most extreme visible order of NEWTON's tints is formed. But I have also repeated the experiment successfully with the colours formed under the base of a prism placed upon a lens of very small convexity; and according to the analysis of these colours given by Sir JOHN HERSCHEL*, the distance is here about the 1100dth of an inch.

Beyond these small distances other methods must be resorted to. But the certainty of the result within these limits, confirms its probability at greater distances, as inferred by FRESNEL and SAIGEY.

I have tried many experiments with the view of ascertaining the relations of the repulsion by heat to different *substances and conditions of surface.* There are obvious difficulties in the way of such experiments, except in a few cases. I have found that the effect is produced not only between two surfaces of glass, but between glass and metal. I applied heat at the back of a plate of speculum metal, with a highly polished surface, on which the rings were formed with a convex lens; and on comparing the effect in this case with that similarly produced when a plate of glass of the same thickness was employed, the effect was decidedly less with the metal, notwithstanding its better conducting power: but its highly polished surface rendered it a much worse radiator.

In attempting to repeat the experiment with a coated or roughened surface, there is the radical difficulty of rendering the rings visible. There are obvious objections to coating the polished surface, and leaving a small space clear in the centre to form the rings, owing to inequality of surface and contact. But I have found that this is not absolutely necessary. The rings may be formed if the central part of the coating be only slightly rubbed, and particles of the coating left adhering. I have formed the rings when such particles are seen in the middle of them. With this precaution I tried many comparative experiments. The metal plate coated with China ink gave a greater effect than when plain. With a plate of glass coated with China ink, the smoke of a candle, and leaf-gold, the effect with the smoke and China ink was greater than with the leaf-gold, which accords with the greater radiating power of those surfaces.

But with all these coatings the effect was greater than with the plain glass. Whereas, according to Sir JOHN LESLIE, both the China ink and the leaf-gold have lower radiating powers than the glass. This difference I ascribe to the better contact which the lens has with the softer and more yielding surface of the coating against which it is pressed.

These comparative experiments were made by placing the plate, with the lens resting on it, at an invariable height above the flame of a spirit lamp.

From these experiments, then, though we may infer that, *cæteris paribus,* the better *radiating* power of the surface increases the effect, yet there are other circumstances which affect the result more powerfully, and these seem to be, in general, *whatever may tend to the more rapid communication of heat.*

* On Light, p. 641.

This is still more conspicuous when the rings are formed in a thin plate of *water* between the lenses. The effect is here even greater than in air, and, as we may presume, independent of radiation*.

We may then conclude, upon the whole, that the repulsive effect depends upon the *amount of heat communicated to the second surface by whatever means.*

Also, according to what was shown at first, viz. that heat cannot overcome capillary attraction, it follows that in the case of an interposed liquid the heat must be supposed to act by exciting repulsion directly between the two surfaces themselves, through the fluid, and not by weakening the attraction of the liquid for either of them.

* This variation of my experiment was first tried by Professor FORBES of Edinburgh, on receiving an account which I communicated to him of my experiments in January 1834.