

Ongoing News

concerning the
long suspected new primary planet
of our Solar-System.

NOVEMBER, 1801.

On the 15th of October, I obtained a letter from Palermo by way of Vienna, from the director of the Royal Sicilian Observatory, DR. GIUSEPPE PIAZZI, from the 1st of September of this year, in which this renowned astronomer had the kindness to send over his small treatise on the new star discovered by him. We know that the thirst for knowledge [*Wissbegierde*] of our astronomers, and the curiosity [*Neugierde*] of our non-astronomical readers will not be better or more suitably assuaged, than if we communicate a short and choice excerpt from this small Italian print, which will probably not come so easily or so quickly into the German bookselling trade.

This [*zwey Bogen starke*] treatise bears the title: *Results of the observation of the new star, discovered on the 1st of January at the Royal Observatory of Palermo.* GIUSSEPE PIAZZI [*Ghierico regolare?*]. Director of the same. Presented to the supreme general deputation of [*Studj.?*]. In Palermo, 1801. With the royal stamp.

For nine years, PIAZZI had occupied himself with a revision of our most eminent star catalogue¹. On the first of January of this year, at about nine in the evening, according to his custom, wishing to observe, among others, the 87 stars of Taurus according to LA CAILLE's star catalogue with his meridian telescope², he saw another small star which preceded them. Thus, he observed it, as he tended to do, as long as this small star did not hinder his actual superior main observation³. The light of this small star was faint; it appeared to him to be of a color like Jupiter, but it appeared to him as a common magnitude 8 star, such that it aroused entirely no suspicion, and he presumed nothing of its singularity.

¹*M. C. III.*

²*M. C. III. B. S. 612*

³This star of LA CAILLE's only went past the meridian 61 seconds of time before the planet, and was 15' 19'' more north than it. Its apparent right ascension was = 52° 3' 1'' at this time; apparent northern declination was 15° 53' 3''.

When he repeated the same observation the following day, he found that neither the time of culmination, nor the declination of this star were in accordance with the observations made the evening before. His first suspicion therefore, fell upon his first observation, where some error could have crept in. However, a small presentiment came over him, that this star could very well be a foreign visitor. On the third evening, his conjecture turned into certainty, as he considered himself assured, that this small star were no fixed star. However, before he said anything thereof, he awaited the fourth evening, for more surety, upon which he had the satisfaction of seeing that this star had continued to move according to the same laws as the days before.

From the 4th until the 9th of Jan., the sky was covered. On the evening of the 10th, the star revealed itself in the field of the meridian telescope at culmination, near four other stars of approximately the same magnitude. Uncertain as to which among these were the new wanderer, he observed all of them, and after he had compared them to the same observations on the following evenings, it was easy for him to recognize his new star again, by its singular motion.

Now the desire arose in him to be able to examine and investigate this singular star with more leisure, outside of the meridian circle. He could not follow it through its entire circuit, because he could not bring this instrument outside of the area of the meridian [*Mittags-Flache*], and his recently begun meridian observations in which he was engaged, could be interrupted. However, despite all possible efforts applied, neither could he, nor his assistant D. NICCOLA CACCIATORE, nor D. NICCOLA CARIOTI, beneficiary of the royal chapel [??- k. Kapelle], find this newcomer and distinguish it from the rest by which it was surrounded, even though both have very sharp vision, and are quite knowledgeable in the starry sky; neither with a cometseeker, nor with an achromate with a four-inch aperture. Thus, he had to make do solely with the view which his meridian instrument would permit him over the short time interval of 2', which the star spent traveling through the field of these telescopes. The former magnifies the circle 50 times, with a 3-inch aperture, the meridian telescope 80 times; by this latter one D. CARIOTI estimates the new star to be a star of between the 7th and 8th magnitude. In order to be all the more sure of the observations, PIAZZI observed the star through its entire circle, while in the meantime D. CARIOTI took the culmination with the meridian telescope. In this manner, they carried on their observations until the 11th of Feb., after which time this star had drawn so very near to the sun, that it was no longer possible to see it in the meridian circle. PIAZZI had indeed resolved to follow this star outside of the area of the meridian by means of the azimuth; only a heavy illness, which overtook him on Feb. 13th, prevented him from carrying out this intention, and thus he could not make any further observations. He is of the opinion, however, that the observations at hand were sufficient, in order to be able to pronounce a judgement concerning the character of this star with some certainty.

