ART. XXVII. — *Preliminary Account of a Speculative and Practical Search for a Trans-neptunian Planet*; by D. P. TODD, M.A., Assistant Nautical Almanac.

Introductory and Historical.

THE suggested probability, on scientific grounds, that there revolves about the sun a second planet exterior to the orbit of Uranus, is not new. So early as 1834, when the foremost astronomers of the day were by no means settled in their convictions that even the greater portion of the then rapidly increasing residuals in the longitude of Uranus was due to the perturbing action of a single exterior planet, Hansen is credited with expression of the opinion, in correspondence with the elder Bouvard, that a single planet would not account for the differences between theory and observation.* Dr. Gould, however, in his *Report on the History of the Discovery of Neptune*,† says, “I have the authority of that eminent astronomer himself [Hansen] for stating, that the assertion must have been founded on some misapprehension, as he is confident of never having expressed or entertained that belief.” Professor Peirce’s criticism of the investigations of LeVerrier, to the effect that his predicted orbit of Neptune was so widely discordant from its

* *Memoirs Royal Astronomical Society*, vol. xvi, p. 388.

† Published by the Smithsonian Institution, 1850.

observed orbit as to indicate that his computations did not pertain to the actual disturbing planet, elicited from him the reply that the perturbations of Uranus due to a possible planet exterior to Neptune might readily cause an uncertainty of 5" to 7" in the fundamental data of his research.

In 1866, the Smithsonian Institution published the general tables of Neptune, by Professor Newcomb. In the investigation of its orbit the author proposed: "3. To inquire whether those motions [of Neptune] indicate the action of an extra-Neptunian planet, or throw any light on the question of the existence of such a planet." He concludes (page 73) that it is "almost vain to hope for the detection of an extra-Neptunian planet from the motions of Neptune before the close of the present century."

In 1873, the Smithsonian Institution published the general tables of Uranus, by Professor Newcomb. His success in the treatment of the theory of Uranus was such that astronomers generally may be said to have been satisfied, from the smallness of the longitude-residuals, that there existed no evidence of perturbative action upon Uranus other than that actually taken into account in the construction of the tables. It is well known, however, that since the publication of these tables the error of longitude has been on the negative increase, and the latest observations place the planet increasingly more in advance of its theoretic position.

Sometime in the spring of 1874, the first preliminary outline of the very simple method which I have here employed in the treatment of planetary residuals with reference to exterior perturbation, suggested itself to me. For more than three years, very little opportunity offered for consideration of the problem of a trans-neptunian planet, and I gave it merely desultory attention. In August, 1877, however, I began to devote the larger portion of my leisure time to the theoretic side of the question. It was soon evident that no certain hold upon any possible cause of exterior perturbation could be obtained from the residuals of Newcomb's tables. And I may remark here that I have consequently chosen the term *speculative* rather than *theoretic* as applying more fitly to the investigation which preceded the actual telescopic search.

The Speculative Search.

While the magnificent researches of LeVerrier and Adams on the perturbations of Uranus are masterpieces of analytic skill, I felt that they should not be taken as models in the present investigation—for two reasons:

- (1) The residuals of longitude which must form the basis of

the investigation are not sufficiently well marked to justify the execution of so laborious a research, especially if it be found that a simple, rational treatment, unencumbered with the refinements of analysis, may be fairly interpreted as indicating the position of an exterior perturbing body with merely a rough approximation.

(2) Even in the case of Uranus, and the theoretic search for Neptune, where the residuals of longitude were very strongly marked, many of the elements pertaining to the disturbing planet which Adams and LeVerrier sought to determine theoretically, turned out afterward, when their real values became known, to have been indicated with only meagre precision. Much less should we now expect these elements to be given with any certainty in the case of a planet exterior to Neptune.

