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been systematically but fruitlessly sought for, and I entertained a very strong opinion, notwithstanding the circumstantial nature of Struve's account and his confidence (shared in unquestioningly by others who were present), that he had been in some way deceived. But I could not then see, nor has anyone yet explained, how this could be. The fact, however, that he had been deceived is now undoubted. Subsequent research has shown that the Pulkowa telescope, though a very fine instrument, possesses the undesirable faculty of making a companion orb for all first-class stars in the position where O. Struve and his assistant Lindenau saw the supposed companion of Procyon.

I may as well point out, however, that theories so wild have recently been broached respecting Venus, that far more interesting explanations of the enigma than this optical one may be looked for presently. It has been gravely suggested by Mr. James Brett, the artist, that Venus has a surface of metallic brilliancy, with 'a vitreous atmosphere,' which can only be understood to signify a glass envelope. This stupendous theory has had its origin in an observation of considerable interest, which astronomers (it is perhaps hardly necessary to say) explain somewhat differently. When Venus has made her entry in part upon the sun's face at the beginning of transit, there is seen all round the portion of her disc which still remains outside the sun an arc of light so brilliant that it records its photographic trace during the instantaneous exposure required in solar photography. It is mathematically demonstrable that this arc of light is precisely what should be seen if Venus has an atmosphere like our earth's. But mathematical demonstration is not sufficient (or perhaps we may say it is too much) for some minds. Therefore, to simplify matters, Venus has been provided with a mirror surface and a glass case.

We owe, by the way, an astronomical myth of the first water to the same ingenious artist. The sun's corona, seen during the time of total solar eclipse, vanishes from view to ordinary eyesight with the first returning rays of direct sunlight. But Mr. Brett sees it, or what he takes to be the corona, in full daylight.

The enigma next to be considered is of a more doubtful character than the myth relating to a satellite of Venus. Astronomers are pretty well agreed that Venus has no moon; but many, including some deservedly eminent, retain full belief in the story of the planet Vulcan.

More than seventeen years ago the astronomical world was startled by the announcement that a new planet had been discovered, under circumstances unlike any which had heretofore attended the discovery of fresh members of the solar system. At

that time astronomers had already become accustomed to the discovery year after year of several asteroids, which are in reality planets though small ones. In fact, no fewer than fifty-six of these bodies were then known, whereof fifty-one had been discovered during the years 1847–1858 inclusive, not one of these years having passed without the detection of an asteroid. But all these planets belonged to one family; and as there was every reason to believe that thousands more travelled in the same region of the solar system, the detection of a few more among the number had no longer any special interest for astronomers. The discovery of the first known member of the family had indeed been full of interest, and had worthily inaugurated the present century, on the first day of which it was made. For it had been effected in pursuance of a set scheme, and astronomers had almost given up all hopes of success in that scheme when Piazzi announced his detection of little Ceres. Again the discovery of the next few members of the family had been interesting as revealing the existence of a new order of bodies in the solar system. No one had suspected the possibility that besides the large bodies which travel round the sun, either singly or attended by subordinate families of moons, there might be a ring of many planets. This was what the discovery of Ceres, Pallas, Juno, and Vesta seemed to suggest, unless—still stranger thought—these were but fragments of a mighty planet which had been shattered in long-past ages by some tremendous explosion. Since then, however, this startling theory has been (itself) exploded. Year after year new members of the ring of multitudinous planets are discovered, and that, not as was recently predicted, in numbers gradually decreasing, but so rapidly that more have been discovered during the last ten years than during the preceding twenty.

The discovery of the giant planet Uranus, an orb exceeding our earth twelve and a half times in mass and seventy-four times in volume, was a matter of much greater importance, so far as the dignity of the planetary system was concerned; for it is known that the whole ring of asteroids together does not equal one-tenth part of the earth in mass, while Uranus exceeds many times in volume the entire family of terrestrial planets—Mercury, Venus, the Earth, and Mars. The detection of Uranus, unlike that of Ceres, was effected by accident. Sir W. Herschel was looking for double stars of a particular kind in the constellation Gemini when by good fortune the stranger was observed.

