IX. *Researches on Heat. Third Series.* § 1. *On the unequally Polarizable Nature of different kinds of Heat.* § 2. *On the Depolarization of Heat.* § 3. *On the Refrangibility of Heat.* By JAMES D. FORBES, Esq. F. R. SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.

Read 16th April 1838.

Introductory.

1. THE following paper is divided into three sections, containing three distinct yet intimately connected investigations. The two first on the Polarizability and Depolarization of Heat have arisen immediately out of the train of investigation contained in my two former papers, and the researches of others to which they gave rise. The third is on the Refrangibility of Heat, a point of the highest importance for theory.

2. The experiments on which these investigations are based have been performed almost exclusively during the past winter. Part of the experiments on Depolarization were, however, made in the winter 1836-7. The mode adopted for trying Refractive Indices I had long ago contemplated. It was not, however, put in practice until January last.

3. The methods of measuring heat, &c. are exactly those fully detailed in the Second Series, § 1. The only modification of importance was attaching a lens in front of the pile, as described in Art. 56 of this paper.

4. During the two years which have elapsed since the publication of the Second Series, I have not discovered any correction which I have to make upon the statements of my former papers, excepting as to the measure of the polarizing power of a pile of plates of rock-salt (Second Series, art. 25), which I find to be inexact.

§ 1. *On the unequally Polarizable Nature of different kinds of Heat.*

5. It has been my anxious wish to preserve these papers pure from even the appearance of controversy, and such members of the Society as have paid attention to the recent history of our present subject must be aware that, without making direct allusion to the doubts which have at different times been thrown upon my experiments, I have contented myself with adducing new facts and more convincing reasonings; and I have had the satisfaction to see that the general result

of this course has been the gradual abandonment of such doubts, and the entire adoption of my conclusions.

6. I believe that only a single exception remains to this statement. I expressed my belief in my *first* paper that heat was differently polarizable, according to the source whence it was derived. M. MELLONI* failed to verify this result, and the opposite conclusion, namely, that all kinds of heat are equally polarized by a given pile of mica, was prominently put forth by himself and M. BIOR as an important discovery.† Without any undue confidence in my first, confessedly imperfect, researches, I proceeded in my second paper (art. 22, *et seq.*) to give what I considered ample proofs of the correctness of the statement, though the great dissimilarity of the numbers arrived at from those of my first paper, shewed that the latter were worthy of very little confidence on the ground of numerical exactness, which, indeed, I never claimed for them. The later experiments, however, were made with a view to accurate results, and I stated certain forms of the experiment which I had devised on purpose to meet the objections of M. MELLONI, although I avoided mentioning his name.

7. It seems, however, that M. MELLONI, returning to the subject with his accustomed diligence, after receiving my second paper, still confirmed his former results, and he has attempted to shew, in a very long paper, published in the *Annales de Chimie* for May 1837 (which only appeared in October), that his results must be exact, and the probable source of my errors. I contented myself with giving a very brief answer to this paper in the *Philosophical Magazine* for December 1837, admitting the improbability that so experienced an operator as M. MELLONI should be wrong in his numerical results, but stating convincing grounds for believing that his explanation of my conclusion, founded on experimental errors, was inapplicable. The inquiry which I have since been led to make, and the entirely satisfactory explanation at which I have arrived of a difference so puzzling, terminating in a confirmation of my original statement, I now proceed to lay before the Society.

8. I have not the remotest intention of examining and criticising M. MELLONI's paper in the *Annales de Chimie* for May 1837, as respects trifling or personal matters, which I readily confide to the impartiality of those best qualified to judge: but it is quite necessary to state the facts which I had observed, and M. MELLONI's mode of accounting for them.

9. With two polarizing mica bundles of great tenuity, prepared in the method described, (II.‡ art. 20) marked I and K, I found that, with heat from an Argand-lamp, 72 to 74 per cent. of the incident rays were polarized, that is,—of 100

* Comptes Rendus de l'Académie des Sciences, ii. 140.

† Ibid. p. 194.

‡ To avoid circumlocution, I shall denote by I. II., &c. the First, Second, &c. Series of Researches, and by the succeeding Arabic numeral the Article referred to.

rays transmitted when the plates were parallel, 72 to 74 were stopped when one was *crossed* or its plane of refraction turned through 90° . With heat from boiling water, but 44 per cent. were polarized, and heat from sources of intermediate intensities gave intermediate results.

10. M. MELLONI ingeniously argued that this appearance might arise from the circumstance that the mica bundles becoming most heated by those kinds of heat which they absorbed most readily, or transmitted least easily (viz. heat of low temperature), the pile was continually receiving a supply of heat by secondary radiation from the mica, which, having no relation to the *Parallel* or *Crossed* positions of the plates I and K, of course tended to diminish the apparent polarization of the heat, or to equalize the effect in the two positions.

11. The supposed effect of secondary radiation from plates had been so often urged against my experiments, that, though as often proved to be insignificant or insensible, it gave me no surprise to see it started afresh, and in so plausible a manner. M. MELLONI was probably not aware that the screen for intercepting the heat was placed *between* the source of heat and the polarizing plate K (as shewn in the figure above), so that the mica plates were only absorbing heat during the exceedingly short time (10 seconds) of one swing or dynamical impulse of the needle, otherwise I do not think he would have urged so infinitesimal an objection.* I endeavoured, however, to meet it directly in this way. I took two mica bundles, G and H (see II. 22), and placed them parallel, as shewn in the figure below. But instead of placing the pile at P, where it receives at once the directly transmitted heat from S (the screen being removed), and the supposed secondary radiation of the surface *ab* of the mica plate, I placed it at *p*, identically situated with respect to the surface *ab*, but wholly removed from the influence of direct radiation from S.

* I might add, too, that, had he been aware of the extreme tenuity of the mica plates employed (of which more hereafter), he must have been led as a necessary consequence of his own reasonings to admit that the effect must be insignificant.—*Ann. de Chimie, Mai 1837, p. 13, note.*

When this experiment was performed with dark heat (which, according to MELLONI, ought to give the greatest effect), not the slightest movement of the Galvanometer-needle was observable on removing the screen, during a far longer space of time than is ever in practice allowed for the absorption of heat. This experiment ought to be considered quite conclusive.

12. M. MELLONI had hinted that the different dimensions of the Sources of Heat, and the various angles under which the rays fell on the mica plates must materially affect the results; and, as I was quite convinced that operating with parallel rays was the most correct method, I proceeded to repeat my experiments on his plan, with a salt lens placed in front of the source of heat so as to render the rays parallel; I also removed the polarizing and analyzing plates to a considerable distance from the pile, and afterwards varied their distance in order to see whether any adequate explanation of the discrepancy could thus be obtained.

13. The apparatus was arranged in the following way. P, the pile; A, a square pasteboard tube to protect it from currents of air; I and K, the analyzing and polarizing plates; B, a moveable screen; L, a rock-salt lens, in the focus of which is placed S, the source of heat. In these experiments the distance from the centre of the pile to the centre of the first mica plate or P I was 12 inches; PS = 24 inches. With this apparatus I found my former conclusions fully confirmed.

The apparent polarization was somewhat increased, as I had anticipated from the rays falling more nearly at a constant angle when previously rendered parallel; but the different polarizability of the different kinds of heat was even more distinctly marked than ever; whilst the distance of the mica plates from the pile was now such as to reduce to insignificance any effect of secondary radiation, had such before been sensible.

14. In prosecuting these experiments, most of which were repeated many times under various circumstances, I remarked more distinctly than formerly the influence of particular states of combustion of the source of heat upon the index of polarization, and the accidental variations to which this gives rise on different days, and even during the progress of an experiment. Heat from brass about 700° I have generally found the most uniform on different days, though there occasionally occurs in a series of experiments, considerable deviations from the mean. The Locatelli Lamp seems subject to greater variations, and the Argand still more; indeed, I have found it so impossible to maintain an Argand-lamp in a uniform state of combustion, even for a quarter of an hour, that I have lately abandoned the

use of it. But the quality of the heat from incandescent platinum varies between the widest limits. Nor is this wonderful; it is composed of heat from two very different sources combined in uncertain proportions, that from the incandescent coil of wire, and that from the alcohol flame which heats it. The intensity of incandescence, too, varies exceedingly. On one occasion when the incandescence was unusually bright, and the alcohol flame very low, I obtained a higher degree of polarization than I have ever done before or since. The ordinary proportion between the indications with I and K *parallel* and *crossed*, is with incandescent platinum 100 to 26 or 27. In this case it was 100 : 20; and when the heat was lifted by an interposed plate of thin glass, it rose as high as 100 : 13.

15. The general results obtained in the way above described are stated in the following Table, in which I have included the numbers for mercury heated to 410° , and for boiling water taken from the Second Series, art. 22; those experiments not having been repeated because the use of a lens is in those cases of little avail.

Polarizing Plates I and K.

