

SCIENCE BYWAYS:

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A SERIES OF FAMILIAR DISSERTATIONS ON LIFE IN OTHER
WORLDS; COMETS AND THE SUN; THE NORTH POLE;
RAIN; DANGER FROM LIGHTNING; GROWTH
AND DECAY OF MIND; THE BRAIN AND
MENTAL FEATS; AUTOMATA; &c.

TO WHICH IS APPENDED AN ESSAY ENTITLED

MONEY FOR SCIENCE

BY

RICHARD Aⁿ PROCTOR,

AUTHOR OF 'SATURN,' 'THE SUN,' 'THE MOON,' 'THE UNIVERSE,' ETC.

Curiously I roam
As through a wide museum,—from whose stores
A casual rarity is singled out
And has its brief perusal, then gives way
To others, all supplanted in their turn.

WORDSWORTH.

WITH A PHOTOGRAPHIC PORTRAIT OF THE AUTHOR.

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THE PLANETS PUT IN LEVERRIER'S BALANCE.

LEVERRIER has recently completed the noblest work in pure astronomy which this age has seen. Five-and-thirty years ago he began to weigh the planets of the solar system in the balance of mathematical analysis. 'To-day,' said he, addressing the Academy of Sciences at Paris, on December 21 last, 'I have the honour to present a paper completing the *ensemble* of work the first piece of which goes back to September 16, 1839.' At that time he had only seven leading planets to deal with; it affords some idea of the nature of his work that the discovery of the eighth planet, Neptune, was a mere incident in the progress of his labours. Perplexed by peculiarities in the motion of one particular planet of the set he had undertaken to weigh, Leverrier quietly undertook to calculate the cause of those peculiarities, and so found Neptune. It was a matter of small account that another great mathematician almost simultaneously accomplished the same task. With Adams the discovery of the unknown planet was the ultimate object of inquiry; with Leverrier it was a mere step in a long series of investigations. To the outside world indeed it was the achievement of all others most deserving of notice in Leverrier's work, just as the discovery of Uranus by Sir W. Herschel attracted attention which labours altogether more important both in their nature and in their

results had failed to secure. But Leverrier himself can hardly have so regarded the discovery of Neptune. For him, its chief interest must have resided in the confirmation of his method of procedure afforded by the discovery of a planet through the careful study of perturbations due to that planet's attraction. Such confirmation was afforded at other steps of the work. In fact the whole series of Leverrier's labours affords perhaps the noblest illustration of the value of deduction guided by and suggesting observations since Newton's *Principia* first proved the superiority of that method over *mere induction*.¹

I propose to give such a sketch of Leverrier's method and results as would alone be suited to these pages. It need hardly be said, perhaps, that his work

¹ According to Bacon, science was to be advanced by making great collections of observations and classifying them—sorting and sifting until the grains of truth were winnowed out. No great discovery has ever been effected in this manner. The real use of observation and experiment has been found in their application to test the deductions from theories formed long before materials sufficient for Bacon's inductive method had been gathered. The question is one of fact. Theoretically, Bacon's method is perfect; it has hitherto failed in practice. Take any of the great discoveries of science, and it will be found that observations and experiments merely gathered together had no part in leading to the discovery; but that observations and experiments suggested by the deductions from theory were all-important. The moon might have been observed at Greenwich for all time without the observations leading to the discovery of gravitation. But Newton's deductions from the theory (when as yet the theory was but a guess) at once showed what observation might do; and it was by observation so made that the theory was established. In spectrum analysis a perfect heap of experiments had been collected without any useful results. Kirchhoff is led by a single observation to think of a theory, deduces certain consequences, tests these by three experiments, and the great discovery is to all intents and purposes effected.

is essentially mathematical—nay, his method, though not belonging to the very highest developments of modern mathematics, requires (even to be understood) a higher degree of mathematical skill than would be implied by mere familiarity with more recent methods in mathematics. Yet it is possible to exhibit the general principles and the results of Leverrier's work in a manner which everyone can understand.

In the solar system, we see first a mighty central ruler, whose mass so enormously exceeds that of all the planets taken together, that he is capable of swaying their motion without being himself disturbed. He is not indeed quite fixed. Whatever force he exerts on any planet, precisely that same force the planet exerts on him; but then he is so massive that the pull which compels the planet to circle around the sun scarcely displaces him at all. ‘If he pulls the planets,’ says Sir John Herschel, ‘they pull him and each other; but such family struggles affect him but little. *They amuse them,*’ he proceeds quaintly, ‘*but don't disturb him.* As all the gods in the ancient mythology hung dangling from and tugging at the golden chain which linked them to the throne of Jove, but without power to draw him from his seat, so, if all the planets were in one straight line and exerting their joint attractions, the sun—leaning a little back as it were to resist their force—would not be disturbed by a space equal to his own radius; and the fixed centre, or as an engineer would call it, the centre of gravity of our system, would still lie far within the sun's globe.’

