The Discovery of the Planet Neptune*

Was It Chance or the Result of Scientific Plan?

By David Rines

I. THE PUZZLING MOTION OF URANUS.

THE annals of astronomy can boast of few events so wrought with dramatic character as the discovery of Neptune. The discovery of a celestial body is in itself a matter of no rare occurrence; but Neptune did not follow the common-place, prosaic methods of straying into the field of view of the telescope, or of unwittingly leaving a tell-tale impression upon the photographic plate. One of the foremost mathematical astronomers of his time, after a train of brilliant analysis and laborious calculation extending over a period of more than a year, directed the attention of astronomers many miles away to a region of the sky where, he asserted, would be found a new planet. Toward that region of the sky was pointed a telescope, and there, true enough, hung an additional member of the solar system. To intensify the situation, hardly had the world recovered from her thrill of astonishment, when the honors lavished upon the wonderful man who had thus discovered a planet without so much as placing an eye to the telescope were suddenly laid claim to in behalf of a youth until then unknown. To make the episode still more extraordinary, men of science had just begun to regain their composure when they were again startled by the leading mathematician of the western hemisphere darting forward boldly to challenge the claims of both men. The scientific world became bewildered.

The planet Uranus was the cause of the turmoil. Discovered in 1781 by Sir William Herschel, it became at once the object of much attention, solicitude and care on the part of astronomers. Telescopes were turned upon it nightly, its physical appearance was carefully watched, and its motion and position from night to night carefully recorded.

Within a few months of the planet's discovery, attempts were made to reach a rough determination of its orbit. This, for the time being, was assumed to be circular. As the path of a planet, however, although nearly a circle, is really an ellipse, the computations served merely as temporary approximations, awaiting the accumulation of a sufficient number of observations for the computation of the true orbit. The next two years brought forth from various quarters accurate elliptic elements.

Buoyed up by the hope that the planet, before discovery by Herschel, might have been observed as a fixed star, astronomers searched through the records of their predecessors, and were delighted to find their labors rewarded with the discovery of some twenty odd observations by Flamsteed, Mayer, Bradley and Lemonnier, extending over the period from 1690 to 1771.

Nine years elapsed, and observations were accumulating. The Academy of Sciences of Paris, deeming the data by this time sufficient in character and number for a fairly good determination of an orbit, proposed the theory of the planet's motion as the subject for a prize. Delambre carried off the prize as reward for a skillful mathematical treatment by which he deduced the elements of the planet's orbit and, with the help of these data, constructed tables for the planet's motion.

The utility of Delambre's work, however, was short-lived. The planet, though for a few years it faithfully conformed to the tables, soon disregarded the vast labors which Delambre had expended upon the computation of its orbit, and began to pursue a course totally different. In view of the fact that the tables were, after all, a crude attempt, because based upon comparatively few observations and uncertain data, their failure to predict accurately the motion of the planet did not cause surprise.

The computation of an orbit, it must be remembered, is quite complex. In accordance with the universal Law of Gravitation, a planet is influenced not only by the sun, but also by the other planets; and it is pulled by these planets considerably away from the theoretical ellipse determined by the action of the sun alone. The deviations—known technically as "perturbations"—thus introduced into the planet's path enormously complicate a problem otherwise comparatively simple; for not only is there necessary a knowledge of the sun's action, but also of the perturbations introduced by every planet large enough and close enough to exert appreciable influence.

As time went on, and observations continued to accumulate, and more nearly perfect information was obtained of the perturbing planets, and better theory was established, men of science began to feel that the time had arrived for a new discussion of the motion of Uranus. In the second decade of the nineteenth century, accordingly, Alexis Bouvard attempted to construct a new set of tables.