The interest with which astronomers received the announcement of the discovery of Uranus, though great, was not to be compared with that with which they deservedly welcomed the discovery of Neptune, a larger and more massive planet, revolving at a distance

one-half greater even than the mighty space which separates Uranus from the sun—a space so great that by comparison with it the range of 184,000 of miles, which forms the diameter of our earth's orbit, seems quite insignificant. It was not, however, the vastness of Neptune's mass or volume, or the awful remoteness of the path along which he pursues his gloomy course, which attracted the interest of astronomers, but the strangeness of the circumstances under which the planet had been detected. His influence had been felt for many years before astronomers thought of looking for him; and even when the idea had occurred to one or two, it was considered, and that too by an astronomer as deservedly eminent as Sir G. Airy, too chimerical to be reasonably entertained. All the world now knows how Leverrier, the greatest living master of physical astronomy, and Adams, then scarce known outside Cambridge, both conceived the idea of finding the planet, not by the simple method of looking for it with a telescope, but by the mathematical analysis of the planet's disturbing influence upon known members of the solar system. All know, too, how these mathematicians succeeded in their calculations, and how the planet was found in the very region and close to the very point indicated first by Adams, and later, but independently (and fortunately for him more publicly), by Leverrier.

None of these instances of the discovery of members of the solar system resembled in method or details the discovery announced early in the year 1859. It was not amid the star-depths and in the darkness of night that the new planet was looked for, but in broad day, and on the face of the sun himself. It was not on the outskirts of the solar system that the planet was supposed to be travelling, but within the orbit of Mercury, hitherto regarded as of all planets the nearest to the sun. It was not hoped that any calculation of the perturbations of other planets would show the place of the stranger, though certain changes in the orbit of Mercury seemed clearly enough to indicate the stranger's existence.

Early in 1860, Leverrier had announced that the position of Mercury's path was not precisely in agreement with calculations based on the adopted estimates of the masses of those planets which chiefly disturb the motions of Mercury. The part of the path where Mercury is nearest to the sun, and where, therefore, he travels fastest, had slightly shifted from its calculated place. This part of the path was expected to move, but it had moved more than was expected; and of course Mercury, having his region of swiftest motion somewhat differently placed than was anticipated, himself moved somewhat differently.

Leverrier found that, to explain this feature of Mercury's motion,

either the mass of Venus must be regarded as one-tenth greater than had been supposed, or some unknown cause must be regarded as affecting the motion of Mercury. A planet as large as Mercury, about midway between Mercury and the sun, would account for the observed disturbance; but Leverrier rejected the belief that such a planet exists, simply because he could not 'believe that it would be invisible during total eclipses of the sun.' 'All difficulties disappear,' he added, 'if we admit, in place of a single planet, small bodies circulating between Mercury and the sun.' Considering their existence as not at all improbable, he advised astronomers to watch for them.

It was on January 2, 1860, that Leverrier thus wrote. On December 22, 1859, a letter had been addressed by a M. Lescarbault of Orgères to Leverrier, through M. Vallée, hon. inspector-general of roads and bridges, announcing that on March 26, 1859, about four in the afternoon, Lescarbault had seen a round black spot on the face of the sun, and had watched it as it passed across, like a planet in transit, not with the slow motion of an ordinary sun-spot. The actual time during which the round spot was visible was one hour seventeen minutes nine seconds, the rate of motion being such that, had the spot crossed the middle of the sun's disc at the same rate, the transit would have lasted more than four hours. The spot thus merely skirted the sun's disc, being at no time more than about one forty-sixth part of the sun's apparent diameter from the edge of the sun. Lescarbault expressed the conviction that on a future day a black spot, perfectly round and very small, will be seen passing over the sun, and 'this point will very probably be the planet whose path I observed on March 26, 1859.' 'I am persuaded,' he added, 'that this body is the planet, or one of the planets, whose existence in the vicinity of the sun M. Leverrier had made known a few months ago' (referring to the preliminary announcement of results which Leverrier published afterwards more definitely).