I was also much impressed by a remark in Sir George Airy's *Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus*—"I have always considered the correctness of a distant mathematical result to be a subject rather of moral than of mathematical evidence."*

This provisional treatment of the residuals of Uranus was undertaken, then, as a preliminary to the proposed telescopic search, to determine whether that search was worth undertaking; and, if so, at what point, approximately, it was best to begin. I should remark, also, that this portion of the work, as an investigation to these ends, was never undertaken with reference to publication.

I.—Let us now consider, *seriatim*, the errors of the elements of the perturbed planet—errors which the very hypothesis of a disturbing body introduces, and which must have entered into the tables of the inferior planet, as constructed independently of unknown exterior perturbation: we consider what the effect of these errors may be, and how far it may be eliminated or subtracted from the residuals of the actual theory of the planet. These residuals are, of course, first corrected for any known error of theory or tables, or erroneous masses of known perturbing planets.

(1) *The error of mean distance of the perturbed planet.*—Any error of radius vector enters very largely into the residuals of heliocentric longitude, if the observations are made at any considerable interval from the planet's opposition. If it is suspected that the error of radius vector will vitiate the residuals of longitude, we may avoid its effect by passing to residuals of geocentric longitude. Or, we may confine our research to the mean residuals of observations near the opposition-points, and symmetrically placed with reference thereto. The effect of erroneous radius vector is thereby eliminated.

* *Memoirs Royal Astronomical Society*, vol. xvi, p. 398.

(2) *The error of periodic time of the perturbed planet.*—If the residuals are examined graphically, the eye will readily detect whether any correction to the periodic time is advisable. If, in general, the mean line of the residuals is nearly a right line, and makes a given angle with the line of zero-residual, it may fairly be concluded that the residuals need a correction depending directly on the time, the magnitude of the coefficient of which is indicated by the divergence of the two residual-lines.

I had considered the problem only thus far when it occurred to me to apply the method, only partially developed, to the determination of an approximate position of Neptune from the residuals of Bouvard's Tables of Uranus, published in 1821. Taking also the residuals from observations up to 1824, and not permitting myself a knowledge of the longitude of Neptune at any epoch, a very little labor gave me an approximate position of the disturbing planet from which, it now appears, Neptune might easily have been found some twenty years in advance of its actual discovery.

When my work had advanced to this stage, a mere chance threw in my way a copy of Sir John Herschel's *Outlines of Astronomy*, (which I had never before examined): I at once observed that my treatment of the residuals of Uranus with reference to a planet exterior to Neptune was quite similar to his "dynamical" exposition of the perturbations of Uranus arising from Neptune itself. And I was further gratified to find that he had given a very full and lucid statement of the effect upon the longitude-residuals caused by errors of the third and fourth elements of the perturbed planet—the error of eccentricity, and the error of longitude of perihelion. I therefore adopted, without hesitation, the continuance of the graphical method therein detailed; and shall do no more here than to refer to the pages of Herschel's treatise where these elements are dealt with.

(3) *The error of eccentricity of the perturbed planet.*—(See Sir John Herschel's *Outlines of Astronomy*, page 536.

(4) *The error of longitude of perihelion of the perturbed planet.*—(Ibid., page 537.)

When the longitude-residuals have been corrected in this manner, we proceed on the assumption that any outstanding residuals are due to unexplained exterior perturbation.

II.—Of the seven elements of the disturbing planet, we must assume a value of one: the values of three others, together with the mass of the disturbing planet, we may consider as theoretically determinable from the longitude-residuals themselves.

(1) *The mean distance of the disturbing planet.*—Regarding

the next order of distance beyond Neptune as occupied by the planet for which we are searching, I assumed, as a first value of mean distance, $a=46.0$: this value seemed to be indicated by a fair induction. The periodic time of the planet would then be 312 years, and conjunctions with Uranus would occur nearly at intervals of 115 years.