Source of Heat.	Rays out of 100 polarized.
Argand-Lamp,	78
Locatelli-Lamp,	75 to 77
Incandescent Platinum (usually)	74 to 76
Incandescent Platinum, with Glass .06 inch thick, interposed, 6 to 7 per cent. more, or	80 to 82
Alcohol Flame,	78
Brass heated to about 700° ,	66.6
Ditto, with a plate of Mica .016 inch thick interposed, (between K and B)	80
Mercury in a Crucible at 410° ,	48
Boiling Water,	44

16. I next tried whether the closest possible approximation of the mica plate I to the pile would produce any effect. The pasteboard A was removed, and the mica plate I was brought up until it touched the funnel-shaped reflector of the pile. In this extreme case the apparent polarization was found to be diminished about 2 per cent., whether in the case of incandescent platinum or of dark heat. I shall not inquire whether any or how much of this effect was due to the heating of the mica surface, and how much to the reflection of heat from the interior of the cylindrical tubes containing the mica plates, since it is evident that this could not have produced the variation of effect shewn in the above experiments.

17. I presume that it will be conceded, that the experiments now cited, incontrovertibly establish the unequal polarizability of heat from different sources. Yet, I confess, I should have felt uneasy, could I have thrown no light upon the cause of the discrepancy between M. MELLONI'S results and my own. This I believe, that I am able completely and satisfactorily to do, allowing him every credit for the perfect exactitude of his experiments. For the sake of clearness, I will state the course by which I myself arrived at this result.

18. It occurred to me, that it would be satisfactory for the farther and independent confirmation of the conclusions just given (which were then only partially obtained), to examine the index of polarization (by which I mean the percentage of the heat stopped in the crossed position of the polarizing and analyzing plates) deducible for different sorts of heat, from a series of experiments made wholly without reference to this question, I mean those on Depolarization, considered in another section of this paper, and which, it will be seen by a reference to the mode of reduction there employed, required to be recomputed in order to give the index of polarization.

19. I at first imagined, that the experiments made with each of the three kinds of heat then employed (Argand-lamp, incandescent platinum, and dark hot brass) would give throughout the same result. This was far from being the case; the interposition of the depolarizing plate of mica between the polarizing and analyzing plate, acting simply by transmitting only certain rays of heat, had modified the index of polarization, and that more or less, as the thickness of the interposed mica was more or less considerable. Such a result might have been anticipated, as in exact conformity with the discovery I had formerly made; but I was misled by a false notion, which I had heedlessly adopted, and suffered to remain unquestioned, that, in order to affect the index of polarization, the heat must have been modified by transmission *previous* to its falling upon the first or polarizing plate, whilst, in the experiments referred to, the modification took place between polarization and analyzation.* Of course, when I perceived this oversight, the confirmation of my views was greater, because it was unforeseen.

20. But the most material result of the examination of those experiments was this. By a reference to the section on Depolarization, it will be seen that five different thicknesses of mica (varying from three to sixteen thousandths of an inch) were interposed successively, and the index of polarization determined for each of the three kinds of heat. Now, upon examining the result of these fifteen experiments, I clearly perceived (amongst occasional irregularities) this law to prevail,—*that, whilst a film of mica .003 inch thick scarcely altered the characteristic properties of heat from different sources, as shewn by their variable indices of polarization, an increased thickness of mica had almost no sensible effect upon the heat from the Argand-lamp, but it increased the index of polarization of dark heat so fast, that, with a thickness of mica of .016 inch interposed, the apparent index of polarization for heat from the Argand-lamp, incandescent platinum, and dark hot brass, was almost the same.*

* Lest this confusion should, by possibility, occur to any one, as it did to myself, I will observe that the position of the sifting or modifying plate, absorbing the least refrangible rays, is quite immaterial, provided it occur between the *source* and the *indicator* of heat, for whether the rays in question are absorbed before or after polarization, those which ultimately escape and reach the pile are the only ones of which the index of polarization is measured.

21. When I had fully seized this conclusion, the explanation of M. MELLONI's results was easy and complete. It appears from the account of his experiments, that he still employs piles of mica of the form I at first used, consisting of distinct laminæ separated by a knife, then laid together and united at the edges, up to the number of 30, 60, and even more.* On the other hand, the piles I and K, which for two years and a half I have employed, are of a degree of tenuity really surprising. The mode of their construction I mentioned briefly in my last paper, art. 20, and it is so very superior to any other, that it is probably from inadvertence that it has not been generally employed. The piles laminated by the action of violent heat, afford a multiplicity of parallel surfaces in a given thickness of mica, which no mechanical method can approach. The actual thickness of mica which they contain, I am unable accurately to estimate. The plates marked G and H are much thicker, perhaps twice as thick as those marked I and K, which I commonly use; yet the former, as I roughly estimate by the tint they give in polarized light, are only about one-thousandth of an inch in thickness. At the utmost, the plates I and K can be but one fifteen-hundredth of an inch; and yet it appears that their polarizing power (depending solely on the number of surfaces they contain) is equal to M. MELLONI's pile of ten distinct plates placed at the same angle (35° to the incident rays). The mean thickness of the elementary plates can, therefore, be only one fifteen-thousandth of an inch; and they reflect abundantly the colours of NEWTON's rings.

22. Now, I have found by the depolarization experiments, that it requires a much greater thickness of mica than that traversed by the heat in passing through the plates I and K (even allowing for the obliquity) to affect materially the index of polarization of heat from different sources, such as from brass at 700° , and incandescent platinum. It is, therefore, a necessary consequence of the construction, that the heat passes through such piles as I use unaltered, or nearly unaltered, in its character, whilst in passing through bundles of detached plates laid together, the thickness of mica to be traversed is sufficient to modify the heat by absorption, in such a way that *the difference of quality has vanished, whatever be the source, in the very act of transmission*. It is hardly likely, considering the size

* *Annales de Chimie*, Mai 1837. At p. 17, &c., M. MELLONI has given a minute account of that method of constructing the piles, which, "amongst several different ways, he considers the preferable one." No one could doubt from his language that he is describing a new and improved form of the apparatus. I regret for a moment to descend to notice an apparent want of justice and courtesy towards myself; but it is impossible for me not to observe, that the procedure he so exactly details, is, to almost the minutest particular, identical with that which I myself used in June 1835, in constructing, in M. MELLONI's presence, the first pair of piles used for polarizing heat which existed in France, at a time when M. MELLONI expressed his unqualified scepticism as to the polarization of heat generally; which piles I left, at his desire, where I presume they now are,—in his own possession. This mode of construction I soon after abandoned, for the improved one alluded to in the text.

of M. MELLONI's mica plates (4 inches long and 2 wide), that they could be less than one fifteen-hundredth of an inch thick each; a pile of ten would then be ten times as thick as my pile of equal energy, and at an incidence of 55° the thickness traversed would not be much shorter than that of the mica plate alluded to in art. 20, which we have there seen to be sufficient to obliterate all distinctive character as to polarizability between an Argand-lamp and dark heat.

23. Being now fully aware of the importance of the construction of piles of mica which I had adopted, I thought it worth while to examine the proportions of heat from different sources, which these very delicate laminæ were capable of transmitting, which, I presumed,* would be found far less variable than when plates of the usual thickness are employed. My expectations were more than realized, as is seen in the following Table, the second column of which shews the proportion, to the whole incident heat, of that transmitted by the two mica piles I and K placed parallel to each other; by far the greater proportion of the loss being that due to the obliquity of reflection and the number of surfaces.† By way of contrast, I have placed in the third column the proportion of the whole incident heat transmitted at a vertical incidence by a plate of mica .016 inch thick.

Source of Heat.	Rays out of 100 transmitted by	
	Plates I and K parallel.	Mica Plate .016 inch thick.
Locatelli-lamp,	18.8	57
Ditto, with plate of glass .06 inch thick interposed,	16.2	72
Incandescent platinum,	17.6	50
Dark hot brass (700°),	15.5	15
Heat from boiling water,	10.	8

24. It is very evident that, for the first four sources of heat at least, the transmissive power of the plates I and K varied little, and in no sort of proportion to the characteristic action of mica even in moderate thicknesses. This will be more evident, if we compare the ratios of the heat from different sources transmitted in the two cases, taking the heat from the lamp sifted by glass as the standard for each column.

	Plates I and K.	Mica .016 inch.
Locatelli with glass,	100	100
Locatelli,	116	79
Incandescent platinum,	108	70
Brass at 700° ,	96	21
Heat of 212° ,	62	11

25. I need hardly add, that so remarkable a result as that the heat sifted by glass should be less readily transmitted by the thin mica laminæ, than the direct heat from a lamp, was carefully verified.

* I do not state this as a new idea; it has been repeatedly remarked by M. MELLONI, that, in proportion as substances are thinner, they possess a more equable diathermancy for heat of different qualities.

† The part of the effect due to reflection, I had previously established to be nearly the same for different kinds of heat.

26. Since, then, the first four kinds of heat are transmitted without any great difference of proportion, by the piles I and K, and since, especially, the heat from a lamp sifted by glass and that from dark brass possess almost exactly similar characters in this respect, it is very clear that we have a new ground for rejecting as untenable M. MELLONI'S supposition (mentioned in art. 10), that the apparent differences of polarization in my experiments, arose from the unequal proportions of heat absorbed by the mica piles when the source varied.

