XXIII.—*On Mirage.* By Professor TAIT. (Plate XXXIII.)

(Read 5th December 1881.)

I was led to the following investigations while seeking an elementary, and at the same time instructive, application of HAMILTON'S *General Method in Optics*.^{*} They were completed in all but a few of their numerical details before I met with the remarkable paper by WOLLASTON,[†] in which the subject of multiple atmospheric images seems first to have been treated by a sound physical method. WOLLASTON'S experiment with a long bar of iron raised to a high temperature suggests undoubtedly the true explanation of at least many of the curious phenomena seen by VINCE,[‡] SCORESBY,[§] and others. But he seems to have thought that sufficient temperature-differences for the natural production of the phenomena could not exist in the atmosphere; and thus the latter part of his paper, in which he tries to explain them by the agency of aqueous vapour, presents a singular contrast to the strength and correctness of the earlier part. A good deal of what follows is implied, if not directly stated, in WOLLASTON'S paper; but I think there is sufficient novelty in what remains to justify my bringing it before the Society.

The subject is one which offers immense facilities for the construction of elegant "Problems," but I have confined myself to the simplest hypotheses which (while enabling me to obtain exact results) promised to throw light upon it:—feeling that anything else would be out of place in endeavouring to explain a class of phenomena which have probably never occurred twice in exactly the same way. I have, however, shown at least the *general* nature of the alterations to which my results would be subject in consequence of modification of the assumptions.

1. Most of the images seen by SCORESBY were inverted, and elevated above the apparent position of the object seen directly, and each series of them (when there were more series than one) can be explained at once by the existence of a horizontal stratum of air in which the rate of diminution of refractive index in ascending is greater than that in the air immediately below. [This is merely the sort of arrangement which, as is perfectly well known, produces the mirage of the desert; but turned upside down.] But the chief phenomenon figured by

^{*} *Trans. R. I. A.*, 1833.[†] *Phil. Trans.*, 1800.[‡] *Phil. Trans.*, 1799.[§] *Greenland*, and *Trans. Roy. Soc. Edin.*, ix. and xi.

VINCE, and also in a few cases by SCORESBY, involves an inverted image with a direct image above it. In some other cases observed by SCORESBY, the direct or the inverted image alone was seen, the object itself being situated far below the horizon. Some excerpts from SCORESBY's figures (which are themselves *composite*) are given in fig. 1. A comparison of these observations with VINCE's diagram of the supposed courses of the rays seemed to me to show that a *single* transition stratum may be capable of giving either a single image, direct or inverted according to circumstances, or an inverted image with a direct image above it. As, in at least the greater number of the observations to which I have referred, both the object and the spectator seem to have been *below* the transition stratum which caused the phenomena, I do not think that WOLLASTON's square bottle with two inter-diffusing liquids presents a fair analogy. For, with that arrangement, the rays enter and emerge from the transition stratum by its ends, and not by its lower side, as, from VINCE's diagram, they would appear to do in nature.

I propose to return to the consideration of this arrangement of WOLLASTON's. But meanwhile I will sketch (1) the mode in which I was led to see that, under proper conditions, a simple *continuous* law of refractive index may lead to the formation of three images, (2) how the consideration of the mode in which these are produced in a medium whose refractive index varies to four-fold or more of the minimum value, led me by *necessary* steps to see how they can be produced in the lower atmosphere whose refractive index can vary, even in extreme cases, by only $\frac{1}{40,000}$ or so.

2. To fix the ideas, we will begin with a particular case, which is a thoroughly illustrative one so far as theory is concerned, and is also interesting as it reproduces, with singular accuracy, the exaggerated diagram by which VINCE endeavoured to explain his observations.

The ordinary characteristic of a maximum or minimum is that it differs from neighbouring values of the function by a quantity depending on the *square* of the increment of the independent variable. Assuming then, without any inquiry as to the other physical circumstances, the existence of a medium whose refractive index is represented by the equation

$$\mu^2 = a^2 + y^2,$$

it is clear that $y=0$ is a plane of minimum refractive index.

HAMILTON's equation for this case is, τ being the characteristic function,

$$\left(\frac{d\tau}{dx}\right)^2 + \left(\frac{d\tau}{dy}\right)^2 = a^2 + y^2 \quad (1)$$

since it is obvious that the path is in a plane perpendicular to $y=0$.

A complete integral is

$$\tau = ax + \int dy \sqrt{a^2 + y^2 - a^2}.$$

Hence the equation of a ray is

$$C = x - a \int \frac{dy}{\sqrt{a^2 + y^2 - a^2}}. \quad (2).$$

[This result might, of course, have been at once obtained from the corpuscular theory. For its principles give

$$\dot{x} = a, \quad \dot{y} = \sqrt{a^2 + y^2 - a^2}.]$$

Equation (2) has two distinct forms according as a is greater or less than a . These are separated by the limiting form when $a = a$, viz :—

$$y = C_1 e^{\frac{x}{a}},$$

a logarithmic curve asymptotic to the axis of x . When a is less than a , the ray *passes through* the plane $y=0$, and we need not consider it further.

We may therefore assume

$$a^2 = a^2 + \eta^2,$$

and it is obvious that y cannot be less than η . With this expression for a , the mere form of the equation (2) shows that the curve has a vertex at the point $y=\eta$, and that it is symmetrical about the ordinate through that point.

We must remark, in passing, that this property of symmetry about an axis, at the extremity of which is a vertex, is common to groups of rays in all media in which the refractive index depends only on the distance from a particular plane :—the groups which possess it being those which either do not reach that plane, or pass through it more than once.

3. Let us now consider only rays which have vertices, and which pass through a particular point $x=0, y=b$. Then if ξ be the x -cöordinate of the vertex, equation (2) becomes

$$\xi = \sqrt{a^2 + \eta^2} \int_{\eta}^b \frac{dy}{\sqrt{y^2 - \eta^2}}. \quad (3).$$

This is the equation of the *Locus of Vertices* of all rays (having vertices) which pass through the point 0, b . We may write it in the form

$$\xi = \sqrt{a^2 + \eta^2} \log \frac{b + \sqrt{b^2 - \eta^2}}{\eta}. \quad (3).$$

To draw the corresponding curve we may construct, for different values of b , the set of curves

$$\xi' = \log\left(\frac{b}{\eta} + \sqrt{\frac{b^2}{\eta^2} - 1}\right) \quad . \quad . \quad . \quad . \quad . \quad (4),$$

or

$$\frac{2b}{\eta} = e^{\xi'} + e^{-\xi'}.$$

The ordinates of these curves are proportional to the reciprocals of those of a common catenary.

Next construct, for the given value of a , the equilateral hyperbola whose equation is

$$\xi'' = \sqrt{a^2 + \eta^2}.$$

Then we have, at once, for any given value of η ,

$$\xi = \xi' \xi''.$$

For the purpose of carrying out this process we have tabulated as below, a few rough numerical values:—and by the help of these the curve (4) has been drawn, along with (3), in three forms; for $b=2a$, $b=4a$, and $b=6a$. See fig. 2. In each case (4) is represented by a dotted curve, (3) by the corresponding full curve.