Leverrier, when the news of Lescarbault's observation first reached him, was surprised that the observation should not have been announced earlier. He did not consider the delay sufficiently justified by Lescarbault's statement that he wished to see the spot again. He therefore set out for Orgères, accompanied by M. Vallée. 'The predominant feeling in Leverrier's mind,' says Abbé Moigno, 'was the wish to unmask an attempt to impose upon him, as the person more likely than any other astronomer to listen to the allegation that his prophecy had been fulfilled.'

'One should have seen M. Lescarbault,' says Moigno, 'so small, so simple, so modest, and so timid, in order to understand the

emotion with which he was seized, when Leverrier, from his great height, and with that blunt intonation which he can command, thus addressed him: "It is then you, sir, who pretend to have observed the intra-mercurial planet, and who have committed the grave offence of keeping your observation secret for nine months. I warn you that I have come here with the intention of doing justice to your pretensions, and of demonstrating either that you have been dishonest or deceived. Tell me, then, unequivocally, what you have seen."' This singular address did not bring the interview, as one might have expected, to an abrupt end. The lamb, as the Abbé calls the doctor, trembling, stammered out an account of what he had seen. He explained how he had timed the passage of the black spot. 'Where is your chronometer?' asked Leverrier. 'It is this watch, the faithful companion of my professional journeys.' 'What! with that old watch, showing only minutes, dare you talk of estimating seconds. My suspicions are already too well confirmed.' 'Pardon me, I have a pendulum which beats seconds.' 'Show it me.' The doctor brings down a silk thread to which an ivory ball is attached. Fixing the upper end to a nail, he draws the ball a little from the vertical, counts the number of oscillations, and shows that his pendulum beats seconds; he explains also how, his profession requiring him to feel pulses and count pulsations, he has no difficulty in mentally keeping record of successive seconds.

Having been shown the telescope with which the observation was made, the record of the observation (on a piece of paper covered with grease and laudanum, and doing service as a marker in the 'Connaissance des Temps,' or French Nautical Almanac), Leverrier presently inquired if Lescarbault had attempted to deduce the planet's distance from the sun from the period of its transit. The doctor admitted that he had attempted this, but, being no mathematician, had failed to achieve success with the problem. He showed the rough drafts of his futile attempts at calculation on a board in his workshop; for said he naïvely, 'I am a joiner as well as an astronomer.'

The interview satisfied Leverrier that a new planet, travelling within the orbit of Mercury, had really been discovered. 'With a grace and dignity full of kindness,' says a contemporary narrative of these events,[1] 'he congratulated Lescarbault on the important discovery which he had made. Anxious to obtain some mark of respect for the discoverer of Vulcan, Leverrier made inquiry concerning his private character, and learned from the village curé, the juge de paix, and other functionaries, that he was a skilful

[1] *North British Review* for August 1860.

physician and a worthy man. With such high recommendations, M. Leverrier requested from M. Rouland, the Minister of Public Instruction, the decoration of the Legion of Honour for M. Lescarbault. The Minister, in a brief but interesting statement of his claim, communicated this request to the Emperor, who by a decree dated January 25 conferred upon the village astronomer the honours so justly due to him. His professional brethren in Paris were equally solicitous to testify their regard; and MM. Felix Roubaud, Legrande, and Caffe, as delegates of the scientific press, proposed to the medical body, and to the scientific world in Paris, to invite Lescarbault to a banquet in the Hôtel de Louvre on January 18.'

The announcement of the supposed discovery caused astronomers to re-examine records of former observations of black spots moving across the sun. Several such records existed, but they had gradually come to be regarded as of no real importance. Wolff of Zurich published a list of no fewer than twenty such observations made since 1762. Carrington added many other cases. Comparing together three of these observations, Wolff found that they would be satisfied by a planet having a period of revolution of $19\frac{1}{4}$ days, agreeing fairly with the period of rather more than $19\frac{1}{2}$ days inferred by Leverrier for Lescarbault's planet. But the entire set of observations of black spots require that there should be at least three new planets travelling between Mercury and the sun. Many observers also set themselves the task of searching for Vulcan, as the supposed new planet was called. They have continued fruitlessly to observe the sun for this purpose until the present time.

