ADMISSIONS TO SICK REPORT—*Concluded*

NUMBERS OF INTERNATIONAL CLASSIFICATIONT	UNITED STATES		
		White	Colored
	Mean strength	545,518	13,150
	Causes of admission to sick report	Ratio per 1000 of mean strength	
00–02, 07, 15, 16, 24–27, 29–31, 49, 57, 58, 60, 61, 63–65, 70–72, 74, 76–78, 82–84, 97–99	Traumatisms, others	9.14	10.04
80	Sunstroke	0.04	0.08
	Total for diseases	882.51	1064.41
	Total for external causes	91.69	90.42
	Grand total	974.19	1154.83

PERTURBATIONS AND TABLES OF THE MINOR PLANETS DISCOVERED BY JAMES C. WATSON

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Among the many important contributions to Astronomical Science by James C. Watson, one of the original members of the National Academy of Sciences, is the discovery of twenty-two minor planets commonly known as the Watson asteriods. The first of these, (79) Eurynome, was discovered at Ann Arbor on September 14, 1863, and the last, (179) Klytaemnestra, on November 11, 1877, three years before his death. By his will a fund was bequeathed in trust to the National Academy of Sciences for the purpose of promoting astronomical research. One of the objects specifically designated was the construction of tables of the perturbations of the minor planets discovered by the testator. From the beginning Prof. Simon Newcomb was a member of the board of Watson Trustees; later he became chairman of the board, and remained in that capacity on the board until his death in 1908, being succeeded by Prof. E. C. Pickering, whose associate trustees now are Professors E. B. Frost and W. L. Elkin. The National Academy of Sciences through its various boards of trustees has at all times made the most persistent efforts to discharge the chief obligation imposed by the trust, by providing tables of the perturbations of the Watson asteroids.

My own connection with this great undertaking is best explained by the following substance from a letter addressed to me by Professor Newcomb under date of June 4, 1901.

> I have consulted with the Watson Trustees on the subject of having the perturbations of certain Watson asteroids computed under your direction. We are all favorable to the project provided we can have some assurance of success. We have had the coöperation of many capable men but so far the result has not been satisfactory. The general outcome of the matter is that we are supplied with a mass of papers, computations, perturbations, and perhaps tables, but nothing has yet reached us in a form complete and perfect for publication as tables. We had just about reached the conclusion that the work must be done here at Washington under my personal direction and according to formulae which I should supply.

For further details in regard to the earlier history of the work I must refer to the introduction written by Professor Newcomb in March, 1908, to the tables of twelve Watson planets completed under my direction in 1907 and published in 1910 as volume X, Seventh Memoir of the National Academy of Sciences.

Actual work was commenced by myself with the assistance of Drs. Russell Tracy Crawford and Frank Elmore Ross according to a somewhat definite program carefully arranged by Professor Newcomb. Our investigations were to be confined to those thirteen planets, for which so far no attempt for derivation of the perturbations had been made, Professor Newcomb's purpose being to have the difficulties encountered with the remaining planets investigated under his personal direction at Washington. One planet, (132) Aethra, was to be entirely excluded from the program, having remained lost since the year of its discovery in 1873. But in April, 1903, it was agreed that all the previous investigations should be turned over to me. At the same time it had become apparent that the success of the undertaking demanded that I should be given the utmost freedom in planning and conducting the investigations, to which the trustees readily consented.

Today it is my great privilege to be able to report to you, that except for the completion of the manuscript for publication, and some minor numerical work, the task is accomplished and that the obligations of the National Academy under the Watson Trust pertaining to the perturbations and tables of the minor planets discovered by Watson, will soon be fully discharged.

It is impossible in this brief space of time to give a complete history of the work, including the part taken by my numerous assistants. I must also refrain from reporting on the many scientific results attained as to methods of attack and solution, as these involve mathematical detail. I can touch here only on a few points which must serve to illustrate the general character of the work. But first of all it would seem necessary briefly to review what objects are to be served in determining perturbations.