To give clearness to our conceptions, let the mass of the sun be compared with that of all the other planets taken together. If we take the earth's mass as one thousand, then the mass of the eight chief planets of the solar system is represented by about four hundred and twenty-two thousand, and the sun's mass by three hundred and fifteen millions. Thus the sun's mass exceeds that of the whole system nearly seven hundred and fifty times; for in such a computation the combined mass of all such bodies as the asteroids, moons, meteors, &c., counts for nothing.

We see, then, that the movements of the eight planets must necessarily be determined in the main by the sun's attractive energy. What can even Jupiter, the mightiest of all the planets, do to disturb his giant neighbour Saturn from the path on which the sun, a giant so far mightier than either, would, by his attractive energy, compel the ringed planet to travel? The sun is more than a thousand times more massive than Jupiter, and though Jupiter when between the sun and Saturn is at but one-half the sun's distance, yet this nearness only quadruples the relatively small power of Jupiter, and leaves the sun's force on Saturn still two hundred and fifty times greater. Besides, Jupiter is only from time to time placed in this favourable position. Half the time he is even farther from Saturn than the sun is, and thus exerts less than a thousandth part of the sun's influence. And it need hardly be said that, if Jupiter is thus ineffective in disturbing a

neighbouring planet, every other planet is still weaker to disturb its neighbours. Our earth, for instance, with a mass barely equal to one three-hundred-and-fifteen-thousandth part of the sun's, has but small power to disturb her nearest neighbours, Mars and Venus, from that steady motion in their sun-ruled orbits which they would have if the earth did not exist. Venus is still weaker in disturbing the earth and Mercury, her neighbours; Mars weaker still; and Mercury weakest of all. Nor does the gradual diminution of the planetary distances as we draw nearer to the sun at all increase the relative disturbing power of the different planets. It might seem that the contrary should be the case. For instance, the other day, when Venus was in transit she was but about twenty-four millions of miles from us, and it might seem that Venus must then have disturbed the earth, and the earth Venus, very much more effectively (in proportion to their mass) than Jupiter can disturb Saturn or Saturn Jupiter, seeing that these planets never approach within three hundred and fifty millions of miles from each other. But in reality, the effect of proximity in such cases is counterbalanced by the much greater velocity with which the nearer planets travel. It would be easy to make an exact comparison, but the calculation would be unsuited to these pages. Let it suffice to say that throughout the whole of the solar system there is no disturbance greater than that resulting from the mutual attraction of Jupiter and Saturn; and how small this attraction is, com-

pared with the sun's influence on either planet, we have already seen.

The sun being thus placed as supreme ruler over the motions of the planets, their motions, starting from any given moment as a beginning, are in the main those due to solar influences. If, instead of being in the main so ruled, they were ruled *absolutely* by the sun, Leverrier's great work would have had no existence, as it would have had no utility. If the planets did not act upon each other by their attractive energies, any planet might be doubled or halved in mass, and all would go on unchanged. Nay, we might substitute for the eight chief planets as many peppercorns, and still the motions of these eight bodies would remain precisely the same. Calculated for one epoch, they would have been calculated for all time. No deviations would take place from which any inferences could be drawn as to the relative mass of the eight planets; but one continuous series of orbital circlings would go on, without change, for ever and ever.

But once recognise the fact that the planets disturb each other, and all this is changed. The more massive a planet is, the more potently will it disturb its neighbours. If we cannot tell exactly how much it does disturb its fellows, we can tell how large its mass is, compared with the earth's for example, which we may take as a convenient unit of reference. But it is clear that a planet's mass may be determined thus in many different ways. For instance, we may consider how much Venus disturbs the earth, and judge of Venus's

mass in that way ; or instead, we may consider how much Venus disturbs Mercury, her next neighbour on the other side, and infer her mass in that way. We might also perhaps have an opportunity of seeing how Venus affected some unlucky comet which passed near to her, and thus obtain yet another determination of her mass. If these estimates did not agree, we should know there was something wrong either in our observations or in our calculations. We should be set on the track of some error. And it has been in this manner that science has almost invariably been set on the track of important truths. If we hunted down the error successfully, we should probably be led, not merely to correct that particular mistake, but also to discover some fact before unsuspected.