$\frac{\eta}{b}$	$\log\left(\frac{b}{\eta} + \sqrt{\frac{b^2}{\eta^2} - 1}\right)$	$\sqrt{1 - \frac{\eta^2}{b^2}}$	$\sqrt{\frac{1}{4} + \frac{\eta^2}{b^2}}$	(ratio).
0.0	∞	1.0	0.5	0.5
0.05	3.69	0.99	0.51	0.51
0.1	2.99	0.99	0.51	0.51
0.2	2.29	0.98	0.54	0.55
0.3	1.87	0.95	0.58	0.61
0.4	1.57	0.92	0.64	0.70
0.5	1.32	0.87	0.71	0.82
0.6	1.10	0.8	0.78	0.97
0.7	0.89	0.71	0.86	1.20
0.8	0.69	0.6	0.94	1.56
0.9	0.47	0.44	1.03	2.36
1.0	0.0	0.0	1.12	∞

4. Let us digress to consider what we learn, in any case, from the *form* of the Locus of Vertices.

It is obvious that if, instead of the special law of refractive index assumed in the preceding section, we had written quite generally

$$\mu^2 = f(y),$$

(3) would have become

$$\xi = f(\eta) \int_b^\eta \frac{dy}{\sqrt{f(y) - f(\eta)}} \quad . \quad . \quad . \quad . \quad . \quad (3'),$$

while (2) would have been (for rays passing through the point 0, b),

$$0 = x - a \int_b^y \frac{dy}{\sqrt{f(y) - a^2}} \quad \dots \quad (2').$$

The new *form* of (2') shows that, for a given value of y , x increases with increase of a ; provided no vertex is reached. For the denominator of the differential is less, and the integral is multiplied by a greater factor, than before. Hence two contiguous rays from the same point cannot again intersect till one, at least, has passed its vertex. When the vertex is included within the limits of integration, (2') may by the symmetry of the ray be written

$$0 = x - a \int_b^y \frac{dy}{\sqrt{f(y) - a^2}} - 2a \int_y^\eta \frac{dy}{\sqrt{f(y) - a^2}}, \quad \text{or} \quad 0 = x + a \int_b^y \frac{dy}{\sqrt{f(y) - a^2}} - 2\xi.$$

Now the middle term (as we have seen) is positive, and increases with a , if $y > b$. Hence the second intersection of the rays which have the common point 0, b , is at a point where $y > b$, if and only if, ξ increases as a increases; *i.e.*, if the line, drawn from the vertex of the ray nearer to the minimum plane to that of the other, leans back towards the first common point of the two rays. The converse is easily seen to hold, by taking the second point of intersection as the starting-point and reversing the rays. Hence, if the minimum stratum be horizontal, two neighbouring rays, issuing from a common point below it, and originally directed above the horizon, intersect again *before* they have got back to the level of their former intersection, if their vertices be at a part of the curve of vertices where the tangent leans backwards over the starting-point, and *vice versa*. This proposition is, in fact, obvious from a mere inspection of the diagram fig. 3, in which the dotted curve is that of vertices, the eye being at E.

To apply it to the case of phenomena such as those observed by VINCE and SCORESBY, suppose the strata of equal refractive index to be horizontal. Then two rays slightly inclined to one another, leaving any point in a common vertical plane, will in general intersect one another before they again reach the level of the starting-point, if, and not unless, the vertex of the higher ray be *horizontally* nearer to the starting-point than that of the lower ray; *i.e.*, if the part of the curve of vertices concerned leans *towards* the starting-point. Also, as is well known, when two rays slightly inclined to one another, cross *once* between the eye and the object, the image formed is an inverted one.

5. Hence the following graphical method for finding the number and characters

of the images of an object situated at the level of the eye. Trace the curve of vertices for all rays leaving the eye in the vertical plane containing the object. Draw also a vertical line midway between the eye and the object. The intersections of this line with the curve of vertices are the vertices of all the paths by which the object can be seen, when the eye is in the assigned position. Or, what comes to the same thing, but (unlike the simpler construction) admits of application to an object at *any* level, draw the curve of the vertices as before, and then draw another for an eye placed *at the object*. Their intersections determine the vertices of the rays giving all possible images.

It is easy to see that, at the intersections with the vertical line midway between eye and object, the curve of vertices, if continuous, must alternately lean from, and towards, the eye, *i.e.*, the images seen are alternately erect and inverted; their number depends of course upon the form of the curve of vertices; which, in its turn, depends not only upon the law of refractive index in terms of level, but also upon the position of the eye. [This alternation of images does not necessarily hold when eye and object are at different levels.]

Thus, as has long been known, the vertices of all the coplanar paths in which a projectile, fired with a given velocity, can move, with different elevations of the piece, lie in an ellipse whose major axis (double the minor axis) is horizontal. The lower half of this ellipse leans *from* the gun, the upper half *towards* it, and these correspond to angles of elevation of the piece, respectively less and greater than 45° . In the former case (when the elevation is less than 45°), a slight increase of elevation increases the range on a horizontal plane, so that the new path is wholly above the old one; which, however, would intersect it *under* the horizon. In the latter case a slight increase of elevation shortens the range, so that the two paths must intersect before reaching the ground.

6. Recurring to the imagined medium in which

$$\mu^2 = a^2 + y^2,$$

we see by fig. 3 the paths of the rays by which the three images of AB are seen by an eye placed at E. This figure, as already remarked, is (with the exception of the introduction of the curve of vertices) almost identical with that of VINCE in the *Phil. Trans.* for 1799.

But it is easy to see that, although this shows the possibility of three images in the relative positions observed by VINCE, it is in no way capable of explaining his observation. For the existence of three images, in such a medium, requires (as I have found by an approximate method)* that b be *at*

* When $\frac{d\xi}{d\eta} = 0$, we have $1 + \frac{a^2}{\eta^2} = \sqrt{1 - \frac{\eta^2}{b^2}} \log \left(\frac{b}{\eta} + \sqrt{\frac{b^2}{\eta^2} - 1} \right)$. Plotting the curves whose ordinates (in terms of η) are expressed by these two quantities, we find that they *touch* when $b = 3.68a$.

$least = 3.68a$. Hence the refractive index at the level of the eye ($\sqrt{a^2 + b^2}$) must be at least 3.8 times that in the minimum stratum. And the distance at which an object on the horizon requires to be situated, in order that there may be three images of it, lies within exceedingly narrow limits, unless the refractive index at the level of the eye very greatly exceed this lowest admissible value.

7. The possibility of three images of an object at the level of the eye evidently depends on the existence of three values of y , for the same value of x in the curve of vertices. It is therefore necessary that we should study the question from this point of view.

On thinking of the relative forms of the curves of vertices in fig. 2; the first of which gives only one image, the second and third (in certain cases) three:—I saw that the point of inflexion, on which the triple value of y depends, is due to the gradual diminution of curvature of the ray near the eye (for rays of a given inclination to the vertical) as the eye is placed lower in the medium. Hence any arrangement which lessens the curvature of the lower parts of the rays will increase this effect.

In fact, the portion ABC of the ray OB (fig. 4) is congruent with the ray abc , if only the tangents at A and a be parallel. Hence the point B would be shifted to b if the ray Oa were straight (or at all events, less curved than OA) and the angle at a equal to that at A.

Thus it was at once obvious that the curve of vertices (fig. 5) in the stratum above RS, might be made asymptotic to that line towards the right of the figure (the eye being still at O), if only the stratum below it were of uniform refractive index, or at least of a refractive index diminishing so slowly with increased height that a ray from O could intersect RS at a practically infinite distance. This at once showed me the general nature of one mode of explanation. The curve of vertices QPQ' in the stratum RU will now be asymptotic, towards the right, to both RS and TU, and therefore can be cut in *two* points by a sufficiently distant vertical. These points correspond to VINCE's two upper images, the third and lowest is seen by rays which have not reached the upper stratum, and for which the corresponding branch of the curve of vertices is the horizontal line OM, passing through the eye.

8. To repeat:—the conditions requisite for the production of VINCE's phenomenon, at least in the way conjectured by him, are, a stratum in which the refractive index diminishes upwards to a minimum (or, at all events, nearly to a stationary state); and, below it, a stratum in which the upward diminution is either considerably less or vanishes altogether. The former condition (the

fall to a nearly stationary state) secures the upper erect image, the latter the inverted image. When the former is not present, we have the phenomenon so often observed and figured by SCORESBY. This requires merely a change from a slowly diminishing refractive index to a more quickly diminishing one, and may occur simultaneously in more than one horizontal layer. Turned upside down, this arrangement gives the ordinary mirage of the desert. When this condition is not present, but only the stationary state, we have VINCE's upper erect image without the inverted one. This is figured several times by SCORESBY.

