CLASSICS OF SCIENCE: The Discovery of Neptune

Astronomy

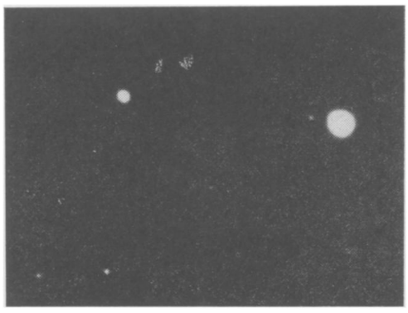
The fulfillment of prophecy is both gratifying and amazing to the human mind. The discovery of the trans-Uranian planet was a very spectacular fulfillment of prophecy, in which several of the foremost astronomers and mathematicians of the world played their parts. We give here a summary of their work, rather than a quotation from any one of them.

REPORT ON THE HISTORY OF THE DISCOVERY OF NEPTUNE. By Benjamin Apthorp Gould, Jr. Washington City: Published by the Smithsonian Institution. 1850.

Motion of Uranus

The strange series of wonderful occurrences of which I am to speak is utterly unparalleled in the whole history of science;—the brilliant analysis which was the direct occasion of the search for a trans-Uranian planet,—the actual detection of an exterior planet in almost precisely the direction indicated,—the immediate and most unexpected claim to an equal share of merit in the investigation, made in behalf of a mathematician till then unknown to the scientific world,—and finally the startling discovery, that, in spite of all this, the orbit of the new planet was totally irreconcilable with those computations which had led immediately to its detection, and that, although found in the direction predicted, it was by no means in the predicted place, nor yet moving in the predicted orbit. This series of events, together with the since developed theory of Neptune, constitute the subject of my report. . . .

The planet Uranus was discovered by Sir William Herschel on the 13th of March, 1781, and, although at first supposed to be a comet, was before the end of the year recognized as one of the primary planets of our solar system. Circular elements were first computed during the summer of 1781, by Lexell, of St. Petersburg, at that time in London; and others were soon after published in Russia, France, and Germany. The computation of a planetary orbit was at that time a most laborious and troublesome process, by no means to be compared with the easy methods in use since Gauss gave to the world the elegant and simple formulas of the *"Theoria Motus."* No elliptic elements were computed, therefore, until the year 1783, during which year elliptic orbits differing but slightly from each other were published by Mechain, Laplace, Caluso, and Hennert; and in the French and German astronomical Ephemerides for 1787 (published in 1784) were tables of the new planet. . . .



THE DISK OF NEPTUNE with its satellite is at the right of the picture photographed at the Yerkes Observatory. It is seen to be appreciably larger than the star at the left.

In the mean time Bode, the Astronomer Royal of Prussia, had suggested that Uranus might have been observed by astronomers before the discovery of its planetary nature, and consequently that earlier observations might be found by a proper search in the catalogues of fixed stars. This happy idea prompted him to study over the old star-catalogues, and his search was crowned with abundant success. . . .

The best tables of Uranus which existed before the masterly and accurate researches of Le Verrier, in 1845 and 1846, were those computed by Bouvard in 1821. Bouvard was acquainted with all the ancient observations which we know, excepting three by Flamsteed in 1715. In the introduction to his tables, he announced that he had been utterly unable to find any elliptic orbit, which, combined with the perturbations by Jupiter and Saturn, would represent both the ancient and the modern observations. The best tables which he could obtain by the use of both represented neither of them, in any way at all satisfactory. On the other hand, by using modern observations only, he was enabled to find elements which, although they gave errors amounting sometimes to 74″ for the ancient observations, still satisfied all the modern ones comparatively well,—never differing more than 10″ from theory, and generally much less. . . .

He therefore summarily rejected the former observations, and founded his tables upon the latter alone, adducing arguments against the accuracy of the ancient observations, and forgetting how well they harmonized with one another, and had harmonized with the elements obtained soon after the discovery of the planet.

But a very few years after the publication of Bouvard's tables, important differences between theory and observation became again manifest, and attracted the attention of astronomers.

Airy alluded, in 1832, to these discrepancies, in his report to the British Association on the Progress of Astronomy, and mentioned that the tables, constructed only eleven years previously, were in error nearly half a minute of arc.

It is an easy thing to censure Bouvard for the readiness with which he abandoned the ancient observations,—now that we know that the discrepancies were caused by the action of an exterior planet, and that the maximum of error in the ancient observations amounted to only nine seconds. . .

Influence of an Unknown Planet

Mr. Eugene Bouvard, nephew of the author of the Tables, wrote as follows on the 6th October, 1837, from Paris, to the Astronomer Royal of England:—"My uncle has given me the tables of Uranus to reconstruct. In consulting the comparisons which you have made between observations of this planet and the calculations in the tables, it will be seen that the differences in latitude are very large, and are continually becoming larger. Does this indicate an unknown perturbation exercised upon the motions of this star by a body situated beyond? I do not know, but this is at least my uncle's idea."

Professor Airy remarked in his reply, that the error in latitude was very small;—that it was the errors in the longitude which were increasing with so fearful rapidity. And, a few months after, he showed that the tabular radius-vector of Uranus was much too small. This result of observations at the quadratures was one to which Professor Airy, both at that time and uniformly since, attached great importance.