He was much trouble over his inability to reconcile the ancient observations—that is, those made of the planet before the discovery of its planetary nature—with those of more recent date. The ancient observations, he found, could perfectly well be represented by points on an ellipse; so, too, could the observations of more recent date; but, strange to say, the two ellipses were not identical. Despairing finally of reconciling the two sets of observations, Bouvard, reluctantly contenting himself with the explanation that the older observations were less precise and therefore untrustworthy, based his tables exclusively on the newer data.

For a few years the planet revolved in the orbit determined for it by the new tables. Soon, however, it began, as on the previous occasion, to assert its independence. Before very long, therefore, Bouvard's Tables became as unreliable as those of Delambre.

The inexplicable wanderings of the planet, combined with Bouvard's unsuccessful attempt to reconcile the old with the new observations, could not fail to produce a profound impression upon the minds of astronomers. The accuracy of the computations was now almost certain. The trouble was not with the tables, but with the planet.

One more attempt was made to harmonize the theoretical with the actual motion of Uranus, but with no better result. A quarter of a century after the completion of Bouvard's Tables, his nephew, Eugene, upon undertaking a similar task, found as much difficulty in reconciling the new observations which had by this time accumulated, with those of his uncle's time, as the uncle himself had experienced when attempting to harmonize the observations of his day with those of Flamsteed's.

This last laborious but unsuccessful attempt to account for the motion of Uranus created dismay. LeVerrier, at the suggestion of his friend, Arago, the astronomical chief of France, dropped for the time his researches on comets to undertake the solution of the mystery.

II. AN UNDISCOVERED PLANET.

No one could have been found better qualified to undertake this task than Urbain Jean Joseph LeVerrier. The high order of analysis displayed in his mathematical astronomical researches bore witness to his worth as a practical mathematician, and as a skilled computer he stood unexcelled. Though but thirty-one years of age, he had already attained a place among the foremost of contemporary mathematical astronomers.

To assure himself that the deviations of Uranus from its computed path were not merely apparent, but actual, LeVerrier determined first on a thorough investigation into the accuracy of Bouvard's Tables. He consequently revised the theory and, after his own fashion, checked the computations. These lengthy preliminary labors served a purpose in that they allayed the suspicions of astronomers that the mystery of Uranus lay concealed in imperfections of Bouvard's Tables. They proved instead that the theory was at fault. But one conclusion was possible. Uranus moved in an orbit different from that determined for it by the sun and the known planets.

Arrived now at the stage where he was compelled to bring to his aid the hypothesis of an unknown force acting upon the wandering planet, LeVerrier began examining in detail the speculations of men of science regarding the nature of this force. One suggestion was that the Law of Gravitation, as enunciated by Newton, was not quite accurate; and that at a distance from the sun as great as that of Uranus, the consequent error introduced on the assumption that Newton's Law held good might become appreciable. A second hypothesis for the failure of the tables was that in their construction no account had been taken of the retarding influence of a rare ether, which those who held this view conceived to be diffused throughout space. Another conjecture was that Uranus had possibly been dragged out of its orbit by the perturbative effect of, or collision with, a passing comet; and still another explanation was that the planet was perhaps influenced by a large undiscovered satellite. Not a few astronomers hinted that Uranus was swerved from its computed path by the disturbing action of an unknown planet. Of the various hypotheses thus tentatively put forward, that of an unknown planet seemed to LeVerrier to be the most plausible; and he accordingly set himself the task of determining where in the heavens this planet might be found.

The task was not easy. The problem before him, known as the inverse problem of perturbations, was new; and though it can not be said to rank among the most difficult ever proposed, it was sufficiently difficult to require the skill of a master in astronomical researches.

It is not our purpose here to delve into mathematical intricacies; yet, in order to make the sequel more intelligible, we should not omit to mention one of the obstacles presented by the problem. The problem seemed impossible of solution without a previous knowledge of the distance from the sun of the unknown planet. In the absence of this knowledge it was necessary to guess a value for this distance. Should the guess, in the course of the solution, prove too large or too small, it would serve to suggest an improved value with which to solve the problem anew. An intelligent guess, therefore, would result in considerable saving of labor.