9. If, instead of a plane of minimum, we have a plane of maximum, refractive index, we may assume

$$\mu^2 = a^2 - y^2.$$

An investigation precisely similar to the preceding gives for a ray passing through 0, b the equation

$$x = \sqrt{a^2 - \eta^2} \left(\sin^{-1} \frac{y}{\eta} - \sin^{-1} \frac{b}{\eta} \right).$$

Each ray therefore is a harmonic curve, whose level line is in the maximum stratum, and which passes through that stratum an infinite number of times. The locus of vertices is

$$\xi = \sqrt{a^2 - \eta^2} \left(\cos^{-1} \frac{b}{\eta} + n\pi \right).$$

Here η is to be taken positive when n (any integer) is even, and negative when it is odd.

The following rough table suffices to determine the general form of this curve in the particular case $a = 5b$. It is shown in fig. 6; and it has been foreshortened for convenience of representation.

$\frac{b}{\eta}$	$\frac{2}{\pi} \cos^{-1} \frac{b}{\eta}$	$\frac{\xi}{b}$		
		$n=0$	$n=1$	
1.0	0.0	± 0.0	9.8	9.8
0.95	0.2	± 0.98	8.8	10.76
0.9	0.29	± 1.39	8.35	11.13
0.8	0.41	± 1.98	7.70	11.66
0.7	0.51	± 2.42	7.16	12.0
0.6	0.59	± 2.78	6.64	12.2
0.5	0.67	± 3.05	6.11	12.21
0.4	0.74	± 3.20	5.46	11.86
0.3	0.80	± 2.97	4.47	10.41
0.25	0.84	± 1.9	2.5	6.3
0.2	0.87	0.0	0.0	0.0
0.1	0.93	...	...	...
0.0	1.00	...	...	...

The general problem of determining the images is, in this case, a very complicated, though not difficult, one; but it becomes much simplified if we assume as before the object and eye to be at the same level. It is obvious that a vertical line, midway between the eye and the object, will cut the curve of vertices an infinite number of times, both above and below the maximum stratum. Thus there is in such a case an infinite number of images, which are seen by rays which have crossed the maximum stratum an even number of times, in which zero may be included. These must each have one, or some other *odd* number, of vertices between the eye and the object, and the horizontal distance between two such vertices is

$$\pi \sqrt{a^2 - \eta^2},$$

which is therefore less for that one of two rays which intersects the maximum plane at the greater angle.

In nature, of course, the number of images depending on a law like this must always be finite, because the utmost percentage change of refractive index in the lower atmosphere is very small. But, independent of equilibrium considerations, there is the farther objection that it cannot be reconciled with the appearances seen by VINCE and SCORESBY. For these were, in the main, very similar to one another for all distances of the object beyond certain limits; while with the present assumption, the appearances presented by an object moving to successively greater distances would exhibit a species of *quasi periodic* change which I have nowhere seen described. And, if we keep to probable changes in the refractive index of the atmosphere, this law will give only one image:—not, of course, in the true direction of the object:—but erect, and therefore not properly coming under the designation of “mirage.”

10. After trying a number of assumptions as to the law of refractive index in the transition stratum, I finally chose for detailed examination the following:—

$$\mu^2 = a^2 + e^2 \cos \frac{\pi y}{b}.$$

This seemed to me particularly worthy of investigation, for it must be at least a fair approximation to the state of matters near the common boundary of two inter-diffusing fluids, or of two masses of the same fluid at different temperatures. This follows from the facts that:—it gives a stationary state at $y=0$, with a maximum refractive index; and another at $y=b$, with a minimum index. Near $y = \frac{b}{2}$ there is a stratum of greatest rapidity of change of index.

This hypothesis has also the advantage of leading to equations which can be treated by the ordinary elliptic integrals.

With this law it follows that, if the eye be in the plane $y=0$, the equation of the curve of vertices is

$$\begin{aligned} e\xi &= \sqrt{a^2 + e^2 \cos \frac{\pi\eta}{b}} \int_0^\eta \frac{dy}{\sqrt{\cos \frac{\pi y}{b} - \cos \frac{\pi\eta}{b}}} \\ &= \frac{\sqrt{2} \cdot b}{\pi} \sqrt{a^2 + e^2 \cos \frac{\pi\eta}{b}} F_1 \left(\sin \frac{\pi\eta}{2b} \right) \end{aligned}$$

The equation of the path of a ray is

$$\begin{aligned} ex &= \sqrt{a^2 + e^2 \cos \frac{\pi\eta}{b}} \int_0^y \frac{dy}{\sqrt{\cos \frac{\pi y}{b} - \cos \frac{\pi\eta}{b}}} \\ &= \frac{\sqrt{2} \cdot b}{\pi} \sqrt{a^2 + e^2 \cos \frac{\pi\eta}{b}} F \left(\sin \frac{\pi y}{2b}, \phi \right), \end{aligned}$$

where

$$\sin \frac{\pi y}{2b} = \sin \frac{\pi\eta}{2b} \sin \phi.$$

We have also

$$\frac{dy}{dx} = e \frac{\sqrt{\cos \frac{\pi y}{b} - \cos \frac{\pi\eta}{b}}}{\sqrt{a^2 + e^2 \cos \frac{\pi\eta}{b}}},$$

and, for $y=0$, this takes the value

$$\frac{\sqrt{2} \cdot e \sin \frac{\pi\eta}{2b}}{\sqrt{a^2 + e^2 \cos \frac{\pi\eta}{b}}}.$$

For the application of these formulæ the following little table has been prepared :—

$\frac{\eta}{b}$	$\frac{1}{k} = \operatorname{cosec} \frac{\pi\eta}{2b}$	$F_1(k)$	$\frac{E_1(k)}{1-k^2}$
0.0	∞	$\frac{\pi}{2}$	$\frac{\pi}{2}$
0.1	6.39	1.58	1.60
0.2	3.24	1.61	1.69
0.3	2.20	1.66	1.87
0.4	1.70	1.74	2.18
0.5	1.41	1.85	2.70
0.6	1.24	2.01	3.67
0.7	1.12	2.24	5.74
0.8	1.05	2.60	11.53
0.9	1.012	3.26	42.24
0.95	1.003	3.94	164.17
0.975	1.00077	4.62	650.85
1.0	1.000	∞	∞

The headings explain themselves. The last column is required, as will soon be seen, for the determination of the *magnitudes* of the images, as compared with that of the object when seen (at its true distance) through uniform air.

11. Let us now extend the formulæ of § 4 to the case of a stratum of depth c , in which the refractive index is constant ($=\sqrt{f(c)}$); surmounted by another of thickness b , in which the index is $\sqrt{f(y)}$.

The equation of a ray, passing from the origin, which we now take in the lower surface of the inferior stratum, is

$$x = a \int_0^y \frac{dy}{\sqrt{f(y)} - a^2}.$$

While y is not greater than c , this is the straight line

$$x = \frac{ay}{\sqrt{f(c)} - a^2}.$$

But when y is greater than c , we have

$$x = \frac{ac}{\sqrt{f(c)} - a^2} + a \int_c^y \frac{dy}{\sqrt{f(y)} - a^2}. \quad (2'')$$

Also, for the branch of the curve of vertices which is in the upper stratum (the other branch being, of course, the axis of x),

$$\xi = \frac{c \sqrt{f(\eta)}}{\sqrt{f(c)} - \sqrt{f(\eta)}} + \sqrt{f(\eta)} \int_c^\eta \frac{dy}{\sqrt{f(y)} - \sqrt{f(\eta)}}. \quad (3'')$$

Fig. 5 has been roughly traced from this formula and the curve of fig. 2.

12. In the next following equations, recurring to the form

$$\mu^2 = a^2 + e^2 \cos \frac{\pi y}{b},$$

we will simplify matters by making $a=1$, and altogether neglecting the terms in e^2 when they are added to others not containing e . This will be fully justified, so far as air is concerned, in a subsequent section.

