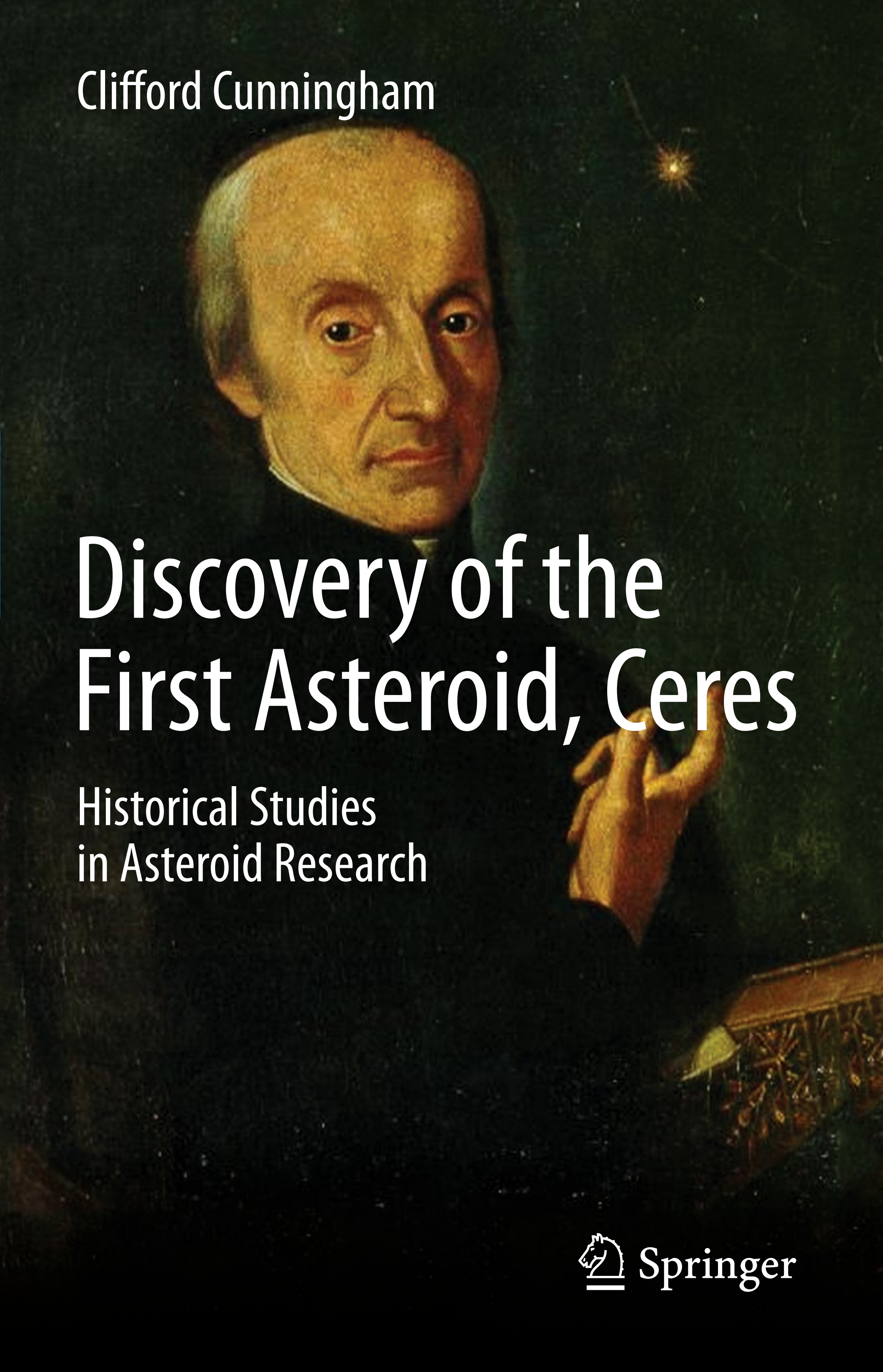

Clifford Cunningham

Discovery of the First Asteroid, Ceres

Historical Studies in Asteroid Research

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Heavily revised, this version of a 2002 book self-published by the author via Star Lab Press as The First Asteroid: Ceres 1801-2001 (09708162-2-7) includes new opening sections, newly translated documents that did not appear in the earlier version, and many revisions to correct errors in the original translations.

Cover illustration: Ceres, picture taken February 19, 2015, by NASA's Dawn spacecraft, from a distance of nearly 29,000 miles (46,000 km). (Credit: NASA) Painting of Piazzi pointing toward Ceres (credit to come)
Cover image courtesy of INAF-Osservatorio Astronomico di Palermo

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Foreword

Clifford J. Cunningham has been genuinely passionate about asteroids ever since I first met him. It is hard to explain to others why one cares deeply about such esoterica. As a graduate student at Stanford, I once heard the famed astrophysicist S. Chandrasekhar lecture about his work on a topic that seemed completely outside the mainstream of what was then considered interesting. Someone in the audience actually asked him why anyone of his ability would waste time on such an apparently useless matter. Chandrasekhar replied very cheerfully, “That is precisely why I have chosen to do so!”

This is Mr. Cunningham’s second book about the Minor Planets, following the publication of his well-received and much-utilized Introduction to Asteroids. In this new work readers are transported back more than two centuries to the end of the eighteenth century and the quest by the “Celestial Police” for the missing party in the Titius-Bode “law” of planetary distances. In his profusely illustrated and well-documented history of the discovery of Ceres on the first day of the nineteenth century, Mr. Cunningham brings to life a vanished world of astronomy and astronomers. The personal anecdotes that are included in his narrative are particularly useful in understanding the motivation and character of the principal players.

In that spirit, the reader also deserves to know something of the author. Mr. Cunningham was rather impatient as a youth and chose to skip high school entirely, entering the University of Waterloo (Ontario, Canada) directly from Grade 9. In 1976, he published the first statistical analysis of genealogy (of the House of Stuart) and thereby came to the attention of the British Royal Family, especially the Queen Mother. Mr. Cunningham’s great love of astronomy was also manifest early when he almost single-handedly founded the Dance Hill Observatory near Kitchener (Ontario) in 1981.

My personal knowledge of the author began in the mid-1980s when I realized that he was one of the best science journalists in Canada. Subsequently, I utilized his valuable book and Minor Planet Index to Scientific Papers when I began to study asteroid regoliths by means of their submillimeter emission. I have always been impressed by Dr. Cunningham’s quiet tenacity and the ability to get over seemingly insurmountable hurdles. He is particularly proud of the asteroid that was named

4276 Clifford in his honor and of his cameo appearance on the last 1-hr episode of "Star Trek: Deep Space Nine." Both of these achievements fulfilled his lifelong dreams. And yet another such dream was fulfilled in 2015 when he earned a Ph.D. in the History of Astronomy from the University of Southern Queensland.

Victoria, BC, Canada P.A. Feldman

Preface

As Wordsworth said, "Words last, while all of us will one day pass." So it is with those scientists and dilettantes who gazed at Ceres and the other asteroids through their nineteenth century telescopes. They have all passed, but their words live on in scientific papers and personal correspondence.

In most cases, their words are in foreign languages, most usually German, Italian, and French, with lots of Latin thrown in. For the vast majority of twenty-first century readers who know only English, this book brings their words to life for the first time in two centuries.

"The discovery of the new planets, with which our century began so brilliantly, indisputably deserves the first place in a history of astronomy," wrote Baron Franz von Zach in 1811. This book serves to present that history. And that history continues: a Mars-approaching asteroid designated 2001 AA was discovered on January 1, 2001, exactly 200 years after the discovery of Ceres. Also in January 2001, the space agency NASA selected a Ceres orbiter as a potential mission in its Discovery program. This probe, dubbed Dawn, reached Ceres in 2015, thus heralding the next great phase of study of the first dwarf planet (and the first asteroid) ever discovered.

This first volume in a four-volume set about the early nineteenth study of Ceres, Pallas, Juno, and Vesta places the discovery of Ceres in context by looking at its intellectual antecedents in ancient Greek thought about harmony. Important translated material in this book includes both monographs about Ceres by its discoverer, Giuseppe Piazzi, and the Ceres-related sections of books by the German astronomers, Johann Schroeter and Johann Bode. Source materials never before considered by historians of astronomy are also included here: a conversation with Piazzi, and another one with Niccolo Cacciatore that should force a reevaluation giving him the status of co-discoverer of Ceres. All the correspondence between the astronomers of Europe dealing with Ceres in 1801 and early 1802 are included here, while the second volume in the set will focus on the scientific papers they published about Ceres. Editorial insertions throughout the book are in square brackets.

The great archaeologist Sir Flinders Petrie used to say that those without history were forced to live in one dimension of time—the present—whereas those who knew history could live in as many as they pleased. Enjoy this multidimensional journey.

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Ft. Lauderdale, FL, USA Clifford Cunningham
June 2015

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About the Author

Clifford J. Cunningham did his Ph.D. work in the history of astronomy at James Cook University and the University of Southern Queensland in Australia, and he is affiliated with the National Astronomical Research Institute of Thailand. He has written or edited 13 books on the history of astronomy, and his papers have been published in many major journals, including *Annals of Science*, *Journal for the History of Astronomy*, *Culture & Cosmos*, *Journal of Astronomical History and Heritage*, *Studia Etymologica Cracoviensia*, *The Asian Journal of Physics*, and Renaissance and Reformation. Asteroid (4276) was named Clifford in his honor by the International Astronomical Union based on the recommendation of its bureau the Harvard–Smithsonian Center for Astrophysics.

Chapter 1
The Unseen Planet

A Beautiful Concert

As with most great thoughts in the Western world, we must look to ancient Greece for the spark that inspired the search for the unseen planet between Mars and Jupiter. Our story begins in 531 BC, when the philosopher Pythagoras (580–500 BC) established a school of philosophy in Croton in what is today southern Italy based on understanding the universe in terms of numbers (Fig. 1.1).

Legend has it that Pythagoras was passing a smithy, and was intrigued to hear that the four blacksmiths made different notes as they struck the anvil with their hammers. Upon investigation, he found the hammers were of different weights, and set up an experiment showing how strings exhibiting different degrees of tautness gave off different notes. And when the strings were shortened in proportion to the weights hanging from them, they again produced the same series of notes. Two thousand years later, a room in Florence, Italy, was filled with weighted strings of varying lengths, diameters, and tensions to test harmonic ideas. In this case the investigators were Vincenzo and his son Galileo Galilei (Sobel, 1999) (Fig. 1.2).

Excited at the idea of a link between measurements of length—a quantitative experience with musical notes—Pythagoras suspected the existence of a fundamental secret of the universe. He went on to theorize that the distance between the planets also corresponded to musical notes (Wilson, 1980). Although the smithy legend itself is "impossible and absurd," (Burnet, 1908, p. 118), the statement that he discovered the "consonances" by measuring the lengths corresponding to them on the monochord is quite credible.

It is said that Pythagoras himself was the first to apply the word 'cosmos,' with its implication of neatness and good order, to the world. The most intellectually powerful notion arising out of the spherical Pythagorean cosmos is that of the music of the spheres.

> *The Pythagoreans, who were the first [philosophers] to take up mathematics, not only advanced this study, but also having been brought up in it they thought its principles were*

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
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Fig. 1.1 Johannes Kepler

Fig. 1.2 Pythagoras experimenting with harmonics

the principles of all things. Since all other things seemed in their whole nature to be modeled on numbers, and numbers seemed to be first in all of nature, they supposed the elements of numbers to be the elements of all things things, and the whole heaven to be a musical scale and a number. (Aristotle, *The Metaphysics, 986*) (Jones, 2012, p. 9)

Again, we are indebted to Aristotle (*The Heavens* II.9) for giving us their reasoning:

Some believe that sound must occur when bodies of such magnitude are in motion, since it is so with the movement even of earthly bodies. The Sun and Moon, and also stars, which

> *are so great in number and size, moving at such speed, must necessarily produce a noise of unimaginable volume. On these assumptions, and supposing that their speeds, determined by their relative distances, have the ratios of the musical concords, they say that the sound of the stars moving in circular orbits is harmonious.* (Furley, 1987, p. 58)

As Plato (428–347 BC) put it, "as the eyes are designed to look upon the stars, so are the ears to hear harmonious motions, and these are sister sciences as the Pythagoreans say." The harmony of the spheres appealed to poets and musicians, especially because, as the elder Scipio (236–184 BC) explains, human music is an imitation of the cosmic music. Chaucer (1342–1400) paraphrases him in *The Parliament of Birds* (Chaucer, 1380), where he talks about the melody produced by nine spheres and the resulting harmony.

> And after shewed he him the nyne speres
> And after that the melodye herde he
> Thet cometh of thilke speres thryes three
> Thet welle is of musyke and melodye
> In this world heer, and cause of armonye

Poetry is appropriate here because ancient music included not only music but also metrics—poetry—for Greek poetry was composed to be chanted. "Moreover, it had an ethical and cosmological aspect; the theory of harmony in music was a part of the theory of harmony in the whole cosmos. Thus music was a branch of philosophy as well as a branch of mathematics." (Sarton, 1952). In this mold, the main surviving work of Aristoxenos (Elements of Harmony, c. 335 BC; see Macran, 1902) was of seminal influence. The higher learning of late antiquity and of the medieval period included four main subjects: arithmetic, music, geometry and astronomy. Thanks to Pythagoras and Aristoxenos, music was a mathematical science, while physics remained closer to philosophy. "As an evidence of the explanation of the world by numbers, the Pythagoreans pointed to the strings of musical instruments and to the motions of stars and planets, thereby uniting music, poetry, matter, and mind into a harmonious whole. At least this was their ultimate dream" (Wilson, 1996).

This concept of the unity of mathematics, music and astronomy influenced astronomers down to the time of Johannes Kepler (1571–1630) and Marin Mersenne (1588–1648; see his Traits of Universal Harmony, 1627). At the core of this philosophy is the attempt to find simplicity in the mathematics of the cosmos, a goal shared by modern-day cosmologists. The fifth-century BC Pythagoreans were the first in the Greek tradition to fuse religious feeling with mathematics in their astronomy. And according to Aristoxenos, the Pythagoreans used music to purify the soul. Pythagoras went even further when he proclaimed the pursuit of disinterested knowledge to be the greatest purification—the highest kind of life is the theoretical or contemplative.

According to Newton's nephew, John Conduitt (1688–1737), "Sir Isaac used to say he believed Pythagoras had some notion of gravity, and meant by that what is vulgarly called the Musick of the Spheres" (Conduitt, 1732).

The Soul of Science

In many ways Kepler was the quintessence of this tradition. He was immersed in it, and although he eventually rejected both the geocentric view of the Pythagoreans, and the circle in favor of the ellipse to represent planetary orbits, he retained a firm belief in a harmony of the spheres. Some of the mathematical problems he discovered answers to (how many spheres can be packed into a given space?) are only now being rigorously solved (Cipra, 1991). The sphere packing problem, which has implications for the atomic theory of matter, was among the topics discussed in letters between Kepler and the English mathematician Thomas Harriot (Szpiro, 2003). Just as fundamental as the sphere for Kepler was the triangle.

For the ancient Greeks, relative speeds and distances, like musical intervals, could be expressed as ratios of small integers (Furley, 1987, p. 59). For Kepler, the structure of the planetary system was derived from the Platonic solids, known as regular polyhedra (tetrahedron, cube, octahedron, dodecahedron and icosahedron). Each of these can be constructed by triangles. For example, four equilateral triangles make a tetrahedron, 20 an icosahedron. In the Timaeus, Plato (428–348 BC) made the first attempt to show the mathematical simplicity behind the misleading complexity of astronomical appearances by asserting that triangles are the most basic of figures (Archer-Hind, 1888, pp. 191–208). But why triangles?

To look at the issue from another angle, as it were, consider this passage from the pen of Thomas Paine (1794):

> *The scientific principles that man employs to obtain the foreknowledge of an eclipse, or of anything else relating to the motion of the heavenly bodies, are contained chiefly in that part of science which is called trigonometry, or the properties of a triangle, which, when applied to the study of the heavenly bodies, is called astronomy; when applied to direct the course of a ship on the ocean, it is called navigation; when applied to the construction of figures drawn by rule and compass, it is called geometry; when applied to the construction of plans or edifices, it is called architecture; when applied to the measurement of any portion of the surface of the earth, it is called land surveying. In fine, it is the soul of science; it is an eternal truth; it contains the mathematical demonstration of which man speaks, and the extent of its uses is unknown.*

Indeed, triangles are so fundamental that when Rene Descartes (1596–1650) sought to examine geometric truth, he used them as his prime example: "I saw very well that if we suppose a triangle to be given, the three angles must certainly be equal to two right angles." (Descartes, 1637). He used triangles at great length in a further discourse 4 years later: "When I imagine a triangle, I do not conceive it only as a figure comprehended by three lines, but I also apprehend these three lines as present by the power and inward vision of my mind, and this is what I call imagining" (Descartes, 1641).

The idea was taken up by another French philosopher, Nicolas Malebranche (1638–1715): "If a Man should apply himself to consider the Properties of all the diverse Kinds of Triangles, although he should eternally continue this sort of Study, he would never want new and particular Idea's" (Malebranche, 1674).

The constellation Triangulum was named for both the Greek accomplishments in mathematics, and the triangular-shaped island of Sicily, from where the first asteroid was found. (See also the poem about Piazzi and Ceres by Ponta in Chap. 4.) Galileo elucidated the link between philosophy, the study of the heavens and mathematics, a link that was rejected by Georg Hegel in his infamous dissertation (examined later in this series).

"Philosophy is written in this grand book the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and to read the alphabet in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures, without which it is humanly impossible to understand a single word of it without these one wanders about in a dark labyrinth" (Galileo, 1618).

But before he imagined using the triangular-derived Platonic solids, Kepler wrestled with other ideas, and while doing so planted the seed that led to the search for the first asteroid.

Kepler's Bold Hypothesis

Kepler himself seems to have had a prescient view of his work, the Mysterium Cosmographicum, as he related in a letter to Michael Maestlin (1550–1631), a professor of mathematics at the University of Tübingen, on October 3, 1595: "If this is published, others will perhaps make discoveries I might have reserved for myself. The more others build on my work the happier I shall be" (Beck, 1937) (Fig. 1.3).

Before reading Kepler himself, let us set the scene with the words of the man who discovered the first asteroid, Piazzi (1802a):

> *The first we can mention to have an idea about a planet between Mars and Jupiter was Kepler's thought as the father of modern astronomy. Living at the time of the Renaissance, he was overwhelmed by the fascination, common at that time, of the ancient philosophy made majestic by the names of Pythagoras and Ptolemy. He believed in the mysterious property of numbers: he thought that in the multi-plicity of their relationship was the seed of human knowledge "so I looked in their order and structure in the sky." But being a great genius more worthy of the title of divine than Ptolemy, submersed by the most absurd extravagance of a dream of celestial harmony and by a myriad of combinations, he pointed out an emptiness between Mars and Jupiter that could only be explained through a dissonance and lack of harmony. This dissonance was not felt by him about the other planets, which combined in direct or inverse order to create a beautiful concert.*

"When Kepler's music of the spheres is played, it sounds most pleasant." (Ovenden, 1975). The cosmological theory propounded by Kepler explains the gaps between the planetary orbits by the relationship between the five Platonic solids. Kepler presented this system working inwards from the sphere of Saturn, so that he alternately inscribed a regular polyhedron in a sphere and inscribed a sphere in a regular polyhedron. Kepler himself wrote that the idea of using the Platonic solids to explain the planetary orbits occurred to him on July 19, 1595. He believed that these five polyhedra either described or determined some fundamental property of the corporeal world. (Field, 1988; Cornford, 1937; Stephenson, 2000). This is how Kepler (1595) expressed it in the preface to the *Mysterium Cosmographicum:*

> *It is my intention, reader, to show in this little book that the most great and good Creator, in the creation of this moving universe, and the arrangement of the heavens, looked to those*

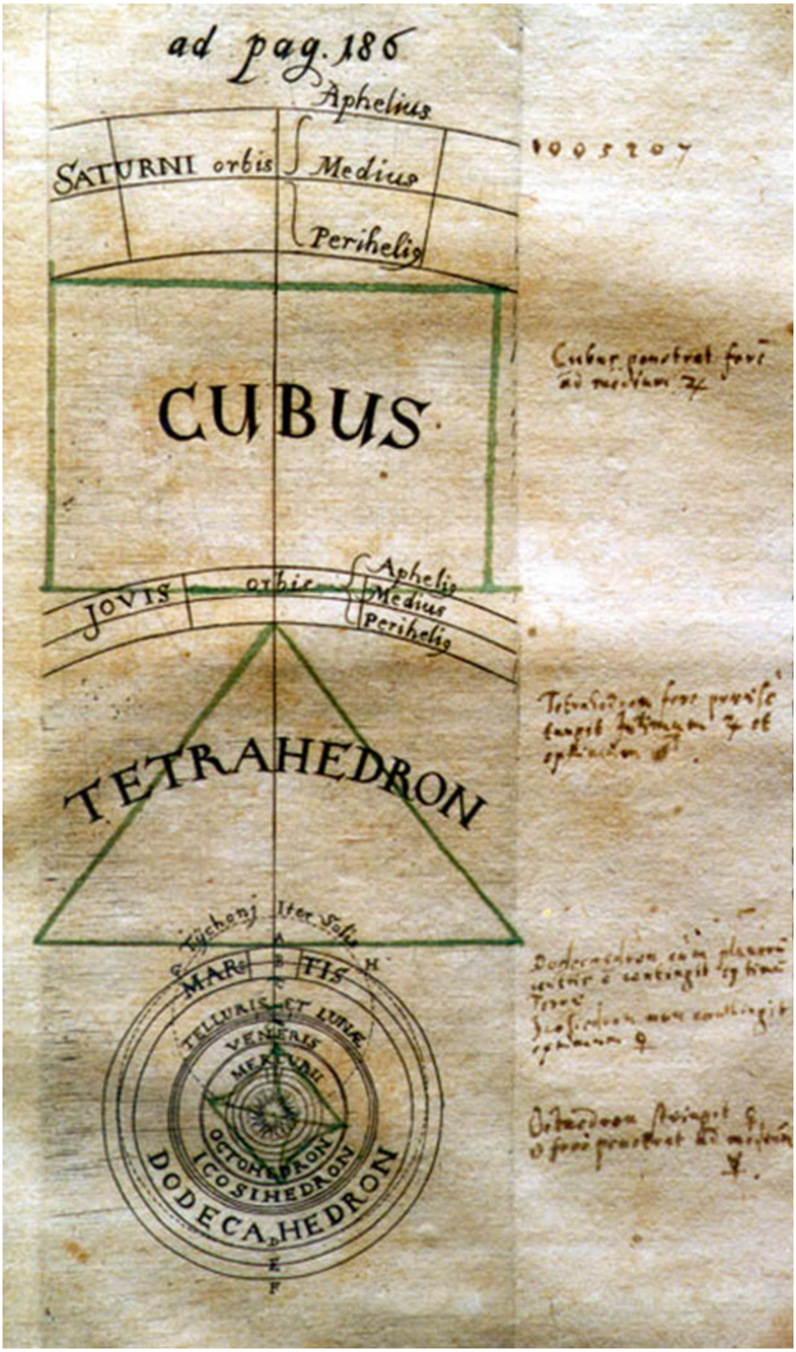


Fig. 1.3 The five Platonic solids (*at right*) and their relationship to the planets, according to Kepler.

> *five regular solids, which have been so celebrated from the time of Pythagoras and Plato down to our own, and that he fitted to the nature of those solids, the number of the heavens, their proportions, and the law of their motions. There were three things in particular about which I persistently sought the reasons why they were such and not otherwise: the number, the size, and the motion of the circles.*
>
> *That I dared so much was due to the splendid harmony of those things which are at rest, the Sun, the fixed stars and the intermediate space, with God the Father, and the Son, and the Holy Spirit. In the beginning I attacked the business by numbers, and considered whether one circle was twice another, or three times, or four times, or whatever, and how far any one was separated from another according to Copernicus. I wasted a great deal of time on that toil, as if at a game, since no agreement appeared either in the proportions themselves or in the differences. Since, then, this method was not a success, I tried an approach by another way, of remarkable boldness. BETWEEN JUPITER AND MARS I PLACED A NEW PLANET* [capitals added here], *and also another between Venus and Mercury, which were to be invisible perhaps on account of their tiny size, and I assigned periodic times to them. For I thought that in this way I should produce some agreement between the ratios, as the ratios between the pairs would be respectively reduced in the direction of the Sun and increased in the direction of the fixed stars. Yet the interposition of a single planet was not sufficient for the huge gap between Jupiter and Mars; for the ratio of Jupiter to the new planet remained greater than that of Saturn to Jupiter; and on this basis whatever ratio I obtained, in whatever way, yet there would be no end to the calculation, no definite tally of the moving circles, either in the direction of the fixed stars, until they themselves were encountered, or at all in the direction of the Sun, because the division of the space remaining after Mercury in this ratio would continue to infinity.*

Most astronomers in the past century have discounted Kepler's ideas as nothing but mysticism and numerology. "Indeed, the subject is still felt to be slightly subversive, enough to make a good solid astronomer uneasy: those who can swallow the wildest flights of cosmological speculation choke over a spoonful of heavenly harmony!" (King-Hele, 1972, p. 376). According to Heward (1912), Kepler first postulated the existence of an unseen world while assisting Tycho Brahe in preparing the Rudolphine astronomical tables:

> *Tycho's very exact observations of the places of the planets suggested to Kepler that Jupiter was very much farther away from Mars than accorded with his sense of just proportion of distances. All through his life Kepler had been dominated by a sense of analogy; he believed with unwavering faith that unity of design was an ordinance of the Creator's plan. Hence he concluded that, though invisible to the eyes now, a large planet existed in this region.*

Unfortunately Kepler discarded the idea of a new planet in favor of the Platonic solids. In concluding his preface, Kepler reassures his readers that "you will not find any new and undiscovered planets interpolated, as I did a little while ago: I do not favour that piece of audacity." And there the matter rested.

The Gap Between Mars and Jupiter

A century passed before speculation was renewed about the gap between Mars and Jupiter (Hoskin, 1993). The extent of the gap was quite apparent at the beginning of the eighteenth century. William Whiston (1667–1752), Newton's successor in

Cambridge, gave the actual distances of the planets in millions of miles as 32, 59, 81, 123, 424 and 777, the jump from 123 to 424 being readily apparent (Whiston, 1707).

Thomas Wright of Durham (1711–1786), writing a few years later in an unpublished manuscript, paints a comet as the agent provacateur. “That comets are capable of distroying (sic) such worlds as may chance to fall in their way, is, from their vast magnitude, velocity, firey (sic) substance, not at all to be doubted, and it is more than probable from the great and unoccupied distance betwixt ye planet Mars and Jupiter some world may have met with such a final dissolution” (Hoskin, 1968).

Around 1739, the famous Scottish mathematician Colin Maclaurin (1698–1746) also became fascinated by the gap. James Ferguson, writing in 1809, noted that “By comparing the great interval between the Orbits of Mars and Jupiter, it was surmised upwards of 70 years ago, by Mr. Maclaurin and others, and lately by C. Lofft, Esq that there must, at least, be one planet, whose orbit is exterior to that of Mars, and interior to the Orbit of Jupiter” (Capel Lofft, English antiquarian, 1751–1824).

Zach’s Dream

In 1783 an historic meeting took place in London between a 29-year-old Hungarian looking to make his mark on astronomy and an Englishman who just 2 years before had made one of most startling discoveries of the eighteenth century. Among the subjects under discussion between William Herschel, discoverer of the planet Uranus, and the young Franz Xaver von Zach was the apparent vindication of Bode’s Law by the discovery of Uranus, which fit neatly into its mathematical progression of planetary distances. In an age of dubious dental medicine, the discovery of Uranus was put in perspective by the German physicist and philosopher Georg Christof Lichtenberg (1742–1799): “To invent an infallible remedy against toothache, which would take it away in a moment, might be as valuable and more than to discover a new planet.” During Zach’s visit to England, he found the papers of Thomas Harriot (1560–1621), the astronomer who corresponded with Kepler (Brosche, 2009). Following in the footsteps of Kepler, Zach also posited a new planet between Mars and Jupiter.

Just 2 years after meeting Herschel, Zach (1785, p. 162) went so far as to predict its orbital elements: distance from the Sun 2.82 AU; eccentricity 0.14; orbital period 4.74 years; inclination 1° 36′; heliocentric longitude of perihelion 192° 6′. He deposited these data in sealed envelopes with Ernst II, the Duke of Gotha (Zach’s patron), Hans Count von Bruhl (Zach’s mentor) and his astronomical colleagues Johann Bode (editor of the Berlin Astronomical Yearbook and Director of Berlin Observatory) and Johann Köhler (astronomer in Dresden):

> *Of the supposed planet between Mars and Jupiter I will disclose to you my dreamings orally as well, and show you the chimerical calculations I have been occupying myself with. My fate may turn out to be that of the alchemists who are looking for gold—they have everything except one thing. I also seem to have all the elements of the orbit of this yet unknown*

planet except one, that is the epoch of longitude; it's quite amusing that among all these errors and different mazes, one finds not gold, but a very useful chemical process.

Six months after the 'new planet' had been discovered, Zach (1801c) reviewed the issue and Bode's Law in a paper triumphantly entitled "Regarding a New primary planet of our Solar System long suspected between Mars and Jupiter and now likely discovered." The full paper is in a later volume of this series; here is an excerpt:

To represent this in an approximate way and with small numbers, the distance of the Sun from Saturn is divided into 100 equal parts; it follows:

1. Mercury	*4 such parts distant from the Sun*
2. Venus	$4+3=7$
3. Earth	$4+2\cdot 3=10$
4. Mars	$4+2\cdot 2\cdot 3=16$
5. Hera or Juno	$4+2\cdot 2\cdot 2\cdot 3=28$
6. Jupiter	$4+2\cdot 2\cdot 2\cdot 2\cdot 3=52$
7. Saturn	$4+2\cdot 2\cdot 2\cdot 2\cdot 2\cdot 3=100$
8. Uranus	$4+2\cdot 2\cdot 2\cdot 2\cdot 2\cdot 2\cdot 3=196$

Or, expressed more simply, the nth planet calculated from the Sun is distanced $4+(2^{n.2}\cdot 3)$ from it. The one represents the mean distance of the first planet by "a," the difference of distance between the first and second by "b," the mean distance of Earth from the Sun = 1; therefore, the mean distance of the nth planet from the Sun is $= a+(2^{n.2}\cdot b)$.

This law is founded on no theory known to us; at least, no one has been able to prove it mathematically, and it was concluded empirically out of analogous conclusions. Mathematical astronomers do not accept something that cannot be mathematically proven. However great the possibility that the announced relationship of planetary distances, at least an approximation of it, could occur in nature, there still were astronomers who doubted the conclusion of this unproved law, and consequently also doubted the existence of an invisible planet to be found supposedly between Mars and Jupiter. Professor Bode made mention of this planet in his valuable textbooks and in all their numerous editions since 1772, but he speaks of its existence as "suspected" and as an "analogy," but not as a proven truth.

Reason, Harmony and Nature

"Who does not know that there is a most agreeable harmony among all truths of nature, and a most sharp dissonance between false positions and true effects?" (Galileo, 1615).

What were the philosophical underpinnings that compelled a small coterie of astronomers to search for an unseen planet based on an unproven theory of universal harmony? The foundation upon which it was based can be traced back to the oldest monument of Greek prose, "On Nature" (Patrick, 1889) by Heraclitus (535–475 BC), who postulated an "opposite tension that keeps things together, like that of the string in the bow and the lyre, though it is a hidden attunement, is better than any open one. For all his condemnation of Pythagoras, Heraclitus cannot get away from the tuned string." (Burnet, 1914, p. 62). He believed in the truth that much learning does not teach men to think.

How to think, or reason, was the province of Descartes, whose treatise "Discourse On The Method Of Rightly Conducting The Reason And Seeking For Truth In The Sciences" was published in 1637. It set the stage for the Age of Reason (Lipking, 2014).

In the eighteenth century theorizing about the nature of reality was very much in vogue. Consider some book titles: *Contemplation of Nature* (Bonnet, 1764), *The System of Nature* (D'Holbach, 1770), *Theory of Natural Philosophy* (Boscovich, 1763), *Rational Ideas about the Intentions of Natural Things* (Wolff, 1724), *Ideas for a Philosophy of Nature* (Schelling, 1797) and *Philosophy of Natural History* (Smellie, 1790). The forerunner of it all was Newton's *Mathematical Principles of Natural Philosophy* (1687; third edition, 1726).

For the German philosopher Georg Hegel nature was a system of reason, and the concept of the philosophy of nature "is nothing other than the comprehension of nature." (Hegel, 1817). His rejection of the Newtonian approach will be considered in a later volume in this series.

The philosophical framework that allowed thinking people to put some credence in Bode's law was espoused in the *Critique of Pure Reason* by Kant (1787): "For human reason, without being moved merely by the idle desire for extent and variety of knowledge, proceeds impetuously, driven on by an inward need, to questions such as cannot be answered by any empirical employment of reason, or by principles thence derived… Thus the critique of reason, in the end, necessarily leads to scientific knowledge."

Nothing could better encapsulate the motives—conscious or unconscious—of those who sought scientific truth in Bode's law. As Hegel wrote in the early nineteenth century: "Reason is the highest union of consciousness and self–consciousness. The knowing of Reason is therefore not the mere subjective certitude, but also TRUTH, because Truth consists in the harmony, or rather unity, of certitude and Being" (Hegel, 1808–1811/1844).

Here we come full circle. The search for scientific truth in nature, and the application of reason to enable the search, leads to harmony. The harmony exemplified by Bode's Law.

Bode's Law: Astronomical Alchemy

The origins and development of Bode's law is a tangled mass of confusion that took decades to unravel. There are five main players: Bonnet, Titius, Wolff, Lambert and Bode. Even at the early date of 1801, when Zach was writing, the formulation of the law of planetary distances was ascribed solely to Bode. But he was little more than a vehicle for its popularity.

Step #1: David Gregory's Book

In 1702, while Savilian professor of astronomy at Oxford, David Gregory (1661–1708) published a book in Latin, with an English translation appearing in 1715, and a second edition in 1726. In this work, *The Elements of Astronomy,* he puts the planetary distances into proportional numbers: "…supposing the distance of the Earth from the Sun to be divided into ten equal Parts, of these the distance of Mercury will be about four, of Venus seven, of Mars fifteen, of Jupiter fifty two, and that of Saturn ninety five."

Step #2: Christian Wolff's Book

Christian Wolff (1679–1754) was professor of mathematics and philosophy at the University of Marburg when he wrote his *Rational Ideas about the Intentions of Natural Things* (also called *German Physics*) in 1726. In it he wrote about the progression of planetary distances. Indeed, he must have taken it from Gregory's book, as the numbers he used are exactly the same, and the same sentence is paraphrased (Hoskin, 1993). But like Gregory he did not take the crucial step of formulating it into mathematics, and thus he did not specifically note the gap between Mars and Jupiter (Jaki 1972a, 1972b):

> *The planets that move around the Sun are located very far from one another. If one divides the distance of the Earth from the Sun into 10 parts, the distance of Mercury takes 4 of it up, that of Venus 7, that of Mars 15, that of Jupiter 52, that of Saturn 95. If one accordingly imagines that the centres of all planets are in one line, which is drawn from the centre of the Sun to the centre of Saturn, and the whole line is divided into 95 parts, then at the end of the tenth is the Earth, at the end of the fifteenth is Mars, at the end of the fifty-second is Jupiter, and finally at the end of the ninety-fifth is Saturn. Thus Mercury and Venus are separated by 3 parts, Venus and the Earth also by 3, the Earth and Mars by 5, Mars and Jupiter by 37, Jupiter and Saturn by 43 parts* (Fig. 1.4).

Step #3: Johann Lambert's Book

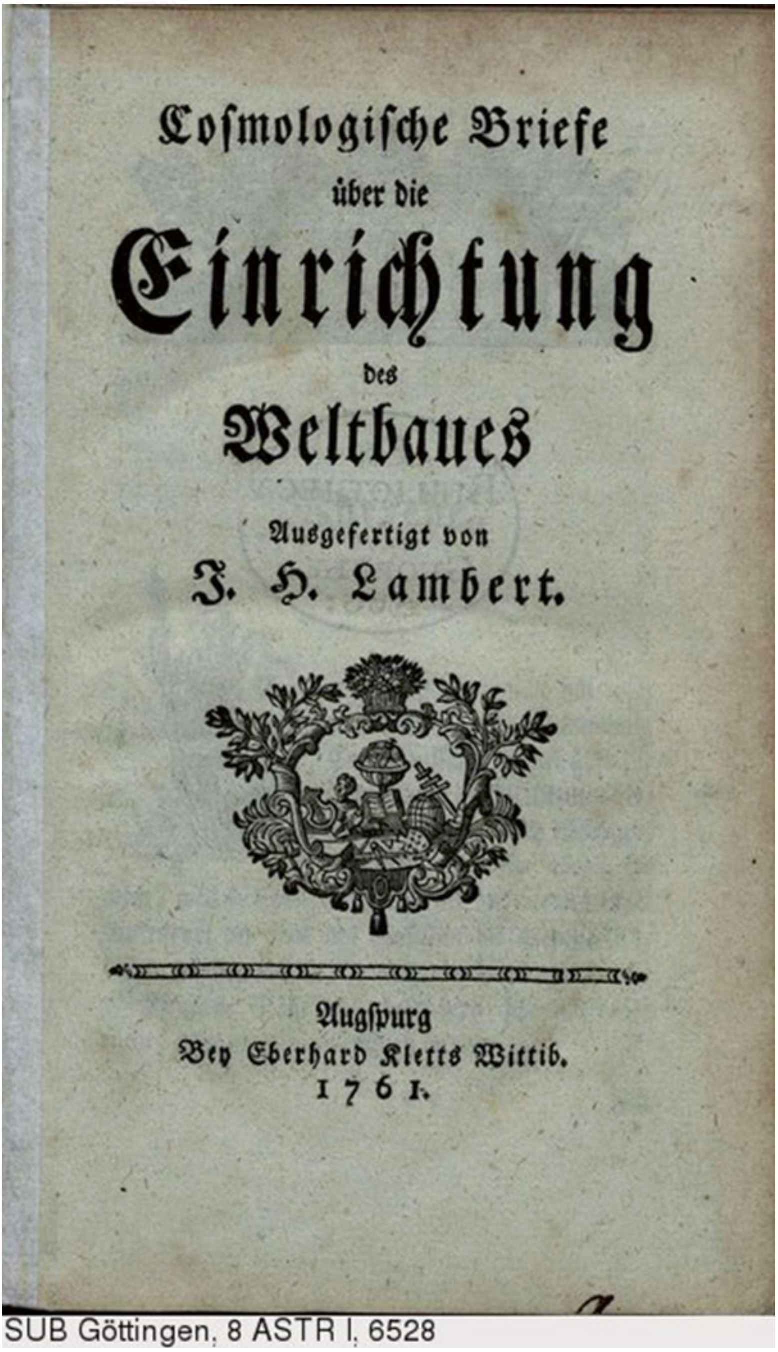
The gap was first noted by Johann Lambert (1728–1777) in his 1761 book *Cosmological Letters Regarding the Constitution of Planets.* Lambert was a German mathematician who first demonstrated that pi is an irrational number. The unit of light intensity is named after him, because he was the first to measure the intensity of light. In his book Lambert (1761, p. 7) said "And who knows, whether or not there are lacking planets which have progressed out of the wide space existing between Mars and Jupiter." To fill this space Lambert placed a primary planet (Figs. 1.5 and 1.6).

Fig. 1.4 Christian Wolff

Fig. 1.5 Johann Lambert

Step #4: Charles Bonnet's Book

As we shall see, Bode lifted the law that bears his name directly from the book *Contemplation of Nature* (1764) by Charles Bonnet (1720–1793). Bonnet was a Swiss naturalist who invented the idea of periodic catastrophes to explain how


Cosmologische Briefe
über die
Einrichtung
des
Weltbaues
Ausgefertigt von
J. H. Lambert.

Augspurg
Bey Eberhard Kletts Wittib.
1761.

Fig. 1.6 The title page of Lambert's 1761 book

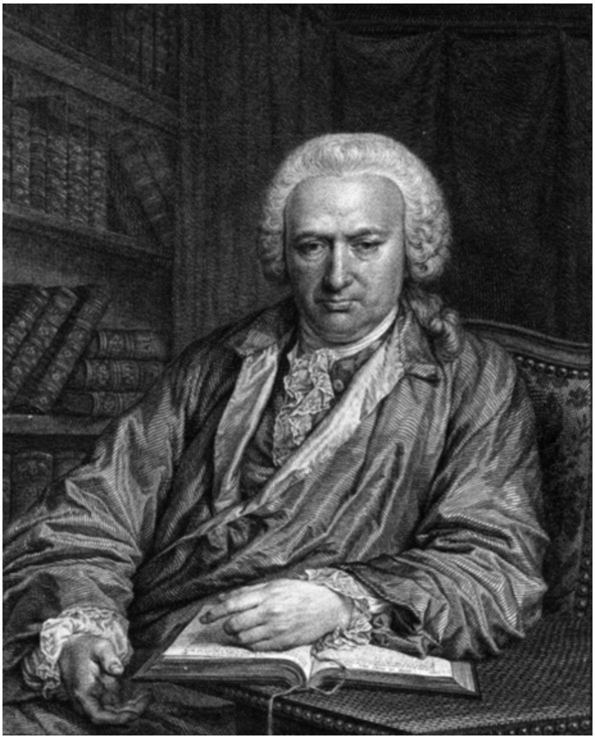


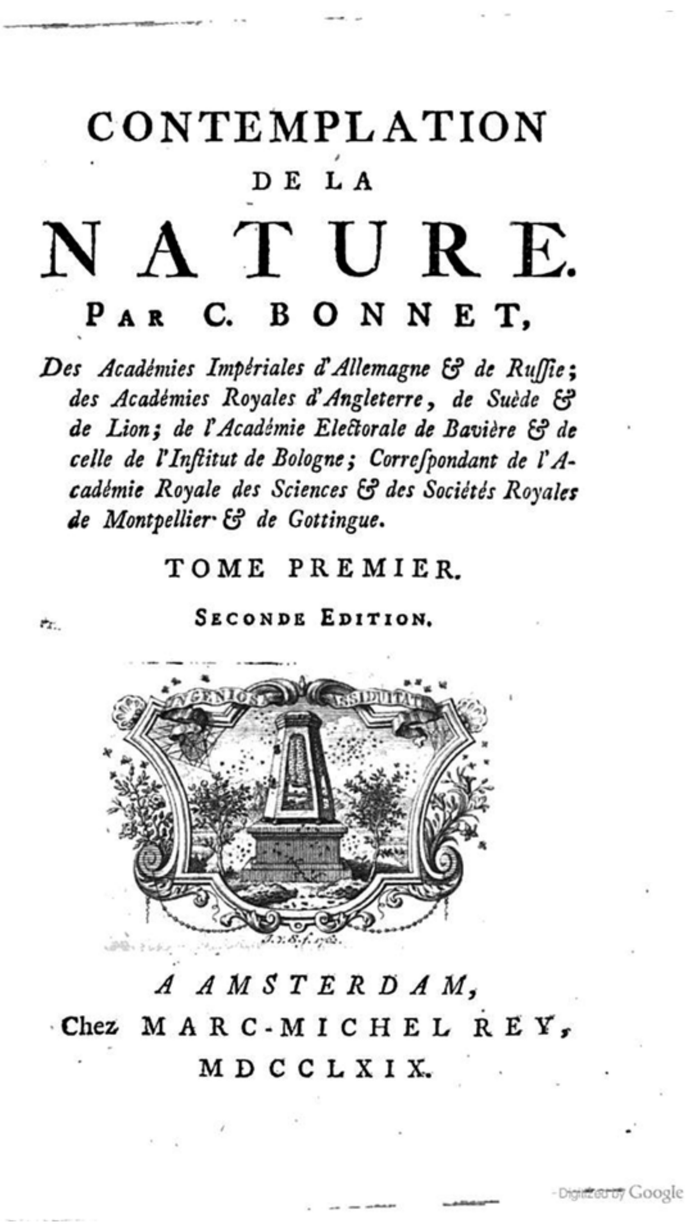
Fig. 1.7 Charles Bonnet

fossils exist of animals that are no longer found in nature. In this book, Bonnet sought to prove the existence of an order and purposefulness inscribed into nature by its Creator. Bonnet mentions that the telescope had increased the number of known planets and satellites. In Chap. 1, he held out the hope that the "satellite of Venus vaguely sighted in the last century, and seen again not long ago, augurs well for new conquest for astronomy." In the next sentence he said "Not only has it reserved for modern astronomy to enrich our sky with new planets, it was also given to it to roll back the frontiers of our vortex [planetary system]." For more about Bonnet, see Dawson (1990) (Figs. 1.7 and 1.8).

Step #5: German Translation by Titius

In the German translation of Bonnet's book, published in 1766, 22 lines were inserted as a footnote between the two sentences just quoted. They were written by Johann Daniel Titius (1729–1796), a Professor of Mathematics in Wittenberg. It was in these lines that the law of planetary distances was first expounded:

> *Take notice of the distances of the planets from one another, and recognise that almost all are separated from one another in a proportion which matches their bodily magnitudes.*



CONTEMPLATION

DE LA

NATURE.

PAR C. BONNET,

Des Académies Impériales d'Allemagne & de Russie; des Académies Royales d'Angleterre, de Suède & de Lion; de l'Académie Electorale de Bavière & de celle de l'Institut de Bologne; Correspondant de l'Académie Royale des Sciences & des Sociétés Royales de Montpellier & de Gottingue.

TOME PREMIER.

SECONDE EDITION.

A AMSTERDAM,

Chez MARC-MICHEL REY,

MDCCLXIX.

Fig. 1.8 Title page of the 1769 edition of Bonnet's book

Divide the distance from the Sun to Saturn into 100 parts; then Mercury is separated by 4 such parts from the Sun, Venus by 4 + 3 = 7 such parts, the Earth by 4 + 6 = 10, Mars 4 + 12 = 16. But notice that from Mars to Jupiter there comes a deviation from this so-exact progression. From Mars there follows a space of 4 + 24 = 28 such parts, but so far no planet

Fig. 1.9 Johann Titius

or satellite was sighted there. But should the Lord Architect have left that space empty? Not at all. Let us therefore assume that this space without doubt belongs to the still-undiscovered satellites of Mars; let us also add that perhaps Jupiter still has around itself some smaller ones which have not been sighted yet by any telescope. Next to this for us still-unexplored space there rises Jupiter's sphere of influence at 4 + 48 = 52 parts; and that of Saturn at 4 + 96 = 100 parts. What a wonderful relation! (Johann Titius 1766, p. 13) (Fig. 1.9)

Step #6: Bode Uses Text by Titius

The second edition of Bonnet's work by Titius is published in 1772. Bode adds a reference to it in a footnote on pg. 462 to the second edition of his own work, *Introduction to the Knowledge of the Starry Heavens,* also published in 1772. It was this book that linked him with the expression that thus became known as Bode's law (Fig. 1.10).

Step #7: Bode Adds His Own Words

In the third edition of his book, Bode (1777, p. 635) added the following sentence of his own: "That this chief planet between Mars and Jupiter must complete its revolution around the Sun in 4½ years can be computed from a law discovered by Kepler, namely that the squares of the orbital periods of two planets are to one another as are the cubes of their distances from the Sun." The following year Bode published another book, *Terse Explanation of Astronomy and the Associated Sciences,* that

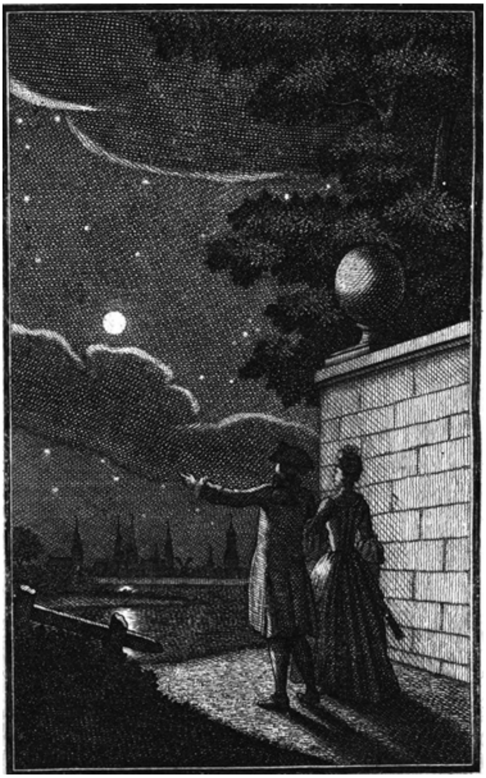


Fig. 1.10 Frontispiece of Johann Bode's 1772 book

dramatically expressed his firm belief in the progression. A diagram in Table X at the end of the book included an R marking the place where a planet was supposed to exist between Mars and Jupiter.

Step #8: Titius Attributes Law to Wolff

In the first discussion of the history of the law (Benzenberg, 1803), Titius is not given credit with formulating it, because it "was nothing new, as Wolff already 40 years earlier had similar ideas." This unfortunate state of affairs existed because Titius himself refused to take credit for his discovery. In the fourth edition of Bonnet's book (1783, p. 14) Titius specifically credits Christian Wolff with noticing the progression in 1726 (Step #2): "This relationship and the related considerations which Herr Bonnet thought had first been observed by Herr Lambert had already been recited by Freyherr von Wolff in his German Physics more than 40 years earlier." Titius also amplified the now famous footnote, describing the space between Mars and Jupiter as one in which no planet or satellite had yet been sighted.

Step #9: Bode Reveals Origins of Law

It was only in 1784 that Bode disclosed that the contents of his footnote were due to Titius! He did so in a book (*On the Newly Discovered Planet*) about Uranus, which fitted nicely into the progression formulated by Titius. (Jaki 1972a, 1972b) Let us hear Bode (1802a) himself explain the law that bears his name. Here he writes after the discovery of the first asteroid, Ceres, which he refers to as Piazzi's star:

> *In the 2nd edition of my* Introduction to the Knowledge of the Starry Heavens, *published in Hamburg in 1772, I speak thus (pg. 462) of the probable existence of a far greater number of planets in our solar system beyond those we already know of. 'Are the limits of the solar system in effect limited to the point where we see Saturn? (Since 1781 we know of Uranus, twice as far from the Sun as Saturn)…and why this considerable gap between Mars and Jupiter, where heretofore one finds no planet? Is it not likely that in this space wanders one of the celestial bodies to which the finger of God gave motion?' This progression moves forward only in small numbers, and consequently provides only approximate results; even though it is an incontrovertible experiment, a fact which doesn't need mathematical proof nor a setting forth of its physical causes, and which was first of all confirmed by the discovery of Uranus in 1781, and secondly most recently by that of Piazzi's star. This law of the progressive distance between the planets and the stars, evaded the famous Lambert. He wrote me at Hamburg dated 3 February 1772: 'Your noticing (pg. 462 in your work) of the distance of the planets would have pleased Kepler, who wrote an entire book on this question; it could lend a reason as to why the planets are distant from the Sun according to a particular and simple law unrelated to their mass.' The question then appeared worthy of notice in the view of this great man, which, in my view, is worth much more than the objections of a few modern astronomers. What would Lambert have said if he had seen the confirmation of this beautiful progression with the happy discovery of Uranus and Ceres? This progression remains, so long as there is no intermediary planet found in the intervals of the orbits of the 8 planets known to us. It conforms even more greatly to the experiment, according to Prof. Wurm, if one takes 387 as the basis of the average distance between Mercury and the Sun (distance from Sun = 1000), and 293 as the difference at the distance from Mercury and Venus (see Astronomical Ephemerides 1790, p. 168). I found in 1772 the first idea of this remarkable progression in Contemplation of Nature by Bonnet, translated by Titius, 2nd edition of 1772 in a note from the translator pg. 7 (Bonnet's original does not mention this), and since then I have frequently mentioned it in my astronomical work.*

According to Jaki (1972a, 1972b) this claim by Bode is highly suspect. He believes Bode used the first edition of Titius' translation, 1766.