27. Admitting, then, the fact of the variable index of polarization exhibited by heat of different qualities similarly treated, we are tempted to inquire what explanation can be offered of it. This question, inferring for its answer a knowledge of the nature of heat, we are not prepared to answer with confidence. My former suspicion of its being due solely to the difference of the refractive index of mica for heat of different kinds (II. 24), I am disposed to retract as inadequate, or at least to suspend my judgment respecting it. I at one time thought, that, supposing the mica bundles unequally permeable to heat from different sources, a difference of ratio in the total heat reaching the pile with the plates I and K *parallel* and *crossed* might be accounted for. But a careful analysis of the circumstances convinced me, that the absorptive action, if assumed the same for common and polarized heat, could produce no such effect. One of the most plausible suppositions which occurred to me was this,—that, supposing the reflection of luminous heat to take place more copiously at the mica surface than that of dark heat, and supposing the angle of incidence to be that of total polarization, since the refracted ray contains as much heat (if heat be like light) polarized *perpendicular* to the plane of incidence, as is reflected and polarized *in* the plane of incidence, the ratio of the polarized to the total heat transmitted would be greatest in the heat of highest temperature. Unfortunately for this theory, careful experiments assured me that heat from different sources underwent the same, or nearly the same, intensity of reflection under the same circumstances.

28. We are, therefore, led to regard this character of unequal polarizability, as probably indicating a difference of character of a fundamental kind between heat and light; at least a superadded quality or peculiarity of vibration, which becomes more and more sensible as heat is removed in its character from light, or has (as we shall hereafter see), generally speaking, a lower degree of refrangibility. A sensible undulation, normal to the surface of the wave, would of course satisfy this condition. I am far from saying that my experiments warrant such a conclusion. I am aware that it is inconsistent with the ideas entertained by some ingenious speculators upon the nature of heat;* but this very circumstance has led me to bestow the greater pains upon establishing the phenomenon in an incontrovertible manner.

* KELLAND on Heat, Art. 166.

§ 2. *On the Depolarization of Heat.*

29. In the first series of these researches, § 4, I entered pretty fully into the subject of depolarization. The establishment of the fact was of the highest importance, since there is little probability of proving in any more direct manner the doubly refractive energy of crystals with respect to heat. But, besides the demonstration of the fact, I pointed out in that paper the important numerical determinations to which it might lead; determinations of the first consequence to the theory of heat, and the discrimination of heat from light. The measure of depolarization in the case of light, or the quantity of light which has become polarized in a new plane by passing through a doubly refracting plate, such as mica, depends, 1. upon the length of a wave of light; and, 2. upon the retardation which one of the doubly refracted pencils of light suffers, upon the other, in passing through the mica, which retardation differs with the material of the plate, varies directly as its thickness, and *may* also vary with the quality of the incident ray.

30. Hence, as a little reflection clearly shews, if the quantity of light (or, by analogy, of heat) depolarized by a plate of given thickness be numerically estimated, we may, if the length of the wave be given, determine the retardation, or energy of double refraction; or, if the latter be assumed or known, we may find the length of a wave. Considering the latter element as the more important, and not being then in possession of any more direct mode of determining it numerically, I proposed to *assume* the retardation due to double refraction as the same for heat as in the case of light, (considering heat as but less refrangible light), and to determine the length of a wave in the way which I fully explained in the First Series, art. 68–75.

31. Two circumstances require notice by way of precaution. The first is, that, for the very reason that we have periodical colours in the case of light, there are *different* thicknesses of mica and different measures of retardation, which, for the same length of a wave, will give the same measure of depolarization; these dubious cases (which the formula of depolarization completely expresses) must be distinguished. The second remark is, that all our sources of heat furnishing heterogeneous rays, each has its own period of maximum and minimum intensity, just as in the case of solar light, and since our means of numerical estimation embraces the sum of all the effects of heterogeneous rays, we cannot expect results which shall rigorously satisfy a formula, in which homogeneity (or constancy of λ , the length of a wave), is assumed, but consider the approximate result as representing the mean or predominating character of the heat employed.

32. Recalling, then, Fresnel's formula, quoted in art. 70 of the First Series, we have

$$\frac{E^2}{F^2} = \sin^2 180^\circ \left\{ \frac{o-e}{\lambda} \right\}$$

where F^2 is the intensity of the whole incident polarized ray; E^2 the intensity of that portion which, after transmission through the depolarizing plate, is capable of being analyzed in a perpendicular plane. These two quantities being determined from observation, the first side of this equation, or their ratio, becomes known. On the second side we have two quantities, either of which may be assumed, and the other becomes known, viz. $o-e$ the retardation of the one doubly refracted ray upon the other within the crystal, and λ the length of a wave. Now, it is obvious from the form of the expression, that an infinite number of values of $\frac{o-e}{\lambda}$ will satisfy the equation; in light there can be little ambiguity arising from this cause, because the phenomena of periodic colours at once afford the means of selecting the true solution. In the case of heat, we must proceed with more caution, the value of $\frac{o-e}{\lambda}$ being wholly unknown; we only assume (as we are entitled to do) that this quantity increases uniformly with the thickness of the plate, which it necessarily must, since the retardation is as the thickness, and λ is independent of it. By a very simple process, the true value was easily selected.

33. Five depolarizing mica plates, of different thicknesses, of exactly the same quality, and each as uniform as possible, were provided. They were cut to the same size, and of such a form that each could at once be placed with its neutral axis (a line in the plane passing through the two axes of double refraction) vertical, or inclined 45° at pleasure. Their thickness was next to be determined. The examination of the colours shewn by polarized light was the most obvious method, but not susceptible of the exactness which was required. It was, however, used as a check. These colours were:

Retardation in Millionths of an inch.*									
No. 1. White inclining to yellow,	.	.	.	.	.	.	.	.	12
No. 2. Rich blue,	.	.	.	.	.	.	.	.	28
No. 3. A blue purple,	.	.	.	.	.	.	.	.	43
No. 4. Between red and orange,	.	.	.	.	.	.	.	.	36
No. 5. Pink,	.	.	.	.	.	.	.	.	80

34. The relative thicknesses which these numbers afford, are tolerably verified (excepting the first) by the following results of actual measurement, by means

* These numbers are obtained by doubling those due to the corresponding tints of thin plates of air in NEWTON'S Table. In the case of the two last numbers, there might have been some doubt as to the order of colours to which they belonged, but this was removed by the measurements given farther on, which shewed that the pink of No. 5. is a colour of the fourth order.

of a pair of callipers constructed for such purposes by TROUGHTON. These results are the mean of ten measures each, which were rendered difficult by the elastic and fissile nature of the substance.

	Thickness in parts of an inch.
No. 1.	.0026
No. 2.	.0044
No. 3.	.0074
No. 4.	.0060
No. 5.	.0157

35. With these mica plates in succession, employed for depolarizing, I proceeded to determine the ratio $\frac{E^2}{F^2}$ (art. 32) for the most part exactly in the way described and illustrated by an example in art. 71, *First Series*, which I found preferable to any other. This laborious investigation I performed for heat from three sources; (1), an Argand-lamp with glass chimney; (2), incandescent platinum; and, (3), brass heated (not to visible redness) by an alcohol flame. The thickness of the plates No. 3. and No. 4. being very nearly the same (and giving, as they ought to do, almost exactly the same measure of depolarization), I preferred using the united thickness of Nos. 2. and 3. as an interpolation between Nos 3. and 5. The swings of the needle, or dynamical effects, (II. 8) were always observed, and are alone given. The polarizing and analyzing plates were the same, marked I and K, before fully described (II. 20), and a plate is said to be at 0° or at 90° as its plane of refraction is vertical or horizontal. With these explanations, and a reference to art. 71, *First Series*, the following specimens of observations will, it is hoped, be intelligible.

ARGAND-LAMP: 16 inches from centre of Pile, depolarizing Mica Plate No. 3.

Position of Polarizing Plate K (I being always at 0°).	Position of Neutral Section of Mica.	Galvanometer. Dynamical Effect.	Total Polarization F^2 .	Depolarization E^2 .
At 0°	At 0°	11.9	8.45	
At 90°	—	3.45		+ 5.35
—	At 45°	8.8		
At 0°	—	6.75		— 5.35
—	At 0°	12.1	8.35	
At 90°	—	3.75		+ 5.05
—	At 45°	8.8		
At 0°	—	6.7		— 5.35
—	At 0°	12.05	8.35	
At 90°	—	3.7		
		Means	8.38	5.27

36. The following experiment was made with heat wholly unaccompanied by light, and with the same mica plate.

DARK HOT BRASS: 14 inches from Pile, depolarizing Mica No. 3.

Position of Polarizing Plate K (I being always at 0°).	Position of Neutral Section of Mica.	Galvanometer. Dynamical Effect.	Total Polarization F².	Depolarization E².
At 0°	At 0°	5.25	[3.25]*	+ 3.75
At 90°	—	2.0		
—	At 45°	5.75		
At 0°	—	2.15	4.0	— 3.8
—	At 0°	5.95		
At 90°	—	1.95		
—	At 45°	5.75	3.95	+ 3.7
At 0°	—	2.6		
—	At 0°	5.9		
At 90°	—	1.95	4.0	— 3.5
—	At 45°	5.65		
At 0°	—	2.3		
—	At 0°	5.8	3.98	3.64
At 90°	—	1.8		
		Mean		

37. It now remains to explain how these observations have been discussed. The ratio $\frac{E^2}{F^2}$ is at once obtained by dividing the second mean result by the first, and I have purposely quoted these observations, to shew how very nearly the plane of polarization was thrown at right angles by the action of this particular thickness of mica, especially in the case of dark heat, which appears to be owing to its greater homogeneity, as we shall presently have reason to infer.

