



XII. Result of the observations of the new star, discovered on the 1st of January 1801, at the royal observatory of Palermo

Joseph Piazzi

To cite this article: Joseph Piazzi (1802) XII. Result of the observations of the new star, discovered on the 1st of January 1801, at the royal observatory of Palermo , Philosophical Magazine Series 1, 12:45, 54-62, DOI: [10.1080/14786440208676025](https://doi.org/10.1080/14786440208676025)

To link to this article: <http://dx.doi.org/10.1080/14786440208676025>



Published online: 18 May 2009.



Submit your article to this journal [↗](#)



Article views: 2



View related articles [↗](#)

impossible to detach it by softening it. This hard mass is formed, in a great measure, by the lime crystallized in a confused manner; which it is more difficult to dilute in the water than the same earth in powder, or very much divided.

XII. *Result of the Observations of the new Star, discovered on the 1st of January 1801, at the Royal Observatory of Palermo.* By JOSEPH PIAZZI, Director of the Observatory*.

I. **H**AVING been engaged for nine years in verifying the positions of the stars as collected in the catalogues of various astronomers, I was searching, on the 1st of January 1801, among many others, for the 87th in the catalogue of the zodiacal stars of the abbé De la Caille, when I observed that this star was preceded by another, which, according to my usual custom, I wished to observe also, especially as it did not interrupt the principal observation. Its light was somewhat faint, and in colour resembled that of Jupiter, but like that of many others which in regard to their magnitude are usually placed in the eighth class. At that time no doubt arose in regard to the nature of it; but on the evening of the 2d, having repeated my observations, and finding that they did not correspond either in time or zenith distance, I suspected that some error had been committed in my observations on the preceding day. I then began to entertain some idea that it might perhaps be a new star. In the evening of the 3d my conjecture was confirmed, as I assured myself that it was not a fixed star. However, before I would speak of it, I waited till the evening of the 4th, on which I had the satisfaction of finding that it had moved according to the same laws which it had observed on the preceding days. From the 4th to the 10th the sky was overcast: on the evening of the 10th it appeared to me in the telescope, accompanied by four others nearly of the same magnitude. Being still uncertain in regard to the nature of the new star, I observed them all as well as possible, and, comparing these observations with others which I made on the evening of the 11th, I easily distinguished the new one from the rest by its motion. I was, however, exceedingly anxious to see it when not on the meridian, and therefore employed myself

* From *Risultati delle Osservazioni della nuova Stella scoperta il de' 1 Gennajo all' Osservatorio Reale di Palermo da Giuseppe Piazzi, Ch. Reg. Direttore del medesimo.*

in examining and contemplating it with great care; but, notwithstanding all my exertions, and those made by my assistants Cacciatore and Carioti, both possessing excellent sight, and accustomed to viewing the heavens with a night telescope and an achromatic of four inches aperture, it was not possible to distinguish it from many others by which it was surrounded. I was therefore obliged to be contented with seeing it on the meridian for the short space of about two minutes, that is, during the time it employed to pass over the field of the telescope; the other observations, made at the same time, not permitting me to remove the instrument from its position. To render these observations, however, more certain, while it was observed by myself with a circle, it was observed at the same time by Carioti with a transit instrument. In this manner, the heavens being constantly nebulous, and often obscured by thick clouds, our observations were interrupted till the 11th of February; after which the star having approached very near to the sun, it was no longer possible to see it at the time of its passing the meridian. I resolved then to search for it beyond the meridian by the means of azimuths; but having fallen ill on the 13th of February, I was not able to make any further observations. Those, however, which have been made, though not at the necessary distance to ascertain the real path which this star pursues in the heavens, are sufficient, as far as I am able to judge, to make known, with certainty, the nature of it; as may be seen by the results I have deduced from them.

The magnifying power of the telescope of the circle was 50, and that of the telescope of the transit instrument 80; whence it was judged by Carioti that the new star is of the 7th or 8th magnitude.