Step #10: Wurm's Algebraic Formulation

The first to formulate Bode's Law algebraically, as Bode mentioned in the quote above, was the Reverend Johann F. Wurm, pastor of Gruibingen in Wurttemberg. It is in this form that the law is recognized today. In a communication to Bode on February 27, 1787, Wurm gave the mean distance of the nth planet as the expression noted above, where $a = 0.387$ was the mean distance of Mercury from the Sun, and $b = 0.293$ was an adjusted value of the Mercury-Venus distance, with the Earth-Sun distance taken as unity. The adjusted value of b was a 12.8 % reduction in the true value.

With the exception of the Venus-Sun distance the solar distances predicted by the formula came within 3 % of the correct value. Wurm also speculated about a missing planet (Wurm, 1786, 1787).

Step #11: Bode Accepts Wurm's Figures

In the second edition of his book *Terse Explanation of Astronomy* (1793) Bode reproduced Wurm's table, but did not give his formula. Bode wrote that Wurm's figures agreed "almost exactly" with the actual distances of the planets. He attributed the fact that the planet remained undiscovered to its small size and low albedo.

The Celestial Police

The intellectual milieu had not only been established—it was becoming agitated. Expectations of an unseen planet had been raised so high it would have been embarrassing to much of the astronomical community if nothing were found. Baron von Zach, who was the first to begin searching for the unseen planet, galvanized the astronomical community and took two momentous steps, beginning with the founding of a journal in 1798, *The Allgemeine Geographische Ephemeriden* (AGE, or General Geographical Ephemeris). (Herrmann 1969; Christoph, 2013, pp. 59–61) In 1800 Zach turned over the journal to other editors and founded a less formal journal, the *Monatliche Correspondenz* (Monthly Correspondence), which was destined to publish nearly all the scientific results about the asteroids for more than a decade. It was in fact the world's first journal devoted entirely to astronomy. Lalande described it as "the depot of astronomy for every part of Europe." His second major effort in 1798 was organizing the world's first astronomical congress, held in Gotha (Herrmann, 1970). In attendance that August were:

Joseph-Jerome Lalande (1732–1807. France's most famous astronomer, who arrived in Gotha with his niece on July 25.)
Johann Elert Bode (1747–1826. Director of Berlin Observatory from 1786 to 1825.)
Martinus van Marum (1750–1837. Director of the Teyler Museum in the Netherlands and discoverer of carbon monoxide.)
Johann Friedrich Wurm (1760–1833. In 1800 he became a professor for both classical languages and mathematics at the grammar school in Blaubeuren, near Ulm.)
Johannes Feer (1763–1823. Zurich astronomer and engineer.)
Johann Kaspar Horner (1774–1834. Swiss astronomer, Zach's assistant from 1798 to 1799.)
Johann Konrad Schaubach (1764–1849. In 1795, while principal at the secondary school in Meiningen, he published an edition of *Pseudo-Eratosthenes,* a treasury of Greek myths associated with the constellations.)

Fig. 1.11 Seeberg Observatory, around 1810

“They all lodged at my place at Seeberg,” wrote Zach in a letter to Professor von Schedius on January 26, 1799. (Lajos Schedius, 1768–1847. A member of the Academy of Sciences, the first Protestant to be appointed as a professor of Pest University.) (Fig. 1.11)

Lodged in town were:

Georg Simon Kluegel (1739–1812. German mathematician from Halle, who introduced the concept of the trigonometric function.)

Ludwig Wilhelm Gilbert (1769–1824. from Halle. He was editor of the *Annals of Physics,* in which he published attacks on natural philosophy and appended critical notes to articles with a speculative tendency.)

Karl Philipp Heinrich Pistor (1778–1847. In 1810 he founded a workshop where astronomical and other scientific instruments were manufactured; from Halle.)

Johann Gottfried Köhler (1745–1801. He was director of both the Kunstkammer and the Mathematical-Physics Salon in Dresden.)

Johann Heinrich Seyffert (1751–1818. A horologist who succeeded Köhler as director of the Mathematical-Physics Salon in Dresden.)

Karl Felix von Seyffer (1762–1822. Professor of astronomy at Göettingen Observatory from 1789 to 1804.)

George Butler (1774–1853. A mathematical lecturer from Cambridge. He became the Dean of Peterborough in 1842.)

Num 121.

Gothaische Zeitung.

Auf das Jahr 1798.

31te Woche, Dienstags den 31. Julii.

Gotha, vom 30. Jul.

Gestern ist der berühmte französische Astronom, Bürger la Lande, begleitet von seiner Nichte, welche sich durch ihre Kenntnisse in der Astronomie bereits Achtung erworben hat, hier angekommen.

Nürnberg, vom 17. Julius.

genommen worden. Die Bauern um Ehrenbreitstein haben die dortigen Aecker bis an die Festungswerke mit Korn bestellt; der Commandant macht Ansprüche darauf.

Das Gerücht sagt aufs neue, daß nach der Erndte eine französische Armee auf dem rechten Ufer des Niederrheines auftreten soll. Sie soll, der vorläufigen Sage nach, in drey Corps getheilt werden, deren eines zwischen

Fig. 1.12 A report on the Astronomical Congress, in the Gotha newspaper of July 31, 1798

For 10 days in August these illustrious men of science and mathematics discussed matters as diverse as mean time, the adoption of the metric system, the demarcation of new constellations in the southern heavens, and the likelihood of a missing planet between Mars and Jupiter (Bode, 1801c).

The congress got some notable press coverage, both in England and in Gotha. The 7 August 1798 issue of *The Times* newspaper in London ran an incendiary article about Lalande's visit (the Directory was the government of France from 1795 to 1799): "The old Citizen Lalande, whom the Directory has sent to Gotha for the purpose of making astronomical observations, is known to be a professed Atheist, and a staunch Revolutionist. For aught we know, he may be charged with some revolutionary attempts against Heaven" (Brosche, 2014, p. 48) (Fig. 1.12).

Even though the French in general were not popular in Germany, this did not prevent the German astronomers from welcoming Lalande. "On the 9th of August I reached Gotha," wrote Bode, "where I had the pleasure of becoming personally acquainted with the long celebrated and meritorious French astronomer Lalande and his learned neice, and of embracing my worthy friend Von Zach." (Bode, 1802c) A more sober assessment of Lalande, who features prominently in the saga of the early work on Ceres, is that he "was by no means a revolutionist." He certainly enjoyed his notoriety at the event, however. "The object of the congress was perhaps not unmixed with personal vanity" (*Encyclopedia Britannica,* 1842, Vol. 13, p. 29).

Energized by the congress, Zach now devoted himself to the hunt for the missing planet. In 1799 he met with several astronomers, and concluded that a coordinated effort was needed (Cunningham, 1988b). Thus was founded in 1800 the Vereinigten Astronomischen Gesellschaft (VAG), consisting of six astronomers: Johann Schroeter, Karl Harding, Wilhelm Olbers, Ferdinand Adolf von Ende (a senior appellate official in the duchy of Brunswick-Luneberg), Johann Gildemeister (1753–1837; senator of the government of Bremen) and Zach. Here are Zach's own words explaining the establishment of the group that became popularly known as the Celestial Police (see Chap. 9 for the report issued by Johann Schroeter in 1805):

> *Six astronomers gathered in Lilienthal thus founding on September 21, 1800 an exclusive society of 24 practical astronomers throughout Europe to systematically search for the planet suspected between Mars and Jupiter. They elected Schroeter as their president, and I* [Zach] *was granted the honour and trust to be nominated permanent secretary of this astronomical society. The plan of this society was to divide the entire zodiac among the 24 members.*
>
> *These astronomers were: Johann Bode (Berlin), Joseph Buerg (Vienna), Thomas Bugge (Copenhagen), J. C. Burckhardt (Paris), William Herschel (Slough), Johann Huth (Frankfurt), Georg Kluegel (Halle), Dr. Koch (Danzig), Nevil Maskelyne (Greenwich), Daniel Melanderhjelm (Stockholm), Pierre Mechain (Paris), Charles Messier (Paris), Barnaba Oriani (Milan), Giuseppe Piazzi (Palermo), Friedrich Schubert (St. Petersburg), Jan Sniadecki (Cracow), Jacques-Joseph Thulis (Marseille), Johann Wurm (Blaubeuren), Ferdinand von Ende (Celle), Johann Gildemeister (Bremen), Karl Harding (Lilienthal), Jons Svanberg (Uppsala), Wilhelm Olbers (Bremen), Johann Schroeter (Lilienthal), Franz von Zach (Gotha).*
>
> *Through a draw, each member received a zone of 15° in longitude and 7–8° in northern and southern latitude for inspection, and each was entrusted with very watchful supervision. Each member was to draw up a very exact star chart including the smallest telescopic stars of his section, and through repeated revisions was to ascertain the unchanging state of his district or every wandering celestial body. Through such a strictly organised policing of the heavens, divided into 24 sections, we hoped eventually to find a trace of this planet, which had so long escaped our scrutiny, if it did exist and make itself seen.* (Zach, 1801c)

In May 1801, Zach wrote a paper that was published in the June issue of the *Monthly Correspondence,* in which he opined that the missing planet would be discovered by one of the 24 putative members of the society:

> *Should the honour of the first discovery of this planet be denied our embryonic Society, not only will the presumable discoverer of it rank among the members of our Society, the delivery of our invitations being hindered by the disquiet of war, of postal delivery and of ocean travel, but this Society has already contributed much and will continue to amend our star catalogues in future.*

In fact he was correct, as Piazzi was one of the 24 people selected. We now know that Piazzi himself never received an invitation directly from Zach to join the society. Did he regard this as a snub, and thus decide not to send Zach his discovery data in January 1801? The answer is clearly no. In his second monograph (see Chap. 7 in this book), he writes: "While so much zeal was animating Europe and parts of Germany, I, far from the others and ignorant of the formation of the Society and of the honor given me to be among the 24 astronomers, following only my very own method of study, without meaning it, I found the much wanted Planet." Piazzi obviously harbored no grudge against Zach, since he was not even aware of the society or his election to it.

Setting out on a prolonged search for an invisible planet can only be compared to some of the greatest adventures in history, like the quest for the Holy Grail. "To those who have paid but little attention to the circumstances under which this strange enterprise was undertaken, nothing can appear more wild and chimerical" (Mitchell, 1851, p. 126).

So the die had been cast! Pure theorizing was to be replaced with some nitty-gritty observing, and some of Europe's best astronomers were pledged to the task. Then fate intervened.

Chapter 2
The Discovery of Ceres

A New Era

The years 1790–1805 were a turbulent time in Europe, a time of huge shifts, with events of historic proportions ranging from the French Revolution to the final collapse of the Holy Roman Empire. "Here and now begins a new era in the history of the world, and you can say you were there," declared Johann Goethe (1749–1832; quoted in Boyle, 2000). It was on October 21 in 1802 that Goethe met Hegel, whose notorious role in the story of the asteroids will be related in a later volume of this series.

In England, Prime Minister William Pitt resigned in 1801, as the high price of wheat caused bread riots. Italy, too, was in turmoil. A riot in Rome led to French occupation of the city in 1798. Pope Pius VI died in French captivity the following year, marking a collapse of the Church's central administration.

A collapse of a different kind occurred in Germany in 1801. In Munich a 15-year-old who was buried in the rubble after the collapse of his house came out alive after several hours. The escape was so remarkable that after his wounds healed the king himself gave him money that he used to purchase a glass-cutting machine. The boy, Joseph Fraunhofer (1787–1826), became a physicist whose work set the stage for the development of spectroscopy. The *Times*, in its January 1, 1901, issue, listed the greatest scientific achievement of the nineteenth century to have been the discovery, by astronomers using spectroscopy, that the material composition of the stars was the same as that of Earth. Spectroscopy was also used, in the twentieth century, to determine the mineralogy of asteroids.

The year 1801 also saw Thomas Jefferson, president of the United States, deliver his first inaugural address. The American civil engineer Robert Fulton produced the first submarine, the *Nautilis*. A Concordat was reached on July 15 between Napoleon and the new pope, Pius VII, defining the status of the Roman Catholic Church in France. In the Peace of Luneville with France, the Emperor Francis II consented to

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2_2

the virtual dissolution of the Holy Roman Empire. On October 1, a truce was declared between Britain and France. On the very day of the discovery of Ceres, January 1, 1801, there was a legislative agreement uniting Great Britain (England and Scotland) and Ireland under the name of the United Kingdom of Great Britain and Ireland. This union was reaffirmed by the voters of Scotland in 2015.

The great British artist John Constable, only 25 years old, visited the Peak district in England on a sketching expedition in 1801, and William Wordsworth was busily writing some of his greatest poetry. The first English poem to mention a vampire, *Thalaba* by Robert Southey, was published in 1801. More than a century before Einstein, the great poet and philosopher Samuel Taylor Coleridge wrote to a friend in 1801 that, working hard at metaphysics, he had "completely extricated the notions of Time and Space."

Novalis (Friedrich von Hardenberg), Germany's purest Romantic, died in 1801 at the tender age of 28; in the same year a young man of 22 wrote to a friend that "The voice of fame is murmuring in my ear. I dream of greatness." That young man became Sir Humphry Davy (1778–1829), the most famous chemist of the century (Cunningham & Jardine, 1990). Imagine the intellectual confluence when Gauss met Davy in Olbers' home in Bremen in 1801!

In science, the year 1801 saw some notable events:

- Lalande published his *Histoire célèste* (Celestial History), a catalog of 47,390 stars.
- Bode published his *Uranographia* star atlas of 17,240 stars.
- The law of partial pressure was formulated by John Dalton (1766–1814).
- Davy worked on the electric arc.
- Andre del Rio (1764–1849) developed compounds of the element vanadium.
- Charles Hatchett (1765–1847) isolated the element niobium in ores.
- A century before Einstein, the deflection of light by the Sun's gravity was calculated by Johann van Soldner (1776–1883), using Newtonian physics.
- Ultraviolet radiation was discovered by Johann Ritter (1776–1810).
- Johann Blumenbach (1752–1840), the great paleontologist, published *A Case Study of the Archeology of the Earth*. Goethe's recent prediction that fossils would be classified according to geological age came true in Blumenbach's work.

The literary rage of 1801 was the romance *Atala*, by François-René Chateaubriand (1768–1848). It was one of the cult books of the age and had bewitched many reading females into a sort of idolatry of the writer. Set in American Indian country, it is a heart-rending story of love and death that must have made many ladies swoon.

Discovery by Piazzi and Cacciatore

"It is a night like any other, and yet different. All across Europe, and indeed wherever people keep time by the Gregorian calendar, tonight will be celebrated with special fervour. For this is the night when not only another year passes, but another century passes: it is December 31, 1800" (Cunningham, 2001).

The wine flowed freely throughout taverns and homes in Sicily last night; January 1, 1801 is definitely not the day for sober reflections on the past century, much less the past year. The most important thing about today for a 54-year-old monk in Palermo is the simple fact that it is going to be clear tonight. And tonight, like so many before, he climbs the steps to his Ramsden Circle in the observatory at the top of the royal palace to make a few more precious measurements for his great star catalogue. Oblivious to the cold and the excesses of merriment of the last few hours, he points his meridian telescope at the constellation Taurus and makes one of the most important discoveries of the newly born century.

Giuseppe Piazzi was engrossed in updating a star catalogue by Francis Wollaston (1789). Replete with inaccuracies, the catalogue had to be checked star by star, a task Piazzi was performing with the 1.5-m vertical circle to determine star positions (Chinnici, Fodera-Serio, & Brenni, 2001). This instrument, built in England by Jesse Ramsden (1735–1800) was the finest astronomical circle in existence (see Appendix A in this book). For any particular star, Piazzi could observe it for only 2 min a night as it passed through the meridian (Fig. 2.1).

At 8:43 p.m., local mean time, he was startled to see in Taurus a light which was not in the catalogue: the veil which for so long had covered the unseen planet had been lifted.

The next night, he found the star had shifted position about 4′ to the west and slightly less to the north. He saw it again on 3 and 4 January, and continued following

Fig. 2.1 A painting of Giuseppe Piazzi pointing to Ceres, his discovery in the sky. In the collection of Palermo Observatory

Fig. 2.2 Niccolò Cacciatore (In the collection of Palermo Observatory)

its movement until 11 February. We are indebted to an English traveler, Captain Basil Hall (1841), for a first-hand account of what actually happened. The story was related to him by Piazzi's assistant, Niccolò Cacciatore (1780–1841), with the moment of realization highlighted here in bold (Fig. 2.2):

> *Most people are aware that the celebrated astronomer Piazzi discovered the small planet Ceres at Palermo in this very observatory, with an instrument of Ramsden's which we had the satisfaction of seeing. It was made on the 1st of January, 1801, at which period the present astronomer, Cacciatore, was Piazzi's assistant in the observatory of which he is now the chief. As Piazzi was at that time engaged in making the noble catalogue of the stars, which has since become so well known, he placed himself at the telescope, and observed the stars as they passed the meridian, while Cacciatore wrote down the times, and the polar distances, as they were read off by his chief. Certain stars passed the wires, and were recorded as usual on the 1st of January, 1801. On the next night, when the same part of the heavens came under review, several of the stars observed the evening before were again looked at, and their places recorded. Of these, however, there was one which did not fit the position assigned to it on the previous night, either in right ascension, or in declination. "I think," said Piazzi to his companion, "you must, accidentally, have written down the time of that star's passage, and its distance from the pole, incorrectly." "To this," said Cacciatore, who told me the story, "I made no reply, but took especial pains to set down the next evening's observations with great care. On the third night there again occurred a discordance, and again a remark from Piazzi that an erroneous entry had probably been made by me of the place of the star. I was rather piqued at this," said Cacciatore, "and respectfully suggested that possibly the error lay in the observation, not in the record. Under these circumstances, and both parties being now fully awakened as to the importance of the result, we watched for the transit of the disputed star with great anxiety on the fourth night. When lo, and*

> *behold! it was again wide of the place it had occupied in the heavens on the preceding and all the other nights on which it had been observed. '**Oh, oh**!' **cried the delighted Piazzi**, '**we have found a planet** while we thought we were observing a fixed star; let us watch it more attentively.'" The result soon confirmed this conjecture, and thus was made one of the most interesting, and I may say useful, astronomical discoveries of modern times.*

Piazzi's own (sanitized) account of these nights are in his first monograph on Ceres (Piazzi 1801a, 1802a) printed in Chap. 7. Based on the account just given, Cacciatore has the right to be credited as the co-discoverer of Ceres. To announce the discovery, Piazzi made the decision to send a very brief notice to the *Journal de Paris*. Based on just the first four nights of observations, it erroneously included a mention of the star Mayer 19 as being near the object. A notice about a "new comet" dated January 15 appeared in the *Journal de Paris* in February 1801. [see Chap. 6 for this image] "Sicily—Palermo, January 15. On the 1st of this month, a new comet in the shoulder of Taurus, near the 19th star of Mayer, has been discovered from our observatory. It was observed on the first, second, third and fourth, as it passed the meridian. Although it is not covered with any kind of nebulous spot, it still cannot be seen with the naked eye. Its movement is retrograde; it goes forward toward the north."

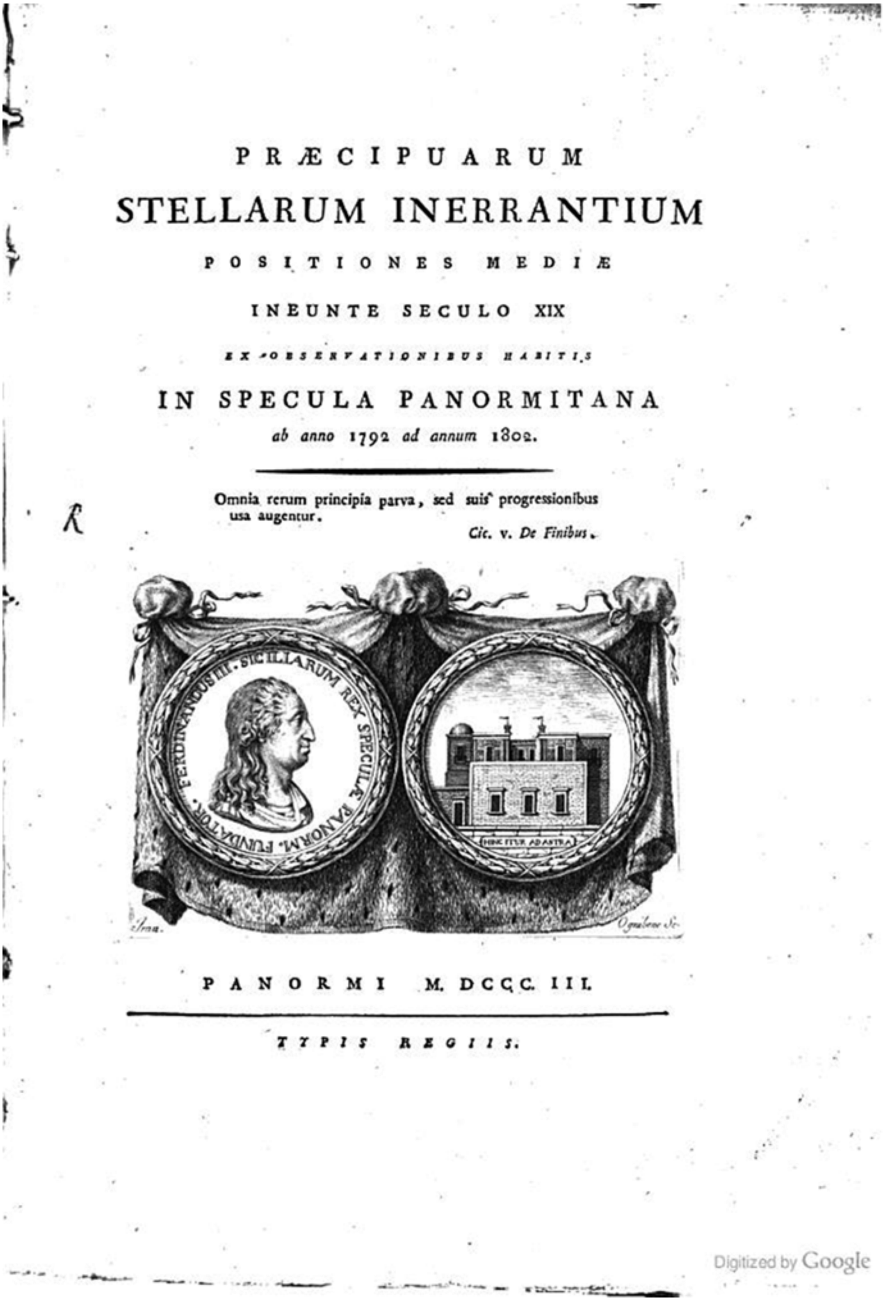
Lalande first learned of the discovery of Ceres from this popular press report. Baron von Zach states that Lalande wrote him a letter in February about Piazzi's discovery. But Zach was under the assumption that Piazzi had written directly to Lalande—he never imagined that the great French astronomer had to find out about it in the popular press! Zach even published his belief that "Lalande had received the first report of the comet from Piazzi." (MC, June 1801 issue, p. 592, and MC, July 1801 issue, p. 54). The *Journal de Paris* article appeared to be an official news release from the Palermo observatory, but Piazzi is not mentioned by name. It must have been sent by sail for a southern French port (likely Marseilles), and then on to Paris. Thus, Johann Bode was technically right to claim that he (as an individual) had been the first (March 20) to be informed about the discovery, although astronomers in Paris knew about it 3 weeks earlier. In this Bode was lucky, as Piazzi also sent a letter to Oriani on the same day (January 24), but Oriani did not see it until April 5 due in part to disruptions caused by war.

The Mistaken Identity of Mayer 109

A critical point to note about the content of the announcement is the identification of the object as being near the 19th star of Mayer. Mayer 19 was in fact some 45° away from the position of Ceres! The star Mayer 19 is 60 Piscium = BD +5.104. It was Piazzi's star 0–183.

Did the January 15 press announcement intend to say Mayer 109, rather than Mayer 19? Figure 2.3 shows the seven stars from Piazzi's 1814 catalog with right ascensions in the range 3 h 16 m–3 h 32 m and declinations +14 to +18°.

Although III-70 (Mayer 109) was 9 time minutes ahead of Ceres, it perhaps did make some sense for Piazzi to mention it, because its declination is similar to what



PRÆCIPUARUM
STELLARUM INERRANTIUM
POSITIONES MEDIÆ
INEUNTE SECULO XIX
EX OBSERVATIONIBUS HABITIS
IN SPECULA PANORMITANA
ab anno 1792 *ad annum* 1802.

Omnia rerum principia parva, sed suis progressionibus usa augentur.
Cic. v. De Finibus.

PANORMI M. DCCC. III.

TYPIS REGIIS.

Fig. 2.3 The seven stars from Piazzi's 1814 catalog. The correct position of Ceres on its discovery night is given, along with the incorrect position 30′ north, as reported by Piazzi in his discovery letter of January 24, 1801 he had (erroneously) given for Ceres in his January 24 letters, and during January 1–4 Ceres was moving more-or-less in that direction. Stars III-82 and (in reality) III-92 may have been closer, but they were not in previous catalogs.

As for references to Mayer's catalogue, he clearly means Tobias Mayer (1775), as he lists many of these stars in his star catalogue (Piazzi, 1814). [Tobias Mayer, 1723–1762, Director of Goettingen University Observatory from 1751 to 1762; see Forbes, 1967, 1980]. In his catalogue, Piazzi clearly concentrated on the Tobias Mayer (zodiacal) stars (of which there were 998), well known to potential readers.

Wollaston's Star Catalog and the Mistaken Identity of Lacaille 87

Piazzi created confusion in his first monograph on Ceres, where he begins by explicitly stating he was searching on the night of January 1, 1801, "for the 87th of the Catalogue of the zodiacal stars of Mr. la Caille" (LaCaille, 1763).

In reality Piazzi was concentrating on Francis Wollaston's (1789) star catalog. He was looking for the Wollaston's 'Mayer 87' and realized that the position given did not agree with Mayer's 'Mayer 87'. The real Mayer 87 (μ Arietis) is Piazzi II:153. By searching for Wollaston's 'Mayer 87' he found both Lacaille 87 and Ceres! The seven stars from Piazzi's 1814 star catalog that were near the position of Ceres on its discovery night, January 1, 1801 are shown in Table 2.1. Piazzi found Ceres preceding the star Lacaille 87. These stars are all plotted in Fig. 2.3. The designations of the stars by Piazzi, Mayer and Lacaille are given, followed by the right ascension, declination, and magnitude as given by Piazzi in his catalog.

It is only in his second monograph on Ceres that Piazzi (1802a) mentions Wollaston, but even in this expanded treatise he does not mention Wollaston's error or Mayer 87. He simply states that "When in 1792 I started to study the stars that are in Mr. Wollaston's catalogue, I decided to study the stars that would be in the

Table 2.1 The seven stars from Piazzi's 1814 star catalogue that were near the position of Ceres on its discovery night

Piazzi	Mayer	Lacaille	RA(1800)	Decl.	Mag.
III-70	109		03 18 26.24	+16 03 46.6	8
III-82			03 21 41.60	+15 54 56.5	8
III-87	113		03 22 45.90	+17 09 58.8	7–8
III-92			03 24 51.02	+15 48 28.0	8
III-99	114		03 26 35.34	+14 45 47.3	7–8
III-103		87	03 28 08.04	+15 52 32.5	7
III-120			03 31 17.78	+16 38 29.0	8

Given are their designations, positions and magnitudes (III is the hour of R.A., here in Roman numerals, which is what Piazzi used, except for 0 h) area of my telescope and to note them.” We know the true sequence of events because it was reported in a letter from Oriani to Bode on 17 June 1801, and related by Bode (1802a) in his book about Ceres. Why would Piazzi deliberately gloss over the details of his search methodology that crucial night of January 1, 1801? It was likely part of the deliberate whitewash that also eliminated any notion that Cacciatore was the co-discoverer of Ceres, as Piazzi became obsessed with literally owning the new planet.

Five years before his star catalog was published, Wollaston (1784) presciently remarked on the value of making careful measurements of the fixed stars: “Whoever undertakes a constellation, or district, should determine to examine it with as great accuracy as he can; yet never be ashamed to let others know of his mistakes… His frequent sweeping over his district in this way may lead him to a discovery which might escape a more regular astronomer. But whoever can, ought to do more. By degrees the exact position of every star he has noted down may be ascertained, by the method practised by Mr. De la Caille.”

Why did Piazzi not mention Lacaille 87 in the press release of January 15? Probably because he did not consider it relevant. In any case it was Bode, not Piazzi, who recognized (i.e., after mid-June) that the star Wollaston had listed as Mayer 87 was actually Lacaille 87. Although it also became Piazzi’s III:103, Piazzi would not have called it that in January—he would not have known what to call it. So that is why he did not mention it, either in his January 24 letters or, more particularly, in the January 15 press release, for which he therefore picked a relevant star he could name—i.e., Mayer 109. It is also apparent that Piazzi’s reference to Lacaille 87 in his first monograph was *ex post facto* and thus quite misleading for no apparent purpose.

If Piazzi wrote the January 15 notice why did he send it to the popular press instead of his peers? Even though the precise origin of the ‘press release’ remains obscure, the erroneous inclusion of the “19th star of Mayer” appears to have been a simple misprint, as Mayer 109 was meant instead.

Lalande (1802a) had this to say about LaCaille’s catalogue and its importance in the discovery of Ceres [for more on star catalogues, see Appendix D]:

> *Among the benefits that I announce in my* Histoire célèste *of 50,000 stars*, *I stated one*: *to find among all those stars the observations of a new planet if it was coincidentally discovered. Until today our efforts have been fruitless*; *but I do not give up hope. I thought to have this satisfaction when on March 13*, *1797*, *I observed a star at 8 h 19′ and 15° 58′ zenith distance. This is almost the position the new planet had on that day*; *but it was 27′ too much in declination*; *probably the planet was in the telescope but on that day my nephew observed only 14–16° of zenith distance. This planet could neither be found among La Caille*’*s zodiacal stars since it is too small. But this is an obligation that we owe to this great astronomer forty years after his death*: *his precious star catalogue that cost his life*, *gave Piazzi the occasion to verify the 87th star*, *the small star that he observed which had a neighbour and which would have been ignored even longer without La Caille*’*s catalogue*. (*Coelum Australe Stelliferum* [Southern Sky Star Catalog] appeared a year after La Caille’s death in 1762.)

The Discovery Letter

Piazzi's next communication about his discovery came 9 days later when he sent word to only two people: his friend Barnaba Oriani (1752–1832), an astronomer at Brera Observatory in Milan and the German astronomer Johann Bode. It has often been asserted (e.g., Forbes, 1971) that Piazzi sent a letter at the same time to Zach, but is this mistaken. The letters he wrote to them on January 24, 1801 were similar, but not identical. In the letter to Oriani (see Chap. 11), Piazzi prophetically states that his discovery may be "…better than a comet." The text of the letter to Oriani is more descriptive than the one he sent to Bode. Here is how Bode described the letter he received (Fig. 2.4):

> *I had received on the 20th of March* (*1801*) *a letter from Mr. Piazzi*, *the King*'*s astronomer of the Two Sicilies in Palermo*, *dated the 24th January in which he told me the following*. "*I discovered on 1 January of this year a comet* (*such was what he called the new star*) *at 51° 47′ right ascension and at 16° 8′ north declination* (*consequently in Taurus*). *On 11 January it had changed its retrograde movement to direct*; *23* (*January*) *it was at 51° 46′ right ascension and 17° 8′ north declination. I will continue to observe it and I hope that I can continue to follow it during all of February. It is very small and resembles a star of the 8th magnitude without any perceptible nebulosity*." *From the month of February on*, *the public papers announced the discovery made by Mr. Piazzi*, *without indicating the appearance*, *nor the*

a

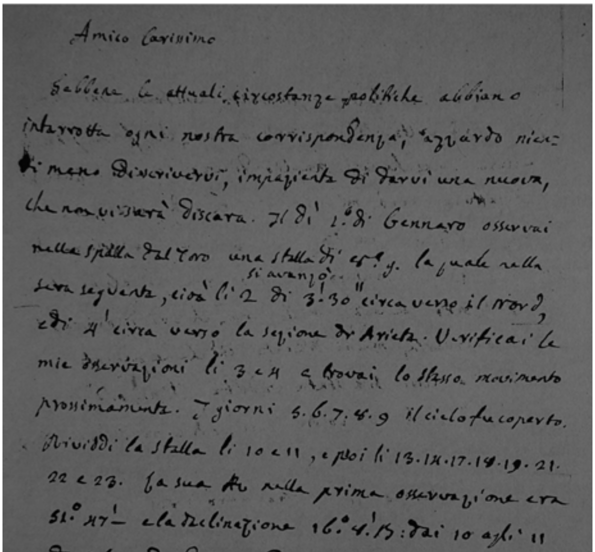
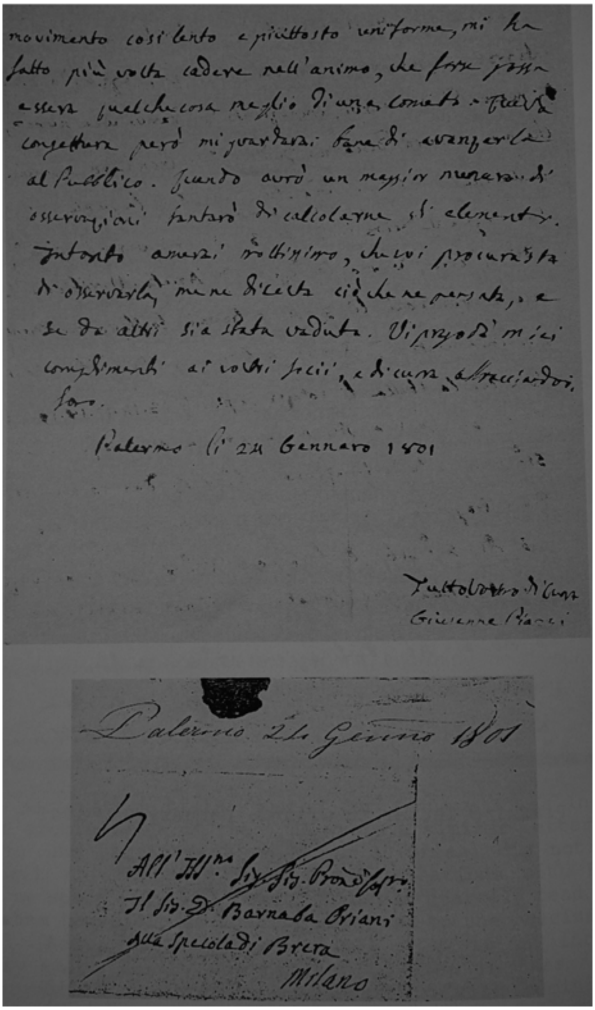

Amico Carissimo
Sebbene le attuali circostanze politiche abbiano
interrotta ogni nostra corrispondenza, riguardo niente
di meno dovere, impaziente di darvi una nuova,
che non vi sarà discara. Il dì 1.° di Gennaro osservai
nella spalla del Toro una stella di 8.a g. la quale nella
si avanzò
sera seguente, cioè li 2 di 3.' 30" circa verso il nord,
e di 4' circa verso la regione di Ariete. Verificai le
mie osservazioni li 3 e 4 e trovai lo stesso movimento
prossimamente. I giorni 5.6.7.8.9 il cielo fu coperto.
Rividdi la stella li 10 e 11, e poi li 13.14.17.18.19.21.
22 e 23. La sua AR nella prima osservazione era
51.° 47' e la declinazione 16.° 8'.15: dai 10 agli 11
da retrograda divenne diretta, e nell'osservazione di
23 AR 51.° 46', declinazione 17.° 8'. Io ho annunziato
questa stella come cometa; ma il non essere essa
accompagnata da alcuna nebulosità, e più il suo

Fig. 2.4 (**a**, **b**) The discovery letter (pages 1 and 2) of January 24, 1801, sent by Piazzi to Oriani b


movimento così lento e piuttosto uniforme, mi ha fatto più volte cadere nell'animo, che forse possa essere qualche cosa meglio di una cometa. Tali congetture però mi guarderai bene di avanzarle al Pubblico. Quando avrò un maggior numero di osservazioni tenterò di calcolarne gli elementi. Intanto avrei piacere, che voi procuraste di osservarla, me ne diceste ciò che ne pensate, e se da altri sia stata veduta. Vi prego de' miei complimenti ai vostri socii, e di cuore abbracciandovi sono.

Palermo li 24 Gennaro 1801

Tutto Vostro di Cuore
Giuseppe Piazzi

Palermo 24 Genno 1801

All'Ill.mo Sig. Sig. Pron. Colmo
Il Sig. D. Barnaba Oriani
alla Specola di Brera
Milano

Fig. 2.4 (continued)

position, nor the movement of this singular comet. However, at the only reading of Mr. Piazzi's announcement the terms of his letter struck me and I quickly supposed that this small star without nebulosity odd.

As a discovery announcement, this one was lacking in nearly every detail to make it useable, as Zach made clear in his dissection of the information it contains (MC, June 1801 issue, p. 592):

Piazzi's reported observations are for the calculation of a path partly incomplete and partly inadequate. 1) His two known observations are reported only in minutes and are therefore only approximate. 2) At least three observations are required to calculate the path of a comet or a planet. 3) The times of the observations are not given. With the first one, one may assume

> *that at least the "next" minute of the observations is correctly given. As far as the second difficulty is concerned, Piazzi perhaps intentionally withheld the third observation, perhaps because he wanted first to calculate the path of the planet himself (since he did regard it as such before January 24). But if this was the case, then he did give away his third observation in a way, in that he referred to the circumstance of the planet's standstill from the 10th to the 11th of January. Oriani and Bode knew how to use this circumstance—and I likewise used it—to calculate a more precise path of this planet from these sparse observations.*

In this extraordinary passage, Zach reveals to all the readers of his journal the *Monthly Correspondence* that Piazzi was deliberately concealing data from other astronomers so that he, and only he, could have the glory of ascertaining the orbit of Ceres. Zach slyly notes that this attempt backfired by Piazzi's mention of the standstill, but the lack of detail was still a major hindrance to orbit determination. But Zach protests too much. He must have been aware that Piazzi was making meridian observations, so why did he complain that the times of observation were not given? If he could use knowledge of the Ceres' stationary point to calculate the orbit, he could surely have figured out the observation times, at least to the accuracy with which Piazzi gave the R.A. The issue of timing has consistently been inaccurately portrayed by historians: "It was not until January 23, 1801, that Piazzi took any steps to communicate his discovery, when he sent letters to Barnaba Oriani in Milan, Johann Bode in Berlin, and Franz Xaver Zach at Seeberg near Gotha. Because of war conditions, these letters took more than 3 months to reach Germany." (Howse, 1989).

In reality, Piazzi dated his letters January 24, and he did not write to Zach at this time. Also, the letter to Germany took 2 months, not 3, to arrive. In any case these private letters were sent more than a week after a notice was sent to the *Journal de Paris*.

The Half Degree Error

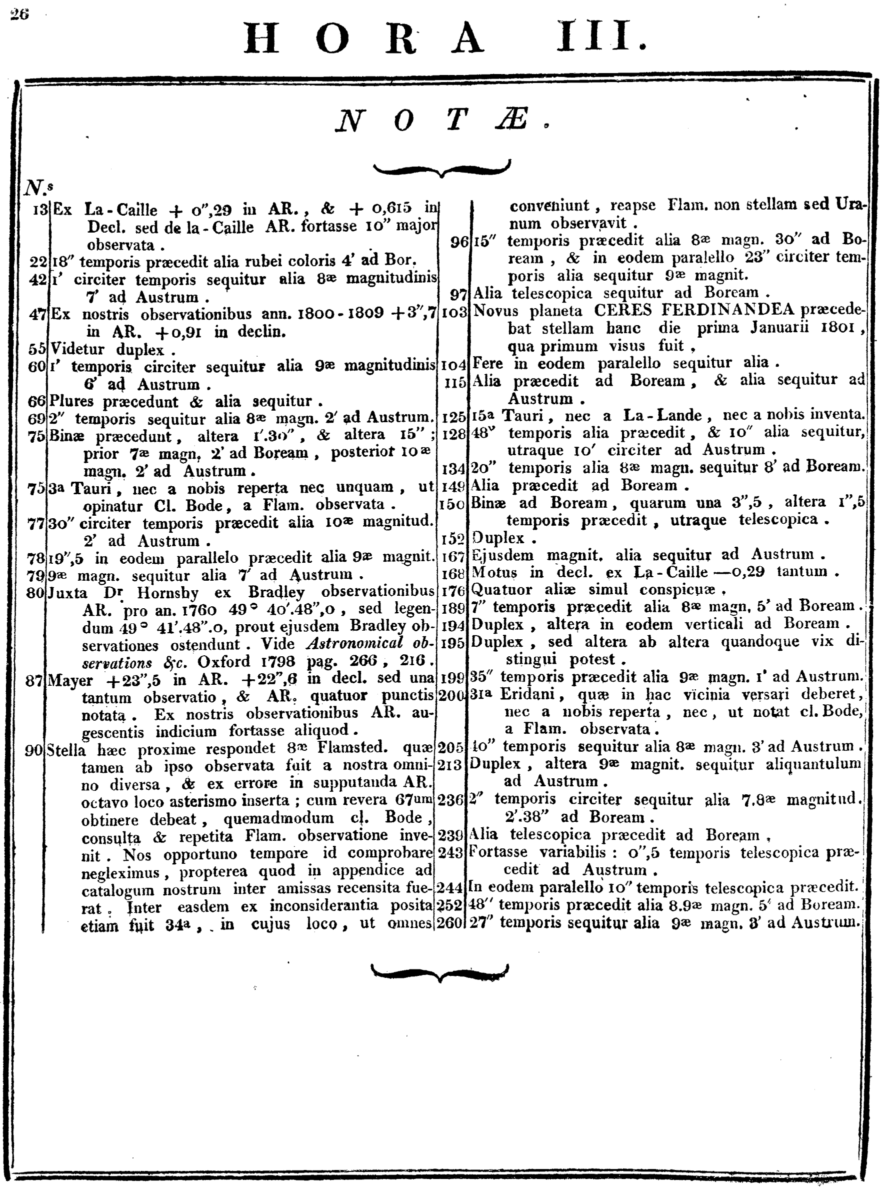
Oriani, Bode and Zach were also led astray by the declination figures given by Piazzi in the discovery letter. The position given was actually 30 arcmin away from the true position. Bode (1802a) wrote about the implication of this error in attempts to determine an orbit:

> *I had already tried to determine preliminarily from the very first data that star's heliocentric longitude and latitude in a presumed circular orbit. But because Mr Piazzi stated in his letter the declination on the day of its discovery, the first day of January, 15° 38' instead of 16° 8', and thus smaller by half a degree, the inclination of the orbit, which was according to the first observations 6°, therefore doubles and is almost 12°.*

Zach (1801b) wrote about the error in a July 6 letter to Oriani, and implied it was not accidental: "Burckhardt suspects that his (Piazzi's) observations are very faulty; actually, he (Piazzi) gave you and Bode a false declination by a half-degree. Burckhardt says there are many others. Now I cannot conceive how an experienced observer such as Piazzi, armed with the best instruments—an entire wall quadrant and a Ramsden meridian telescope—could commit such similar mistakes in the meridian observations?"

Bode (1802a, p. 18) was more charitable, writing "… that this must have been a slip of the pen, as could be seen from the regular progression of the right ascension."

Piazzi identifies the star III-103 Tauri (also called BD +16.484) in the Latin notes to his star catalog: "103 The new planet CERES FERDINANDEA preceded this star on the first day of January 1801, when it was first seen." It is interesting that this star's declination is midway between the +16° 08′ given for Ceres in the January 24 letters and the correct value of +15° 38′ (Piazzi, 1814, p. 26) (Figs. 2.5 and 2.6).


26

HORA III.

NOTÆ.

*N.*s

13 Ex La-Caille + 0",29 in AR., & + 0,615 in Decl. sed de la-Caille AR. fortasse 10" major observata.
22 18" temporis præcedit alia rubei coloris 4' ad Bor.
42 1' circiter temporis sequitur alia 8æ magnitudinis 7' ad Austrum.
47 Ex nostris observationibus ann. 1800-1809 +3",7 in AR. +0,91 in declin.
55 Videtur duplex.
60 1' temporis circiter sequitur alia 9æ magnitudinis 6' ad Austrum.
66 Plures præcedunt & alia sequitur.
69 2" temporis sequitur alia 8æ magn. 2' ad Austrum.
75 Binæ præcedunt, altera 1'.30", & altera 15"; prior 7æ magn, 2' ad Boream, posterior 10æ magn. 2' ad Austrum.
75 3a Tauri, nec a nobis reperta nec unquam, ut opinatur Cl. Bode, a Flam. observata.
77 30" circiter temporis præcedit alia 10æ magnitud. 2' ad Austrum.
78 19",5 in eodem parallelo præcedit alia 9æ magnit.
79 9æ magn. sequitur alia 7' ad Austrum.
80 Juxta Dr. Hornsby ex Bradley observationibus AR. pro an. 1760 49° 40'.48",0, sed legendum 49° 41'.48".0, prout ejusdem Bradley observationes ostendunt. Vide *Astronomical observations &c.* Oxford 1798 pag. 266, 216.
87 Mayer +23",5 in AR. +22",6 in decl. sed una tantum observatio, & AR. quatuor punctis notata. Ex nostris observationibus AR. augescentis indicium fortasse aliquod.
90 Stella hæc proxime respondet 8æ Flamsted. quæ tamen ab ipso observata fuit a nostra omnino diversa, & ex errore in supputanda AR. octavo loco asterismo inserta; cum revera 67um obtinere debeat, quemadmodum cl. Bode, consulta & repetita Flam. observatione invenit. Nos opportuno tempore id comprobare neglex. imus, propterea quod in appendice ad catalogum nostrum inter amissas recensita fuerat. Inter easdem ex inconsiderantia posita etiam fuit 34a, in cujus loco, ut omnes conveniunt, reapse Flam. non stellam sed Uranum observavit.
96 15" temporis præcedit alia 8æ magn. 30" ad Boream, & in eodem paralello 23" circiter temporis alia sequitur 9æ magnit.
97 Alia telescopica sequitur ad Boream.
103 Novus planeta CERES FERDINANDEA præcedebat stellam hanc die prima Januarii 1801, qua primum visus fuit,
104 Fere in eodem paralello sequitur alia.
115 Alia præcedit ad Boream, & alia sequitur ad Austrum.
125 15a Tauri, nec a La-Lande, nec a nobis inventa.
128 48" temporis alia præcedit, & 10" alia sequitur, utraque 10' circiter ad Austrum.
134 20" temporis alia 8æ magn. sequitur 8' ad Boream.
149 Alia præcedit ad Boream.
150 Binæ ad Boream, quarum una 3",5, altera 1",5 temporis præcedit, utraque telescopica.
152 Duplex.
167 Ejusdem magnit. alia sequitur ad Austrum.
168 Motus in decl. ex La-Caille —0,29 tantum.
176 Quatuor aliæ simul conspicuæ.
189 7" temporis præcedit alia 8æ magn, 5' ad Boream.
194 Duplex, altera in eodem verticali ad Boream.
195 Duplex, sed altera ab altera quandoque vix distingui potest.
199 35" temporis præcedit alia 9æ magn. 1' ad Austrum.
200 31a Eridani, quæ in hac vicinia versari deberet, nec a nobis reperta, nec, ut notat cl. Bode, a Flam. observata.
205 10" temporis sequitur alia 8æ magn. 3' ad Austrum.
213 Duplex, altera 9æ magnit. sequitur aliquantulum ad Austrum.
236 2" temporis circiter sequitur alia 7.8æ magnitud. 2'.38" ad Boream.
239 Alia telescopica præcedit ad Boream,
243 Fortasse variabilis: 0",5 temporis telescopica præcedit ad Austrum.
244 In eodem paralello 10" temporis telescopica præcedit.
252 48" temporis præcedit alia 8.9æ magn. 5' ad Boream.
260 27" temporis sequitur alia 9æ magn. 8' ad Austrum.

Fig. 2.5 The Latin note about Ceres in Piazzi's star catalog

Ineunte Seculo XIX.

I I I.

Differentiæ.						Animadversiones.
In Ascensione Recta juxta.			In Declinatione juxta.			
Flamstedium.	De la Caille.	Tob. Mayer.	Flamstedium.	De la Caille.	Tob. Mayer.	
S.	S.	S.	S.	S.	S.	
. . .	. . .	. . .	. . .	. . .	. . .	La Lande − 2″ in A.R. − 3″ in declinatione.
. . .	+ 8	. . .	. . .	− 10	. . .	Novus Planeta *Ceres Ferdinandea*, præcedebat stellam hanc die prima Januarii 1801, qua primum visus fuit.
. . .	. . .	. . .	. . .	. . .	. . .	La Lande − 12″ in A.R. + 7″ in declinatione. Fere in eodem paralello sequitur alia.
. . .	. . .	. . .	. . .	. . .	. . .	La Lande + 2″ in A.R. − 2″ in declinatione.
+ 17	− 13	. . .	− 21	+ 2	. . .	La Lande + 1″ in A.R. + 8″ in declinatione. Juxta Tobia Mayer annuus motus proprius in A.R. − 0″,068. Cl. Zach + 1″ in A.R.

Fig. 2.6 Another entry from Piazzi's star catalog, noting the "New planet Ceres Ferdinandea"'

Since Piazzi would have been working with the 15′ declination difference of Ceres and the star, it is likely he accidentally added 15′ to, rather than subtracted 15′ from, the star's declination in approximating the January 1 position for Ceres in his January 24 letters! He obviously had not completed the reductions yet. There was nothing insidious in this. There was no reason for him deliberately to mislead Oriani.