38. We have seen above (art. 32) that

$$\frac{E^2}{F^2} = \sin^2 180^\circ \left\{ \frac{o-e}{\lambda} \right\}$$

And therefore,

$$\frac{o-e}{\lambda} = \frac{\sin^{-1} \sqrt{\frac{E^2}{F^2}}}{180^\circ}$$

Since the radical has an ambiguous sign, the equation will be satisfied by a value of $\frac{o-e}{\lambda}$ equal to a fractional number a , or by $1-a$, or $1+a$, or $2-a$, or

* Omitted in the mean as manifestly too small, arising from the lamp being just lighted, and the brass not fully heated.

$2 + a$, or $3 - a$, &c. In the case of the two examples given above, we have for the Argand

$$\frac{E^2}{F^2} = \frac{5.27}{8.38} = .629; \sqrt{\frac{E^2}{F^2}} = \pm .793$$

And
$$\frac{o-e}{\lambda} = .29 \text{ or } .71 \text{ or } 1.29 \text{ or } 1.71, \text{ \&c.}$$

For the dark heat,
$$\frac{E^2}{F^2} = \frac{3.64}{3.98} = .915; \sqrt{\frac{E^2}{F^2}} = \pm .957$$

And
$$\frac{o-e}{\lambda} = .41 \text{ or } .59 \text{ or } 1.41 \text{ or } 1.59, \text{ \&c.}$$

The true value must be such, that, when a number of plates are employed, $\frac{o-e}{\lambda}$ must increase *uniformly* with the thickness of the plates.

39. Clearly to mark this, and at the same time to combine the results by graphical interpolation, I projected the numbers obtained as above in the way shewn in Plate XI. Figs. 1, 2, and 3. On a horizontal line spaces representing the thickness of the plates (art. 34) were set off as abscissæ, and a few of the ambiguous values of $\frac{o-e}{\lambda}$ as ordinates, which are marked by dots. It was then easy to select those points thus set off, through which a straight line could most nearly be drawn, representing the linear relation between the thickness of the plate and the quantity $\frac{o-e}{\lambda}$, (both vanishing when the thickness = 0), and inspection of the figures will shew that no doubt can attach to the choice of the ambiguous numbers, and also that the straight line represents in general remarkably closely the course of those points.

40. There is one exception to this statement, and it is an important one. It will be observed that in all the three figures the interpolating line, instead of passing through any of the dots set off for the mica plate No. 3, bisects exactly *two* dots, which are nearest to one another in the case of dark heat,—wider apart with incandescent platinum, and widest of all in the case of the Argand-lamp. The explanation is complete and satisfactory. The interpolating line in all these cases gives a value of $\frac{o-e}{\lambda} = \frac{1}{2}$, which gives a value of $\frac{E^2}{F^2} = 1$; in other words, infers a *total* polarization of the heat in the horizontal plane (or in the case of light total darkness, when the polarizing and analyzing plates are parallel) which we know can only occur when the heat is absolutely homogeneous. The want of mathematical coincidence in this case infers the admitted physical condition of want of homogeneity in the incident rays. Hence, we infer that dark heat is most homogeneous; next, that from incandescent platinum; and, least of all, that from the Argand.

41. The numbers from which these projections are derived are contained in the following Table.*

Source of Heat.	Depolarizing Plate.	$\frac{E^2}{F^2}$	Values of $\frac{c-e}{\lambda}$
ARGAND-LAMP, . . .	No. 1.	$\frac{2.15}{7.38} = .291$	.18, .82, 1.18, &c.
	repeated.	$\frac{1.91}{6.67} = .286$	
	No. 2.	$\frac{5.65}{8.53} = .662$	
	No. 3.	$\frac{5.27}{8.38} = .629$	.29, .71, 1.29, &c.
	No. 2. + No. 3.	$\frac{2.07}{5.55} = .373$	.21, .79, 1.21, &c.
	No. 5.	$\frac{1.31}{4.38} = .299$	.185, .815, 1.185, &c.
	repeated.	$\frac{1.44}{4.83} = .298$	
INCANDESCENT PLATINUM,	No. 1.	$\frac{2.00}{7.60} = .264$	.17, .83, 1.17, &c.
	repeated.	$\frac{1.90}{7.68} = .248$	.165, .835, 1.165, &c.
	No. 2.	$\frac{4.66}{7.31} = .638$	.30, .70, 1.30, &c.
	No. 3.	$\frac{5.02}{6.70} = .749$	.335, .665, 1.335, &c.
	No. 4.	$\frac{5.63}{7.08} = .795$	.35, .65, 1.35, &c.
	No. 2. + No. 3.	$\frac{1.48}{4.66} = .318$	.19, .81, 1.19, &c.
	No. 5.	$\frac{1.35}{6.36} = .212$	.15, .85, 1.15, &c.
DARK HOT BRASS, .	No. 1.	$\frac{1.94}{7.35} = .264$	.17, .83, 1.17, &c.
	No. 2.	$\frac{3.17}{4.15} = .764$	.34, .66, 1.34, &c.
	No. 3.	$\frac{3.64}{3.98} = .915$	.41, .59, 1.41, &c.
	No. 2. + No. 3.	$\frac{1.01}{3.38} = .299$	.185, .815, 1.185, &c.
	No. 5.	$\frac{0.62}{4.89} = .127$	.115, .885, 1.115, &c.

42. When we examine the projected interpolating lines of Plate XI, which an attentive inspection will shew to have been laid down with the greatest care,† we are struck by the remarkable coincidence which obtains between them; a result so far contrary to what I expected, that it shews that by this method we cannot hope

* Most of the experiments on incandescent platinum were made early in 1837, the remainder during the winter 1837-8.

† The interpolating line for *Incandescent Platinum* is in the engraving placed at rather too high an angle.

to discriminate the different lengths of waves of these kinds of heat, as I had formerly supposed, and shews that the variation of λ must be very small, or else (what is improbable) that it is constantly proportional to the variation of the retardation $o - e$.

43. All the three figures give as nearly as possible a value of 1.4 for $\frac{o-e}{\lambda}$ at a thickness of depolarizing mica, equal to .020 inch, or .07 for a thickness of .001 inch. Let us compare this with the case of light. The sum of the retardations for the various mica plates, as given in art. 33, amounts to .000199 inch; the sum of the thicknesses in the next article is .0361 inch, consequently the mean value of the retardation or $o - e$ is .0000055 for a thickness of mica of one-thousandth of an inch. But the length of λ for extreme red is .0000266, for extreme violet, .0000167 inch. Hence for a plate of mica .001 inch thick the values of $\frac{o-e}{\lambda}$ are

For extreme Red light,	.	.	.	$\frac{55}{266} = .207$
For extreme Violet light,	.	.	.	$\frac{55}{167} = .329$
For Heat,	.	.	.	$= .07$

44. If we assume the retardation, or $o - e$, to be the same for all lengths of waves, and for heat as for light, we immediately deduce the value of λ , or the length of a wave of heat. For since for a plate .001 inch thick, $\frac{o-e}{\lambda} = .07$, as above, $o - e = .0000055$, we have

$$\lambda = \frac{o-e}{.07} = \frac{.00055}{7} = .000079 \text{ inch,}$$

about three times as long as a wave of red light, and four and a half times that of violet. But it is always to be remembered, that this proceeds on the supposition of the retardation being invariable.

45. I have taken the trouble to calculate and project in a similar manner my original observations on Depolarization given in the First Series of these researches, art. 74, in order that, though probably less accurate, they might form a check upon the results just given. The plates *then* employed, and marked No. 1 and No. 2, (which are not to be confounded with those so designated in this paper) had thicknesses (deduced from the retardations) of .0072 and .0036 inch. I have the gratification to find that the computed results agree almost precisely with those just obtained, although from the accidental thicknesses of the two plates employed the observations with *these alone* do not enable us to select the appropriate value of $\frac{o-e}{\lambda}$, there being at least two values which still remain ambiguous, but when taken in conjunction with the observations of art. 41, the ambiguity is at once removed, and the numerical value of λ comes out almost ex-

actly as stated above, for incandescent platinum and dark heat, and somewhat smaller for that of the Argand-lamp.

46. I desire it to be recollected, that, in speaking of these somewhat startling lengths of waves of heat, I am using the language of only one of the two hypotheses which serve to interpret the results of this section ; for, if the variation be in $o - e$, or the difference of the velocities of the doubly reflected rays in mica, the result would be the same. The experiments in a subsequent part of this paper may serve to guide us in our choice. Meanwhile, I would observe, that, supposing the above results to be explained on the supposition that $o - e$ is smaller, instead of λ greater for heat than for light, it is equivalent to supposing the doubly refracting energy weaker, or a greater thickness of a crystal required to produce a given effect. Our suggestion respecting the existence of sensible vibrations normal to the wave surface (art. 28) will not avail us here. For, by the mode of reducing the experiments on Depolarization, the unpolarized part of the heat does not enter into consideration at all ;* consequently those parts of the total effect which are due to transverse vibrations alone, are not modified by double refraction as so much light would be.