Since, naturally, PIAZZI considered his new star to be a comet: he first tried the calculation of a parabolic orbit, which he based upon the observations from the 1st and 19th of Jan., and the 11th of February. Because he found that these elements could in no way satisfy the remaining observations, he tried a second

parabola with the other observations, with which he was no more fortunate; he always encountered the same difficulty of bringing them into agreement with the rest of the observations. On closer examination, he realized that every observation conformed to absolutely no parabola, and consequently the described part of the orbit of this star could not be represented by a parabolic curve, which a comet initially describes, in any satisfactory way. From the parabolic hypothesis, he proceeded to a circular one. He found two radii of a circle; the one 2.7067, the other 2.6862, both of which represented the complete observations far better than any of the other parabolas.

Naturally, a circular orbit must lead to an elliptical one, and therefore, a planetary orbit. However, PIAZZI preferred the circular orbit at hand, because the arc observed up until now is much too small, such that one might therefore be expected to await very certain elliptical elements; at least he believes that they are not much more certain than those, which one would find in a circular orbit.

On the 10th of Jan., the star which was retrograding up until then, became direct. From the observations on these days, PIAZZI calculated the time of its stationary point, and hence, the elongation as 4th sign, 4° , whereupon he furthermore found the mean radius of a circular orbit to = 2.9352. The difference between this and any radius which comes from the observations from the 1st of Jan. through the 11th of Feb., would assume a very large eccentricity of the elliptical orbit; on the contrary the observations themselves appear to indicate only a very small one. Only it is well known that this radius cannot be calculated with great precision, especially in the present case, from the elongation of a planet to the time of its stationary point, as we have just noted this in the IVth volume of the *M. C.*, pg. 166.

After properly fitting adjustments, PIAZZI finally found the following parts of the determination of a circular orbit as an end result:

Radius of the circular orbit	2.6862.
Motion of the orbit from Jan. 1 until Feb. 11th $9^\circ 2' 29.7''$	
1801 epoch	2 S $8^\circ 46' 22.0''$
Motion in 100 days	0 22 6 33.7
Longitude of the ascending node	2 20 46 48.0
Inclination of the orbit	10 51 12.0
Mean distance from the time of the stationary point . . .	2.9352
Tropic revolution from the above mean distance . . .	5.03 years
derived from KEPLER'S laws.	
Sidereal revolution of the motion of the orbit	1628.27 days
Apparent diameter at the mean distance	
of the Earth from the Sun	$19''$
Solid contents (volume)	$1\frac{1}{3}$ of the Earth
1802 opposition around the beginning of March.	

PIAZZI found the diameter of the star in the following way: in the first observation, he brought the star into the horizontal cross-hairs, and it was

almost entirely covered. Since this edge appeared below an angle of $6''$ to his eyes: he estimated the diameter of the star as somewhat larger, namely $7''$. In the last observations he was unable to pass a judgement concerning its diameter because of the ever-cloudy condition of the atmosphere.

The following table contains his complete improved observations, as we have already partially communicated them in the fourth volume of the *M. C.*, pg. 280. However, here they appear revised, calculated by the author himself, and compared with his above circular orbit. The differences of longitude and latitude, with their signs added to the observed geocentric latitude and longitude, give that which was calculated from the elements. The observations denoted by two points (:) are somewhat doubtful; those denoted by four points (::) are very uncertain. Instead of the time of observation set in decimals of days, as PIAZZI had done, we have given them in hours, minutes, and seconds.