While the excitement over Lescarbault's discovery was at its height, another observer impugned not only the discovery but the honesty of the discoverer.

M. Liais, a French astronomer of considerable skill, formerly of the Paris Observatory, but at the time of Lescarbault's achievement in the service of the Brazilian Government, published a paper, 'Sur la nouvelle Planète annoncée par M. Lescarbault,' in which he endeavoured to establish the four following points :—

First, the observation of Lescarbault was never made.

Secondly, Leverrier was mistaken in considering that a planet such as Vulcan might have escaped detection when off the sun's face.

Thirdly, Vulcan would certainly have been seen during total solar eclipses, if the planet had a real objective existence.

Fourthly, M. Leverrier's reasons for believing that the planet

exists are based on the supposition that astronomical observations are more precise than they really are.

Probably, Liais's objections would have had more weight with Leverrier had the fourth point been omitted. It was rash in a former subordinate to impugn the verdict of the chief of the Paris Observatory on a matter belonging to that special department of astronomy which an observatory chief might be expected to understand thoroughly. It is thought daring in the extreme for one outside the circles of official astronomy (as Sir Isaac Newton in Flamsteed's time, Sir W. Herschel in Maskelyne's, and Sir J. Herschel in the present century) to advance or maintain an opinion adverse to that of some official chief; but for a subordinate (even though no longer so) to be guilty of such rash procedure 'is most tolerable and not to be endured,' as a typical official has said. Accordingly very little attention was paid by Leverrier to Liais's objections.

Yet, in some respects, what M. Liais had to say was very much to the point.

At the very time when Lescarbault was watching the black spot on the sun's face, Liais was examining the sun with a telescope of much greater magnifying power, and saw no such spot. His attention was specially directed to the edge of the sun (where Lescarbault saw the spot) because he was engaged in determining the decrease of the sun's brightness near the edge. Moreover, he was examining the very part of the sun's edge where Lescarbault saw the planet enter, at a time when it must have been twelve minutes (in time) upon the face of the sun, and well within the margin of the solar disc. The negative evidence here is strong; though it must always be remembered that negative evidence requires to be overwhelmingly strong before it can be admitted as effective against positive evidence. It seems at a first view utterly impossible that Liais, examining with a more powerful telescope the region where Lescarbault saw the spot, could have failed to see it had it been there; but experience shows that it is not impossible for an observer engaged in examining phenomena of one class to overlook a phenomenon of another class, even when glaringly obvious. All we can say is that Liais was not likely to have overlooked Lescarbault's planet had it been there; and we must combine this probability against Vulcan's existence with arguments derived from other considerations. There is also the possibility of an error in time. As the writer in the 'North British Review' remarks, 'twelve minutes is so short a time, that it is just possible that the planet may not have entered upon the sun during the time that Liais observed it.'

The second and third arguments are stronger. In fact, I do not see how they can be resisted.

It is, in the first place, clear from Lescarbault's account that Vulcan must have a considerable diameter—certainly, if Vulcan's diameter in miles were only half the diameter of Mercury, it would have been all but impossible for Lescarbault with his small telescope to see Vulcan at all, whereas he saw the black spot very distinctly. Say Vulcan has half the diameter of Mercury, and let us compare the brightness of these two planets when at their greatest apparent distances from the sun, when each looks like a half-moon. The distance of Mercury exceeds the estimated distance of Vulcan from the sun as 27 exceeds 10; so that Vulcan is more strongly illuminated in the proportion of 27 times 27 to 10 times 10, or 729 to 100—say at least 7 to 1. But, having a diameter but half as large, the disc of Vulcan could be but about a fourth of Mercury's at the same distance from us, and they would be at about the same distance from us when seen as half-moons. Hence Vulcan would be brighter than Mercury in the proportion of 7 to 4. Of course, being so near the sun, he would not be so easily seen; and we could never expect to see him at all, perhaps, with the naked eye—though even this is not certain. But Mercury when at the same apparent distance from the sun, and giving less light than at his greatest seeming distance, is quite easily seen in the telescope. Much more easily, then, should Vulcan be seen, if a telescope were rightly directed at such a time, or when Vulcan was anywhere near his greatest seeming distance from the sun. Now, it is true, astronomers do not know precisely when or where to look for him. But he passes from his greatest distance on one side of the sun to his greatest distance on the other in less than ten days, according to the computed period, and certainly (that is, if the planet exists) in a very short time. The astronomer has, then, only to examine day after day a region of small extent on either side of the sun, for ten or twelve days in succession (an hour's observation each day would suffice), to be sure of seeing Vulcan. Yet many astronomers have made such search many times over, without seeing any trace of the planet. During total solar eclipses, again, the planet has been repeatedly looked for unsuccessfully—though it should at such a time be a very conspicuous object when favourably placed, and could scarcely fail of being very distinctly seen wherever placed.