§ 3. *On the Refrangibility of Heat.*

47. Since the admirable discovery by M. MELLONI of the power of rock-salt to transmit and refract heat of every kind, one of the most obvious and important questions (formerly intractable) of which it seemed to offer the means of solution, was the accurate determination of the refrangibility of heat from various sources, luminous or non-luminous. Such a determination is of the first consequence to the formation of a just theory of heat, and a detection of the subtle bond by which it is connected with the comparatively familiar modifications of light.

48. Such experiments have not been wanting. M. MELLONI, in his Second Memoir on Radiant Heat, in the *Annales de Chimie* for April 1834, has described the apparatus which he employed, and which is figured in Plate III. of that volume. It consists of a thermo-electric pile, constructed of a single vertical row of elements, so as to be exposed to a very narrow beam of heat. It was made to move on a sector of a circle, at whose centre was placed a prism, by which the beam of heat was refracted from its primitive direction ab into that cd , (see next page), and therefore produced a maximum effect on the galvanometer when

* I do not mean to offer any opinion on the nature of light in a partially polarized ray generally ; but, as in the present case, the angle of incidence is that of complete polarization nearly, I presume that the transmitted ray is undoubtedly composed partly of light polarized perpendicularly to the plane of incidence, and partly of common light.

the pile was at d . The other parts maintaining the same positions, it is evident that the pile must be moved into the position d' , if the source of heat be now one

yielding rays of greater refrangibility. Although the radius of the circular arc was (if I understand the account rightly) eleven inches, but little deviation of position was required for heat from different sources; and M. MELLONI admits that, whilst his experiment *indicates* the difference of refrangibility, it is inadequate to *measure* it.

49. There are many reasons why such a form of apparatus must be rejected for accurate observations. I will mention only the impossibility of obtaining a beam of heat which shall preserve the same breadth at different distances from its source (of course supposing the rays rendered as parallel as possible by refraction through a rock-salt lens), arising, 1. from the angular magnitude of the source; 2. from the scattered reflection and refraction at the surfaces of the lens and prism; 3. from the want of homogeneity of the ray. On all these accounts, the beam must have acquired a very sensible breadth at the distance of the pile, and consequently the effect of heat must be perceptible, and even nearly uniform, through a certain space. I may also add from experience, that the difficulty of varying the arrangement of an experiment, so as to get a maximum heating effect at the pile, is so considerable, that no delicate result can be deduced from the merely tentative procedure. Finally, the smallness of the variation of refrangibility, seems to require some more critical method of ascertaining its measure. On all these grounds, it seemed to me desirable to discover a method in some degree less open to objection.

50. The phenomenon of total reflection, successfully employed by Dr WOLASTON in the measurement of refractive indices in the case of light,* presents the advantage of being (theoretically at least) abrupt in its action, the transition from partial to total reflection being (with the necessary exception arising from the want of homogeneity) an instantaneous change, amounting in the case of light to many times the intensity of the smaller effect. It seemed reasonable to expect, that an apparatus constructed on the principle of determining the critical angle

* *Phil. Trans.* 1802.

of total reflection of heat from different sources within a prism, would afford much more definite information as to the refrangibility of heat than any other method. After much consideration, an apparatus of the following kind was adopted.

51. It is fundamentally composed of a jointed frame, resembling a box exactly square, ten inches in the side, without top or bottom, and having hinges at every angle, so that it may be formed into a lozenge of any degree of obliquity. This is seen in Plate XIII, Fig. 1, and marked AB. By an arrangement presently to be described, the rays of heat are made to pass parallel to the edge ac of one of the sides of the box, and to fall upon the prism P, whence, after undergoing reflection (total or partial) at the posterior surface of the prism, they proceed parallel to the line ad , and fall upon the sentient extremity of the pile at p . Now, in order that this course may be taken by the reflected rays, it is necessary that, supposing the prism to be an isosceles one, the posterior reflecting surface $a'b'$ Fig. 2, should form equal angles with the incident and reflected rays ce and fd . It was to effect this that the arrangement of the jointed lozenge was adopted. The prism P (Fig. 1) rests on a column O, moveable round the line of junction of the sides C and D of the lozenge. The column O has connected with it a tail-piece of brass aE passing through the diagonal of the frame, and preserved constantly in that position by a slit parallel to its length, through which passes a clamping screw b , serving at once to maintain this constancy of direction, to secure the form of the moveable lozenge, and by means of an index pointing to a graduated scale of inches reckoned from a , along aE , to determine the length of the diagonal ab at any moment, and consequently the angles of the lozenge.

52. A little consideration of this mechanical arrangement, will shew how it is adapted to the end in view. The rays from a source of heat S, rendered parallel by the lens of rock-salt L, fall upon the prism P, and, after undergoing two refractions and one reflection, they fall upon the sentient surface of the pile p . This will always take place so long as the posterior surface of the prism forms equal angles with the lines ac , ad , which will be secured by making it truly perpendicular to the tail-piece aE , by which it is guided, and which of course always bisects the angle cad . Now, it is evident that, whilst the angle cad remains small, the reflection will continue *partial*, but that as the diagonal ab is shortened, a point will be reached when *total* reflection abruptly commences, which ought to be indicated by a *saltus* in the movement of the galvanometer connected with the pile. This critical angle will be soonest attained for rays of greatest refrangibility, and the calculation of the refractive index of the prism is reduced to a simply mathematical problem.

53. Before going farther, we shall proceed to solve this problem, viz.: *A ray of light GD (fig. next page) falls upon the surface AC of a prism, which has the angles at A and B equal; it falls upon the surface AB at the critical angle of total reflection; required the index of refraction (μ) of the prism the angle of incidence (α) being given.*

What is true of one ray GD, which after refraction meets the posterior surface at K, its middle point, will be true of any other parallel to it; also the incident and emergent rays DG, EH, form equal angles with the surface AB when the angles A and B are equal. By hypothesis $\angle DKC =$ the angle of total internal reflection $= \sin^{-1} \frac{1}{\mu} = \beta$. Let $\angle KDC = k$, the angle of refraction, $= \epsilon$, then $\sin \alpha = \mu \sin \epsilon$. Also, considering α positive when G falls between L and C, and the corresponding value of ϵ also +, we have in the triangle KDC

$$180^\circ = \beta + \angle ACK + (90^\circ + \epsilon)$$

And, calling the angle at C, $\angle I$, $\angle ACK = \frac{1}{2}I$,

$$90^\circ = \beta + \frac{1}{2}I + \epsilon.$$

But

$$\begin{aligned} \sin \alpha &= \mu \sin \epsilon = \mu \sin (90^\circ - (\beta + \frac{1}{2}I)) \\ &= \mu \cos (\beta + \frac{1}{2}I) \\ &= \mu \{ \cos \beta \cos \frac{1}{2}I - \sin \beta \sin \frac{1}{2}I \} \\ &= \mu \{ \sqrt{1 - \sin^2 \beta} \cos \frac{1}{2}I - \sin \beta \sin \frac{1}{2}I \} \\ (\text{Also since } \sin \beta &= \frac{1}{\mu},) \end{aligned}$$

$$= \mu \left\{ \sqrt{1 - \frac{1}{\mu^2}} \cos \frac{1}{2}I - \frac{1}{\mu} \sin \frac{1}{2}I \right\}$$

$$= \sqrt{\mu^2 - 1} \cos \frac{1}{2}I - \sin \frac{1}{2}I$$

Whence

$$\mu = \sqrt{1 + \left(\frac{\sin \alpha + \sin \frac{1}{2}I}{\cos \frac{1}{2}I} \right)^2}$$

54. I had a rock-salt prism constructed, so that the incidence on the first surface might be nearly vertical at the critical angle of total reflection, so as to avoid as much as possible any error arising from imperfections of the surface, or want of absolute equality of the angles at A and B; and likewise, that within the limits of the experiment, the loss of heat by reflection at the two surfaces might be nearly unaltered, as it is believed to be almost constant at incidences tolerably nearly perpendicular.* This prism, constructed for me by Mr JOHN ADIE, had two angles of 40° and one of 100° ; and so accurately was it made, that (satisfying myself with a careful measurement by the common goniometer, extreme nicety being unimportant) the angles appeared to be true to those quantities within a few minutes of a degree.

55. By a reference to Plate XIII, Fig. 1, it will now be understood that the required arrangement is of this kind. The heat diverging from the source S, is converted into an approximately parallel beam by the lens L. It then passes

* See MELLONI on the Reflection of Heat, *Annales de Chimie*, Dec. 1835.

through a diaphragm T , placed on one or other side of the prism (it does not much matter which, as the beam which arrives at the pile is always much wider than the second diaphragm t , placed there to admit only the central rays arriving parallel to the line ac). The use of this diaphragm is, that a narrow enough pencil of rays may be employed, to be independent of the variable breadth under which the surface of the prism is presented to the incident beam. The usual dimension of this diaphragm was one inch in breadth, and one and a quarter in height, but in some instances its breadth was reduced to three-eighths of an inch.

56. The pile p has its funnel-shaped orifice closed by a screen with a vertical slit, an inch wide, in the direction of its axis. But there is a peculiarity in the arrangement of the pile very essential to the success of these experiments, where the pile itself is moveable, which I must not omit to mention. Its exposure to currents of air would render the observations, when the pile cannot be entirely enclosed by a box or screen, very capricious in its action. I therefore adapted to the end, bearing the conical reflector (II. 6), an adjustable wooden tube r , containing a rock-salt lens, which still farther increased its sensibility, and totally protected it from aërial currents.