By § 11 the equation of the curve of vertices is

$$e\xi = \frac{c}{\sqrt{2}} \operatorname{cosec} \frac{\pi\eta}{2b} + \frac{b\sqrt{2}}{\pi} \int_0^{\frac{\pi}{2}} \frac{d\phi}{\sqrt{1 - \sin^2 \frac{\pi\eta}{2b} \sin^2 \phi}},$$

If we write

$$k = \sin \frac{\pi\eta}{2b} = \frac{\tan \theta}{\sqrt{2} \cdot e},$$

where θ is the inclination of the straight part of the ray, this becomes

$$e\xi = \frac{c}{\sqrt{2} \cdot \pi} \left\{ \frac{\pi}{k} + \frac{2b}{c} F_1(k) \right\} = \frac{c}{\sqrt{2} \cdot \pi} \mathfrak{F}.$$

Differentiating, and eliminating δk , remembering that θ is always very small (so that $\sec \theta = 1$ practically), we have

$$\begin{aligned} \frac{\delta \xi}{\delta \theta} &= -\frac{c}{2\pi e k} \left\{ \frac{\pi}{k} + \frac{2b}{c} F_1(k) - \frac{2b}{c} \frac{E_1(k)}{1-k^2} \right\} \\ &= -\frac{c}{2\pi e k} \left\{ \mathfrak{F} - \frac{2b}{c} \mathfrak{E} \right\}. \end{aligned}$$

By means of these expressions we can easily calculate the relative apparent vertical heights of the various images.

For, if there be a small object, of height h , at a distance 2ξ in a horizontal direction, it will be seen *direct* (through the stratum of uniform density) under the angle

$$\frac{h}{2\xi}.$$

But the image is obviously seen under the angle $\delta\theta$, corresponding to rays which, leaving the eye, pass through its upper and lower points. If $2(\xi + \delta\xi)$ be the range of the ray through the upper point of the object, we obviously have (to a sufficient approximation),

$$h = 2\delta\xi \tan \theta = 2\sqrt{2} \cdot ek \delta\xi;$$

and thus we find

$$\delta\theta = \frac{h}{2\sqrt{2} \cdot ek \frac{d\xi}{d\theta}}.$$

Hence the ratio, of the apparent altitude of the image to that of the object as seen directly, is

$$\frac{\xi}{\sqrt{2} \cdot ek \frac{d\xi}{d\theta}} = -\frac{\mathfrak{F}}{\mathfrak{F} - \frac{2b}{c} \mathfrak{E}} = \mathfrak{G}.$$

In passing, we observe that, as it is easy to see, the multiplier of h in the above expression for $\delta\theta$ represents the divergence or convergence of the pencil which reaches the eye from a point of the image. It expresses convergence when its value is positive.

13. Let the value of ξ be given, a suppose; then we have, to determine k , the equation

$$\sqrt{2} \cdot \pi e \frac{a}{c} = \mathfrak{F}.$$

The short table (§ 15) which follows shows that this equation gives two real values of k (say k_1 and k_2) for all values of a exceeding a certain limit. For the

quantity $\mathfrak{F}$ is infinite alike for $\eta=0$ and for $\eta=b$, and has one minimum only. The angular separation of the two corresponding images of a point on the horizon is

$$\sqrt{2.e(k_1-k_2)}.$$

This quantity must obviously become less as the object approaches the spectator, for the values of k become more nearly equal. Its utmost value is $\sqrt{2.e}$.

14. If the object be now raised above the horizon, let its co-ordinates be $2a, h$; then for either of the rays which pass through an image

$$2a=2\xi-h \cot \theta,$$

or

$$\begin{aligned} 2a &= 2\xi - \frac{h}{\sqrt{2.ek}}, \\ &= \frac{\sqrt{2.c}}{\pi e} \mathfrak{F} - \frac{h}{\sqrt{2.ek}}. \end{aligned}$$

By the process of § 13, above, we find that the ratio of the apparent heights of the image and object is now

$$= \frac{\mathfrak{F} - \frac{\pi h}{2kc}}{\mathfrak{F} - \frac{\pi h}{2kc} - \frac{2b}{c} \mathfrak{E}},$$

which agrees with the former result when $h=0$.

By a well-known optical theorem, the result would have been the same if the object had been left where it was, and the eye had been elevated through a height h .

It is well to observe here that, as the eye and object are not now on the same horizontal line, we can no longer conclude without special investigation that only three images will be produced. But this opens up a new question, somewhat more complex than those with which we are engaged. I may recur to it on some other occasion. For the present I confine myself to repeating the remark, that if we draw the curve of vertices as before, and in addition draw that corresponding to an eye placed *at the object*, the intersections of these two curves give all the possible vertices. This is the obvious modification which the process requires, when the eye and object are not at the same level. In the present case, we see at once by this process that no new images come in.

15. The following numerical values have been calculated for the purpose of illustrating these formulæ. The symbols employed are the same as in the analysis above:—

	$b=10c$		$b=c$		$b=\frac{c}{10}$		$b=\frac{c}{100}$	
$\frac{\eta}{b}$	$\mathcal{F}$	$\mathcal{G}$	$\mathcal{F}$	$\mathcal{G}$	$\mathcal{F}$	$\mathcal{G}$	$\mathcal{F}$	$\mathcal{G}$
0.0	∞	- 1	∞	- 1.0	∞	-1.0	∞	- 1.0
0.1	51.7	- 2.63	23.24	- 1.16	20.40	-1.02	20.11	- 1.0
0.2	42.4	- 4.99	13.39	- 1.34	10.49	-1.03	10.20	- 1.0
0.3	40.2	-14.73	10.25	- 1.58	7.25	-1.05	6.95	- 1.01
0.4	40.2	+11.95	8.83	- 1.97	5.69	-1.08	5.38	- 1.01
0.5	41.5	+ 3.32	8.15	- 2.97	4.81	-1.13	4.48	- 1.01
0.6	44.1	+ 1.51	7.91	-13.91	4.29	-1.21	3.92	- 1.02
0.7	48.4	+ 0.73	8.01	+ 2.31	3.97	-1.41	3.57	- 1.03
0.8	55.3	+ 0.32	8.50	+ 0.58	3.82	-2.52	3.36	- 1.07
0.9	68.3	+ 0.09	9.69	+ 0.13	3.83	+0.83	3.25	- 1.35
0.95	81.9	+ 0.026	11.02	+ 0.03	3.94	+0.14	3.23	+60.59
0.975	95.6	+ 0.007	12.39	+ 0.01	4.07	+0.03	3.24	+ 0.33
1.0	∞	0.0	∞	+ 0.0	∞	+0.0	∞	+ 0.0

16. We must now consider, so far as is necessary, the physical properties of air :—and observations which have been made as to actual changes of temperature at different elevations above the earth's surface. There is no necessity for dealing with very exact physical data, because we must make assumptions as to distribution of temperature which cannot, at the best, be more than rough approximations. All that we can attempt to show is, that the observed phenomena are of a character and on a scale compatible with the known properties of air, with observed changes of temperature in the atmosphere, and with the arrangement we have suggested for the production of these phenomena.

Thus, although aqueous vapour diminishes the refractive index of air, the practical effect is so minute at its utmost that we neglect it :—a very slight change in our assumption as to temperature would be sufficient to make up for it.

Assume, then, for air at 0°C. and 760 mm.,

$$\mu_o = 1.000294 = 1 + \frac{1}{3400}.$$

Assume farther, what is only approximately true, that the refractive power depends on the density alone, and is proportional to it :—*i.e.*,

$$\mu = 1 + \frac{1}{3400} \frac{\rho}{\rho_o}.$$

The next assumption :—that the air is practically in hydrostatic equilibrium, when such phenomena are observed :—is probably not far from the truth, except in the case of the mirage of the desert. It gives

$$\frac{dp}{dy} = -g\rho;$$

or, with the laws of BOYLE and CHARLES,

$$R \frac{d(\rho t)}{dy} = -g\rho.$$

Now if $H = 26,000$ feet, be the "height of the homogeneous atmosphere," we have

$$p_o = R\rho_o t_o = g\rho_o H,$$

so that the hydrostatic equation becomes

$$\frac{1}{t_o} \frac{d(\rho t)}{dy} = -\frac{\rho}{H};$$

or, to a sufficient approximation,

$$\frac{1}{t} \frac{dt}{dy} = -\frac{1}{H} - \frac{1}{\rho_o} \frac{d\rho}{dy}.$$

Instability occurs when $\frac{d\rho}{dy}$ is positive. Hence the greatest rate of fall of temperature, per foot of ascent, which is consistent with stability is

$$\frac{dt}{dy} = -\frac{t_o}{H} = -\frac{274^\circ}{26,000} \text{C.},$$

or $-1^\circ.05$ C. per hundred feet.