The fourth argument of Lescarbault is not so effective, and in fact he gets beyond his depth in dealing with it. But it is to be noticed that a considerable portion of the discrepancy between Mercury's observed and calculated motions has long since been

accounted for by the changed estimate of the earth's mass as compared with the sun's, resulting from the new determination of the sun's distance. However, the arguments depending on this consideration would not be suitable to these pages.

There was one feature in Liais's paper which was a little unfortunate. He questioned Lescarbault's honesty. He said: 'Lescarbault contradicts himself in having first asserted that he saw the planet enter upon the sun's disc, and having afterwards admitted to Leverrier that it had been on the disc some seconds before he saw it, and that he had merely inferred the time of its entry from the rate of its motion afterwards. If this one assertion be fabricated, the whole may be so.' 'He considers these arguments to be strengthened,' says the 'North British Review,' 'by the assertion which, as we have seen, perplexed Leverrier himself, that if M. Lescarbault had actually seen a planet on the sun, he could not have kept it secret for nine months.'

This charge of dishonesty, unfortunate in itself, had the unfortunate effect of preventing Lescarbault or the Abbé Moigno from replying. The latter simply remarked that the accusation was of such a nature as to dispense him from any obligation to refute it. This was an error of judgment, I cannot but think, *if* an effective reply was really available.

The remarks with which the 'North British Reviewer' closes his account may be repeated now, so far as they relate to the force of the negative evidence, with tenfold effect. 'Since the first notice of the discovery in the beginning of January 1860 the sun has been anxiously observed by astronomers; and the limited area around him in which the planet *must be*, if he is not upon the sun, has doubtless been explored with equal care by telescopes of high power, and processes by which the sun's direct light has been excluded from the tube of the telescope as well as the eye of the observer, and yet no planet has been found. This fact would entitle us to conclude that no such planet exists if its existence had been merely conjectured, or if it had been deduced from any of the laws of planetary distance, or even if Leverrier or Adams had announced it as the probable result of planetary perturbations. If the finest telescopes cannot rediscover a planet which with the small power used by Lescarbault has a visible disc, within so limited an area of which the sun is the centre, or rather within a narrow belt of that circle, we should unhesitatingly declare that no such planet exists. But the question assumes a very different aspect when it involves moral considerations. If, proceeds the Reviewer, writing in August 1860, after the severe scrutiny which the sun and its vicinity will undergo before and after and during his total

eclipse in July, no planet shall be seen; and if no round black spot distinctly separable from the usual solar spots shall be seen on the solar spots' (*sic*, presumably solar disc was intended), 'we will not dare to say that it does not exist. We cannot doubt the honesty of M. Lescarbault, and we can hardly believe that he was mistaken. No solar spot, no floating scoria, could maintain in its passage over the sun a circular and uniform shape; and we are confident that no other hypothesis but that of an intra-mercurial planet can explain the phenomena seen and measured by M. Lescarbault, a man of high character, possessing excellent instruments, and in every way competent to use them well, and to describe clearly and correctly the results of his observations. Time, however, tries facts as well as speculations. The phenomena observed by the French astronomer may never be again seen, and the disturbance of Mercury which rendered it probable may be otherwise explained. Should this be the case, we must refer the round spot on the sun to some of those illusions of the eye or of the brain which have sometimes disturbed the tranquillity of science.'