When a planet moves solely under the central attraction of the Sun it describes an ellipse defined in space by six elements or constants, of which for our present purposes three are of prime importance, namely the mean daily motion, or briefly, mean motion, which is the average angular velocity about the Sun, or 360° divided by the period of revolution, in days; the eccentricity of the ellipse; and the inclination of the plane of its motion to the ecliptic. The six elements at any instant depend on the three coördinates defining the position of the planet at the instant and on the three components of velocity. When the constant elements are ascertained and the body continues to move solely under the Sun's attraction, the position and velocity of the planet may be obtained from them at any other time and vice versa. Such positions and velocities are called undisturbed positions and velocities. But if to the Sun's attraction is added that of one or more major planets, like Jupiter, then the velocity is changed and the planet will depart from its undisturbed ellipse. These departures are called perturbations. From a disturbed position and velocity at any instant elements may again be determined for that instant. They represent disturbed elements, as compared with the former. Thus different elliptic elements may be computed for different instants from the corresponding positions and velocities attained under the attraction of the Sun and major planets. These are called osculating elements. Any one set may be adopted as undisturbed and the differences of the others from that set represent the perturbations in the elements.

Whether the perturbations are expressed in the elements or in the coördinates, they are determined by the integration of differential equations, the solution of which in the present state of astronomical science involves expansion in trigonometrical series. The integration introduces divisors which become very small, when the mean motion of the minor planet is in a commensurable ratio to that of a disturbing planet. Therefore since Jupiter's mean motion is 299″ or nearly 300,″ such series fail for instance for a planet with mean motion nearly 300″ or 600″ or 900″. Since Hansen's method, to the application of which our work was originally to be restricted, is based on series of the character referred to, it was bound to fail for planets with a commensurable ratio. This was our actual experience with the first planet undertaken.

This difficulty, however, is purely a mathematical one and has been overcome by Bohlin in his "Gruppenweise Berechnung der Stoerungen" for the group 1/3 by the introduction of the simple expedient of using the exponential for the trigonometrical form in the series. In fact he has published tables for the exact commensurability 1/3 in which all of the elements appear explicitly in the coefficients. The series referred to progress according to powers of the eccentricities of the disturbed and disturbing planets and of the mutual inclination of their orbit planes. In order to avoid unmanageable expressions Bohlin had to confine himself to terms of the second and sometimes third degree in these quantities, while Hansen's method with the aid of Bessel's functions for the eccentricities imposes no restrictions in this respect.

Bohlin's method otherwise closely resembles Hansen's in the treatment of the perturbations, the only other distinctive feature being that for planets with mean motion nearly commensurable with Jupiter's, the perturbations are developed by Taylor's theorem in ascending powers of a quantity w which depends on the difference between a multiple of a planet's actual mean motion and a multiple of Jupiter's so that ultimately series within series progress according to powers of the mass of the disturbing body, of the eccentricities of the disturbed and disturbing bodies, of the mutual inclination of their orbit planes, and of the quantity w.

Now several of the Watson planets belong to the Hecuba Group or Group 1/2, having a mean motion of nearly 600″ or about twice that of Jupiter, and as no theory then existed for this group it was decided to develop the theory and tables for this group 1/2 on Bohlin's plan for the group 1/3. But owing to the great complexity of the problem and the intricate transformations involved it was thought wise first of all to assure ourselves of an exact understanding of Bohlin's method by reproducing selected values of his tables for the group 1/3. In this we failed in many instances. After much fruitless search for the cause of the discrepancies these were called to the attention of Bohlin, who promptly replied that he had become aware of inaccuracies in his theory and tables and that he had already completed a revision of his work, sending at the same time advance proof sheets, verifying our conclusions. We now felt safe in attacking the mathematical theory of group 1/2 and after another year's work on the theory and tables of group 1/2 preparatory to the application of Bohlin's method to the Watson planets of that group, we learned from Bohlin that von Zeipel was engaged in the same task. A little later we received von Zeipel's printed tables. These we at once compared with our own, many transformations from one to the other being necessary on account of the difference of developments used, but to our dismay we discovered many disagreements. By correspondence these have practically all been cleared up and thanks to the careful system of checks adopted in our work we found it unnecessary to change any of our results. In some minor respects we still differ, but the expressions on which the numbers in question are based are so complicated that von Zeipel doubts whether he can remember how he has obtained his values. We are thus abiding by our own results, which have been fully verified as I shall show a little later.

The mathematical and numerical work involved in the revision of von Zeipel's theory has been performed under my direction by Miss Anna Estelle Glancy and Miss Sophia H. Levy. The former has also prepared a complete set of directions for the application of von Zeipel's theory for the Hecuba Group and has illustrated the same with full numerical details for his example, 10 Hygiea, for the guidance of computers.

The work to which I have just referred serves to indicate how the original program of routine computation to the first order of mass with respect to Jupiter by Hansen's established formulae was completely changed to include extensive theoretical investigations.