II. Combining in a parabola the two observations of the 1st and 19th of January with the third of the 11th of February, which were very good, I find the following results:

Perihelion	-	-	4 ^s 5 ^m 28 ^s 36 ^{''}
Long. 8	-	-	2 19 43 0
Inclination	-	-	10 34 0
Log. dist. of the perihelion	-	-	0.3713077
Passage of the perihelion July 1801	-	-	3.6985

As these elements, however, did not agree with other observations, I tried another parabola combining other observations, and experienced the same difficulties. Reflecting afterwards, that to make the two observations, that of the 1st of January and that of the 11th of February, agree, it was necessary to suppose 0.26 of difference between the radius vector corresponding to the first, and the other corresponding to the

second, I described mechanically my first parabola; and applying to it the longitudes and latitudes thus calculated and observed, it was easy for me to ascertain that the motion of this star could not, indeed, be represented in the arc of a parabola such as is nearly described by comets.

III. From parabolic hypotheses I proceeded to circular, and, having made a few suppositions, I found two radii, viz. 2.7067 and 2.6862, with each of which the observations could be represented much better than with any parabola; for, as the planets describe ellipses more or less eccentric, and not circles, it is to be supposed that our planet will not deviate from this law. I ought therefore to have continued my calculations on an ellipse; but as the observed arc was very small, the results would have been uncertain, and the labour tedious and painful. On this account I preferred the circle, especially as the elements obtained from the ellipse to determine the place of this star do not appear to me to be more certain than those afforded by the circle.

IV. On the 10th of January the star, from being retrograde, became direct. Setting out, therefore, from the observations of this day, I endeavoured to find its elongation while stationary, which was $4^{\circ} 4'$; whence we have for mean radius of its orbit 2.9352. But the difference between this radius and the other, obtained by the motion of the star between the 1st of January and the 11th of February, seems to indicate a great eccentricity, whereas the series of the observations seems rather to indicate that the eccentricity ought to be small: on the other hand, the diameter, deduced from the elongation of a planet while stationary, can never be very exact, and particularly in the present case.

V. During the first observations, bringing the star under the horizontal thread of the telescope, it remained, as it were, entirely covered; and, as the thread subtends to the eye an angle of about $6''$, I judged the diameter of the star to be somewhat greater, that is to say, $7''$. During the last observations I was not able to form any judgment of its diameter, on account of the darkened state of the atmosphere.

VI. RESULTS.

Radius of the orbit	-	-	-	2.6862
Motion in the orbit from January 1st to				
February 11th	-	-	9° 2' 29.7"	
Epoch 1801	-	-	28 8 46 22	
Motion in 100 days	-	-	0 22 6 33.7	
Longitude \odot	-	-	2 20 46 48	
Inclination of the orbit	-	-	10 51 12	
				Mean

Mean distance deduced from the time of
being stationary - - - 2'9352
Tropical revolution, deduced from the
mean distance according to the law of
Kepler - - - years 5'03
Sidereal revolution from the motion in the
orbit - - - days 1628'27
Apparent diameter at the distance of the
earth from the sun - - - 19"
Magnitude, $1\frac{1}{3}$ d that of our earth.
Opposition, about the 1st of March 1802.

VII. Table of the Mean Time, Right Ascension, and
Declination of the new Star as observed; together with
the Longitude of the Sun, and the Logarithm of its Di-
stance from the Earth.

	Days of the Month.	Sea Thousands of the Day in mean Time.	Right Ascension.	Declination.	Sun's Place.	Log. Distance of from ☉
January.	1	3635	51° 47' 48.7"	15° 37' 43.5" N	9° 11' 1' 33.1"	9,992617
	2	3606	43 27.7:	41 5'5	12 2 31.7	9,992629
	3	3577	39 36::	44 31.6	13 3 30.2	9,992641
	4	3547	35 47.2	47 57.6	14 4 29.0	9,992652
	10	3378	23 1.5:	16 10 32.0	20 10 29.5	9,992768
	11	3350	22 26.0	14 30. eft.	21 11 29.5	9,992794
	13	3295	22 34.5	22 49.5	23 14 28.0	9,992848
	14	3268	22 55.8	27 5.7	24 14 27.3	9,992882
	17	—	27 35::	40 13.0	—	—
	18	—	28 45::	—	—	—
	19	3136	32 2.2	49 16.1	29 19 14.1	9,993060
	21	3084	38 34.0	58 35.9	10 1 21 2.5	9,993151
	22	3059	42 21.3	17 3 18.5	2 21 55.1	9,993196
	23	3033	46 43.5	8 5.5	3 22 46.4	9,993242
	24	—	51 45::	—	—	—
	28	2909	52 13 38.3	32 54.1	8 26 45.5	9,993522
	30	2860	27 2.1	43 11.0	10 28 10.6	9,993645
	31	2837	34 18.8	48 21.5	11 28 55.5	9,993708
February.	1	2813	41 48.0:	53 36.5	12 29 36.6	9,993773
	2	2789	49 45.9	58 57.5	13 30 17.0	9,993851
	4	—	53 7 45::	—	—	—
	5	2719	15 40.5	18 15 1.0	16 32 13.9	9,994083
	8	2650	44 37.5	31 23.2	19 35 2.2	9,994328
	11	2583	54 16 23.1	47 58.8 N	22 35 41.3	9,994588