It is precisely in this way that Leverrier has dealt with the planetary motions. Taking first the seven chief planets known when his labours began, he set himself to inquire into their motions. He found before long that the tables hitherto in use did not accord rigorously with observation. Now, if every discrepancy had had a single cause, it would then have been a work of no small labour to determine each such cause. But the great difficulty which the astronomer has to deal with in considering the planetary perturbations resides in the fact that multitudinous causes are in operation, the effects of which are intermingled. Watch the troubled surface of a storm-swept ocean, and notice how every wave differs from its fellows in one respect or another, usually in many. Suppose now that the

task were assigned of analysing the causes of these varieties of forms. How difficult would the task be to distinguish one effect from another, when so many were manifestly in operation. A sudden gust of wind blowing against the sloping side of a great wave may aid to heap up or to depress the mass of water which at the moment forms the wave, and thenceforth through many oscillations the effect of that accident will remain. A wave under observation may have been affected by many gusts, acting in various ways. Again, a wave may be increased or diminished by combining with a cross-wave belonging to another series than the first, and such causes of change may have operated over and over again. Peculiarities of the sea-bottom act to modify the shape and size of waves, and a wave observed in one place may have been affected by such peculiarities in regions many miles away from the observer's station. It will be seen, then, that though the observer might find it an easy task to give a general explanation of the sea-waves before him, he would have a task of enormous difficulty—in fact, an altogether hopeless task—if he were asked to ascertain from the varieties of form presented by the waves, the peculiarities of all the modes of disturbance operative in giving to the waves their actual forms. Somewhat similar, though not altogether hopeless, as will soon appear, is the task of the astronomer called upon to assign to their several causes, *not* the observed perturbations—that would correspond only to explaining the general nature of the wave-motion—but the peculiari-

ties recognised in these perturbations, the various ways in which these differ from what may be described as their normal character.

It need scarcely be said that the motions of the earth herself have to be considered in this inquiry. I do not mean merely the motion of the earth on her orbit round the sun, but the disturbances which affect that motion. The earth herself is riding on the waves of perturbation. Her movement on these waves must be as carefully considered as her motion in her course. For not merely will that movement indicate directly the nature of those waves which particularly affect herself, but also, unless that movement is taken into account, the earth-borne observer will form an incorrect estimate of the waves by which the other vessels in sight are perturbed.

To this work, then, of determining exactly the characteristics of the earth's motion round the sun, Leverrier from the very outset of his inquiry devoted close attention. It need hardly be said that the method of dealing with the question was to study very carefully the sun's apparent motion from day to day, for this motion precisely corresponds with the real motion of the earth. It will give some idea of the extent of Leverrier's field of research, though but a faint idea of the nature of his work therein, to mention that, in dealing only with this one part of his subject, he reviewed and discussed nine thousand distinct observations of the sun, made since Bradley's time at Greenwich, Paris, and Königsberg. The first result which

attracted his attention was rather an unsatisfactory one. It is commonly supposed that the observations of the sun at those three observatories, and especially at Greenwich, have been so exceedingly precise as to leave nothing to be desired on that score. Bessel, of Königsberg, was led to remark, many years since, with some degree of surprise, that the theory of the sun (or, which is the same thing, the theory of the earth's motion) had not made the progress which might have been expected from so many and such accurate observations. Leverrier's opinion, which must be accepted as final, owing to the enormous number of observations he has examined and his unsurpassed skill as a mathematician, is very different. 'Our conclusion is,' he says, 'that the observations of the sun leave much to be desired, on account of systematic errors affecting them; and there is no discordance between theory and observation which cannot be attributed to errors in observing.'

Yet Leverrier dealt so successfully with these observations, though thus imperfect, that he educed from them a noteworthy result. One class of disturbances affecting the earth's motion arises from the moon's disturbing influence. Its nature may be indicated by saying that in every lunar month the earth circuits around the common centre of gravity of her mass and the moon's. The diameter of this monthly orbit amounts to about six thousand miles, and as a result of this motion, she is about three thousand in advance of the centre of gravity just named when the moon is in her first quarter, and as far behind when the moon is in her third

quarter. Now it is that centre of gravity which alone follows the true orbit around the sun which is attributed to the earth herself in the books. The earth no more follows that orbit than the moon does. These two bodies dance round and round each other (if we may follow Sir John Herschel in using a rather homely illustration), while the pair are swung round the mighty mass of the sun. Of course this peculiarity of the earth's real motion is reflected in the sun's apparent motion. He seems at the time of the moon's first quarter to be in advance, and at the time of her third quarter to be behind, his mean place; just as if *he* were waltzing around in a monthly orbit six thousand miles in diameter, while being also swung round in his mighty annual path with its diameter of a hundred and eighty millions of miles. But it is clear that, if we can tell how large this apparent monthly orbit looks as seen from the earth, we shall know how far off the sun is. For the real size of this orbit is a matter depending only on the earth and moon, and can be inferred independently of the sun's distance. We know, then, how large the path really is; and if we know how much the sun seems displaced in traversing it, we have in fact learned how large a space of six thousand miles looks when removed to the sun's distance. This is equivalent to determining the sun's distance. Accordingly, Leverrier, having carefully estimated the sun's apparent monthly displacements, deduced thence an estimate of the distance of the sun, and confidently informed astronomers, sixteen years ago,

that their accepted estimate of the sun's distance was too large by between three and four millions of miles.

