VIII. *Observations of the Transit of Mercury over the Disk of the Sun; to which is added, an Investigation of the Causes which often prevent the proper Action of Mirrors. By* William Herschel, *LL. D. F. R. S.*

Read February 10, 1803.

THE following observations were made with a view to attend particularly to every phenomenon that might occur during the passage of the planet Mercury over the sun's body. My solar apparatus, on account of the numerous observations I have lately been in the habit of making, was in great order for viewing the sun in the highest perfection; and, very fortunately, the weather proved to be as favourable as I could possibly have wished it.

The time at which the observations were made, not being an object of my investigation, is only to be considered as denoting the order of their succession.

November 9, 1802. About 40′ after seven o'clock in the morning, I directed a telescope, with a glass mirror of 7 feet focal length, and 6,3 inches in diameter, to the sun; and perceived the planet Mercury. It was easily to be distinguished from the openings in the luminous clouds, generally called spots, of which there were more than forty in number. Its perfect roundness would have been sufficient to point it out, had I not already known where to look for it.

$10^h\ 0'$. When the sun was come to a sufficient altitude to show objects on its surface with distinctness, I directed my attention to the contour of the mercurial disk, and found its termination perfectly sharp.

With a 10-feet reflector, and magnifying power of 130, I saw the corrugations of the luminous solar surface, up to the very edge of the whole periphery of the disk of Mercury.

$10^h\ 27'$. When the planet was sufficiently advanced towards the largest opening of the northern zone, I compared the intensity of the blackness of the two objects; and found the disk of Mercury considerably darker, and of a more uniform black tint, than the area of the large opening.

$10^h\ 32'$. The preceding limb of Mercury cuts the luminous solar clouds with the most perfect sharpness; whereas, in the great opening, the descending parapet, down the preceding side, was plainly visible.

It should be remarked, that the instrument here applied to the sun, with the moderate power of 130, is the same 10-feet reflector which, in fine nights, when directed to very minute double stars, will show them distinctly with a magnifier of 1000.

Having often attempted to use high magnifiers in viewing the sun, I wished to make another trial; though pretty well assured I should not succeed, for reasons which will appear hereafter.

With two small double convex lenses, both made of dark green glass, and one of them having the side which is nearest the eye thinly smoked, in order to take off some light, I viewed the sun. Their magnifying power was about 300; and I saw Mercury very well defined; but that complete distinctness, which enables us to judge with confidence of the condition of the object in view, was wanting.

With a single eye-glass, smoked on the side towards the eye, and magnifying 460 times, I also saw Mercury pretty well defined; but here the sun appeared ruddy, and no very minute objects could be perceived.

$11^h\ 28'$. The planet having advanced towards the preceding limb of the sun, it was now time to attend to the appearances of the interior and exterior contacts.

$11^h\ 32'$. 10-feet reflector. The whole disk of Mercury is as sharply defined as possible; there is not the least appearance of any atmospheric ring, or different tinge of light, visible about the planet.

$11^h\ 37'$. Appearances remain exactly as before.

$11^h\ 42'$. The sharp termination of the whole mercurial disk, appears to be even more striking than before. This may be owing to its contrast with the bright limb of the sun, which, having many luminous ridges in the northern zone, is remarkably brilliant about the place of the planet.

$11^h\ 44'$. I was a few moments longer writing down the above than I should have been, to see the interior contact so completely as I could have wished; however, the thread of light on the sun's limb was but just breaking, or broken; but no kind of distortion, either of the limb or of the disk of Mercury, took place.

The appearance of the planet, during the whole time of its emerging from the sun, remained well defined, to the very last.

The following limb of Mercury remained sharp, till it reached the very edge of the sun's disk; and vanished without occasioning the smallest distortion of the sun's limb, in going off, or suffering the least alteration in its own figure.

As soon as the planet had quitted the sun, the usual appearance of its limb was so instantly and perfectly restored, that not the least trace remained whereby the place of its disappearance could have been distinguished from any other adjacent part of the solar disk.

It will not be amiss to add, that very often, during the transit, I examined the appearance of Mercury with a view to its figure, but could not perceive the least deviation from a spherical form; so that, unless its polar axis should have happened to be situated, at the time of observation, in a line drawn from the eye to the sun, the planet cannot be materially flattened at its poles.