The agreement of a longitude of this star, calculated in a circular orbit, with the observations; the motion of the same in the zodiac, from which it distances itself a bit more only in its great latitude; its position between Jupiter and Mars' orbit; all of these seemed to PIAZZI to indicate, that this star was a true planet, and were indeed the same, as many astronomers had already anticipated, and as Professor BODE had indicated in his writings since 1772. Also, PIAZZI attributes it to the extraordinary apparent diminutiveness of this star, and its great latitude, that it had evaded every astronomer who had occupied themselves with observations of the stars of the zodiac. He finds it not improbable, that the same star could be come upon somewhere in the original observations of LA CAILLE's or TOB. MAYER's, preserved at Paris or Göttingen. For in the star catalogue, this smaller star, located by the astronomer, which was only observed once, and which could never be found by PIAZZI again, regardless of how many times he had retried it, and at how many different times he had searched the heavens. In his work *Positione delle Fisse*, currently in the process of being printed, which should appear soon, he will give an entire catalogue of such missing stars,⁴ which will facilitate the seeking out of the new star tremendously. This find will, admittedly, be of the utmost value for the theory of this planet, just as FLAMSTEED's and MAYER's observations of *Uranus*.⁵ PIAZZI is

⁴Catalogues of such missing, and absent-from-the-heavens stars are found in many various volumes of the Berlin Astronomical Journal; an excellent catalogue of 146 stars which are absent from the heavens, given by LA LANDE, is found in the *Conn. d. t. année VII* (1798. 1799.) pg. 355, and which appear, however, in the catalogues of FLAMSTEED, HEVELIUS, LA CAILLE, and TOB. MAYER. Also, page 360 announces an abundance of printing errors, which appear in the star catalogues of these astronomers. Many stars were given as having disappeared, which had never stood in the heavens, and had only cropped up as errors of writing, calculation, or printing. Therefore, Miss CAROLINA HERSCHEL's revision of FLAMSTEED's observations and guesses will also be of great use, which was dispatched on order of and at expense to the Royal Society of the Sciences in London, and is surely in the hands of all astronomers.

⁵Prof. BODE had the supposition that the class I, nr. 7 nebula, which HERSCHEL had observed on the 23rd of Jan., 1784, but had not been able to find again after that (Berlin Astr. Journal, pg. 172, 1791.), had been PIAZZI's star; it must have stood at that time at the northern side of the Virgin [Virgo], and the longitude agrees quite well, only the latitude does not want to harmonize, for it were, that HERSCHEL had miswritten, and had meant to put

not disinclined to agree with the opinion that it would be able to give this similar planet in space much better yet. **However, since stars below the 7th magnitude tend to be observed by astronomers only rarely, or when it does occur, they are satisfied at most with one, or a few observations: it is thus nearly impossible to ever rediscover them. If Piazzi had not made it law to observe each star four, five, or six times, and repeatedly as well; he himself thus doubts whether he would ever have discovered this new star. Had he not immediately examined his observations of this star, from the first and second of January, a fairly long time having passed, and first found by hand, as it tends to occur quite often, that they do not agree: then he will admittedly have had to search in the same location in the heavens. Since, however, he had not found it again (as it had to have come as well): thus, he would have placed it among the doubtful stars without any further ado, and consequently, would never have come upon its track again, as had only too often happened to him with other stars which he could not continue to follow, due to disadvantageous weather.** [Help!]