It had long before been discovered that the distances of the known planets harmonized with an empirical formula known as Bode's Law. It seemed reasonable to suppose that the unknown planet, too, would follow this law. If so, then the planet should be twice as far removed from the sun as Uranus. LeVerrier consequently decided, as a first approximation, upon a value for the unknown distance twice that of Uranus.

Many were the difficulties to be overcome, many the obstacles to be surmounted, many the pitfalls to be avoided. At one stage of the solution, LeVerrier became so discouraged that he did not advance a step in his researches for three whole months. Eventually, however, he solved the problem. The mystery of Uranus seemed at last explained. Its movements could be accounted for by the perturbations of a theoretical planet.

It is customary with investigators, in order that they may establish their title to priority, to publish news of their researches, either through the medium of the scientific journals, or by addresses before scientific societies. In accordance with this custom, LeVerrier on three different occasions reported his progress before the French Academy of Sciences. On November 10th, 1845, he presented that body with an account of his preliminary labors. On June 1st, 1846, he announced the results of a first approximation. And now, on August 31st, he presented the final results. On this occasion, he pressed upon astronomers the task of seeking for the planet with their telescopes. The further to encourage them, he declared that the object of their search could easily be distinguished, in a good instrument, from neighboring stars of the eighth or ninth magnitude, by the fact that, like other planets, it possessed a sensible disk.

It is regrettable to record the exceedingly slight interest aroused in men of science by researches so extraordinary. For one reason or another, whether because of the enormous difficulties entailed in the task, the lack of necessary appliances, or their incredulity, astronomers were backward in undertaking the search. LeVerrier became almost frantic in his appeals. At length his exhortations fell upon willing ears. In acknowledging the receipt of a scientific paper from Dr. J. G. Galle of the Berlin Observatory, he seized the opportunity to urge that the Berlin astronomers employ their telescope in a search for the planet. Galle, nothing loth, immediately set about looking for a chart of the heavens to aid him in the work.

Twenty years previously there had been inaugurated the construction of a series of star maps detailing specified portions of the heavens. The map of that region containing the position of LeVerrier's planet had been completed by Dr. Bremiker late in the year 1845, but it had not yet been distributed. Astronomers consequently were unaware of its existence. At the suggestion of a young student, d'Arrest—the famous astronomer of later years—Galle sought for and obtained possession of the map. On the evening of September 23rd, 1846, the very day on which he received LeVerrier's letter, he seated himself at the telescope, and called out the configurations of the stars in the field of view, as d'Arrest checked them off on the map. Close to the planet's theoretical position the observer found a star of the eighth magnitude which the assistant could not find depicted on the chart. Joy reigned! The object proved to be the planet.

The enthusiasm which seized upon scientific men, following the announcement that the planet had been actually detected with the telescope, stands out in marked contrast to the indifference and lack of interest which had prevailed previously. The whole world thrilled with wonder and amazement. Astronomers became lost in a frenzy of admiration and delight, and eagerly hastened to turn their telescopes upon the newly discovered inhabitant of the heavens. Not scientists alone, but men of all classes joined in paying tribute to the genius of LeVerrier. Everywhere the discovery was termed the most brilliant in the annals of astronomy, and it was rated the crowning achievement of the human intellect.

Suddenly, from across the English Channel came

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strange tidings. To the honor of discovery had appeared a rival claimant.

III. JOHN COUCH ADAMS.

John Couch Adams displayed while quite young a strong love for mathematics and astronomy. At ten years of age he had begun the study of algebra, as a boy he found pleasure in reading astronomical and mathematical works, and in his youth he derived untold delight from watching the heavenly bodies, computing their motions, and recording their positions with instruments of his own device. Though born of humble parents, his talents obtained early recognition, and it was accordingly decided to send him to college. Entering St. Johns College, Cambridge, at the age of twenty, he assumed at the start, and maintained to the end, the highest rank in his studies, graduating as first Smith's Prizeman and Senior Wrangler.