The evidence which has accumulated against Vulcan in the interval since this was written is not negative only, but partly positive, as the following instance, which I take from my own narrative at the time in a weekly journal, serves to show:—After more than sixteen years of fruitless watching, astronomers learned last August that in the month of April Vulcan had been seen on the sun's disc in China. On April 4, it appeared, Herr Weber, an observer of considerable skill, stationed at Pecheli, had seen a small round spot on the sun, looking very much as a small planet might be expected to look. A few hours later he turned his telescope upon the sun, and lo! the spot had vanished, precisely as though the planet had passed away after the manner of planets in transit. He forwarded the news of his observation to Europe. The astronomer Wolf, well known for his sun-spot studies, carefully calculated the interval which had passed since Lescarbault saw Vulcan on March 26, 1859, and to his intense satisfaction was enabled to announce that this interval contained the calculated period of the planet an exact number of times. Leverrier at Paris received the announcement still more joyfully; while the Abbé Moigno, who gave Vulcan its name, and has always staunchly believed in the planet's existence, congratulated Lescarbault warmly upon this new view of the shamefaced Vulcan. Not one of those who already believed in the planet had the least doubt as to the reality of Weber's observations, and of these only Lescarbault himself received the news without pleasure. He, it seems, has never

forgiven the Germans for destroying his observatory and library during the invasion of France in 1870, and apparently would prefer that his planet should never be seen again rather than that a German astronomer should have seen it. But the joy of the rest and Lescarbault's sorrow were alike premature. It was found that the spot seen by Weber had not only been observed at the Madrid observatory, where careful watch is kept upon the sun, but had been photographed at Greenwich; and when the description of its appearance as seen in a powerful telescope at one station, and its picture as photographed by a fine telescope at the other, came to be examined, it was proved unmistakably that the spot was an ordinary sun-spot (not even quite round), which had after a few hours disappeared, as even larger sun-spots have been known to do in even a shorter time.

It is clear that, had not Weber's spot been fortunately seen at Madrid and photographed at Greenwich, his observation would have been added to the list of recorded apparitions of Vulcan in transit, for it fitted in perfectly with the theory of Vulcan's real existence. I think, indeed, for my own part, that the good fortune was Weber's. Had it so chanced that thick weather at Madrid and at Greenwich had destroyed the evidence actually obtained to show that what Weber described he really saw, though it was not what he thought, some of the more suspicious would have questioned whether, in the euphonious language of the 'North British Reviewer,' 'the round spot on the sun' was not due 'to one of those illusions of the eye or of the brain which have sometimes disturbed the tranquillity of science.' Of course no one acquainted with M. Weber's antecedents would imagine for a moment that he had invented the observation, even though the objective reality of his spot had not been established. But if a person who is entirely unknown states that he has seen Vulcan, there is antecedently some degree of probability in favour of the belief that the observation is as much a myth as the planet itself. Some observations of Vulcan have certainly been invented. I have received several letters purporting to describe observations of bodies in transit over the sun's face; either the rate of transit, the size of the body, or the path along which it was said to move being utterly inconsistent with the theory that it was an intra-mercurial planet, while yet (herein is the suspicious circumstance of such narratives) the epoch of transit accorded in the most remarkable manner with the period assigned to Vulcan. A paradoxist in America (of Louisville, Kentucky), who had invented a theory of the weather in which the planets, by their influence on the sun, were supposed to produce all weather-changes, the nearer planets being the most