GLAISHER,* in a captive balloon, on two occasions out of twenty-seven, observed the fall of temperature in the first hundred feet to be $1^\circ.8$ F. and $1^\circ.9$ F. respectively. On other three of these occasions it was $1^\circ.7$ F., $1^\circ.5$ F. and $1^\circ.3$ F. respectively. The first two correspond almost exactly to the $1^\circ.05$ C. above computed for a stratum of uniform refractive index. The temperature near the earth's surface was on these occasions $73^\circ.6$ F. and $76^\circ.2$ F.; or, roughly 24° C. The greatest *rise* of temperature per 100 feet of ascent, which he observed on any of these twenty-seven occasions was $0^\circ.3$ F. only. It seems from what follows, therefore, that on none of these occasions would VINCE's phenomena have been possible.

17. To fix the ideas, let us now assume that the first 50 feet of air is of uniform density, and that next there is a stratum of 50 feet thick in which the refractive index is given by

$$\mu^2 = a^2 + e^2 \cos \frac{\pi(y-50)}{50},$$

* B. A. Report, 1869, p. 37.

y being measured from the surface of the earth. Since we may look on μ as practically unity, we have by the formulæ above

$$\frac{1}{\mu} \frac{d\mu}{dy} = \frac{1}{3400} \frac{1}{\rho_0} \frac{d\rho}{dy} = -\frac{1}{3400} \left(\frac{1}{H} + \frac{1}{t_0} \frac{dt}{dy} \right).$$

Hence, by our assumed law of refractive index,

$$\frac{1}{H} + \frac{1}{t_0} \frac{dt}{dy} = \frac{3400}{2} \frac{\pi e^2}{50} \sin \frac{\pi(y-50)}{50}.$$

Hence the greatest rate of change of temperature per foot of ascent (at $y=75$ feet) is

$$274 \times 34\pi e^2 - 0.0105.$$

The whole change of temperature, from the bottom to the top of the stratum, is

$$274 \times 3400e^2 - 0.53.$$

Both of these quantities are in degrees centigrade.

18. To get an idea of the magnitude of e^2 , we note that, by SCORESBY'S observations, the elevation of the images above the horizon is usually about 10 or 15 minutes of arc at the utmost. Hence, by the value of $\frac{dy}{dx}$ in § 10, we may assume as an upper limit,

$$\sqrt{2} \cdot e = \frac{1}{250},$$

or

$$e^2 = 0.000008.$$

With this, the greatest rate of rise of temperature in the assumed stratum is 0.22°C . per foot of ascent, and the whole rise is about 6.9°C . These quantities, moderate as they are, would be greatly diminished by our relinquishing the assumption that the density in the lower stratum is constant.

But even this indicated rise of temperature with elevation has been actually observed. Thus GLAISHER* gives, for July 17th, 1862,

Time.	Altitude.	Temperature.	By Gridiron Thermometer.
10.30 A.M.	19,415 feet.	38.1°F .	38.1°F .
10.35 A.M.	19,435 feet.	43.0°F .	42.2°F .
10.39 A.M.	19,380 feet.	37.0°F .	36.5°F .

The greatest difference here observed is as much as 5°F . in 20 feet; *i.e.*, at the rate of 12.5°F . or 7°C . per 50 feet, precisely what is required above.

* *B. A. Report*, 1862.

19. We have another and independent mode of testing whether this value of e accords with observation. For SCORESBY tells us that, only on rare occasions and then only slightly, were objects at four miles' distance affected. The usual distance was 10 to 15 miles. Now, by the table in § 15 we see that the nearest object of which an image can be formed is distant

$$\frac{50\sqrt{2}}{\pi e} 7.9 \text{ feet ;}$$

or, with the above value of e , about 12 miles.

There is thus a fair agreement, so far at least as these tests can tell us, between the results of our hypothesis and observation.

The table in § 15 shows that, with the same value of e , and the same thickness of the lower stratum, as before, but with the assumption of a transition stratum of a thickness of five feet only; the distance of the nearest object of which an image could be formed would be about six miles only. A still farther reduction of the thickness of the transition stratum reduces this least distance still farther; but it is clear from the table that there is a limit somewhere about five miles. This would be still farther reduced if we supposed the lower uniform stratum to have a depth of less than 50 feet. On the other hand, we see that an increase of thickness of the transition stratum introduces distances greater than are consistent with observation; unless indeed, the thickness of the lower stratum be at the same time reduced. In the table $\mathfrak{F}$ and $\mathfrak{G}$ depend upon the ratio of b to c ; ξ is proportional to c .

20. The columns headed $\mathfrak{G}$ in the table of § 15 give, as shown in § 12, the magnitudes of the images relative to that of the object seen directly. They show that the inverted image is always taller than the object. This is consistent with SCORESBY's observations. When the object is not near the critical distance, however, this magnification is not considerable:—even if we assume a 50-foot transition stratum. On the other hand, the erect image, except when the object is not far beyond the critical distance, is much smaller than the object. Moreover, as is obvious from §§ 12, 15, this image is seen by *converging* rays. No doubt they are so nearly parallel as to be capable of producing distinct vision in a normal eye; but the remark is necessary as showing how different, in some respects, is the phenomenon from one of WOLLASTON's imitations of it. Both images become infinite:—*i.e.*, there is simply “looming”:—when the object is situated at the critical distance. And, as the tables show from the result of § 13, the ratio of the distance between the images to the apparent size of the object seen directly, increases as the object recedes beyond the critical distance. All this seems to accord completely with VINCE's and SCORESBY's observations. The only additional remark I need make is that

possibly SCORESBY, from insufficient telescopic power, failed to see (or at least to recognise as part of the phenomenon) the upper erect image, when the object was much beyond the critical distance. The table shows the great rapidity with which its height diminishes as the object recedes. The disparity between the images depends of course upon the fact that we have assumed a law which places the plane of most rapid change in the middle of the stratum. This may often not be the case in nature. It might be useful to work out the whole again, assuming a law (for the transition stratum) which would place the plane of most rapid change considerably out of the middle of the stratum. But I cannot attempt this at present. The results of § 14 seem also to be in complete accord with SCORESBY's observations at Bridlington Quay, which are the only detailed ones I have met with in which the point of view was shifted to or from the transition stratum.

21. For an approximate estimate of the effect of the earth's curvature on these phenomena, let us suppose the same law of density as before; but let the strata be now *level*, *i.e.*, spheres concentric with the earth. The path of a ray in the lower stratum will still be straight, but the angle at which it meets the transition stratum ($\theta + \psi$, suppose) will now be necessarily greater than its original inclination (θ) to the horizon. See fig. 7.

If R be the radius of the earth, we find to a sufficient approximation,

$$(R+c)\cos\psi - R = R\psi\theta,$$

or

$$\theta = \frac{c}{R\psi} - \frac{\psi}{2}.$$

As θ cannot be negative, the greatest value of ψ is

$$\sqrt{\frac{2c}{R}} = \frac{1}{460}$$

nearly; c being 50 feet, as before. If we write $\frac{1}{p}$ for this quantity, we have

$$2p\theta = \frac{1}{p\psi} - p\psi,$$

whence, by giving $p\psi$ the values 1, 0·9, 0·8, &c., we easily obtain the following table:—

θ	$\theta + \psi$
0·0000	0·0022
0·0002	0·0022
0·0005	0·0022
0·0008	0·0023
0·0012	0·0025
0·0016	0·0027
0·0023	0·0032
0·0033	0·0040
0·0053	0·0057
0·0110	0·0112

Now if we take the value of e as in § 18, we have 0.004 for the greatest value of $\theta + \psi$, which is consistent with the rays not passing *through* the transition stratum. This corresponds to

$$\theta = 0.0033 = \frac{1}{300} = 12' \text{ nearly.}$$

Hence, with this value of e , other assumptions remaining the same, even the upper erect image could not (on account of the earth's curvature) be elevated more than about 12' above the horizon, and the nearest object of which multiple images could be formed would be at a distance of about 13 miles. Greater values of e might remove this difficulty, but they would introduce greater changes of temperature. This shows, therefore, that the assumption of a lower stratum of uniform density is untenable. If there is to be a *simple* arrangement in that stratum, it must therefore be such that the refractive index diminishes with elevation, but, of course, less rapidly than in the lower half of the transition stratum. The effect of this would be to slightly raise the images, and to reduce the critical distance.