It was during the second year of his undergraduate career that chance directed to his hands a copy of Airy's report to the British Association on the progress of astronomy during the early years of the nineteenth century. In this report was mentioned the puzzling motion of Uranus, but without the least attempt to suggest an explanation. Deeply impressed, the young student seized in a flash of genius upon the true cause of the phenomenon, and fearful lest other matter should drive the subject from his mind, he was inspired to record the following memorandum:

"1841, July 3rd. Formed a design, in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus, which are yet unaccounted for; in order to find whether they may be attributed to the action of an undiscovered planet beyond it; and if possible thence to determine the elements of its orbit, etc., approximately, which would probably lead to its discovery."

Graduating in 1843, at the age of twenty-four, and finding himself at last with sufficient leisure to warrant his taking up the problem, he put his ideas into execution by attempting a rough test computation based upon the assumption that the orbits both of Uranus and of the supposed disturbing body were circular. For the radius of the unknown orbit he decided to adopt a value, in accordance with Bode's Law, double that of Uranus, and for the data to rely exclusively upon the modern observations. Encouraged by the results of the rough solution, he attempted a closer approximation; then, convinced by these two preliminary attempts that his hypothesis was based upon fact, he determined upon a more accurate solution.

This third solution he based on better data received from the Astronomer Royal, Sir George Biddle Airy, through the agency of their common friend, Prof. Challis, of the Cambridge Observatory. Though this solution proved unsatisfactory, it served to suggest slight improvements in method. Adams accordingly proceeded to a fourth, and this time was quite successful.

In September, 1845, more than a month preceding LeVerrier's paper on November 10th, and fully seven months earlier than that investigator's announcement, on June 1st, 1846, of the results obtained by his first approximation, Adams placed in the hands of Prof. Challis the elements of a theoretical planet, the perturbations by which, he claimed, would satisfactorily account for the motion of Uranus.

Possessed of but little initiative, Prof. Challis, upon receiving the solution, found himself unable to decide upon a course of action befitting the occasion. He made no telescopic search of the heavens. It was "so novel a thing," in his own words, "to undertake observations in reliance upon merely theoretical deductions; and while much labor was certain, success appeared very doubtful." It was at length decided to lay the matter before the Astronomer Royal, and Adams was accordingly provided with a letter of introduction.

Hoping that such a procedure might lead to instituting a search for the planet, Adams presented himself at the Royal Observatory, Greenwich, but learned that the head of the institution had some time previously departed for the Continent. He was invited to communicate with Airy by letter. Preferring a personal interview, however, he called again a month later; and finding Airy temporarily absent, he left his card with the statement that he would soon return. Upon his return he was informed simply that the astronomer was at dinner. Disappointed at the outcome of his visits, he left a paper containing the results of a fifth solution, and departed.

Airy had repeatedly dampened the enthusiasm of eager astronomers by his skepticism regarding the existence of an undiscovered planet. This skepticism was still unshaken. Further, Airy had discovered, in the thirties, that the motion of Uranus was faulty not only in longitude, but also in the matter of its distance from the sun. He now seized upon this fact to test the value of Adams' work. In replying to Adams, fifteen days after the latter's visit, he accordingly inquired whether the hypothetical planet, besides accounting for the motion of Uranus in longitude, would also cause to disappear the errors in distance.

To Adams the question appeared extremely trivial and evasive. He had hoped for greater encouragement. Already deeply hurt by his reception at the Royal Observatory—for he was of a highly sensitive nature—he found difficulty in replying to the Astronomer Royal, and in the several attempts he made he failed to persevere. Meanwhile, believing that he could obtain a slightly more satisfactory solution he started to solve the problem for the sixth time, and finally decided to postpone replying to Airy's letter until he could present the new results.