Even towards the end of 1801 errors in reduction particularly peeved Zach, who wrote twice to Carl Gauss about them (see Chap. 14):

- November 29: "Piazzi has been wrong with several reductions, especially with the mean times."
- December 2: "I cannot understand how Piazzi could be so wrong on average in reducing his times; there are errors of 12 time seconds, like on 3 January."

Piazzi was creating a large star catalog at this time, which required him to be meticulous. His fellow astronomers rightly wondered how such a careful observer could have made such errors. That said, the final positions produced by Piazzi for Ceres are in fact of excellent quality. A modern orbit computer would have no trouble getting an excellent least–squares result from them—something that even Gauss was not able to do at the time. Among the 19 complete observations, one R.A. gives a residual of 13″ another 9″ and all the other R.A. and Decl. residuals are less than 5″. For the time period this is quite remarkable, Piazzi's positions being superior to the non-meridian observations by Zach and Olbers at recovery in December 1801/January 1802 (Cunningham, Marsden, and Orchiston, 2011a). "It is an interesting commentary which should not be lost to view in these times of highly organized research teams, overlaid with administrative facades, that when the first minor planet was discovered, it was found by a lone but ardent worker who was not part of a highly organized research team at all" (Herget, 1974).

The discovery announcement was made in the *Monthly Correspondence* in the summer of 1801. The beautiful order in the Solar System established by the discovery of Ceres seemed to fulfill the hopes and desires of generations of scientists and philosophers. It was the beginning of the great role played by asteroids in the rapid development of both theory and observation.

Before we leave Piazzi and his observatory that night, mention must be made of the man who produced the catalog he was using. Francis Wollaston (1737–1815) had a private observatory with a triplet telescope Peter Dollond (1730–1821). His autobiography *The Secret History of a Private Man* (1795) explains that his pursuit of astronomy was intended to separate him at a "distance from the misrepresentations of narrow-minded bigots." In 1802, his son, William Hyde Wollaston (1766–1828), was the first to observe the dark lines in the spectrum of the Sun (later rediscovered and named Fraunhofer lines, the same Fraunhofer who was buried in that rubble in 1801). In an interesting asteroid connection, William discovered an element which he named palladium after the newly discovered asteroid Pallas; and he worked with William Cary (1759–1825), an instrument maker who served an apprenticeship under Jesse Ramsden who built the transit circle Piazzi used to find Ceres (Chaldecott, 1979). (See Appendix D in this book for more on Francis Wollaston's star catalogue.)

When Was Ceres Seen for the First Time?

"There is nothing so minute or inconsiderable that I would not rather know it than not." In the spirit of Dr. Samuel Johnson's dictum, the following remarkable (and anonymous) article from 1815 is hereby presented. It relates a response to a question posed to the great Swiss mathematician Leonhard Euler (1707–1783). The discussion, held in 1746, was about the great comet of 1744 (Fig. 2.7):

> *In Michael Christoph Hanow's Nature's and Economy's Curiosities, edited by Joh. Dan. Titius, vol. II, Leipzig 1753, on page 565 you can find the following: "Mr. Euler answered various questions regarding comets and he also mentioned a comet or new planet, whose perihelion is slightly greater than that of Mars and its aphelion closer than that of Jupiter and whose path ends every four years and then returns. But he does not say who calculated its orbit nor when and where it appeared—he only distinguishes it from the actual one which appeared two years ago*[ie the 1744 comet]. *Probably it is the one that appeared last year for a short time only which might be calculated in Paris according to Bouguer's Method or Gregorii's instruction in London."*
>
> *Euler's paper does not contain any word of this but he seems to know that this star was none other than Ceres since its orbit is between Jupiter and Mars, its orbital period is four years and 282 days and since its orbit has an inclination of 10.5 degrees it could only be taken as a comet* [*or new planet*]. *Juno, Pallas and Vesta are even smaller and it is thus less believable that one of those was the star mentioned by Euler.*

Fig. 2.7 Leonhard Euler

The above account was published by Streit (1815). The Bouguer mentioned here is the French mathematician and astronomer Pierre Bouguer (1698–1758). He was a pioneer of photometry. His method is the "straight line method" for obtaining the magnitude of a celestial body. Gregorii is likely the Latinized version of the name of the mathematician James Gregory (1638–1675).

Chapter 3
The Recovery of Ceres

Searching in Vain

The astronomers of the day were presented with a conundrum—the newly discovered planet was lost! Piazzi, anxious to publish the first orbit himself, was reluctant to send positional data to anyone. On May 30 Bode received a second letter from Piazzi simply telling him that the 'comet' had been followed until February 11. Piazzi did send his data to Lalande, however, enabling Lalande's colleague Johann Burckhardt (1773–1825) on May 31 to calculate both circular and parabolic orbits for the object (Fig. 3.1).

Bode did not receive Piazzi's observations until June 11, and Piazzi's own analysis was published shortly thereafter, concluding that it had a circular orbit with a mean distance of 2.69 AU. Ephemerides for the latter part of 1801 were published by Burckhardt, Olbers and Piazzi, but they ranged over 5° in the sky. Olbers was convinced that Ceres would be recovered by September, but it was not to be. The frustration across Europe was evident in the letters that flew between all the leading astronomers as the year 1801 dragged on. Each used the same forlorn expression—searching in vain:

Feb. 1801: "After mid-February 1801 it passed too early in the evening through the meridian to observe its culmination. Mr. Piazzi and his assistants searched for it with the best telescopes and utmost effort beyond the meridian and in the evening sky in vain." (Bode, 1802a)

Apr. 1801: "I searched for it on several clear evenings of April and May with a 2 ft Dollond night-telescope and a 3.5 ft achromat in vain." (Bode, 1802a)

May 1801: "I searched for it in vain on several clear evenings. Bode told me that he was just as unlucky." (Letter from Zach to Oriani, May 29)

Sept. 1801: "Probably, you did not find any trace of Hera [Ceres] either. We chased her in vain at Seeberg." (Letter from Ende to Olbers, Sept. 22, 1801)

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
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197 Num. 18. 198

Kaiserlich privilegirter

Reichs-Anzeiger.

Dienstags, den 19 Januar. 1802.

Nachricht für die Freunde der Astronomie.

Den Freunden der astronomischen Wissenschaften glaube ich einen angenehmen Dienst zu erweisen, wenn ich ihnen über die Wiederauffindung des vom Professor Piazzi in Palermo den 1 Januar 1801 entdeckten neuen Hauptplaneten unseres Sonnensystems, welcher von den Astronomen für den, zwischen der Jupiters- und Mars-Bahn längst vermutheten Planeten gehalten wird, folgende vorläufige Nachricht ertheile.

Seit länger als acht Monaten wurde die Sichtbarkeit dieses äußerst kleinen und unansehnlichen Gestirns von den Astronomen in ganz Europa mit der größten Sehnsucht erwartet, und dasselbe in dem unermeßlichen Sternen-Heer mit unermüdetem Eifer aufgesucht, wie den Lesern der Monatlichen Correspondenz zur Beförderung der Erd- und Himmelskunde, welche der Oberstlieutenant Freyherr v. Zach im Verlage der Becker'schen Buchhandlung in Gotha herausgibt, hinlänglich bekannt ist. Alle Mühe und Anstrengung war lange Zeit vergeblich. Endlich, am siebenten December 1801, ist dem Director der Sternwarte Seeberg bey Gotha, Oberstlieuten. Freyhr. von Zach das Glück zu Theil geworden, dieses so schwer aufzufindende Gestirn zuerst wieder zu entdecken. Er fand es am 8 Decemb. (bürgerl. Zeitrechnung) um 6 Uhr 48 Minuten

Der Reichs-Anzeiger, 1 B. 1802.

des Morgens, zwischen dem Kopfe und dem nördlichen Flügel der Jungfrau, ungefähr 1½ Grad oberhalb des Sterns o im Auge der Jungfrau. Von dieser Zeit an bis zum 11 Jan. 1802 ist dieses Gestirn 8½ Grad in seiner scheinbaren Bahn fortgerückt. Nach den Beobachtungen des Freyhrn. v. Z. befand sich dieser neue Planet am 11 Januar auf dem rechten Arm der Jungfrau, ungefähr in der Mitte, unterhalb und zwischen den beyden Sternen Nro. 20 im Flügel und ε auf dem rechten Arm der Jungfrau. Gegenwärtig rückt dieses, nunmehr leicht aufzufindende Gestirn gegen den Stern Vindemiatrix.

Ohne von des Freyh. von Zach Entdeckung etwas zu wissen, entdeckte der berühmte Astronom und Arzt D. Olbers in Bremen diesen neuen Planeten, gerade am Jahrstage seiner ersten Entdeckung in Palermo, den ersten Januar 1802 gegen Mitternacht auf der rechten Schulter der Jungfrau; den 5 Januar sah er ihn wieder, bis unterhalb den Stern Nro 20 fortgerückt.

Bis zum ersten Januar 1802, denn so weit gehen die neuesten Pariser astronomischen Nachrichten, war der neue Planet in Frankreich noch nicht entdeckt; ob die Italienischen, Englischen und Nordischen Astronomen hierin glücklicher waren, muß die Zeit lehren. Dem Frhrn. von Zach gebühret also, aller Wahrscheinlichkeit nach, die Ehre der ersten Wiederauffindung dieses so schwer zu findenden Planeten, der den Astronomen so viele Anstrengung und schlaflose Nächte verursacht hat.

Dieser

Fig. 3.1 The recovery of Ceres was announced to the German public in the *Kaiserlich Privilegirter Reichs Anzeiger* of Jan. 19, 1802. The author of the article was D. J. S. Sennicke

Oct. 1801: "Mr. Messier had spent the night of October 3rd searching in vain and to console himself for the failed attempt he watched the beautiful spectacle that took place within the sign of Leo, in which Saturn, Jupiter, Venus and the moon were gathered round the bright star Regulus." (Bode, 1802a)

Nov. 1801: "Dr. Maskelyne, who wrote me November 16th, searched for it in vain with Burckhardt's elements." (Piazzi, 1802a)

DEC. 1801: “I searched for it in vain at the end of November and during December, from β Leo to η Virgo, examining that region of the sky with the greatest attention but without any success.” (Letter from Sniadecki to Zach, March 22, 1802)
DEC. 1801: “After constant bad weather we have frost now, and since the wee hours I have been eagerly searching for Ceres with my 3½ f. Dollond everywhere, even there where Gauss’ elements put her, but in vain.” (Letter from Bode to Olbers, Dec. 19, 1801)

In the summer of 1801 Great Britain’s Astronomer Royal, Nevil Maskelyne, gave full vent to his frustration and minced no words about what he thought of Piazzi:

> *There is great astronomical news: Mr. Piazzi, Astronomer to the King of the Two Sicilies, at Palermo, discovered a new planet the beginning of this year, and was so covetous as to keep this delicious morsel to himself for six weeks; when he was punished for his illiberality by a fit of sickness, by which means he lost track of it; and now a German Astronomer* [Olbers], *having got some of his observations, has calculated an orbit in our system as near as he could from such few observations, and had just informed us where he thinks it should be looked for in the course of the summer and autumn.*
>
> *It will not be so easy to recover, as the lost Cupid, when Venus said you might spy among 20 immediately by his air and complection. But this having been only a star of the 8th at first, & now for some months to come not bigger than the 10th or 12th will not be easily distinguished among 40,000 or 50,000 stars of similar appearance as it can be only known by its motion, which cannot be seen immediately but require observations of the relative position of several stars among which it is to be looked for. What a deal this imprudent Astronomer has to answer for! It is now publicly proposed, in a German publication, to all Astronomers in Europe to hunt for it.*

To understand Maskelyne’s reference to Venus and Cupid, we have to go back into Elizabethan English literature, specifically a poem by Barnabe Barnes (1569–1609) entitled “Parthenophe and Parthenophil” (1592):

> *VENUS aloud, for her son CUPID cried,*
> *'If any spy LOVE gadding in the street,*
> *It is my rogue! He that shall him betray,*
> *For hire, of VENUS shall have kisses sweet!*
> . . .
> *By many marks, the Boy thou mayst bewray!*
> *'Mongst twenty such beside, thou shalt perceive him!*
> *Not of a pale complexion, but like fire!*
> *Quick rolling eyes, and flaming in their gyre!*

Gauss to the Rescue

In this selection of orbit determinations of Ceres in 1801, Zach, Olbers and Piazzi all assumed circular orbits, while Burckhardt assumed an eccentricity to derive his elliptical orbit. Gauss derived an eccentricity that is close to the correct value of 0.097. The determinations of Zach, Burckhardt, Olbers and Piazzi appear in Bode (1802a). The first elements by Gauss appear in the December 1801 issue of *Monthly Correspondence,* p. 639 (Table 3.1).

Table 3.1 Early attempts at computing orbital elements of Ceres in 1801

	Zach	Burckhardt	Olbers	Olbers	Piazzi	Gauss
	May 24	June 9	June 24	August	August 1	October
Aphelion	66° 55′	68° 59′ 37″	67° 40′		68° 46′ 22″	330° 14′ 33″
Node	55 43	80 58 30	81 55	80 22 45	80 46 48	81 8 50
Incl.	6 14	10 47 0	7 54	11 3 36	10 51 12	10 32 19
Radius	3.071	2.5743	2.94746	2.730185	2.6862	2.74226
Period	5.382 years	4.13	5.04096	4.511	4.535	4.541
Eccentricity			0.0364			0.0832836

Fig. 3.2 An engraving of Carl Gauss in 1828

Depending on the text used, the terms "distance," "radius," and "semi-major axis" all mean the same and are expressed in astronomical units (the mean Earth-Sun distance). The "node" (more properly the longitude of the ascending node) is often denoted by Ω. Aphelion is the most distant point from the Sun; in tables the longitude of the aphelion is given.

In the table, the elements by both Zach and Olbers were based on the vague and erroneous data in Piazzi's discovery letter of January 24, resulting in an inclination of 6 or 7°. Burckhardt was in possession of the corrected data when he calculated his orbit in June, and of course Piazzi and Gauss also had these corrected data on which to base a calculation. The mathematical problem posed by the object Piazzi had named Ceres Ferdinandea was the sort which Newton had said belonged to the most difficult in astronomy (Fig. 3.2).

Among the subscribers to Zach's journal was a 23-year-old in Brunswick, Carl Friedrich Gauss (1777–1855). Gauss was just beginning work on a theory of the Moon

Table 3.2 The first five elements of Ceres derived by Gauss

	I	II	III	IV	V
Aphelion	330° 14′ 33″	330 33 20	326 53 50	326 27 38	324 37 11
Node	81 8 50	81 2 35	81 1 44	81 0 44	80 59 12
Incl.	10 32 19	10 36 30	10 36 21	10 36 57	10 37 9.55
Radius	2.74226	2.73548	2.76370	2.76728	2.78407
Period	4.541	4.524	4.593	–	4.497
Eccentricity	0.0832836	0.0705553	0.0819603	0.0825017	0.0879111

"when news of Piazzi's Ceres observations drew me in an entirely different direction. Eberhard Zimmermann [1743–1815], professor at the Carolineum in Brunswick, at the moment of his departure for Weimar gave me the numbers of the *Monthly Correspondence* in which the discovery of Ceres by Piazzi was reported." (Quoted in Dunnington, 1955, p. 71) Perhaps the greatest mathematical genius in history, Gauss easily developed the techniques necessary to compute the orbit of Ceres. To all others the problem seemed insurmountable; to Gauss, it was like an Ingres portrait—miraculously clear. As the weeks progressed, Gauss was able to refine his elements. The sixth and seventh elements of Gauss are the ones printed by Piazzi (1802a) Here are the first five (Table 3.2):

In November 1801, he published the orbital elements IV, which are very close to the currently accepted values. How did he do it? According to Taff (1985, p. 216), Gauss did not use the method he later published, although Taff′s assertion that Gauss' 1809 modification of the method in *Theoria Motus* "suffers from a fatal flaw" is disputed by Brian Marsden (1937–2010): "There are clues in the *Theoria Motus*—as well as contemporary publications and documents—that show rather clearly what Gauss did in 1801." (Marsden, 1985, 1995) This is how Gauss himself expressed his solution to the problem in 1809 in his landmark book (he alludes here to three other new planets, namely the asteroids Pallas, Juno and Vesta):

> *Some ideas occurred to me in the month of September in the year 1801 which seemed to point to the solution of the great problem. Just about this time the report of the new planet, discovered on the first day of January of that year with the telescope at Palermo, was the subject of universal conversation; and soon afterwards the observations made by that distinguished astronomer Piazzi from the above date to the eleventh of February were published. Nowhere in the annals of astronomy do we meet with so great an opportunity, and a greater one could hardly be imagined, for showing most strikingly, the value of this problem, than in this crisis and urgent necessity, when all hopes of discovering in the heavens this planetary atom, among innumerable small stars after the lapse of nearly a year, rested solely upon a sufficiently approximate knowledge of its orbit to be based upon these very few observations. Could I ever have found a more seasonable opportunity to test the practical value of my conceptions, that now in employing them for the determination of the orbit of the planet Ceres, which during these forty-one days had described a geocentric arc of only three degrees, and after the lapse of a year must be looked for in a region of the heavens very remote from that in which it was last seen? This first application of the method was made in the month of October, 1801, and the first clear night, when the planet was sought for as directed by the numbers deduced from it, restored the fugitive to observation. Three other new planets, subsequently discovered, furnished new opportunities for examining and*

verifying the efficiency and generality of the method. Several astronomers wished me to publish the methods employed in these calculations immediately after the second discovery of Ceres; but many things prevented my complying at the time with these friendly solicitations. The methods first employed have undergone so many and great changes, that scarcely any trace of resemblance remains between the method in which the orbit of Ceres was computed, and the form given in this work.

A Starfish on the Beach

"Then felt I like some watcher of the skies When a new planet swims into his ken."

John Keats, 1817

Armed with Gauss' search ephemeris, several astronomers began the search for Ceres. Baron von Zach had at his disposal the finest observatory in Germany—Seeberg.

It was here that Zach made the first new sighting of Ceres on December 7, 1801. As there were four small stars near the predicted position, he was not able to confirm the rediscovery. A break in the clouds on December 18 allowed another look. One of the stars was missing. On the night of December 31 Zach took another look, but it was not until the early morning hours of January 1, 1802, that he was able to conclusively identify the elusive Ceres. Remarkably, this was exactly a year after it had been discovered. On January 13 he proudly told Méchain of his monumental find: "I am pleased to tell you that I discovered the new planet on Dec 7, 1801. I saw the planet again on Dec 31, then again on Jan 11, 1802. There can be no doubt that this is Ceres Ferdinandea." Zach deliberately kept his recovery of Ceres a secret until after January 11, to be certain he had actually found it. He gave a full account of his recovery methodology in a letter of January 14, 1802, to Lalande (see Chap. 14).

Ceres was only 15–20′ from Gauss' prediction. Independently, Ceres was found again by Olbers on January 1, 1802, but he needed a second night, January 2, to confirm the motion. So either one combines December 7 of Zach and January 1 of Olbers as first sightings or December 31 of Zach and January 2 of Olbers as first sightings. The date January 1, 1802, is most often quoted as the rediscovery date of Ceres (Combes, 1975).

"With no-one else I would like to share the *small* honour of Ceres' rediscovery better than with my honourable friend Zach," wrote Olbers. "I say *share*: for Zach had *sighted* it indisputably earlier, but he seems not to have recognized it with certainty much earlier than me, and thus at least a shy claim for participation might be justified" (Olbers to Gauss, Jan. 22, 1802; see Chap. 12).

Piazzi was jubilant at the recovery. "Please express my compliments and my thanks to Mr. Gauss," he wrote Zach, "who has saved us much effort and work, and without whom perhaps it would not have enabled me to confirm my discovery." Piazzi himself saw Ceres again on February 23, 1802.

Baron von Zach basked in vindication. "Finally, the new primary planet of our solar system has again been discovered and found, like a starfish on the beach."

(Zach, 1802z) Gauss himself became famous, and his patron the Duke of Brunswick basked in the reflected glory. “The Duke of Brunswick,” declared Laplace, “has discovered more in his country than a planet: a super-terrestrial spirit in a human body” (Quoted in Teets & Whitehead, 1999).

The French Perspective

Not surprisingly, the French had a very different perspective on the recovery of Ceres. In this account by Lalande (1802a), Gauss is not even mentioned! (Fig. 3.3)

> *On December 7 Mr. von Zach found the new planet in Gotha at 8 h 8′ 10″ m. t. He observed its RA 178° 33′ 31″ and its declination about 11° 41½ : But it was only on December 31 he was assured, because he had observed four small stars of which he could not say with certainty which was the planet. Finally, on January 1, 1802, Dr. Olbers was equally lucky; on this day the planet was forming a triangle with two small stars which can be found in my* Histoire Célèste *and the following day the triangle had changed its shape so that the planet could be recognised. It was then continuously observed in different places, and Mr. Burckhardt calculated anew its orbit.*
>
> *On February 16 we got the new elements, and on the very same day Mr. Burckhardt started to calculate the perturbations experienced by this planet and which go to 30 minutes, an enormous impact that would change many of the elements. This work was done*

Fig. 3.3 Johann Burckhardt

within two days, which appeared to be incredible if one did not know the capabilities of Mr. Burckhardt. From the observations he again calculated elements that represented, up to 4" close, 15 months of observations. But since one month had passed without being able in Germany and Paris to observe it, I sent all astronomers of Southern France the position of the planet so we could be more certain to get observations and soon Mr. Thulis, Director of Marseille Observatory, wrote me that he was observing it every night. [Jacques Thulis, 1748–1810, was a close acquaintance of Zach.] *Those great perturbations, which could seem strange at first, are rather a result of Jupiter's proximity, the largest and most massive of all planets.*

Here are the elements which were considered for a long time the most exact and which Mr. Burckhardt employed for his tables of this planet which were used by everyone for their calculations. [Ed: the letter "z" denotes 30 degrees in the sky, so 5z = 150°]

Epoch 1802	*5z 5° 23′ 59″*
Aphelion	*10z 26° 33′ 37″*
Node	*2z 21° 2′ 30″*
Annual motion	*2z 18° 13′ 41″*
Mean distance	*2.76572 or 95028000 leagues*
Eccentricity	*0.0757*
Equation	*9° 0′ 40″*
Inclination	*10° 37′ 5″*
Tropical revolution	*1679 days, 67, or 4 years 7 months 9days 16 h 15′*
Sidereal revolution	*1680 days 00*

Synodical revolution or return of the conjunctions and oppositions 456.85 or one year 91 days 20 h 21′.

This inclination, greater than that of any other planet, forces us to stretch what we call the Zodiac. Indeed, Venus never moves away more than 8°, and we say that the width of the Zodiac is 16°. But since the new planet goes up to 18½ we are forced to give the Zodiac 37°.

On March 17 the planet should be in opposition and Mr. Burckhardt and my nephew were prepared several days ahead and obtained the most exact results possible with the excellent instruments of the house of the Champ-de-Mars.

On the 17th at 2 h 46′ 8″ m.t. reduced at the observatory, the longitude was 5z 26° 21′ 26′.5 and the latitude 17° 7′ 57″.5. Burckhardts tables give only 5″ more. Following Mr. von Zach you had 3 h 44′ 15″, 5z 26° 21′ 26″.5 and 17° 8′ 9″.0.

Thus one could say that the motion was already known with a singular precision and in one century the error will not exceed 7 minutes.

The influence of Burckhardt was also felt outside of France. Elements derived by Burckhardt, *not* by Gauss, were quoted in an English-language book by Charles Hutton (1815, p. 302). As these elements (undated by Hutton) differ from the two Burckhardt sets given previously in this chapter, they are as follows:

Epoch of 1801	67° 19′
Aphelion	326 9
Node	81 6
Incl.	10 37
Radius	2.7677
Period	4.606 years
Eccentricity	0.0784

Passage of the aphelion 8 h

Another English-language book of the previous year by Peter Barlow (1814) also quoted these elements by Burckhardt, but balanced the equation by giving elements derived by Gauss. These Gaussian elements are quoted in a letter by Zach to Lalande on Nov. 26, 1801 (see Chap. 14). For a survey of the development of orbital elements for Ceres, see Appendix C in this book.

The Spirit of the Method

Gauss differed from his contemporaries by avoiding any arbitrary assumption for the initial orbit; his ellipse was based on the available observations only. This was in contrast to Olbers, for example, who assumed a circular orbit. The initial assumption of a circular orbit for the planet Uranus in 1781 proved to be a good choice, but Ceres' eccentricity made this hypothesis quite inefficient.

Gauss' work offers a rare instance of solving an historically great problem in applied mathematics using only the most modest mathematical tools. It is a complicated problem, involving over 80 variables in three different coordinate systems, yet the tools that Gauss uses are largely high school algebra and trigonometry! (Teets & Whitehead, 1999, p. 83).

As explained by Bühler (1981), Gauss used an analytic expansion of the elements of a perturbation. Only the first elements of the resulting infinite series were taken into account and used. This was not an original idea with Gauss, the method having been used by Pierre-Simon Laplace (1749–1827), but Gauss was more efficient because of his familiarity with a great number of infinite series and his skills in manipulating them. The analytic method was satisfactory for Ceres, but even then Gauss expanded the perturbations into trigonometric series that he integrated numerically with the help of tables.

Gauss' procedure was based on using only three observations, selected from Piazzi's data. His original choices were January 2, January 22 and February 11. In a second round of calculations, he used the observations of January 1, January 21 and February 11.

Gauss' first goal, and the most challenging one, was to determine the distance of Ceres from Earth for at least one of the observations. He chose the second of the unknown distances—the one corresponding to the intermediate of the three selected observations—as the prime target of his efforts. Finding that distance "broke the back" of the problem.

In fact, Gauss used his calculation of that value to determine the distances for the first and third observations; from that he determined the corresponding spatial positions of Ceres, and from the two spatial conditions and the corresponding time, he calculated a first approximation of the orbital elements. Using the coherence provided by that approximate orbital calculation, he revised the initial calculation of the distances to obtain a second, more precise orbit. The iteration continued until all the values in the calculation became coherent with each other and the three selected observations (Tennenbaum and Director, 1998). Gauss himself, in his *Theoria Motus* of 1809, emphasized the importance of having several good observations:

If the astronomical observations and other quantities, on which the computation of orbits is based, were absolutely correct, the elements also, whether deduced from three or four observations, would be strictly accurate (so far indeed as the motion is supposed to take place exactly according to the laws of Kepler), and, therefore, if other observations were used, they might be confirmed, but not corrected. But since all our measurements and observations are nothing more than approximations to the truth, the same must be true of all calculations resting upon them, and the highest aim of all computations made concerning concrete phenomena must be to approximate, as nearly as practicable, to the truth. But this can be accomplished in no other way than by a suitable combination of more observations than the number absolutely requisite for the determination of the unknown quantities. This problem can only be properly undertaken when an approximate knowledge of the orbit has been already attained, which is afterwards to be corrected so as to satisfy all the observations in the most accurate manner possible.

Needless to say, Gauss' contemporaries were very keen to know how he had managed to predict the position of Ceres with such apparent ease and accuracy. Most of them would have to wait years for the answer, but on August 6, 1802, Gauss sent Olbers a brief manuscript entitled "Summary Survey of the Methods Applied in the Determination of the orbits of Both New Planets." Olbers responded to Gauss on October 10:

My dear friend, you have done me a great favour by your explanations and remarks concerning your method. My little doubts, objections, and worries have now been removed, and I think I have broken through to grasp the spirit of the method. Once again I must repeat, the more I become acquainted with the entire course of your analysis, the more I admire you. What great things we will have from you in the future, if only you take care of your health!

After 3 years, the manuscript was returned to Gauss in November 1805. Shortly after the appearance of Gauss' book *Theoria Motus,* the German astronomer Bernhard August von Lindenau (1780–1865) got the 1802 manuscript and had it published with Gauss' consent in the *Monthly Correspondence* of September 1809 (Dunnington, 1955). This is how Lindenau himself described the situation in a footnote to the paper (MC 1809, pp. 197–198) (Fig. 3.4):

When I had the pleasure of making the personal acquaintance of Herr Professor Gauss some time back, I saw among his papers the following essay, already outlined many years ago and yet nowhere published, which contained the earlier method of the author for determining the orbit. In my cursory reading of this summary overview I was soon convinced that the method developed here by the author, for making a first approximation of two distances of the planets from the earth, was essentially different from that which the author has now publicly expounded upon in his larger work. So I asked him for permission that I might make this treatise known, with the assumption that it would be interesting to all connoisseurs to know the way in which the author succeeded at arriving at a complete solution—which differed from that of which an overview had been communicated to our readers in earlier issues. I originally had the goal to accompany the essay with some remarks for the purpose of making a comparison of the earlier and later methods of the author; but these, had they actually been explained, would be somewhat extensive, and without reference to the work itself, remain ever unclear. It thus appeared advisable to me to communicate

Fig. 3.4 Bernhard von Lindenau

the entire essay, without further addenda (which is more intended for connoisseurs who have the work itself at hand [the Theoria Motus]*), to the astronomical readers of this periodical, just as it was set down by the author in writing six years ago.*

Ceres 200: An Essay by Brian Marsden

On the 200th anniversary of Ceres discovery, Dr. Marsden wrote this essay that originally appeared in Cunningham and Marsden (2001):

So how did Gauss actually make his impressively correct prediction? Why did he succeed when others failed? The second question is easier to answer than the first. There simply weren′t any very practical methods available for computing elliptical orbits—as Burckhardt's experience illustrates. Sure, Anders Lexell had succeeded in computing an elliptical orbit with eccentricity 0.79 for the comet observed during June to October 1770, but he spent several years making a very detailed and innovative study of that comet. This included his realization that a close approach to Jupiter in 1779 could subsequently put the comet on a hyperbolic track. There was thus never any need for a test of his work in terms of predicting the comet′s return. The available methods by Dionis du Séjour (popularized by Olbers) and Laplace were appropriate enough for computing the parabolic orbits that were sufficient for other comets (Halley's being a separate case because the 0.97 eccentricity was prescribed by the dates of the previous apparitions), and a circular orbit had sufficed to recover Uranus.

Right at the start, Gauss knew that a successful outcome for Ceres would require a reliable and QUICK method for yielding an elliptical orbit. Piazzi made

19 observations during the 6 weeks he had Ceres in view. During that time the object had moved within an area of sky no larger than 4° square. But by the end of November it would have traveled one-third of the way around the sky. Given that Gauss had already invented the least-squares adjustment procedure and made a clear application of it in 1799, some have been tempted to deduce that he applied it to the Ceres calculation. Although the idea probably crossed his mind, he would surely have appreciated the impossibility of carrying out, in any reasonable time, the necessary computations for the six orbital elements from the 38 equations he would first need to produce. This was in the pre-computer era! Gauss was as brilliant an arithmetician as he was a mathematician, but even he could not countenance that.

Gauss recognized instead that he could in principle derive the six orbital elements from just three of Piazzi's observations, each of which would provide two equations. It seemed appropriate both to equalize the time intervals and to utilize the maximum extent, so he decided upon the observations of January 2, January 22 and February 11 (which is in effect January 42). Of course, if one or more of them were seriously in error (say, by significantly more than 5″ or 10″), he would be severely up the creek. Partly to counteract this possibility, he made a second computation from the observations on January 1, January 21 and February 11. Of course, he would still be in trouble if the February 11observation were off, but he could judge the overall reliability by examining how well his orbits actually fitted all 19 observations, those immediately prior to February 11 having been made on February 8 and 5. After all, such an initial orbital solution and the calculation of the residual discordances of the observed positions and those consistent with that solution would be necessary if he were in fact to go about a least-squares "differential correction," as it is called.

Gauss described his method—actually, several methods—or three-observation orbit solutions, in his famous book *Theoria Motus Corporum Coelestium,* published in 1809 (see Wilson, 2005). By then, of course, he had the chance to perfect his techniques, which were obviously much more primitive in 1801. First and foremost, in both his 1809 book and his 1801 calculations, he made use of the fact that (if the perturbations by the planets are ignored) the orbit to be determined lies in a plane that passes through the Sun. Part of what had to be done was to establish the orientation of that initially unknown plane with respect to a plane and a standard direction. Such a plane might be the ecliptic and the direction that of the vernal equinox. This is a three-dimensional problem, and the position of the orbiting object at any time can therefore be expressed by three rectangular components.

As Gauss considered in his 1809 book the fact that the orbit is a plane means that each component at some *particular* time can be expressed as the same linear combination of the corresponding components at two other times. These times are taken to be the times of the three observations, usually with the times in sequence and the particular time between the other two times. It is not difficult to see that, at least if the time intervals are short in comparison to the object's orbital period (as the 20-day spans for Ceres are in terms of the 4.6-year revolution), the factors that describe the linear combination are approximately the ratios of the time intervals. This is what one would expect from a simple linear interpolation. For the January

2/22/42 case the factors are each approximately 0.5. For the 1/21/42 January case they become approximately 0.512 for the first time and 0.488 for the last, the sum being unity. Because of Kepler's first law, which states that the line from the Sun to the orbiting body sweeps out equal areas in equal times, we can speak of these factors as the ratios of the areas of the respective "sectors" of the orbit.

But the sector-area ratios are only an approximation to the required combination factors. These factors should rigorously be instead the ratios of the areas of the *triangles,* one of the vertices of which is always the Sun, the others being the pairs of points on the orbit as for the sector areas. Each of the three time intervals involved, as proportional to each respective sector area, therefore needs to be divided by the ratio of the area of the sector to the area of the corresponding triangle, if it is to be correctly used to compute the linear-combination factors. The need for these SECTOR-TRIANGLE RATIOS, which for normal orbit-determination cases are somewhat greater than unity (and of which the one relating the January 1 and February 11 points for Ceres has a value of about 1.0043), was already understood in the 1740s by Leonard Euler, who then devised an approximate way of computing them. This was improved by Euler and Lexell around 1770, and Gauss improved it further in his 1801 work on Ceres. However, it was still a few years more before Gauss devised a completely rigorous and, indeed, elegant way of computing the sector-triangle ratios. This he probably accomplished around Thisis discussed in the *Theoria Motus.*

Of course, the observations are being made from a point on the surface of Earth, rather than from the Sun, and the geometry of this means that the positional components relative to the Sun need to be replaced by the known corresponding component of the position of the observer relative to the Sun and the product of the observed directional component of the object in the sky and its unknown distance from the observer. The basic component equations utilizing the triangle-area ratios now therefore become equations involving linear combinations of the unknown distances at the times of the three observations. The three linear equations for these three unknown distances can be solved, provided that the triangle-area ratios are known. For the initial solution, it is necessary to use the time-interval ratios.

As soon as this initial solution for the distances is available, a first approximation to the components of the three points in the orbit relative to the Sun follows. This allows an initial calculation of the sector-triangle ratios and thence of the triangle ratios themselves, allowing an improved solution for the distances, and so on. When the whole process has converged, the final values for the three components of each of the first and third orbital positions relative to the Sun can be converted to the standard Keplerian orbital elements, noting in particular that conversion of the associated triangle area back to the sector area immediately allows the calculation of the semilatus rectum of the ellipse.

That is the standard Gauss method of orbit determination, described in the *Theoria Motus* and countless subsequent publications, in the past often with further approximations to simplify or streamline the calculations.

What did Gauss actually do with Ceres in 1801? In addition to utilizing what was still a non-rigorous form of the sector-triangle ratios, Gauss concentrated, not on the components of the orbital positions in the ecliptic-equinox system but on the components in the orbit planet-ascending node system. These quantities can be expressed in terms of the corresponding coordinates in the ecliptic-equinox system and the orbital inclination and the longitude of the ascending node directly. As in the standard method, the ecliptic-equinox coordinates can be expressed in terms of the single unknown distance from the observer.

One of the components in the orbit-plane-ascending node system is always zero. If values for the inclination and nodal longitude are then assumed, this component yields a value for the distance from the observer. Appropriate assumptions might come, for example, from the computation of a circular orbit. For Ceres, Gauss assumed something like 11° for the inclination and 81° for the nodal longitude. Substitution into the other two components then confirms the distance from the Sun—or radius vector—and yields the angle along the orbit from the ascending node—the argument of the latitude. This is done separately for each of the three observations. The area of the triangle involving the Sun and the first and last observations is then computed, converted to the area of the corresponding sector, thence to the latus rectum, after which the remaining elements are computed. These elements will now predict the first and last values of the radius vector and the argument of the latitude exactly, but there will be discrepancy in the values for the middle observation. Of course, this is because the inclination and nodal longitude were assumed.

A second computation is then performed using the same inclination but with the nodal longitude changed by a fraction of the degree. A third computation has the inclination changed by a fraction of a degree and the original nodal longitude. Again, there will be discrepancies in the initial and orbit-based values of the radius vector and the argument of the latitude for the middle observation. But the three sets of discrepancies will be different. The differences between the first and second sets show how the radius vector and argument of the latitude change with a change in the nodal longitude, and the differences between the first and third sets show how they change with a change in the inclination. In fact, these yield linearized forms of partial derivatives that permit the discordances in radius vector and argument of the latitude on the first pass to be written as a linear combination of the corrections to inclination and nodal longitude necessary to remove these discordances. The two equations are solved for the two corrections. Finally, a fourth computation is carried out, starting from the inclination and nodal longitude corrected in this way. This time, there should be little or no discordance in the radius vector and argument of the latitude for the middle observation [end of Dr. Marsden's essay].

An Interview with Giuseppe Piazzi

In 1808, the English traveler Charles Kelsall (1782–1857) arrived in Sicily to "collect materials for an original work." Finding that other writers had pre-empted his plan, he decided to publish his observations in the postscript to a book he had written about Cicero. Embedded in this postscript, and apparently unknown to any historian of astronomy since then, Kelsall published the only known interview with

Fig. 3.5 Palermo Observatory (Photo by Dr. C. Cunningham in 2013. Used with permission)

Giuseppe Piazzi. While brief, it contains not only Piazzi's proud reference to the discovery of Ceres Ferdinandea, but his views on the state of astronomy in France and England. Together with Kelsall's own views about Piazzi's character, this offers us—two centuries later—a unique insight into the life and personal views of the co-discoverer of the first asteroid (Fig. 3.5):

Voulez-vous connoitre le père Piazzi? Said the Prince of Belmonte one evening to the translator. The astronomer named an hour on the following day when he would be ready to receive him. He found him in his study, richly stocked with works of science, which was a small room contiguous to the observatory, over the viceregal palace. He was engaged in looking over some nautical tables, which had just been sent him from the astronomer royal in England. The translator was desirous of ascertaining his opinion relative to the state of astronomical science in Europe, as he was now closested with a man, who from the sublimity of his pursuits, would in all probability be superior to any paltry prejudices. The Sicilian astronomer was very lavish of his praises of the French mathematicians, "but I don't think," he said, "that any men of transcendant merit have appeared in France since the revolution; all those who have distinguished themselves in philosophical pursuits, such as Lalande, Lagrange, La Place, Messier, and Delambre, were formed under the old regime." Generally speaking, he had not a high idea of the state of the science in England; but he acknowledged the merit of Maskelyne, and said, that astronomy was more indebted to him, than any man living. The translator asked his opinion relative to astronomical treatises. He replied; "that of Lalande is a chaos, and contains a heterogeneous mixture of subjects; that of La Place is a prodigious effort of human ingenuity. His Mécanique Céleste *will remain one of the proudest monuments of science. But it presupposes a depth of mathematical skill attainable only by a few. I consider La Place the first geometer in the world. We have not as yet a clear and purely scientific work on astronomy; it is still a desideratum."*

> *Of observatories, he had not seen any so good as that at Oxford; next to which he preferred the observatory at Paris; and he expressed surprise that there was not a better at Cambridge, the principal residence of abstract science in England. "No nation," he observed, "comes near the English, in the manufacture of philosophical instruments; which are not only unrivalled for the fineness of the work, but also for the science displayed in the design." Laying his hand on an entire circle, the work of Ramsden, and which had occupied that instrument-maker for two years; "it was with this I ascertained that the Ceres Ferdinandea is a planet; three others have been discovered since, and from their minuteness, from the circumstance of each of their orbits being between Jupiter and Mars, and from there being little or no difference in their mean movements, I conjecture that they may be the fragments of a destroyed planet. I am the first who have paid attention to stars from the sixth to tenth magnitudes; before, they were neglected, or at best but cursorily surveyed." The translator said that probably the serenity of the sky in Sicily afforded him greater facility in examining stars of small magnitude, than astronomers in more northern countries. To which he was far from agreeing, alleging, that though the atmosphere was more free from clouds, the sky was generally obscured by a haze, very unfavourable for astronomical observations. Padre Piazzi unites to profundity of science, a suavity of manners and politeness not often seen in those who devote their time to philosophical pursuits; and he brought to the translator's recollection the Rev. Thomas Jones, late senior tutor of Trinity College, Cambridge, and whose memory is stored in the hearts of his numerous friends.* (Kelsall, 1812, pp. 321–323)

Kelsall undoubtedly told Piazzi that he had been educated at Eton and Trinity, Cambridge. Thomas Jones [1756–1807] was head tutor at Trinity for 20 years and was known as an outstanding teacher of mathematics. In his career, Kelsall argued for university reforms, such as a more modern focus in academic courses and college buildings that reflected architectural history. He held that the Greek Doric style was the best model for reformed modern architecture. Kelsall designed buildings in various styles, including models for university buildings, and argued for a wider university syllabus, to be followed by a world tour. In his 1820 book *Classical Excursion from Rome to Arpino,* Kelsall published designs for a monument to Cicero in the Amalthea at Arpino, having been amazed to discover that none existed there. He later renamed his Hampshire house the Villa Amalthea, setting up busts of poets and scholars in the garden.

Chapter 4
The Great Nomenclature Controversy of 1801

The name given to the object discovered on January 1, 1801 generated huge controversy in Europe, and the debate raged throughout 1801 and into 1802 (Cunningham, Marsden, & Orchiston, 2009) (Fig. 4.1).

On May 7, 1801, Giuseppe Piazzi wrote a letter to Barnaba Oriani, in which he stated his intention to name his discovery Cerere Ferdinandea, the Italian version of Ceres Ferdinandea. This was reiterated in his first monograph on the discovery, *Results of the Observations of the New Star Discovered the 1st of January 1801 at the Royal Observatory of Palermo*. (Its completion can be dated to August 25, 1801, since Piazzi mailed a copy on that date to Oriani.) Piazzi also made his choice known directly to Bode: "I embrace you heartily that you have first announced my new planet, to which I would like bestowed the name Ceres Ferdinandea." (Letter from Piazzi to Bode, August 1, 1801; quoted in Bode [1802a].) Piazzi chose Ceres as the patron goddess of Sicily in the ancient Roman pantheon, and Ferdinandea in honor of Piazzi's patron King Ferdinand of Naples and Sicily (Cunningham, 1991).

The debate began at once but was at first confined to a squabble between German astronomers. First off the mark was Bode. As he related in a paper written in September 1801, it was in May that he wrote to Zach: "I would like to suggest the name Juno (Hera, in Greek), as I already informed Baron von Zach in Gotha in May. We must remain with mythology for the sake of analogy and to avoid flattery, and because the planets found over Jupiter carry the name of his ancestors and those standing closer to the Sun the names of his spouse and children" (Bode 1801a).

Also in May, Zach wrote to his close friend Oriani in Milan about the machinations of Bode, who is likened to a farm animal by the haughty French astronomers:

> *Bode wrote me confidentially that he had already thought about a name for the new planet and that it should be Junon* [Junon is the French name for Juno]. *But since I have been talking about this planet for 16 years now and been hoping to find it working on my zodiacal catalogue*, *the Duke* [Ernst II] *has already jokingly baptised this new hidden planet Hera or γρα*, *which means Junon in Greek. Thus I did not mention anything of Bode's nice idea in my journal since he told me the secret*, *I only said that 16 years ago the Duke of Saxe-Gotha gave this planet between Mars and Jupiter the name Hera and that it absolutely and*

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Fig. 4.1 King Ferdinand of Naples and Sicily, the patron of Giuseppe Piazzi

> *necessarily must be Hera and not Juno. Here is the demonstration: 1. the new planet cannot be called Juno since this name is already consecrated to Venus. Pliny Hist. Nat. Lib. II chap. VI said: Below the Sun walks the great star some call Venus… others call it, however, Juno. L. Apuleius said at the beginning of de Mundo: Juno, which esteems to be the star of Venus, is ranked as the fifth. St. Augustine De Civitate Dei Lib. VIII c. 15 calls Venus Stellam Junonis. Hence it is against the rules to give this name to the new planet. 2. It must be Hera because Hera is the mother of Vulcan who resides in Sicily.* [it was believed Vulcan, the god of fire, had his smithy under the volcano Mt. Etna]. *This city of Hera is also named Hybla Minor, and it is of this which Cicero talks in ad Atticum II.2. and in Pausanias in Elis Lib. VI c.6, and which comes up in the Antonine Itinerary* [a register of stations and roads in the Roman Empire]; *this will conserve, perpetuate and bequeath at the same time the discovery made by a Sicilian astronomer in Sicily to posterity. 3. It must be the Greek name Hera and not the Latin Juno, because Herschel's planet also has a Greek name—Uranus, it should be Coelus in Latin, but it is very good, all the ancient planets will have Latin names, the modern Greek ones, this distinguishes them at a glance, so if a new planet beyond Uranus will be discovered, it needs a Greek name. And here is my poor Baudet (as La Lande called him writing to Gotha) fleeced of the honour to be the parent of the new planet, as well of the honour to have recognised the planet and to have said it was the one between Mars and Jupiter for it belongs to two fine Italians and not to a heavy German like Baudet.* [in French, baudet means donkey]. (Zach, 1801a; his underlinings)

The ink was scarcely dry on this letter before Zach (1801d, p. 56) became aware of other contenders for the nomenclature crown, as he wrote in the July issue of the *Monthly Correspondence*:

That a new planet would be conferred several new names was to be expected. In the Leipziger Allgem. Literar. Anzeiger no. 72, an unnamed source suggested the name Vulkan. He believed it would not be improper to give the god who forged the weapons of Achilles a place in the sky next to the god of war [Mars], *the husband of Venus next to her lover. Vulkan would also not be able to complain that the honour was paid to him too late and that such an inconspicuous planet had been given his name, since he himself, due to a small mistake on the foot, is not fleet of foot or otherwise of splendid form. Vulkan, as the son of Jupiter, belongs to the family and has, in this respect, a well-founded claim to the honour intended for him. Doctor and Professor* [Heinrich] *Reimarus in Hamburg is of the opinion that it should be called Cupido. Because it was once established that planets be named according to the gods of antiquity, he would therefore be (counting from Venus downward) the next from Mars, a lover of Venus. Others believe the name Cupido is fitting because the name is associated with the idea of blindness. The new planet appears only as a magnitude eight star and cannot be seen with the naked eye. But should the planet be confirmed, the question of a name will be decided by the majority, and perhaps even by chance. It is also possible that a general consensus will never come to be, as was the case with Uranus.*

Bode (1802a) added another name to the list: "A known chemist [Martin Klaproth] wants to christen the new planet Titan after his newly discovered and named metal [titanium], because he had given shortly after the discovery of Uranus the element discovered by him the name uranium."

On July 25 Oriani warned Piazzi of the naming situation in Germany: "I must tell you that the name Hera or Juno has been given universally by all of Germany, for which it will be very difficult now to rename it Ceres" (Oriani 1801a).

Piazzi was scathing in his response on August 25: "If the Germans think they have the right to name somebody else's discoveries they can keep calling the new star the way they want, for we will always call it Cerere. I will be very glad if you and your colleagues will do the same" (Piazzi, 1801b).

By the time Ceres had been recovered in December 1801i (by Zach) and January 1802 (by Olbers), Bode caved in to the pressure: "I accept with much pleasure the name Ceres Ferdinandea. You discovered it in Taurus, and it has been found again in Virgo, the Ceres of ancient times. These two constellations are the symbol of Agriculture. The chance is very singular" (Bode, 1802b).

The Controversy in France

Even before Ceres had been recovered, the French weighed in with their own views. We gain a unique insight into Joseph-Jérôme Lalande (who held the chair of astronomy in the Collège de France) and the search for Ceres through a diary that was kept by L. V. Brugnatelli of Pavia. In 1801 he set out from Italy for Paris with the physicist and electrical pioneer Alessandro Volta (1745–1827), who had been invited there by Napoleon (Fig. 4.2):

Oct. 5, 1801: We went to Lalande, the great Parisian astronomer, an old man of 70. We found him in very good health and mood. He was discontented with Piazzi's behaviour for not having informed him at once of the discovery of the new planet, and he denied its existence together with another of his pupils Burckhardt of Lipsia whom he praised as one of

Fig. 4.2 Joseph-Jérôme Lalande

the best astronomers. They said that Piazzi and Oriani were good theoreticians but not yet practitioners: that Piazzi had to announce his discovery to Paris, where they are continuously in activity in observing the sky, and to other similar observatories.

Oct. 7, 1801: I asked Méchain if he had seen Piazzi's new planet, he replied to me 'NO' and that it was unlikely that it could be observed now owing to the variableness of the sky, but that he intended to do it if, he said, it existed.

The new object and its name were the topic of a memoir that Lalande presented at the opening ceremony at the Collège de France on November 21, 1801, in the presence of the Interior Minister:

Lalande invited us to the opening of the French College. His [Lalande's] *memoir begins with the discovery of the new planet made by Piazzi about which he* [Lalande] *doesn't raise any doubt anymore. He said that this discovery had been made on the first of January. Lalande spoke about the name that was given to the new planet discovered by our Italian. Piazzi would call it 'Ferdinandum sidus', Bode and other astronomers named it Juno or Hera. For me* [said Lalande] *I always call it—Piazzi—and I think that most astronomers agree.* (Brugnatelli, 1953)

Early in 1802 Napoleon Bonaparte, who always took a keen interest in scientific matters, made his views known. Laplace, writing to Zach in 1802, states that he had mentioned the discovery of the new planet to Napoleon, "who, in the midst of his great occupation, took a lively interest in the progress of the sciences, and particularly of astronomy." Bonaparte thought Juno was a preferable name to Ceres, and Laplace says he held the same opinion, since it appears natural to place Juno near Jupiter. He adds that a Latin name was better than a Greek one, the German astronomers having already suggested Hera, the Greek name of Juno. Piazzi rejected the name of the "jealous and vindictive" Juno. Zach passed this information along to Oriani on February 25: "Senator [Pierre-Simon] Laplace writes me that Bonaparte would like the new planet to be called "Junon." Lalande wants to call it "Piazzi." As for me, I will continue to call it Ceres while begging Mr. Piazzi to dispense with "Ferdinandea," which is a bit long" (Zach, 1802y).

In his annual paper "History of Astronomy," Lalande (1802a) leads off the list of accomplishments of the year 1801 with Piazzi's discovery, including his opinion on a suitable name: "As he hopes that this star will be acknowledged to be a planet, he has given it the name of Ceres Ferdinandea, in honour of the king of Naples; and Bode wishes it to be called Juno: as for my part, I shall call it Piazzi, as I gave the name Herschel to the planet discovered in 1781. The pagan deities are no longer interesting; and adulation pleases only the person who is the object of it."