Hardly had the observations from the 1st and 23rd of January come before the view of ORIANI, BODE, and v. ZACH, which PIAZZI had made known with the conditions that the star had gone from retrograde, to direct motion on the 10th of January, but that they immediately came upon the supposition, that this star were nothing other than a new planet. Hence, they calculated approximately the same circular orbit which PIAZZI had also found on his side. Only, since after the 23rd of Jan., the star began to decrease very noticeably in magnitude and light, PIAZZI, uncertain as to whether he should attribute this phenomenon to its rapid distancing from the Sun, or to the obscure, and ever-cloudy conditions of the atmosphere, wavered in his first-conceived opinion; he began to doubt in its planetary-like nature, and to consider it as more probably a comet, than a planet. Only the calculation of his entire observations in a circular hypothesis could disperse his doubts. However, at the same time, due to other more pressing affairs, and especially due to his very poor health condition, he was not in a position to occupy himself with calculations of the same. As he had recovered in April, and now thought to take up this work, he fell into a sickness for the second time, which he had incurred from drawing a meridian line in the main church of Palermo, and which had put him into a much worse condition than the one before. Uncertain as to when he would ever be able to take up this work again, and prompted by some of his correspondents, he sent his complete observations to LA LANDE, ORIANI, and BODE. This latter one responded to him, that these just received complete observations only strengthened him in his opinion concerning the planetary nature of this star still more, however, for that reason, he wondered that PIAZZI had subsequently changed his opinion

N. instead of S., northern instead of southern for the difference of declination. However, this circumstance also makes BODE's supposition strange, because HERSCHEL called this nebula *remarkable* and put it in class I; he also supposed, that the same were a *more considerable* telescopic comet: only, PIAZZI's star could certainly never appear as a *more remarkable* and *more considerable* nebula.

again, and now considered this star to be a comet, since he had been inclined to perceive it as a planet in his first letter to ORIANI. However, P. believes that if Prof. BODE had known of the swift decrease in light of this star, he would have perhaps been disposed to the same doubt. In the meantime, PIAZZI also feared that if we should not succeed in finding this star again, a doubt may always remain concerning its nature. He maintains that the rediscovery, as much due to the uncertainty with which the elements of its orbit are determined, as especially due to the extraordinary diminutiveness of this star, is not such an entirely easy thing. He does not believe that it were possible, due to its great distance from us, to see it in the summer months. At the beginning of November it will be much easier to discern, and at the beginning of March, where it passes into opposition, it will be the best and the most useful to observe. He hopes, that this wanderer will not be able to easily elude the pursuit of astronomers.

Thus, should this star be rediscovered by him or by some other astronomer: then PIAZZI thinks, after the example of a HALLEY, HEVELIUS, BODE, or emphHerschel, whose praiseworthy names have been affixed to the heavens by a KARL II, a SOBIESKY, a FRIEDRICH II, a GEORGE III, as gaurdians of astronomy, to have a similar one, if no stronger right to it, to thereby immortalize the name of his king and benefactor of the Palermo observatory, that he names this new planet *Ceres Ferdinanda* after him, of which he has already notified all of his correspondents as well, that he will avail himself of this apellation in the future for the honor of the king of Naples; he then concludes his treatise with the following two Latin verses of the [Piarissten-??] MICH. ANG. MONTI:

*Telluris patriae ductura a Principe nomen
Astra inter Siculis fulsit ab axe Ceres.*

A small attachment follows PIAZZI's treatise, in which PIAZZI announces that ORIANI and other astronomers had communicated their calculations of the star to him. He therefore cites both ORIANI's parabola and BURKHARDT's parabolic, circular, and elliptical orbits as entirely the same, as those which already appeared in the July issue of the *M. C.* pg. 59-61. At first glance, it seems in particular that PIAZZI named Dr. BURCKHARDT as the calculator of its elliptical, but not its parabolic and circular orbit, which he seems to ascribe to other astronomers, unknown to him. However, this can be explained in the following manner. P. so apologizes, that he had not been able to make out the names of the astronomers who had calculated the parabolic and circular orbits in the two German letters that ORIANI had sent to him. These two German letters were none other than pg. 59-62 of the July issue of our *M. C.* On the preceding page, 58, which Oriani, however, had not sent, Dr. BURCKHARDT's name actually appeared; upon those, which PIAZZI received, however, this name was mentioned with the elliptical orbit; therefore it so happened that PIAZZI could not have known from whom the elements of the parabolic and circular orbits originated, which is also why he has also only suggested the names of these astronomers, out of respect.