As for Airy, paying Adams no further attention pending the receipt of a reply to his letter, he took no steps either to commence a search for the planet, or to publish the results of Adam's calculations. This proved most unfortunate; for allowing LeVerrier time to step into the breach, it deprived Adams of claims to priority.

The publication of LeVerrier's paper of June 1st awoke Airy from his lethargy. Finding that the longitude of the theoretical body, as determined by LeVerrier, differed but little from that which Adams had communicated to him the previous October, he began to reflect whether, after all, there might not be some plausibility in the hypothesis of a disturbing planet. The fact that two investigators, working independently, and unknown to each other, could arrive at the same result, strengthened immeasurably the probability that both were right. He soon changed from a doubting skeptic, carrying words of discouragement, into a zealous believer intent upon converting others.

His enthusiasm aroused like emotions in Challis, and between them they began to devise plans for a search of the heavens. Challis was to drop all work not of an urgent nature, and undertake to map carefully all stars as small as the eleventh magnitude included within a large region of the heavens having for its center the theoretical position of the planet. The plan called for three different sweeps of the region. If a star found in the first sweep were missing from its place in the second, it would be inferred that the object was the planet, and in case of doubt, reference could be made to the third sweep.

It may be wondered why a search on so elaborate a scale should have been thought necessary. It was not expected that the body could be easily distinguished from neighboring stars of equal magnitude. The only recourse, consequently, was to distinguish the stranger by its motion among the fixed stars. With the appliances of our day, the planet could much more easily have been found with the aid of photography. In the absence of this aid the next most ready method was to compare the heavens with previously published charts, for the purpose of ascertaining whether there were a star visible in the telescope which was missing from the charts. This was the method employed later with such good effect by Galle. Unfortunately, however, the English astronomers, in common with astronomers of other countries, not only did not have in their possession a copy of Bremiker's Map, but they were even unaware of its existence. The only course open was to prepare a map of their own by observation from night to night, and the method they adopted for this, though elaborate, offered this advantage, that if it failed to disclose the planet, it would at least lead to the definite decision that no moving body so bright as the eleventh magnitude was to be found in the broad region of the heavens under consideration.

So immense was the task that Challis doubted whether he could complete it within a year, but nothing daunted, he set to work.

On September 29th, 1846, he received a copy of LeVerrier's paper of August 31st, and became much impressed with the French geometer's confidence that the undiscovered planet could be readily distinguished from neighboring stars through the fact that it possessed an appreciable disk. Adams had long before this concluded from his calculations that the brightness of the planet was at least as great as that of a star of the ninth magnitude, and had suggested that it might be picked up in the telescope by its physical aspect. Challis had preferred, however, from considerations of thoroughness, to follow the plan of observation originally adopted. Finding now Adams' belief corroborated from another source, he decided to act upon it. Directing the telescope that very night to the immediate neighborhood of the planet's theoretical position, he began scrutinizing in detail the larger stars. One out of some three hundred of these attracted his attention through the fact that it appeared to have a disk. It was the planet.

Alas for Challis's hopes! He was too late to claim the honor of discovery. The planet has been observed six days previously at Berlin. The news of Galle's good fortune put an end to the work at Cambridge. By that time, Challis had observed some three thousand stars, and was just getting ready to commence mapping. Going back over his notes, he found matter for deep vexation. Within the first four nights of observing he had recorded two positions of the planet, and had obtained sufficient data to assure its discovery. Had he but looked over the records of his observations from night to night—labor postponed for more urgent work on comet reductions—he might have obtained for himself the glory now Galle's. Fate was unkind. With success all but assured, the faithful astronomer had failed.

If fate was unkind to Challis, how much more so was it to Adams! He was the first to solve the problem of Uranus, he was the first to make known the existence of the new planet, yet his researches had exerted not the least influence upon the actual discovery, and at the time of discovery, his name even was unknown.