Lalande amplified his opinion on the subject in a letter to Zach, the contents of which were then passed on in this letter to Carl Gauss:

> *La Lande really wrote*: "*Soon we will have all satisfaction. And the name Juno is being used. The senator La Place uses it exclusively*." *Méchain plays the diplomat and is still manoeuvring. He neither writes Juno nor Ceres*, *but only* "*the new planet*"; *it is ridiculous to see how anxiously and world-wisely he tries to avoid the nomen proprium* [*proper name*]. *La Lande who is French*, *too*, *with all his heart but still a respectable and honest soul with his own head*, *is different as he writes*: "*To me*, *it will always be Piazzi and nothing else*, *if someone wants to steal his treasure*, *I do not want to be part of this injustice*." *That is great*! *But incompatible with the court and an affront to Bonaparte*, *who calls him* [Lalande] *his grandpa*. (Zach 1802h)

Within the next few weeks, Pierre Méchain had softened his stance. In a letter to William Herschel, Méchain first uses the phrase "planete de Piazzi," then the name Ceres a few lines further (Mechain 1802).

All of this was contained in private correspondence. When Zach went public with the controversy in the *Monthly Correspondence* he presented a stoic face, likening it to a religious schism:

> *La Lande*, *true to his principle wants to name it Piazzi—just as he insists to call Uranus George*'*s planet or Herschel. Some time ago he wrote regarding this matter*: "*I will never consent to rip off of this small planet the name of my student Piazzi and replace it by Ceres*, *who is nothing to me. The rural deities were something in former times but are nothing today. The names had a meaning once but none today*." *Senator La Place wrote in his latest letter*: "*Bonaparte*, *to whom I talked about the new planet some days ago*, *and who has despite all his other obligations a vivid interest in science and especially astronomy and its progress*, *prefers the name Juno to Ceres*, *and I agree with him. It is only natural to place Juno close to Jupiter*. *The German astronomers were the first to give it the name of this Greek goddess*, *but it certainly is better to give it a Latin name*." *Well*, *again a schism in the church of astronomy*, *just as with Uranus*. (MC, March 1802, p. 280)

Piazzi was determined to have his way, and wrote in very strong terms to Zach in late April 1802:

> *I*'*ve noted in one of your memoirs in your journal the desire of a few to give this new planet the name Juno instead of Ceres*. *I trust that these astronomers*, *who are peaceful people*, *will never consent to having their deities called the name of a goddess as anxious*, *jealous and vindictive as Juno*. *Jupiter finally chased her from the sky as he had threatened a number of times*; *in her place he had Ceres appear*, *who has so much more right to the homage of mankind*, *and whom he hid very close to himself*, *loving her passionately… These questions should always be treated light-heartedly*. (MC, June 1802, p. 590)

Fig. 4.3 David Brewster

The November 1801 paper in the *Monthly Magazine* concludes with a section taken from the *Monthly Correspondence* about what name should be given to the new object. Some suggest the name Vulcan, while others opt for Cupid. Brewster (1802) here seamlessly inserts his own views without giving readers notice that the words are his, not an extract from the *Monthly Correspondence* (Fig. 4.3):

> *Notwithstanding the arguments, however, which have been brought forward to vindicate the propriety of each of these appellations, it is extremely probable that it will obtain the name PIAZZI; and it is surely much more proper, and congenial to the human mind, that the names of men of genius should be connected with their own discoveries, than that the titles of imaginary deities should be attached to the most stupendous works of the Creator. In the first case, some important advantages are obtained.—When we hear, for instance, of the planets Herschel, or Georgium Sidus, of Piazzi, or Ferdinandea Sidus, we are both acquainted with the astronomers by whose assiduity these planets were discovered, and with the name of the monarch in whose reign and territories this discovery was made. But, in the other case, no information is gained;—a name is merely given without any meaning whatsoever. If the planet distinguished by the name of Jupiter had been denominated Galileo, because this philosopher discovered its satellites; and if Saturn had been called Cassini, because the greatest number of its moons were discovered by this astronomer, how much more consistent would have been the appellation?*
>
> *Philosophers, indeed, have in all ages shewn an inclination for this method of nomenclature. The names of ingenious men have been used for distinguishing the spots upon the surface of the moon, even though they did not discover them. The Boylean Vacuum, the Toricellian Vacuum, Galvanism, and many other instances, shew that a nomenclature of this kind has not grown obsolete, even in later ages. Nay, if the planets and comets which*

may hereafter be discovered, should always be named from their discoverers, the most beneficial effects might be produced. The latent springs of unknown genius might be roused into action, and the indolence of philosophy might be stimulated to research, when the most illustrious of all honours was held forth as the reward of their labours.

P.S. Since the above remarks were written, I have seen a letter from a Member of the Royal Society of London, which mentions, that a paper, containing some particulars concerning the new planet was read at a meeting of the Society, on Thursday, December 10th, and that its magnitude is 1⅓ *the magnitude of the earth being unity.*

By suggesting that the first asteroid be called Piazzi, Brewster was of the same mind as Lalande in Paris and the American poet and diplomat Joel Barlow (1809, pp. 192–193). But what neither of them thought of was what should be done if two planetary objects were discovered by the same person. This very event happened, as Olbers discovered both Pallas and Vesta. Little more is heard of the idea of giving the name of the discoverer to his discovery after this, although Juno was called Harding (discussed in a later volume in this series).

The Affix Ferdinandea

By the middle of 1802 the name Ceres had been adopted by everyone except Laplace and Lalande. But what of Ferdinandea? (Foderà and Chinnici, 2001) Piazzi had added this name to honor his patron, Ferdinand, who was King of Sicily as Ferdinand III and King of Naples as Ferdinand IV (Ferdinand I, King of the Two Sicilies from 1816 to 1825). Piazzi was strident in his claims, the raw emotion that the controversy had generated within him literally overflowing the page:

Being the first in the discovery of this new planet, I thought to have the full right to name it in the most convenient way to me, like something I own. Thankful to my master, thankful to the Sicilian nation, willing to maintain a certain coherence with the other planetary names, it looked right to me to name it Ceres Ferdinandea. I will always use the name Ceres Ferdinandea, nor by giving it another name will I suffer to be reproached for ingratitude towards Sicily and its King, who with so much zeal, protects the sciences and arts, and without whose favour, perhaps we may never have arrived at this discovery. It is not adulation, but tribute, right and fair homage. (Piazzi 1802a)

Piazzi's letter to Oriani of Dec. 24, 1802, reveals his endearing devotion to the king (see Chap. 11). The double-barreled name found few friends, as we learn in a letter from Olbers to Zach on August 18, 1801: "I like the name Ceres since it reminds one of Sicily. Piazzi has certainly earned the right to name the new planet. But the affix Ferdinandea will meet with as little luck as Herschel's George's planet" [named for King George III of England] (Olbers, 1801).

Olbers was correct, but it met with at least a polite reception from the British Astronomer Royal, Nevil Maskelyne, in a letter to Piazzi on March 11, 1802: "You had the right to name the planet, which you discovered, and you paid due homage to your King, patron of the Arts and Sciences and founder of your observatory. I will call, and it will be called in England, Ceres Ferdinandea" (Maskelyne, 1802b).

Despite his lofty proclamation, the affix Ferdinandea was never used in England by Herschel in his published papers on Ceres. Zach used "Ceres Ferdinandea" in private correspondence and in his journal:

> *Since Piazzi has baptised his own child and named it Ceres Ferdinandea, which is entirely within his right as the discoverer, and since all of his correspondents have been asked to use this designation, we on our part also subscribe to this fitting designation with genuine and therefore greater pleasure, because the King of Naples, being an eager protector and patron of astronomy, as well as the magnanimous founder of a new, splendid observatory, indisputably deserves our gratitude, since he not only started to build an observatory, but completed it; not only bought the most valuable and splendid English instruments and instead of keeping them in boxes and crates in junk rooms, put them where they belong, and entrusted these splendid instruments not to unskilled and lazy hands, but rather to a scholar of recognised merit and skilfulness, and placed him in a position to promote his work and observations to print at the expense of the king. Since then, in such a short time, the most helpful and brilliant fruits have come from the Palermo Observatory, the learned world has been given several volumes of the most valuable observations, and this temple of Sicilian Urania has been immortalised, with its founder and priest, for millennia through the remarkable discovery with the coming new century. Piazzi therefore says in his discourse, and rightly so, that Ferdinand IV has more of a right a place in the heavens than some other protectors of astronomy.* (MC, 1801, pp. 577–578)

Piazzi could hardly have asked for a more ringing endorsement, but this resolution did not last long, as the final appearance of Ferdinandea in the title of a paper of the MC appeared in March 1803. In Russia, the mathematician Nikolaj Fuss (1755–1825) wrote several short papers in Russian about Piazzi's discovery, but the affix Ferdinandea was never used (Fuss 1802a, 1802b). However, the name Piazzi continued to be used for a while, and even appears on a map of the Solar System (circa 1803) and a French-made orrery (circa 1809).

The English poet Samuel Taylor Coleridge (1772–1834) made it clear he had no use for the concept of naming planetary bodies after royalty, making particular mention of Ferdinandea as the second part of the name given to Ceres by Piazzi. Here he invokes the name of Sostratos, who designed the lighthouse of Alexandria in the third century BCE: "'Sostratos of Gnydos, son of Dexiteles, to the Gods, Protectors of Sailors.'"—So it will be with the Georgium Sidus, the Ferdinandea, &c. &c.—Flattery's Plaster of Paris will crumble away, and under it we shall read the names of Herschel, Piazzi, and their compeers" (Jackson and Jackson, 1995, p. 321).

The controversy was fodder for the British press, too. There was, for example, an article in the Chester Courant newspaper in Chester on December 29, 1801. It read that while some "want to call it Juno, in analogy with the names of the other planets, M. de Piazzi wishes it to be called Ceres Fernandia (sic)." It was dryly noted in the *The Annual Review of History and Literature* for 1804 (1805) that "… the King of Naples has added sixty pounds a year to Mr. Piazzi's salary, for the discovery of the new planet, and honouring it with the royal name. So small a reward assuredly justifies astronomers in refusing to accede to the new title, and in immortalising the discoverer rather than the monarch."

The name Ferdinandea was perhaps last formally used on the occasion of the centenary of the discovery of Ceres, in a paper by Prof. Filippo Angelitti (1901), director of Palermo Observatory.

There was an ironic twist in the nomenclature saga. The French general Joachim Murat (1767–1815), who was king of Naples after the overthrow of Ferdinand, had one frigate—it was named Cerere.

Verses Commemorating the Discovery of Ceres

As a preface to understanding the inclusion here of a section on poetry, we quote from Lawrence Lipking (Professor Humanities Emeritus at Northwestern University): “In 1600 and long afterward, natural history belonged largely to poets, whose business requires attention not only to the human microcosm and cosmic macrocosm but also to everyday creatures and things.” (Lipking, 2014, p. 79) As time went on, poetry became entwined with historical inquiry itself. “Most historians, throughout the mid-eighteenth century, perceived themselves as the philosophers and poets of their day, revealing truths of universal value.” (Leffler and Brent, 1990, p. 37). Poetry dealing with Ceres and Pallas together will be in a later volume of this series, and those dealing with all four asteroids will be considered in a later volume.

In his 1773 didactic poem “Il Sistema dei Cieli” (System of the Skies), Carlo Gastone della Torre Rezzonico of Como (1742–1796) wrote about a little unknown planet between Mars and Jupiter (quoted in Sicoli, 2000). He was quite correct in surmising that it was both the smallness of its disk, and its low albedo, that had prevented it from being seen. The largest asteroid Ceres has a diameter of 960 km and a geometric albedo of 0.09. This compares to the smallest planet Mercury, with a diameter of 4878 km and an albedo of 0.138, lowest among the major planets (Fig. 4.4).

Fig. 4.4 Carlo Gastone della Torre Rezzonico

Sola poi vien la rubiconda stella
Del Fero Marte e dopo lui l'immenso
Giove, che tanto gli è lontan quant'esso
Dal Sol due volte. In così vasto campo
Forse alcun'altra dell' erranti stelle
Ruota da noi non conosciuta, e forse
Suo picciol disco, o per gran macchia oscuro
Fe sì, che invan della ritrosa in cerca
Al notturno favor di doppia lente
Vagò pel ciel l'astronoma pupilla…

Alone then arrives the reddish star of the proud Mars,
and after it the huge Jupiter, that is twice as far from it as
it is from the Sun. In so vast space maybe some
other of the wandering stars revolve unknown to us and
maybe because of its little disk, or because of darkness stain,
made so that in vain was a search of the bashful made
with the favored nocturnal double lens
as the astronomer's pupil roamed the sky....

King Ferdinand's Tribute

Since the newly found object was partially named in his honor, King Ferdinand felt obliged to commemorate the event. He first proposed to strike a gold medal, but was dissuaded in this intention by Piazzi, who asked that the funds instead be used for astronomical instruments.

As already stated "the King of Naples has added sixty pounds a year to Mr. Piazzi's salary, for the discovery of the new planet, and honoring it with the royal name. So small a reward assuredly justifies astronomers in refusing to accede to the new title, and in immortalizing the discoverer rather than the monarch."

Since the commemoration of the discovery was thus somewhat subdued in Sicily, it was left to those with the power of verse to mark the event for the ages. One who took the opportunity to do so was the Italian poetic improviser Pietro Scotes from Verona, who was quite the sensation just after the turn of the century in Weimar and elsewhere in Germany.

The various themes he [Scotes] set for himself to render in various poetic meter, in ottava rime, etc., included the advantages of blondes over brunettes, Achilles' lament for Patroclus, Nina's lament for her beloved, the advantages of music over painting, and of hope over fulfillment. One of his most beautiful poems was dedicated to the discovery of Ceres Ferdinandea, whereby he took every opportunity to extol the merits of his fatherland.

Unfortunately the text of his poem on Ceres has not survived. "All these things were extemporized at (often exclusive) social gatherings (what professional musicians today call "one nighters"), with individual poems often prompted by a topic, line, meter, or even end rhyme supplied by the audience or guest of honor—but the poems themselves were to my knowledge neither written down nor published. The reviews (there are two) address his performance rather than the text of the extemporized poems." Perticari (1779–1822; 1802) devoted a whole 98-page book of poetry to the subject of Ceres. This survey covers somewhat shorter verse.

Verses in the Monatliche Correspondenz

As editor of the world's only astronomical journal, the *Monthly Correspondence*'*s* Baron Franz von Zach was in a unique position to publish whatever he saw fit. Not content with printing positional measurements of Ceres, he often inserted personal comments and quoted directly from the letters he received. "One of my friends expresses the order of the now eight planets in the following not unsuccessful verses, which, according to the custom of usual memorial verses, expresses a further thought." (MC, July 1801, p. 67) Here the name Hera (spouse of Zeus in the Greek pantheon) is used instead of Ceres. Anticipating the discovery of a new planet, it was the name selected by Zach's patron, Duke Ernst II of Saxe-Gotha, 16 years earlier. Here are verses in the *Monatliche Correspondenz*:

Mercurius primus; *Venus altera*; *Terra deinde*;
Mars posthac; *quintam sedem sibi vindicate Hera.*
Juppiter hanc ultra est. Sequitur Saturnus; *at illum*
Uranus egreditur, *non ausim dicere summus.*
Oder:
Mercurius Solem comitatur proximus. Illum
Insequitur Venus, *hano Tellus*, *Luna comitante*;
Mars posthac, *Martem prohibit Jovis esse sequacem*
Hera lateens srustra, *et melioribus obvia vitris.*
Saturnum extrema Proavi statione locabant,
Nos aliter. Supremara coeli nunc Uranus arcem
Usurpat, *poenas ausi fortasse daturus.*
Mercury first, Venus second, then the Earth;
Mars after Earth; Hera lays claim to the 5th place.
Jupiter is beyond that one. Saturn comes next; but
Uranus (I should scarcely dare to say the last) makes his way beyond Saturn.
Mercury is the closest companion of the Sun.
Venus follows Mercury, and Venus is followed by Earth, with its companion the Moon;
Mars comes after Earth; Jove forbids Mars to be a close follower.
Next is Hera hiding in vain and exposed by better lenses.
Earlier generations situated Saturn in the outermost place,
But not we. Uranus now lays claim to the farthest arc of heaven,
Destined, perhaps, to be punished for his daring deed.

This is the only one of the verses dedicated to the discovery of Ceres that mentions—albeit rather obliquely—the technology that made it possible (i.e., "better lenses"). Likewise, the Ramsden Circle used by Piazzi to discover Ceres did not feature in any paintings or engravings done to commemorate the event, although it was depicted in relation to Piazzi's star catalogue (Cunningham, Marsden, & Orchiston, 2011b).

Capel Lofft

Lofft gave public vent to his long-held fascination with astronomy in several sonnets. With his lengthy poem "Eudosia," he encompassed most of what was known in astronomy on the eve of the discovery of Uranus. On August 29, 1801, he penned a sonnet about the newly discovered planet (Lofft, 1814):

To Miss Sarah Watson Finch.
With a Sketch of THE SOLAR SYSTEM, according to the latest Discoveries.
On the supposition of a new-discover'd Planet..
To thee whom as MINERVA I revere,*
To whom may cares and happier thoughts all tend,
This sketch of every planetary Sphere
Known to obey our central Sun I send.
In these the eccentric orbs have ear
To Harmony divine! The wild career
Of Comets thus revolves: prompt to descend
To that great source which rules their mighty year.
O might my Griefs and my charm'd Passions hear
Like influence divine! Thus should I know
Like thee to teach my moments how to flow
Useful and calm; unrackt by Doubt and Fear,
And thus ascend above all earthly Woe;
That Order, Heaven's bright Grace, anticipating here.
C.L. 1801.

**It was hop'd the New Planet, if ascertain'd to be such, would be nam'd Minerva: in conformity to the other mythologic designations, and in honour of Science, and of the Arts of Peace* (Fig. 4.5).

Fig. 4.5 Capel Lofft

Nicolaus Lipari

In 1801 Piazzi sent a copy of his treatise about Ceres to Lofft, together with a Latin epigram by a Sicilian, Nicolaus Lipari. Here is how Lofft (1802) described it:

> *If these Observations will be acceptable for the Mirror they are much at your service, as also the subjoin'd Epigram on its discovery and name,* CERES FERDINANDEA, *which I think has not appear'd in Print in England. Piazzi has prefixt it to his Account.*

> *Alma Ceres, pertaesa hominum consortia, summas*
> *Ut Superum tetigit, non reditura, Domos,*
> *Septem inter Caeli volventia sidera, cursum*
> *Flectere, et immensas caepit inire vias;*
> *Mortales fugiens oculis! Post saecula tandem*
> *Longa, ubi conspectum non renuisse datum est,*
> *Ante alias SICULAE voluit nova fulgere Terrae*
> *Immemor haud Patriae, quae sibi culta, suae.*
>
> *Nicolaus Lipari.*
>
> *Ceres from Human intercourse had fled*
> *And viewless through the Heavens her orb had led*
> *Mid seven companion Planets fond to stray*
> *Latent, through the immense aerial way,*
> *When, after Ages, to our sight was given*
> *This last—discover'd Daughter of our Heaven.*
> *As chief Sicilia, while on earth, she blest,*
> *On SICILY her STAR first shone confest.*
> *C.L.*

Marcin Odlanicki Poczobut

The 73-year-old Polish astronomer Marcin Poczobut (1728–1810) was an assiduous observer of Ceres from Vilnius Observatory in Lithuania. His colleague at Cracow Observatory, Jan Sniadecki, kept Zach apprised of Poczobut's work. A letter from Sniadecki to Zach dated May 24, 1802, includes more than just positional data from Poczobut. "He loves to write Latin verses and sometimes quite good ones. You will find at the beginning of his observations two Latin verses about the distinctive character of Ceres." The verse was in the MC (July 1802, p. 63):

> *Quae segetum culmos docuisti falce secare*
> *Falx dentate sacrum sit tibi stemma Ceres.*
>
> *Thou hast taught her to cut the stalks*
> *Of standing corn with a sickle.*
> *The toothed sickle shall become for you*
> *The consecrated garland of Ceres.*

This verse refers to the use of the sickle (suggested by Zach) as the planetary symbol to denote Ceres.

Fig. 4.6 Michelangelo Monti

Michel Monti

The Piarist monk Michelangelo Monti (1751–1822) gave the reason for the naming of Ceres in Latin verse (MC, Jan. 1811, p. 7) (Fig. 4.6):

> *Telluris patraie ductura a Principe nomen*
> *Astra inter Siculis fulsit ab axe Ceres.*
>
> From the most important of the fatherland of the Earth
> the name will be derived
> Immortality shone from the eye of Ceres
> among the Sicilians.

Monti was a poet and an orator. A native of Genoa, he became Professor of Eloquence in the University of Palermo. Thus his line about Sicily being "the most important fatherland of the Earth" is a homage to his place of residence. He also penned a lengthy poem in Italian entitled "Sulla Cerere Ferdinandea." (Monti, 1839, pp. 265–302) There is a monument to him in the San Domenico church in Palermo, the city in which he died at age 71.

Pietro Contrucci

The Italian professor Pietro Contrucci penned these lines a decade after Piazzi's death:

> *Piazzi,*
> *threw himself where the Eternal placed more*
> *wonderful marvels, with the discovery of Ceres carried on the perfectioning*
> *of the great work,*

with a lot of toil and glory, initiated by
Galileo.
Hail to thee, veracious erudite that to the
persecutors
you replied with the virtue of the works.
(Contrucci, 1837, p. 289)

Gioacchino Ponta

The noted Genoese poet Gioacchino Ponta (1770–1844) penned a verse in 1821 for the birth in Palermo of Jacobo FitzJames Stuart, duke of Alba and Liria. Trinacria means "triangle" and refers to the shape of Sicily; Modica is a city in Sicily:

Dal pianeta a Trinacria
Propizio, e muto in cielo
Pria che di Piazzi il geno
Non gli squarciasse il velo,
Cerere al caro infant
Della sua fertile Modica
Offerse un biondeggiante
Manipolo di messe,
E unaltro Liria che il suo stemma intesse.

From the planet propitious
to Sicily, mute in heaven
until Piazzi's genius
lifted her veil,
Ceres offered the dear infant
a blonde handful of wheat
from his fertile Modica,
and another Liria for the family crest.

Giuseppe Saverio Poli

Poli (1746–1825), who was a noted physicist, biologist and natural historian, also invoked the name Trinacria in his "Ode to the Illustrious Piazzi." Here are the opening and closing lines that both mention Ceres (Poli, 1803, pp. 105–106). The poem invokes the great pastoral enterprise of Spain, the Mesta. The Spanish empire at one time controlled Sicily, but the Mesta ruined the land for other forms of agriculture. It is invoked here as Ceres was the goddess of agriculture. As the poem says, she was "immersed in grief." The negative tone at the start of the poem is balanced as, at the end, there are no more sorrows and praises are sung to Ceres:

Mesta giacea Trinacria
Sol grato adusto suolo,

Priva de' don di Cerere,
Immersa in forte duolo.

Lieta per te Trinacria
Non piu dogliosi accenti,
Ma inni canori a Cerere
Discioglie all' aure, e ai venti.

Sicily lay sadly
On the beloved [but] dry soil,
Wanting Ceres' gifts,
Immersed in deep grief.
[…]
[Now], happy because of you [the newborn], Sicily
Fills the air
no more with mournful accents,
but with sonorous hymns to Ceres.

The goddess Ceres has been praised in poetry for a long time. Here are two examples, four centuries apart.

WITH fair Ceres, Queen of Grain,
The reaped fields we roam, roam, roam: Each country
peasant, nymph, and swain,
Sing their harvest home, home, home; Whilst the Queen of
Plenty hallows Growing fields as well as fallows.
Echo, double all our lays,
Make the champians [fields] sound, sound, sound To the Queen of
Harvest praise,
That sows and reaps our ground, ground, ground.
Ceres, Queen of Plenty, hallows Growing fields as well
as fallows.
("Praise of Ceres" by the English dramatist Thomas Heywood, 1613)

Another minor planet was discovered In orbit
between Neptune and Pluto. Known only as 2000
EB173
It is second in size
Behind only the asteroid Ceres.
The definitions of major planets and minor planets
Are being challenged yet again.
The line between them
Is a weak and uncertain one
As they orbit together around the sun. Such distinctions are
made by us
In an attempt to categorize
The seemingly infinite confusion of the universe. To us it makes things easier
But to the planets themselves
It does not make any difference.
("Minor Planet" by the American poet Steve Lucky, b 1969; 2000)

How the 58th Element Got Its Name

The German chemist Martin Klaproth (1743–1817), a noted experimentalist who had already discovered zirconium, uranium, titanium and chromium, found another unusual rare-earth element in 1803. The same year, the famous Swedish chemist Jöns Berzelius (1779–1848) and the geologist Wilhelm Hisinger (1766–1852) also isolated it (Fig. 4.7).

This rare-earth element was named ceria, but who named it? Klaproth had named uranium in 1789, after the newly spotted planet Uranus, and he was named by Hogg (1947) as the person who named ceria. But recent research by Fontani, Costa, & Orna (2014) has revealed the true story.

Hisinger and Berzelius sent the results of their experiments to Adolph Ferdinand Gehlen (1775–1815), editor of the *Neues Allgemeines Journal der Chemie*. To support their claim, they printed a pamphlet limited to just 50 copies. Independently, Klaproth (1804) sent his results to the same journal; in this paper Klaproth suggested the name achroite, due to the yellow-brown color of the metal oxide. His article appeared in an issue preceding that of Hisinger and Berzelius, whose attribution of the name cerium to Piazzi's discovery is made clear (Hisinger and Berzelius, 1804, p. 403) (Fig. 4.8).

In a letter sent to Hisinger in May 1804, Gehlen credited him and Berzelius as the discoverers of the metal and gave them the honor of naming it. Klaproth accepted

Fig. 4.7 Martin Klaproth

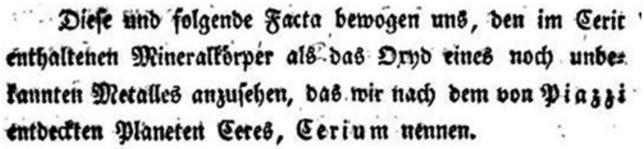

Diese und folgende Facta bewogen uns, den im Cerit enthaltenen Mineralkörper als das Oxyd eines noch unbekannten Metalles anzusehen, das wir nach dem von Piazzi entdeckten Planeten Ceres, Cerium nennen.

Fig. 4.8 An excerpt from the 1804 paper by Hisinger and Berzelius, linking Piazzi's discovery to cerium

Fig. 4.9 Jöns Berzelius

the decision, suggesting the name be modified to cererium, adding a syllable to emphasize that the etymology derived from the Roman goddess Ceres, not from the Greek κηρα, which means wax. Like his suggestion that Piazzi's discovery be named Titan (after the element titanium), this change was not accepted.

Berzelius became a chemist of world fame when he proposed the letter plan for symbols and formulas in 1813. He was also noted as the discoverer of the elements silicon and selenium (Fig. 4.9).

As originally isolated, cerium was in the form of its oxide, and was named ceria, a term that is still used. Isolation of the element in ceria had to wait until the late 1830s, when a pupil of Berzelius, Carl Mosander (1797–1858), managed to decompose the oxide using potassium vapor. By the usual convention, the element he obtained became known as cerium. (All the rare-earths had names ending in -ia, with the ending changed to -ium for the element.) Cerium is the most abundant of the rare-earth metals and is found in minerals including allanite, monazite, cerite, and bastanite. There are large deposits found in India, Brazil, and the United States (Ede, 2006, p. 88).

Chapter 5
The Physical Properties of Ceres

Doubts About Ceres: Planet or Comet?

Bode (1802a) very nearly accused Piazzi of duplicity as he described the difference between the January 24, 1801, discovery letter he received and the one Oriani received: "It is absolutely incomprehensible to me why Mr Piazzi in his letter of the same date to me calls his discovered moving star a comet, and even in some following letters insists on this opinion, disregarding my objections, and nevertheless in his first letter to Mr Oriani favours its planetary nature" (Fig. 5.1).

In a letter to Zach, Oriani (1801b) reveals what Piazzi was thinking in early April about the nature of Ceres: "He first announced this planet only as a comet, solely because he continuously observed it without a nebula and with a very slow movement. He therefore came upon the thought and the suspicion several times, that this could very well be a planet."

The following month Piazzi was still equivocating on this point with both Oriani and Lalande. Oriani relates that, in a letter of May 7, 1801, Piazzi "… told me of his bad health and replied to me about his doubt that the comet he discovered could be a planet." Oriani (1801c) had no compunction about sending this letter on to Zach, who duly published its substance in his journal.

On June 20 Piazzi expressed similar reservations to Lalande (1802a): "Many astronomers believe that it is a planet. I am still doubting it." On July 13, Zach (1801j) posed a series of uncomfortable questions to Lalande: "We start being suspicious of Piazzi's comet. Why is he acting so mysteriously? Why does he sometimes call it a planet and sometimes a comet?"

It is only in August that Piazzi (1801b), in a letter to Bode, accepts the reality of the situation. "Since I am convinced that my star is a real planet you can imagine how impatient I am to find it again." Despite this impatience, Piazzi was not sanguine about the prospects of ever seeing Ceres again. On December 8, 1801, just a day after Zach made the first sighting of Ceres since February 11, Piazzi wrote to Olbers that he was very doubtful it would ever be recovered.

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2_5

Fig. 5.1 A 1785 portrait of William Herschel by Lemuel Abbott (Courtesy of the National Portrait Gallery, London)

Since he expressed his doubts to all three of his principal correspondents (Oriani, Bode and Lalande) over a period of several months, it seems Piazzi was genuinely uncertain about what he had discovered. Indeed, in his second monograph on Ceres, Piazzi (1802a) still equivocated: "This planet is certainly very singular, and it wouldn't be so strange if someone can't think of it as a comet, which once entered our Solar System, remained linked and kept by the action of other planets."

Zach muddied the waters further by his speculation that a planet can exhibit a tail. This actually proved prescient, as just such a phenomena was finally observed in 2013 when the Hubble Space Telescope imaged six tails emanating from a body in the inner Asteroid Belt designated P/2013 P5 (Jewitt, Agarwal, Weaver, Mutchler, & Larson, 2013), showing that the distinction between comets and asteroids is not rigid but rather covers a spectrum. Here is Zach (1801a):

> *But why can a planet not have a tail as Saturn and Uranus have rings? Do the elements of the comet of 1770 not resemble those of Piazzi's star? I answer this with Lexell's* [Anders Johann Lexell, 1740–1784] *words that Jupiter's influence has totally changed the orbit of this comet. Why was it not seen any earlier and more often? Schröter and Herschel have proven that comets can be visible in a certain period and not in another.*

Lalande (1802a) was also very skeptical about the true nature of Ceres, as he related in this paper about its discovery, read in Paris on April 5, 1802:

> *The first day of the XIX century was marked by the discovery of a new planet. We owe this discovery to mere coincidence like that of Herschel in 1781; but the coincidence could only favour someone skilful and assiduous: This is what Plutarch calls fortunate work.*

In the evening of January 1, 1801, Mr. Piazzi, astronomer of Palermo, who is working on a star catalogue, wanted to observe the 87th star of Lacaille's zodiacal catalogue between the tail of Aries and Taurus; he saw very close an 8th magnitude star which he observed as well. His custom is to make the same observation on two following days but on the second day he noticed a difference. He soon recognised the motion of this little star which he considered a comet.

Mr. Piazzi wanted to save the pleasure of calculating his comet for himself and yet assure his data. He sent Mr. Oriani on January 24 two observations—that of January 1 and that of the 23rd—adding that it was stationary on the 10th. Mr. Oriani, seeing that it did not have a nebulosity like other comets, that it was stationary and retrograde in a quite small period of time, calculated it in a circle like a planet. He had only two observations that could determine a circle only.

Mr. von Zach did the same thing in Gotha and sent me his elements; he first believed it was the comet of 1770. Mr. von Zach maintained this idea, because he had been doing calculations since 1781 regarding the relations of the intervals between planets and thus concluded the existence of a planet between Mars and Jupiter. He attached enough importance to the matter to put it into the hands of Mr. Bode. (Eph. of Berlin, 1789, p. 163)

Lambert had already mentioned in his Lettres Cosmologiques, published in 1761 *(p. 51, ed.* 1801*), a planet that could exist between Mars and Jupiter. Titius and Bode, in his Knowledge of the Starry Heavens (*1772*), which has seven editions, concluded from the progression of the distances of the planets that there might be one, and Bode mentioned it several times. Indeed, if the distance of Mercury is 4, that of the other planets always doubles: 3, 6, 12, 24, 48, 96, 192. And that is why Mr. Bode presumed the existence of a planet between Mars and Jupiter.*

Lexell, who calculated the comet of 1770, found its orbit to be five years and thus placed it between Mars and Jupiter. The skilled research of Mr. Burckhardt led him to the same result in his piece that won the prize of the Institute in 1799.

Clairaut [Alexis Clairaut, 1713–1765] *talked in his Book About the Comet of 1759 of the attraction of another still unknown planet: "All this seemed very vague to me and I could see nothing other than a comet. But upon reading the article about the discovery of a new comet in Palermo in the Journal de Paris, I wrote Mr. Piazzi on February 27 in order to ask for his observations."*

On April 10 he answered as follows: "I did not want to give my observations to anyone before having derived the elements; but it is you who is asking; you will find them enclosed." I received his letter on May 31. Burckhardt immediately calculated an elliptical orbit; it was the first we had. On June 30 Piazzi wrote: "Many astronomers believe that it is a planet. I am still doubting it."

On July 1, Mr. von Zach sent me an engraving of the path that the planet was supposed to take after its conjunction according to the elements calculated by Burckhardt.

He, occupied with more important and more difficult studies, did no longer think of the planet. And other astronomers calculated other elements: Piazzi himself gave his together with Burckhardt's. In an Italian memorandum with the title Risultati… he saw that Burckhardt's elements corresponded very well to the observations. It was no difficulty to name his new planet Ceres Ferdinandea, to honour the goddess of Sicily and the ruling sovereign. Other astronomers preferred the name Juno because of its proximity to Jupiter. I always wanted to call it Piazzi's Planet.

Finally, on August 25 he wrote: "I hope you are interested in this discovery made by one of your most respectful and most grateful students."

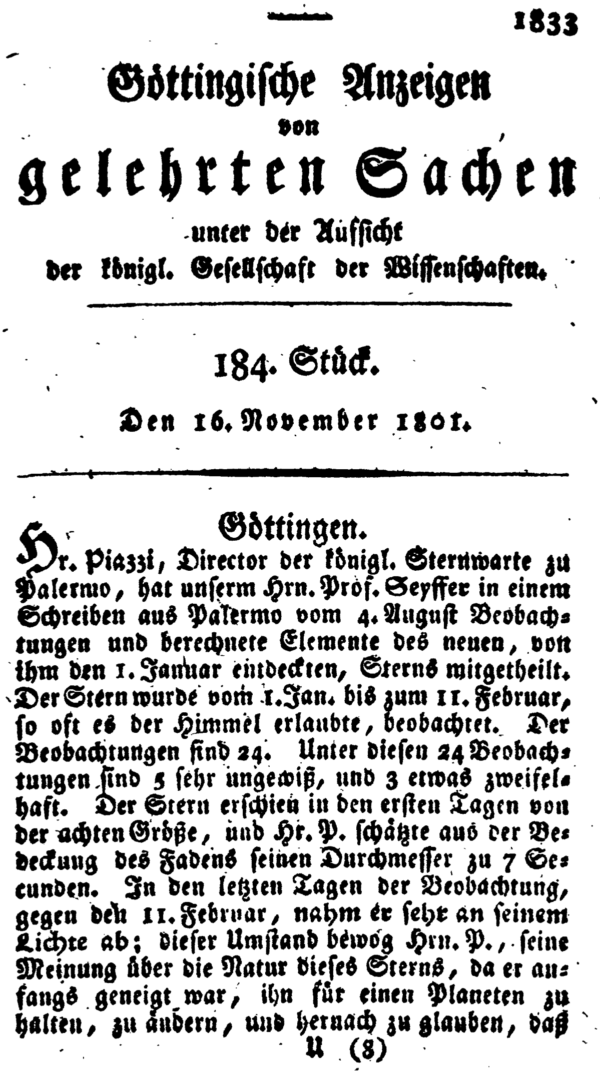
But though one could easily suppose a period and an elliptical orbit, we had to wait until it was discovered again after the return from the sun's rays. This was very difficult because of its small size and the uncertain motion.

> *In October Dr. Gauss of Brunswick was able to represent all of Piazzi's observations to 5″ and Mr. von Zach used these elements to calculate the planet's positions and he worked until he was the first to find it again.*
>
> *On November 26 he sent me new elements again together with an ephemeris of the planet until the end of the year. On December 6 he wrote that Schroeter, Bode, Olbers and he himself were searching it needlessly and he sent me Piazzi's observations better calculated.*
>
> *However, I continued to doubt the existence of the planet. The intervals of the observations were too short and a comet, disturbed by foreign attractions like that of 1770, seemed to me to describe the observed arc. I could not believe in such a small planet, which had not been noticed yet. But Dr. Olbers could, to whom we owe a wonderful treatise about comets, and who is specialised in this branch of astronomy and who worked tirelessly to solve the problems. The studies were very difficult because of the small size of the star and the uncertainties regarding the region where it had to be searched.*

Karl Seyffer's Treatise on Ceres

In late 1801 Karl Felix von Seyffer at the University of Göttingen wrote a treatise about the 'new star' discovered by Giuseppe Piazzi. After translating Piazzi's monograph for his German-language audience, Seyffer added his own analysis of the importance and meaning of this discovery. Piazzi himself praised Seyffer's work: "Dr. Seyffer has translated in German my memoirs on the new planet, to which he added a very wise appendix". The nature of Ceres was central to his thoughts—was it a comet or a planet? It is particularly valuable to examine his views at this crucial time, before William Herschel used the term 'asteroid.' Like Herschel, he was struggling to identify criteria that should be used to distinguish comets from planets. Most intriguingly, he used Herschel's own writings, published in 1795, on the subject of how to classify celestial objects. The main portion of the treatise is translated here (Fig. 5.2):

> *Mr. Piazzi was so nice to send me this book promised in his letter of August 4. I shared the contents of his letter already in the Göttingischen Gelehrten Anzeigen No. 184 of November 16, 1801* [pg. 1833–1835]. *I believe there is no better way to equal his fame and discovery than by publicising his Risultati in Germany word by word. It is a wonderful monument of his discovery and his spirit and is a sign of his tireless effort, his astuteness, his believing in the truth, and of the most humble carefulness, which is characteristic for Herschel, and of the decent character of this famous man.*
>
> *According to the already well-known ratio of the distances of the main planets from the Sun, which was examined in detail by Prof. Wurm in his On possible Planets and Comets of our Solar System in* 1786, *in Bode's Astron. Yearbook of 1789 and in his History of the new Planet Uranus,* 1791, *several astronomers have since long suspected a planet between Mars and Jupiter. Since to this analogy was added another element by Dr. Herschel's discovery of the Georgian Planet, whose observed distance matched that expected, the belief in this ratio received more importance. This analogy is expressed by the following formula:*



1833

Göttingische Anzeigen
von
gelehrten Sachen
unter der Aufsicht
der königl. Gesellschaft der Wissenschaften.

184. Stück.

Den 16. November 1801.

Göttingen.

Hr. Piazzi, Director der königl. Sternwarte zu Palermo, hat unserm Hrn. Prof. Seyffer in einem Schreiben aus Palermo vom 4. August Beobachtungen und berechnete Elemente des neuen, von ihm den 1. Januar entdeckten, Sterns mitgetheilt. Der Stern wurde vom 1. Jan. bis zum 11. Februar, so oft es der Himmel erlaubte, beobachtet. Der Beobachtungen sind 24. Unter diesen 24 Beobachtungen sind 5 sehr ungewiß, und 3 etwas zweifelhaft. Der Stern erschien in den ersten Tagen von der achten Größe, und Hr. P. schätzte aus der Bedeckung des Fadens seinen Durchmesser zu 7 Secunden. In den letzten Tagen der Beobachtung, gegen den 11. Februar, nahm er sehr an seinem Lichte ab; dieser Umstand bewog Hrn. P., seine Meinung über die Natur dieses Sterns, da er anfangs geneigt war, ihn für einen Planeten zu halten, zu ändern, und hernach zu glauben, daß

U (8)

Fig. 5.2 Karl Felix von Seyffer's report about Piazzi's 4 August 1801 letter on Ceres

a = *mean distance of the first planet*
b = *the difference of distance between the first and second*
n = *the nth planet from the Sun*
x = *mean distance of the nth planet from the Sun*

This formula gives the following mean distances:

Formula	*For*	*Observed*	*Difference*
0.387	*Mercury*	*0.387*	*0*
0.680	*Venus*	*0.723*	*0.043*
0.973	*Earth*	*1.000*	*+0.027*
1.559	*Mars*	*1.524*	*−0.035*
2.731	*Ceres Ferdinandea*	*2.574*	*0.157*
5.075	*Jupiter*	*5.201*	*+0.126*
9.763	*Saturn*	*9.540*	*−0.223*
19.139	*Georgian planet*	*19.184*	*+0.045*

Although this analogue law shows only little deviation from the observed distances the mathematician still needs proof. We do not know the exact number of planets in this row and from these seven elements, and especially not from the latest, anything can be concluded for certain. And Prof. Wurm adds, although the unexpected affirmation by the discovery of Uranus the following confession "that he had scarcely any choice but to think that the shown ratio might have some probability at least as an approximation to the true ratio", as an astronomer: "In astronomy as a science only experience is of value; this is the sole guideline and analogous conclusions should never mix with proofs." Keppler's five regular bodies, and Altobelli's [Ilario Altobelli, professor of mathematics in Verona, 1560–1637] *satellites of Saturn are warning examples. The beautiful law which Graf von Platen Hallermund* [Ernst Franz Reichgraf von Platen-Hallermund (1739–1818)] *discovered: that the outer satellites are in the same ratio as the distances from the Sun of their main planets; and astute expansion of this analogy by Prof. Wurm to the distances of the single satellites makes the probability of this ratio even more likely. Already Keppler noticed a dissonance between Mars and Jupiter in his Music of the Spheres. "If you compare the extreme intervals of different planets with one another, some harmonic light begins to shine. For the extreme diverging intervals of Saturn and Jupiter make slightly more than an octave. The diverging extremes of Jupiter and Mars embrace approximately the double octave." That means: the ratios are like that of the triple and double octave to the small third; or directed to the monochord as 1:8 and 1:4. Somewhere else he said: "The harmonies of the planets are so harmonious that they usually give a melodious sound, to such a degree that if a stringed instrument was tuned accordingly, our ears would hear a harmonious music without offending dissonance, except for the ratios between Jupiter and Mars for these would result in a dissonance." If one considers such speculations dreams one has to admit that Keppler's fantasies were at least heavenly fantasies and that we would, without Keppler's fantasies, still be dreaming about those wonderful laws of planetary motion. Fantasies of a great mind often outwit the ideas of others and open a new world in science. Mr. von Zach already engaged himself in these fantasies about a missing planet between Mars and Jupiter 16 years ago and determined by a fortunate divination several of its elements* (Berlin. Astron. Yearbook, 1789, p. 162), *distance from the Sun, period of revolution and eccentricity of which the first two correspond very well to that of Piazzi's star; we cannot decide on the third element yet, cf. MC June, p. 605. These elements calculated from analogy were: from the Sun 2.82, period of revolution 4.74 years or 4 years 9 months. From Piazzi's first observations of January 1 and 23 Mr. Bode found: distance 2.75 and period of revolution 4 years 9 months. This curious correspondence of the divination also occurs with the elements of Piazzi and Burckhardt. Prof. Bode mentioned in his earlier astronomical writings assumptions and estimated elements as well. Dr. Burckhardt was the first to calculate the observations of the new body in an ellipse just as Prof. Wurm was the first to determine the elliptical orbit of the Georgian Planet then. Dr. Burckhardt and Dr. Olbers found* *after several attempts and sensible methods (von Zach MC, July, October 1802) that the observations could not be described in a parabola. The circular elements of the latter differ only by 23 minutes in longitude of the ascending node, 19 days in period of revolution, and 12 minutes in inclination form Piazzi's. The error in longitude does not exceed +2.5 minutes and that of the latitude are negligible.*

Piazzi's request to call the star CERES FERDINANDEA if he or others succeed in finding it again, is righteous and all astronomers will comply out of honest respect for its merited discoverer and the donor of Palermo Observatory and gratitude to the noble and fine patron of astronomy. Freiherr von Zach has already complied (MC November). Regarding the symbol it appears appropriate to me to stick to the analogy of the other signs: Mercury's caduceus, Mars' shield and spear, Venus' mirror are matching, natural and ancient symbols (Lalande Astronomy, 591). *Consequently, I would choose for Ceres' regalia a torch, which she lit at Mount Etna to search for her kidnapped daughter* (Euripides Supplic., 260, Ovid Fast, IV 493) *or the head of a corn poppy, or an ear* (Horat. Carm. Sec. v. 30). *That this symbol must be in analogy with the others which are in the spirit of the beginnings of the art of drawing, regarding the degree of elaborateness and due to economy, is only natural. If future studies will result in the invisibility of this body one has to turn the torch over. Some efforts from Berlin, to run a planet factory by wind and to make the audience believe they had the monopoly of foreign correspondence do not appear to need any disproof. The ancients called Ceres, regarding the Eleusinian Mysteries and the punishment of the uninitiated, Arcanam Cererem. Keppler says of the tone of those efforts: "The Earth sings MI FA MI, so that even from the syllable you may guess that in this home of ours MIsery and FAmine hold sway."* [Like the earlier Kepler quote, this comes from Harmonices mundi, Harmony of the World, 1619.]

Mr. Piazzi's discovery became known in England only in August and was announced in a newspaper: This news was certainly not handed in by any Italian or French astronomer.

The question for the nature of this celestial body, whether it is a planet or a comet, cannot easily be answered. It seems to be remarkable that among those many voices which were gathered by Baron von Zach in his correspondence, since they belong to men whose name alone demands admiration, there was not one that expressed a difference of both kinds of moving stars. The term Planet, as it can be found in the most famous books on astronomy, does apparently no longer fit the expansion and unveiling of the building of the world as we know it now. After this tight terminology, although Herschel and La Place unlock new heavens due to their analysis and their telescopes, I dare prove that the zodiacal light is a planet, which revolves around the Sun. I consider it certain that this atmosphere which is reflected by the zodiacal light, is an atmosphere different from the solar atmosphere, cf. La Place's Mécanique céleste, vol. 2, p. 170. Probably our Sun is a planet, probably all nebulae are, and all stars together that we can see in a bright winter's night which, like our Sun does, revolve as planets around dark Suns; and their orbits will be calculated in several thousand years from now, just as we determine the elements of our seven planets! Through the field of Herschel's telescope with a diameter of 15 minutes, at some place in the Milky Way, at 41 minutes time, a compressed cluster of stars moves which consists of not less than 258,981 stars; and Dr. Herschel (1795) *says in his treatise On the nature and Construction of the Sun and fixed Stars, p. 26 with good reason: that these clusters of stars are huge lucid main planets: "They are in fact, only very capital, lucid, primary Planets, connected together in one great System of mutual Support." I do not know any other certain way to distinguish a planet from a comet than by the great eccentricity of the latter. But if this feature is constant and determined I ask each astronomer with what reliability and accuracy can the eccentricity of this celestial body be determined from 24 observations? How reliable can elements be, calculated from such a small visible arc? The answer, that was actually given, that this body was already calculated in an ellipse and that the eccentricity and period of revolution (extremely accurately for a comet) were determined would be something similar to an ellipse, a circle—in conclusion. I do ask, with what degree of reliability can these elements be derived from such an extremely small visible arc?*

The greatest or smallest inclination is nothing but an analogy of hitherto observations and cannot be a certain proof for planetism, for it is appropriate to call the moving stars of 1702, 1743, and 1759 comets and not planets. And it would be difficult, since the inclination of the orbit is most difficult to determine, to classify such stars whose inclination would come close to the assumed limit. And I do not consider any physical feature of moving stars like tail, nebulosity, lucidity or lack of these a distinctive characteristic for planets or comets. What physical character did the five comets, discovered by Mademoiselle Caroline Herschel (sister of William Herschel), have? Do we not owe this discovery a new opinion?!

The distinction between planet and comet should consequently be derived from something more consistent and more obvious, like the orbit of moving stars. Whether these orbits are returning or not returning is hard to determine as can be seen from the different results of the most diligent comet-calculators. Actually, we only know of the return of one single comet of 1531, 1607, 1632, 1759 of whose identity of the orbits is a fact. In order to constitute the character of moving stars one should pick something more certain and constant. The only characteristic which is left and which is convenient regarding the small number of moving stars, to distinguish planets from comets:

The distances of several stars from the Sun be at perihelion p, p′, p″, ...p(n)

The distances at aphelion a, a′, a″ ..... a(n)

I would call the star K (whose distances at perihelion and aphelion are p(k), a(k)) a comet if there is another star I whose aphelion a(i) < a(k) and perihelion is p(i) > p(k). The star K would be a planet if no other star I described such an orbit, that at the same time a(i) < a(k), p(i) > p(k). But whatever special or general terms are applied to Piazzi's star due to the uncertainty of the elements, its nature will always remain uncertain if we will not see it again. After all the above studies it appears to me that considering all observations nothing certain can be said about the nature of Ceres Ferdinandea. One or the other opinion seems to belong to those things of which [David] *Hume said about* [George] *Berkeley* [1685–1753]: *that they do not allow falsification but neither create conviction* (Seyffer, 1801).

For Seyffer's own observations of Ceres from March 29 to April 3, 1802, see Seyffer (1802).

The Atmosphere of Ceres

One of the characteristics of a comet is that it has a coma, or atmosphere. The most curious aspect of early asteroid research was the belief that Ceres, and the other asteroids, possessed atmospheres.

Why an Atmosphere Might Be Expected

The issues involved can be formalised as an application of probability theory to variative induction. In the following quote from the British philosopher John Stuart Mill (1806–1873), his words "animal or plant" have been replaced for the subject under discussion here by the word "planet":

If we discover, for example, an unknown planet, resembling closely some known one in the greater number of the properties we observe in it, but differing in some few, we may reasonably expect to find in the unobserved remainder of its properties a general agreement with those of the former, but also a difference corresponding proportionately to the amount of the observed diversity (Mill, 1843, p. 89).

A less rigorous formulation of this concept comes from the French author Bernard de Fontenelle (1657–1757; 1686, p. 115): “You must own that when two things are similar in all that I know of them, I may reasonably think them similar if what I am unacquainted with in respect to them.”

As Aït-Touati (2011, p. 87) has noted, this “… is the condition of a valid comparison, founded on bringing together similar objects, a formulation reminiscent of Descartes.”