(2) *The eccentricity of the disturbing planet.*—Even with the large residuals of Uranus employed in the investigations of LeVerrier and Adams, the derived value of the eccentricity of Neptune was entirely illusory. The several values of eccentricity of Neptune resulting from their investigations are as follow:

Adams (<i>first hypothesis</i>)	0.16103
LeVerrier	0.10761
Adams (<i>second hypothesis</i>)	0.120615

The eccentricity given by investigation of the orbit of Neptune from observations of the planet was:

Newcomb (<i>Tables of Neptune</i>)	0.0089903
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We should, therefore, expect nothing of any attempt to arrive at the eccentricity of an orbit exterior to that of Neptune.

(3) *The longitude of perihelion of the disturbing planet.*—Much the same remark obtains in reference to this element. The several values of longitude of perihelion of Neptune, resulting from the researches on perturbations of Uranus, are as follow:

Adams (<i>first hypothesis</i>)	315° 57'
LeVerrier	284° 45'
Adams (<i>second hypothesis</i>)	299° 11'

The longitude of perihelion given by observations of the planet is:

Newcomb (<i>Tables of Neptune</i>)	46° 6' 39".7
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Evidently it would not be wise to include this element in the investigation.

(4) *The epoch of the disturbing planet.*—If we can obtain even a rough approximation to the value of this element, the end of the investigation is fully attained. An inspection of the outstanding residuals, graphically exhibited, will show, without further labor, the epochs of maximum disturbance. The best that can be done will be to prepare an approximate perturbative curve, the epochs of maximum disturbance of which shall be in harmony with the assumption of mean distance of the exterior planet. By applying this to the plot of outstanding residuals, we may decide at what points the appli-

cation of the perturbative curve best accounts for them. The amount of excursion in the several sines of the perturbative curve we need not for this purpose attend to with any great care: this will depend upon the mass and distance of the disturbing planet; and, that it will be unavailing to attempt any determination of the mass in the present case will be evident from the fact that the mass of Neptune, from the theoretical investigations of LeVerrier and Adams, was widely discrepant:

Adams (<i>first hypothesis</i>)	0.0001656	$\frac{1}{6039}$
LeVerrier	0.0001075	$\frac{1}{9300}$
Adams (<i>second hypothesis</i>)	0.00015003	$\frac{1}{6688}$

While the most reliable mass of Neptune from observation is:

Newcomb (<i>motion of the satellite</i>)	0.00005160	$\frac{1}{19380}$
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We have thus reduced the inverse problem of perturbation to a very simple, rational form. The residuals of longitude of Uranus were next treated in accordance with this method.

In his *Investigation of the Orbit of Uranus*, Newcomb presents three series of residuals: the mass of Neptune finally adopted in the tables, $\frac{1}{19380}$, corresponds very nearly to the mean of the first and third series. But the mass of Neptune which was employed in this investigation is that given by Newcomb's discussion of the motion of the satellite of Neptune, and is $\frac{1}{19380}$. Our first step, then, was to correct these mean residuals into accordance with this adopted mass. The following table presents the date, the mean residuals, the correction for mass, and the corrected mean residuals.

Date.	$\frac{1}{2}(\Delta_1 l + \Delta_3 l)$.	Mass-correction.	Σ .
1691.0	—11"	+3"	—8"
1715.2	—8.5	+1.9	—6.6
1751.1	+2.8	—0.7	+2.1
1769.0	—1.5	+1.5	0.0
1783.3	—0.17	+2.58	+2.41
1790.0	+0.76	+2.27	+3.03
1795.0	—0.36	+1.73	+1.37
1802.0	—1.06	+0.72	—0.34
1806.5	—0.86	+0.09	—0.77
1810.5	—0.21	—0.32	—0.53
1814.5	—0.32	—0.64	—0.96
1819.5	—0.37	—0.79	—1.16
1824.8	+1.45	—0.79	+0.66
1829.7	+0.86	—0.76	+0.10
1835.2	—0.37	—0.77	—1.14
1839.8	—0.33	—0.82	—1.15
1844.8	—0.02	—0.93	—0.95
1849.9	—0.37	—1.08	—1.45
1854.9	—0.25	—1.20	—1.45
1860.0	—0.14	—1.27	—1.41
1865.0	+0.49	—1.24	—0.75
1870.0	+0.12	—1.05	—0.93