For this state of affairs he was in no way to blame. He had solved his problem, and communicated his results to two of the foremost astronomers of Europe, one of them the official head of astronomy in England. Receiving but little encouragement, he had made no further effort to spread the news of his calculations, or to urge astronomers that they search for his planet. He had even intended, if no one else would do so, to undertake the search himself, and with this end in view, had actually had overhauled the instruments at St. John's College. Young, shy and sensitive, of a retiring and bashful disposition, unacquainted with the ways of the world, he had done his best. More could not have been expected.

The fault undoubtedly lay with the astronomers in whom he had confided. For almost a full year they had had in their possession data sufficient to assure discovery of the planet, and had failed until too late to act. Through their negligence, they had both deprived themselves of rewards now claimed by foreign astronomers, and had allowed a foreign mathematician to snatch from a young countryman the right to claim priority.

For these sins they received severe punishment at the hands of public opinion in England. It cannot be said, nevertheless, that they had wilfully neglected their duty. They were busy men. As Astronomer Royal, Airy had work sufficient on his hands. Challis, as director of the Cambridge Observatory, was actively engaged in furthering extensive researches on comets and asteriods. To start a systematic search for a theoretical planet meant to drop for an indefinite period other important work, and for months to devote endless labor and expense to a task regarding the successful outcome of which they were skeptical. It is to be noted, too, that once the probability of success became strengthened through the publication of LeVerrier's independent labors, they no longer hesitated.

The announcement of Adams' name as entitled to rank with that of LeVerrier in the story of the new planet awoke in France hostile and bitter criticism. And it must be admitted that there was just ground for complaint. The planet had been discovered entirely as a result of LeVerrier's researches, and in obedience to his earnest exhortations that the telescope be turned toward that portion of the heavens indicated by his theory. Previous to the discovery no mention had been made of Adams. The appearance now of a rival claimant to LeVerrier's honors seemed a dastardly attempt on the part of English astronomers to step beyond the fair limits of national rivalry for the purpose of robbing the French of their due.

English astronomers, strange as it may seem, were many of them no less opposed to Adams' claims than the French. If the latter were guided in their criticisms by a sense of injustice, the former were swayed by a warped feeling of national fairness. However mortified that the honors should have gone to a foreigner, they refused—and with some show of justice perhaps—to look upon Adams' communications to Challis and Airy as a publication, and conceded to LeVerrier the honor of being the sole theoretical discoverer of the planet.

Foreign astronomers on both sides of the Atlantic took a fairer view of the situation. Though recognizing that LeVerrier had established his title to priority through previous publication, they did not lose sight of the fact that Adams had in reality preceded LeVerrier in the theoretical discovery. They realized, too, that the fault was less Adams' than that of Airy and Challis. Their verdict has been upheld by later astronomers, but only after the lapse of years.

Adams himself took no part in the controversy. Not a word of criticism ever passed his lips; not an accusation did he ever make; not a man did he ever blame. Personally, he made no efforts in his own behalf. On the contrary, he awarded to LeVerrier "his just claims to the honor of discovery."

Upon the fortunate Frenchman the world heaped countless honors. From all directions came words of congratulation and praise. Scientific academies all over Europe and America vied with one another in hastening to elect him one of themselves. Prizes and medals came to him unsolicited. Kings paid him homage. It was even proposed that the planet bear his name. And Galle, too, received a share of the honors. Adams alone was neglected.

IV. LATER DEVELOPMENTS.

The accumulation of observations brought to light a startling fact. The observations failed to conform to the tables furnished by the theory of Adams and LeVerrier! It was accordingly attempted to compute an orbit entirely independent of the theory, and based upon the observations only. The result was even more startling. The orbit, far from being quite elliptical, as predicted, was very nearly a circle, and with a radius much smaller than that indicated by theory.

Here indeed was a mystery. Relying upon predictions based upon calculations, astronomers had discovered a planet, only to find that the planet moved in an orbit totally different from that furnished by the calculations. LeVerrier and Adams had obtained an ellipse, with a mean distance from the sun nearly in accordance with Bode's Law. The actual planet moved in an orbit nearly circular and paid no attention whatever to the law!