57. The more important adjustments of the apparatus previous to use, are these : 1. To place the surface $a'b'$ of the prism (Fig. 2) so as to form equal angles with the sides of the lozenge $K\gamma$, $K\lambda$, the point K being precisely above the angle of the lozenge frame. To accomplish this, the prism rests upon a brass plate, having an adjusting motion concentric with that of the pillar O (Fig. 1), on which it rests. The adjustment was made by placing a piece of truly parallel mirror-glass in the position of the posterior surface of the prism, suspending two plumb lines in the prolongation of the lines ac , ad , and observing by the eye placed at c whether the reflection of the other was seen in the direction ac , and adjusting the brass plate before mentioned, bearing the mirror, until such was the case, then making it fast by a clamping screw. 2. The next adjustment was to bring the centre of the lens L into the line ac , which was done by placing a small flame of a lamp in the position of the axis of the pile p , and regulating the position of the lens until the image of it fell exactly upon the prolongation of the line ac , the prism being so placed that the angles of incidence were almost perpendicular; (reflection at a mirror would have been preferable). 3. The adjustment of the source of heat behind the lens is the next point. When the source is luminous, it is done by causing the axis of the refracted cylinder of light to coincide with the line ac ; when not luminous, its breadth being usually considerable, it is found that a small displacement in one direction or another, makes but a small difference in the effect upon the pile.

58. The abruptness of the effect of transition from partial to total reflection is far from being so complete as might be wished; and this is easier accounted for than remedied. It arises mainly from the magnitude of the source of heat, the

consequent want of parallelism of the refracted rays, the scattering of these rays in consequence of the imperfect polish of the surfaces, the unequal intensity of the rays in different parts of the section of the cylinder, and lastly, from the want of homogeneity of the rays of heat from any source, which the method would serve to measure, were the other imperfections removed, just as in the course of the total reflection of light, prismatic colours are successively presented.

59. My first rude attempts shewed all this very clearly. As the diagonal ab of the lozenge (Fig. 1) shortened, total reflection obviously succeeded to partial, and the change was not only very great, but near one point very rapid. The point where the most rapid increase took place, is obviously that where the greater proportion of the incident rays underwent total reflection, and might therefore be taken as a mean representation of the quality of the heat. Still the change was too gradual to enable one by mere inspection to determine this point with accuracy, and I speedily resolved to take the sure but laborious method of ascertaining at a number of points intermediate between total and partial reflection the intensities of the reflected heat, and by constructing a curve having measures of the diagonal of the lozenge (a function of the angle of incidence) for abscissæ, and intensities for ordinates, I endeavoured to discover graphically for what value of the former the measure of the latter increased most rapidly, in other words, where the tangent made the greatest angle with the axis, or where was the point of contrary flexure of the curve.

60. Plate XIII, Fig. 3, may represent such a curve. I have found that when the diagonal of the lozenge was 14.5 inches, the reflection was in all cases nearly total, or the galvanometer was little affected by any increase of the angle of incidence. This effect, measured by the vertical line AB, was denoted by 100. When the diagonal was increased to 15.0, the effect was reduced, we shall suppose, to 90, expounded by the line CD, at 15.5 by EF, and so forth. An interpolating curve drawn through the points so fixed, would have its greatest inclination to the axis AX, when, for a given variation of the diagonal, the decrement of the intensity was a maximum, in other words, at the determining angle for the predominating part of the heat used. Such a point of contrary flexure would therefore determine the *mean* index of refraction of the given kind of heat by the aid of the formula above investigated, whilst the form of the curve would lead to some conjecture at least, respecting the distribution of heat of the more or less refrangible kinds in the given ray. Heat of low refrangibility being the last to be totally reflected, would cause the curve to droop fastest near the extremity B, the more refrangible rays would be cut off at the other end I of the curve.

61. I lost no time in verifying the general truth of the principle, and also of the received doctrines respecting heat, by examining the *quality* of the heat which reached the pile at different stages of total reflection. If, as M. MELLONI first rendered probable, heat of *low* temperature is *least* refrangible, and *vice versa* ;

and farther, if it be admitted that such heat passes most difficultly through such substances as glass, it follows, that after total reflection has proceeded a certain way, so that the more refrangible, and therefore more transmissible, rays have suffered total reflection, whilst the remaining rays constituting the primitive beam continue to be refracted, the heat thus reflected will be more copiously transmitted by glass, than when it came direct from the source. This conjecture was precisely verified.

62. Subsequent experiment still more fully confirmed this result, and by shewing that, during the whole progress from partial to total reflection, the specific quality of the heat changes, gave countenance to the view that the gradation is in a great measure owing to the want of homogeneity of the heat, and that the figure of the curve becomes (as we have said) a real test of the composition of a ray.

63. At the inferior limit of the curve, or when partial reflection takes place, all kinds of heat are equally reflected (in the case of light, the light is white), just as at the superior limit, or after total reflection is complete, the beam has exactly the same relative composition as before. In the intermediate stages the composition is perpetually varying. The first rays totally reflected (and combining with the scattered and partially reflected rays) are the more refrangible, or those more easily transmitted by glass. At a certain point a maximum proportion of these enter into the reflected beam. As the angle of incidence becomes greater, more and more of the less refrangible rays enter into the composition of the reflected heat, which at last possesses the same qualities as at first. This is well illustrated by the following early experiment which I made on the proportion of the reflected rays transmitted by a plate of glass .06 inch thick, at different stages of reflection (7th February 1838).

Diagonal <i>a b</i> , Fig. 1, in Inches.	Deviations of Galvanometer.		Ratio.	REMARKS.
	Glass.	No Glass.		
14.5	8.3	13.75	60 : 100	Total reflection complete.
15.0	7.85	12.65	62 : 100	
15.25	7.1	10.9	65 : 100	
15.5	5.5	7.85	70 : 100	
15.75	3.4	5.1	67 : 100	
16.0	2.3	3.75	61 : 100	Partial reflection.
16.5	1.45	2.3	63 : 100	

64. The experiments of which I am now to state briefly the results, were made with heat from various sources, and modified by transmission through different media. Considering them of great importance, I have spared no pains in verifying the results, and ascertaining the limits of error. My latest experiments, in which I availed myself of the experience which earlier ones had afforded, are of course most to be depended upon, and to them I shall chiefly refer

(made between the 21st March and the present date) ; but it is important for the credit of the results to observe, that they not only derive a general confirmation, but exhibit an almost exact numerical coincidence with those obtained formerly, and with less careful adjustments.

65. Avoiding, then (as in these papers I have habitually done), the tedious detail of minute precautions which the experienced operator will soon discover, and which to others would be of little use, it is to be understood that in the following experiments on the law of the Transition from Partial to Total Reflection, the arrangement was that shewn in Plate XIII. Fig. 1, and described (with the adjustments) in arts. 51–57;—that the centre of the pile p was 13 inches from the prism P , and the distance of the source of heat S from P was 12 inches;—that a diaphragm T , whose aperture was 1 inch by $1\frac{1}{4}$, was placed in the path of the ray usually between P and L near P ;—that the aperture of the pile was contracted to a breadth of one inch, whose centre was exactly in the line ad ;—and that only that part of the prism was employed which was free from flaws capable of producing total reflection.

66. The diagonal of the lozenge frame was varied from 14.5 inches up to 16.5 or 17.0, about eight observations of the intensity of reflected light being made at intervals. The series was then frequently *reversed*, and the mean results of the going and returning series taken to allow for any change which might have occurred in the intensity of the course. In all cases an observation of verification was made and such change allowed for. The dynamical effect on the galvanometer (II. 8) was observed and noted.

67. In reducing the observations the following plan was adopted. The intensity corresponding to the diagonal 14.5 inches being assumed = 100, the other intensities were reduced relatively to it, and projected, as explained in art. 60. By this means different series of observations became at once comparable with each other, and the beauty and regularity of the curves thus formed, and the almost perfect identity of those obtained on different days, and with different adjustments, give a degree of confidence in the results which is extremely satisfactory. When from the nature of the heat the effect was very small (as in the case of alum being interposed, or the source being of low temperature), I have endeavoured to supply the deficiency by multiplying observations, and the uniformity of the curves thus obtained has been the test of my success. Where this test has failed (as in the attempt to work with heat of 212°), I have suppressed the results.

68. I am unwilling to swell this paper by a quotation of individual experiments, of which the number is very great,* but I think it fair to give specimens of the actual work in a few cases.

* It may not be superfluous to state, that during the course of the experiments referred to in this series of papers, I have adopted a uniform and clear system of recording my experiments, which admits of subsequent reference, and, if necessary, of publication. The experiments have been fairly written out,

DARK HOT BRASS. March 31. 1838.

Measure of Diagonal <i>a b</i> , Plate xlii. Fig. 1, in Inches.	Galvanometer-Needle		Excess.	Ratio to Result with Diagonal = 14.5.
	Stands at	Swings to		
14.5	A 0.1	A 11.0	10.9	100 : 100
15.0	0.0	9.3	9.3	85 : 100
15.25	0.15	8.0	7.85	72 : 100
15.5	0.15	6.2	6.05	55 : 100
—	0.3	6.3	6.0	
15.75	0.25	4.6	4.35	40 : 100
16.0	0.15	3.15	3.0	28 : 100
16.25	0.2	2.25	2.05	19 : 100
16.5	0.15	1.9	1.75	16 : 100
14.5	0.05	10.8	10.75	

INCANDESCENT PLATINUM. February 10. 1838.