N. B. The observations marked with two points (:) are a little doubtful; those marked with (::) very uncertain.

VIII. Table of the Geocentric Longitudes and Latitudes of the new Star, both by Observation and Calculation; together with their Differences.

Days of the Month.	Geocentric Longitude.		Differ.	Geocentric Latitude.		Differ.
	Observat.	Calculat.		Observat.	Calculat.	
1	15 23° 22' 58".	15 23° 21' 59".	-9' 3"	3° 6' 32".	3° 6' 50".	+17' 8"
2	19 44' 8"	18 40' 2"	-64' 6"	2 13' 1"	2 27' 7"	+16' 6"
3	16 40' 3"	15 47' 1"	-62' 2"	2 57' 58".	2 58' 6".	+7' 4"
4	14 16' 5"	13 18' 9"	-57' 6"	53 44' 5"	53 48' 4"	+3' 9"
10	7 59' 4"	7 19' 5"	-39' 9"	28 50' 9"	28 31' 8"	-19' 1"
11	8 25' 7"	7 43' 4"	-42' 3"			
13	9 58' 0"	9 38' 9"	-19' 1"	16 49' 6"	16 21' 0"	-28' 0"
14	12 1' 6"	11 32' 6"	-29' 0"	12 47' 1"	12 23' 2"	-23' 9"
19	25 49' 4"	25 51' 5"	+2' 1"	1 53' 28".	53' 13"	-27' 0"
21	34 21' 8"	34 25' 4"	+4' 6"	45 58' 9"	45 31' 6"	-27' 3"
22	39 1' 8"	39 6' 7"	+4' 9"	42 18' 7"	41 51' 3"	-27' 4"
23	44 15' 6"	44 17' 4"	+1' 8"	38 39' 2"	38 12' 3"	-26' 9"
28	24 15 16' 0"	24 15 28' 1"	+12' 1"	20 58' 7"	20 32' 0"	-26' 7"
30	30 5' 4"	30 23' 0"	+17' 6"	14 5' 3"	13 43' 5"	-21' 8"
31	38 8' 6"	38 20' 5"	+11' 9"	10 45' 0"	10 22' 4"	-22' 6"
January.						
1	46 19' 6"	46 38' 0"	+18' 4"	7 23' 8"	7 3' 6"	-20' 2"
2	54 55' 6"	55 12' 8"	+17' 2"	4 0' 7"	3 47' 0"	-13' 7"
5	25 22 43' 5"	25 22 52' 8"	+9' 3"	0 54 19' 0"	0 54 9' 6"	-9' 3"
8	53 17' 9"	53 15' 6"	-2' 3"	44 42' 7"	44 50' 9"	+8' 2"
11	26 26 26' 1"	26 26 24' 8"	-1' 3"	35 47' 9"	35 50' 4"	+2' 5"
February.						

IX. The correspondence of the observed longitudes of this star with those calculated according to the circular hypothesis; its motion in the zodiac, from which it deviates only a little in its greatest latitudes; and its position between Mars and Jupiter; seem to leave no doubt that it is a real planet, and, in all probability, the very same deduced from calculation in the year 1772 by professor Bode, and announced to the Academy of Sciences of Berlin. That it was not then observed, though the zodiac was diligently examined by the best astronomers, ought to be ascribed chiefly, in my opinion, to its smallness compared with its distance from the earth and with its greatest latitudes; though it is not improbable that it was seen by the abbé De la Caille or Tobias Meyer. In the catalogues of the stars made by these two astronomers there are some observed only once; which I have not been able to find, though I searched for them repeatedly and at different times. If the original observations of Meyer are preserved at Göttingen, and those of the abbé De la Caille at Paris, they may possibly be yet discovered. In the end of my work on the position of the fixed stars, the printing of which, through the munificence of our gracious sovereign, is already far advanced, there will be given a catalogue of the lost stars, by which this research will be greatly facilitated.