OBSERVATIONS AND EXPERIMENTS RELATING TO THE CAUSES WHICH OFTEN AFFECT MIRRORS, SO AS TO PREVENT THEIR SHOWING OBJECTS DISTINCTLY.

It is well known to astronomers, that telescopes will act very differently at different times. The cause of the many disappointments they may have met with in their observations, is however not so well understood.

Sometimes we have seen the failure ascribed to certain tremors, as belonging to specula; and remedies have been pointed out for preventing them. Not unfrequently again, the telescope itself has been condemned; or, if its goodness could not admit of a doubt, the weather in general has been declared bad, though possibly it might be as proper for distinct vision as any we can expect in this changeable climate.

The experience acquired by many years of observation, will however, I believe, enable me now to assign the principal cause of the disappointments to which we are so often exposed. Unwilling to hazard any opinion that is not properly supported by facts, I shall have recourse to a collection of occasional observations. They have been made with specula of undoubted goodness, so that every cause which impeded their proper action must be looked upon as extrinsic. I shall arrange these observations under different heads, that, when they have been related, there may remain no difficulty to draw a few general conclusions from them, which will be found to throw a considerable light upon our subject.

Moisture in the Air.

(1.) October 5, 1781. I see double stars, with 460, completely well. The air is very damp.

(2.) Nov. 23, 1781. $15^h\ 30'$. The morning is uncommonly favourable, and I see the treble star ζ Cancri, with 460, in high perfection. The air is very moist, and intermixed with passing clouds.

(3.) Sept. 7, 1782. I viewed the double star preceding 12 Camelopardalis,* with 932. In this, and several other fine nights which I have lately had, the condensing moisture on the tube of my telescope has been running down in streams; which proves that damp air is no enemy to good vision.

(4.) Dec. 28, 1782. $17^h\ 30'$. The water condensing on my tube keeps running down; yet I have seen very well all night. I was obliged to wipe the object-glass of my finder almost continually. The specula, however, are not in the least affected with the damp. The ground was so wet that, in the morning, several people believed there had been much rain in the night, and were surprised when I assured them there had not been a drop.

(5.) Feb. 19, 1783. I have seen perfectly well till now† that

* See Phil. Trans. Vol. LXXV. Part I. page 68; II. 53.

† The time is not marked in the journal; but, from the number of the observations that had been made during the night, it must have been towards morning.

a frost is coming on; though Datchet Common, which is just before my garden, is all under water; and the grass on which I stand with my telescope is as wet as possible.

(6.) Feb. 26, 1783. All the ground is covered with snow; yet I see remarkably well.

(7.) March 8, 1783. The common before my garden is all under water; my telescope is running with condensed vapour; not a breath of air stirring. I never saw better.

(8.) August 25, 1783. My telescope ran with water all the night. The small speculum, which sometimes gathers moisture, was never affected in the 7-feet tube, but was a little so in the 20-feet. The large eye-glasses and object-glasses of the finders, required wiping very often. I saw all night remarkably well.

Fogs.

(9.) Oct. 30, 1779. It grows very foggy, and the moon is surrounded with strong nebulosity; nevertheless, the stars are very distinct, and the telescope will bear a considerable power.

(10.) August 20, 1781. It is so foggy that I cannot see an object at the distance of 40 feet; yet the stars are very distinct in the telescope. By an increase of the fog, α Piscium can no longer be seen by the eye; yet, in the telescope, it being double, I see both the stars with perfect distinctness.

(11.) Sept. 6, 1781. A fog is come on; yet I see very well.

(12.) Sept. 9, 1781. There is so strong a fog, that hardly a star less than 30° high is to be seen; and yet, in the telescope, at great elevations, I see extremely well.

(13.) March 9, 1783. It is very foggy; yet in the telescope I see the stars without aberration, and they are very bright. α Serpentarii is without a single ray.

(14.) April 6, 1783. A very thick fog settles upon all my glasses; but the specula, even the 20-feet, which has so large a surface, remain untouched. I see perfectly well.

Frost.