Measure of Diagonal <i>a b</i> , Plate xlii. Fig. 1, in Inches.	Galvanometer-Needle		Excess.	Ratio to Result with Diagonal = 14.5.
	Stands at	Swings to		
14.0	B 0.25	A 15.25	15.5	100 : 100
14.5	A 0.05	15.5	15.45	100 : 100
15.0	B 0.15	13.65	13.8	89 : 100
15.25	0.0	11.75	11.75	75 : 100
15.5	0.1	8.8	8.9	58 : 100
15.75	0.1	6.1	6.2	40 : 100
16.0	0.1	4.05	4.15	27 : 100
16.25	0.0	3.3	3.3	21 : 100
16.5	0.15	2.5	2.65	17 : 100
16.75	0.0	2.25	2.25	15 : 100
14.0	0.9	14.4	15.3	
.....	1.25	13.75	15.0	
.....	1.7	13.75	15.45	

HEAT FROM LOCATELLI LAMP SIFTED BY A PLATE OF ALUM. March 31. 1838.

Diagonal.	Dynamical Effects.		Mean Effect.	Ratio to 14.5.
	Direct Series.	Reversed Series.		
14.5	3.1	3.2 3.25	3.18	100 : 100
15.0	2.95	2.9	2.92	91 : 100
15.25	2.9	2.65 2.5 2.7	2.69	84 : 100
15.5	2.15	2.1	2.12	66 : 100
15.75	1.75 1.7	1.75	1.73	54 : 100
16.0	1.2	1.25	1.22	38 : 100
16.25	0.8	0.6 0.6	0.67	21 : 100
16.5	0.75 0.75	0.7	0.73	23 : 100
17.0	0.55		0.55	17 : 100

averages and ratios taken, generally on the same day on which they were made. This methodical plan cannot be too strongly recommended. Much after anxiety is spared, the calculations are lightened, errors avoided in the reduction after minute circumstances have been forgotten, and suggestions are afforded by the result of past experiments for the conduct of new ones.

69. After the observations made as now described have been projected in the form shewn, Plate XIII, Fig. 3, and Plate XII, the diagonal corresponding to the maximum rate of decrease of the intensity was determined, for the purpose of deducing the index of refraction. The following enumerations of the kinds of heat employed, and the results derived from the several projections, will give a just idea of the confidence due to the results. They are distinguished into those made since, and those previous to the 21st March, because some additional precautions have been taken since that time, which do not, however, appear to have produced a sensible change. Of the experiments made in the way above described, only one series is rejected, on account of its discrepancy from others of the same kind, (the discrepancy was so large as to indicate a displacement of the prism, or some fundamental derangement not perceived at the time); and another on account of the irregularity of the points marked out for the curve, although the general form of the curve did not differ from others similarly obtained.

70. *Sources of Heat.*—(1.) The direct rays of the Locatelli Lamp. A slightly concave reflector was employed. (2.) The same lamp, with a reflector having the form of a portion of a sphere concentric with the wick; the heat transmitted through *alum*. (3.) Heat from the same source transmitted by *window-glass* .06 inch thick. (4.) Heat from the same transmitted by opaque *black glass* (through which the disk of the unclouded sun is just visible). (5.) Heat from the same transmitted through *dark coloured mica*, by which direct sunlight is absolutely stopped. This singular substance I long sought for in vain, it is unknown to many practical mineralogists; it transmits green light at small thicknesses, when thicker its colour is hair-brown. By reflected light its colour is between green and black. (6.) Heat from incandescent platinum. (7.) The same sifted by *window-glass* as above. (8.) The same sifted by *opaque mica*. (9.) Heat from dark brass about 700°. This is obtained from a nearly cylindrical cover of smoked brass placed over the flame of a spirit-lamp, so as entirely to conceal it, and which gives remarkably good results, without increasing considerably the angular breadth of the source (which is greatly to be avoided when a lens is used). It is in fact not much greater in size than the helical coil of platinum wire used in (6). (10.) The same, sifted by clear *mica* .0044 inch thick. (11.) Heat from a crucible of mercury about 450°. The crucible was about 2 inches in the side, smoked externally, and heated by a spirit-lamp. The temperature of the mercury which it contained (covered with sand) was noted at each observation by means of an inserted thermometer.

71. The results were the following:

SOURCE OF HEAT.	Diagonal corresponding to Point of contrary flexure of Curve.				
	Before March 21.			Since March 21.	
Locatelli ; direct,	15.47	15.50	15.60 *	15.54	15.47
----- with Alum,				15.79	15.73
----- Window-Glass, . .	15.64			15.70	15.60
----- Opaque Glass, . .				15.75	15.67
----- Mica,				15.60	15.62
Incandescent Platinum, . .	15.51	15.47		15.52	
Ditto, with Glass,				15.64	15.67
Ditto with opaque Mica, . .				15.62	
Brass at 700°,	15.44	15.42		15.45	15.47 15.45
Ditto with clear Mica, . .				15.62†	15.55
Mercury at 450°,				15.52	15.52 15.45

72. From these numbers we can of course compute the corresponding angles of incidence, and thence the value of the index of refraction by the formula of art. 53. For the purpose of ready comparison I have calculated the following table, which gives the angles of incidence, and consequently the indices of refraction, corresponding to different values of the diagonal computed by the formula of art. 53 :

Diagonal <i>ab</i> .	Angle of Inci- dence = α .	Angle of Total Reflection = β .	Index of Re- fraction = μ .
Inches.			
15.0	— 1° 25'	40° 55'	1.527
15.1		40 37	1.536
15.2	— 0 32	40 20	1.545
15.3		40 3	1.554
15.4	+ 0 21	39 47	1.563
15.5		39 30	1.572
15.6	+ 1 16	39 12	1.582
15.7		38 55	1.592
15.8	+ 2 11	38 37	1.602
15.9		38 20	1.612
16.0	+ 3 8	38 4	1.622

73. We have the following mean values of *ab* for the points of contrary flexure, and consequent values of the indices of refraction of the *most abundant* rays in each source :

* This observation was made with a very contracted diaphragm ; the readings therefore were very small. It is omitted in the final reductions.

† Omitted in the final reductions on account of the irregularity of the observations.

SOURCE OF HEAT.	ab .	μ .
Locatelli, direct,	15.49	1.571
----- with Alum,	15.76	1.598
----- Window-Glass,	15.65	1.587
----- opaque Glass,	15.71	1.593
----- Mica,	15.61	1.583
Incandescent Platinum,	15.50	1.572
Ditto with Glass,	15.66	1.588
----- opaque Mica,	15.62	1.584
Brass at 700°,	15.45	1.568
Ditto with clear Mica,	15.55	1.577
Mercury at 450°,	15.50	1.572
MEAN LUMINOUS RAYS,	15.8	1.602

74. In the following table I have given the *mean* results of the different series of observations on which the above conclusions are founded, and from these numbers I have projected the curves exhibited in Plate XII, the dots corresponding to the numbers here given, and the mode of projection being that already explained :

SOURCE OF HEAT.	Values of the Diagonal ab .							
	14.5	15.0	15.25	15.5	15.75	16.0	16.25	16.5
Locatelli; direct,	100	93	80	60	41	30	2	18
----- with Alum,	100	91.5	84.5	71.5	51	39	24	18.5
----- Window-Glass,	100	93	84	66	47	33	23.5	18
----- opaque Glass,	100	97.5	89	75.5	55	42.5	26	22.5
----- Mica,	100	94	82	67.5	48.5	32.5	23	20
Incandescent Platinum,	100	89	75	58	41	30	22	18
Ditto with Glass,	100	88	77	62.5	42.5	30.5	23	17.5
----- opaque Mica,	100	92	78	63	46	30	22	18
Brass at 700°,	100	84	69	51	35.5	25	20	15
Ditto with clear Mica,	100	85	71	52	33	26	13	11
Mercury at 450°,	100	92	77	57	42	29	22	13

75. When we compare the preceding results, obtained with a rock-salt prism, with those for light, we find that the *received* index of refraction for that substance would give to heat a higher degree of refrangibility than light, a result contrary to all probability. This, however, is not confirmed by direct experiment. Placing a bright small source of light at S (Plate XIII. Fig. 1), and a screen at p , I find the index of refraction for the most luminous rays to be higher than that of any of the above kinds of heat, being at least 1.602, corresponding to a diagonal $ab=15.8$ inches, as I have given it above. By two series of results derived from a very small oil flame (without wick), I got 15.87 for the diagonal both times; and from the Locatelli-lamp (which on account of the size of the flame forms a better standard of comparison with the experiments on heat) 15.76; so that I consider 15.8 as a fair representation of the case of light.