Irradiation and Spurious Disks

This expectation to find properties in the “unobserved remainder” led Schroeter, Herschel and others to search for two properties in particular that are associated with the known planets—namely, satellites and an atmosphere. Schroeter, however, was misled by his interpretation of the work of the Swedish astronomer Daniel Melanderhjelm (1726–1810). His theory of planetary atmospheres was originally published in Swedish in 1798, but it came to the attention of Schroeter in its German version (Melanderhjelm, 1800). He specifically cited the work in his book on the asteroids: see section 66 in Chap. 9 of this volume. Thus, Olbers (1805, his underlining) wrote to Gauss (Fig. 5.3):

Fig. 5.3 Daniel Melanderhjelm

Schröter has, as he informs me, changed much in his work concerning the new planets based on ideas I had pointed out; I thus hope that you will no longer consider the calculation of the masses, densities, and gravitation at the surface of these small heavenly bodies. The determination of these details rests upon a totally erroneous application of an unprovable statement of Melanderhjelm. He had adopted the hypothesis that the planet's atmospheric density at the surface varies as the square of the gravitational force at the surface. Schröter believed he could conclude the reverse, that the atmospheric density at the surface varied as the height of the visible portion of the atmosphere. For our Earth he adopted, along with La Hire [Philippe de La Hire, 1640–1718], *a height of 38,000 Toisen. Since his telescopic observations gave him the heights of Pallas' and Ceres' atmospheres from 100 to 150 miles, he thus decided on a high atmospheric density at the surface of both planets, and this the same for the gravitational force and density. The result is, e.g., that the density of Ceres is 4½ to 5½ times that of gold, etc.—I pointed out to him (1) that Melanderhjelm's so-called theory merely entails the somewhat strangely expressed theory that the ratio of the mass of the atmosphere of every planet to its total mass is always the same, and thus with every planet it would be about 1/800000 of its mass; (2) that this hypothesis, in itself very improbable, is refuted precisely by his observations of such large atmospheres surrounding such small heavenly bodies; and (3) that the heights of the visible atmospheres could by no means vary just like their density at the surface, etc. Just between us, I can't at all believe that Ceres and Pallas have these large atmospheres. Rather, I assume them to be due to irradiation in the telescope.*

In fact, Olbers had written to Gauss about the irradiation matter 3 years before:

What kind of small planets are Pallas and Ceres? Herschel found an apparent diameter of Ceres, as Zach writes, of only 1″, and of Pallas, as Bode informs me from LaLande's letter, of only 1 1/2″. In this way, speaking confidentially, irradiation must have interfered with our friend Schröter's observations. I admit, I have always suspected this; for my very nice 5-foot Dollond, at 240-times magnification, does not even show an appreciable disc for either planet, nor is there a definite difference from a fixed star. (Olbers, 1802b, his underlining).

As Olbers rightly pointed out, the theory upon which Schroeter based his conclusions was faulty. He also rightly identified irradiation as the cause of these unsupportable atmospheres. The subject of spurious disks and irradiation was examined by Cooke (1896, p. 38):

Since the spurious disk is brightest at the centre, and really shades off into the dark ring, it is evident that its apparent linear extension will depend very intimately upon the brightness of the star in question, that the spurious disk formed when a bright star is viewed will appear larger than in the case of a dim one, although the maximum size can never amount to as much as the diameter of the first dark ring. To this must be added the effect of irradiation in the case of the brighter stars. As a matter of fact, it is notorious how much smaller the star–disks appear to be in the case of small (ie faint) stars than in the case of bright ones. In all objectives having their focal lengths equal to 15 times the aperture, then the linear diameter of the spurious disk may be said to average 0.0004 inches. With 6 inches aperture this corresponds to an angular diameter of 0.9 seconds, and in a 12-inch aperture to 0.45 seconds.

The observed 'nebulosity', which was interpreted as an atmosphere, was originally reported by Schroeter. He said that "Ceres' atmosphere is to some extent similar to atmospheres of comets," and it is interesting to note that it was still accepted as fact (by some astronomers, at least) as late as the mid-nineteenth century. It was believed, for example, by Stephen Alexander, Professor of Astronomy at Princeton (Alexander, 1849).

Piazzi himself was convinced, as he wrote in his book about Ceres (1802a):

But, what will we think about this nebulosity or atmosphere? Does it really exist? After what we have reported, I don't think there is any doubt. If we have to consider the noted variations related to this atmosphere, it will be convenient to suppose it to be continuous with very strong agitations and disorders, or made of irregular different stratas of different density, twirling round the planet at very high speed. In this second case it would be a very thin and compact ring rather than an atmosphere.

Herschel remained unconvinced. While Schroeter asserted that apparent brightness variations were due to asteroidal atmospheric changes, Herschel called the supposition "unlikely; and it appears to me would be better accounted for by supposing asteroids are irregular rather than a round figure." Olbers too thought Schroeter mistaken. In a letter to Gauss on May 8, 1802, he wrote that "speaking confidentially, irradiation must have interfered with our friend Schroeter's observations. I admit, I have always suspected this; for my very nice 5-foot Dollond, at 240 times magnification, does not even show an appreciable disk for either planet, nor is there a definite difference from a fixed star" (Olbers, 1802a).

The Scottish physicist Sir David Brewster (1781–1868; Principal of the Univ. of St. Andrews from 1838), writing in 1811, proposed a particularly imaginative theory to account for the supposed atmospheres around Ceres and Pallas.

It is a very singular circumstance, that while two of the fragments, Juno and Vesta, are entirely free from any nebulous appearance, the other two fragments, Ceres and Pallas, are surrounded with a nebulosity of a most remarkable size. Now, the Comet of 1770, if it is lost, must have been attracted by one of the planets whose orbit it crossed, and must have imparted to its nebulous mass; but none of the old planets have received any addition to their atmospheres; consequently, it is highly probable that the Comet has passed near Ceres and Pallas, and imparted to them those immense atmospheres which distinguish them from all the other planets.

Brewster's theory was dissected and discounted by British astronomer David Milne (1828). [Brewster wrote in the 1811 ed. of Ferguson (1809).]

In a footnote on page 194, the unknown author of the 1811 book *Essay on the System of the Earth* gives further support to the idea of the intercession of a comet in the history of Ceres, a supposition that was widely considered and even favored by many other authors in this era: "I think I once saw in some publication (but wholly forgot where,) a statement that Ceres had been discovered to have a remarkably large atmosphere. If this be correct, it might be supposed to favour the presumption of an accession of a comet to this planet, and therefore to raise the possibility that the great angle of inclination of the orbit arises from the same cause."

The presence of an atmosphere for Ceres was still accepted by some "atmosphere is reckoned at about 1086 km in height" (Dick, 1846). Certainly by the late nineteenth century the idea had been dismissed entirely: "The aureoles seen by Schroeter to surround Ceres and Pallas have been dissipated by optical improvements" (Clerke, 1885).

The idea never completely died though. Witness this passage from the pen of rocket pioneer Hermann Oberth (1957). "No atmosphere has been found on any of them. It is possible, however, that remains of air, water, and gas have been retained in hollows because if an asteroid is actually a large planet which was never completed, the biggest of them would consist of blocks falling on top of each other with hollows and cracks between them. These would light the space traveler artificially and would perhaps make life possible so that Jules Verne's *Journey to the Center of the Earth* (1864) might become a reality, though not on the Earth but on Ceres, Pallas or Vesta." In 2014, water vapor was detected escaping from two regions on Ceres by the aptly named Herschel Space Observatory (Küppers et al., 2014)

Asteroids and Meteorites

The idea that meteors are but atmospheric manifestations dated to the time of Aristotle. As late as the seventeenth century in England, even the aurora borealis was called a "meteor" (Cunningham, 2014). A cosmic origin for meteors was suggested by Edmond Halley (1714; 1656–1742) after he saw a fireball, but he later reverted to Aristotle's ideas about their origin (Halley, 1719), so the cosmic origin lay fallow for close to a century. Scientifically, the half century before the discovery of Ceres saw the rise of the science of geology. In 1794, Ernst Chladni (1756–1827) published the discovery by German naturalist Peter Pallas (1741–1811) of a large iron mass in Siberia that must have had a cosmic origin (Marvin, 1996; Gallant, 1999). (It is interesting to note that Chladni was an expert on acoustics; in 1790 he created a musical instrument, the euphone, and he was certainly familiar with the "music of the spheres". The euphone comprises a number of metal rods, which are resonated by glass rods.) Almost all scientists rejected Chladni's cosmic origin result: the first scientists who accepted it were Oriani and Zach (Knöfel & Rendtel, 1994). In 1802 the French physicist Jean-Baptiste Biot (1774–1862) finally proved that a shower of stones in Normandy was celestial in origin (Sears, 1975).

The theory that meteorites were of lunar origin began with the Italian philosopher Paolo Terzago (1664), and it was advanced by Olbers in 1795, without any knowledge of this earlier proposal. The possible link between meteorites and the lunar volcanoes reported by Herschel's observations through his telescope 'tube' was such a powerful one that it inspired a few lines in a poem about geology by John Selby Watson (1844, p. 89)

Many such rocky lumps,
Or small or great, are known by men to fall,
And many doubtless fall that ne'er are known.
Nor is it known of any whence they fall;
Whether, far heav'd from forth the fiery hills
That Herschel's tube shows flaming in the moon,
They lose th' attraction of their native orb,
And feel themselves resistless urg'd to earth.

Olbers (1803a, 1803b) was the first to mention the asteroids in the context of a discussion about meteorites, although he did not attribute the asteroids as their origin:

You know that in a lecture which I delivered in the Museum of Bremen in the year 1795, on the shower of stones, as it is called, which fell at Siena in Italy, I expressed the same idea

> *which I lately read in a letter of Laplace, in which he says: "It is not impossible that large masses, detached from some of the celestial bodies, and particularly the Moon, may have sometimes been projected to the Earth." (early in 1802 Laplace raised the issue at the National Institute in Paris)*
>
> *I must readily acknowledge, that when I wrote the before-mentioned essay,…I considered these stones to be of a* [lunar] *volcanic origin. It has lately been shown by the mineralogical description of* [Jacques Louis] *Count de Bournon, and the chemical analysis of Mr. Howard, that the stones found at Siena have a perfect resemblance to all the other stones which have been seen to fall from the heavens. My former explanation and conjectures fall therefore to the ground.*
>
> *It takes great forces to propel these heavy bodies, which is why only a few of these masses which are thrown off the Moon fall on the Earth. The Moon, therefore, must perceptibly decrease in size, as it would be necessary that it should throw out a great number of masses to make some of them reach the Earth. And would not an infinite number of such small fragments move around our earth as satellites? Would they not be visible through our best telescopes, as we know that fireballs sometimes are of very great size, and as the observations of Ceres and Pallas has shown that bodies of a very small diameter become visible to us when illuminated by the Sun; or are the shooting stars, which seem to have a cosmical origin, such small satellites of our earth?*

As Olbers mentioned, in 1800 Edward C. Howard in London (1774–1816) undertook a survey of the so-called native irons and of stones reported to have fallen from the sky. He had been asked by Joseph Banks, President of the Royal Society, to analyze stones from two witnessed falls (1794 in Siena and the 1795 fall in Wold Cottage, England) to see if they might be meteorites. Howard sought out additional examples and analyzed four 'fallen stones' and four masses of 'native irons'. Howard measured about 10 % nickel in the meteorites and several per cent of nickel in the four 'native irons' (Howard, 1802). The text of Howard's report was read at three successive meetings of the Royal Society, "where it is said to have been heard by an unusually large audience because the readings were interspersed with updated observations on the new asteroid, Ceres" (McCall, Bowden, & Howarth, 2006, p. 47). In 1803 Martin Heinrich Klaproth in Berlin reported similar results on fallen stones (Habashi, 1998). It was Klaproth who named a newly discovered element cerium, after the asteroid Ceres (see Chap. 4).

Palon Heinrich Boguslawski (1789–1851, Director of Wroclaw Observatory) published a work on meteorites, in which he said that these are minor cosmic bodies and not stones ejected by Moon volcanoes, as Olbers had suggested. Boguslawski also made a particular study of Ceres and Pallas, but it is to Johann Christian Daniel Wildt (1770–1844; 1805, p. 468) that we must attribute the earliest and most direct linkage of meteorites and the asteroids. Wildt, a Professor of Philosophy at Goettingen who also wrote a book about Saturn in 1795, unreservedly asserted that meteorites were: "…the ruins of some globe which has been destroyed, and which revolve round the sun till, sooner or later, they fall in with a planet. They, without doubt, belong to the group of Ceres, Pallas and Juno, and thus we see how it is that their appearance and composition bear such general resemblance to each other."

Olbers (1837, p. 61) took note of Wildt's idea just before mentioning his own hypothesis that the asteroids were fragments of an exploded planet. Although the scientific community in 1833, at the time of the great Leonid meteor shower, generally did not support theories involving weather and flammable gases, these ideas were popular with the public, and some were quite imaginative (Romig, 1966).

Many correspondents embraced the so-called terrestrial comet theory, in which the meteor storm was caused by solid bodies that revolve around Earth and, for some reason, are captured by the Earth's atmosphere and ignite. Opinions differed as to the origin of these revolving bodies. An article in the *Advocate of Science* (Wilmington, Delaware) linked the asteroids to meteorites: "Some have ascribed [the orbiting bodies] to the explosion of the ancient planet of which the four asteroids were composed. A vast quantity of small fragments… continues to revolve in the Solar System, and when they chance to enter the earth's atmosphere, take fire, owing to friction, and give rise to meteors" (quoted in Chambers, 1837, p. 232).

The great Leonid shower of 1833 prompted people to check the historical record, revealing a periodicity of 34 years. Olbers (1837) wrote, "Perhaps we shall have to wait until 1867 before seeing this magnificent spectacle return." Even though he did not live to see it, there were reports of 1000 meteors an hour in full moonlight in that year (Sanderson, 1998). The influential Prussian naturalist Alexander Baron von Humboldt (1849, p. 97) boldly called meteorites "the smallest of all asteroids." The link between asteroids, meteorites and comets in the context of Olbers' hypothesis is neatly summed up by the British mathematician-astronomer Charles Babbage (1791–1871; 1815) (Fig. 5.4):

> *The hypothesis is certainly very extraordinary and may perhaps be controverted, but it has been too fortunate in its result to incur the disapprobation of astronomers. The idea itself and the consequences which resulted from it are equally the property of Dr. Olbers. This skillful observer was able to explain the phenomena presented by the smallness of the new planets and their nearly equal distance from the Sun framed this hypothesis. That possibly these small bodies might be the fragments of a much more considerable planet which some extraordinary cause had burst in pieces and that these parts continued to circulate round the Sun at the same distance and with equal velocities. This theory does credit to the ingenuity of its author and is not opposed by an argument which has frequently overturned such speculations. It is not repugnant to the principles of mechanics. It is not impossible that such an occurrence should have taken place and if such had been the case it might have happened that several fragments would revolve in nearly an equal time and the orbits of all would cut each other in two points. If however any of these parts should pass within the sphere of attraction of any large body its orbit might be considerably altered. This has perhaps happened in the present case. It is not probable to suppose that the convulsion which thus destroyed a planet should have divided it into precisely the parts which have been discovered. It is more likely that an immense number of pieces of different magnitude should have been formed, the larger parts would revolve regularly in certain orbits but possessing a considerable mass they would only be disturbed by the action of the other planets and would perform their course subject to these irregularities. The smaller fragments would be much more considerably affected by the attractions of the larger, and as they passed within the reach of each new body their orbit would be altered. Thus it might happen that some of these small fragments coming within the sphere of attraction of the Earth may be precipitated on it and thus produce those meteoric stones which are frequently discovered. It is not impossible that at the original disruption one part of the planet might be projected nearly in a right line towards the Sun. This would revolve in a very eccentric ellipse and would consequently become a comet.*

Fig. 5.4 Charles Babbage

The Size of Ceres

Of course, for Ceres to have an atmosphere, it must be large. Schroeter made it quite large, while Herschel thought it only a tenth as big (Fuss, 1802a). The modern value is from Millis et al. (1987). Here are the figures in miles derived in the nineteenth century, as given by those astronomers who actually tried to calculate or measure the diameter of Ceres:

Observer	Diameter	Date
Herschel	162	1802
Schroeter	1569	1811
Galle	395	1839
Argelander	230	1855
Bruhns	226	1856
Knott	630	1866
Stone	196	1867
Muller	590	1893
Barnard	485	1895
Bauschinger	482	1900
Millis	579	1987

Even before he had announced his diameter results, Herschel was being shamelessly goaded on the subject by his friend the physician and naturalist Sir William

Watson (1744–1824), who launched into an extraordinary digression about the possible size of the inhabitants of Ceres and Pallas:

> *I am much pleased that you propose to send me your further thoughts on these curious bodies. I am quite impatient to know how small you make them. One may be king of one of these planetels, and have no mighty kingdom either. I suppose they must be inhabited by Liliputians, and I please myself with the idea of their little cities, houses and ships, tho' after all, as size is merely relative to other things, they will feel themselves as great as we do. The truth is that if the heavenly bodies are inhabited by such beings as we are, and have trees as we have, they cannot differ much from us and ours in size. For men and trees are so constructed that they could not subsist much larger or much smaller. Trees 40 times as big as ours it is well known could not sustain their branches, nor could the heart in a man of so much greater dimensions drive the blood to the extremeties. So a man too small would be subject to too great an irritability of the system. But enough of these surmises.* (Watson, 1802b, his underlining).

Herschel's Method for Determining Size

The first attempt to measure the size of Ceres was made on April 1, 1802. Herschel undertook a series of experiments of a curious nature purposely with a view to ascertaining the diameter of these objects. He found that their extreme smallness rendered the common methods inapplicable and therefore resorted to others of his own invention. Having heated some sealing wax and drawn it out into small threads he passed the ends of them through the flame of a candle. They consequently had at the end of each thread a small round globule of wax. It was now necessary to measure the diameter of these balls, and this was accomplished by means of a solar microscope that projected their images on a sheet of paper and their size was thus ascertained with great accuracy. A row of these waxen balls thus arranged was placed on a card at the distance of 700 or 800 ft and viewed with a telescope. By knowing the distance at which they were placed, and their real diameters, it was easy to calculate the angles under which they would be seen (Babbage, 1815). Herschel examined the balls with different magnifying powers. With a telescope magnifying 150 times he could perceive a globule subtending only an angle of part of a second in diameter. (It results from this that Ceres is about 161 miles in diameter and Pallas 147 according to greatest extent, or 40 times smaller than the Moon.)

While the description by Babbage seems straightforward, Herschel's contemporaries were somewhat perplexed. The French astronomer Pierre Méchain (1744–1804) was his usual diplomatic self (Mechain, 1802):

> *The observations of the new star discovered by Dr. Olbers and of Piazzi's star which you kindly gave me are endlessly curious and interesting. I admire your ingenious means of determining such small diameters and I would be delighted to be able to understand them, though I do not at all doubt their accuracy.*

Herschel had founded his career as an astronomer on his unwavering belief that he could see things through the telescope that no one else could, and he used a simile from his first love, music, to make the point. In response to those who found his use of the magnification 6450× unbelievable, he wrote to his friend Watson:

> *Seeing is in some respect an art which must be learnt. To make a person see with such a power is nearly the same as if I were asked to make him play one of Handel's fugues upon the organ. Many a night have I been practicing to see, and it would be strange if one did not acquire a certain dexterity by such constant practice.* (Herschel, 1782).

Reaction by Other Astronomers to Herschel's Measurements

Never one to mince words, Baron von Zach was ruthless in his condemnation of Herschel's diameter measurements, as evidenced in this letter he wrote to Barnaba Oriani, Director of Brera Observatory in Milan (the best appreciation of Oriani was published on the 100th anniversary of his death by Bianchi, in 1933): "What is your opinion of Herschel's asteroids? Do you believe in the measurements of the diameters of Ceres = 0.″216 and of Pallas 0.″13. I do not believe it, with these pretensions one makes a fool of oneself" (Zach, 1802s, his underlining).

Gauss reinforced this viewpoint in a letter to Olbers:

> *I wouldn't have thought Pallas to be so small that Herschel would require 73,000 to produce one Mars. I don't entirely comprehend on what Herschel could have based his statement, for he knew nothing whatever about the distance of Pallas. To want to distinguish between 'planeta' and 'planetula' seems to me to be almost pedantic. Mercury, Venus, Earth and Mars are also 'planetulae' compared with Jupiter, and perhaps our Sun compared with other fixed stars would just be a tiny 'solculus.'* (Gauss, 1802d).

Olbers, however, had a more balanced approach:

> *The contrast between Schröter's and Herschel's measurements is most surprising. Just between us, I trust neither of them. I believe Schröter has included too much spurious light in his measurements, and he would have perhaps found a fixed star to be just as large.—And Herschel?—I mean, the eye could easily be misled in comparing such small dimensions. Even if he enlarged Pallas 500 times it would have appeared to him (according to his stated diameter) only as a 1′ 5″—diameter disc appears to the naked eye. With such a diameter a disc actually still appears as a point, and whether one of two such small disks appears larger than the other depends only on the brightness of these small disks. The light from Pallas must certainly have become very feeble in the telescope after a 500-times magnification, and hence a probably brighter, though much smaller, disc could still appear as large as Pallas to the naked eye.—Nevertheless, I am convinced that Herschel is much nearer the truth than Schröter.* (Olbers, 1802b, his underlining).

Maskelyne, in a private memorandum of May 1802, did not quite know what to make of Herschel's diameter measurements: "Dr. Herschel has made some curious observations of the apparent diameters both of Pallas and Ceres, from which he infers the real diameter of Pallas to be 95 miles and that of Ceres 162 miles. He considers them as if a different species from the known planets. In their motions and smallness they resemble comets, but in the clearness of their light the other planets."

In Paris, Lalande expressed his utter rejection of Herschel's results, telling him bluntly that no one else could be persuaded of their soundness: "I have found with pleasure your letter about the planets of Piazzi and Olbers. I wish that we could interest others for acceptance of your ideas. But it is not possible to believe that the planet of Olbers has a diameter of only 70 miles. We could not see it [if it were that small]" (Lalande, 1802b).

Lalande (1802a) also wrote this on the topic:

> *As for its size it appeared to Piazzi as an 8th magnitude star, when it came closer to us we estimated it about 7th magnitude. This seemed to me at most one half-second of apparent diameter. Mr. Herschel wrote us that with his best telescope it had not more than one second of diameter at most and that it did not have any noticeable nebulosity. But in the following he gave it not more than a quarter of a second. If I assume one half-second, I find its true diameter to be 290 leagues, this means 20 times less than the earth. Mr. Herschel gives it only 54 leagues. This extreme smallness of the new planet escapes the adopted rules since it is a main planet, much smaller than the moon which is the largest of the secondary planets.*

On August 5, 1802, the German astronomer Johann Elert Bode wrote to Herschel with some astonishment regarding the diameter Herschel had assigned to Ceres and Pallas:

> *Let me thank you for your kind letter of May 22 and your results of your studies regarding Ceres and Pallas. I read them not only with pleasure but also with a kind of astonishment because it remains inexplicable to me how you can obtain such small true diameters of these two celestial bodies since you agree with Dr. Gauss regarding their true distances, as I believe. I cannot imagine how extremely small you must find according to this the apparent diameters and how you are able to determine such small diameters. How can planetary bodies reflect their light if they themselves have only apparent diameters of parts of seconds. In March Ceres appeared as a 7th magnitude star and was visible to some of my friends with the naked eye.* (Bode, 1802b).

Controversy with Johann Schröter

In addition to the magnification issue, many astronomers were incredulous when Herschel informed them that Ceres and Pallas were very small. These results were in contrast to those obtained by Johann Schröter, who found them much larger (Schröter, 1805, 1816). The fact that these were the two most celebrated observational astronomers of their day is not in dispute. Herschel employed the largest telescopes in England, and Schröter, likewise, had the largest telescopes in Germany (e.g., see Gargano, 2012). Their respective and contrasting observations were the subject of a treatise by a teacher at the Collegium Carolinum in Brunswick, August Christian Gelpke (1764–1842; 1801, second edition 1806). His chapter on the asteroids will be published in a subsequent volume of this work.

That Herschel and Schröter were well aware of each other's methods is apparent in a letter Herschel wrote to him a decade before the asteroid size controversy erupted. The letter is an openly-sarcastic swipe by Herschel, contrasting his 'old' method with Schröter's 'new' method. His use of underlining affirms this is a sarcastic tone, which he is to use more than once in his later public writing about Schröter's Solar System observations. Clearly, Herschel was not impressed by Schröter's claims, either then or a decade later when they used their respective instruments to measure the asteroidal diameters:

> *You mention your new Projection's Micrometer; as I suppose that you have undoubtedly taken notice of my camera-eye-piece etc: whereby I project objects on a sheet of paper, upon a wall, upon a measuring scale, upon a set of disks, peripheries, lucid points, draw images of objects, let the points of a pair of compasses that they will exactly fit into any two holes that a person makes upon a card fixed up at a distance etc. As I suppose you [are] acquainted with all these things I should be glad to know in which respects your new differs from my old Projection-micrometer.* (Herschel, 1792, his underlining).

The year 1792 was an important one in the relationship between Herschel and Schröter, due to some extent to poor translation from German to English, as noted by the Greenwich astronomer William Thynne Lynn (1835–1911). In referring to recent observations of Venus, Lynn (1892, p. 346) wrote:

> *No one can read them without being struck by the fact to how great an extent they confirm the observations made by Schröter a century ago, the accuracy of which was so strenuously contested by Herschel in the 'Philosophical Transactions' for 1793, and reasserted by Schröter in 1795. My present purpose, however, in referring to this controversy is to point out the danger of trusting translations in matters of this kind and the importance of referring in disputed points to the originals. Amongst the observations of Schröter to which Herschel alluded, in a tone which he must have afterwards regretted, were what he calls "flat spherical forms conspicuous on Saturn." What Schröter really wrote was "abgeplattete Kugelgestalt des Jupiter und Saturn," meaning flattened spherical shape of the planets themselves, not of markings on them.*

A point on which Herschel and Schröter were clearly at odds was the height of the mountains of Venus (James, 2003; Dunér, 2013). Schröter believed them to be five or six times as high as those on Earth. Herschel correctly countered that no eye which is not considerably better than his, or assisted by much better instruments, will ever get a sight of them. So from this time it was made plain that Herschel believed his telescope was the worlds' finest, and that Schröter was seeing things that could not in fact be seen. As in many propositions put forward by Schröter, he was unable to distinguish between an alethic possibility and an alethic necessity (Cunningham & Orchiston, 2015).

Despite this dispute, they maintained a correspondence. Herschel asked him for his views on the planetary nebula that he, Herschel, had discovered, and in 1797 Schröter sent him a short paper outlining his investigation. Herschel remained unimpressed by Schröter's originality. In an undated personal note, he wrote: "Mr. Schröter says he cannot consider every Nebula a distant Milky Way. I have already proved the same in my paper on nebulous Stars and mention the Nebula in Orion among others as an instance" [RAS W.7/6].

Herschel and his observations of the nebula are further considered by Bode in his book about the asteroids, where he identifies such an observation as a pre-discovery view of Ceres; see Chap. 8 of this volume, and Steinicke (2010) for background on Herschel's nebular work.

With the relationship between them already strained in such a public forum as the *Philosophical Transactions*, and Herschel's reaction to Schröter's nebular conclusions, their subsequent disagreement about Ceres and Pallas is thus placed in a broader context. In his letter to an unknown correspondent in England (probably his friend George Best, 1756–1823), Schröter makes his case quite forcefully:

> *After having read Mr. Herschel's paper on the two new planets (not asteroids) I discovered the reasons for his mistakes in measuring their diameters: Dr. Herschel measured the same way I did but*
>
> *(a) He positioned the projection disc at an immense distance from the eye, from 124 to 178 feet without realising that an illuminated body seen with the naked eye, except for a certain distance appears relatively the larger the farther it is removed from the eye. I made several tests with an identical illuminated disc of 1.2 inches by seeing it with one eye and with the other through a sextant's tube without glass. By this it appeared at a distance of 170 feet 5 times smaller than with the other naked eye. The more I was approaching the larger it became proportionate to that one seen with the naked eye and finally both agreed at eight feet. I changed the eyes; but it was and remained the same. Consequently, Dr. Herschel obtained, since he did not use the greater but the true and much smaller diameter for his calculation, a five times smaller diameter as product.*
>
> *(b) He did not measure, as I did, the nebulosity as well but only the brighter disc. And he used magnifications of 400–800, much too great for such a pale and comet-like planet. Due to lack of light and acridity he thus did not distinguish the entire disc with nebulosity but only its brighter centre part which he, as he himself says, saw as a cometary nucleus. Thus he saw the nebulosity's diameter sometimes six to seven times greater than this nucleus, which was not the case with my magnifications. A calculation for his errors produced his diameter of Pallas equally great as I found it. As a test I will soon measure the Georgian planet (Uranus) in the same way and Mr. Harding, who is working incredibly eagerly, is writing a little work on it to which he will also attach a chart of the smallest stars of that celestial region which Pallas will pass next year to find it wherever possible.* (Schröter, 1802, his underlining).

Gauss also weighed in on the discrepancy between the Herschel and Schröter diameter results, as reported by Zach:

> *Gauss finds the diameter according to Dr. Herschel's own measurement slightly greater. Dr. Herschel gives on April 22 according to a fairly good observation the diameter = 0″.17; and Dr. Gauss calculated the true diameter 26½ German miles (the distance from earth = 1.562).* [Ed: a German mile is 25,000 feet, compared to 5,280 feet in an English mile.] *In his latest letter he expressed his astonishment about Dr. Herschel's and Dr. Schröter's different results of the diameters because they were made according to one method. "I am very curious to learn what magnifications Dr. Herschel used. A magnification of 500 times would hardly turn an apparent diameter of 0″.17 into a disc, would it?" I, for my part, could not discern a trace of a disc at 300× magnification of neither Olbers' nor Piazzi's planet.* (Zach, 1802t, p. 195).

As a matter of comparison, it is worthwhile looking at the diameter measurements made by both men of the planet Mars. At the opposition of 1783, Herschel made micrometer observations on three nights. He found an equatorial diameter of 9.13″ and a polar diameter of 8.57″. These figures were the first ever proving the oblateness of Mars, the ratio derived being 1:16.3 (Herschel, 1784).

Also employing a micrometer, Schröter observed Mars on 1 and 3 September 1798 when Mars was near opposition. His figures were 9.84″ and 9.72″, giving an oblateness of 1:81 (quoted in See, 1901, p. 97).

In his study of 22 diameter measurements of Mars, See (1901, p. 104) gives a mean value of 9.67″. He notes that the filar micrometer is considered to be the best standard for such a measurement. So we see that Herschel underestimated the diameter of Mars; in fact, of the 22 measurements given by See, Herschel's is by far the smallest, a figure of 9.47″ being the next closest (by Kaiser in 1862–1864). While Schröter's measurement is higher than the mean, several others were larger still, with Lowell (1896) at 9.92″ being the largest. Schröter was also closer to the correct figure of the oblateness, the modern figure being 1:500.

See (1900) also did a survey of diameter measurements of Venus. He lists one by Herschel, based on a single micrometer observation, of 18.790″ (Herschel, 1793, p. 217). Based on four days of study in 1792, Schröter derived a diameter of 16.7″ (Schröter, 1792, p. 317). See (Schröter, 1792) gives a best value of 16.8″, nearly identical to Schröter's result.

In 1807 Schröter wrote a book about the first three asteroids. Its contents were summarised in *The Eclectic Review* (1807, p. 182–183), which included this pithy sentence: "The observations themselves, Mr. Schröter defends against every possible objection, especially against the measurements of Dr. Herschel, which are in strong opposition to them."

Huth (1804), observing in Frankfurt, also thought the asteroids larger than Herschel believed. The situation did not improve for years. Alexander Maxwell (1817, p. 32) wrote about the discrepancy between Herschel and Schroeter, although he mistakenly cited an exact knowledge of the distance, rather than angular diameter, as the issue.

> *If anything could awaken the attention of mankind to the impossibility of attaining an exact knowledge of the distance, and consequently of the size of the planetary bodies, it must be the continual difference which appears between calculations made by able and distinguished astronomers. The two greatest astronomers of the present age are supposed by some to be Schroeter in Germany, and Dr. Herschel in England. They have both exercised their continual skill in the measurement of the new planets, found to exist between the orbits of Mars and Jupiter. With the application of the nicest instruments, they differ so materially, as to prove to a demonstration the uncertainty, if not the impossibility, of such calculations.*

Various authors of popular astronomy books quoted different values for the diameter of Ceres as the century progressed. Guy (1819) gives 2834 km. Aspin (1825) quotes both 262 km (Herschel) and 2614 km (Schroeter). Tomlinson (1840) gives Ceres as 2832 km, while Dick (1846) quotes 2614 km. Whewell (1853) gives 262 km. Flammarion (1881), by combining estimates of angular diameter with measurements of brightness came up with a figure of 350 km. The figure of 767 km, arrived at by Edward Emerson Barnard (1895) from filar micrometer measurements using both the 36-in. Lick and 40-in. Yerkes refractors, became the accepted value well into the twentieth century. His survey of the historical diameter measurements is also very valuable (Barnard, 1894).

The Color of Ceres

Basic to these studies of the asteroids was an interpretation of what their light (reflected from the Sun) revealed. It was while Ceres was being searched for in late 1801 that the dual nature of light itself was discovered.

A paper entitled "On the theory of light and colours" was read before the Royal Society of London in November of 1801. It was written by the polymath Thomas Young (1802), who believed that Newton's corpuscular theory of light was in error (Burwick, 1986, p. 30). Like William Herschel, Young was savaged in the pages of the January 1803 issue of the *Edinburgh Review* by Henry Brougham (1778–1868), but in this passage about Young's paper, Brougham threw Herschel a 'bone': "This paper contains nothing which deserves the name, either of experiment or discovery. It is in fact destitute of every species of merit. In the name of science let his papers not find admittance into that venerable repository which contains the works of Newton and Maskelyne and Herschel."

It must be noted that Brougham's attack was not merely literary 'fluff'—it proved disastrous to Young. According to several contemporaries, his diatribe virtually stopped the spread of Young's wave theory (e.g., see Götschl, 1995, p. 1).

In his first paper on Ceres, Herschel (1802a: cx) noted its color as "… faintly ruddy," but he qualified it by stating that "… perhaps it appeared rather the more so, on account of my viewing it after the Georgian planet which is of a mild bluish tint." The well-known nineteenth-century observer Thomas William Webb (1807–1885) complained that Herschel was rather too "… partial to red tints." (Holmes, 2010, p. 87).

Herschel's second paper contains three brief notes on color: "February 13: The colour of Ceres is ruddy, but not very deep. April 21: Ceres is much more ruddy than Pallas. April 22: Pallas is of a dusky whitish colour" (Herschel, 1802b, p. 220). He assigned no remarks on color to either Juno or Vesta in his published papers, but in his second-to-last observation of Vesta, he wrote "Its light is ruddy." This appears in his unpublished notes on February 16, 1814. The amateur astronomers George Gilpin, William Walker (1766–1816) and Alexander Aubert (1730–1805) also saw Pallas as reddish, and Maskelyne thought Ceres "… white with a reddish hue."

The dictionary definition of ruddy is "reddish," or "rosy," so Herschel was seeing the same color many attribute to the planet Mars. Modern data give a U-V for Ceres of 1.15, with a flat spectrum in the visible region. The general consensus of modern telescopic observers is that Ceres appears white or bluish-white. It should be noted that Hubble Space Telescope images of Ceres appear to show a broad reddish area on its disk, but this is a false-color image. Likewise Vesta appears white in a modern telescope with no ruddiness apparent.

Whether Herschel's observation of Ceres, Vesta and other celestial objects as reddish "… was a purely subjective problem, a physiological one, or down to his speculum metal being a better reflector at the long-wavelength end of the spectrum, is still open to debate" (Holmes, 2010, p. 87).

Despite the suggestion by Holmes, based on a modern understanding of the situation prevalent two centuries ago, there is no definitive evidence that the speculum metal used by Herschel was responsible for him seeing reddish objects. This leaves

Table 5.1 English astronomers who reported on the color of asteroids in 1802

Observer	Object	Color	Date	Magnification	Comments
Walker	Pallas	Red	April	100	Brighter than Ceres
Herschel	Ceres	Ruddy	13 Feb	600	
	Pallas	Dusky whitish	22 Apr	881	
Gilpin	Ceres	Red	18 Feb		Mag. 8
Maskelyne	Ceres	Nearly white, possible reddish cast	23 Feb	50 and 200	Moonlight strong
Aubert	Pallas	Reddish	April		Larger than Ceres
Lee	Pallas	Dull red	April		

the first two possibilities mentioned by Holmes as the culprits, and they almost certainly both had an effect. According to Dr. Roger Ceragioli of the Steward Observatory Mirror Laboratory, a mechanical cause is not to blame: "I doubt that the 18.7″ mirror used in the 20-ft was more than just slightly yellow. His smaller mirrors looked 'white.' Only the 48″ may have looked positively yellow, and Herschel almost never used that telescope. I very much doubt that his mirrors were the cause of his bias toward seeing faint stars as reddish. It was something else."

There is no way to make a certain assessment of Herschel's personal proclivity to assign reddish tints where they were unwarranted, but this personal bias was the likely factor. Both Schröter and Harding also (sometimes) found Ceres to be reddish, while at other times white or bluish, so atmospheric conditions, such as highly variable particulate matter, is also a possible component. There is also a real "outlier" here: George Knott, observing from Woodcroft Observatory in Cuckfield, England, reported he saw Ceres thus: "The disc on this occasion was well defined, and of a ruddy yellow hue" (Knott, 1866) (Table 5.1).

Ceres in Early English Literature

Ceres, the goddess, figured in many early English writings from Chaucer ("Ceres, that doth of hunger bote") to Edmund Spenser ("Corne to the Lady Ceres"). Here are some notable examples:

> *The Assembly of Gods* (c. 1480, anonymous)
> *And next in ordre was set by hys syde*
> *Ceres the goddesse, in a garment*
> *Of sak clothe made with sleves large and wyde, Embrowderyd with*
> *sheves and sykelys bent.*
> *Of all maner greynes she sealyd the patent, In token*
> *that she was the goddesse of corne. Olde poetys sey*
> *she bereth the hervest horne.*

In *The Tempest* by William Shakespeare (1564–1616), Prospero invokes Ceres to bless the marriage of Ferdinand and Miranda. Ceres sings:

Earth's increase, foison plenty, Barn and garners never
empty,
Vines with clust'ring bunches growing, Plants with goodly burthen
bowing: Spring come to you at the farthest
In the very end of harvest' Scarcity and want shall shun you;
Ceres' blessing so is on you.

And here in Book IV of *Paradise Lost* by John Milton (1608–1674):

Not that fair field
Of Enna, where Proserpin gath'ring flow'rs
Herself a fairer Flow'r by gloomy Dis
Was gather'd, which cost Ceres all that pain
To seek her through the world

Chapter 6
How Did the Public Learn About Ceres?

Press Reports on the Discovery of Ceres

On August 7, 1801, *The Times* of London, the leading newspaper of the day, reported without hyperbole the discovery of Ceres in just two sentences: "An important discovery has been announced—that of a new planet. M. Piazzi, an Italian astronomer, claims the merit of this discovery" (Fig. 6.1)

Vying for coverage that day were reports that show a Europe in turmoil: political assassination, naval bombardments and horrendous war casualties. "The weather has been so thick and hazy the whole of today, that neither the French coast nor Lord Nelson's fleet have been distinguishable. The cannonading at Boulogne continued all day yesterday. Out of 11,000 recruits sent to the Emperor of Germany's own Regiment of Dragoons, 3800 men only have been left. The Count de Lichtenberg, Major General in his Imperial Majesty's Service, was assassinated on the 12th of last month, in his house at Dobling in Austria." Many of the letters found in this book refer to the war-induced inconveniences and losses the asteroid pioneers were subjected to.

On a lighter note, we read in the newspaper of August 7 that the "King of Prussia gives every possible encouragement to the silk manufacturers in his dominions, which had suffered much for the last 2 years in consequence of the prohibition of Paul I against their importation into Russia. Silk is generally worn at the Court of Berlin." And in England, "this being the birth day of her Royal Highness the Princess Amelia, their Majesties (sic) youngest daughter, who completes the 18th year of her age, the Royal Family will receive the compliments of the Nobility."

But in an age of widespread illiteracy, newspapers were not what they are today. "I never reads a newspaper," said one merchant captain lately arrived at Madras in this period. "I have a large family and I never suffers such a thing to come into my house" (Longford, 1969, p. 79).

Merchantmen had risky lives, especially in the Mediterranean. The pirates of the Barbary States—Algiers, Morocco, Tripoli, and Tunis—declared open warfare on

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
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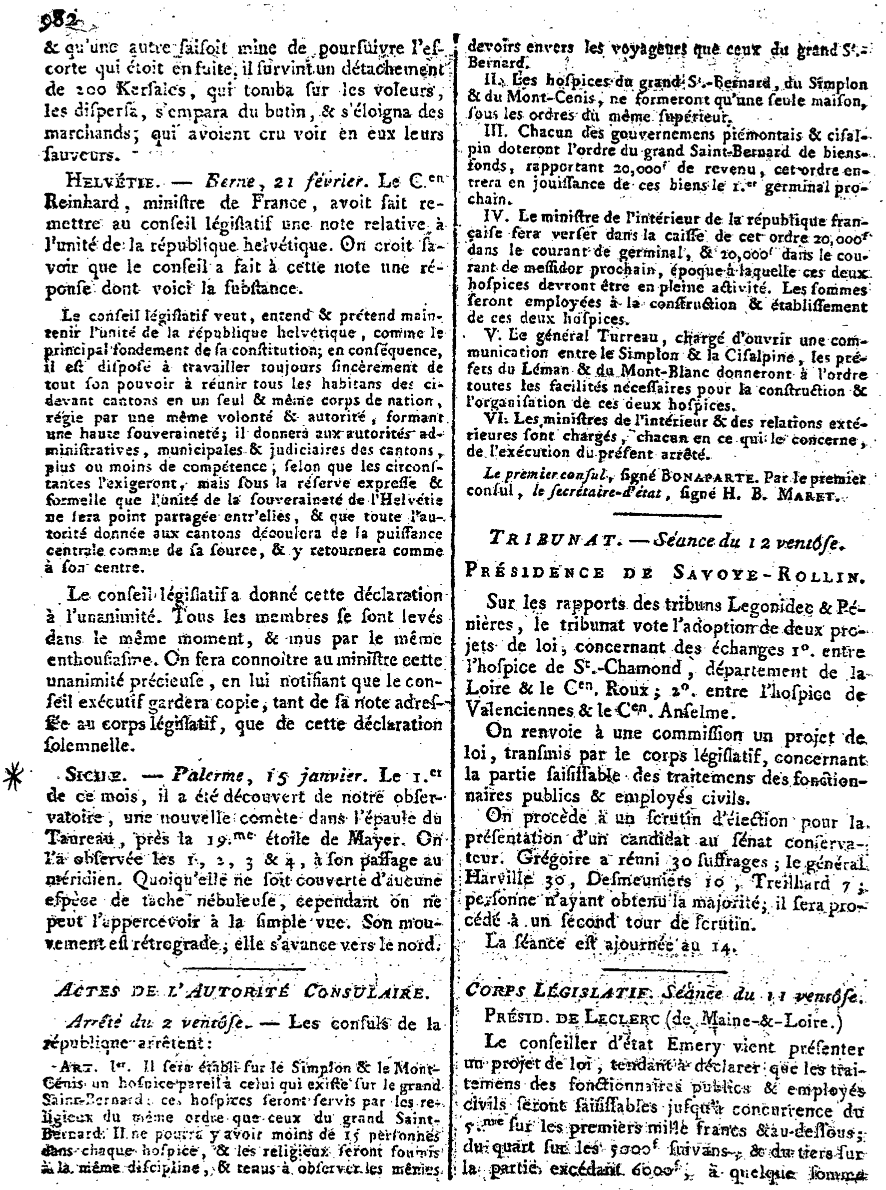

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& qu'une autre faiſoit mine de pourſuivre l'eſcorte qui étoit en fuite, il ſurvint un détachement de 200 Kerſales, qui tomba ſur les voleurs, les diſperſa, s'empara du butin, & s'éloigna des marchands; qui avoient cru voir en eux leurs ſauveurs.

Helvétie. — *Berne, 21 février.* Le C.[en] Reinhard, miniſtre de France, avoit fait remettre au conſeil légiſlatif une note relative à l'unité de la république helvétique. On croit ſavoir que le conſeil a fait à cette note une réponſe dont voici la ſubſtance.

Le conſeil légiſlatif veut, entend & prétend maintenir l'unité de la république helvétique, comme le principal fondement de ſa conſtitution; en conſéquence, il eſt diſpoſé à travailler toujours ſincèrement de tout ſon pouvoir à réunir tous les habitans des ci-devant cantons en un ſeul & même corps de nation, régie par une même volonté & autorité, formant une haute ſouveraineté; il donnera aux autorités administratives, municipales & judiciaires des cantons, plus ou moins de compétence, ſelon que les circonſtances l'exigeront, mais ſous la réſerve expreſſe & formelle que l'unité de la ſouveraineté de l'Helvétie ne ſera point partagée entr'elles, & que toute l'autorité donnée aux cantons découlera de la puiſſance centrale comme de ſa ſource, & y retournera comme à ſon centre.

Le conſeil légiſlatif a donné cette déclaration à l'unanimité. Tous les membres ſe ſont levés dans le même moment, & mus par le même enthouſiaſme. On fera connoître au miniſtre cette unanimité précieuſe, en lui notifiant que le conſeil exécutif gardera copie, tant de ſa note adreſſée au corps légiſlatif, que de cette déclaration ſolemnelle.

* Sicile. — *Palerme, 15 janvier.* Le 1.[er] de ce mois, il a été découvert de notre obſervatoire, une nouvelle comète dans l'épaule du Taureau, près la 19.[me] étoile de Mayer. On l'a obſervée les 1, 2, 3 & 4, à ſon paſſage au méridien. Quoiqu'elle ne ſoit couverte d'aucune eſpèce de tache nébuleuſe, cependant on ne peut l'appercevoir à la ſimple vue. Son mouvement eſt rétrograde; elle s'avance vers le nord.

Actes de l'Autorité Consulaire.

Arrêté du 2 ventôſe. — Les conſuls de la république arrêtent:

Art. 1[er]. Il ſera établi ſur le Simplon & le Mont-Cénis un hoſpice pareil à celui qui exiſte ſur le grand Saint-Bernard: ces hoſpices ſeront ſervis par les religieux du même ordre que ceux du grand Saint-Bernard. Il ne pourra y avoir moins de 15 perſonnes dans chaque hoſpice, & les religieux ſeront ſoumis à la même diſcipline, & tenus à obſerver les mêmes devoirs envers les voyageurs que ceux du grand S[t].-Bernard.

II. Les hoſpices du grand S[t].-Bernard, du Simplon & du Mont-Cenis, ne formeront qu'une ſeule maiſon, ſous les ordres du même ſupérieur.

III. Chacun des gouvernemens piémontais & ciſalpin doteront l'ordre du grand Saint-Bernard de biens-fonds, rapportant 20,000[f] de revenu, cet ordre entrera en jouiſſance de ces biens le 1.[er] germinal prochain.

IV. Le miniſtre de l'intérieur de la république françaiſe fera verſer dans la caiſſe de cet ordre 20,000[f] dans le courant de germinal, & 20,000[f] dans le courant de meſſidor prochain, époque à laquelle ces deux hoſpices devront être en pleine activité. Les ſommes ſeront employées à la conſtruction & établiſſement de ces deux hoſpices.

V. Le général Turreau, chargé d'ouvrir une communication entre le Simplon & la Ciſalpine, les préfets du Léman & du Mont-Blanc donneront à l'ordre toutes les facilités néceſſaires pour la conſtruction & l'organiſation de ces deux hoſpices.

VI. Les miniſtres de l'intérieur & des relations extérieures ſont chargés, chacun en ce qui le concerne, de l'exécution du préſent arrêté.

Le premier conſul, ſigné Bonaparte. Par le premier conſul, *le ſecrétaire-d'état*, ſigné H. B. Maret.

Tribunat. — Séance du 12 ventôſe.

Présidence de Savoye-Rollin.

Sur les rapports des tribuns Legonidec & Pénières, le tribunat vote l'adoption de deux projets de loi, concernant des échanges 1°. entre l'hoſpice de S[t].-Chamond, département de la Loire & le C[en]. Roux; 2°. entre l'hoſpice de Valenciennes & le C[en]. Anſelme.

On renvoie à une commiſſion un projet de loi, tranſmis par le corps légiſlatif, concernant la partie ſaiſiſſable des traitemens des fonctionnaires publics & employés civils.

On procède à un ſcrutin d'élection pour la préſentation d'un candidat au ſénat conſervateur. Grégoire a réuni 30 ſuffrages; le général Harville 30, Deſmeuniers 10, Treilhard 7; perſonne n'ayant obtenu la majorité; il ſera procédé à un ſecond tour de ſcrutin.

La ſéance eſt ajournée au 14.

Corps Législatif. Séance du 11 ventôſe.

Présid. de Leclerc (de Maine-&-Loire.)

Le conſeiller d'état Emery vient préſenter un projet de loi, tendant à déclarer que les traitemens des fonctionnaires publics & employés civils ſeront ſaiſiſſables juſqu'à concurrence du 5.[me] ſur les premiers mille francs & au-deſſous; du quart ſur les 5000[f] ſuivans, & du tiers ſur la partie excédant 6000[f], à quelque ſomme

Fig. 6.1 The first public notice of the discovery of Ceres was published in this single paragraph in the *Paris Journal* in February 1801, highlighted with an asterisk at left

U.S. shipping in 1801, thus breaking the treaty of 1796 which was drafted by Joel Barlow (1754–1812; see Chap. 4). Yusuf Karamanli, pasha of Tripoli, opened the Tripolitan War (1801–1805) by the symbolic act of ordering his soldiers to cut down the flagpole at the U.S. consulate. The action followed U.S. refusal to satisfy

Tripolitan demands for more tribute to guarantee protection from piracy against American ships.

At the heart of contemporary events, recording them with wit and clarity in her journals and letters, was Fanny Burney (1752–1840). In the 1770s she lived in a house in St. Martin's Street, London, where Sir Isaac Newton (1642–1727) had once built an observatory on the roof. In 1801, while astronomers were busy searching for Ceres, Burney put the finishing touches on her play *A Busy Day.* It was not produced in her lifetime, and it finally debuted in the West End of London two centuries later (Neill, 2000). Burney was quite fascinated by William Herschel, and it was Fanny's brother Charles who created the word "asteroid" for Herschel in 1802, a saga that will be related in a later volume of this series.

The first newspaper reports were published the day before it appeared in *The Times*. On Aug. 6 both *The Morning Chronicle* in London and the Caledonian Mercury in Scotland announced it. *The Lancaster Gazette* and the *Royal Cornwall Gazette* followed suit on August 8. The *Bury & Norwich Post* ran a report on August 12, followed by the *York Herald* and *Caledonian Mercury* on August 15, the *Hull Packet* on August 18, and the *Exeter Flying Post* on August 20.