This was not the first great result which rewarded Leverrier, though we have set it first because it followed from the inquiry which formed in a sense the basis of his whole system of researches. The first noteworthy result of his labours was that mentioned at the beginning of this paper, the discovery that the system of seven great planets was incomplete, another body, as yet unseen and unknown, travelling beyond the path of Uranus, and by its attraction disturbing the movements of that planet, for sixty years regarded as the remotest member of the sun's family.

And here, as in the case of the discovery of Uranus by Sir W. Herschel, good fortune as well as mathematical insight came into play. Herschel discovered Uranus by a lucky accident, when engaged in far other work than the search for new members of the solar family. Leverrier was not quite so lucky. He deliberately cast a line into space, hoping to capture the unknown disturber of Uranus. He satisfied himself by the most careful analysis of all available observations that Uranus really is disturbed by an unknown body (and, in passing, we may remark that in this respect Leverrier's work differed from that of Adams, who assumed this particular point). How then, it may be asked, was fortune concerned? I may illustrate the matter by the waves which we have already found convenient for such purposes. Suppose that an observer engaged in analysing a series of wave-disturbances

travelling (say) along a canal, observed some new class of effects, as, for instance, that certain waves which had long been of a particular size began to grow larger. Suppose, that, struck by this, he instituted a careful series of measurements of their size, and at last satisfied himself that they had increased. He might be utterly at a loss to conjecture a cause. But if even he conjectured a cause, as, for instance, some disturbance taking place at a part of the canal out of his sight, he might still find it impossible to conjecture how far off that part might be. If, however, while he had satisfied himself by his wave-measurements that the waves really had increased in size, he had also satisfied himself that during his observations the increase had reached its full extent, and had even begun to give place to a slow decrease, tending to restore the original size of the waves, he would manifestly have here an indication which might serve to tell him of the very spot where the disturbance had taken place. For example, the rate at which the waves were travelling, combined with the time elapsed since the peculiarity had been noticed, might indicate exactly how many miles away was the scene of the disturbance. Now something of this kind had happened in the case of Neptune. When astronomers were thoroughly convinced that Uranus had been perturbed, or, in effect, when Leverrier had completed his analysis (surpassing all others in completeness) of the planet's observed motions, it had also become known that the displacement had reached its maximum, and

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was beginning slowly to decrease. This showed astronomers that the disturbing planet had made its nearest approach to Uranus, and was now slowly drawing away. Nor let the reader wonder that this was a process requiring years to produce perceptible effects. For Uranus himself moves so slowly that he only completes his circuit in 84 years, and Neptune (we now know) requires more than 164 years; so that they come sluggishly into conjunction and pass sluggishly out of conjunction.¹ Only when Adams and Leverrier began to angle for the unknown planet had it become quite certain that that body had been lately in conjunction with Uranus. If these astronomers had not known when this happened within a few years either way, it would have been utterly useless for them to have sought for Neptune by mathematically analysing the disturbance affecting the movements of Uranus. Their good fortune consisted in this, that the conjunction had opportunely occurred just when the motions of Uranus were sufficiently observed to satisfy astronomers that there was an external planet.²

¹ That is, they pass slowly into and away from the position in which the sun, Uranus, and Neptune are nearly in a straight line.

² The general public, while underrating the mathematical difficulties which Adams and Leverrier had to encounter, altogether overrated the actual extent of the field over which Neptune had to be searched for. It was tolerably certain already that Uranus and Neptune had been in conjunction between 1820 and 1825. Between 1841 and 1846 then (i.e in 21 years) Uranus would have gone round a fourth of the ecliptic as viewed from the sun; and the unknown planet probably about half as far. Neptune, then, was to be looked for near the ecliptic, and about one-eighth of its circuit *behind* Uranus (both being supposed to be viewed from the sun, which, in the case of planets so distant, is much