Although Hansen's and Bohlin's methods—the latter extended by von Zeipel and ourselves, and also by D. F. Wilson, to provide tables for groups 1/2, and 5/2 in addition to group 1/3—were entirely sufficient to master the Watson planets, for which, however, the group 5/2 was not applied, an extensive theoretical analysis has also been made of Brendel's recent methods based on the researches of Gyldèn. These methods appeared while our work was well under way. In passing I might state that all this theoretical work was done independently of the Watson funds.

Aside from these purely theoretical problems the Watson work naturally falls under two heads:

(1) The investigation of the causes of the failure of the earlier work as referred to by Newcomb.

(2) The application of the most suitable methods to the planets on which work had not been undertaken.

In addition the ultimate investigation of the cause of the loss of (132) Aethra was constantly kept in mind.

Under the first head I wish to make especial reference to the work done by Eichelberger on (93) Minerva. Professor Newcomb naturally suspected an error of computation on account of an apparent motion of the node indicated by the outstanding discrepancies in the representation of the observations. Newcomb himself attempted to correct Eichelberger's investigations by further computations at Washington. But later I found the whole trouble to be due to the fact that too much had been expected from first order perturbations by Jupiter, that is, perturbations involving only the first power of the mass. I suggested that a repetition of the work on Minerva to only four or five places would accomplish as much as the previous investigation to seven figures and that the residuals of the observations would reveal the same implied motion of the node; that the latter was of a higher order; and that it could be determined empirically either from our own or Eichelberger's outstanding residuals. Professor Newcomb welcomed this test and all of Eichelberger's work was repeated under the plan proposed, but on the basis of mean instead of Eichelberger's osculating elements and with the mean instead of the eccentric anomaly as independent variable, with the result that the suspected cause of the trouble was fully substantiated. The outstanding discrepancies which were really negligible under the original program but appeared large because of the great accuracy with which Eichelberger had computed, were then removed by an empirical determination of the still remaining motion of the node.

At first Professor Newcomb was greatly surprised by the considerable difference between our coefficients of the perturbations and those of Eichelberger, but formulae were soon derived which enabled me to deduce one from the other on the basis of the difference in elements used and thus to verify both sets of developments. After this investigation Eichelberger's original tables were published in an abridged form.

More serious complications were encountered in the revision of other computations, which were due to misconceptions of theory and numerical errors.

The integration of the differential equations for the perturbations gives rise to certain constants in the developments, to terms multiplied by the time (secular terms), and to purely trigonometrical or periodic terms. All of these added to the undisturbed coördinates give the disturbed place. Some of these terms are of the same form as terms employed in computing the undisturbed positions and may be combined with them or introduced in the elements. It then becomes merely a matter of arbitrary classification or combination, as to what part of the disturbed coördinate shall be directly computed from properly changed elements and what part shall be left over as perturbations. When all but the purely periodic terms are put into the elements, we may deduce a sort of mean elements, and we can thus speak also of a mean mean motion. These elements involve properly chosen constants and are not of the sort that they represent the actual position and velocity at any particular time. It was found that some of my predecessors in the work had misinterpreted the terms they were dealing with although their developments were numerically correct.

The tracing of these and other inaccuracies for the planets to be revised have absorbed more time and effort than all the work on the planets that had not been previously undertaken. To simplify the determination of the constants and to secure greater accuracy Professor Newcomb's program originally called for adopting the mean of the several sets of elements printed for each planet in the B. J. as approximate mean elements, but later we shall see that in the case of Andromache the actual mean elements lie outside of the osculating elements obtained from observations and special perturbations over a period of thirty-three years. Thus we actually at first started with elements which were neither osculating nor mean elements, which involved corresponding difficulties later. But it should not be forgotten that at that time the whole problem of proper attack was still in an experimental stage.

With reference to Brendel's very exhaustive and brilliant methods I may remark that I have come to the conclusion that even if they stand the test of the very critical cases of near commensurability, for practical purposes they do not have the advantage of the same simplicity as a proper choice between Hansen's and Bohlin's methods.

For six of the twelve sets of tables published in 1910 comparison of observations taken in 1912–13 have been made with theory by Miss A. R. Kidder, with an agreement of less than the proposed 1′ of arc, excepting in one case, (133) Cyrene, which although approaching the Hecuba type had been developed by Hansen's method in the early stages of our investigations. As the observation used in this case, however, is only approximate, a further test may show the slight discrepancy to be erroneous. These comparisons represent a remarkable result for the published tables, as in these cases some twelve years had elapsed since the last opposition used for the correction of the elements and for the tables.

In conclusion I may refer briefly to two of the most interesting Watson planets.