(15.) Nov. 15, 1780; 5 o'clock in the morning. An excellent speculum, No. 2, will not act properly; the frosty morning probably occasions an alteration in its figure. Another speculum, No. 1, acts but indifferently, though I have known it to shew very well formerly in a very hard frost: for instance, November 23, 1779, I saw with the same mirror, and a power of 460, the vacancy between the two stars of the double star Castor, without the least aberration.

(16.) Oct. 22, 1781. Frost seems to be no hindrance to perfect vision. The tube of my 7-feet telescope is covered with ice; yet I see very well.

(17.) Nov. 19, 1781. It freezes very hard, and the stars, even those which are 50° high, are very tremulous. I suspect their apparent diameters to be diminished; and, if I recollect right, this is not the first time that such a suspicion has occurred to me.

(18.) Jan. 10, 1782. My telescope would not act well, even at an altitude of 70 or 80 degrees. There is a strong frost.

(19.) Jan. 31, 1782. I cannot see with a power of 460, the stars seem to dance so unaccountably, and yet the air is perfectly calm: even at 60 or 70 degrees of altitude, vision is impaired.

(20.) Feb. 9, 1782. That frost is no hindrance to seeing well is evident; for, not only my breath freezes upon the side of the tube, but more than once have I found my feet fastened to the ground, when I have looked long at the same star.

(21.) Oct. 4, 1782. It froze very severely this night. At first, when the frost came on, I saw very badly, every object being tremulous; but, after some time, and at proper altitudes, I saw as well as ever. Between 5 and 6 o'clock in the morning, objects began to be tremulous again; occasioned, I suppose, by the coming on of a thaw.

(22.) Jan. 1, 1783. I made a number of delicate observations this night, notwithstanding, at 4 o'clock in the morning, my ink was frozen in the room; and, about 5 o'clock, a 20-feet speculum, in the tube, went off with a crack, and broke into two pieces. On looking at FAHRENHEIT's thermometer, I found it to stand at 11°.

(23.) May 6, 1783. It freezes, and in the telescope the stars seem to dance extremely.

Hoar-frost.

(24.) Nov. 6, 1782. There is a thick hoar-frost; yet I see extremely well. It seems to enlarge the diameters of the stars; but, as I see the minutest double stars well, the apparent enlargement of the diameters must be a deception.

(25.) Dec. 22, 1782. There is a strong hoar-frost gathering upon the tubes of my telescopes; but I see very well.

Dry Air.

(26.) Dec. 21, 1782. The tube of my telescope is dry, and I do not see well.

(27.) April 30, 1783. The stars are extremely tremulous and confused; the outside of the tube of my telescope is quite dry.

Northern Lights.

(28.) Sept. 25, 1781. There are very strong northern lights; their flashing does not seem to interfere with telescopic vision; but all objects appear tremulous, and indifferently defined.

(29.) Aug. 30, 1782. There are very bright northern lights, in broad arches, with white streaks; yet I see perfectly well.

(30.) March 26, 1783. An Aurora Borealis is so bright, that η Herculis, which it covers, can hardly be seen; yet, in the telescope, and with a power of 460, I find no difference. I compared that star with γ Coronæ, which was in a bright part of the heavens, and in the telescope they appeared nearly alike. I suspected η Herculis to be somewhat more tinged with red than it should be; and examined it afterwards, when clear of the Aurora: it was indeed less red; but, as it had gained more altitude, the experiment was not decisive.

Windy Weather.

(31.) Jan. 8, 1783. It is very windy. The diameters of the stars are strangely increased, even those at 60 and 70° of altitude. Every star seems to be a little planet.

(32.) Jan. 9, 1783. Wind increases the apparent diameters of the stars.

(33.) Sept. 20, 1783. The night has been very windy; and I do not remember ever to have seen so ill, with such a beautiful appearance of brilliant star-light.

Fine in Appearance.

(34.) May 28, 1781. The evening, though fine in appearance, is not favourable. No instrument I have will act properly. The wind is in the east.

(35.) August 30, 1781. The stars appear fine to the naked eye, so that I can see ϵ Lyræ very distinctly to be two stars; yet my telescope will show nothing well. There are flying clouds, which, by their rapid motion, indicate a disturbance in the upper regions of the air; though, excepting now and then a few gusts of wind, it is in general very calm. At a distance there are continual flashes of lightning, but I can hardly hear any thunder.