Setting, however, this piece of good fortune aside, which rendered their labours possible, the actual nature of the work of Adams and Leverrier was sufficiently arduous. And though their hypothetical Neptunes moved quite differently from each other, and departed still more widely from the path of the real Neptune, yet under the actual conditions, both astronomers were led, as we know, to point to a place very near to that occupied by the real Neptune at that particular time. It was as though, in the illustrative case just imagined, the observer had made some error in estimating the rate at which the wave-disturbance had travelled down the canal to his place, but yet guessed very nearly the true spot where it arose, because the time it had taken was but short: for instance, if the calculated rate were too great by half a mile per hour, but the time occupied were only twenty minutes, then he would only be in error by the sixth part of a mile. But if the time were, say, ten or twelve hours, then the error would be five or six miles. So Leverrier and Adams had their hypothetical Neptunes travelling too slowly by a quite appreciable amount: but yet, owing to the shortness of the time which had elapsed since Neptune and Uranus were in conjunction, the resulting error was very small; and, as we know, the planet was found at the first cast of the telescopic line.

the same as viewing them from the earth). It was, in fact, tolerably certain before Adams and Leverrier began their calculations, that the unknown planet occupied a position somewhere on a known strip of the heavens not more than ten or twelve degrees long by about three degrees broad.

In passing to the next result of Leverrier's researches, we have to turn from the outermost planets of the solar system to Mercury, the one that, so far as is as yet known, travels nearest to the sun. The motions of Mercury have been determined with a great degree of accuracy, because Mercury often passes across the face of the sun, and can at those times be observed very exactly. Now it was found that the observed movements of this planet did not accord with those calculated. 'This result,' says Leverrier, quaintly enough, 'naturally filled us with inquietude. Had we not allowed some error in the theory to escape us? New researches in which every circumstance was taken into account by different methods, ended only in the conclusion that the theory was correct, but that it did not agree with the observations. Long years passed, and it was only in 1859 that we succeeded in unravelling the cause of the peculiarities recognised. We found that they were all included under a simple law, and that—a certain slight change only was needed to bring everything into order. The nature of this change was such as to indicate 'the existence of cosmical matter, as yet unknown, circulating like the planets around the sun. The consequence,' proceeds Leverrier, 'is very clear. There exists in the neighbourhood of Mercury, doubtless between that planet and the sun some matter as yet undiscovered. Does it consist of one or more small planets, or other more minute asteroids, or even of cosmical dust?'¹ The

¹ I follow in general a translation of Leverrier's paper in the

theory tells us nothing on this point. On numerous occasions trustworthy observers have declared that they have witnessed the passage of a small planet over the sun; but nothing has been established in this matter. We cannot, however, doubt the exactness of this conclusion.'

Such are Leverrier's latest utterances on this interesting question. He takes no notice, on the one hand, of the discoveries recently effected in meteoric astronomy, which demonstrate the existence of at least some matter in the sun's neighbourhood; nor, on the other, of the objections raised by Sir W. Thomson and others to the theory that large quantities of meteoric matter travel close by the sun. Nor does he speak of the singular statements made by the French doctor, Lescarbault, and once to some degree sanctioned by Leverrier himself, respecting the transit of a small black disc across the face of the sun on March 26, in the very year 1859, when Leverrier first laid his results respecting Mercury before the scientific world. We venture to quote Leverrier's account of his visit to Lescarbault's small observatory, as abridged from the *North British Review* for August 1860, in Chambers's useful treatise, 'Descriptive Astronomy.' It is well worthy of examination, whether it be regarded as evidence for the new planet—so confidently believed

'Monthly Notices of the Astronomical Society,' not having by me the original; but verbal changes have been made, the translation being, to say the truth, in very singular language. Leverrier, for instance, is made to say that 'a matter exists in the sun's neighbourhood,' and to ask if it 'consists in cosmic dust.'

in once, that astronomers assigned a name to it, calling it, appropriately enough, *Vulcan*—or as showing the circumstantial way in which incorrect statements are sometimes advanced :—

‘On calling at the residence of the modest and unobtrusive medical practitioner, Leverrier refused to say who he was, but in the most abrupt manner, and in the most authoritative tone, began, “It is then you, sir, who pretend to have observed a new 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.” The doctor then explained what he had witnessed and entered into all the particulars regarding his discovery. On speaking of the rough method adopted to ascertain the period of the first contact, the astronomer inquired what chronometer he had been guided by, and was naturally enough somewhat surprised when the physician pulled out a huge old watch with only minute hands. It had been his faithful companion in his professional journeys, he said ; but that would hardly be considered a satisfactory qualification for performing so delicate an experiment. The consequence was that Leverrier, now beginning to conclude that the whole affair was an imposition or a delusion, exclaimed with some warmth, “What ! with that old watch showing only minutes, dare you talk of estimating seconds ? My