The Watson planet which promises to become one of the most interesting of the twenty-two under consideration, if not of all the 1400 or more so far discovered, is (175) Andromache, which belongs to the Hecuba group, or group 1/2, the mean daily motion being approximately twice that of Jupiter. Andromache was found by Watson on October 1, 1877 and was the last but one discovered by him, his latest discovery, as stated before, being (179) Klytaemnestra on November 11, 1877. Andromache was assiduously observed for one month, 43 observations at widely scattered observatories being secured by the most competent observers. The calculation of the orbit yielded a mean motion of approximately 550″. In spite of diligent search the planet was not found again from any predictions based on the original elements. This failure to find Andromache is a striking illustration of the fallacy of some of the orbits included as thoroughly reliable in our published lists. On May 19, 1893 a presumably new planet was discovered at Nice and given the provisional designation 1893 Z. From observations extending over more than two months an orbit resulted resembling the orbit of Andromache, except for the mean motion which was found to be at first 617″ and later 610.″

The following possibilities existed then with reference to Andromache and 1893 Z. (1) the planets might be identical, the adopted mean motion of 550″ of Andromache being erroneous. (2) The planets might be identical and the mean motion might have changed from the smaller value of 550″ to the larger value of 610,″ passing through 598″ which is twice the mean motion of Jupiter. If this were true a phenomenon of the utmost importance to science, known as libration, would come under consideration. (3) The planets might not be identical.

A study of the observations of Andromache revealed an uncertainty of the comparison star of one of the fundamental positions. From the orbit of 1893 Z positions were then calculated for the dates of observation of Andromache in 1877 and a satisfactory agreement found. The identity of the two planets was thus established in 1893–94. At the same time the hope for the occurrence of the first case of libration for a planet of the group 1/2 was shattered, for the mean motion in 1877 turned out larger by several seconds than the 610″ calculated for 1893. On the basis of these computations Andromache was also found on some Heidelberg plates taken in 1892.

The close commensurability of the mean motion with twice Jupiter's mean motion made it apparent that a satisfactory determination of the perturbations of this planet by Hansen's method was entirely out of the question; in fact impossible. After the revision of von Zeipel's tables for the Hecuba group, to which I have already made reference, this, our most difficult and striking case of the Hecuba group, was selected as a test case for our tables. After applying our perturbations, the least squares correction was based on ten oppositions from 1894 to 1907 with eminently satisfactory results, not only for these thirteen years but also for the 1877 oppositions, seventeen years before, and for a subsequent opposition in 1911. According to Miss Sophia H. Levy's computations, the perturbations of this planet are the largest we have experienced, the coefficient of the largest term in the perturbation of the mean anomaly reaching nearly 30°. For the 1877 opposition the perburbation in the mean anomaly is 24°. These amounts generally produce double the displacement in geocentric position at opposition and yet an approximate right ascension and declination published by Wolf in 1911 is represented by our theory to within 4′ of arc in right ascension and less than 1′ in declination. A further very slight revision of the theory is contemplated before publication to include some terms of higher degree depending on the very large eccentricity of 20°. This revision will make this representation still more satisfactory, although the larger part of all outstanding differences is due to perturbations by Saturn.

Even then our representation is much closer than was to be expected. Another approximate position has recently been published by Wolf for January 16, 1914. This is equally well represented, showing that the theory has definitely been verified by observation. The importance of this result lies in the fact, that with the most difficult case of the Hecuba type conquered, the revised tables of von Zeipel now provide a ready and accurate means of representing the motions of all planets of this type at present known. Confirmation of this statement is afforded by our subsequent work on (104) Clymene, (106) Dione, and (168) Sybilla of the same group with mean motions of 634″, 629″, and 572″.

But this planet has another striking significance. To prevent its loss, pending the computation of its theory under the Watson Fund, Berberich of the Kgl. Rechensinstitut, Berlin, has applied the laborious process of special perturbations by Jupiter and Saturn for the determination of osculating elements. The extent of his unpublished work was not known to me, until some years ago I addressed an inquiry to Berlin in regard to any unpublished data, particularly the Saturn perturbations.

Among the unpublished data are osculating elements for epochs in 1877, 1892 and from then on for practically every year until 1910. According to these elements the mean motion in 1877 was 617″.7. In 1892, fourteen and a half years later, it was 614″. For three and a half years it oscillated about this figure and then in another fourteen and a half years to 1910 gradually diminished to 608″.2, the last figure available. Thus in 1910 Hecuba had already encroached well within the previously known gap about 600″.