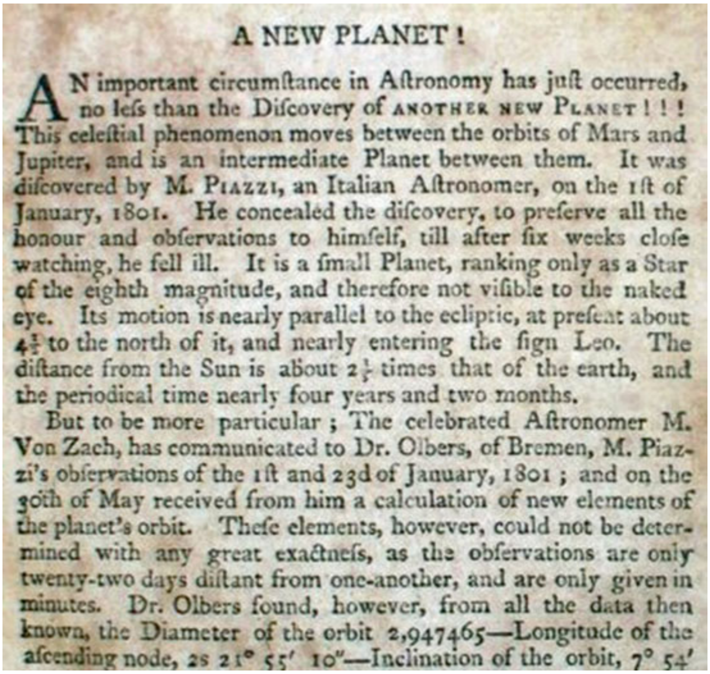
The first reports in the British magazines also appeared in August 1801. The reason for this tardiness was made bluntly apparent to the public in words that certainly were penned by Nevil Maskelyne. His text was published in the *Monthly Magazine* (1801), and then reprinted with additional material across the pond in American newspapers (Fig. 6.2):

The *Monthly Magazine* article ended with this: "Other particulars shall be given in our next." Despite this promise of more in the September issue, none was forthcoming, which sent Capel Lofft into a rage, as indicated by the following letter penned on September 8, 1801:

> *I confess myself one, perhaps of many, who have been mortified by your taking no notice of the New Planet in your last published number, although your preceding number announced a further account to be given of it in your next.* I have seen a private letter, by which it appears, that since the discovery of it by PIAZZI at PALERMO, it has been seen by Professor BODE at BERLIN: and I think there is no reason to doubt that it has been seen by the ASTRONOMERS at the NATIONAL OBSERVATORY at PARIS.*
>
> *By the same letter it appears, that the discovery was communicated to the Royal Professor of Astronomy, Dr. MASKELYNE. In the dearth of astronomical intelligence, which we generally suffer in this country, it would be kind, as early as possible, to fulfil your intimation, and to lay before the public, as much as can be learnt respecting this interesting discovery at present.*
>
> **The account we promised exists in Von Zach's Geographical Ephemerides, which, from some accident, has not yet come to hand from Germany.* (Lofft, 1801).

This short paper and its editorial comment give rise to several interesting points. What is this "private letter" he refers to? Who wrote it? This cannot be answered for a certainty, but it contained a report that Bode and the French astronomers had seen Ceres, which was false.

The editorial comment gives us the very important clue that the sole source of information the *Monthly Magazine* was relying on was Zach's first journal, the *General Geographical Ephemerides.* Unfortunately this cannot be correct, as the



A NEW PLANET!

AN important circumſtance in Aſtronomy has juſt occurred, no leſs than the Diſcovery of ANOTHER NEW PLANET!!! This celeſtial phenomenon moves between the orbits of Mars and Jupiter, and is an intermediate Planet between them. It was diſcovered by M. PIAZZI, an Italian Aſtronomer, on the 1ſt of January, 1801. He concealed the diſcovery, to preſerve all the honour and obſervations to himſelf, till after ſix weeks cloſe watching, he fell ill. It is a ſmall Planet, ranking only as a Star of the eighth magnitude, and therefore not viſible to the naked eye. Its motion is nearly parallel to the ecliptic, at preſent about 4½ to the north of it, and nearly entering the ſign Leo. The diſtance from the Sun is about 2½ times that of the earth, and the periodical time nearly four years and two months.

But to be more particular; The celebrated Aſtronomer M. Von Zach, has communicated to Dr. Olbers, of Bremen, M. Piazzi's obſervations of the 1ſt and 23d of January, 1801; and on the 30th of May received from him a calculation of new elements of the planet's orbit. Theſe elements, however, could not be determined with any great exactneſs, as the obſervations are only twenty-two days diſtant from one-another, and are only given in minutes. Dr. Olbers found, however, from all the data then known, the Diameter of the orbit 2,947465—Longitude of the aſcending node, 2s 21° 55′ 10″—Inclination of the orbit, 7° 54′

Fig. 6.2 *The New England Quarterly Magazine,* Number II of 1802. This Boston publication reprinted exactly the original newspaper report on the discovery of Ceres that appeared in the British press

AGE was published for only 2 years: in 1798 and 1799! In reality, Zach closed the AGE and opened a new chapter in his life and astronomy with the introduction in 1800 of his journal the *Monatliche Correspondenz* (Monthly Correspondence). It was the first journal devoted exclusively to the science of astronomy, and it was in this publication that all the reports about the discovery of the asteroids were printed. But how did the *Monthly Magazine* get a copy? Neither Herschel nor Maskelyne is known to have been a subscriber. Perhaps it was an exchange between editors—Zach sent his journal to the publication in London, and vice versa. Thus, it appears the readers of the *Monthly Magazine* would have been better informed than any of England's leading astronomers. This makes a study of their publications about Ceres and Pallas all the more important, and indeed a critical factor in showing how these new objects were understood and subsequently studied in England. In Germany, a newspaper in Berlin was one of the first to report the discovery:

> *Piazzi in Palermo has claimed to have discovered a comet on 1 January 1801 in the shape of a star of the eighth magnitude and without any obvious nebulosity. But in the light of those observations of Piazzi, Bode now feels justified in regarding this supposed comet as the planet presumed to exist between Mars and Jupiter. The famous astronomers von Zach, Oriani, and even Piazzi agree with him.* (Berliner Haude- und Spenersche Zeitung, 1802, Issue 57)

The discovery reached Russia with a report by Rumovsky (1802). Despite all the orbital calculations and false reports of observation, the recovery of Ceres was still unknown to the public as 1802 dawned. A report early in the year gave some credence to the idea it was all a mistake:

> *The Planet which was supposed to be discovered by M. Piazzi, at Palermo, about a year since, has hitherto eluded the researches of other astronomers. Similar in brilliancy and light to the stars of the eighth magnitude, it has none of those peculiar appearances which serve to distinguish comets of the same small size. In respect to colour, it resembles Jupiter; and, from the meridional observations taken by M. Piazzi, and his colleague, M. CACCIATORE, it appears, that this star, if a planet, possesses a revolutionary period that may be calculated at four and half or five years. About the beginning of May, 1801, the supposed planet crossed the Meridian at an early hour, when it disappeared. Since that time, M. Piazzi, assisted by M. M. Cacciatore and CARIOTTI, have been unable to discover it again either with a night telescope, or with an achromatic, having large apertures. It is not surprising, therefore, that during the last nine months other astronomers have failed in their researches, since to common difficulties is added an uncertainty of some degrees as to the precise point of the heavens in which it should be sought for. It has lately resumed the same situation in which it was at the time of its discovery; we expect, therefore, in a short time to be able to state the reports of the foreign astronomers, and to confirm or reject the existence of this supposed planet.* (The Monthly Magazine, 1802).

Press Reports on the Recovery of Ceres

In England

The next issue of the *Monthly Magazine* was able to report, at long last, the confirmation that Ceres was a real object. It featured two articles, including a first-hand account of its observation from London. The first one, written on March 25, began with an editorial note in square brackets:

> *[The following Communication came to hand too late to appear in its proper place, and we were unwilling to defer it on account of the interesting nature of its contents.]*
>
> *Finding amongst my astronomical friends as well as in the public in general a very high degree of solicitude respecting the appearance, situation, and other attendant circumstances of the newly-discovered planet, the Ceres Ferdinandea, I think it right to send to your useful Magazine the latest situation amongst the neighbouring stars, that the month will admit; that your readers may be enabled by a common night–glass, or a pocket telescope and a little attention, to ascertain it. If an imaginary line is drawn from the star Theta Leonis, through Beta or the Lion's Tail, and continued to the same distance to the left a little above that termination, a cluster of stars will be seen forming an equilateral triangle; the two western most stars being of the fourth magnitude, and the other point of the triangle formed by a star of very minute size. This last star forms also a smaller equilateral triangle with two stars of minute size, nearer to it than the larger ones (of the fourth magnitude). The Ceres, on the fifteenth instant (March) was to the east of the smaller point of the equilateral triangle; I saw it on that evening, and have regularly traced it on the sixteenth, twentieth, twenty-first, twenty-second, and this evening, the twenty-fifth, when it is arrived between the two western-most stars of the fourth magnitude. By continuing this line, it will be very easily perceived, for several evenings to come.*

The Planet appears as large as most of the stars in its neighbourhood. It is calculated to be about half as large as the moon; and to be one third of the distance between Mars and Jupiter from the sun. It performs its period round the sun in four and a half of our years.

Discovered by M. Piazzi, of Palermo, in Sicily, on the first of January, 1801. It has been named Ceres Ferdinandea, in honour of the Goddess of Corn, the Protectress of Sicily, and the reigning Monarch of that island and Naples.

I have examined this Planet with magnifying powers from forty to one thousand times, but hesitate in asserting, that I can see it with a disc or decisive magnitude, as I can the Georgium Sides. (Walker, 1802a, pp. 272–273).

Early newspaper reports in England were published in the Lancaster Gazette (Jan. 9, 1802), and the Bury & Norwich Post (February 3). Publications in Scotland were quick to print articles about the recovery. *The Edinburgh Magazine; or Literary Miscellany* published an article in January by Brewster (1802). It was (as he says) mostly written from "… facts taken from *The Monthly Magazine* for October last." This was surely a mistake, as the October issue only contained the outraged letter by Lofft. The real report was published in the November issue. The *Scots Magazine* related the recovery in the May 1, 1802, issue.

In England, the first report in the April issue of the *Monthly Magazine* about the reality of Ceres was signed W. Walker, Lecturer on the Eidouranion. This person can be identified as William Walker son of the famous Adam Walker (1731–1821), a popular science lecturer who moved to the fashionable Hanover Square in London after many years as a travelling science lecturer (Dictionary of National Biography, 1885–1900). Adam and William wrote a popular work, *An Epitome of Astronomy* that went through 14 editions by 1800, and was still being published in a 31st edition in 1824. In the 1780s, Adam invented a type of orrery he dubbed the Eidouranion. A huge machine some 20 ft in diameter, it is considered to be the ancestor of planetarium projectors. Adam and William were both involved in lecturing about the Eidouranion to audiences in London (King & Millburn, 1978, p. 310).

Walker's article in the *Monthly Magazine* was accompanied by a table of positional data from April 3 to 18, and a small chart showing the stars mentioned along with the path of Ceres (Fig. 6.3).

Walker was also a regular contributor to *The Gentleman's Magazine*. He submitted a nearly identical article, and the same diagram, to that publication (Walker, 1802b, p. 197). He dated the article the same day, March 25, but towards the end inserted a claim that appears nowhere else: "It was discovered by M. Piazzi, at Palermo, on Jan. 1, 1801 and by the most honourable perseverance re-discovered by Dr. Maskelyne early in this year." Since he then gives the positions of Ceres "… by Mr. Zach, at Gotha …," he knew full well that Ceres had been rediscovered by Zach in December 1801, and later by Olbers. By giving credit to Maskelyne, he was obviously subject to craven motives of patronage.

While the search for Ceres had taken on epic dimensions, and the reading public was certainly under the impression that only the world's finest instruments could locate it, the article by Walker (1802b) blithely states that a "… common night-glass …" is sufficient to see it! Many people must have wondered why it took a year to find something that could be spied by a common night-glass.

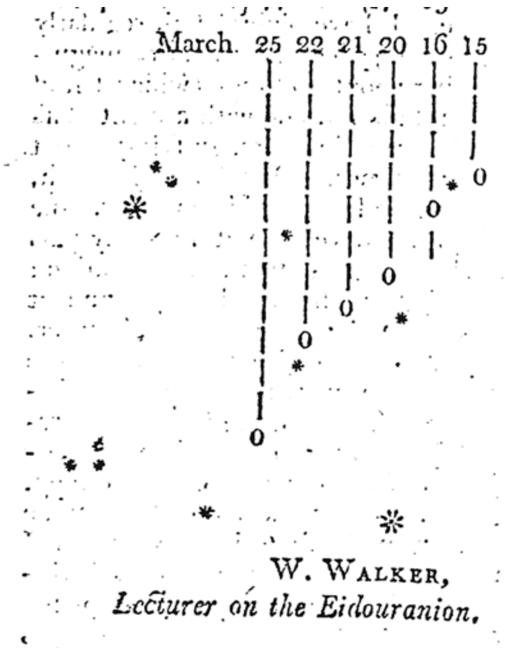


Fig. 6.3 The star chart from Walker's article in the *Monthly Magazine*

The second article in the April 1802 issue of the *Monthly Magazine* (1802a) followed the more traditional lines of reporting what had been done on the Continent. This article was written by the Astronomer Royal, Nevil Maskelyne, but was signed with the pseudonym 'Astrophilus.' It is of interest because it lists those astronomers in England who had seen Ceres: Aubert, Lee, Gilpin, Herschel and Maskelyne. The fact that Maskelyne is not mentioned by name is another confirmation that the author was Maskelyne himself. As we also know from the previous article in this issue, Walker had seen Ceres. How many more is unknown, but certainly the number is very small, an indication of the true size of the active observational community in England at this time. A survey of professional and private observatories in England is given in The Monthly Magazine (1813). At the time of this survey some 20 private observatories were listed, along with eight others such as Greenwich and Oxford. The article concludes with an estimate of the total number of observers worldwide—only about 100.

Now that the missing planet had been found, really serious information about Ceres could be printed. Herschel himself was kept personally informed about the coverage of this work through the publication edited by Alexander Tilloch (1759–1825):

> *I have this moment, just at post hour, received a note from Mr. Tilloch editor of the Philosophical Magazine, very respectfully extracting some short account of your paper, about the new Planet, to appear on the 1st March—and such an account as may appear to be communicated by one who attended the meeting—He says if he receives it on Monday it will be in time for insertion—so I have acquainted him that probably I may get something*

> *drawn up that would be correct in particulars; so as to do justice to your papers.—I wish much however that you yourself would draw up such an abstract and send it* [to] *me for tomorrow's post, and which I could transcribe for Mr. Tilloch.* (Wilson, 1802b, his underlining).

This was of great importance, for Herschel's February 18, 1802, report to the Royal Society was not published until 1912, 110 years later! Tilloch pulled out all the stops, devoting pages 54–83 for a complete coverage of everything written about Ceres by Piazzi, Herschel and Capel Lofft, referring specifically to Lofft's article in the *New London Review* of March 1800, in which he offered some conjectures about an intermediate planet between Mars and Jupiter. Thomas Firminger described Tilloch's journal as "…one of the first channels of periodical scientific information" (Firminger, 1811, p. 307).

The intelligentsia in England was also kept apprised of Continental reporting about Ceres. An article that appeared in the Paris-based *Moniteur* (1802) was translated into English for the *Journals of the Royal Institution* (1802). And the *Monthly Magazine* (1803a) published in summary an English translation of Lalande's *History of Astronomy for the Year 1802,* which mentions Ceres perturbation calculations by Gauss and Oriani. The article led with the discovery of Pallas. It was also published in Lalande (1803). *The Monthly Magazine* (1803b) also gave a full account of what Schröter and Harding were doing:

> *M. Schroeter, grand bailiff at Lilienthal, has several times observed the new planet Ceres Ferdinandea, and he has communicated certain results of his observations to the Royal Society of Sciences at Gottingen. On the 11th of January, 1802, M. Harding likewise observed this new planet in a magnified state of 136 and 288 times, with his reflector of 13 feet, and found its disc in a reddish light, nebulous, not terminated, and larger than a satellite of Jupiter. The infavourable weather and indisposition were the reason that M. Schroeter could not combine his observations with those of M. Harding, till the 25th of January. On that day, with the same magnifying, and by the same reflector, the disc of Ceres appeared to M. Schroeter under the perfectly round form of a planet, without scintillation, and for that time in a light not reddish, but perfectly white; it was exactly terminated, and every way similar to that of the planet Herschel; but it was inclosed in a nebulosity like that of a comet, very narrow, which completely environed it, and which made a strong contrast with the exact manner in which it was terminated. With respect to this singular termination, the new planet, in some measure, resembled the comet of 1799, described in the third volume of the Memoirs of M. Schroeter; only that its disc appeared clearer and more distinct, and its atmospherical nebulosity was extremely narrow. M. Schroeter, on the same night, by means of a microscope, with a magnifying power of 288 of his reflector, found the diameter of the disc exactly terminated, 1″815, and the entire diameter, including the nebulosity, 2″514, (M. Harding, 2″330); the right ascension was at 11 h 36′ 188° 19′ 50″, the north declination 11° 54′ 43″. At the time of the following observations, the planet appeared always sometimes more, sometimes less, nebulous, and it no longer appeared exactly terminated as before; so that its aspect sometimes resembled a planetary nebula near ϒ of Aquarius. Its white light varied on the 26th to a bluish; but on the 28th and 31st of January, with the same magnifying of the reflector, it approached to a reddish colour. On the 26th of January, the apparent diameter was 2″687; on the 28th 2″793; on the 31st 2″930.* [The paper concludes with a table of positional measurements from January 10 to 31.]

The wholly spurious observations of a nebulosity, which became equated with a dense atmosphere, was repeated in books and journals for decades to come.

The Repertory of Arts (1802) seems an unlikely place to find mention of the asteroids, but as it published a summary of the proceedings of the Royal Society, we find the following, which is one of the first notices the reading public had about the new word 'asteroid'. This comes from Herschel's second paper read to the Royal Society:

> *On the 6th of May, Dr. Herschell's* [sic] *observations on the two lately discovered celestial bodies were read: he states the result of his attempts to measure the diameters of Ceres and Pallas, which are about 163 and 95 or 71 English miles. He proceeds to consider the nature of the new stars, and from various circumstances in which they differ from the general character of planets, he wishes to call them asteroids.*

Nicholson's publication, *A Journal of Natural Philosophy,* was a prime conduit of public information about Ceres. In its first volume (1802a) it reprinted a verbatim report about Ceres from the Royal Institution, an extract from Bode's *Kurzer Entwurf der Astronomischen Wissenschaften* (1794), where he postulated the existence of a planet between Mars and Jupiter; and a letter from Sir Henry Englefield [1752–1822] to Young.

The reading public was kept informed about Ceres for decades after its discovery. Here is a letter to *The Times* of London, reprinted in *The Mechanics' Magazine* (1830, p. 111):

> *The following information with respect to another of the new planets, is contained in the letter of a second correspondent of The Times:–*
>
> *On Friday night last, (9th April) at 12 hours 48′, sidereal time, the planet followed the star Bode Librae, 3″ 3/10, and was 6′ 46″ of a degree to the south of it. On Saturday night, at 14 hours 56′, she preceded the same star 13″ 75/100, and was 4′ 8″ 55/100 to the south of it. Clouds prevented me observing it on the meridian. Applying these quantities to the place of 82 Bode Librae, the data furnished by Encke's, or the Berlin Ephemeris, are, I find, amply sufficient to enable the observer to direct his instrument to the spot which the planet occupies in the heavens. She has the brightness of a star of the seventh magnitude, has no peculiar colour, and can only be distinguished from the star 82 Bode Librae, by her motion. She is almost 4° south of β Librae, and precedes it about 6′ of time; hence the field of the finder being 4° in diameter, if β Librae be brought into the lower part of the circumference of the field, Ceres will be found in the upper.*

This letter, which was not signed, concludes with the ephemeris positions of Ceres from April 18 to May 10.

In France and Germany

The reading public in France was informed about the recovery in the January issue of *The Moniteur* in an article written by Delambre (1802). [Jean-Baptiste Delambre, 1749–1822; Director of Paris Observatory 1804–1822]. At this time, *The Moniteur* had the largest daily circulation (20,000) of any publication in Paris (*The Scots Magazine,* 1802, vol 64, 421):

> *Astronomy. Piazzi's star, the star, discovered one year ago in Palermo by Mr. Piazzi, has escaped all astronomers until today and it seems according to Mr. Piazzi's and his adjunct Mr. Cacciatore's observations that this star was a planet whose revolution seems to be four*

years and five or six months. Around the end of Pluviose (February, 1801), the star ceased to be visible. It will soon be in the same situation where it was at the time of its discovery; and if the sky is a bit more serene, we can still maintain our hope of finding it again. [Ed: Pluviose refers to the French republican calendar; it describes the "month" between January 20 and February 18.]

Articles in the Berlin and Hamburg newspapers in January 1802 also alerted the public to the recovery:

The new planet, which was first discovered by Piazzi on the 1st day of January 1801, was rediscovered by Olbers on January 1, 1802 after astronomers had searched for it for four months in vain. That this star is a planet, a fact that many question, has been decided by this and at the same time its orbit is so exactly determined that it can be refound at any time. It revolves around the sun between Mars and Jupiter and appears now as a 9th magnitude star which only differs from a fixed star in its motion at a magnification of 100 times. (Kaiserliche privilegierte Hamburgische Neue Zeitung, January 12, 1802.)

Dr. Olbers in Bremen was so fortunate as to find on January 1 the new planet Ceres, as a 9th magnitude star in the northern arm of Virgo, west of the star ς. On January 2, around 11:58 pm its right ascension was 185° 9′ and the declination north 11° 7′. On the 6th at 5 am, when Ceres was close to No. 208 of Bode, the first was 185° 45′ and the latter 11° 8′. Thus at last the existence of this planet has been completely proven and the assumption of the same mentioned by Mr. Bode has been confirmed successfully. (*Vossische Zeitung* [Berlin] January 14, 1802.)

A German deserves respect for the discovery of Uranus (on March 13, 1781)—Herschel became famous through this—and our compatriots have made great contributions to the rediscovery of Ceres. Olbers in Bremen found the Piazzian star on Jan. 1, and thus confirmed our Bode's opinion—that it really was that planet, whose existence he had announced already 30 years ago. [The article went on to describe the planetary progression now known as Bode's law. *Berliner and Vossische Zeitung,* January 16, 1802.]

The first book to depict Ceres in a map of the Solar System was written by a physics professor at the École Polytechnique in Paris, Jean Henri Hassenfratz (1755–1827; 1803). The map was almost certainly drawn in early 1802, as the asteroid Pallas does not appear. Here, Ceres is denoted simply by the letter C (Figs. 6.4 and 6.5).

Fig. 6.4 Jean Henri Hassenfratz

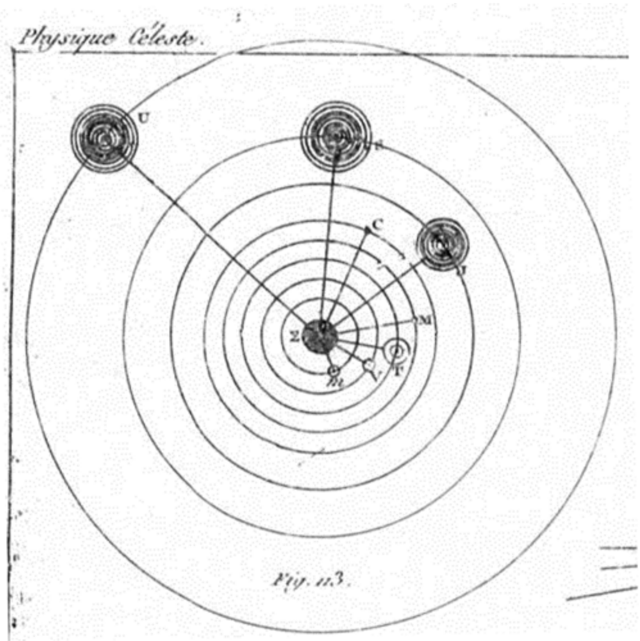


Fig. 6.5 The first map of the Solar System in a book that depicts Ceres. From Hassenfratz (1803)

Chapter 7
Piazzi's Monographs

The general community of European astronomers had to wait several months to get a first-hand account of the discovery of Ceres, one that minimized the role of Cacciatore. Piazzi's first memoir about Ceres was sent to Oriani on August 25, 1801, and he received it on September 18. On September 1, Piazzi mailed his memoir to other astronomers in Europe. It did not reach Zach until October 13. Lalande got his on October 25. Maskelyne received his copy on October 22, likely the same time Herschel received his. An English translation of Piazzi's memoir was written by Antonio Parachinatti, a teacher of the Italian language, and given to Maskelyne.

In Section 11 of his first monograph on Ceres, Piazzi states he sent notice of the discovery of Ceres to Oriani, Bode and Zach on January 24, 1801. But in Section 6 of his second monograph, he retracted that claim. "Realising that the new star was a planet, or a comet, but more probably a planet, I wrote to a few astronomers with whom I was in touch, among them Bode, who right away gave notice to Baron von Zach." While he states here that he wrote to a "few astronomers," there is no evidence he wrote to anyone other than Bode and Oriani on January 24. His statement that Bode wrote "right away" to Zach is also incorrect. Bode received Piazzi's discovery letter on March 20, but took more than 3 weeks to pen a letter to Zach on April 14. It is curious that Zach did not write directly to Piazzi until November 30, 1801, although he may have felt slighted that Piazzi did not notify him at once. Communication between them only began with a belated letter from Piazzi to Zach dated September 1, 1801. It seems reasonable to conclude that Zach was waiting for the discoverer to open a dialogue. Zach did not want to appear to be begging for information.

Results of the Observations of the new star discovered the 1st of January 1801 at the Royal Observatory of Palermo by Joseph Piazzi Director of the same, presented to the general deputation of the Academy.

Having been nine years labouring in verifying the position of the stars, which are collected in the Catalogues of Astronomers, on the evening of the 1st of January of the current year, together with several other stars, I sought for the 87th of the Catalogue of the zodiacal stars of Mr. la Caille. I then found that it was preceded by another, which, according to my

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2_7

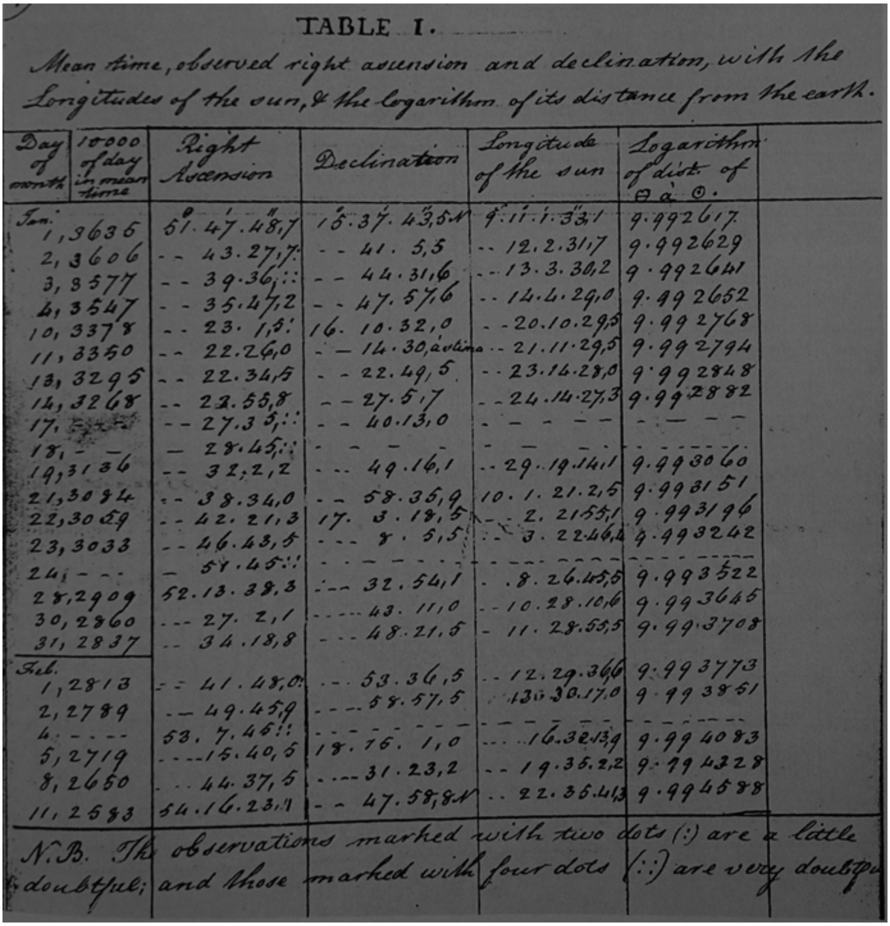


TABLE I.

Mean time, observed right ascension and declination, with the Longitudes of the sun, & the logarithm of its distance from the earth.

Day of month	10000 of day in mean time	Right Ascension	Declination	Longitude of the sun	Logarithm of dist. of ⊖ à ☉.
Jan. 1	3636	51°.47′.48″,7	15°.37′.43″,5 N	9s.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,àestima	-- 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	[illegible]	-- 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
Feb. 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. 7.46::	- - - - -	- - - -	- - - -
5	2719	----15.40,5	18.16. 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 dots (:) are a little doubtful; and those marked with four dots (::) are very doubtful

Fig. 7.1 The observed RA and Dec. of Ceres in early 1801

custom, I observed likewise, as it did not impede the principal observation. Its light was a little faint, and of the colour of Jupiter, but similar to many other which generally are reckoned of the eighth magnitude. Therefore I had no doubt of its being any other than a fixed star. In the evening of the 2nd I repeated my observations, and having found that it did not correspond either in time or in distance from the zenith with the former observation I began to entertain some doubts of its accuracy. I conceived afterwards a great suspicion that it might be a new star. The evening of the third, my suspicion was converted into certainty, being assured it was not a faint star. Nevertheless before I made it known, I waited till the evening of the 4th, when I had the satisfaction to see it had moved at the same rate as on the preceding days. From the fourth to the tenth the sky was cloudy. In the evening of the 10th it appeared to me in the Telescope, accompanied by four others, nearly of the same magnitude. In the uncertainty which was the new one, I observed them all, as exactly as possible, and having compared these observations with the others which I made in the evening of the 11th by its motion I easily distinguished my star from the others. Meanwhile, however, I greatly wished to see it out of the Meridian, to examine and to contemplate it more at leisure: But with all my labour, and that of my assistant D. Niccola Cacciatore and D. Niccola

Carioti belonging to this Royal Chapel both enjoying a sharp sight, and very expert in the knowledge of the heavens, neither with the night Telescope, nor with another achromatic one of 4 inches aperture, was it possible to distinguish it from many others among which it was moving. I was therefore obliged to content myself with viewing it on the meridian, and for the short time of two minutes, that is to say the time it employed in traversing the field of the Telescope; other observations, which were made at the same time, not permitting the instrument to be moved from its position. In the meantime, in order to render the observations more certain, while I was observing with the Circle, D. Niccola Carioti observed with the transit instrument. The sky was so hazy, and often cloudy, that the observations were interrupted till the 11th of February, when the star having approached so near the Sun, it was not possible to see it any longer at its passage over the meridian. I intended to search for it, out of it, by means of the Azimuths; but having fallen ill on the thirteenth of February, I was not able to make any further observations. These, however, which have been made, though they are not at the necessary distance from one another in order to assure us of the true course which the star describes in the heavens, are, notwithstanding, sufficient in my opinion, to make us know the nature of the same, as one may collect from the results, which I have deduced from them. The magnifying power of the Telescope of the circle is 50 times, and 80 that of the transit instrument, by which the new star was judged by D. Carioti of 7th or 8th magnitude.

Below is an English version of the tables of data on Ceres from Piazzi's first monograph (Figs. 7.1 and 7.2).

2

Combining in a parabola the two observations of the 1st and of the 19th January with the third of the 11th February which are very good, I find the following results [1Z = 30 degrees]

Perihelion	*4Z 5° 28′ 36″*
Long.	*2 19 43 0*
Inclination	*10 34 0*
Log. perihelion dist.	*0.3713077*
Passage through the perihelion July 3, 6985 [sic]	

These elements not according with the other observations, I attempted another parabola, by combining other observations, and I met with the same difficulties. Reflecting afterwards that in order to reconcile the two observations of the 1st of January and the 11th of February one ought to have supposed 0.26 for the difference between the radius vector, which corresponds to the first, and the other which corresponds to the second, I described mechanically my first parabola, and having referred to it the longitudes and latitudes thus calculated as observed it was easy to me to know that the motion of this star was not to be represented by a parabolic arc, such as the comets describe nearly.

3

From the parabolic hypothesis I passed then to the circular, and having made a few suppositions, I found two radii, 2.7067 and 2.6862; with each of which all the observations were represented a great deal better than in any parabola. The planets describing ellipses more or less eccentric, and not circles, it is to be believed that ours will not deviate from this rule. In an ellipse I should then have continued my calculations, but as the arc observed is very small, the results would be very uncertain, and the labour long and painful. I have therefore preferred the circle, especially as it appears to me that the elements given by the ellipsis are not more likely to determine the place of this star than those given by the circle.

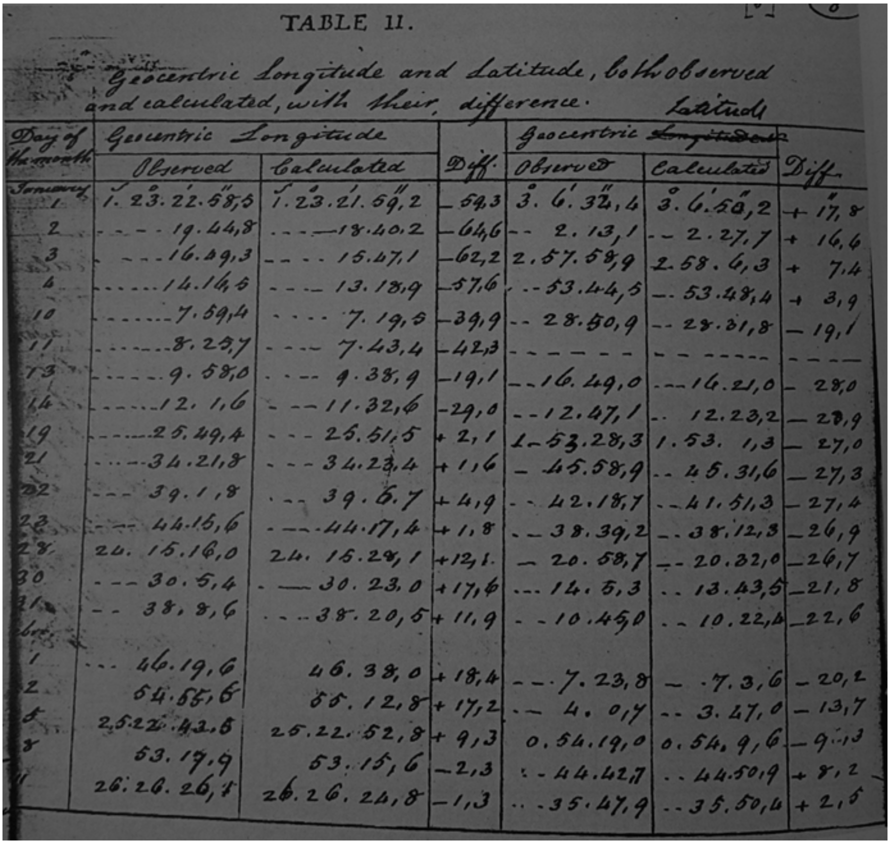


TABLE II.

Geocentric Longitude and Latitude, both observed and calculated, with their difference.

Day of the month	Geocentric Longitude Observed	Geocentric Longitude Calculated	Diff.	Geocentric Latitude Observed	Geocentric Latitude Calculated	Diff.
January	s ° ′ ″	s ° ′ ″		° ′ ″	° ′ ″	″
1	1. 23. 22. 58,5	1. 23. 21. 59,2	– 59,3	3. 6. 32,4	3. 6. 50,2	+ 17,8
2	19. 44,8	18. 40,2	– 64,6	2. 13,1	2. 27,7	+ 16,6
3	16. 49,3	15. 47,1	– 62,2	2. 57. 58,9	2. 58. 6,3	+ 7,4
4	14. 14,5	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. 49,0	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. 28,3	1. 53. 1,3	– 27,0
21	34. 21,8	34. 23,4	+ 1,6	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
Febre.						
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. 42,6	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

Fig 7.2 The geocentric longitude and latitude of Ceres in early 1801

4

The 10th of January the star from retrograde, as it was at first, became direct. In going from the observation of this day, I sought for its elongation for the time of the station. It was 4 h 4°; therefore one has for the mean radius of the orbit 2.9352; but the difference between this radius and the other which is deduced from the motion of the star between the 1st of January and the 11th of February, would indicate a great eccentricity; when on the contrary the progress of the observations seems to me rather to indicate that the eccentricity must be very small. However the diameter of the orbit deduced from the elongation at the time of the station can never be of great accuracy, and particularly in our present case.

5

In the first observations the star passing under the Horizontal wire of the Telescope remained almost entirely covered, and as the wire subtends at the eye an angle of about 6 seconds, I judged the diameter of the star to be a little greater, viz 7 seconds. During the last observation I would not form any judgment of the diameter, on account of the hazy state of the atmosphere.

ERRATA CORRIGE

HORA O.

232 Motus in declinatione...... +0, 2........ +1, 2
277 Motus in declinatione......—0,65........ —1,65
309 AR................ 14.50.34,5.... 14.50.40,5

HORA I.

1 AR................ 15. 1.59,7.... 15. 1.44,7
10 Declinatio............13. 8.59,9.... 3.16.59,9
180 AR................ 24.51.17,4.... 24.50.17,4

HORA II.

98 { AR.............. 34.51.13,8.... 34.51.43,8
Motus in AR. .... —0,115........ +0,25 }
176 AR................ 38.59.50,1.... 38.59.20,1
187 AR................ 39.44.48,0.... 39.44.18,0
258 Praec. in declin. .... 11,39........ 14,39

HORA III.

Notae 103 *adde* . Ex duobus meis opusculis, altero —*Risultati della nuova stella &c. Palermo* 1801, & altero — *Della scoperta del nuovo Pianeta* Cerere Ferdinandea ec. *Palermo* 1802 — abunde patet me prosequutum esse novum Planetam a die 1a Januarii 1801 ad diem 11am Februarii, quo intervallo eum vigesies observavi. Hinc mihi difficile intellectu est unde hauserit Cl. VOIRON me ter tantum, imo nec plene tertia vice, *Cererem* observasse. Descripta enim — *Histoire de l' Astronomie &c. Paris* 1810 — occasione, qua novus Planeta se mihi conspiciendum obtulerat, subdit pag. 70. — *Mais il en avoit fait à peine trois observations, dont la troisiéme étoit même incomplète, qu' il fut attaqué d' une maladie dangereuse, qui manqua lui faire perdre avec la vie son heureuse découverte* Morbus autem non lethalis, nec ante medium circiter Februarium.

Fig. 7.3 A Latin note from Piazzi's star catalog gives the titles of both of his monographs about Ceres

Results	
Radius	*2.6826*
Motion on the orbit from January 1st to February 11th	*9° 2′ 29″.7*
1801	*2Z 8 46′ 22″.0*
Motion in 100 days	*0 22 6′ 33″.7*
Longitude	*2 20 46′ 48″.0*
Inclination of the orbit	*10 51′ 12″.0*
Mean distance deduced from the time of the station	*2.9352*
Tropical revolution deduced from the mean distance by the law of Kepler	*5.03 years*
Sidereal revolution from the motion in the orbit	*1628.27 days*
Apparent diameter, at the earth's distance from the Sun 19″	
Bulk 1 1/3 that of the Earth	
Opposition 1802 about the first of March	

9

The agreement of the observed longitudes with the calculated ones in the circular hypothesis, its motion in the Zodiac, from which it only departs a little way in the greatest latitudes, and its position between Mars and Jupiter, leave no doubt that this new star is a true planet, and most probably the very same which in the year 1772 was deduced by the calculation of and announced by Sig. Bode, astronomer of the Academy of the Sciences of Berlin. The reason why it has not been observed before, though the Zodiac has been examined with great accuracy by the best astronomers, must be attributed, as I think, chiefly to the minuteness of the planet with regard to its distance from the Earth, and to its greatest latitudes. Nevertheless it is very likely that it has been seen either by the Abbe de la Caille, or by Tobias Mayer. In the catalogues of the zodiacal stars of these two astronomers, there are some observed only once, which I could never find, though I have sought them several times, and on different occasions. If the original observations of Mayer are preserved in Goettingen, and those of la Caille at Paris, it is possible that some light may be thrown by them on this matter. At the end of my work on the position of the fixed stars, the impression of which, by the munificence of our most gracious Sovereign is in great forwardness, I shall give a catalogue of lost stars, which will much facilitate this research. Such a discovery would be of the greatest value, and it would give us the means to find this new star again, without the fear of losing it any more.

10

It is the opinion of many persons, to which I greatly incline, that there are other planets similar to this: and as astronomers do not observe stars of a magnitude below the 7th or are contented to observe them once or twice at most, it could hardly be expected that such planets should ever be discovered. If I had not been in the habit of observing the stars four, five, six times, and even more, I should certainly not have discovered my present one. It might probably happen that I might go over again the observations of the 1st and the second of January, after a length of time, and finding again they did not agree, I should have sought the star in question in the same place of the sky, and, not finding it, which would have been the case, I should have placed it in the number of the doubtful ones; as I was necessitated to do with others, the observations of which could not be continued through the inclemency of the weather.

11

Messrs. Oriani, Bode and Zach, as soon as they saw the observations of the first and the 23rd of January, which I communicated to them on the 24th of the same month, pointing out in the meantime the circumstance that the tenth day the star from retrograde became direct, were instantly of opinion it was a new planet; and settled nearly the same elements of its orbit, as I have done; so after the 23rd the star began sensibly to diminish in size and brightness, uncertain whether it was to be attributed to its rapid receding from the earth, or rather to the state of the atmosphere, which became after that time still more dark and hazy, I began to doubt of its nature, so as even to believe it was a comet and not a planet. Nothing but the calculation of all my observations could clear up my doubts. But other pressing occupations, and particularly the very bad state of health in which I found myself, would not permit me to apply to it. In the month of April however, being a little better, I intended to submit my observations to calculation but I contracted another illness in placing the meridian of this metropolitan church, and reduced to a state still more serious than the preceding one, uncertain of the time when I could resume my studies, and solicited in the meantime by the above eminent astronomers to communicate to them all my observations, I sent them to Messrs. Lalande at Paris, to Oriani at Milan and to Bode at Berlin. Except for Mr. Bode, I have received no answer to this day. He, at the sight of my further observations, was confirmed in his first opinion, and only remains a little surprised, that in my first letter to Sig. Oriani I expressed myself inclined to believe that my star was a planet, and afterwards I should have regarded it as more likely to be a comet. Had I communicated *to him the circumstance of the diminution of the light, he would perhaps have conceived the same doubts.*

12

If one cannot attain to see this star again, there will always remain some doubts about its nature; and to have another sight of it, perhaps will not be an easy matter, on account of the uncertainty of the elements of the orbits which it describes, and more particularly of its great minuteness. Now it remains a good part of the 24 hours above our horizon, being in the sign of Cancer, but too distant for me to flatter myself with being able to distinguish it; not to mention that I am unprovided with instruments adapted to search for it out of the meridian. Towards the beginning of November, it will be more easy to find it; and particularly the first day of March, the time of its opposition, and the most opportune to observe it with advantage, and trace its progress. I therefore greatly hope it will not escape the research of astronomers.

13

Meanwhile, whether myself, or others should first happen to find it again, I shall be as much pleased as Halley, [Edmond Halley, 1656–1742, British Astronomer Royal], *Hevelius* [Johannes Hevelius, 1611–1687, Polish astronomer who made a catalogue of 1,564 stars], *Bode and Herschel, who introduced into the Heavens the glorious names of Charles the Second [King of England], of Sobiesky* [King of Poland, as John III], *of Frederick II* [King of Prussia], *of George the Third* [King of England]; *illustrious protectors of astronomy with equal right, and perhaps greater, I shall be able to engrave in eternal characters that of the August, and magnanimous founder of this observatory, Ferdinand our King, joined to that of the native divinity of this kingdom, which he now renders more happy by gladdening it with his presence. I have therefore informed the astronomers, my correspondents, that this start will by me be named Ceres Ferdinandea.*

From the most important of the fatherland of the Earth
The name will be derived
Immortality shone from the eye of Ceres
Among the Sicilians.
(Michael Angelus Monti Scol. Piarist)

14

This was not yet published, when I received a letter from the eminent astronomer of Milan, Sig. Oriani, dated the 25th of last July, in which he points out to me the results of his calculations, and sends me at the same time those of some other astronomers, who have done me the honour to work upon my observations. Sig. Oriani has calculated the orbit in a parabola, whose elements are

Long.	*2Z 21° 48′*
Inclination	*9 33′*
Perihelion	*4 10 14′*
Perihelion distance	*2.1045*
Time of the perihelium 1801 June 21	*07*
Sig.—[name not given] *has found likewise the orbit in a parabola*	
Long.	*2Z 20° 50′*
Inclination	*9 41*
Perihelion	*4 8 38′ 25″*
Perihelion distance	*2.21883*
Time of the perihelium 1801 June 30	*19° 1′*

The disagreement of the three parabolas, calculated by Sig. Oriani, by Sig. –, and by myself, neither of which represents all the observations, confirms it the more, that the motion of this star cannot be represented by a parabolic arc, as I have mentioned in my memoire. The same person, who had calculated the second parabola, has attempted to make the observations agree in a circle.

His elements are:

Radius	*2.74*
Epoch 1801	*2Z 8° 16′ 20″*
Long.	*2 20 15 0*
Inclination	*11 21*
Sidereal revolution	*4½ years*

In this circle the errors in excess amount to 2′ 30″ and as much in defect.

Sign. Burckhardt has calculated the orbit in an ellipsis

Long.	*2Z 20° 58′ 30″*
Inclination	*10 47 0″*
Aphelion	*2 8 59 37*
Time of the aphelion 1801 January 7	*1.3328*
Eccentricity	*0.0364*
Log. of the semimajor axis	*0.4106586*
Sidereal revolution	*4.13 years*

In this ellipsis the longitudes and latitudes of five observations are represented very well, as there is not a difference of more than a few seconds between the calculation and the observation. The astronomer, who has calculated the parabola and the circle, pointed out above, whose name I could not know by the two German leaves, which Sig. Oriani did me the favour to send me, doubts whether in the copy which I had sent of the observations there may not be some error; really in the first it was so; but afterwards I sent another corrected to Sig. De Lalande, as well as to Sig. Oriani, and Bode, and entirely agreeing with that upon which I have formed my calculation. [For an explanation of the missing name, see Monthly Correspondence, November 1801]. *However, in order to correspond as much as I am able with the interest, which has generally be shown for this discovery, whatever it may be, of mine, and not to leave any doubt in regard to the observations, I have lately examined them, and instead of some stars not so much to be depended on, which at first I had employed in the calculation, I have substituted others. I have taken into account the proper motion of them, and allowed for the deviation of the instrument, and have employed the greatest attention, which is practiced, when one desires the greatest precision. From this has resulted but very little differences in some of the right ascensions, which can but little if at all, influence the last results of the calculations; on which account, in the first reductions, I did not judge it essential to use a scrupulous accuracy. According to this last rigorous examination, from the first four right ascensions, one ought to take 1″.5; and one ought to add 1″.5 to those of the 10, 11, 14, 19, 21, 23, 28, 30, 31 of January, and 1st of February and to take 3″ from those of the 5 and 8 of February. With the transit instrument several times the observa-*

tion has been made at all the five wires, and then I have always preferred these to the others at the circle. I made use of those at the circle, when those of the transit were wanting, as in the first four days, and the 13th of January; when the observation has not been made at all the five wires (sometimes the observations being made with difficulty; and in the interval between clouds) I have taken a mean between the observation of the circle and the other of the transits; However, the difference between the transits and the circle has never been greater than 0″.2 in time, except on the 19th January, when I find 1″ in time more at the circle. As for what regards the declinations, I found no corrections to make. Nevertheless, if any one should desire the original observations, for his greater satisfaction, I shall do myself a pleasure to transmit them on the first notice. The same will be published in the 6th book of the astronomical specula, with the rest of my observations since the year 1794.

Palermo, 1801.

Piazzi's Second Monograph on Ceres

The goddess Ceres, in the Roman pantheon, has always been associated with agriculture (Spaeth, 1995). As such the sickle or scythe is an implement she often holds. Alternatively she is shown holding a cornucopia, symbolizing the bounties of agriculture. Sheafs of wheat are usually depicted with Ceres, and her hair is often adorned with stalks of corn (Fig. 7.3).

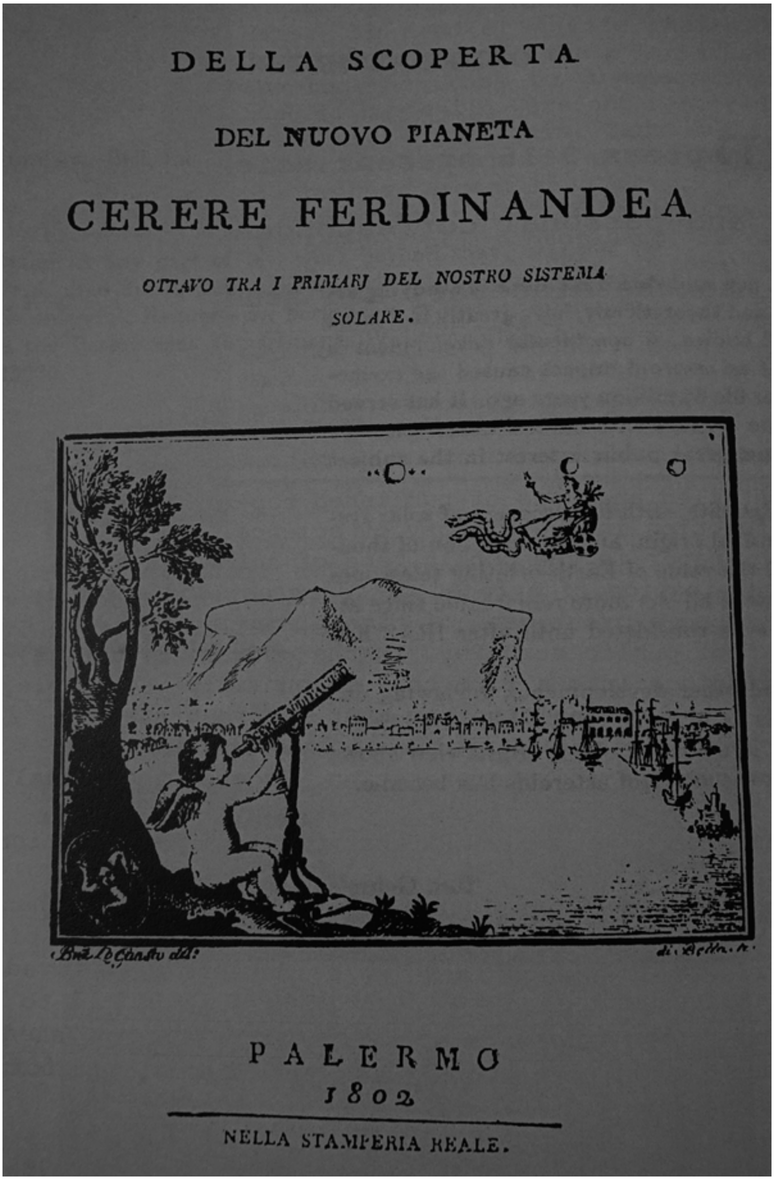
But there was another very different depiction of Ceres, developed from one of the most famous events of Greek mythology, where Ceres was known as Demeter. Her daughter Persephone was much desired by Pluto, who abducted the youngster and took her to be his queen in the Underworld. Ceres was much distraught by the disappearance of her beloved daughter, and decided to search for her. Thus Ceres is often depicted in a chariot riding through the sky to see where on earth Persephone was. Ceres was associated in Rome with the symbols of the Eleusinian Mysteries, notably snakes that are depicted pulling her chariot. As time went on the snakes were often replaced by dragons or lions.