suspicious are already too well founded." To this Lescarbault replied that he had a pendulum by which he counted seconds. This was produced, and found to consist of an ivory ball attached to a silken thread, which being hung on a nail in the wall is made to oscillate and is shown by the watch to beat very nearly seconds. Leverrier is now puzzled to know how the number of seconds is ascertained, as there is nothing to mark them; but Lescarbault states that with him there is no difficulty whatever in this, as he is accustomed "to feel pulses and count their pulsations," and can with ease carry out the same principle with the pendulum. The telescope is next inspected, and pronounced satisfactory. The astronomer then asks for the original memorandum, which after some searching is found "covered with grease and laudanum." There is a mistake of four minutes in it when compared with the doctor's letter, detecting which the *savant* declares that the observation has been falsified. An error in the watch (regulated by sidereal time) accounts for this. Leverrier now wishes to know how the doctor managed to regulate his watch by sidereal time, and is shown the small telescope by which it is accomplished. Other questions are asked and satisfactorily answered. The doctor's rough drafts of attempts to ascertain the distance of the planet from the sun, from the period of four hours which is required to describe an entire diameter of that luminary, are produced chalked on a board. Lescarbault's method, he being short of paper, was to make his

calculations on a plank and make way for fresh ones by planing them off. Not being a mathematician, it may be remarked that he had not succeeded in ascertaining the distance of the planet from the sun. The end of it all was that Leverrier became perfectly satisfied that an intra-Mercurial planet had been really observed. He congratulated the medical practitioner upon his discovery, and left with the intention of making the facts thus obtained the subject of fresh calculations.'

This, however, was not the actual end of the matter ; for news came from an astronomer in Brazil, M. Liais, that at the very time during which Lescarbault said he watched the black spot crossing the face of the sun, he (Liais) was observing the sun, and nothing of the kind could be seen, though he was employing a telescope much more powerful than the one used by the French physician. It has also been pointed out that any planet nearer to the sun than Mercury ought to be a conspicuous object during total eclipse of the sun, whereas no such object has ever been noticed. On the whole it seems very doubtful how far the records of supposed transits can be trusted, and we seem almost compelled to adopt the opinion that the meteoric and cometic matter undoubtedly existing in the sun's neighbourhood in enormous quantities, produces the observed peculiarities in the motion of Mercury. In this case the united mass of all the meteoric matter within the orbit of Venus (not of Mercury, for Leverrier's result admits of explanation

by matter lying anywhere within about twice Mercury's distance from the sun) amounts, according to Leverrier's original estimate, to about a tenth part of the mass of Venus, or exceeds the mass of Mercury himself. This is not inconsistent with an exceeding tenuity of material. If the matter consists of small solid or liquid bodies, the sparseness of distribution would be very great. Suppose, for example, these bodies were of the same density as water; then together they would make a globe having about half the volume of the earth. Now, if they were scattered over a flat region shaped like a grindstone, extending all round the sun to Venus's distance, and having a thickness equal to the earth's diameter, this region would exceed the total volume of the scattered meteors no less than four hundred and thirty-five millions of times. So that, on the average, each meteor would have (wherein to disport itself free from contact or collision) a space exceeding its own volume to this degree. A meteor, for example, one cubic inch in volume, would have on the average a space equal in volume to a cube twenty-one yards in length and breadth and height. But the actual space occupied by meteors within the orbit of Venus is far greater, seeing that near the sun it has a thickness (so to speak of this disc-shaped region) of many millions of miles. Supposing the matter occupying this space to be a uniform gas, it would certainly be one hundred thousand million times rarer than water, or much more than a thousand million times rarer than air.

But it will presently appear that since Leverrier made that estimate of the mass of the disturbing matter, the estimate of our earth's mass, relatively to the sun's, has been increased by at least one-tenth part; and this would leave a much smaller quantity of matter to be provided by meteoric systems. There remains, however, sufficient evidence to show that the total mass of matter within the orbit of Mercury amounts, in all probability, to thousands of millions of tons.

I may remark here on an objection which has been urged by Sir E. Beckett (then Mr. Denison) in his fine work *Astronomy without Mathematics*, to the theory that vast quantities of meteoric matter in the sun's neighbourhood supply, as it were, the fuel or part of the fuel, by which the sun's fires are maintained. He showed that the quantity of matter necessary to produce this effect would be such that the sun would grow annually by a quantity equal to more than a twelve-millionth part (he gives exact numbers) of the sun's actual mass; and he proceeds to show that the effect of this would be to shorten the year by nearly one twenty-five millionth part of its length—that is, by about four seconds in three years. This would make our year shorter by about forty-seven minutes than the year in the time of Hipparchus, and we know quite certainly that there has not been a change even of half as many seconds. He proceeds then to touch on an objection to this reasoning, in the following words:—‘If the meteors were all, before

their absorption within the earth's orbit, forming a sort of spherical extension of the sun, it is true that their joint attraction on the earth would be the same as after they had fallen into the sun. But I have seen no suggestion that this is so, and many meteor systems, especially the two largest that we know of, have orbits extending far beyond the earth's.'