The numbers, then, in the column Σ , taken in connection with the corrected residuals given by observations up to 1877, formed the basis of the subsequent investigation. I need only state here that, examining these residuals according to the method just related, in reference to unexplained perturbing action, I concluded that Uranus was in conjunction with an exterior perturbing body between the years 1780 and 1795, and that another conjunction would take place at some time before the close of the present century. The most probable position of the exterior planet I therefore considered to be about 170° of longitude: the probable error of the position I estimated to be, roughly, 10° . This result was reached on the morning of the 10th of October, 1877. During the few days immediately following, I reviewed this examination, as much as possible independently of the previous result, and at the same time varying the assumed mean distance. With a value of $a = 52.0$ (which I finally considered inductively the most probable) I set down the longitude of the exterior planet equal to $162^\circ \pm 6^\circ$: this result was reached on the afternoon of the 14th of October. I now turned my attention toward a similar treatment of the residuals of Neptune, with a slight hope of getting a confirmatory result: two suppositions agreed in fixing the longitude at about 180° and 200° , respectively. I therefore, on some day in the latter part of October, 1877, wrote down as the exposition of all my enquiry, the following results:

EXTERIOR PLANET.—Longitude (1877-84), $170^\circ \pm 10^\circ$
 Mean distance from the sun, 52.0
 Period of revolution about the sun, 375 yrs.
 Mean daily motion, $9''.46$
 Angular diameter, $2''.1$
 Stellar magnitude, 13+
 Longitude of ascending node, 103°
 Inclination of orbit to ecliptic, $1^\circ 24'$

To the determination of the four latter results I shall allude presently. I may now add that this result for longitude of the exterior planet is sustained by observations of Uranus up to and including the late opposition. If a new, disturbing planet really exists in the longitude here indicated, nearly a century must elapse before its existence can be asserted at all positively from residuals of Neptune alone.

The Practical Search.

I should never have been able to execute the telescopic search consequent upon the investigation just related, had it not been for the courteous offices of Rear Admiral Rodgers,

Superintendent of the Naval Observatory, and Professor Hall, in charge of the great refractor. It was with this instrument—the 26-inch equatorial—that the search was conducted, beginning on the night of the 3d of November, 1877. It seemed to me that I should begin the search at a point about 20° preceding that indicated as the most probable position of the planet, and continue it to a point following by the same distance. But a careful search extending over a zone of this length, and of sufficient width to be certain to contain the supposable planet would be a work of such magnitude that I could not expect its completion under several years. I therefore had recourse to an inductive determination of the inclination and longitude of node of the planet's orbit.

I computed anew the position of the invariable plane of the solar system. A differential comparison of its inclination with the inclinations of the orbits of the major planets gave, with little uncertainty so far as the mere induction was concerned, the inclination of the orbit of the trans-neptunian planet equal to $1^\circ 24'$. Similarly I obtained for the longitude of node, though not so certainly, 103° . For the preliminary search, I determined to fix the latitude-limits of the zone at a width of one degree to the north, and one degree to the south of this adopted plane. To these elements I strictly adhered,—with the intention, however, of alternately increasing and decreasing the inclination, and varying the longitude of node, if I should arrive at no successful result from the search of this limited zone.

I may remark that the detailed plan of the instrumental search had been completely digested and written out as early as the 5th of September. To assist in a decision as to what method of search I should employ, I had recourse to an inductive consideration of the real diameters of the known planets of the solar system: I need only mention here that I arrived at the result that a diameter of 50,000 miles might be taken as the minimum value for a planet next beyond Neptune. On this assumption, the mean distance of 52.0 gave for its apparent diameter $2''.1$. I did not, therefore, hesitate in adopting the method of search depending upon the detection of the planet by contrast of its disk and light with the appearance of an average star of about the thirteenth magnitude. I considered a magnifying power of 350 diameters sufficient for the detection of a disk of this diameter: in the actual search, a power of 600 was often employed, but most of the search was conducted with a power of 400 diameters.