The title page of Piazzi's second monograph shows a cherub looking at the goddess Ceres through a telescope. On the tube of the telescope is written "Ceres added to the sky." The goddess herself is shown in a chariot. In her right hand she appears to hold a sickle, while her left appears to be holding a sheaf of wheat (Fig. 7.4).

Her chariot is drawn by dragons, beasts that were said to have helped the goddess as she looked for her abducted daughter Persephone (Nalezyty, 2009). To the left of Ceres in the sky is the planet Jupiter with four satellites (two on each side). To the right of Ceres is the planet Mars, and directly above her head is another circle representing the celestial object Piazzi discovered. The meaning of this is clear, as Ceres was found to orbit the Sun between Mars and Jupiter (Fig. 7.5).

The view is across the harbor to Palermo, behind which rises Monte Pellegrino. Described by Goethe (1816) as "the most beautiful of all the promontories in the world—a large rocky mass, broader than it is high," Pellegrino rises to a height of 609 meters from the plain lying close to the sea north of Palermo.

Here is a cover letter Piazzi sent along with his second treatise:



DELLA SCOPERTA

DEL NUOVO PIANETA

CERERE FERDINANDEA

OTTAVO TRA I PRIMARJ DEL NOSTRO SISTEMA SOLARE.

PALERMO

1802

NELLA STAMPERIA REALE.

Fig. 7.4 The title page of Piazzi's second monograph on Ceres

I take the liberty of addressing to you six copies of the enclosed pamphlet upon the new Planet Ceres Ferdinandea, which I have published only for the satisfaction of the people of this place, and I beg you will be so good as to accept one for you, to present one to the Royal Society, to deliver one for each to Dr. Maskelyne, to Mr. Herschell, and to the Neopolitan Ambassador; and lastly to forward one through Mr. Young's means to Mr. Lofft of Troston

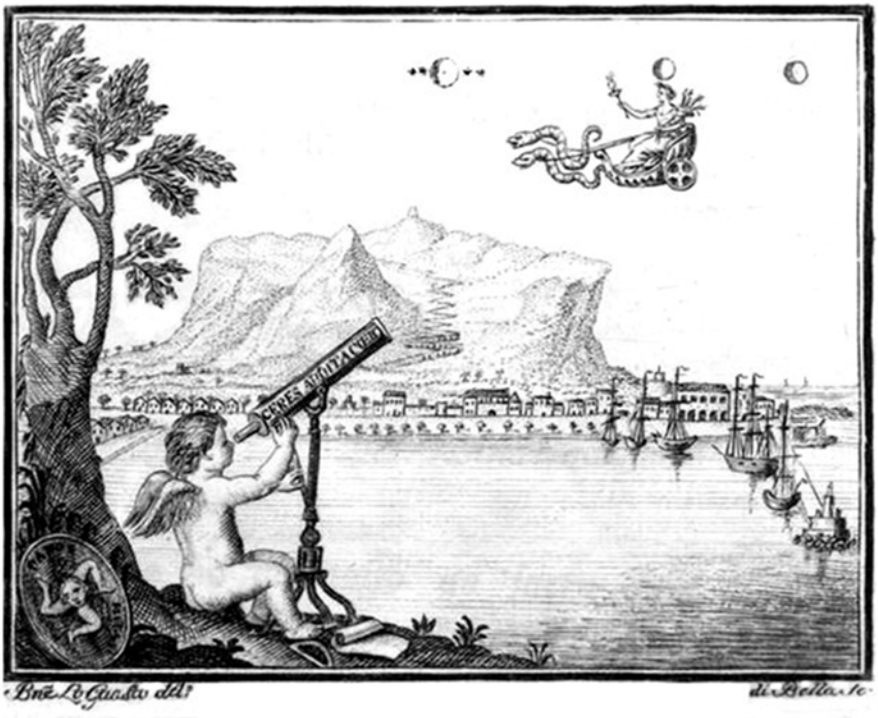


Fig. 7.5 Drawing on the title page of Piazzi's second monograph

in Suffolk, which I do at the particular request of my friend Mr. Balsamo [Paolo Balsamo, a teacher]. I hope your love and zeal for the advancement of the sciences, and your goodness for me and my former works will plead my excuse for troubling you on this occasion. (Piazzi to Banks, Palermo, July 7, 1802)

Of the Discovery of the New Planet Ceres Ferdinandea, eighth among the primaries of our solar system by Giuseppe Piazzi, 1802

Sua de coelo prospicit arva Ceres

(Ceres herself surveys ploughed fields from heaven)

Tibullus eleg.3.lib.3.

Your Majesty, because I fulfilled a sacred duty in giving the new planet the august name of Your Majesty, so I am returning to you what belongs to you as an indisputable right. The history of the discovery done in one of the observatories that was built under your royal favour and sponsored by your royal magnificence. I don't have anything else to offer from my part, but the pledge to continue the astronomical studies as much as my strength will allow and my most sincere expressions of indelible gratitude and reverence, with which I am signing of your Royal Majesty your very humble servant Giuseppe Piazzi CR

The discoveries give a new strong impulse to the spirit, usually exciting the curiosity, the emulation, the zeal, for this they are soon considered, discussed, developed and stated. This happened in the lucky combination of two lenses at the end of a tube, in the marvellous consequences that were the first results made in 1781 when Herschel showed to astronomy a new star in the sky. The same happened recently for a discovery made by me similar in all respects to the one done by Herschel. At the announcement of the new celestial Guest instruments and calculations were used to prove its existence. Due to the result achieved *first and in a pleasing way, the circumstances of this singular happening are highly interesting to astronomers and very beloved by Sicily where they belong. So I will describe very simply and truthfully everything that has been tried, thought and done about it here and everywhere, now and after.*

II

The first we can mention to have an idea about a planet between Mars and Jupiter was Keplers' thought as the father of modern astronomy. Being in the time of the Renaissance, he was overwhelmed by the fascination, common at that time, of the ancient philosophy made majestic by the names of Pythagorus and Ptolemy. He believed in the mysterious property of numbers: he thought that in the multiplicity of their relationship was the seed of human knowledge "so I looked in their order and structure in the sky" But being a great genius more worthy of the title of divine than Ptolemy, submersed by the most absurd extravagance of a celestial harmony dream and by a myriad of combinations he pointed out an emptiness between Mars and Jupiter that could only be explained through a dissonance and lack of harmony so distant from the Sun. This dissonance was not felt by him about other planets which combined in direct or inverse order to create a beautiful concert.

III

These fantasies of Keplers' compelled other astronomers to try to find some laws to rule the distances of the Planets from the Sun so that from the known one we could find the unknown planets. Therefore they found that if they supposed one planet between Mars and Jupiter the distances of each of them from the Sun would be equal to the sum of the distances of the first, and of the differences of the distances between the first and second multiplied by two, raised to the power equal to the number of the Planets, starting to count from the first minus two (1).

(1). Note—The respective distances of the Planets (given the Earth = 10) give, disregarding the fractions, the following numbers: 4:7:10:15:52:95 from each term of which subtracting the first will come this series: 0:3:6:11:48:91 increasing in double ratio from the second to the third, from the third to the fourth, from the fifth to the sixth, but more than quadruple from the fourth to the fifth. This irregularity was observed by Kepler and as a consequence there was no harmony between Jupiter and Mars, and they were not well set from the Sun. As a matter of fact by interposing the 23 between 11 and 48 we have 0:3:6:11:23:48:91. We have a progression very similar to the continuous geometrical doubled one, in which if we add to every member of it the distance of Mercury from the Sun we obtain the one of the other planet with only one term exceeding. Based on this progression another very similar but more correct one has been developed which is possible to find in the German translation of the Nature by Bonnet, which Bode used in the introduction to the second edition of his Study of the Sky reprinted in Hamburg in 1772, where as a conclusion he foresaw the existence of a planet between Mars and Jupiter. The same, later improved by others and especially by Wurm, has given the previously stated law that comes down to the simple formula $x = a + d\,2^{n-2}$ in which x is the distance of the Planet closest to the Sun, d is the difference between the two of them and n is the Planet's number counting from the first. According to this law, once the distances of the planets have been determined and compared to the observed values, only very slight differences are found. But as small as they are, considering the as other than a possible conjecture. We don't know the whole order and number of the planets, and it is not permissible to imagine the number and disposition of the other only from seven links of the chain as Dr. Seyffer very wisely stated. In astronomy, as a science, we have to rely only on facts. It is better to cling to them and not put together analogic conclusions, demonstrations and observations. These are the words of Dr. Wurm referring to the overestimated Dr. Seyffer. In 1781 the new planet Herschel discovered as well as his distance from the Sun was found to be in remarkable accord with the law which gained more probability and value. So the opinion about the existence of a planet between Mars and Jupiter prevailed. These opinions after all were already promoted and reinforced by Mr. Bode, astronomer at the Academy of Science in Berlin.

IV

It happened to these conjectures, what usually happens to all the others: in time they are proven and become important or are totally forgotten, depending on whether they are ingenious and imaginative or have a foundation in theory and observation. The astronomer Baron von Zach of His Highness Duke of Saxe-Gotha and Altenbourg examined these points very seriously and, being very talented and knowledgeable, realised the probability of the existence of a planet between Mars and Jupiter of which he dared to calculate and determine the probable characteristics (It is possible to see it in the Ephemeris of Berlin 1799 in which is published a letter of Dr. Zach to Mr. Bode with this news). These elements, adjusted by a fortunate divination, were already given in a sealed document by him to Count von Bruhl in London and to Mr. Bode in Berlin 16 years before. Convinced he was right and seeing also that the telescopes were perfect, the observations were many and expert and always studying, there was no hope for the discovery of the unseen planet; he believed it was too small to be seen without an extremely accurate examination of the Zodiac. He proposed then to form a Society of Astronomers appointed to search for it. The idea was explained, and the Society was formed, whose president was elected the famous Schroeter of Lilienthal; the perpetual secretary was Baron von Zach. 24 members were chosen amongst the best astronomers in Europe and to each of them was given half of the Zodiac sign of 6° latitude. Everyone was supposed to draw a map of the given part, examine it often, note any change if observed, and notify Zach.

V

While so much zeal was animating Europe and parts of Germany, I, far from the others and ignorant of the formation of the Society and of the honour given to me to be among the 24 astronomers, following only my very own method of study, without meaning it, I found the much wanted Planet. When in 1792 I started to study the stars that are in Mr. Wollaston's catalogue, I decided to study any other that would be in the area of my telescope and to note them [for a description of Wollaston's catalogue, see Appendix D]. *This method, always followed by me, caused me to see twice Herschel's planet. Therefore I owe to this method and to luck that I found the star that later was recognised as a planet. The particular circumstances of this discovery have already been told by me in my first memorandum printed on the matter.*

VI

Realising that the new star was a planet, or a comet, but more probably a planet, I wrote about it to a few astronomers with whom I was in touch, among them Bode, who right away gave notice to Baron von Zach. "At the time (he wrote me on November 30, 1801) I [Zach] *received the letter of Bode, the first expressing his ideas that the star discovered by you was a planet, Mr.* [Johann] *Pasquich* [1753–1829], *famous Professor of Mathematics in Pest, was by chance at my place and I showed to him my suppositions and comparing them in his presence, to your observations, I had the pleasure to see that they fitted with them. His satisfaction in that moment was not inferior to the one I experienced, after I doubted my first observations were wrong when I realised that they belonged to a moving planet, not to a fixed star. He was so pleased that he immediately wrote of the new discovery in an astronomical newspaper I had been publishing for 4 years, and sustained so strongly as if it were his own discovery." Therefore I am in debt to him for the amount of news I received, which he collected from everywhere and he published in his newspaper, from which I will use a great deal in what I am about to write.*

VII

After having made known the discovery and given to the astronomers all my observations from January 1st to February 11, 1801 (after that I could not follow the new star anymore for lack of instruments and due to a serious illness I had) we started to discuss them deeply to determine if they belonged to a comet or a planet. During the time of observations, I never found any circumstances that usually accompany comets; rather a decrease in light *from middle January on, a decrease that could not be said to be attributed to its departing from Earth or to a dense fog that was in the sky during a good portion of January and all February. Only the calculation of the observations could shed light on the question, but because the observed arc was only 10°, it seemed difficult to establish anything. Anyway, it was attempted this way. So a similar work was started by Dr. Burckhardt in Paris, by Dr. Olbers in Bremen and by me here. All three of us concluded that the observations could not be represented by a parabolic arc, as is usual for comets. It was calculated in ellipses by Burckhardt, in a circle by Olbers and by me, each convinced that it would have been unsuccessful in strict elliptic calculus, because of the small size of the observed arc. The elliptic elements of Dr. Burckhardt were represented satisfactorily only for a few observations; the circular elements of Dr. Olbers differed only for two or three minutes; mine were the closest, only in a few differing by one minute. Due to these discrepancies, some wanted to argue that the observations were too inexact and therefore they could not establish anything; others said that the discrepancies actually proved that the star was a comet.*

VIII

To confirm their opinion, the latter availed themselves of the example of the famous comet of 1770 and of the kind of paradox which is the inclination of the orbit which being about 11°, isolates the borders of the Zodiac always considered as the proper area of the Planet. We can't deny that the comet of 1770, which was moving in an ellipse and whose period was 5 years only, could be an exception to the general nature of comets which move in parabolic orbits. However, as Baron von Zach noticed, such an exception is so small that it could only be considered as 1–97. Regarding the inclination of the orbit, that in this new Planet is greater than in any other, we can't understand how this could influence the judgment about its nature. There is no reason to think than we should substitute the old Zodiac with a new bigger one, which with other discoveries could again have new borders. In the matter of facts from the theory of attraction, we don't have any arguments to prove that planets have to be restricted in a certain zone, without being able to be outside of it. Therefore, as Dr. Gauss thought, about whom we will talk, every paradox vanishes and the analogy of the planetary inclination, how many times their orbits come back to the Solar equator, which is the real plane they have to be referred, as Dr. Laplace did for the orbits of the Uranian satellites than the orbits of the Earth and of the new Planet will be the limits of our Solar System.

IX

It would have been easy to decide such a matter if we knew a characteristic by which planets can be differentiated from comets. But this is what we don't know. Our knowledge about comets is still imperfect: this is a part of modern astronomy that is sketchy. The same comet, with more or less observations, could probably be given different parabolas, and even more ellipses, because we have some examples. We know the identity of the orbit of only one comet; of the rest of them, which number 97, we can say nothing precise. They generally have a great eccentricity; the inclination varies in everyone without any law. This makes Laplace think that such heavenly bodies have been thrown at random into space. It is not far from the truth that most of them, after appearing once, don't ever come back again to show themselves in our Solar System, but they go into infinite space of which the Universe is full. It is certain that we could not subject some comets to calculations, except by supposing a hyperbolic orbit.

X

Being in doubt of a sure sign about how to recognise and define the nature of the new star, whatever its weight might be, we also had the authority of the illustrious names of Laplace, Maskelyne, Zach, Bode, and Oriani being generally convinced that it was a planet. If we did not go back and review the matter it would always have been unsolved. Therefore all the observers started to search for it and it is difficult to say how difficult it has been. Because of the favourable opinion of Dr. Burckhardt, everybody adopted his elliptic elements; and along with them were made the first searches. We were already at the month of December *and we had not discovered anything yet. With these elements it was searched for with no results in October, and in November by Zach, Bode, Maskelyne, Weiner and others. Already we thought we would never see it again, and it was put among the number of the comets; there were some who thought my observations visionary, and without a young geometer full of talents and modesty, who revived the almost lost hopes, perhaps every major research would have been abandoned by more than one astronomer. This is Dr. Gauss of Brunswick, who, having combined my observations with astonishing sagacity, after painful and very complicated calculations, which inspired a great confidence and gave a new high degree of probability to the opinion that the new star is a real planet whose orbit, according to Kepler's laws, lies between Mars and Jupiter.*

XI

The finding of this ellipse was important also because we were in a season not favourable for celestial observations, and it was very difficult to get many clear nights, as it was necessary to be able to study a wide part of the sky and to do repeated observations on each star in it: this kind of work was necessary either with the circular elements or with the elliptic ones of Burckhardt. The research had to be extended to the elements of Gauss, whose results differed from the others by 9° in longitude. For this reason, Baron von Zach, to whom they were first communicated, made it his duty to communicate them and recommend them to every astronomer through his Monthly Correspondence of December 1801. Although they were interesting, the arc of only 9° from which they were derived, compared to the one of 360° to which it had to extend, made the majority of astronomers doubtful of their certainty. They had been considered at first as a pure calculational effort and not as a legitimate result of observations. Dr. Gauss perceived the difficulty himself, he foresaw it, resolved it, and the facts justified its elements. The planet was found again, generally in every observatory of Europe, and the astronomers unanimously confessed that they could see the planet only using the elements of Gauss.

XII

The first to use these elements was Baron von Zach in his observatory at Seeberg near Gotha, and he was the first to again see the new planet. On December 7, 1801, he observed many small stars, some of which were not in any catalogue, not even among the 50,000 of Lalande. They were in the area in which the planet should have been and precisely in its parallel of latitude, which is the same according to every hypothesis. The weather had been bad for such a long time that it was not possible to make any verification before the 17th. That day, despite the not completely clear sky, he saw the passage of one of those stars, which gave him the suspicion it was the one he was looking for. From the 17th through the 31st of December he continued to be in that suspicion which became certainty on that day. Due to sudden cold weather the sky cleared and he was able to prove that the star seen the first time on December 7 was really the planet, the movement and position of which was in accordance with Gauss' ellipse. Because the December 31st observation didn't succeed very well, he was not in the position of deducing very well the right ascension which later he observed more precisely on the 11th of January. It was not possible to observe before this because of bad weather conditions.

XIII

On the December 7th observations, Baron von Zach estimated the little star, later recognised as the planet, to be of tenth magnitude. It should not be supposed then, that it was in vain searched for in October and November by the astronomers, because in those months it would have appeared even fainter. And I was very surprised when someone wrote to me from Paris that Mr. Messier was looking for it in June. The astronomers proved themselves very impatient to make sure of the new discovery, and this impatience observatory at Seeberg near Gotha, and he was the first to again almost caused them to jeopardise the cause they were sustaining. For sure, without Gauss' elements, and because the poor season was not permitting the study of an area of 8 or 10° for a few nights in succession, it would have been difficult to verify it, although it would have been later on. Thinking of the faint-

ness of the planet, and of the uncertainty in the circular elements calculated by me, I said in my Memoirs that it wouldn't be easy to see the new star again, but I was really hoping that after the first days of March it wouldn't evade the astronomical searches.

XIV

On January 1, 1802, the anniversary day of his discovery, Dr. Olbers in Bremen had the pleasure to find it again. Because he is an excellent comet researcher, he was tracing it using a predisposed path between β Leonis and ρ Virginis, plotting every star appearing in his telescope field. Therefore on January 2 he saw that one of them, not too far from the Virginis star, had changed its place. The night before it formed a triangle with the other two stars, one of which was #191 of Bode's catalogue and the other was of the Celestial History by Lalande; on the night of January 2 it came much closer to the two mentioned stars, forming a triangle very much obtuse. Using a circular micrometer he compared it with #191 of Bode, and he determined its ascent line and declination. On January 3, 4 and 5 the weather was cloudy; on the morning of the 6th at 5:30 am Dr. Olbers saw that the planet was farther apart from the last observed position, according to the theory he was asserting.

XV

After Zach and Olbers, the first to recognise it were Harding in Lilienthal and Bode in Berlin. Harding observed and examined it the night of January 11 with a seven feet telescope, that I will mention later. On the night of the 15, Bode, as he wrote me on January 26, saw a small star on the west side of the two stars ρ and 27 Virgo; he compared its position with Dr. Olbers' observation, and recognised, without any doubts, that it was the new planet; on the 23rd he observed a star near the two previous ones which he judged to be the planet. But only on the night of the 25th did he succeed in recognising and observing it well.

XVI

I don't know if anyone else observed it in January, but the named observers ensured enough observations. To Germany goes the honour to have first seen the new planet. And it really deserves it, because in no other country has it been worked on more, before or since. In Germany was born the idea of another planet between Mars and Jupiter, there it was promoted and sustained, there the first news of its discovery was accepted enthusiastically, there in the end were deduced the best elements from a few observations. It is strange how something that happened in the southern part of Europe and was debated and opposed, was then confirmed and sustained in the northern part. We can just say that somehow it was a repetition of what already happened concerning the earth's movement, whose theory was first born in Sicily, and then was completely developed in Germany. Those two facts will become even closer if the new star discovered in Bremen by Dr. Olbers turns out to be a real planet, as it seems. In section 10 of my Results of the Observations etc. I asserted that it was quite probable that the other planet similar to Ceres would exist, but they would have been discovered with difficulty, because astronomers generally were not studying stars fainter than 7th and 8th magnitude, and these only once or twice. This discovery, about which I just received notice (1), while these papers were printed, confirms my opinion and shows that in Germany they already started to study the small star, work from which we can expect rich fruits. (1) Note: On April 5 Baron von Zach wrote me that on March 28 a new star of 7th or 8th magnitude was discovered by Dr. Olbers in Bremen; the same was observed by him on April 4 in his Observatory of Seeberg. Dr. Olbers named it Pallas. "But what really is Pallas? (von Zach adds). Maybe a comet? The regularity of its movement, its appearance without nebulosity does not suggest it is a comet. A Planet? How large is its inclination? Could it be the Lexell comet of 1770? If its inclination is small, what would it be then? We don't know anything." In another letter of April 8 he is talking about it with lots of confidence, as a primary planet located between Ceres and Mars. To give satisfaction to the reader I will quote here the original letter.

"Seeberg April 8, 1802. Dr. Olbers' star, which I had the honour to announce to you, is in effect a primary Planet moving around the Sun in a very inclined orbit, with a period of revolution of 3 years and a medium distance of 1, 2 5, 20°. So it is between Mars and Ceres,

and there is no more doubt that it exists in the space occupied by several other planets of the same kind; and because you observed so many small stars, so it will not be impossible that you will find more Planets. It is to you, dear Confrere, that we owe all these discoveries—without your Ceres we would not have Pallas. Without Pallas we would not discover anything at all. What a new creation! I sent you my two observations of Pallas of April the 4th and 5th; the weather was covered on April the 6th, I saw the planet again on the 7th. These are all my observations of Pallas. These are the observations of Olbers. I am very much afraid and I regret even more that you will not see Pallas any more, because our letters take four weeks to arrive. I shall take care to send news through two channels." This letter, with the others of the 8th, arrived on the 17th of May. A few days later Oriani kindly informed me of the same discovery and sent me his observations of April 25, 26 and 27. With the observations of March 28 in Bremen, April 7 in Seeberg, April 27 in Milan. Right away I calculated the circular elements and I found the heliocentric longitude, for March 28 at the moment of the observation in Bremen, 183° 47′ 170° 12.′ Inclination 27° 1′ daytime movement on the ecliptic 1083″. With these dates I looked for the star three times at its passage at the meridian, but with no result; it is too close to the Sun to be able to see it with my instruments. If I had an equatorial sector, it probably would not elude me.

XVII

It has been seen in France not earlier than January 24th and Mr. Mechain [Pierre Méchain, 1744–1804, Director of Paris Observatory] *was the one who found it. This astronomer, who has discovered several comets, was looking for it for a long time, and because of that he observed more than 300 small stars in parts of the sky he thought was the right one. Then Messier, and Burckhardt and his nephew Lalande observed it with the mural quadrant of the Observatory in the field of Mars* [see Messier, 1803]. *Lalande, full of zeal of his uncle, who made him work so hard to progress in the study of astronomy, and to whom he probably owes the perfection he reached, calculated its position for several weeks, and sent his observations to the southern part of the Republic, where the sky is better and the observations could be made easier and longer. It seemed logical that in France, rich in astronomers and observatories and where even Messier, the finder of comets, resides, it should have been observed earlier. And probably it would have happened if the French astronomers had believed Gauss' elements, as Lalande himself confessed. I have candidly to confess that so great was the conformity of these elements with the observations that even in me, to whom they were of great interest, they generated a certain admiration not free from suspicion and fear. Note: In 23 Julian years there occur 18 oppositions of Ceres. So in the winter of 1779 it was at the right side of Venus, the same as this year. If the 1779 comet that was observed by Messier that passed in the same position would have done it two months earlier, probably it would have passed near Ceres, and it would have been discovered by Messier. Reflection by Olbers.*

XVIII

In England, and in particular at Greenwich Observatory, where the sky in that season was always covered by thick fog, it seemed it would never be seen. Nevertheless Dr. Maskelyne, who wrote me November 16th, and who searched for it in vain with Burckhardt's elements, having received the Gauss ones, he found it on February 4th. It was seen later by Dr. Herschel, who, having examined a large portion of the sky earlier, without discovering anything, believed that the star in question was something other than one of the many comets, of which the sky is full. About the observations of this astronomer, which must be very interesting, I haven't heard anything yet. The planet has been observed by William Meikleham [1771–1846; Regius Professor Astronomy at the University of Glasgow from 1799 to 1803] *in Glasgow on February 25.*

XIX

Our Italy, where, because of the beautiful sky, it should not have been difficult to see, was the last to observe it. The observers of Bologna, Padua, Florence, and Pisa are not very *active either because of lack of instruments or because those astronomers who are leading them (as in Florence), are more interested in theory; perhaps the cause was also the inclemency of the sky, which was the worst ever in the past winter. At Milan Observatory, of high reputation, it was not lacking either zeal or activity or valour. We all know though, that in that part of Lombardy during the winter it rains often and it is so foggy that often the astronomers can't observe anything for months. Nevertheless, having Oriani come back from Lyon, it didn't take long for him to see the planet again. He observed it on February 24. In April it was observed in Rome at the Observatory of the Roman College.*

XX

But it will be even more surprising that in Palermo, where it was discovered, and where there are better sky conditions, rarely without Sun for several days, it was not seen in the month of January up to the night of February 22 going into the 23rd. It should be noted, however, that because I didn't have either the equatorial circle, or the parallactic telescope, I couldn't search outside the meridian hoping to succeed. Back in November I tried using the azimuths and the distances from the zenith, but I know how difficult and insufficient was this method. Forced to wait for the star at the meridian, the passage of which was not out of the morning Sun before December 22nd, according to Burckhardt. Anyway I searched for it on the 23rd, 24th and 26th but with no results.

After that, even though the planet was passing at the meridian before dusk and I already had Gauss' elements, kindly sent to me by Baron von Zach, and received January 10, I couldn't use them before February 22. The reason was that, excluding two or three days in which the sky cleared at dusk and remained clear till midnight, for the rest of January and most of February the sky was always disturbed and cloudy with strong winds and rain, not according with my ten years' experience. I was so stunned at such a poor season that I was planning to postpone any other research till March, because I had to verify many stars of my catalogue and in the state of uncertainty I didn't want to lose all or part of a clear night that happened to come. Finally on February 22, after a strong rain, the sky cleared and the air was very clean and in spite of my previous determination, I didn't think of anything else, but set up to wait for my star at the meridian. Having already calculated its position according to Gauss' elements, I directed the telescope of the (Ramsden) circle to the ten minute to twelve and I had Beneficiale Carioti to direct the passage telescope ten minutes to the North. In this way the two telescopes were embraced together about the degree of polar distance, having in common 10 minutes so that if the planet was between those limits, as we did not doubt, it would appear in the field of one telescope if not the other. In the two instruments were observed the star, 15 minutes before and 15 minutes after the calculated passage. The evening of the 24th the sky was cloudy; on the 25th, because it was clear, we repeated the observations, and we found that one of those stars observed by the passage telescope, positioned between two of the catalogue stars of Lalande of the year VIII, changed position. I didn't doubt that this was the new planet, as I became certain on the night of the 26th. My observations will be reported at the end.

XXI

We can believe that it has been seen later on in different places, and generally in all the observatories, but this is not within my knowledge. I don't intend in any case to write about every observation done, I just mentioned the ones that could confirm the discovery and demonstrate how it was recognised, verified and made known to all Europe. I doing so I didn't mention the various peculiarities of this planet, noted by me. The first time I saw the planet it looked reddish and bright in colour; quite faint and whitish on Jan 2, 3, 4; and so on from the 10th to the 23rd I noticed a change in light and dimension, but not very strong. Later on it was more noticeable, and it increased very rapidly up to February 11, when I ceased observing it. I didn't pay attention to the appearance of the first night; I attributed the others to atmospheric conditions, but even more to the fact, and by me supposed, removed from Earth and so from the first notion it was a planet, I ceased considering it a comet. I have seen again similar changes also now that we know that it is for sure a planet.

On the evening of March 12th its light was so much increased, compared to the night before, that when I saw it very faint, that being between two stars of 7th and 8th magnitude, we could not tell which one was the planet. In fact I showed them to His Excellency the Sec. of State of Your Majesty and to the Knight [Andrei] *Italinski Plenipotentiary Minister of His Majesty the Emperor of Russia* [1743–1827], *who knows the science of astronomical observations, and they could not see any difference. The same happened the following night to the Prince Belmonte* [Prince Belmonte in the Province of Salerno, Italy, was a leading influence in the 19th century in the use of the carob as an ornamental and avenue tree and in the planting of thousands for reforestation of the slopes of the Appenines], *who was in fact observing them when Your Majesty arrived and wanted to see them and liked to stay in the observatory for quite a long time. Some days later, on the contrary, the planet was so faint that it could not be recognised in the passage instrument and therefore another star was observed instead of it. The Baron von Zach also observed the same phenomena. He says "the changes of light and magnitude are so strong and sudden that from one night to the next we can't differentiate it only by its appearance." Roughly the same thing has been observed by Dr. Maskelyne; he wrote me "Mr. Mechain was examining Ceres with an excellent achromatic telescope, and didn't notice any disc or a noticeable difference with a similar magnitude star. The same happened to me on February 3 with a telescope of 5 feet and an aperture of 4.1 inches; but on the 4th I saw it as a very well defined disc and so on the 23rd with the powers of 50 and 200 times." Dr. Olbers wrote similarly to Baron von Zach. At first we thought the variations in the appearance of the planet were due to atmospheric conditions; but it was recognised that this planet was better visible and much clearer with smaller magnifications, which is common for a comet; this was not a satisfactory explanation. Another explanation was proposed by the astronomer of Lilienthal, very familiar with delicate and difficult observations of the planets, like for instance the one of the spots and their different appearance.*

XXII

Mr. Schroeter, having examined the planet with Mr. Harding very diligently, established that it is wrapped in some kind of nebulosity, or atmosphere very similar to the one accompanying the comets. "I examined Ceres," he says in a note in the MC to Baron von Zach in March, "with a telescope of 13 feet and power of 136 and 288, and I saw it clearly, round without twinkling in such a way as the planets appear, so it did not allow any doubt about its nature. In light and in apparent diameter it was similar to Uranus, and we could see it very clearly, but surrounded by a very small zone, or circle, in the middle of which its disc was very definite; according to this viewpoint it had some similarity with the comet of 1799." On January 26 he examined it again and saw the definite disc, but the light was weaker. "On the evening of March 9, having applied a magnification of 130 to the telescope of Ramsden's circle, and taken off the lantern, I saw it bigger, of dark reddish colour, but not very clear, and the same I noticed the following night. I did not see anything that would make me suspect the existence of an atmosphere. I was only struck by the change in colour, which was pale ashen, being lightened by thread of the micrometer. The same evening I observed two small stars, located very close to it, which at a second observation were not in the same position, and which I didn't see anymore."

XXIII

However, I have candidly to confess that I don't see how we could explain the changes in light and magnitude by means of the atmosphere or nebulosity observed by Schroeter. If Ceres can be seen better with less strong telescopes, it is because I believe of the little light it reflects, which diminishes in proportion with the magnification. This does not happen with the other planets because they are so close to us or have a large diameter, and so they reflect more light. In comets there is the nebulosity, with which they are usually accompanied, which disperses the light rays, causing an effect similar to the one of absolute reduction. For all that, it seems that the reason of the above mentioned changes in the light are due to the condition of the atmosphere in which sometimes are vapors that we can realise in the small *and feebly illuminated object. Observing stars through the telescope it happened many times to me to see them more or less distinct in air conditions that seemed equally pure and clear. From April the 14th when I observed Ceres with Uranus, I always noted that when Ceres was clear and sharp, Uranus was too, and vice versa. The night of April 5th, the atmosphere was not foggy, but I saw Ceres quite dim compared to the night before. This surprised me till having Uranus in the telescope field, I saw it was dim too. The following night I saw better both Ceres and Uranus.*

XXIV

But, what will we think about this nebulosity or atmosphere? Does it really exist? After what we have reported, I don't think there is any doubt. If we have to consider the noted variations related to this atmosphere, it will be convenient to suppose it to be continuous with very strong agitations and disorders, or made of irregular different stratas of different density, twirling round the planet at very high speed. In this second case it would be a very thin and compact ring rather than an atmosphere. It is true that we have singular appearances and differences in the heavenly bodies of our Solar System. Some are isolated and lonely, others have satellites, others with bands, others without; finally we have some with bands, satellites and rings. Though we don't know anyone with a visible atmosphere, in some it is possible to see its effects.

Note: Mr. Schroeter observed the disk of the Moon and Venus and he calculated their magnitude—proof beyond any doubt of the existence of an atmosphere. This planet is certainly very singular, and it wouldn't be so strange if someone can't think of it as a comet, which once entered our Solar System, remained linked and kept by the action of other planets. If that is possible only analysis will decide. We abstain from hypothesis and conjectures because it is too far removed from pure astronomy. Let us wait for the observations to talk, let Ceres itself talk, which, as I strongly hope, will not wait too long to let us know something more precise by the great interpreter of the sky William Herschel.

XXV

The apparent diameter of this planet is another point at issue. As small as it is, and rarely well determined, it is very difficult to measure. In my first observations I judged it about 7 seconds, a size definitely too big. That valuation was based by me only on the observations of January 2, 3 and 4 when, having also another star nearby to observe, I had to put the planet under the horizontal thread at its first appearance in the telescope field. Therefore the aberrations to which lenses are subjected at the extremity, caused to make it appear to me much larger than what it really was. I can't in any case be convinced that it not so small as Mr. Herschel wants to believe; who gives estimates it is not even 600 leagues in diameter. On the 25th of January, Schroeter found 2″.69 with the atmosphere, and 1″.82 for the disc alone; the 28th of March 4″.03. I don't have any micrometer with which to attempt such delicate measurements with certainty; but anyway, by using the one on the horizontal line of the telescope, I am almost sure to have determined it with adequate certainty. From March 11 to 24 my estimation was always of about 4″, which reduces to the average distance of Earth and Sun would be 6″.9, or 1,140 leagues. I want to believe that Herschel won't neglect to measure it with his excellent micrometer.

XXVI

But what is the orbit of this planet? What are the correct elements? This section, after being sketched by Dr. Olbers, Dr. Burckhardt and myself, based on my first observation, was handled with much more depth and accuracy by Dr. Gauss, as I mentioned before. He gave the first elements, which justified the correctness of my observations; later he gave more precise ones which he corrected again for the third, fourth and fifth time, and all this before the planet was recovered. After he saw the observations of Baron von Zach of December 7 and January 16, he made a sixth correction. Finally, with further observations of Zach he made the seventh. So much work, so much zeal to get these elements, to more and more perfection; from one side it is a great honour to the author, but it also shows how difficult *and delicate the matter is. In another of my papers I already published the fifth elements so here I will mention only the sixth and seventh.*

XXVII

Sixth elements of Dr. Gauss	
Epoch: 1802 for Palermo	*155° 33′ 35″*
Tropic heliocentric daily movement	*770″.7376*
Tropic revolution	*1,681 days, 12 hours, 9 minutes*
Log of the semi–major axis	*0.4421189*
Aphelion	*326° 14′ 45″*
Node	*80° 58″.55*
Eccentricity	*0.08086253*
Max equation of the centre	*9° 16′ 23″*
Inclination	*10° 37′ 51″*

Seventh elements of Dr. Gauss	
Epoch: 1801 for Palermo	*77° 27′ 30″.9*
Tropic hdm	*769″.7924*
Log of semi-major axis	*0.4424742*
Eccentricity	*0.0814064*
Aphelion 1801	*325° 57′ 15″*
Node 1801	*80° 58.′40*
Max equation of the centre	*9° 20′ 8″.0*
Inclination	*10° 37′ 56″.6*

The new elements of Dr. Burckhardt	
Epoch: 1801 for Paris Meridian	*77° 19′ 17″*
Aphelion 1801	*326° 42′ 32″*
Annual movement of aphelion	*+2.5*
Declination	*10° 36.′52*
☊ *1801*	*81 5.35*
Annual movement of node	*Very small*
Semi–major axis	*2.76587*
Eccentricity	*0.0788723*
Tropic revolution	*1,679.84 days*
[ed: ☊ is longitude of ascending node]	

XXVIII

With these elements, which we might still improve with other observations to be done this year, it will always be easy to find the new planet in the sky. Accordingly, Gauss is going back with them to the time of Mayer's catalogue, done in 1756. At that time the error in longitude for the position of the planet was only about one degree and a few minutes in latitude. This is not to suggest we can make exact tables. This work is premature. We don't know the perturbations caused by the other planets which it has to endure but which can't be very strong, as Laplace notes: "Lalande wrote me that from a calculation done by Dr. Burckhardt their effect from January 1801 to December was about 30°. And *without this data first deduced from the calculation, then rectified by the observation, there is no hope in getting an exact orbit."*

Note: The calculation has already been done by Oriani, who was happy to communicate it to me. According to Lalande's theory, and assuming the planets' masses given in Exposition of the System of the world by Laplace (second edition) he calculated the perturbations caused by Saturn, Jupiter and Mars. The average motion will only be determined after many years of continuous observations. It may be possible to find this star among the number of Flamsteed's lost stars, as we found Uranus, but its smallness does not give us much hope. [John Flamsteed, 1646–1719, Britain's first Astronomer Royal]. *It will be easier to find it in those of Tobias Mayer, or of de la Caille, as I already pointed out in my first memoir. If this happens, the average movement will be quickly determined, otherwise we will continue to change the elements for quite a few years. It is more likely that this planet is in the observational collection of Lalande, as he thinks; and in the catalogue of Zodiacal stars of Baron von Zach, who observed many of them, which are not in other catalogues. This could in part diminish the work load.*

XXIX

I could collect much information about the planet from specific letters with which astronomers honoured me. If by means of the MC of Zach, about which I will say more with Dr. Gauss, the information had not been spread and excited general interest, the problem likely would have been handled with indifference and coolness. Very few would have taken the trouble to research the new planet, considering that the fathers of astronomy themselves were doubtful about its existence.

XXX

Being the first in the discovery of this new planet as it happened, I thought to have the full right to name it in the most convenient way to me, like something I own. Thankful to my master, thankful to the Sicilian nation, willing to maintain a certain coherence with the other planetary names, it looked right to me to name it Ceres Ferdinandea. The Baron von Zach, Dr. Maskelyne. Dr. Bode, Oriani and many others have already graciously accepted and approved such a name.

Note: Baron von Zach in his monthly mail of October wrote "Prof. Piazzi gave with the right he had the name Ceres Ferdinandea to the new star and having invited other astronomers to approach it, we for our part accepted with much pleasure such a name." Dr. Maskelyne in his letter of March 11, 1802 said "You had the right to name the planet, which you discovered, and you paid a due homage to your King, patron of the Arts and Sciences and founder of your Observatory. I will call, and it will be called in England Ceres Ferdinandea." Bode on January 26 wrote "I accept with much pleasure the name Ceres Ferdinandea. You discovered it in Taurus, and it has been found again in Virgo, the Ceres of ancient times. These two constellations are the symbol of Agriculture. The chance is very singular." I know that some people would like to call it Juno for its vicinity to Jupiter and perhaps because, since this asteroid is wrapped by a dense atmosphere, it well represents this Divinity, sometimes hidden by clouds according to Mythology.

(1) Note: The Prince of Saxe-Gotha, great Maecenas (benefactor) of astronomy, (similar to William IV, Landgraf of Hesse), an astronomer himself for the past 16 years, gave the name of Hera or Juno to the planet supposed between Mars and Jupiter, using the guessed elements calculated by Zach himself. However I will always use the name Ceres Ferdinandea, nor by giving it another name will I suffer to be reproached for ingratitude towards Sicily and its King, who with so much zeal, protects the sciences and arts, and without whose favour, perhaps we may never had arrived at this discovery. The Sciences can't thrive without great Maecenas, and reason demands that in turn the Maecenas receive praises by those whom cultivate them. It is not adulation, but tribute, right and fair homage.

XXXI

With the knowledge of the times and positions where Ceres was found, I think it is convenient that here I report the main observations on which I based this brief narration (Fig. 7.6).

1801	Temp. Medio	A.R. in arco ap.	Decl. Boreale ap.	
Dicem. 7	18.or 48'.10"	178.or 33'.31"	11.° 41'. $\frac{1}{2}$	Barone de Zach all'Osser- di Seeberg.
1802				
Genn. 2	11. 58. 36	185. 9	11. 7 . . .	
5	17. 30. 0	185. 43	11. 8 . . .	Dottor Olbers a Brema.
11	17. 3. 17	186. 45. 50	11. 15 . . .	
16	16. 46. 26	187. 27. 53	11. 26 . . .	Barone de Zach come sopra.
22	16. 25. 24	188. 6. 26	11. 40 . . .	
24	12. 58. 0	188. 15. 38	11. 52.20	
26	16. 10. 48	188. 24. 49	12. 0.43	M. Mechain a Parigi.
31	12. 4. 0	188. 38. 40	12. 23.25	
Febbr. 4	17. 25. 46	188. 42. 56	12. 44.45	
12	15. 4. 18	188. 30. 29	13. 33. 8	Dottor Maskelyne all'Osservatorio di Greenvich.
19	14. 34. 38	187. 44. 18	14. 20. 1	
24	11. 38. : :	187. 25. : :	14. 24.±	Sig. Oriani all'Osservatorio di Brera.
Marz. 10	11. 42. 43	185. 5. 54	16. 28. 31	
11	12. 9. 22	184. 53. 24	16. 34. 53	

Fig. 7.6 Main observations of Ceres Ferdinandea

XXXII

I will now report my observations. They are from February 22 to May 23, and not more than 45; the bad weather, which prevailed during these months, having not permitted me to make more. The apparent magnitude of the planet was at first similar to the star of 8th magnitude; it brightened then to the 7th and then it started to diminish to the 10th. On February 26 and 27 I judged it of 8th and on March 14th of the 7th magnitude; on April 26 of 8th and 9th; on May 10 of 9th; and May 16 of 9th and 10th. On May 23 the air was very clear, and I took off the light. On the 24th, 25th and 26th the sky was covered; on the 27th it could not be seen at all.

1. *The opposition of Mars was on the 17th, and shortly before it was at its minimum distance from Earth.*
2. *The maximum northern inclination happened on April 9th, after that it started to advance towards the equator.*
3. *The retrograde arc has lasted 108 days.*
4. *The stationary point happened on May 10th.*

Having compared some of these observations with the elements of Gauss, they differ only a few seconds either in longitude or latitude. [end of Piazzi's monograph]

In 1803 Seyffer published a monograph that reprinted Piazzi's memoir on the discovery of Ceres together with his own reflections about Ceres. This was printed in Chap. 5 of this book, and was at the time summarized for the public in the *Monthly Magazine* (1804):

> *This is a translation of the "Resultate der Beobachtungen des neuen Sterns, &"—and contains the history of the discovery of the Ceres Ferdinandea, together with the original Observations, as well as the Calculations of M. Piazzi. The additions made by M. Seyffer comprehend the labours of the other Astronomers, as well as Observations on the nature of this Star, concerning which there is some dispute, relative to its particular denomination, some contending that it is a Planet, while others assert that it is Comet, the arch [sic] of its orbit being too small to determine this question with any degree of certainty.*
>
> *M. Seyffer proposes to distinguish the new Star, by means of a sign analogous to the others, such as the Caduceus of Mercury, the Buckler and Lance of Mars, the Mirror of Venus, &. It is accordingly recommended to choose for Ceres, a flambeau* [a flaming torch], *the head of a poppy, or an ear of corn.*
>
> *Dr. Olbers of Bremen lately announced to the Royal Society of Sciences, that he saw, on the first day of January, 1802, precisely a year after its discovery, a Star which he took to be the Ceres Ferdinandea of M. Piazzi; that on the second, he distinguished its motion, and that on the morning of the sixth, he was perfectly assured that it was the new Planet. On the last of these days, he perceived it to have advanced below No. 20, of the Virgin, in conformity to the theory of its movements. It is his opinion, that M. Piazzi has made the apparent diameter two [sic] large.*

Piazzi also wrote about the asteroids in a book of *Elementary Lessons in Astronomy*. He did not break any new ground here, beginning with Kepler's postulate of a missing planet, then going on to Zach's Celestial Police of 1799, and a very brief review of the discovery of all four asteroids. He devotes the bulk of the section to speculation about the origin of the asteroids, mentioning the work of J. L. Lagrange (1812), which will be examined in a later volume of this series. (Piazzi, 1817, pp. 198–204).

Chapter 8
Bode's Book of 1802

In 1802 Johann Bode published a book about Ceres. The foreword is dated April 24, but on the final page 136 as May 17. The final pages of the book, 131–136, deal with Pallas and will be included in a later volume. Bode used extensive footnotes throughout his book. They appear here in square brackets that begin with an asterisk. Summary reviews of Bode' s book appeared in *Staats-und gelehrte Zeitung des Hamburgischen* (July 31, 1802, issue) and *Neue Allgemeine Deutsche Bibliothek* (1803) Vol. 78, 113–116 (Fig. 8.1).

The New Planet Between Mars and Jupiter

> *On March 20th, 1801 I received from Dr Giuseppe Piazzi, royal astronomer and director of the Royal Palermo Observatory [* On July 18th 1793 Mr Piazzi sent me by Mr Graf v. Lepel the first volume of his work: Della specola de Regi Studi di Palermo in folio which contains apart from his astronomical observations a description of the furnishing of the observatory in the royal palace in Palermo and the available astronomical instruments which are partly depicted as copper plate engravings. Moreover he possesses a five foot astronomical circle, the biggest instrument of its kind, made by the well-known English artisan Ramsden, which is described in detail by Piazzi who gives thorough information about its positioning and testing. (The second volume of the supplements to my astronomical yearbook provide a description and an illustration of this important instrument). Prince Caramanico, viceroy of Sicily, who died much too early regarding the sciences, made a major contribution to the perfection of this observatory.* [ed: Prince Tomaso d'Aquino Caramanico was viceroy from 1786–1795.] *In 1795 I received the second volume of Piazzi's work: Della specola astronomica which contains his further astronomical calculations.] a letter dated January 24th wherein the same reported the following: "On January 1st I discovered in Taurus a comet RA 51° 47′, with a northern declination 16° 8′ [* southwest in triangle with the Pleiades and the Hyades.] On the 11th it changed its previous (westerly) retrograde motion into a (easterly) direct motion, and on the 23rd its RA was 51° 46′ and northern declination 17° 8′. I will continue to observe it and hope to do so throughout February. It is very small and resembles an 8th magnitude star without*

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
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Johann Elert Bode,

Königl. Astronom, Mitglied der Akademieen und Gesellschaften der Wissenschaften zu Berlin, London, Petersburg, Stockholm, Göttingen, Utrecht rc.

von

dem neuen, zwischen Mars und Jupiter

entdeckten

achten Hauptplaneten

des Sonnensystems.

Wo nur Bahnen möglich waren, da rollen Weltkörper, und wo nur Wesen sich glücklich fühlen können, da wallen Wesen

Engels Philosoph für die Welt.

Mit einer Kupfertafel.

Berlin, 1802.

In der Himburgischen Buchhandlung.

Fig. 8.1 The title page of Bode's 1802 book about Ceres and Pallas *perceptible nebula. I am asking you to let me know whether it has been observed by other astronomers; in this case I could save myself calculating its orbit." At the beginning of March I had already found in foreign newspapers an announcement of this discovery; but little had been said about the position and orbit of this strange comet and only its appearance had been mentioned. But as I was reading the announcement by the observer himself it immediately attracted my attention and the thought arose that this little star, with its then easterly elongation from the Sun, during 22 days initially moving slowly retrograde, then seeming to stand still, and thereupon moving east again, without having a perceptible nebula, might not be a comet but that Piazzi had discovered something really exceptional. It was most likely the eighth main planet of the Solar System, being announced by me for 30 years, but yet undiscovered, which is to be found between Mars and Jupiter, and whose distance from the Sun states a well-known progression 2.80 and which has to orbit the Sun within 4 years and 8 months. [*In my Introduction to Knowledge of the Starry Heavens, 2. ed.,* 1772*, p. 462] I based my preliminary calculations without delay on this assumption and soon found out that these match exceptionally well the star's positions stated for January 21st and 23rd and its stand still on the 11th. Meanwhile I immediately wrote Mr Piazzi on March 11th and asked for his continued observations of this extraordinarily strange little star, moving in the zodiac so planet-like, and which had captured my whole attention by its appearance and orbit.*

On March 26th I showed Piazzi's letter to the Royal Academy of Sciences [ed: the Academy was established in Berlin in 1700] *during its last meeting before the Easter vacation, announcing that I planned to publicise my investigations on that matter after the vacation. On April 11th I gave a friend, travelling to Paris, a letter for Mr Mechain reporting Piazzi's discovery and my supposition. [*Later I found out that this letter for Mr Mechain by a strange coincidence had only reached him very late or possibly not at all.]*

On April 14th I also informed Oberstleutnant Freiherr von Zach in Gotha of the contents of Piazzi's letter and expressed my opinion on the discovery as follows: "By an easy well-known calculation I've found out that both observations from January 1st and 23rd and the standstill on January 11th match the supposition: this star was not a comet but the remaining unknown planet between Mars and Jupiter which distance has been assumed 2.75. What do you think about that?"