This particular objection, or rather this reply to the original objection, had been advanced by me some years ago. Sir E. Beckett's answer does not seem to meet the argument. For all the meteor systems we can possibly become acquainted with (as such) are those encountered by the earth, and these form so minute a proportion of the total number (on any reasonable assumption of the probabilities) that it would be unsafe to reason from them. In fact, if we could, we might at once dismiss the meteoric theory of the sun's heat, because the two meteor-systems referred to by Sir E. Beckett do not pass within many millions of miles of the sun's surface. All the evidence we have, as I have elsewhere shown, indicates an increase in the density of meteoric distribution as we approach the sun, this increase becoming exceedingly rapid in the sun's immediate neighbourhood. Nor does it in the least matter that a certain proportion of the meteors thus crowded near the sun at any moment are in reality moving in paths carrying them far away from the sun. So long as the movements of the complete system are such that the gathering near the sun is permanent, though the mem-

bers composing it may be continually changing, the consequences would be the same, or so nearly the same as to make no appreciable difference in the observed effects.

But there is in the very results on which the meteoric theory had been based—I mean Leverrier's recognition of the existence of intra-Mercurial matter—the strongest evidence that the sun's heat cannot possibly be due entirely or chiefly to meteoric impact. The quantity of downfalling matter necessary to maintain the sun's heat would be equal to about a fortieth part of the earth's mass annually. Now Leverrier's balance will not allow more than four times this amount for the whole quantity of meteoric matter within the orbit of Venus,—granting, that is, to the region of greatest meteoric condensation the widest permissible extension. So that there is only sufficient matter to last for four years, if meteoric downfall were the sole source of the sun's heat and the meteors were to be continually used up for that purpose. Four times four years have passed since Leverrier first published his results, and neither has the sun grown cold, nor the supply of meteoric matter perceptibly diminished.

Let us next turn to the results obtained by Leverrier when he put the planet Venus in the delicate balance of analysis. Here we come again upon evidence respecting the sun's distance, the theory of Venus leading, like the theory of the sun, to the conclusion that the sun's distance had been over-estimated by three or

four millions of miles. But an interesting confirmation of the accuracy of Leverrier's theory of Venus is the point to which I would chiefly invite the reader's attention. Of course, on the occasion of the late transit, much depended on the accurate calculation of the time when Venus would cross the edge of the sun. The results satisfactorily proved the accuracy of the calculations. For instance, Mr. Hind found that, using the old tables of the sun and Venus, the calculated time of egress at Mokattam in Egypt differed by $13\frac{1}{2}$ minutes from the observed time; whereas when Leverrier's new tables were used the calculated time was only five seconds in error. This is very satisfactory evidence of the value of Leverrier's labours.

We come, finally, to Mars, for the planets Jupiter and Saturn follow exactly the motions which theory ascribes to them.

One of the most interesting points, as it seems to me, in Leverrier's discussion of the motions of Mars is the fact that it indicates the wonderful power of mathematical analysis in dealing with matter, apart from all direct evidence as to the existence of such matter. Suppose no telescopic search had been made for the planet which astronomers of old time supposed to be travelling between the paths of Mars and Jupiter. Leverrier's analysis of the motions of Mars would in that case afford evidence decisive of the question whether a large but as yet undetected planet is really travelling in that region or not: It shows that there can be no such planet, simply because Mars shows no

traces of the disturbing influence of any considerable planet. But Mars does show the influence of disturbing matter, not giving him a strong pull in this direction at one time and in that direction at another, as a single planet would, but exerting a more equally distributed action. This is the influence of the zone of asteroids, and in this action we have a means of weighing that zone.

But here, unfortunately, a difficulty arises. Leverrier long since pointed out that the peculiar form of disturbance thus affecting Mars might be explained either by ascribing to the whole family of asteroids, when taken together, a weight equal to one-eighth of the earth's, or else by adding so much to the estimate of the earth's weight. This last result corresponds almost exactly with the effect of increasing the estimate of the sun's distance to the degree indicated by Leverrier's other researches. Some of our text-books, with their usual happy freedom of manner, combine these two results (stated by Leverrier in 1861), and assign to the asteroids a total mass equal to one-eighth part of the earth's, while *also* asserting that Leverrier's researches on Mars, like those on Venus, proved that the earth's mass must be increased by an eighth. But we cannot assign the observed effects fully to both causes at once, though we may assign part of the observed effects to one cause and part to the other. Leverrier himself does not, indeed, mention this. His words are as follows:—‘Only two hypotheses were possible, as we explained on June 3, 1861; either the hitherto neg-

lected matter resided in the totality of the ring of small planets, or else it must be added to the earth itself. In the second case, and as a consequence, the distance of the sun must be diminished by about a twenty-fourth part of the' (then) 'received value—that is, we are led to the result already obtained from the theories of the sun and Venus.' But then, if we ascribe the whole effect to the original erroneous estimate of the sun's distance, we are left in this predicament—that we can assign *no mass at all* to the whole family of asteroids.