From the same letters, PIAZZI saw that Dr. BURCKHARDT fostered some doubts concerning the accuracy of the transcription of his observations, and

thereby supposed some errors to have crept in. He avows that this was indeed the case with the first copy; however, he had subsequently sent an improved copy to LA LANDE, ORIANI, and BODE, which appeared identical to those, according to which he had made calculations, and which we have inserted in the fourth volume of the *M. C.*, pg. 280. In order to anticipate the wishes of those astronomers, who have taken such a great part in his discovery, as much as it stands in his powers, and in order to dispel the smallest doubts against his observations: he had again take up their reduction entirely anew. Instead of a less-than-exactly determined star comparison, which he had used at the outset, he had chosen a better determined one, had carried out calculations from its proper motion and from the declination of the instrument, and thereby used the very care, which one tends to employ, when one wishes to attain the utmost precision. Irregardless of which, the only very insignificant differences of the preceding occur in the right ascension, which could have almost entirely no influence, or at most, a trifling one, upon the calculations of the orbits, which is why he originally held precision in the reduction of the observations as excessive. In consequence of this last rigorous revision, in order to maintain the utmost exactness, $1.5''$ must be subtracted from the first four right ascensions, and just as much added to those from the 10th, 11th, 14th, 19th, 21st, 23rd, 28th, 30th, and 31st of January and February; $3''$ must be subtracted from the right ascensions of the 3rd and 8th of February. P. had mainly observed the star with both of his instruments, in the meridian telescope, and in the circle of the meridian; but he had always preferred the meridian telescope for the right ascension, if he was able to make the complete observation with this instrument; when that was not the case, however, he had to take the mean between the observations from both instruments. However, the difference never went above $0.2''$, excepting the 9th of Jan., upon which day he found a second of time more on the circle, than on the transit instrument. Concerning the declinations, he had found entirely nothing to improve them. Moreover, if someone wished to see his original observations, he were thus willing to communicate them at the drop of a hat, with the greatest satisfaction. However, the same observations will appear in print in the next volume [VI] of the Palermo observatory, along with the rest of his observations from the year 1794.

This is the only true and authentic account of the discovery of this long suspected, and now possibly discovered primary planet of our solar-system, which we have received, sent from Palermo by the discoverer himself, and which we have communicated in faithful epitome to our readers here. There are indeed, appearing in some political newspapers, completely contradictory and misleading reports, which stir up doubt and misunderstanding, and concerning which the editor of the *M. C.* has invited questioning quite repeatedly, verbally, and in writing. Because it will be impossible to reply to each questioner in writing: we therefore find it doubly compelling to select the present public means, in order to dispel all of these acquired and unfounded rumors before the great public.

Certain newspapers should have acknowledged that Prof. SEYFFER at Göttingen had received a letter from Palermo, from PIAZZI, in which he announces to him that he considers the new star that he discovered, which he maintained was a

planet up until now, as henceforth, a comet. Only, those who have read the above account of the discovery attentively will soon perceive, that everything was due to a mistake with reference to the time, and to a confusion of the data; since PIAZZI could very well have written this opinion to Prof. SEYFFER on a day when he had, however, been late to mail the letter, as is easily possible, and it arrived at a time when PIAZZI had again changed his mind, having come to a better representation according to the calculations employed, as he himself recounts this in his treatise. At most, PIAZZI's letter to Prof. SEYFFER proves that the data of this letter (Aug. 4th) had been either miswritten, or falsely given, or that this letter had been completely misunderstood. For, already at this time, no German astronomer any longer doubted the planetary nature of this star; the calculation of a parabolic orbit had long been abandoned, only occupying themselves with circular orbits, BURCKHARDT had even already calculated an elliptical orbit. Therefore, the report from SEYFFER's letter certainly came at a bad time in political newspapers, and the sender therefore did not particularly like that which preceded astronomy, and which had been known in the history of this remarkable star, to have a lame messenger not insert this news in the newspaper, whereby it had only mistakenly led the public.