X. It

X. It is the opinion of many astronomers, and I am inclined to agree with them, that there are several other planets of the like kind still undiscovered. But as astronomers either seldom observe stars beyond the 7th magnitude, or are satisfied with observing them once, or at most twice, there is reason to doubt whether these planets will be easily discovered. Had I not been accustomed to observe stars four, five, or six times, and even more, I should certainly not have discovered the one in question. Re-examining, as was the case, after a long time, the observations of the 1st and 2d of January, and finding that they did not agree, I should have searched in the same place of the heavens for this new star, and, not finding it, should have classed it among the number of the doubtful ones; a thing I have been too often obliged to do with others, the observations of which I was prevented from continuing by the inclemency of the weather.

XI. Oriani, Bode, and Von Zach, had scarcely seen the observations of the 1st and 23d of January, which I communicated to them on the 24th, adding at the same time that on the 10th the star from being retrograde had become direct, when they were of opinion that it was a new planet; and they next concluded that its elements were the same as those which I had conjectured. But as the star after the 23d began to decrease sensibly in magnitude and in light, being uncertain whether this ought to be ascribed to its receding rapidly from the earth, or rather to the state of the atmosphere, which after that period became more overcast and obscure; I began to entertain some doubts respecting its nature, and to believe that, in all probability, it was a comet and not a planet. It was only by calculating all the observations that my doubts were at length dissipated; but, on account of other avocations and the bad state of my health, I was not then able to pay proper attention to them. Finding myself somewhat better in April, I resolved to calculate my observations; but having brought on a second indisposition while fixing the meridian of the metropolitan church, and being reduced to a weaker state than before; being uncertain at what time I should be able to resume my studies, and anxious to communicate my observations to the above-mentioned meritorious astronomers, I transmitted them to Lalande at Paris, Oriani at Milan, and Bode at Berlin. As yet, however, I have received no answer but from Bode, who, on seeing my further observations, is more and more confirmed in his first opinion, and only seems a little surprised that, as I was much inclined in my first letter to Oriani to consider my new star as a planet, I should

I should afterwards have considered it rather to be a comet. But had I communicated to him also the circumstance respecting the diminution of its light, he would, perhaps, have entertained some doubts of the same kind.

XII. As this star has not been lately seen, some doubt will remain in regard to its nature; and it may be difficult to discover it again on account of the uncertainty of the elements of the orbit which it describes, and more particularly its very small size. At present it remains above our horizon the greater part of the night, being in the sign of Cancer; but it is at too considerable a distance for me to hope that I shall be able to distinguish it: and, on the other hand, I am destitute of the instruments necessary to search for it, with any certainty, out of the meridian. About the beginning of November it will be much easier to discover it; and as the first days of March, the time of its opposition, will be the most convenient period for observing it with advantage, I entertain great hope that it will not escape the research of astronomers.

XIII. However, whether this new planet be found again by myself or by any other astronomer, I shall be the more gratified, as, after the example of a Halley, a Hevelius, a Bode, and a Herschel, who have inscribed in the heavens the glorious names of a Charles II. a Poniatowski, a Frederick, and a George III. all illustrious patrons of astronomy, I think I have an equal, and perhaps stronger reason, for inscribing in indelible characters that of the august and magnanimous founder of this observatory, our sovereign Ferdinand, conjoined with that of the indigenous deity of this part of his kingdom, which he renders more lively and happy by his preference. I have therefore informed those astronomers who are my correspondents that I have given to this new star the name of *Ceres Ferdinanda*.

Telluris patriæ ductura à Principe nomen

Astra inter, Siculis fulsit ab axe Ceres.

Michael Angelus Monti, Scol. Piar.

SUPPLEMENT.

This short memoir not being yet published, I shall here subjoin the substance of a letter which I received from my esteemed friend Oriani; dated Milan, July 25, 1801, in which he communicates to me the result of his calculations, accompanied with those of some other astronomers who have done me the honour to employ their talents on my observations.