On thirty clear, moonless nights, between the 3d of November, 1877, and the 5th of March, 1878, this search was carried on after the manner I have indicated.

After the first few nights, I was surprised at the readiness with which my eye detected any variation from the average appearance of a star of a given faint magnitude: as a consequence whereof my observing-book contains a large stock of memoranda of suspected objects. My general plan with these was to observe with a sufficient degree of accuracy all suspected objects. On the succeeding night of observation these objects were re-observed: and, at an interval of several weeks thereafter, the observation was again verified. At 3 A. M., the 6th of March, 1878, the search was discontinued—my observing-book ends with the following note:

“The adopted plane of orbit of trans-neptunian planet is now searched (without break) from

$$\begin{aligned} v &= 146^{\circ}8 \\ \text{to } v &= 186^{\circ}1.” \end{aligned}$$

I have much confidence in this telescopic search—my aim was to sweep the zone so carefully that there should be no pressing need of duplicating it. If a trans-neptunian planet of an apparent diameter so great as 2" is ever discovered, I shall be much surprised if it is found that it must have eluded my search.

Very soon after the termination of this search, I received the new tables of Uranus, by the late LeVerrier.* I at once instituted a treatment of the residuals of these tables after the method employed with those of Newcomb. I merely mention here that I reached a result entirely confirmatory to that previously obtained. The residuals were first reduced to Newcomb's mass of Neptune.

I ought not to conclude this paper without adverting to the apparently long delay of its publication. From the very beginning, I had approached the entire problem of search for a trans-neptunian planet with resolute direction toward the end which I regarded of the highest scientific import—that of *finding the possible planet at the earliest moment*: if I were successful, observations of its position would then be secured at once, and an accurate determination of its elements would be a matter of earlier realization—it seeming improbable that any prior chance-observation would ever be brought to light. After pursuing the theoretic side of the question for a short time, I saw clearly that many years must elapse before the perturbing action of this body on any interior planet would afford anything like pronounced evidence of its existence: recourse must be had to the practical, telescopic search. So I tarried longer with the residuals of Uranus only in the hope of a possible shortening of the search by some indication that the

* Annales de l'Observatoire de Paris, Mémoires. vol. xiv.

planet was more probably in one portion of the heavens than in another. After the telescopic search which I was conducting had been temporarily brought to an end, by circumstances beyond my control, I was not without hope of effecting some arrangement whereby I might resume the search at an early day, and carry it to a satisfying conclusion. After much thought upon the apathetic reception with which the magnificent researches of Adams and LeVerrier had met, I reached the conclusion that no competent observer would be led to continue the search through knowledge of the little work of speculation that I had done. And, as the work was undertaken with the end always in view of finding the planet, I saw no good to come from its publication.

It will be remarked that this matter now assumes a very different aspect: the publication of a recent memoir *On Comets and Ultra-Neptunian Planets*, by Professor George Forbes, of Glasgow, assigns, by a method of investigation entirely independent of my own, a position to a possible trans-neptunian planet which may be regarded as in exact coincidence with that which I have deduced. The assumption of a mean distance 100, indicated in Professor Forbes' paper, will not appreciably destroy the representation of the residuals with which I have dealt. I have not yet been able to convince myself that the remarkable harmony of the results of the two investigations is simply a chance agreement; and, with the hope that the accumulated evidence of the existence of a far exterior planet may not fail to incite some observer in possession of sufficiently powerful telescopic means to a vigorous prosecution of the search, I have prepared this preliminary paper in order that attention may be called to the matter in sufficient advance of the now approaching opposition-time. I may add here that, should a careful and protracted search of the region adjacent to the indicated longitude prove unavailing, no more certain test of the existence of a trans-neptunian planet admits of application within the next few years than that of telescopic search of a limited zone extending entirely around the heavens—a search which I have been hoping, for more than two years past, for an opportunity to undertake, but which I see no present prospect of realizing.

Nautical Almanac Office, Washington, August 4, 1880.