This news was contradicted in another newspaper, the entire letter of PIAZZI's to SEYFFER cast into doubt, and pronounced as apocryphal. We openheartedly profess our position, that we do not find the slightest reason to doubt this letter. For, why should PIAZZI not write to such an excellent successor of our most renowned German astronomer, TOBIAS MAYER's, the professor of astronomy of such a world-renowned University as Göttingen, as to the renowned astronomer of the Royal Berlin Observatory! Much more, is highly probable, since PIAZZI spoke of and suspected TOB. MAYER's anticipated original observations in his treatise, that an observation of his new star could very well be extant among them, just as Uranus itself had been found. It is therefore very natural to think, that for this reason, he would have asked the primary source. Therefore, it must coax a smile out of many readers, when it is read in the same newspaper, how it had ensured, as a notoriously known thing, in such a childlike and vain manner, that Prof. BODE were the *sole* one among all German astronomers who stood in *exclusive* correspondence with PIAZZI. However, we must candidly contradict this report, since it is *reliably* known to us that PIAZZI stood in correspondence with *three other German* astronomers. Hence, we could not concede, that such an absurd and unfounded report could be published by our highly esteemed friend BODE; and it cannot be a matter of indifference to BODE himself either, when such unbidden, officious minds are permitted to set down the same paltrinesses in public letters in his name, which could cast a dubious and false light upon worthy and unassuming scholars.

In the same publication which reported upon the misunderstanding with SEYFFER's letter, appear some very curious falsities. For example, it is thus ensured, that German astronomers had assigned the name *Hera* to this new planet. Only, to whom among our readers can it be unknown, that this name was *proposed* as an appellation by the Baron of Gotha many years before the discovery of this star? For this reason, we need only call upon our first reports

of this suspected planet in our *M. C.* volume III, pg. 621. And if we have used this name, it occurred very rarely, and purely for abbreviation, in order not to have to always repeat the long circumlocution of “the *newly discovered star of PIAZZI’s.*”

Since Prof. PIAZZI has baptized his child, and henceforth named it *Ceres Ferdinadea*, of which he has the right to, as the first discoverer, all of his correspondents are also entreated by him to use this appellation; thus, we subscribe to this right-proper appellation on our side with sincere and utmost satisfaction, because the King of Naples, undisputed as the devoted guardian and promoter of astronomy, and as the magnanimous sponsor of a new state observatory, is so much the more due our most thankful gratitude, since he did not only *initiate* the construction of an observatory, but has also *completed* it; has not just purchased the most splendid and costly English instruments, and allowed them to be put in boxes and crates in junk-rooms, but rather, where they belong; these excellent instruments were entrusted not to awkward and unskillful hands, but rather, a scholar of established merit and expertise, once and for all capacitated to forward the costs of printing his work and observations to the royal expenses. For thus, in such a short time the most valuable and radiant fruits have arisen from the Palermo observatory, which presents the scholarly world with multiple volumes of the most appreciable observations, and this temple of the Sicilian Urania will be immortalized by its sponsor and priest for the remarkable discovery in the punctually commencing century, for thousands of years. Thus, PIAZZI justly says in his treatise that FERDINAND IV is rightly due a place in the heavens as much as other protectors of astronomy.