76. Yet it is quite certain that the index of refraction of the rock-salt used is really much below 1.60. A single experiment with Dr WOLLASTON'S instru-

ment gave me a result between 1.53 and 1.54. Without dwelling more than necessary on this difference (our great object being gained when we have compared heat and light under similar circumstances), I will mention the two causes which I believe to produce it. (1.) It is undeniable that the transition from total to partial reflection takes place much more gradually than is due to the mere heterogeneity of the rays; (this the experiment with light makes very obvious). The angle of incidence throughout the range of experiment (from $ab = 14.5$ to $ab = 16.5$) *within* the prism varies from $42^{\circ} 22'$ to $36^{\circ} 38'$. The intensity at any point is made up of totally and of partially reflected light. Consequently throughout this range of incidence, the partially reflected light must be more intensely reflected as the incidence is greater, and it is easy to see that the effect of this variation in the intensity of the partially reflected rays will have the effect of shifting all the curves towards the right hand in Plate XII. (2.) We have before remarked, that owing to the dimensions of the source of light or heat, the rays do not form a refracted beam of uniform intensity. The central rays are usually brightest. Now, it may be shewn that in consequence of the varying angle of incidence the central rays travel across the front of the pile, and consequently there would be a maximum effect produced at one point from this cause alone.

77. I believe that the former cause is the only one whose effects are sensible, or at least considerable; and having reason to think that its action is similar upon different kinds of heat, and also of light, we shall probably be very near the truth if we substitute for the indices of refraction above found, others .04 or .05 lower. But relative results are in this case by much the most important.

78. The results which we have obtained apply, it must be recollected, only to the *predominant* kind of heat in any source, and that we have as yet got no information respecting the *composition* of a ray and the amount of dispersion.

79. It is very easy to see that were the mathematical conditions of the experiment (art. 55) fulfilled, we should be led to an exact analysis of heat, more perfect far than we have any prospect of obtaining in the case of light, considering the difficulty of applying the photometer to coloured light. Were the curve in Plate XIII. Fig. 3, solely representative of the progress of reflection due to the heterogeneity of the rays, the increment of intensity between any diagonal E and another C, or D f would denote the proportion of the entire heat incident, which lies between the limits of refrangibility assigned by the diagonal, and found by the table in art. 72. Thus an entire ray would be decomposed into parcels of known proportions, between given intervals of refrangibility. The case is considerably different. Though the points of contrary flexure agree remarkably well, as we have seen, the curves are in some cases much more flattened than in others, where the source of heat is the same; owing probably to the greater parallelism of the rays at one time than at another, depending on the distance of the source of heat from the lens.

80. We can, therefore, in this way form but an imperfect idea of the comparative homogeneity of the different kinds of heat. Such comparisons can only be made advantageously by comparing the results obtained in immediate succession from one and the same source with interposed screens of different qualities, as in the comparison which we instituted between heat direct from Locatelli's lamp, and that transmitted by glass, (art. 63).

81. The facts respecting refrangibility, which may now be considered as ascertained, serve to render our ideas much more precise in several respects. For instance, (1.) the range of *mean* refractive indices for heat is small, all the modifications which we have considered lying within a range of .04, or between 1.51 and 1.55 nearly, which is little more than the commonly assigned dispersion of light, which, for rock-salt, is between the limits 1.54 and 1.57 nearly. This, however, is for *extreme* rays of light, which can hardly be said of heat; the extremes of dispersion are certainly much wider apart. (2.) The *mean* refractive index of *direct* rays from different sources varies surprisingly little. In fact the differences for *direct* rays of heat from the Locatelli-lamp, incandescent platinum, and from a crucible heated to 450°, seem almost insensible, or within the limits of error of experiment. It is to be recollected, however, that this is compatible with the utmost variety in the *composition* of each. (3.) The effect of interposed screens in modifying the transmitted heat is very remarkable. These, so far as I have tried them, invariably *raise* the index of refraction, (alum, glass, opaque glass, and opaque mica for the Locatelli-lamp; glass and opaque mica for incandescent platinum, and clear mica for dark heat). This is the case even with those substances which suppress light altogether, and which therefore cannot be considered to do more than detach the heat of considerable refrangibility from the light which usually accompanies it, not as stopping the most refrangible rays and admitting the passage of those of lower temperature. Probably no substance acts in this way, though some (as black glass and mica, as the experiments of MELLONI indicate) may probably absorb the heat spectrum at *both* extremities. It is probably to this source that we must attribute the very small fraction of heat transmitted by the black glass I used, being only that constituting the rays of the higher degrees of refrangibility, all those of *low* and *mean*, and also of the *highest*, degrees of refrangibility being probably absorbed. (4.) With respect to the homogeneity of different kinds of heat, I have already stated that we can deduce nothing *certain* from the forms of the curves in Plate XII. They confirm, however, a view which I have long entertained, that heat from non-luminous sources is more homogeneous than any other. I argued this partly on the ground stated in art. 40 of this paper, and still more from the uniformity of results which I have in all classes of experiments obtained from dark heat, which often more than made up for the narrower range of the thermal effect, and which shewed that the discrepancies observed in other cases were due not so much to errors of observation, as to unavoidable changes in the

character of the heat, (art. 14). This result is the more probable from the size of the source of heat necessarily used in the crucible experiments (art. 70), which tends to render the passage from partial to total reflection more gradual, and thus to flatten the curve. To the same cause may also probably be attributed the somewhat greater index of mean refraction obtained for heat from this source than that of dark heat of higher temperature.

82. The following method might perhaps be used with success for obtaining more exact data respecting the refrangibility, and especially the *dispersion*, of heat, than that just described pretends to give. It must insure a beam of parallel rays of heat of sufficient intensity and uniform in every part of its section. A small point of heat placed behind a lens (or two or three lenses to diminish aberration) is the most obvious plan. But the intensity would be inadequate. I would, therefore, propose a platinum-wire, heated by one of Mr DANIELL'S constant voltaic batteries, placed behind a refracting semi-cylinder of rock-salt.* The central rays should be alone employed, and the prism for total reflection should be high and narrow as well as the aperture of the pile. It is possible that in this case the transition from partial to total reflection would be so rapid as to make the error arising from the varying intensity of partial reflection (art. 76) inconsiderable. By changing the force of the battery, heat of all temperatures might be employed in succession. The numerical analysis of the heat spectrum would then take place as described in art. 79.

Conclusion.

83. My object in these, as in former researches, has not been to group experiments of mere curiosity indiscriminately selected, but to present a basis for a proper theory of heat. Without some such end in view I should have thought the time and labour spent on these experiments in some degree misapplied. Mere numerical results, though ultimately of the highest consequence to science, should never form the exclusive object of the philosopher. I trust to have shewn that though many of the conclusions in this paper are based upon quantitative results, these have not been the ultimate aim of the inquiry.

84. The mutual bearing of the three sections of this paper, and of all upon what (from analogy to physical optics) we may call *physical thermotics*, is now evident. (1.) In the First Section we have minutely discussed a point apparently perhaps of minor importance, namely, the unequally polarizable nature of the rays of heat. The importance of the doctrine lies in this: that the common theory of undulation recognises no such variation, nor *perhaps* does it exist in the case of light (I know, however, of no decisive experiments on this point), with the excep-

* Such a one I have had executed.

tion of the small effect due to the difference of refrangibility. Now, having proved in the Third Section that this difference of mean refrangibility is from most sources very small, which yet differ widely in their polarizability, we infer that that explanation is probably inadequate, and that we must look for a mechanical theory of heat differing in some particulars from that of light.

85. (2.) This latter conclusion is farther confirmed by the results of the Second Section, in which is deduced, from the singularly accordant results of wholly distinct series of experiments with heat from those distinct sources, that the phenomena of depolarization differ surprisingly, *numerically speaking*, from those of light, whilst in their general character they are entirely similar. The results at which we have arrived oblige us to admit, *either* that the length of a wave of heat is several times greater than that of a wave of light, *or* that the velocities of the ordinary and extraordinary ray in doubly refracting crystals are totally different from those of light; *or else* a combination of these hypotheses. Now, of the two first alternatives we are bound at present, I think, to prefer the latter, since we know nothing of the phenomena of double refraction but from this experiment; whilst the subsequent experiments on the refractive index, would, according to the prevalent theory of dispersion, seem to shew that the mean length of a wave of heat cannot differ very materially from one of light. This amounts to admitting that the doubly refractive energy is more feeble for heat than for light; in other words, that a greater thickness of a crystal is required to produce a given effect. The Second and Third Sections also confirm one another in this respect, that the uniformity of the results of depolarization with heat from different sources, and also of the refrangibility, would both be highly improbable did the length of a wave materially differ in those instances.

86. (3.) Of the results of the Third Section, I have already spoken at sufficient length (art. 81). The mean index of refraction for all kinds of heat tried is *less* than for light;—it ranges within narrow limits;—when the heat from different sources is unmodified by transmission through diathermant bodies, these limits are very narrow indeed;—the measure of dispersion is considerable but unascertained, and opens a fair field for experiment;—dispersion is probably least for sources of low temperature.

87. Such are the chief data for speculation afforded by the experimental results contained in this paper :—too imperfect perhaps in themselves to form the basis of a mechanical theory of heat, yet such I hope as may be considered to be fit contributions towards its construction at a future period.

DEPOLARISATION OF HEAT.

PLATE XI.

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Fig. 1. Argand Lamp

Fig. 2. Incandescent Platinum

Fig. 3. Dark Hot Brass

REFRANGIBILITY OF HEAT.

PLATE XII.

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Fig. 1.

Fig. 2.

Fig. 3.