Oriani, who calculated in a parabola, found

8	-	-	-	2° 21' 48"
Inclination	-	-	-	9 33
				Perihelion

Perihelion	-	-	-	4 ^s 10° 16'
Distance of the perihelion	-	-	-	2.1045
Passage of the perihelion June 1801	-	-	-	21.07
Mr. ——— found also in a parabola,				
☉	-	-	-	2 ^s 20° 50'
Inclination	-	-	-	9 41
Perihelion	-	-	-	4 8 38 25"
Distance from the sun	-	-	-	2.21883
Passage of the perihelion June 1801	-	-	-	30 ^d 19 ^h 1 ^m

The difference of the three parabolas calculated by Oriani, Mr. ———, and myself, none of which represent all the observations, confirm still more that the motion of this star cannot indeed be represented in a parabolic arc, as I have said in my preceding memoir.

The same person who calculated the second parabola tried to make the observations agree with a circle. His elements are:

Radius	-	-	-	2.74
Epoch 1801	-	-	-	2 ^s 8° 16' 20"
☉	-	-	-	2 20 15 0
Inclination	-	-	-	0 11 21 0
Sidereal revolution	-	-	-	years 4 ¹ / ₂

In this circle the errors in excess amount to 2' 30", and those in defect to as much.

Burckhardt calculated in an ellipse, and found:

☉	-	-	-	2 ^s 20° 58' 30"
Inclination	-	-	-	0 10 47 0
Aphelion	-	-	-	2 8 59 37
Passage of the aphelion Jan. 1801	-	-	-	1.3328
Eccentricity	-	-	-	0.0364
Log. of the greater semi-axis	-	-	-	0.4106586
Sidereal revolution	-	-	-	years 4.13

In this ellipsis the longitudes and latitudes of five observations are represented pretty well, there being only the difference of a few seconds between calculation and observation.

The astronomer who calculated the parabola and circle above mentioned, and whose name I was not able to make out, in the few leaves printed in German with which Oriani favoured me, suspected that there might have been some error in the copy of my observations transmitted to him. This was really the case in the first; but I afterwards sent another corrected copy, as well as to Lalande, Oriani, and Bode, all corresponding exactly with that on which I founded my own calculations.

However, that my exertions may correspond with the interest generally taken in my discovery, whatever the result may

may be, and to leave no doubt in regard to the observations, I have again examined them; and in some places which were rather uncertain, and which I employed in my first calculations, I substituted others. I have also made an allowance for the deviation of the instruments, and employed all those precautions which are usual when great precision is required. The result of all these has been only a small difference in some of the right ascensions, which can have little or no influence on the ultimate results of the calculation, and for which reason I did not think great exactness necessary in the first reductions. According to this last rigorous examination, $1^{\circ}5''$ are to be deducted from the first four right ascensions, and $1^{\circ}5''$ to be added to those of the 10th, 11th, 14th, 19th, 21st, 23d, 28th, 30th, and 31st of January, and the 1st of February; and $3''$ must be taken from those of the 5th and 8th of February. With the transit instrument the observations were made, different times, with all the five threads; and these I always preferred to those made with the circle. I used those by the circle when I failed with the transit instrument; as on the first four days; and on the 13th of January, when the observations were not made with all the five threads, I took a mean of the observations made with the circle, and another of those made with the transit instrument. The difference between those with the former instrument and those with the latter, was never greater than 0.2 in time, except on the 19th of January, when I found $1''$ in time more by the circle. In regard to the declinations, I have found no corrections to be made. Should any astronomer, for his greater satisfaction, be desirous to see the original observations, I shall be happy to communicate them to him. They will be published in the sixth book of the *Specola Astronomica*, together with all my other observations made since 1794.

XIII. *Reflections on the new Primary Planet supposed to exist between Mars and Jupiter, and now in all Probability discovered.* By Baron VON ZACH, Lieutenant-Colonel in the Service of the Duke of Saxe-Gotha, and Director of the Ducal Observatory at Seeberg*.

THE existence of a primary planet, which, on account of the faintness of its light and small size has hitherto remained undiscovered, was, as far as I know, first conjectured, or at

* From *Monatlich Correspondenz zur Beförderung der End-und Him-melshunde*, June 1801.