In von Zeipel's theory a transformation of the given osculating elements is made at the outset with the aid of the general tables for the group 1/2. The resultant elements which are retained as constants until the theory is completed, are not exactly mean elements in the sense defined above, but they approximate the same. Since from 1877 to 1910, or in thirty-three years, the osculating mean motion has gradually diminished by approximately 10″, it might be supposed that our own approximate starting mean mean motion would lie somewhere within this range, or at least be less than the original maximum value of 617″.7 in 1877, but as a matter of fact our approximate mean mean motion is larger by 1″.8. The numerical developments indicate that the mean motion may continue to diminish. Whether the mean motion of 598″ will actually be passed, will form a significant problem of research in the theory of motion of minor planets. But the following facts stand out prominently even now: (1) The extent of the hitherto known gap about 600″ has been greatly diminished; (2) stability has not been impaired thereby as our representation of the 1914 observation shows, for which the perturbation in the mean anomaly has now diminished from the original 24° in 1877 to a comparatively small quantity. (3) E. W. Brown's conclusion stated in his vice-presidential address before the Section A of the A. A. A. S. that if instability exists at or near 598″, it must be for mean motion exceedingly close to that figure has been verified, and (4) if Andromache's mean motion should pass through 598″ stability will probably not be impaired, because our developments indicate no discontinuity for that condition.

Another planet to which I desire to refer is the lost planet Aethra. From investigations by Dr. Dinsmore Alter the cause of the loss of Aethra is due to the fact that the elements on which extensive search has been based for several decades are wholly unreliable, although published to seven figures. In fact the observations can be represented by orbits ranging in mean motion from 800″ to 900″ and more. All that can be said about this planet at the present time is that it may be anywhere at any time within a belt of the celestial sphere 2°20′ wide and that its rediscovery and identification will be a matter of chance, unless it is located on the supposition that an unknown planet discovered in 1913 at the Lowell Observatory, but observed only once, was Aethra. An orbit representing all the original observations of Aethra from June 13 to July 5, 1873, as well as the position of the unknown object in 1913 would make the average mean motion in the interval of forty years equal to about 883″. Details of the investigation and a search ephemeris extending to March 1 of this year, were published in a Lick Observatory Bulletin in November, 1915, but war conditions in Europe and unfavorable weather in California seem to have prevented adequate search. (An exhaustive, but unsuccessful, search has since been reported by Dr. Anna Estelle Glancy in *Astronomical Journal*, **31**, No. 723, April, 1918.)

With reference to the other Watson planets it may safely be asserted that excepting the need of further observations of Andromache for reasons stated above, all of these planets may be stricken from any observing program for decades to come, since they may readily be identified if science so demands, on the basis of the orbits and perturbations determined under the auspices of this academy.

CONGENITAL ARTERIO-VENOUS AND LYMPHATICO-VENOUS FISTULAE. UNIQUE CLINICAL AND EXPERIMENTAL OBSERVATIONS.

By William S. Halsted

Medical School, Johns Hopkins University

Read before the Academy, November 18, 1918

A. Advance of proximal arterial dilation conforming to shifted position of fistula.—Thanks to the assistance of highly competent secretaries I have abstracts of about 400 cases of arterio-venous fistula. These have been studied with special reference to occasional observations on the dilation of the arteries. In 52 instances proximal dilation of the arterial trunk has been noted. I am quite sure that in almost every instance in which the fistula had existed for two or more months proximal dilation of the artery would have been demonstrable if looked for.

Congenital arterio-venous fistula is rare, particularly so when unassociated with naevus. We have been able to find reports of only 3 cases without and 6 with naevus. Of the former none has been cured, unless we except a case (von Eiselsberg's) in which an attempt to cure a fistula between the popliteal artery and vein was followed by gangrene, necessitating amputation of the thigh.

The following case, unique in several particulars, is reported to record the arterial changes observed at 2 operations, the second performed six and one-half years after the first. The patient, a girl aged eleven years, was operated upon November 15, 1911, for a congenital arterio-venous fistula below the angle of the jaw on the right side. After the removal of enormously dilated veins (lantern slide) it was found that the fistula was between one of these and the external carotid artery near the bifurcation or ventricle of the common carotid. Fortunately a careful note was made of a very small anomalous, ascending branch given off from the external carotid just proximal to the fistula (lantern slide). There was great dilation of the common carotid artery and of the external carotid proximal to the fistula, whereas the internal carotid was surprisingly small (lantern slide). The vessels concerned in the fistula formation were excised, the aberrant artery happily being spared. The child was relieved by the operation of very distressing symptoms, but a few