Instead of the upper image, consider the lower one. This would be, at its *farthest*, within the distance of the visible horizon as seen from an elevation of 50 feet. Hence no inverted image of the hull of a vessel could be seen if it were more than 18 miles' distant; and even then it would be seen horizontally. The only ways of reconciling this with SCORESBY'S observations are (1) to assume that the lower uniform stratum is much more than 50 feet thick; (2) to assume that it is not uniform, but gives rays a concavity downwards. The former alternative is inadmissible on several of the grounds already mentioned; so we are again forced to assume the latter, which certainly holds if the temperature throughout the lower stratum be constant.

22. In order that the above calculations may be applicable to the phenomena shown by inter-diffusing solutions, it is necessary that the length of the vessel in which the solutions are contained be great enough to allow all rays (by which the images are seen) to enter and escape from the transition-stratum by one of its horizontal surfaces, and not by its ends. By using a vessel nearly 4 feet long, containing a layer of weak brine diffusing into pure water above, I have verified the general accuracy of the results just given. For those rays which enter or escape by an end, the calculation is by no means so simple, and trial shows that the law determining the relative magnitudes of the images is considerably modified. On the other hand, when the vessel is so short and the rays so nearly horizontal, that each ray, while passing through the vessel, may be supposed practically to move in a stratum of uniform rate of change of refractive index, a very simple calculation suffices to give the general nature of the phenomena produced. For the curvature of a ray, in the vessel, may now

be regarded as constant throughout. Here J. THOMSON'S formula * is immediately and usefully applicable. For, if $\theta_1, -\theta_2$, be the angles the ray makes with the horizon just after entering and just before escaping, we have

$$\theta_1 + \theta_2 = -\frac{t}{\mu} \frac{d\mu}{dy},$$

where t is the length of the vessel. But, if $\theta'_1, -\theta'_2$, be its directions before entering and after escaping, we have approximately,

$$\theta'_1 = \mu\theta_1, \quad \theta'_2 = \mu\theta_2.$$

Thus the whole change of direction is

$$\theta'_1 + \theta'_2 = -t \frac{d\mu}{dy},$$

depending only on the rate of change, not on the value, of the refractive index. Parallel rays, passing nearly horizontally through such a vessel, will all be bent in the direction in which the refractive index increases :—but that which passes through the stratum of most rapid change of index will be the most bent, so that the illuminated portion of a sufficiently distant screen on which the rays fall will be terminated by a spectral band of which the violet is outermost. Measurements of the position of this band, from day to day, from hour to hour, or even (in some cases) from minute to minute, will give an extremely accurate mode of measuring the rate of diffusion. To interpret their indications, however, a determination must be made of the law which connects the refractive index of a mixture of the two fluids with the relative proportions in which they are mixed. And it may not always, or even usually, be the case that the stratum of greatest rapidity of change of refractive index is necessarily coincident with that of most rapid diffusion. From the former, however, the latter can always be found; and, so long as the original layers of the fluids remain in part unaltered by the diffusion, the knowledge of the plane and rate of greatest diffusion is sufficient for the complete determination of the other circumstances. I believe that many important questions connected with diffusion may be speedily and accurately investigated by this very simple method. I propose to give a detailed account of it, with experimental results, to the Society on a future occasion.

* *B. A. Report*, 1870. THOMSON finds by a simple process, for the curvature of a ray in a non-homogeneous medium, the expression

$$\frac{1}{\rho} = \frac{1}{\mu} \frac{d\mu}{dn},$$

where n is measured towards the centre of curvature. The result is seen to follow immediately from the corpuscular theory (in which $\mu = v$) by multiplying both sides by μ^2 , for it is thus found to be merely the equation of acceleration of a corpuscle in the direction perpendicular to its path. It is really involved in Prop. I. of WOLLASTON'S paper (*Phil. Trans.*, 1800).

23. In order to calculate roughly the number, position, and dimensions of the images visible to an eye looking through the media nearly horizontally at a distant object, all that is necessary is to draw the caustic, as in fig. 8. It consists, so far as the transition stratum is concerned, of the two (practically) equal and similar curves AB , $A'B'$; which touch the stratum above and below, and have as common asymptote the path of the most deflected ray. So long as the eye is not within the region BAC , only one image is seen. But from any point within this region two tangents can be drawn to the caustic, and a line can be drawn to the object so as to pass altogether below the stratum. Thus there are three images. In order that the middle one may be distinctly visible, the eye must be 10 inches or so beyond the point of contact of the corresponding ray with the lower caustic. Then the image is an inverted one. The others are always direct. [It may be remarked, in passing, that the intersection of the ray AC with the screen is always definite and measurable.]

Here the upper image is always seen by diverging rays, the middle one by diverging or converging rays according to the position of the eye. Contrast this with the results given in § 20. This middle image changes its direction far more rapidly than the others when the eye is moved vertically. It coincides with the upper image when the eye, gradually moved downwards, reaches the line DB . When they meet, both become blue and then disappear by moving the eye farther down. On moving the eye upwards, the middle image approaches the lower one, and they unite and disappear when the eye reaches the line DC . These results are easily verified by trial, and I have mentioned them only with the view of bearing out my statement, that this form of experiment, unless the tank be long enough, does not give results the same as those of Mirage.

(Read 19th June 1882.)

A few days ago, while finally preparing the above pages for press, I had occasion once more to consult WOLLASTON's paper, and inadvertently took down the wrong volume of the *Phil. Trans.* In it (the vol. for 1803) I found another paper on Mirage by WOLLASTON, in which he speaks of certain articles by WOLTMANN and GRUBER, and regrets his inability to read German. This led me to consult the *Register-band* of *Gilbert's Annalen*; and I thus learned the existence of a very elaborate memoir by BIOT* which I had never seen referred to, and in which the subject of mirage is exhaustively treated both by calcula-

* *Mém. de l'Institut*, 1809; *Récherches sur les Réfractions extraordinaires qui ont lieu près de l'horizon*. I presume that my having been altogether ignorant of the existence of this memoir is connected with the fact that it is unintelligible without the plates, and that these were not issued along with it. For in each of the three first libraries which I consulted, that of the Society being one, this volume of the *Mém. de l'Institut* is devoid of plates. Biot's memoir, however, was issued also as a separate volume, and a copy of this, containing the plates, I procured at last from the Cambridge University Library.

tion and by long series of exact measurements of the phenomena as seen by MATHIEU and BIOT at Dunkirk, and by ARAGO and BIOT at Majorca. The previous work of GRUBER, WOLTMANN, BÜSCH, and others, is carefully summarised by GILBERT in vol. xi. of his *Annalen* (1802) in notes to his translation of WOLLASTON'S great paper of 1800. A good deal of BIOT'S work is thus seen to have been anticipated. It may be well to quote here GILBERT'S remark as to the priority of explanation of some of these phenomena—think of it now as we may :—

“ In der That ist WOLLASTON der Erste und Einzige, der die *Spiegelung aufwärts* mit Glück zu erklären unternommen hat, ob er gleich auch hierin noch sehr viel zu thun übrig lässt.”

BIOT, on the other hand, gives WOLLASTON credit only for the physical, as distinguished from the mathematical, parts of his paper. He says :—

“ Sous le rapport de la physique, son travail ne laisse rien à désirer.”