The failure of Bode's Law, as indicated by the preliminary orbits of Neptune, came as a distinct shock. Astronomers refused to believe, and preferred to distrust the orbit computations.

To obtain a better orbit, there was only one of two things to do: to await the accumulation of a number of years' observations or, as was the case with Uranus in the early years following that planet's discovery, to search through the old star catalogues in the hope of finding observations of Neptune unwittingly recorded there as of a fixed star.

Such an observation was actually discovered. Two astronomers, working independently and by different methods, the one at his desk, and the other with his telescope, the one in America and the other in Europe, came almost simultaneously to the same conclusion, that a star recorded as observed on the night of May 10th, 1795, in a position of the heavens where Neptune had probably at that time had its course, was now missing from the firmament. A little investigation showed this star to have been the planet. It showed further that had the observer exercised a little more care he could not have failed to anticipate the discovery of Neptune by fifty years.

The discovery of this observation, by extending the observed arc of Neptune over one third of the planet's course, furnished ample material for the computation of a good orbit.

These curious developments had meanwhile aroused the interest of Prof. Benjamin Peirce, and this eminent American mathematician now turned the glare of his profound intellect upon the problem of Neptune. Following upon a long series of researches, he announced one evening to a learned audience that:

"The planet Neptune is not the planet to which geometrical analysis had directed the telescope; that its orbit is not contained within the limits of space which have been explored by geometers searching for the source of the disturbances of Uranus; and that its discovery by Galle must be regarded as a happy accident."

This statement, apparently grounded upon firm analysis, and coming from a recognized authority, astounded the world. And no wonder. LeVerrier had predicted the position of an undiscovered planet; the prediction had been made independently some months earlier by another investigator, Adams; and in the predicted place, and having the predicted attributes, the planet had been found. Men had marvelled, and termed the discovery of the planet a wonderful witness to the power of the intellect. Were they now to believe that so sublime a discovery was, after all, nothing more than a "happy accident?"

It often happens that a mathematical problem affords more than one solution. Such, said Peirce, was the case with the problem of Uranus. The irregularities in the motion of Uranus could be accounted for by the disturbing action of any one of several theoretical bodies. The actual planet Neptune is such a body; so also is the hypothetical planet of Adams and LeVerrier. The "happy accident" lay in the singular circumstance that these two solutions of the problem, though differing widely in every other particular, agreed at the time of discovery in the matter of their direction from the earth, so that a telescope pointed at one pointed also at the other. As for the coincidence that LeVerrier and Adams had arrived at the same solution, that was to be explained by the fact that in their researches they had both been led astray by too firm reliance upon Bode's Law.

Peirce went further and assailed the theory employed by LeVerrier and Adams, maintaining that in their investigations they had employed inaccurate formulæ. Though the justice of this criticism has been questioned, later developments seem to indicate that the criticism was not unfair. "Nor can I believe," says the present Astronomer Royal of Scotland, "that if Adams or LeVerrier could have foreseen the immense difference of form between their own formulæ and the truth, they could have had the same confidence in the exactness of the predicted places."

Throughout the bitter controversy that followed, Peirce at no time contested the genius of the theoretical discoverers. He drew a sharp distinction between the task of the mathematician and that of the astronomer, and pointed out that Adams and LeVerrier, in demonstrating the existence of a disturbing body, and obtaining a solution which, though hypothetical, could have accounted for the motion of Uranus had it had a real existence, had performed the task required of the mathematician, and were consequently entitled to all the praise and reward that would have been theirs had the solution proved to be the actual solution of nature.