(36.) Sept. 14, 1781. I see very small stars with the naked eye; but the telescope will not act so well as it should.

(37.) Sept. 24, 1781. The evening is apparently fine; but, with the telescope, I can see neither η Coronæ nor μ Bootis double; nor indeed can I see any other stars well.

Over a Building.

(38.) August 24, 1780. I viewed ϵ Bootis with 449, 737, and 910, but saw it very indifferently. The star was over a house.

(39.) Oct. 26, 1780. ϵ Bootis being near the roof of a house, I saw it not so distinctly as I could wish.

The Telescope lately brought out.

(40.) Oct. 10, 1780. 6^h 30′. Having but just brought out my telescope, it will not act well.

6^h 45′. The tube and specula are now in order, and perform very well.

(41.) Jan. 11, 1782. To all appearance, the morning was very fine, but still the telescope, when first brought out, would not act well. After half an hour's exposure, it performed better.

Confined Place.

(42.) July 19, 1781. 13^h 15′. My telescope would not act well; and, supposing the exhalations from the grass in my garden to affect vision, I carried the telescope into the street, (the observation was made at Bath,) and found it to perform to admiration.

(43.) July 19, 1781. My telescope acted very well; but a slight field-breeze springing up, and brushing through the street where my instrument was placed, it would no longer bear a magnifying power of 460.

Haziness and Clouds.

(44.) Sept. 22, 1783. The weather is now so hazy, that the double star δ Cygni is but barely visible to the naked eye. This has taken off the rays of the large star, so that I now see the small one extremely well, which at other times it is so difficult to perceive, even with a magnifying power of 932.

(45.) August 13, 1781. A cloud coming on very gradually upon fixed stars, has this remarkable effect, that their apparent diameters diminish gradually to nothing.

(46.) July 7, 1780. The air was very hazy, but extremely calm. I had Arcturus in the field of view of the telescope, and, the haziness increasing, it had a very beautiful effect on the apparent diameter of this star. For, supposing the first of the points, Plate III. Fig. 1, to represent its magnitude when brightest, I saw it gradually decrease, and assume, with equal distinctness, the form of all the succeeding points, from No. 1 to No. 10, in the order of the numbers placed over them. The last magnitude I saw it under, could certainly not exceed twotenths of a second; but was perhaps less than one. This leads to the discovery of one of the causes of the apparent magnitude of the fixt stars.

Focal Length.

(47.) Nov. 14, 1801. The focal length of my 10-feet mirror increases by the heat of the sun. I have often observed this before; the difference, by several trials, amounts to 8 hundredths of an inch.

(48.) Dec. 13, 1801. The focal length of my 10-feet mirror, while I was looking at the sun, became shorter, contrary to what it used to do; but, there being a strong frost, I guess that the object metal grows colder, notwithstanding its exposure to the sun's rays.

(49.) Nov. 9, 1802. $10^h\ 50'$. The focus of my 7-feet glass mirror became 18 hundredths of an inch shorter, on being exposed for about a minute to the sun. The figure of the speculum was also distorted; the foci of the inside and outside rays differing considerably, though its curvature, by observations on the stars, has been ascertained to be strictly parabolical.

$12^h\ 0'$. The same mirror, exposed one minute to the action of the sun, became 21 hundredths shorter in focal length.

The focus of a 10-feet metalline mirror, when exposed one minute to the sun's rays, became 15 hundredths of an inch longer than it was before.

(50.) January 9, 1803. When I looked with the glass 7-feet mirror, several times, a minute or two at the sun, it shortened generally ,24, ,26, and ,30 of an inch, in focal length.

The observations which are now before us, appear to be sufficient to establish the following principle; namely, "That in order to see well with telescopes, it is required that "the temperature of the atmosphere and mirror should be uni- "form, and the air fraught with moisture."

This being admitted, we shall find no difficulty in accounting for every one of the foregoing observations.

If an uniform temperature be necessary, a frost after mild weather, or a thaw after frost, will derange the performance of our mirrors, till either the frost or the mild weather are sufficiently settled, that the temperature of the mirror may accommodate itself to that of the air. For, till such an uniformity with the open air, in the temperature of the mirror, the tube, the eye-glasses, and I would almost add the observer, be obtained, we cannot expect to see well. See observation 15, 17, 18, 19, and 23.