Here, then, as in the case of Mercury, we see that we have to wait till the sun's distance is determined with much more exactness than heretofore, before we can ascertain the real results of Leverrier's planet-weighing. He has put these planets severally in the balance, and noted the result; but the balance itself has to be inquired into before we know what the result means. It can hardly be doubted that the transit observations made last December will come in very usefully at this point. We shall learn from them how much must be added to the old estimate of the earth's weight (or, which is exactly the same thing, how much must be taken from the old estimate of the sun's weight), and therefore we shall know how much is left, on the one hand, for intra-Mercurial matter, and, on the other, for the asteroidal family.

Now, it is somewhat strange that this being so,—Leverrier's own results pointing to the importance of

the direct measurement of the sun's distance by transit observations, or in any other available manner,—he has nevertheless spoken quite disdainfully of those direct modes of measurement. Because in weighing the planets in his analytical balance, poised and adjusted with marvellous skill, he has found clear evidence that the old measurements of the sun's distance were erroneous, he deprecates new measurements. ‘Here I have,’ he says in effect, ‘a way of testing such measurements so delicate that in itself it is preferable to them all. The balance I have used is one which will improve with advancing years, and as, in 1861, it had detected the error in measurements of the sun's distance effected in 1769, so, long before the transits of the twenty-first century, it will have given results altogether more accurate than those you are attaining at so much expense by observing the transits of the present century.’ This is all very well; but Leverrier’s own results leave something to be explained which these despised transit observations are competent to explain at least a good deal more accurately than he has himself explained them. His method, carefully kept in bottle for another half-century, may, and probably will, give us a much clearer wine (to use Bacon’s simile), but in the meantime we must be content with the vintage of 1874 and 1882.

But this in no sense affects the value of Leverrier’s own labours. Beyond question he has deduced from the observed motions of the planets all that at present can be deduced as to the masses of the different known

and unknown parts of that complex system,—containing bodies of all orders of size, density, and structure,—which occupies the domain of space ruled over by the sun. We spoke of his work, begun more than a third of a century ago, as the noblest work in pure astronomy which this age has seen. This certainly seems no exaggerated estimate of its value. A portion only of the work—that which led to the discovery of Neptune—has been called the greatest achievement of mathematical astronomy since Newton's discovery of the law of gravitation. As regards this portion of his labours, his credit is shared by another astronomer not less skilful than Leverrier, though circumstances have prevented him from pursuing his course along the difficult path for which his powers fit him. Other astronomers, again, have shared with Leverrier the labour of analysing the movements of particular planets, or rather have gone over the same ground with somewhat similar results. But as Sir John Herschel alone of all astronomers ever surveyed with high telescopic powers the whole of the star-lit sphere surrounding our earthly home, so Leverrier alone has submitted to the searching scrutiny of the higher mathematical analysis the whole of that complicated system to which the earth belongs. It adds not a little to the credit due to him for these achievements that during the greater part of his labours he held a high official post, the duties of which (had he been content to follow an example but too common) might well have exonerated

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him from the continuance of independent labours so arduous and exacting.

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COMETS' TAILS.

WHEN we consider the surprising nature of the phenomena presented by the tails of comets, we can scarcely wonder that the most startling theories have been suggested in explanation. Their whole behaviour is anomalous. The head of a comet, or rather the bright almost point-like nucleus, obeys the law of gravity ; and wonderful though the nature of the comet's orbit sometimes is, extending into depths so remote that the mind shrinks from pursuing the comet on its journey through them, there is not a mile of the comet's voyage which does not exemplify in the exactest manner the laws recognised by Newton. But it is quite otherwise with the tail, if we regard the tail as a material object carried along with the comet. The end of the tail, for example, shifts through space with a velocity such as the sun could not possibly generate by his attractive influence, mighty though that influence is, nor control if otherwise generated. Cometic tails are flung forth from the head, or at least appear to be flung forth, with a rapidity far exceeding even the tremendous velocity with which a comet, passing near the sun, sweeps round that orb at the time of nearest approach. Then