effective, found his theory wanted Vulcan very much. Accordingly, he saw Vulcan crossing the sun's face in September, which, being half a year from March, is a month wherein, according to Lescarbault's observation, Vulcan may be seen in transit. By a strange coincidence, the interval between our paradoxist's observation and Lescarbault's exactly contained a certain number of times the period calculated by Leverrier for Vulcan. This was a noble achievement on the part of the paradoxist. It at once established his theory of the weather, and promised to ensure him text-book immortality as one of the observers of Vulcan. But unfortunately a student of science residing in St. Louis, after leaving the Louisville paradoxist full time to parade his discovery, heartlessly pointed out that an exact number of revolutions of Vulcan after Lescarbault's March observation must of necessity have brought that planet on that side of the sun on which the earth lies in March, so that to see Vulcan so placed on the sun's face in September was to see Vulcan through the sun—a very remarkable achievement indeed. The paradoxist was abashed, the reader imagines? Not in the least. The planet's period must have been wrongly calculated by Leverrier—that was all: the real period was less than half as long as Leverrier had supposed; and, instead of having gone a certain number of times round since Lescarbault had seen it, Vulcan had gone twice as many times round and half once round again. The circumstance that, if Vulcan's period had been thus short, the time of crossing the sun's face would have been much less than, according to Lescarbault's account, it actually was, had not occurred to the Louisville weather-prophet.[1]

Leverrier's faith in Vulcan, however, has remained unshaken. He has used all the observations of spots which, like Weber's, have been seen only for a short time. At least, he has used all which have not, like Weber's, been proved to be only transient sun-spots. Selecting those which fit in well with Lescarbault's observation, he has pointed out how remarkable it is that they show this accord. The possibility that some of them might be explicable as Weber's proved to be, and that some even may have been explicable, as completely but less satisfactorily, in another way, seems to have been thought scarce worth considering. Using the imperfect materials available, but with exquisite skill—as a Phidias might model an exquisite figure with materials that would presently crumble into dust—Leverrier came to the conclusion that Vulcan would cross the

[1] He had, indeed, at an earlier stage, shown a marvellous ignorance of astronomy by the remark, which doubtless appeared to him a safe one, that when he saw a planet on the sun in September he supposed it was Mercury; a September transit of Mercury being as impossible as an eclipse of the sun during the moon's third quarter.

sun's disc on or about March 22 last. 'He, therefore,' said Sir G. Airy, addressing the Astronomical Society, 'circulated a despatch among his friends, asking them carefully to observe the sun on March 22.' Sir G. Airy, humouring his honoured friend, sent telegrams to India, Australia, and New Zealand, requesting that observations might be made every two hours or oftener. Leverrier himself wrote to Santiago de Chili and other places; so that, including American and European observations, the sun could be watched all through the twenty-four hours on March 21, 22, and 23. 'Without saying positively that he believed or disbelieved in the existence of the planet,' proceeds the report, 'Sir G. Airy thought, since M. Leverrier was so confident, that the opportunity ought not to be neglected by anybody who professed to take an interest in the progress of planetary astronomy.'

It is perhaps unnecessary to add that observations were made as requested. Many photographs of the sun were also taken during the hours when Vulcan, if he exists at all, might be expected to cross the sun's face. But the 'planet of romance,' as the Abbé Moigno has called Vulcan, failed to appear, and the opinion I had expressed last October ('English Mechanic and World of Science' for October 27, p. 160), that Vulcan might perhaps better be called the 'planet of fiction,' was *pro tanto* confirmed. Nevertheless, I would not be understood to mean by the word 'fiction' aught savouring of fraud so far as Lescarbault is concerned—I prefer the North Briton's view of Lescarbault's spot, that, so to speak,

> 'Twas the blot upon his brain
> That *would* show itself without.

I have left small space to treat of other fancied discoveries among the orbs of heaven. Yet there are some which are not only interesting but instructive, as showing how even the most careful observers may be led astray. In this respect the mistakes into which observers of great and well-deserved eminence have been led are specially worthy of attention. With the description of three such mistakes, made by no less an astronomer than Sir W. Herschel, I shall bring this paper to a close.

When Sir W. Herschel examined the planet Uranus with his most powerful telescope, he saw the planet to all appearance girt about by two rings at right angles to one another. The illusion was so complete that Herschel for several years remained in the belief that the rings were real. They were, however, mere optical illusions, due to the imperfect defining qualities of the telescope with which he observed the planet. Later he wrote that 'the observations which tend to ascertain' (indicate?) 'the existence of rings not being satisfactorily supported, it will be proper that