But, when a frost, though very severe, becomes settled, the mirror will soon accommodate itself to the temperature; and we shall find our telescopes to act well. See obs. 16, 20, 21, 22, 24, and 25.

This explains, with equal facility, why no telescope just brought out of a warm room can act properly. See obs. 40 and 41.

Nor can we ever expect to make a delicate observation, with high magnifying powers, when looking through a door, window, or slit in the roof of an observatory; even a confined place, though in the open air, will be detrimental. See obs. 42 and 43.

It equally shows, that windy weather in general, which must occasion a mixture of airs of different temperatures, cannot be favourable to distinct vision. See obs. 31, 32, and 33.

The same remark will apply to Auroræ Boreales, when they induce, as they often do, a considerable change in the temperature of the different regions of air. See obs. 28.

But, should they not be accompanied by such a change, there seems to be no reason why they should injure vision. See obs. 29 and 30.

The warm exhalations from the roof of a house in a cold night, must disturb the uniformity of the temperature of a small portion of air; so that stars which are over the house, and at no considerable distance, may be affected by it. See obs. 38 and 39.

Sometimes the weather appears to be fine, and yet our telescopes will not act well. This may be owing to dryness occasioned by an easterly wind; or to a change of temperature, arising from an agitation of the upper regions of the atmosphere. See obs. 34 and 35.

Or, possibly, to both these causes combined together. See obs. 36 and 37.

If moisture in the atmosphere be necessary, dry air cannot be proper for vision. See obs. 26 and 27.

And therefore, on the contrary, dampness, and haziness of the atmosphere, must be favourable to distinct vision. See obs. 1, 2, 3, 4, 6, and 8.

Fogs also, which certainly denote abundance of moisture, must be very favourable to distinct vision. See obs. 9, 10, 11, 12, 13, and 14.

Nay, if the observatory should be surrounded by water, we need be under no apprehension on that account. Perhaps, were we to erect a building for astronomical purposes only, we ought not to object to grounds which are occasionally flooded; the neighbourhood of a river, a lake, or other generally called damp situations. See obs. 5 and 7.

It is however possible, that fogs and haziness may increase to such a degree as, at last, to take away, by their interposition, all the light which comes from celestial objects; in which case, they must of course put an end to observation; but they will nevertheless be accompanied with distinct vision to the very last. See obs. 44, 45, and 46.

We have now only the four last observations to account for. They relate to the change of the focal length of mirrors in solar observations, and its attendant derangement of the foci of the different parts of the reflecting surface; and, as simplicity is one of the marks of the truth of a principle, I believe we need not have recourse to any other cause than the change of temperature produced by the action of the solar rays that occasion heat; which will be quite sufficient to explain all the phenomena. But, in order to show this in its proper light, I shall relate the following experiments.

1st *Experiment.*

I placed a glass mirror, of 7-feet focal length, in the tube belonging to the telescope; and, having laid it open at the back, I prepared a stand, on which the iron used in my experiments on the terrestrial Rays that occasion Heat (see Phil. Trans. for 1800, Plate XVI. Fig. 1) might be placed, so as to heat the mirror from behind, while I kept a certain object in the field of view of the telescope. Having measured the focal length, and also examined the figure of the mirror, which was parabolical, the heated iron was applied so as to be about $2\frac{1}{4}$ inches from the back of the glass mirror. The consequence of this was, that a total confusion in all the foci took place, so that the letters on a printed card in view, which before had been extremely distinct, became instantly illegible. In 15 seconds, the focus of the mirror was shortened 2,3 inches; in half a minute, 3,47 inches; and, at the end of the minute, I found it no less than 4,59 inches shorter than it had been before the application of the hot iron.

On repeating the experiment, but placing the heated iron no more than $\frac{3}{4}$ of an inch from the back of the mirror, its focal length, in $1\frac{1}{2}$ minute, became 5,33 inches shorter.

I tried also a more moderate heat; and, placing the iron at 3 inches from the back, the focus of the mirror shortened in one minute 2,83 inches.

A thermometer placed in contact with the reflecting surface of the mirror, could hardly be perceived to have risen, during the time in which the hot iron produced the alteration of the focal length.

2d Experiment.