It is from this period, that the definite belief of most astronomers in the existence of a trans-Uranian planet appears to date. Numerous mathematicians subsequently conceived the purpose of entering earnestly into laborious and precise calculations, in order to decide whether the assumption of an exterior cause of disturbance were absolutely necessary, and, if so, to determine from the known perturbations their (*Turn to next page*)

Discovery of Neptune—*Continued*

unknown cause. The Astronomer Royal most justly expresses himself "confident, that it will be found that the discovery is a consequence of what may properly be called a movement of the age; that it has been urged by the feeling of the scientific world in general, and has been nearly perfected by the collateral, but independent, labors of various persons possessing the talents or powers best suited to the different parts of the researches."

The problem became, from this time forth, one of the most important questions of Physical Astronomy. Astronomers in various countries busied themselves with it, and spoke of it without reserve. . . .

The next letter of Mr. Adams, which has been printed, is dated September 2, 1846. Le Verrier had, in the mean time, not only published the memoir already alluded to, in which the perturbations of Uranus by Jupiter and Saturn are fully developed, calculated, and discussed,—but had communicated to the Academy two other most important papers. In one, presented on June 1st, 1846, he proved that the motions of Uranus could not be accounted for, except by introducing the perturbative influence of an unknown planet, for which he assigned an approximate place. In the other, he found an orbit, a mass, and a more precise position for the disturbing planet. This was presented on the 31st August.

Mr. Airy mentions, that on the 29th June, at a meeting of the Board of Visitors of the Greenwich Observatory, at which Sir John Herschel and Profesor Challis were present, he spoke of the extreme probability that another planet would be discovered within a short time; and assigned, as a reason for this belief, the coincidence between Mr. Le Verrier's results and those of Mr. Adams. He had addressed a letter to Mr. Le Verrier, similar to that sent a year previously to Mr. Adams, to make inquiries about the radius-vector. Mr. Le Verrier answered under date of June 28, stating that the errors of radius-vector must be accounted for, inasmuch as the equations of condition depended on observations at the quadratures as well as at the oppositions. Concerning the correctness of this inference, however, there appears room for discussion. Le Verrier called Airy's attention to the fact, that the position in quadrature in 1844, deduced by means of his formulas from the two oppositions which comprised it, only differed 0".6 from the observed position, which proved, he said, that the error of radius-vector had entirely disappeared. This he considered as one of the strongest arguments in favor of the truth of his results. For, while in his first researches he only made use of oppositions, the quadratures were represented with all precision. "Le rayon vecteur," said he, "s'est trouvé rectifié de lui-même sans que l'on l'eut pris en considération d'une manière directe. Excusez-moi, Monsieur, d'insister sur ce point. C'est une suite du desir que j'ai d'obtenir votre suffrage."

At Airy's suggestion, Professor Challis had already commenced a search for the planet on the 29th July, using a modification of a plan which Mr. Airy had drawn up. The date of the letter suggesting this search was July 9; that of the general plan was July 13. Le Verrier's memoir, which assigned 325° as the probable longitude of the planet, was presented to the French Institute, as we have seen, on June 1st. Still, it does not appear that any search whatever had been instituted in the intervening time in any part of Europe or America; indeed, there is no account of any search having been made excepting by Professor Challis, before the night of September 23.

It must, indeed, be confessed that astronomers in general did not seem to consider the theoretical results published by Mr. Le Verrier as necessarily indicating the *physical* existence and true position of such an exterior planet. Professor Challis alone—the only astronomer who entered into a systematic search for the planet, and the only one excepting Dr. Galle, the assistant at the Royal Observatory of Berlin, whom we know to have even looked for it—has assigned, as a reason which deterred him from an earlier search, that it was "so novel a thing to undertake observations in reliance upon merely theoretical deductions; and that, while much labor was certain, success appeared very doubtful." . . .

The Discovery

A strange contrast to this apathy on the part of other astronomers is furnished by the demeanor of Le Verrier himself. Having fairly arrived at his results, he looked upon them as conclusive. His computations had been an earnest work. He had employed all his analytical powers, and employed them, too, most successfully—to refine the methods which he used, and to narrow the field of his inquiry; all his powers of application and numerical research, to insure precision; and his indomitable perseverance, in carrying out his computations with full vigor, permitted him to omit no possible test of their accuracy. He proved that the observations of Uranus made it necessary to assume the existence of some unknown disturbing body. For the observations which he adopted as the basis of his calculations, he had assigned, *a priori,* the limits of error allowable; and he found that all the observations could be satisfied within these predetermined limits by the assumption of an exterior planet, moving in a given orbit, and having a given mass. The correctness of his results was dependent upon no empirical assumption. He gave them, therefore, fearlessly to the world, and staked his reputation upon their accuracy. This forms by no means the least part of his claims to the respect and admiration of scientists throughout the world. Had the planet not been found in the predicted place, Le Verrier would alone have borne the mortification. Neptune was discovered in almost precisely the direction assigned, and Le Verrier receives the admiration so justly due him.

The mass and orbit given in the memoir of August 31st are as follows:

Semiaxis major	36.1539
Sidereal period	217 yrs. .387
Eccentricity	0.10761
Equation of the center	7° 44′ 44″
Longitude of perihelion, January 1, 1800	284 5 48
Mean longitude	240 17 41
Precession in 47 years	0 39 20
Mean sidereal motion in 47 years	77 50 3
Mean anomaly, 1847, January 1	34 1 56
Mean longitude	318 47 4
Mass	$\frac{1}{9322}$

The geocentric longitude, resulting from this orbit, for the end of September, 1846, was 325°. Le Verrier, in acknowledging the receipt of a memoir, made use of the opportunity thus afforded, to request Dr. Galle to look for the planet. The letter reached Berlin on the 23d September, and Galle, in complying with this request, found, on the same evening, a new planet in longitude 325° 53′, or within 55′ of the geocentric place assigned by Mr. Le Verrier.