BIOT has considered the subject from a point of view somewhat similar to that which I had adopted, and anticipated of course the great majority of the more general results at which I had arrived. I was occasionally almost startled as I looked through his memoir, to find how closely (even in mode of stating them) I had reproduced some of his main ideas. His whole treatment, for instance, of the ordinary mirage of the desert:—on the assumption that the square of the velocity of a luminous corpuscle is proportional to the height above the ground, but only through a limited stratum, together with the important effects of limitation of the stratum:—is almost the same as mine, except that he (inconveniently I think) uses the caustics in preference to the curve of vertices, though he also notices the latter as the *courbe des minima*. In consequence, I had all but made up my mind to withdraw my paper, before I had looked more than half-way through BIOT'S long memoir; for, though I found here and there statements which I think inaccurate, these are of very small consequence compared with the whole. But it was otherwise when I read farther, where BIOT gives his tentative explanation of VINCE'S observation. There I found our assumptions to be so entirely different in character that, being fairly satisfied with my own, I thought I might still reasonably produce them with their results. My paper, therefore, appears as it was presented to the Society, except in so far as (*a*) a part of the introduction, (*b*) the detailed examination of the ordinary mirage of the desert, (*c*) a discussion of the singular outline sometimes presented by the setting sun, and (*d*) a few minor remarks, are concerned. These parts have been simply struck out, the first as historically imperfect, the others as practically a mere reproduction of what had already been satisfactorily done by BIOT, who had many opportunities of observing and measuring the phenomena. As to the ordinary mirage, however, there can be no doubt that the discovery of the existence of *four* images, when

the eye and object are both above the hot stratum, is far more easy by means of the curve of vertices than by the caustics employed by BIOT.

I transcribe some of the more important parts of BIOT's remarks on VINCE's phenomenon, premising that it was of course impossible for him to have been acquainted with SCORESBY's observations, at least at the time when his memoir was written. I fancy that, if he had seen these, he might have felt some doubts as to the accuracy of his inference that the rays, in their course to VINCE's eye, were probably at first *concave upwards*; and this to such an extent as to make a vessel, which was situated close to the ordinary horizon, show only its top-masts above the apparent horizon. He does not advert to the *certainty* that, had this law held over the nearer parts of the sea, VINCE would have seen inverted images *under* ships within the visible horizon. None such are described. After quoting the passages in question, I shall add a few comments on them. To make them as intelligible as possible, I have reproduced BIOT's hypothetical figure; it is numbered as fig. 9 in the plate. In many respects the following passages are obscure, but to clear them up (if it can be done at all) would require a thoroughly careful perusal of the whole minute details of BIOT's volume, and for this I have not been able to find leisure.

Je crois pouvoir expliquer par la même théorie les phénomènes des triples images observés par M. VINCE et dont j'ai déjà parlé plus haut. Quand je dis expliquer, j'entends ramener ces phénomènes à une même cause, à une même forme de caustique, telle que la disposition des images, et leur marche relative quand elles s'abaissent ou qu'elles s'élèvent, soient des conséquences nécessaires de la forme supposée. Car admettre, comme l'a fait M. VINCE, autant de lois différentes de densité qu'il y a d'images visibles, ne me paroît point une explication satisfaisante, puisque les mouvements respectifs des images restent arbitraires; tandis que, d'après la description qu'il en donne, ces mouvements avoient entre eux des rapports déterminés.

Malheureusement M. VINCE n'a pas observé l'élément le plus nécessaire pour l'explication de ces phénomènes, je veux dire la dépression apparente de l'horizon de la mer. De sorte que l'on ne peut pas affirmer *a priori*, si les trajectoires, dans leur partie inférieure étoient concaves ou convexes vers la surface des eaux. Cependant je crois pouvoir conclure qu'elles étoient convexes d'après plusieurs raisons que je vais développer.

Ainsi, pendant l'observation du phénomène, qui se fit depuis 4 heures $\frac{1}{2}$ du soir jusqu'à 8 heures, la température de l'air devoit avoir considérablement diminué, surtout dans les couches supérieures, par l'effet de l'abaissement du soleil. Mais la surface de la mer n'avoit pas dû se refroidir aussi vite. Elle pouvoit donc alors et devoit probablement se trouver plus chaude que l'air, ce qui donne des trajectoires convexes dans leur partie inférieure, et une densité croissante du bas en haut, jusqu'à une petite hauteur; après quoi l'influence de la mer devenant moins sensible, la densité devoit aller de nouveau en diminuant comme à l'ordinaire, et probablement suivant une loi beaucoup plus rapide, tant à cause de l'abaissement subit de la température, qu'à cause de la chute des vapeurs aqueuses qui devoit en résulter et qui par leur accumulation et par le froid qu'elles produisoient en se précipitant pouvoient

contribuer à augmenter la réfraction dans les couches qu'elles traversoient. Ces conjectures sont confirmées par plusieurs remarques de M. VINCE lui-même.

Je tire encore des observations mêmes une autre preuve que les trajectoires n'étoient pas convexes dans toute l'étendue de leur cours, comme cela auroit eu lieu s'il n'y avoit eu dans l'air qu'un seul état de densité décroissante de haut en bas. Cette preuve consiste en ce que les deux images supérieures dont la plus haute étoit directe et l'autre renversée, ont été plusieurs fois complètes, c'est-à-dire que la vaisseau y étoit représenté tout entier depuis le sommet des mâts jusqu'au corps même du bâtiment. Or, d'après les expériences que nous avons faites sur le sable à Dunkerque, si ces deux images eussent été données par des trajectoires entièrement convexes vers la mer, ces trajectoires eussent nécessairement formé une caustique qui se seroit élevée au-dessus de la surface de la mer à mesure qu'elle s'éloignoit de l'observateur. Cette caustique auroit donc caché de plus en plus les parties inférieures du vaisseau à mesure qu'il s'éloignoit, et par conséquent les deux images de ce vaisseau n'auroient pas été complètes . . . . . On peut encore prouver par les observations de M. VINCE que la caustique n'étoit pas formée d'une branche unique, mais de deux branches distinctes réunies par un point de rebroussement et dont la plus basse alloit continuellement en s'approchant de la surface de la mer à mesure qu'elle s'éloignoit de l'observateur. Car puisque M. VINCE a vu des images complètes de vaisseaux qui se touchoient par le corps même du bâtiment, il falloit bien qu'alors le vaisseau reposât sur la caustique; et comme il en a vu aussi d'autres qui se touchoient par le sommet des mâts, il falloit bien qu'alors le vaisseau se trouvât sous la caustique et la touchât par le sommet de ses mâts. Enfin, puisque les images d'un même vaisseau données par ces deux branches s'écartoient continuellement l'une de l'autre, à mesure que le vaisseau s'éloignoit, les deux branches de la caustique s'éloignoient donc aussi l'une de l'autre; ce qui indique une forme . . . . . qui seroit donnée par la combinaison de deux décroissemens de densité contraires.

Cette conséquence déduite immédiatement des observations s'accordant avec l'état décroissant de la température, et avec toutes les apparences que nous avons discutées, je crois pouvoir admettre comme une chose très-probable que, par l'excès de chaleur de la mer, à l'époque où a observé M. VINCE, les couches inférieures de l'air se trouvoient dans un état de densité croissante de bas en haut, jusqu'à une petite hauteur, au-dessus de laquelle les densités alloient de nouveau en décroissant par suite de l'abaissement de la température, avec assez de rapidité pour donner des images par en haut. D'après les élévations données par M. VINCE, nous devons placer l'observateur dans ces couches supérieures, car il dit avoir observé le phénomène à 25 et à 80 pieds de hauteur. Nous avons déjà examiné précédemment les combinaisons de ces deux états contraires, et l'on a vu qu'elle explique très-aisément les images multiples observées au Desierto de las Palmas et à Cullera, phénomènes qui paroissent avoir le plus grand rapport avec ceux que M. VINCE a décrits. Nous supposons donc conformément à l'endroit cité, que la caustique avoit une forme VRV (fig. 9). . . . . Soit AMH la circonférence de la terre, O l'observateur, OMV la trajectoire limite tangente à la surface de la mer. Il s'agit d'examiner les phénomènes résultans de cette loi.