LeVerrier, nevertheless, did not take kindly to the criticism. He was unwilling to grant that Neptune was not the planet of theory. He refused at first to admit that Neptune moved in any other than the predicted orbit; and casting slurs upon the orbit computations declared that the results of the computations were incompatible with the nature of the perturbations of Uranus. Forced eventually to retreat from this position, he hit upon the explanation that just as Uranus was influenced by Neptune, so Neptune perhaps was disturbed by a trans-Neptunian planet. The hypothesis of a trans-Neptunian planet became untenable, however, when Peirce showed that Neptune, though moving in an orbit different from that predicted, could account for all the perturbations of Uranus with the most surprising accuracy. LeVerrier accordingly reverted to his former stand, and maintained stoutly that Neptune was none other than the predicted planet.

Adams, unlike LeVerrier, took no part in the discussion. It was not until many years later, and then upon a most fitting occasion, that he took it upon himself to remark that though in certain particulars Peirce was right, yet so far as the problem under consideration was concerned—the problem of assigning a position in the heavens where the theoretical planet might be sought for—the criticism was without force. Though admitting that in the discovery of Neptune there was an element of chance, he nevertheless joined his impulsive French contemporary in the assertion that Neptune and the predicted planet were the same.

To what extent the discovery of Neptune was fortuitous is a question perhaps still open to argument. Certain it is that both Peirce and LeVerrier, in course of their controversy, gave utterance to statements apparently paradoxical and self-contradictory. Peirce maintained to his dying day the accuracy of his views; and this after he had resumed his researches, and carried them on extensively. Other astronomers, on the other hand, have as firmly maintained the contrary.

Astronomers of the present day have little faith in the existence of more than one solution. Unfortunately, Peirce never published more than a very brief summary of only a portion of his researches. It is devoutly to be wished that some competent mathematician would investigate the original manuscripts.

V. CONCLUSION.

A word with regard to the supposed trans-Neptunian planets.

Ever since the discovery of Neptune, astronomers have lived in the hope of finding a planet even more remote from the sun. LeVerrier himself was perhaps the first to express this hope, and uttered the remarkable thought that the existence of this planet might be detected mathematically, though it be too small and too far away to permit of telescopic vision. Several astronomers have done considerable work on the problem, but the telescopic search of the heavens based on their researches has proved fruitless.

Whether or not a trans-Neptunian planet has actual existence must be left for time to tell. Of one thing, however, we may rest almost certain. The circumstances attending the discovery of such a planet will hardly be of so extraordinary a nature as those interwoven with the discovery of Neptune.

Correspondence

[The editors are not responsible for statements made in the correspondence column. Anonymous communications cannot be considered, but the names of correspondents will be withheld when so desired.]

A Strange Case of Abnormal Anatomy

[In our issue of March 1st, we had occasion to report on a strange case in which the internal organs of a patient were found to be completely reversed as compared with the normal left and right relation. We have received the following very interesting letter, from which it will be seen that a similar case has been observed in this country, in fact, the letter comes from the subject himself.—Editor.]

To the Editor of the Scientific American Supplement:

Regarding the note on "A Strange Case of Abnormal Anatomy," published in your issue of March 1st, I wish to say that strange things *sometimes* are found in America. The undersigned (now thirty) has known for 15 years that his heart is on the right of the median line. But stranger still, I was operated on for peritonitis March 11th, 1912, when the two attending physicians found that I had no appendix—they having decided to remove it so as not to cause me trouble later in life. Further, they found my intestines exactly reversed, even to the large intestine leading from the stomach. I have no ill effects from such disarrangement of anatomy, and am a postman in this city, making my two trips each working day. I thought this might be interesting to you. Harvey J. Hayes.

Pasadena, Cal.

Hygiene and Mortality.

The National Brotherhood of England has made a study of the health conditions in Derby, England. The city was divided into four districts, in which the following conditions were observed:

No. of District.	Population.	Deaths in One Year.	Deaths per Year per 10,000.	Mean Length of Life.
1	31,754	335	106	47
2	32,812	404	123	40
3	31,018	417	134	37
4	28,741	474	165	30

It will be seen that the mean length of life varies within wide limits—the minimum and maximum differ by no less than seventeen years.

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