In the preceding issue, pg. 367, we have already announced Dr. OLBERS’ reasons why the positions of this new star calculated in a circular hypothesis rather holds the mean between those which we were able to calculate with an elliptical orbit. Dr. OLBERS’ proposal, that one proceed from the points calculated with the circular elements in the seeking out of this new planet, and should scan back and forth a few degrees through the same parallel of latitude, is undisputedly the sole and best procedure, by which this search can be advisedly and systematically carried out. Thus, we have resolved ourselves to hereafter calculate a small ephemeris of the course of this planet up until the end of this year, according to the above circular elements of PIAZZI’s, which agree rather exactly with the entire series of his observations, and to thereby do a small service to all astronomers and lovers of astronomy. The titles of the columns are widely recognized to give their contents. We must only more closely expound upon those, above which the title reads: *Proportion of the perceived brightness.*

PIAZZI and CARLOTI have estimated the light of the new star when it first appeared as approximately a star of the 7-8th magnitude, as our readers have seen above. Subsequently, and toward the 11th of February, it appeared still smaller to PIAZZI, and to have decreased considerably, which he ascribed in part to the obscure and cloudy conditions of the atmosphere at the time. Only, if we calculate the distance of this planet from the Earth for both epochs with the circular orbit: it thus follows, that the January 1st distance of this star from us will have been = 1,924, and the Febr. 11th will have been = 2,432; that

is: the latter will again distance itself from us by nearly $\frac{1}{4}$ the distance of the former. The light, or rather, the *apparent clarity* of this planet [*Weltkörpers*], therefore, must have been considerably decreased as well. This apparent clarity (*claritas visa*, by which we perceive a planetary body, not radiant on its own, depends upon the quantity of light which the planet *receives* from the Sun, and upon the quantity of light, which is *sent* to us. The first is in direct proportion to the illuminated surface, and in the inverse proportion of the square of the distance to the Sun; the second is in the inverse proportion of the square of the distance from the Earth. We here abstract from the modifications by which the striking and reflecting light can be subdued, depending on whether the specific physical construction of the surface of the planet catches, engulfs, or reflects the beams of light. Our view is purely to compare the currently apparent clarity of the new planet with the that of earlier one, which he had had from its first appearance, when PIAZZI had estimated it to be a star of the 7-8th magnitude.

Thus, let the diameter of *Ceres* for Jan. 1st, 1801 = D ; whose distance from the Sun = S ; from the Earth = R ; its apparent clarity = H . Likewise for any other epoch of diameter = d ; distance from the \odot = s ; from the \oplus = r , apparent clarity = h : then

$$H : h :: \frac{D^2}{S^2 R^2} : \frac{d^2}{s^2 r^2}.$$

We now set the large clarity, which has been observed up until now, $H = 1$: thus, since we must set the diameter of the planet equally as large in the circular hypothesis, the proportion of the (*Claritas visa*) will be $\frac{h = S^2 R^2}{s^2 r^2}$.

According to this formula we have calculated the proportion of the appearing clarity. It was = 1.000 on Jan. 1st, and = 0.625 on Febr. 11th. Our small *ephemeris* therefore gives to understand that we have anticipated the same clarity first towards the 10th of December as the 11th of February, and that, however, at the end of this, and the beginning of next year, this clarity will not for a long time yet reach that, with which this star had shone at the beginning of its discovery, which will first occur again towards the end of January 1802.

From the 19th until the 25th of November the planet will stand very near to the star $\theta\delta\Omega$; between the 25th and 31st of December it will be found in the vicinity of the very well known stars $\beta\delta\Omega$, and will actually remain parallel with the above star, as well as γ^1 and $\gamma^2\delta\Omega$, from Novbr. 1st through December 31st.

Dr. BURCKHARDT's elliptical elements give exactly the same positions for the *geocentric latitude*; but the *geocentric longitudes* are given as two to three degrees *greater*. Therefore, one would do much better, and go to work with more surety, if one kept to the latitude parallel, and not to that of the equators', since our instruments are only suited for these latter [ones]. For, if we assume an error or an uncertainty of two degrees in geocentric longitude, then this will affect the calculated *deviation* very strongly; e.g. it will change the deviation on Nov. 1st by around $2^\circ 11'$.