La supposition que nous venons de faire sur la non-sphéricité des couches n'est point gratuite, car M. VINCE remarque que des vaisseaux également élevés au-dessus de l'horizon apparent présentent des apparences très-diverses, souvent plusieurs images comme nous

venons de le dire, quelquefois deux seulement, l'inférieure constamment droite, la supérieure renversée, d'autrefois enfin on n'en aperçoit qu'une seule directe et reposant sur l'horizon. Les côtes de Calais qui présentent aussi des phénomènes analogues, offrent aussi les mêmes variétés, quelquefois on les voyait doubles un instant après elles étoient invisibles. Toutes ces apparences sont contraires à l'idée d'une sphéricité parfaite des couches d'air qui produisoient ces phénomènes, et l'on conçoit en effet qu'étant le résultat d'une équilibre non stable, ils peuvent difficilement s'accorder avec une forme constante.

On this I would remark, generally, that I think VINCE is here rather hardly treated. It seems to me, on comparing the two explanations, that the reproach of "*autant des lois différentes qu'il y a d'images visibles*" is not merited by VINCE, and would perhaps more justly apply to his censor. It is certainly most unfortunate that VINCE did not note the level of the apparent horizon; though, unless he had done so from a great many different heights above the sea, I fail to see how the observation would have helped to decide between the various possible explanations. BIOT evidently expected a *depression*, for he states as much in reference to the elevated patches of sea and the "heavy fog" which VINCE observed; yet this is inconsistent with his own figure! But the following passage from VINCE's paper (in which I have italicised some words) seems to have escaped the notice of BIOT.

"The *usual* refraction at the same time was uncommonly great; for the tide was high, and at the very edge of the water I could see the cliffs of Calais a very considerable height above the horizon; whereas they are frequently *not to be seen* in clear weather *from the high lands* about the place. The French coast also appeared both ways, to a much greater distance than I ever observed it at any other time: . . . . ."

Now, one of the most striking of VINCE's observations was that of a ship (hull down) with an inverted image above it, both projected on the confused image of the French cliffs as a background. If BIOT's explanation were correct, this background must have been visible by rays of a truly *schlangenförmig* character (as GILBERT calls them), for they must have been at least *twice* (more probably *thrice*) concave downwards; with a convexity downwards, somewhere between the spectator and the ship (and probably another between the ship and the French coast). It seems much more likely that the ship's hull was really beyond the ordinary horizon, and that the French cliffs were visible by rays originally concave upwards so as to rise up, as it were, behind the ship; and then concave downwards, according to the theory I have propounded, from the ship to the spectator.

BIOT's memoir shows, throughout, the pervading influence of his almost daily observations of rays which were concave upwards, because passing very close to the ground over extensive surfaces of hot sand. If his explanation of VINCE's observation were correct, there would have been an inverted image

(of a part of the top-mast) *under* the lowest of the three images, and objects comparatively near hand would have been affected as well as those at a considerable distance.

But there is much more to urge against BIOT's view of the phenomena in question. VINCE expressly states that "the evening was very sultry." As his observations were made at heights above the sea, varying from twenty-five to eighty feet, it is pretty clear that this sultriness was not due to the exceptionally high temperature of the surface of the sea. BIOT, in fact, allows that the effects of this were only sensible "*jusqu' à une petite hauteur.*" But then he assumes (contrary to VINCE's statement) a rapid *descent* of temperature at higher levels. This he looks on as developed, *how* he does not tell us, by the *cold* produced by vapour in condensing! Besides, if this were true, it would make the diminution of density upwards *less*, instead of *greater* than usual, and the optical results of such an arrangement would be in contradiction to his explanation.

It is much to be regretted that VINCE's description, like his drawings, is of the very roughest character. It is quite otherwise with those of SCORESBY. There can be no doubt whatever that BIOT's mode of explanation is altogether inapplicable to the majority of SCORESBY's observations.

I quote a single passage,* which is apparently decisive.

"A dense appearance in the atmosphere arose to the southward of us . . . . When it came to the S.W. of us, I first noticed that the horizon, under this apparent density, was considerably elevated. . . . . Two ships lying beset about fourteen miles off, the hulls of which, before the density came on, could not be wholly seen, seemed now from the mast-head not to be above half the distance, as the horizon was visible considerably beyond them."

Had the arrangement of strata here been as BIOT supposes in VINCE's case, only the top-masts would have remained visible, the apparent horizon would have come in front of the hulls, and there would have been inverted images of nearer objects visible *under* the objects themselves.

It will be observed that these observations were taken over a surface of *ice* in which the vessels were "beset." The sun is said to have been "powerful," but the lowest strata of air, in contact with ice or ice-cold water, must have been colder than those above them. The haze, or "density" as SCORESBY calls it, probably consisted of minute drops of water, and would thus be much raised in temperature by the sun. In connection with this I may mention that when a trough, in which brine has been diffusing for some time into water, is suddenly and roughly stirred for a short period, it settles in a few minutes into a large number of strata of different densities. Something similar must hold

* SCORESBY's *Arctic Regions*, i. 387 (1820).

in the case of air irregularly heated, and thus we have a very probable explanation of the *series* of inverted images figured by SCORESBY. The strata which produced these, in all likelihood produced direct images also, but (except on very rare occasions) so small in vertical dimensions as to have escaped observation. In the absence of wind such strata, once formed, would last for a long time, in consequence of the very small thermal conductivity of air. I might also refer to an interesting case of inverted images seen from a balloon by TISSANDIER.* The height at which the balloon was situated is not stated expressly, but from the context it must have been somewhere about 6000 feet. This, of course, *proves* the existence, at a great elevation, of a stratum in which there was a comparatively rapid diminution of refractive index with increasing height.

I will quote, in conclusion, SCORESBY's account of his remarkable observation of an isolated inverted image of a ship, which was situated far beyond the horizon. His drawing is reproduced as the second of the series in fig. 1. The obvious and simple explanation of this is what has already been mentioned for TISSANDIER's observation, though, of course, it could also be accounted for by an infinite number of different laws of refractive index, all of more or less ingenious complexity.

"The atmosphere, in consequence of the warmth, being in a highly refractive state, a great many curious appearances were presented by the land and icebergs. The most extraordinary effect of this state of the atmosphere, however, was the distinct inverted image of a ship in the clear sky, . . . the ship itself being entirely beyond the horizon. . . . It was so extremely well defined, that when examined with a telescope by DOLLOND, I could distinguish every sail, the general 'rig of the ship,' and its particular character; insomuch that I confidently pronounced it to be my Father's ship, the 'Fame,' which it afterwards proved to be; though, on comparing notes with my Father, I found that our relative position at the time gave our distance from one another very nearly thirty miles, and some leagues beyond the limit of direct vision."†

It seems hard to reconcile the clearness of definition in this case with any other than a stable state of equilibrium of a transition stratum. The mirage of the desert, where the equilibrium is essentially unstable, is always exceedingly unsteady.

BIOT makes a point, to which I have not yet alluded, from VINCE's statement that the inverted image appeared to rise as the object moved farther away. His mode of explaining this, however, savours of the "*autant des lois différentes*," &c. ; and, besides, the result follows quite as directly from my ex-

* GLAISHER's *Travels in the Air*, p. 297 (1871).

† SCORESBY's *Journal of a Voyage to the Northern Whale Fishery* (1823), p. 189.

planation as from his. VINCE's observations were by no means precise enough to make this point certain ; besides, he speaks of the top-masts and not of the hulls ; and, from the diminution of the image as the distance increases, it may be quite true that the top-masts appear to rise in the inverted image while the hull really sinks. At any rate it is assuredly *not so* in the majority of SCORESBY's careful figures. In fig. 1 several examples are shown of multiple images of ships at different distances in nearly the same direction ; and in all it will be observed that the inverted image of the hull is lower as the vessel is farther off. Also that in the upper direct image the hull appears to rise as the vessel recedes.

[Feb. 10, 1883.—I have to acknowledge the kindness of Mr. J. W. L. GLAISHER in verifying, and in some important instances correcting, the numerical values given in §§ 10 and 15. My own original calculations, made for the most part with four-place logarithms only, were insufficient to give accurately the values of ϕ close to the critical point. The reason is obvious from the form of the expression for that quantity as given in § 12, above.]

Fig. 1

Fig. 4.

Fig. 7.

Fig. 5.

Fig. 3.

Fig. 9.

Fig. 8.

Fig. 6.