Every thing remaining as before, I suspended a small globe of heated iron in front of the mirror, at one inch and a half from its vertex; and, in two minutes, the focus was lengthened 5,3 inches. The figure of the mirror was also deranged; so that the letters on the card could not be distinguished.

I made a second trial, with the suspended iron a little more heated, and brought it as near the surface of the mirror as I judged it to be safe; since a contact would probably have cracked the mirror. In consequence of this arrangement, the focus lengthened, in one minute, 1,64 inch.

On removing the heated iron, the mirror returned, in one minute, to within ,18 inch of its former focal length; and, at the end of the second minute seemed to be nearly restored. But the disagreement of the foci of the different parts of the reflecting surface might be perceived for a long time afterwards, and caused an indistinctness of vision, which plainly indicated that, under such circumstances, the magnifying power of the telescope, 225, was more than it ought to be, in order to see well.

3d Experiment.

I now changed the glass mirror for a metalline one; and, on placing the heater near the back of it, the focus of the speculum, in 30 seconds, became ,77 inch shorter. But, continuing the observation, instead of shortening still farther in the next 30 seconds, it became ,3 inch longer, so that, at the end of a minute, it was only ,47 shorter than before the approach of the hot iron.

4th Experiment.

When the small heated globe of the 2d experiment was suspended in front of the mirror, the focus lengthened ,27 inch in one minute; nor would the lengthening increase by leaving the hot iron longer in its position. The foci in this, as well as in the 3d experiment, were so much injured that they could not be measured with any precision; and it was evident, that high magnifying powers ought not to be used with a mirror of which the temperature is undergoing a continual change.

I repeated the experiment with the iron nearly red hot; and found the focus lengthened 1,48 inch in 30 seconds. Five minutes after the removal of the iron, the regularity of the figure of the mirror was pretty well restored.

With a moderate heat, I had, in 30 seconds, a lengthening of the focus, of ,57 inch; and, in about 1½ minute after the removal of the heated iron, distinct vision was nearly restored.

These four experiments show, that a change in the temperature of mirrors, occasioned by heat, is attended with an alteration of their focal length; and also prove, that the figure of the reflecting surface is considerably injured, during the time that such a change takes place. We are consequently authorised to believe, that the small alteration in the focus of a mirror exposed to the rays of the sun, arises from the same cause. For, since a thermometer, when the sun is shining upon it, will show that its temperature is altered, the action of the solar rays upon a mirror must be attended with a similar effect in its temperature. See obs. 47, 48, 49, and 50.

The same experiments will now also explain why the observations of the sun, related in our transit of Mercury, between $10^h\ 32'$ and $11^h\ 28'$, were not attended with success; for we have seen that heat occasions a derangement in the action of the reflecting surface; and it follows that, under such circumstances, high magnifying powers cannot be expected to show objects very distinctly.

If it should be remarked, that I have not explained why the focus of a glass mirror should shorten by the same rays of the sun which lengthen that of a metalline speculum, I confess that this at present does not appear; and, as it is not material to our purpose, I might pass it over in silence. We are however pretty well assured, that the alterations of the focal length must be owing to a dilatation of the glass or metal of which mirrors are made, and must be greatest where most heat is applied. Our experiments therefore cannot agree perfectly with solar observations; for, in the glass mirror, the application of partial heat in front, must undoubtedly have been much stronger about the middle of the mirror (though the centre of it was sometimes guarded by a brass plate equal to the size of the small speculum) than at the circumference. But when, on the contrary, a mirror is exposed to the sun, every part of the surface will receive an equal portion of heat.

It may also be said, that I have pointed out a defect in telescopes used for solar observations, without assigning a cure for it. It will however be allowed, that tracing an evil to its cause must be the first step towards a remedy. Had the imperfection of the figure brought on by the heat of the solar rays been of a regular nature, an elliptical speculum might have been used to counteract the assumed hyperbolical form; or *vice versâ*.

And now, as, properly speaking, the derangement of the figure of a mirror used in observing the sun, is not so much caused by the heat of its rays as by their partial application to the reflecting surface only, which produces a greater dilatation in front than at the back, there may be a possibility of counteracting this effect, by a contrary application of heat against the back, or by an interception of it on the front. But this we leave to future experiments.

Basire sc.