*On April 16th I gave a report and my opinion on the supposed comet discovered by Piazzi to the Academy on its first meeting after the holidays. Among other things I reported that my calculations of the right ascension and declination match Piazzi's extremely well and that these confirm my supposition. From the observation of January 1st the star's longitude 1Z 23° 29′ and latitude 2° 37′ can be derived whereas from the observation of January 23rd a longitude 1Z 23° 44′ and southern latitude 1° 28′. On January 1st its easterly elongation from the Sun was 132° 28′ and on the 23rd 110° 23′. On the basis of an assumed distance = 2.80 of the supposed planet between Mars and Jupiter, its heliocentric longitude on January 1st was 2Z 8° 46′ and the heliocentric southern latitude 1° 54′ 20″; on January 23rd 2Z 13° 18′ and 1° 21′ 0″. According to this it had moved within these 22 days heliocentrical 4° 32′ and must therefore complete its circuit within 4 years 9 months. [*According to Kepler's Law that the squares of the orbital periods of two planets are proportional to the cubes of their mean distance from the Sun, a planet with a distance of 2.80 … must complete its circuit within 4 yrs 8 months; the above calculation is only a rough one and it is not known whether the supposed planet is now at its mean or its greatest or shortest distance from the Sun.]*

Moreover it must have become exactly on January 11th at 1Z 23° 5′ of the geocentric longitude and 2° 10′ southern latitude…122° 14′ easterly elongation from the Sun stationary. The inclination of the orbit was 6° 45′. If Mr Piazzi would have stated the position of its standstill on January 11th or any other third position between January 1st and 23rd the matter could be generally easier determined. But I still hang on to my favourite idea it might be the aforementioned planet due to both observations of January 1st and 23rd and the standstill on the 11th, because of which Mr Piazzi tacitly passed a third observed position on to me; which is very remarkable and is no accident at all and therefore I am expecting his further observations with impatience. This, might it be whatever, strange *celestial body has been discovered in Taurus right on the evening of the first day of this new century, south of the Pleiades below the ecliptic (cp. fig.II).*

It moved only 24 minutes retrograde until January 11th and resumed its direct motion on the 11th with strongly declining southern latitude and was positioned on the 23rd nearly at the same longitude like on the 1st but 1° further north. It moved to its ☊ which must be situated within Gemini, where the ascending nodes of all other planets are. According to the supposed orbit of this planet I have calculated its position in Taurus for the present time; in accordance with this it should now be positioned roughly between Taurus' horns at a smaller northern latitude. Because nearly three months have passed since the first observations I searched for it there on several clear evenings of April and May with a 2 foot Dollond night-telescope and a 3.5 foot achromat in vain. This region of Taurus is so abundantly set with 8th magnitude stars that only the exact knowledge of its position made it possible to detect it among its neighbours and to recognise it by its motion. On May 12th I published the discovery of this strange star and my suppositions. On May 4th Freiherr von Zach replied to my letter of April 14th.

*He reported he had already been informed roughly about the Piazzian comet by Lalande but because he had written the comet would disperse in the rays of the Sun he had not pursued it further. From Mr Oriani in Milan however he had received the following more precise information. On January 24th Piazzi had reported to Mr Oriani (the letter arrived in Milan only after 71 days, on April 5th.) [*Mr Piazzi's letter to me with identical date reached me 15 days earlier. So I had been the first who had been informed by his amicable kindness about this extremely important discovery, thus the reasons for my claim are based on the original source, that he had observed in Taurus' shoulder an 8th or 9th magnitude star.] On January 2nd this star had moved northward 3′30″ and approximately 4′ towards Aries. On the 3rd and 4th he had found approximately the same motion. On January 5th, 6th, 7th, 8th and 9th it had been overcast. He saw the star again on January 10th, 11th, 12th, 13th, 14th, 17th, 18th, 19th, 21th, 22nd and 23rd. On the night of January 10th it switched from retrograde to direct. RA and declination on January 1st and 23rd like mentioned in the letters to me.*

*To this news Mr Oriani added according to Mr Zach's report the following: Mr Piazzi had reported that he had initially considered this star a comet; but because it constantly appeared without nebula and moved only very slowly, he had had good reason to think it might be a planet. [*It is absolutely incomprehensible to me why Mr Piazzi in his letter of the same date to me calls his discovered moving star a comet, and even in some following letters insists on this opinion, disregarding my objections, and nevertheless in his first letter to Mr Oriani favours its planet nature.] The Milanese astronomers could not find the comet, the sky had always been overcast. Also Mr Oriani believed, writes Mr Zach, the star was a planet with a distance 3.071. Mr Zach further mentions in this letter that he himself [* Probably caused by my former public announcements.] had made some investigations of this supposed planet in 1785 and had ventured all kinds of conjectures. He himself calls such things reverie and chimerical calculations (cp. astr. yearb. 1789, p. 162 and 163); he wrote the results of those to me during his former stay in Berlin. [*These were the following facts which have not become public until now: greatest distance 2.89385; mean 2.822956; eccentricity 0.14089; longitude of aph. 5Z 17° 54′; ☊ 3Z 27° 40′; inclination of orbit 1° 36′; sidereal period 4.741632 yr. It will show afterwards how good these match the orbital elements of the true orbit.]*

On May 19th I once more wrote Mr Piazzi informing him about my further investigations on the orbit of the presumed new planet, congratulated him on his extremely important discovery and asked him yet again for the labourious and resumed observations of the moving star. On May 24th Freiherr von Zach sent me the following orbital elements of the planet, calculated with a circle, which correspond to the three observations of January 1st, 10th and 23rd. Epochae Long. on January 1st 1801 2Z 6° 55′.

☊	*1Z. 25° 43′*
Incl. of orbit	*6° 14′*
Radius	*3.071*
Sidereal period	*5.382 yr*

*On January 1st hel. longitude 2Z. 7° 9′, latitude 1° 58′, on the 10th 2Z. 8° 49′ longitude, latitude 1° 44′. On the 23rd longitude 2Z. 11° 12′, latitude 1° 23′. The elongation at the position 7Z. 26° 41′ 41″. In the same letter Mr Zach expresses the idea whether the Piazzian star might not be the comet of 1770, whose orbital period has been calculated by Mr Lexell as is well known also as 5.5 yr. [*cp. Berlin. astronom. yearbook. 1781, p.21f] and which must, according to the labourious and profound investigations of this astronomer, suffer extreme perturbations in its orbit due to the powerful gravity of Jupiter, approaching it now and again, and therefore must have a variable orbit. How incomparably the former position of its orbit must have changed for 30 years into the one belonging to our new variable star. For instance of the ellipse which Mr Lexell states as the comet's orbit, the part observed in January and February from Earth and directed towards Taurus, where then the Piazzian star showed, lies within the Mars and Earth orbit; but all calculations show that the latter should be approximately twice as distant from the Sun as Mars is.*

On May 30th I received a second letter from Mr Piazzi, dated Palermo, April 10th; but it was not an answer to my former letter and contained only the following concerning the new star: "I have informed you in my letter dated January 24th about a comet in Taurus observed by me. I followed it until February 11th because I had been attacked by a dangerous illness of which I have not yet got rid of.

When I will have recovered completely I will calculate its orbital elements and inform you about them. Meanwhile I have sent Mr Lalande my observations." On exactly the same day I, too, informed Mr Lalande in Paris about my supposition of the Piazzian comet and the obtained provisional results of its observation.

*On June 11th in a third letter from Mr Piazzi, dated May 1st, in answer to my first letter, the long expected complete observations of the new star were given. He also writes: I had intended not to publicize my observations of the comet unless I would have checked and calculated these; but because my delicate state of health does not allow me to do astronomical calculations I do not want to withhold these observations from you any longer. The appearance of this comet also strikes me very much; but I think it wrong to declare it a planet. You will perhaps share my opinion after having gone through my observations. [*How strange is this remark of Mr Piazzi looking back at the fact shown in the footnote of page 9!] [Ed: He refers to the footnote beginning with "It is absolutely.."]*

Meanwhile I request you not to publicize your obtained results earlier than I do mine. The observations were the following and titled: Positions of the comet, Palermo, observed from January 1st until February 11th, 1801. [Ed: here follows a table of RA and DEC positions] (Fig. 8.2);

Actually these are the more reduced or improved observations sent to me in a later letter by Mr Piazzi, dated Jun. 30th. He acknowledges at the same time within this letter that his assistant had made some mistakes in the former calculations; he had been too weak to do anything. Now he wanted to start calculating his observations. If the star were a true planet he would like to suggest naming it Ceres Ferdinandea.

The comet, Mr Piazzi goes on, constantly appeared as an 8th magnitude star; invisible to the unaided eye. It had been observed on the meridian (probably with his excellent 5 foot circle), and actually discovered during the search for the star which Wollaston calls in his General Astronomical-Catalogue arranged in Zones of North Polar Distance (Lond. 1789) Fol. Zone 74. Mayer 87, but which cannot be found in the zodiacal catalogue of this famous astronomer (cp. Opera inedita. Vol.1. p. 52).

[No. 87 in Mayer's zodiacal catalogue is No. 34 in Aries. This fact led Mr Piazzi on the evening of January 1st to have a look himself; and he found this presumed 87th Mayerian*

Oerter des Kometen, zu Palermo vom 1. Januar bis 11. Februar 1801 beobachtet.

1801.	Gerade Aufsteigung in Zeit.			Abweichung. Nordl.		
	St.	M.	S.	Gr.	M.	S.
1 Jan.	3	27	11,25	15	37	43
2 —		26	53,85		41	5
3 —		26	38,4		44	32
4 —		26	23,15		47	58
10 —		25	32,1	16	10	32
11 —		25	29,73	—	—	—
13 —		25	30,30		22	49
14 —		25	31,72		27	6
17 —		—	—		40	13
18 —		25	55	—	—	—
19 —		26	8,15		49	16
21 —		26	34,27		58	35
22 —		26	49,42	17	3	18
23 —		27	6,90		8	5
28 —		28	54,55		32	54
30 —		29	48,14		43	11
31 —		30	17,25		48	21
1 Febr.		30	47,2		53	36
2 —		31	19,06		58	57
5 —		33	2,70	18	15	1
8 —		34	58,50		31	23
11 —		37	6,54		47	59

Fig. 8.2 A table of positional data on Ceres from Jan. 1 to Feb. 11, 1801

star and southwest near to it a smaller one, which is his comet or newly discovered moving star. After having received his above information I investigated the matter and soon found out that Wollaston mentions instead of de la Caille Mayer. Because in the original catalogue of 515 zodiacal stars of the former, in the sixth volume of Ephemerides des mouvemens celestes [the accents aigus are missing in my title! and in mouvement the t is missing], in 4th edition Paris 1763, this star really is listed under No. 87; therefore it is already mentioned in my smaller star maps, 1782, as number 243 in Taurus.]

Afterwards I informed Mr Piazzi about it. According to this, a slip in Wollaston's star catalogue had caused the important discovery of the Piazzian celestial body. Once more a proof that often great discoveries depend on a small coincidence. De la Caille's no. 87 is no. 33 Taurus in accordance with my big star catalogue.

On July 12th Freiherr von Zach wrote me among other things: "Last week I received letters from Lalande, Laplace, Delambre, Mechain, Fleurieu, Henry, Burckhardt, all of *them give me various important information; but no one speaks of the planet. What does this mean? Only Lalande and Mechain mention the Piazzian comet." The former writes on June 1st: "I have received a letter from Mr Piazzi containing the observations of his comet from January 1st until February 11th. Burckhardt is working on its orbital elements."*

Well, still comet not planet. What does it mean that Piazzi does not send you his observations? I neither hear anything from Oriani. But soon the whole matter will be settled because the observations of 40 days are at hand. On May 26th Mechain writes: "Have you seen the comet yet of which the journals report that it has been found by Piazzi in Palermo? No one has found it here. Since December 1799 our astronomers have not discovered a comet. I searched for it several times but without success."

On June 12th Mr Olbers from Bremen wrote me: I can't help congratulating you on your great contribution to the announcement of such an important discovery like the assumed new main planet.—On the assumption that both observations of the 1st and 23rd are made on the meridian and that the orbit is a perfect circle I have calculated the following elements: radius of orbit 2.951, helioc. longitude on January 1st 1801 2Z. 7° 45′, ☊ 2 Z. 21° 55′, inclination of orbit 7° 56′, orbital period 5.069 years. Of course you cannot find from two observations so close to each other, and only given in whole minutes (they were the ones reported on January 24th) the assumed orbit with complete certainty. The probably not inconsiderable eccentricity of the orbit makes all these elements questionable anyway.

Nevertheless it remains, as you remarked rightly that this moving star without nebula is a planet, and to be more precise even your always assumed planet between Mars and Jupiter. To two geocentric observations uncountable conic sections can be assigned but the fact that the Piazzian star came to a standstill on January 10th and 11th can be taken to a certain extent as a third observation and therefore excludes most of the conic sections and shows its distance from the Sun and that the new planet's orbit does not much deviate from a circle.

In a letter, dated June 24th, Mr Olbers gives the following more exact calculated orbital elements according to the first Piazzian observations: radius 2.94746; ☊ 2Z. 21° 55′; inclination 7° 54′; heliocentric longitude on January 1st 2Z. 7° 40; orbital period 5.0409 yr; daily heliocentric motion 11′ 44″.

*In July I gave once more a talk to the Academy on the continued studies and observations on the true orbit of this heavenly body, according to Piazzi's observations. I had already tried to determine preliminarily from the very first data that star's heliocentric longitude and latitude in a presumed circular orbit. But because Mr Piazzi stated in his letter the declination on the day of its discovery, the first day of January, 15° 38′ instead of 16° 8′, and thus smaller by half a degree, [*that this must have been a slip of the pen, could be seen from the regular progression of the right ascension.] the inclination of the orbit, which was according to the first observations 6°, therefore doubles and is almost 12°. This hitherto unknown great inclination of a planetary orbit, could almost wreck my long cherished opinion of Piazzi's star, if in this exceptional inclination could not be seen one more reason, why this planet had not been discovered earlier; because it often exceeds the boundaries of our old zodiac and usually planets are compared with fixed stars only near the ecliptic, it could have escaped even easier the notice of astronomers.*

Fig II (Fig. 8.3) *shows the apparent path of the new star in Taurus for 42 days, from Jan. 1st to Feb. 11th; during this period Mr Piazzi observed it twenty-two times. After having received Piazzi's letter, dated May 1st, I at once did a new calculation of the presumed circular orbit of his supposed comet, based on the three observations of January 1st, 23rd and February 11th. These gave me for true time* (Fig. 8.4)*:*

I assumed the planet's distance from the sun = 2.95; according to this and Kepler's Law, its orbital period must be 5.067 yr. This distance gave, the orbit regarded as circular (Fig. 8.5)*:*

According to this the orbital period is on average 4.83 yr, which is in accordance with Kepler's Law apart from two months. From the difference of the heliocentric longitude and

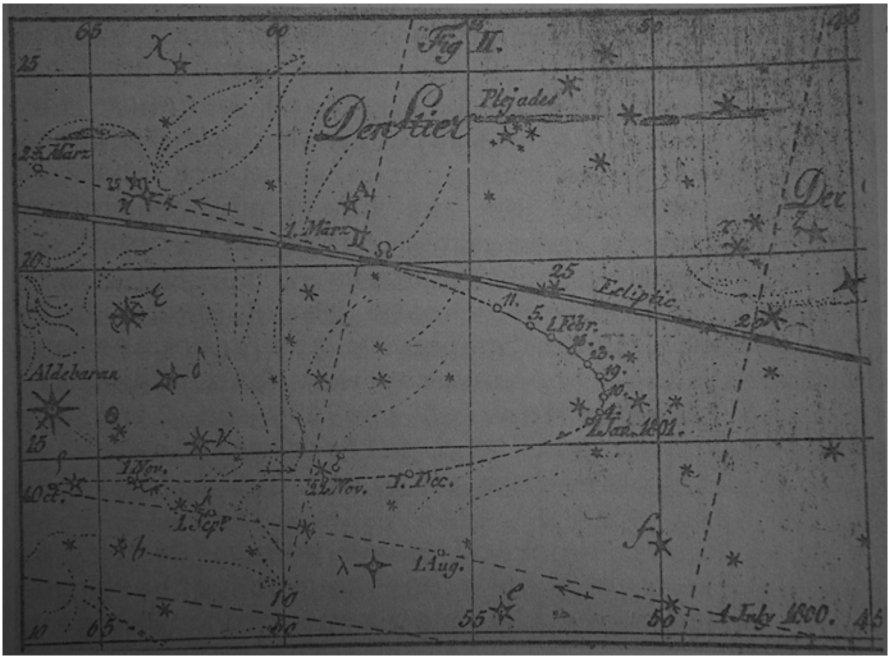


Fig. 8.3 The path of Ceres is the sky is traced from July 1800 to March 1801

		Geocentr. Länge	Geocent. Br. südl.	Elongat. östl.	☉	dist. ☉
	U. M	Z. G. M. S.	G. M. S.		Z. G. M. S.	
1 Jan	8 40	1 23 22 58	3 6 58	132 21 32	9 11 1 26	0,98315
23 —	7 4	1 23 44 12	1 39 7	110 22 2	10 3 22 10	0,98457
11 Febr	5 58	1 26 26 34	0 36 19	93 51 35	10 22 34 59	0,98762

Fig. 8.4 Positions of Ceres on three dates in early 1801. The last two columns refer to the Sun

	Heliocentr. Länge.	Breite. Südl.	Unterschied der helioc. Länge.	Hiernach im Mittel tägl. helioc. Beweg. 12′ 10″
	Z. G. M. S.	G. M. S.		
1 Jan.	2 7 38 22	2 19 17	In 22T. 4° 19′ 50″	
23 —	2 11 58 12	1 22 38		
11 Febr	2 15 57 15	0 33 25	In 19T. 3° 59′ 3″	

Fig. 8.5 Positions of Ceres based on Bode's orbital assumptions

latitude in the intervening periods I have provisionally found the inclination of the orbit 11° 56′ and the position of ☊ 2Z 18° 44′, where the star must have passed through on February 24th. For January 2nd, 19th and February 24th the following geoc. longitudes and latitudes have been calculated by Mr Soldner (Fig. 8.6)*:*

	M. Z. zu Palermo.	Geocent. Länge.	Breite Südl.	⊙	Log. dist. ⊙	Elong.
	U. M. S.	Z. G. M. S.	G. M. S.	Z. G. M. S.		
2 Jan.	8 39 4,12	1 23 19 44	3 2 24	9 12 2 27	9,9926215	131 17 17
19 —	7 81 28,07	1 23 25 59	1 53 38	9 29 18 52	9,9930605	114 7 7
5 Febr.	6 31 50,6	1 25 27 15	0 55 36	10 16 31 44	9,9940749	98 55 31

Fig. 8.6 Positions of Ceres calculated by Soldner

If I calculate these observations in a circular orbit too and use as distance 2.80 an orbital period is obtained that matches Kepler's Law extremely well, namely 4.64 yr.

A calculation based on the observations of January 1st and February 11th that I made using Prof. Kluegel's method to determine an orbit of a distant outer planet from two observations, which is described in my astronom. yearbook 1785, p. 195f, states the Piazzian star as the expected planet.

On January 23rd I received from Freiherr von Zach in Gotha, whom I had sent some of the recent Piazzian observations, a letter with the following content: I am sending you here the Novissima of Paris regarding the new planet. Strange that Mr Burckhardt does not send me all of the Piazzian observations from Jan 1st to February 11th; moreover, that Mr Piazzi does not write anything of the new planet to Paris, continuously calling it a comet. –

These Novissima from Paris are from Mr J. C. Burckhardt [A very talented observational and theoretical German astronomer living in Paris; he works for the Parisian committee of longitude and is member of several scientific academies. No one else of the honoured Parisian astronomers got involved in the calculation of the orbit of the Piazzian star, and only the famous Senator de Laplace deigned to focus his attention to it and agreed early with my opinion of it.] and contain his studies and calculations on the true orbit of the Piazzian star, which Mr Burckhardt is more inclined to call a comet. He writes to Mr Zach on June 6th: I am writing to inform you about what I found out concerning the Piazzian comet, however incomplete it may be. Mr Lalande received Piazzi's observations on the evening of May 31st; I immediately started to work on the calculation of its orbit. Two days later we received your letter with your and Oriani's calculations, which let us hope to find in this body a planet. My studies had shown us already that the described arc was not considerable; I therefore consider it a parabola. The slight geocentric and heliocentric motion of this comet made it very difficult for me to determine its orbit. First I had chosen the observations of January 14th, 21st and 28th and was forced by this circumstance to take the most distant observations, namely of January 1st, 21st and February 11th. Within these 42 days the comet has changed its geocentric longitude only by 3° and its heliocentric longitude only by 10.5°. When I wanted to improve the parabola found by my method, I found by Laplace's method that the equations of condition did not let me hope to do so. I then tried Mr Laplace's method of approximation with just as little success, which I may have foreseen because the unavoidable observational inaccuracies have a too big impact on the differences in the geocentric longitudes and latitudes. I then checked eight hypotheses by using Laplace's method without approaching the truth any further. I then calculated the following circle which corresponds to the three observations apart from min: radius of the circle 2.74, epoch 1801, 2Z 8° 16′ 20″, ☊ 2Z 20° 15′, inclination 11°, orbital period 4.5 yr.*

So diverse the attempts up to now have been they did not prove that there is no possible parabola for these observations. I decided to apply a method which had been successful on several occasions when all other interpolation methods had failed. If the equations of condition are such that the constant cannot be equated with 0 without giving both variable quantities at best probable values, you content yourself with only varying one variable as long as you have found a hypothesis where both errors are of equal size and opposite, which error is *then the smallest possible obtained with keeping the assumed invariable quantity. You now vary this last quantity and determine again by trial and error the value of the first quantity where both errors are equally large but opposite, the alteration of the absolute value of the smallest possible error in both cases shows which changes have to be made so that the value of the smallest error equals 0. For example, I assumed the arc of the distance from the sun of the Piazzian comet 0.438, the smallest error was 8′; I then put the arc of the distance 0.378, the smallest error was 4′; thus I saw that I had to reduce the distance further; after 20 hypotheses I found the following parabola: ☊ 2Z. 20° 50′; inclination of the orbit 9° 41′; position of perihelion 4Z. 8° 38′ 25″; least distance from the sun 2.21883; log. of the daily motion 9.4409408; date of passage through perihelion 1801, January 30th, 19 h. 1′.*

*[*From the date and the distance at perihelion and the mean daily motion I found for January 2nd: the true anomaly 61° 11′; Radius vector 2.994; and for February 5th anomaly 52° and R.v. 2.765. The presumed planet would have moved heliocentrically within these 42 days in its parabola 8° 25′ eastwards and approached its Perihelion which it must have reached, according to Mr Burckhardt, 146 days after February 5th, thus on the 31st day, on June 31st. But it is an unfortunate circumstance that this parabola does not show the observed latitude.]*

This parabola meets the three observed longitudes; but it is not possible to represent the three latitudes by it. The errors in longitude on January 14th and 28th are –1′47″ and +38″. I suppose I can affirm that there is no other parabola corresponding better to these observations. Piazzi did not write anything about the accuracy with which he had been able to observe this comet. For this and other reasons I asked Mr Piazzi for a reliable copy of his observations; then it will show whether something definite can be said of this strange celestial body which will nevertheless always remain very uncertain because the described arc is only 10°.

A later letter from Mr Burckhardt in Paris dated June 9th to Mr Baron von Zach reads: "[Ed: Bode does not close the quotation marks] *I am sending you the promised continuation of my studies on the Piazzian heavenly body. I have not spared the effort to search for an ellipse although the described arc is too small for any accuracy. But I hope this has a beneficial effect on the rediscovery of this heavenly body and makes it easier. ☊ 2Z. 20° 58′ 30″; inclination of the orbit 10° 47′; position of aphelion 2Z. 8° 59′ 37″; passage through aph.1801, January 1st: 1.3328; eccentricity 0.0364; log. of semi major axis 0.4106586 = dist. med. 2.5743; sidereal period 4.13 yr. This ellipse describes the longitudes and latitudes of five observations apart from a few seconds. One could easily obtain a greater accuracy which is however absolutely superfluous because the described arc is so small. To get an idea of the changes of parts belonging together, I have narrowed the position of the aphelion by 45 or set the true anomaly on January 1st =45. Thus the eccentricity is 0.0344 and the log. of the axis 0.41544; orbital period 4.20 yr. I made several attempts to reduce the position of the Aph. by 90 to 100 without success. This ellipse has given me the following position and I wish intensely that the friends and lovers of astronomy might seek to rediscover this heavenly body, although we in Paris try everything to find it. [*Namely in autumn when it returns from the Sun.]*

This matter is so important that it deserves the combined efforts of all astronomers. It would have been better anyway if Mr Piazzi had communicated his observations earlier; one would have found it more easily and could have observed it much longer (Fig. 8.7).

*[*An illness hindered the discoverer after February 11th from observing his presumed comet which culminated even then at dusk. Observing it more exactly in the western sky beyond the meridian was impossible because of the lack of necessary instruments; he mentions this in a later letter to me. Due to the increase in distance from earth the star grew in March and April ever smaller and was lower down in the evening sky. And without doubt these facts made it very difficult or even impossible for his assistant to find and observe it.]*

[ed: the end of Burckhardt's letter]

Oerter des Piazzischen Gestirns.

1801.			Geocentrische Länge.			Geocentr. Br. Nördl.	
	U.	M.	Z.	Gr.	M.	Gr.	M.
20 Jun.	13	4	3	11	45	3	26
17 Jul.	1	43	3	23	3	4	6
12 Aug.	10	54	4	4	21	4	51
7 Sept.	16	19	4	15	23	5	41
12 —	22	0	4	17	40	5	52
18 —	3	0	4	19	50	6	3
23 —	8	0	4	21	58	6	15
28 —	13	0	4	24	5	6	27
3 Oct.	17	41	4	26	9	6	40
8 —	22	0	4	28	12	6	53
14 —	3	0	5	0	12	7	8
19 —	7	0	5	2	11	7	22
24 —	11	0	5	4	8	7	37
29 —	14	45	5	6	3	7	53
3 Nov.	18	0	5	7	56	8	9
8 —	22	0	5	9	48	8	26

Fig. 8.7 A table of positions of the Piazzian heavenly body from June 20 to November 8, 1801

I then, too, made an attempt to describe the orbit of the new star as a parabola—consequently a comet's orbit—and tried to calculate it by using the three observations of January 2nd and 19th and February 5th and Lambert's construction method.

[Ed: in the German original of the following sentence are grammatical mistakes, so that it is quite difficult to understand which parts belong together.] But the success showed that this method in this case can only produce unreliable results due to the small southern latitudes and the slow motion within these 34 days; and therefore the lines are very close together, between which the corresponding parts of the chord of the described parabolic arc are situated, from the first to the second and from the second to the third observation in the past time.

On July 26th Mr Staatsrat Fuss from Petersburg wrote: "I am sending you my and the Academy's sincere thanks for giving us the Piazzian observations and your preliminary results. The arc described until now is of course still much too small to find out anything definite; but your calculation match the observations extremely well. I am in no doubt that further observations of the star, after its return from the sun, will crown your assumptions." On August 11th Mr Olbers from Bremen wrote: "I am hurrying to express my sincere thanks for the report on the Piazzian observations of his strange heavenly body and to present the result of my studies on the orbit to you. I was about to calculate an elliptical orbit for the body using Newton's and Euler's methods, which are based on some quite similar hypotheses on the position of the node and the inclination, with which I had been quite comfortable, when our common friend Mr Freiherr von Zach sent me all of the Piazzian observations from January 1st to February 11th, which had been sent to him by Oriani under the condition not to publicize them. From this number of observations I could easily see that there must be significant errors in the right ascensions of February and I therefore thought it to be an unrewarding effort to calculate the elliptical elements from such uncertain observations covering an arc this small; especially because Burckhardt seemed to have done the best possible.

"I therefore contented myself with trying to present three complete observations, i.e. three longitudes and three latitudes by a parabola. It is known that by a parabola only three longitudes and two latitudes or two longitudes and three latitudes of three observations can be represented if the heavenly body concerned does not really move in an orbit similar to a parabola. ***The result of my calculations was negative****. The observations of the Piazzian body do not correspond to any parabola. You can tell this from the immense difference between the following elements, which I am sending you as sample of my repeated calculations:*

Parabolic elements of the Piazzian heavenly body	*From three longitudes and two latitudes*	*From two longitudes and three latitudes*
Length of Ω	*2 19° 50′*	*2 21° 7′*
Inclination of the orbit	*10 38*	*9 48*
Longitude of perihelion	*3 25 24*	*4 10 6*
Distance of perihelion	*2.5351*	
Date of perihelion	*1801 June 8th 16 h.16′*	*1801 June 25th 7 h. 38′*

It might be nevertheless possible that a combination of different observations will lead to more tallying results. But you can state one thing: If Piazzi's observations are correct his discovered heavenly body does not move on a parabola but on an ellipse not much differing from a circle."

"Does Piazzi not name any reasons why he does not believe in your assumed planet nature of his celestial body but still goes on calling it a comet?"

"The great inclination of the orbit against the ecliptic which the new planet must certainly have, contradicts a similarity with the other planets. But I do not think this to be a reason to doubt its planet nature. We do not know by any means a physical cause why all planetary orbits have such a small inclination.–

"Because both comet orbits shown above are so utterly different and although every single one describing three observed longitudes of the Piazzian star so well, and Mr Burckhardt assures no other than his parabola can be found describing it so well, the reason must undoubtedly be that the observations known show only an arc of the true orbit that is too small and therefore insufficient to decide with certainty whether this celestial body really moves on a parabola and therefore is a comet; whereas the assumption it was a planet does not need a large number of observations to prove it right, as long as one accepts for the assumed and adjusted distance from the sun the mean motion and a circular orbit.

"Thus it can be derived with much more certainty from the 42 observations which Mr Piazzi gave us, it is the long suspected planet between Mars and Jupiter and not a comet passing by. The appearance of this celestial body as an 8th magnitude star, without nebula and tail, its orbit within the zodiac and the unaffected consent of the calculations and observations and the elongation during its standstill favour the first opinion very much."

It is very unfortunate that Mr Piazzi had been hindered by a serious illness to observe his star no longer than February 11th, because otherwise he in his beautiful region would have been able to observe it at least until the end of April and we would enjoy the knowledge of a bigger part of its orbit before its closeness to the sun. Due to this unfortunate incident he either had to entrust his assistant with the task of reducing and calculating the declination and right ascension of his observations or to postpone it until his recovery. This caused the calculation errors and one could not blame him for the slow corrections of the star's observed positions.

Mr Olbers has sent me the following elements of the Piazzian star calculated by him once again and based on a circular orbit and the observations of January 1st and February 11th: length of the ascending node 2Z. 20° 22′ 45″, inclination of the orbit 11° 3′ 36″, helioc. elongation from ☊ in the orbit of the first observation 11° 46 53′ 5″, radius of the *circle 2.730185, sidereal period 1647.75 days, daily heliocentric motion 13′ 6″.528. With this the intervening observations are:*

	Observed longitude Z. degrees m. s.			*Observed south. lat. degrees m. s.*			*calculated longitude m. s.*		*calcul. latitude m. s.*	
Jan 13th	1	23	10	37.6	2	16	59.7	46.7	–0	30.9
19th	1	23	25	59.2	2	53	38.2	17	–0	26.9
31st	1	24	38	7.3	1	10	54.6	56.1	–0	18.2

Mr Olbers believes that even the observations cannot be more precise. Thus he concludes:

1. *The Piazzian star actually moves on an ellipse not much different from a circle and is therefore indeed the suspected planet.*
2. *During the observations it must have been close to the line of apsides, even on an orbit with so little eccentricity otherwise the observations could not correspond to the hypothesis of a circle.*
3. *It cannot be said with absolute certainty from the observations which part of the line of apsides it came close in January and February—the aphelion or perihelion.*
4. *This uncertainty has a great impact on the positions of the star calculated in advance; one had to know whether its distance is increasing or decreasing. If you therefore take the positions calculated from a circular orbit as foundation, the true positions cannot differ much.*

*His geocentric longitude found is by 2° smaller than Mr. Burckhardt's, the latitude is the same apart from a few minutes; the planet could probably not be seen in the mornings before September and only very small. On January 1st it resembled an 8th magnitude star, its distance was 1.968; but on August 19th the distance will be 3.645 and on September 4th 3.536. At last I received a letter from Mr Piazzi dated August 1st: I believe you are right to consider my star a planet. The last observations (in February), where it lost so much of its brightness, misled me. [*Mr Piazzi believed that the star as a comet moved away from Earth fast.] I tried to describe its orbit by a parabola, but in vain. Its path observed until now is much too small to calculate an ellipse. An arc of a circle is the best and is sufficient to find it again after its return from the sun. Accordingly I calculated the following elements of its orbit: radius of the circle 2.6862; ☊ 2Z. 20° 46′ 22″; motion on its orbit from January 1st until February 11th 9° 2′ 29′.7; inclination 10° 51′ 12″; epoch 1801 2Z. 8° 46′ 22″; motion within 100 years 22° 6′ 33″.7; sidereal period 1628.27 days; diameter in the distance of earth from the sun 19″; size 1.33 earth diameter. [* If one assumes the double horizontal parallax of Sun = 17″, it follows that* $\frac{19}{17}$ *= 1.12 Earth diameter.] These elements are taken from my treatise which will be published soon and which I will send you via Vienna and Mr Triesnecker. My observations of this planet were made by a telescope magnifying 50 times and 3 inch aperture; I suppose its diameter 7″. [*Mr Piazzi did not have the possibility to measure the apparent diameter thus his estimated diameter and the deduced size of the planet led to a very uncertain result.] During the first days I tried to observe it with a night telescope and an achromat with a 4 inch aperture, but it was impossible. I am embracing you sincerely for having announced my new planet first. I would like to name it Ceres Ferdinandea. I do not believe it can be found again before November –.*

Nevertheless some months ago, before one could even think of rediscovering the new planet in the early hours, the choice of name had already been discussed among the astronomers. As early as May I wrote to Mr Zach: "I would like to suggest naming the new planet Juno because we should keep to mythology for conformity's sake: Juno was Saturn's daughter *and Jupiter's sister and wife. The planets in the Solar System situated above Jupiter carry the names of his ancestors and the ones closer to the sun of his wife and children." [*I mention this name in my yearbook of 1804, p. 258.] Mr Zach replied: "Because Uranus gives us the right to find for this planet a name from mythology in accordance with all other planets, His Highness Duke of Gotha proposed 15 years ago to call it initially Rhea and then later—more fitting and fortunate—Hera; the Romans named this goddess Juno but the Greek name Hera is to be preferred to the Latin Juno." I was very pleased to have had the same idea as His Highness, patron of astronomy and founder of the Urania-Temple near Gotha. Furthermore several astronomers and friends of astronomy have considered Juno the most fitting. [*Even Bonaparte said to Laplace he preferred Juno to Ceres for the new planet.]*

*It was quite understandable that other names were discussed as well. In one scientific journal an unknown suggests the name Vulcanus. He considers it a good idea to place the god who forged weapons next to the god of war, the husband of Venus next to her lover. Mr Prof. Reimarus from Hamburg says it must be called Cupido, because it is positioned downwards from Venus closest to Mars, Venus' lover. A known chemist [ed: Martin Klaproth] wants to christen the new planet Titan after his newly discovered and named metal, because he had given shortly after the discovery of Uranus the element discovered by him the name Uranium. In Paris one wants to call it after its discoverer Piazzi—like the name Herschel is kept for Uranus. [*Senator Laplace follows in his papers and in his Celestial Mechanics my suggested name Uranus.] Lalande will never agree to deprive the new planet of his pupil's name Piazzi and call it Ceres. But because Mr Piazzi had meanwhile suggested the name Ceres and promotes it in his letters to his friends and other correspondence the prerogative to name his planet will probably be granted. And this name is from a mythological point of view well chosen: [*By adding Ferdinandea Mr Piazzi wants to put up a memorial to his royal patron, the ruling King of Sicily and Naples, Ferdinand IV and founder of the Palermo Observatory.] and therefore all other astronomers will hopefully agree with him; [*Ceres is the patron goddess of the well-known fertile island of Sicily. Her Greek name is Demeter. She was, according to mythology, daughter to Kronos and Rhea, a symbol for mother nature, the fruit bearing earth and agriculture. The production of corn, tillage, threshing and separating the corn from the chaff is attributed to her.] shortly after Piazzi's mentioning they have already used it for the suspected eighth main planet, that means even before its rediscovery in the eastern sky. In his* Monthly Correspondence *of June-December 1801 Mr Freiherr von Zach retold elaborately its history of discovery before the rediscovery itself, accompanied by all hypotheses and calculations of various German astronomers (these have almost exclusively worked on it) on the nature and orbit of this celestial body as well as opinions, remarks and comments. From time to time he had also informed me kindly in letters about the news and calculations he had received. I have published in my astr. yearbook of 1804, p.249–259, an article and my opinion on this important discovery.*

On October 14th I received via Vienna and Mr Triesnecker Mr Piazzi's printed treatise on his important discovery, entitled: Risultati delle Osservationi della nuova Stella scoperta il di 1. Gennaro all'Osservatorio Reale di Palermo, da Giuseppe Piazzi Ch. Reg. etc, Palermo 1801, 25 pages octavo.

Mr Piazzi has been working for 9 years on corrections of the star catalogues. When he wanted to observe on the evening of January 1st 1801 the 87th star Taurus according to La Caille (which Wollaston wrongly calls 87 Mayer; see above), he noticed a small 8th magnitude star which followed it, 61″ in time and 15′ 19″ more southern, in the pale light and of the colour of Jupiter. But Mr Piazzi did not suspect anything of such a fortunate discovery.

On the evening of the 21st he found neither its culmination nor deviation corresponding to yesterday's observation; he now suspected that this might be a stranger. On the third evening he was convinced, that it was not a fixed star; and on the fourth he had the pleasure of noticing its steady motion. From the 4th to the 9th the sky was overcast but on the 10th he could observe the variable star again. Mr Piazzi tells that neither he nor his assistants, D Nic. Cacciatore and D Carioti were able to observe it with a comet searcher or an achromat with a 4 inch aperture or to distinguish it from its neighbours so he was unable to

1801.		M. Z.	Beobachtete geocentrische Länge.	Beobachtete geocent. Br. Südl	Unterschied von der berechneten. In der Länge.	Unterschied von der berechneten. In der Breite.
		U. M. S	Z. G. M. S.	G. M. S.	Sec.	Sec.
Jan.	1	8 43 20	1 23 22 58	3 6 32	— 59	+ 18
—	2	8 39 16	19 45	2 13	— 65	+ 17
—	3	8 35 5	16 49	2 57 59	— 62	+ 7
—	4	8 30 46	14 16	53 44	— 58	+ 4
—	10	8 6 26	7 59	28 51	— 40	— 19
—	11	8 2 24	8 26		— 42	
—	13	7 54 29	9 58	16 49	— 19	— 28
—	14	7 50 35	12 2	12 47	— 29	— 24
—	19	7 31 35	25 49	1 53 28	+ 2	— 27
—	21	7 24 6	34 22	45 59	+ 2	— 27
—	22	7 20 30	39 2	42 19	+ 5	— 27
—	23	7 16 45	44 16	38 39	+ 2	— 27
—	28	6 58 54	24 15 16	20 59	+ 12	— 27
—	30	6 51 50	30 5	14 5	+ 18	— 22
—	31	6 48 32	38 9	10 45	+ 12	— 23
Febr.	1	6 45 4	46 20	7 24	+ 18	— 20
—	2	6 41 37	54 56	4 1	+ 17	— 14
—	5	6 31 32	25 22 43	0 54 19	+ 9	— 9
—	8	6 21 36	53 18	44 43	— 2	+ 8
—	11	6 11 57	26 26 26	35 48	— 1	+ 2

Fig. 8.8 Calculated geocentric positions of Ceres from Jan. 1 to Feb. 11, 1801

observe it beyond the meridian. He thus had to be content with meridian observations. He observed the altitude of the star with his circle and Carioti its culmination with a meridian telescope. They continued their observations until the 11th. After that the star culminated at daybreak and became invisible. On February 13th Mr Piazzi was attacked by a serious illness which kept him from observing it further. He hoped his observations would reveal the nature of his star.

Because Piazzi initially considered it a comet he tried to describe its path through parabolas. But he did not succeed and tried a circular orbit and then an elliptical or planetary orbit. He preferred the circular orbit and found the elements as mentioned above. The following observations have been mentioned alike. Calculated from this the geocentric longitudes and latitudes are as follows (Fig. 8.8)*:*

Mr Piazzi says that the correspondence of its path with that calculated in a circular orbit, its motion in the zodiac which it leaves only at its greatest latitudes, its position between Mars and Jupiter—all this seems to leave no room for any doubt this is a planet or exactly that one which Mr Bode had already announced in 1772.

*[*In the second edition of my* Introduction to the Knowledge of the Starry Heavens, *published in 1772 in Hamburg, I hypothesize on page 162 the probable existence of more planets in the Solar System than known. Should the boundaries of the Solar System really be where we see Saturn?—(Since 1781 we know Uranus at twice the distance of Saturn.)… And why the gap between Mars and Jupiter where until now no main planet has been seen? Isn't it most probable that a planet has its orbit exactly there given by the Almighty? And a further comment on this special spot: the latter seems to follow from the admirable relation which the long-known six main planets have in their distance from the sun. If one assumes the distance of Saturn from the sun 100, Mercury's distance is 4; Venus $4+3=7$; earth $4+6=10$; Mars $4+12=16$. But now follows an interruption in this orderly progression. From Mars onwards follows a gap at $4+24=28$ wherein until now no planet has been found. Could one believe that the Creator has left this space empty? Definitely not! From this we come to a distance of Jupiter $4+48=52$ and Saturn $4+96=100$ (and then Uranus $4+192=196$)… This progression goes on in small numbers and therefore produces only*

			Mittlere Entfern.
Der ☿	387 Theile.		387
Die ♀	387 + 293 =	680	723
Die Erde	387 + 2.293 =	973	1000
Der ♂	387 + 4.293 =	1559	1524
Der zwischen ♂ und ♃ zu vermuthende Planet	387 + 8.293 =	2731	
Der ♃	387 + 16.293 =	5075	5203
Der ♄	387 + 32.293 =	9763	9541
Der ⛢	387 + 64.293 =	19139	19082

Fig. 8.9 The proportional table of planetary distances

preliminary results. I kept on talking about this progression throughout my astronomical papers, explained it through sketches and named more reasons for its confirmation. The discovery of Uranus was the first proof of it. This law of increasing distance of the planets from the sun can of course not be proven by mathematics, it is just empirical and has been derived through analogy but still it can be regarded as proof of the existing harmony in nature. I found the first idea of it in Bonnet's Contemplation of Nature, translated by Titius, 2nd edition, 1772, in an annotation of the translator on page 7. The first edition of Bonnet does not contain anything of it. It is remarkable that this progression had never been mentioned in astronomical works of foreigners. Only German astronomers mentioned it after I had drawn their attention to it through my astronomical papers. Even the progression in small numbers corresponds quite well to the observations. But if one follows Professor Wurm (cp. astron. yearb. 1790, p. 168) and assumes the true mean distance of Mercury from the Sun = 387 (the distance of Earth = 1000) and the difference in distance between Mercury and Venus 293, the proportional distances of the seven discovered planets correspond even better (Fig. 8.9). *Thus the distance from the sun is of mean distance*

Cp. my Terse Explanation of Astronomy, *2nd edition, 1793, part 1, p. 434.* end of footnote.*]*

*Because, Mr Piazzi continues, the new planet appears to be so small and often shows such a great latitude it had escaped the astronomers working on zodiacal stars; but it could also be possible that it is recorded in La Caille and Tob. Mayer. [*This would be an important discovery for the theory of this planet, as important as my find of Flamsteed's and Mayer's observations in the years 1690 and 1752 for Uranus' theory.] In the star catalogues of these two astronomers smaller stars can be found which were observed only once and Piazzi could not rediscover. He soon will publish a catalogue of these. [*The success will teach that most of these stars had never existed but were produced by writing mistakes, calculation errors or errata, like I have shown often in several volumes of my astronomical yearbook for other stars.] Should the astronomers not succeed in rediscovering it, doubts about its nature will remain. And he considers the rediscovery not an easy venture because its small size and the uncertainty of its position caused by the calculated elements will impede the rediscovery in some months very much. At the beginning of November it will be easier to observe it and in March 1802, when it will be in opposition with the Sun best to observe and hopefully the astronomers will not fail to see it.*

Until then (in autumn 1801) Mr Piazzi had been the only astronomer who had seen this planet before its conjunction with the Sun in January and February. To eliminate any doubt *about its existence, it was utterly important to observe it after its conjunction with the Sun which must happen in mid-July, in the early hours and the combined efforts of several, especially German astronomers, were directed at this aim. One wished to find the variable star as soon as possible because one had to fear that the calculated circular orbits and ellipses would state its apparent position increasingly uncertain the longer the rediscovery was delayed. On the other hand there was little hope to rediscover the planet because of its extreme smallness and faintness as long as it was more distant from earth than when Piazzi discovered it, who considers it an 8th magnitude only. The matter was difficult in every respect.*

I drew a map to large scale of the region of Cancer and Leo in which the planet is expected in autumn and entered on it all stars, even the smallest of my star catalogue, adding some smaller stars which I found in the first volume of Lalande's Histoire celeste. I then entered on the map several apparent orbits of the planet according to Mr Burckhardt's, Olbers', Zach's and Piazzi's calculations as well as my own. They all went through the northern part of Cancer and towards the neck of Leo θ and Denebola and ran quite parallel.

At the beginning of September I started to search for the planet eagerly in the morning hours in the eastern sky. If the changeable weather permitted, I normally started at 2 or 3 am, with a 2 foot Dollond and a very good 3.5 foot Dollond telescope (3.5 inch aperture and a magnification of 50 times) and examined these regions with great attention until dawn and continued until October and November. I also entered many small unrecorded stars on my map. The local weather—usually hazy, foggy and stormy—caused some interruptions, vain efforts and sleepless hours. Another time the bright moonlight was a hindrance. Until then it seemed I was on the right track. Normally I confused it with a small fixed star. I hoped to distinguish it from a fixed star by its brightness and greater diameter by using the 3.5 Dollond telescope at a higher magnification of at least 150, but my hopes were shattered.

*In October Mr Olbers wrote the following: "We must not give up hope on its rediscovery; Piazzi would never fabricate observations. And then it is proven: this star, observed in January and February, describing a nearly circular orbit with little eccentricity between Mars and Jupiter was a planet as you had suspected immediately. The planet exists and we will find it again. But can it be now visible at all so soon after its conjunction? Its distance from earth on January 1st 1801 was 1.92 and it appeared then as an 8th magnitude star; in September the distance was = 3.5; thus it had in September 3.5 times less brightness than an 8th magnitude star. [*Namely 3.152/1.922 = 3.32] and could therefore appear only as a 14th or 15th magnitude star. No comet searcher shows such a small star and even a 3.5 foot Dollond only when the sky is clear and under fortunate circumstances. Therefore it is not surprising all our efforts to rediscover the new planet in September were unsuccessful. Maybe we couldn't even have found it if we knew the exact position."*

In October Earth approaches the new planet only slowly, as you can see from my calculations according to my circular elements:

	Geocentric longitude			*Geocentric lat. north degrees*			*Distance from Earth*
October 3	*16*	*4*	*23*	*49*	*6*	*48*	*3.313*
8	*16*	*4*	*25*	*41*	*7*	*1*	*3.262*
13	*16*	*4*	*27*	*31*	*7*	*14*	*3.209*
18	*16*	*4*	*29*	*19*	*7*	*29*	*3.154*
23	*16*	*5*	*1*	*3*	*7*	*45*	*3.097*

In the next days you can hopefully try to rediscover it because it must now be visible before dawn. I have drawn a map for October depicting the apparent path of the new *planet according to my circular elements and Burckhardt's elliptical elements. A copy of this is inserted. It shows all the smaller stars mentioned by Lalande in his Histoire celeste Francaise (which he kindly gave me). The planet–like appearance of the new discovery will probably not be discovered by any telescope yet. With its greater distance from the sun, it would have if its surface reflects light like Mars, with the same apparent diameter about three times less brightness than Mars. At the same distance from earth that the planet had on January 1st, Mars appears as a 1st magnitude star but the new planet as an 8th magnitude star; or it has with equal distance 64 times less brightness; consequently its diameter and Mars' diameter will be in a ratio of 1: √ 64/3 or* $1:4\,{}^{1}\!/_{2}$. *Therefore its apparent diameter would at distance = 1 only be 2 seconds, i.e. now 2/3 seconds because its distance now is more than three times the earth's distance from the sun.—This is all merely speculation but certainly its apparent diameter must be extremely small in these months [* Time will show to what extent these astute remarks of Mr Olbers are correct.] Not until the beginning of March next year it will be—according to the circular elements—at **β** Leonis 17 ½° in opposition with the sun. Burckhardt's elements state the opposition later. I have not heard any news about the new planet yet. Even the latest Moniteur which arrived today, where normally all astronomical news is issued does not contain any, consequently it has neither been found in Paris.—In Gotha the vain search went on until mid–September according to reports from Mr von Ende in Celle."*

On October 18th Freiherr von Zach wrote the following: Thank you for giving me Piazzi's printed observations; I return it enclosed because Piazzi has sent me via Vienna his printed treatise. He too believes that the planet will be invisible before November. The latest Pariser Nachrichten *does not mention Ceres. Mechain writes he will immediately report any news; I promised him the same. With my excellent meridian telescope I hope to find it; meanwhile I am hunting for it when weather is permitting. I agree to the name Ceres. Piazzi has the right to christen his child and the name is quite appropriate. On October 24th I received another letter from Zach: I am very grateful for sending me Piazzi's treatise which I would like to return. I am sorry that my letter wherein I inform you that Piazzi has sent me his treatise too reached you too late. Otherwise you could have spared yourself the effort. But this shows your friendship. –*

*Piazzi's paper caused some thoughts and among others doubts about the given apparent diameter: an 8th magnitude star with a diameter 7″. [*This diameter was only very roughly estimated.] But nevertheless we owe Piazzi a debt of gratitude if he has really discovered a new planet even if it was only by fluke; the discovery was caused by his diligence and hard work.—Here are his positions for November according to Piazzi's circular elements: (a table of positions from November 1 to 25;)* (Fig. 8.10)

When Piazzi discovered his planet its distance from the sun was on January 1st = 1.924, on February 11th = 2.432. I calculated the brightness (claritas visa) assumed it on January

	Geocentr. Länge.				Geoc. Br. N.			Gerade Aufsteig.			Abweich. Nördl.			Culmination.		
	Z.	G.	M.	S.	G.	M.	S.	G.	M.	S.	G.	M.	S.	St.	M.	S.
1 Nov	5	5	3	8	8	2	8	159	57	45	17	7	50	19	54	28
7 —		7	1	59	8	22	20	161	59	21	16	42	50	19	38	59
13 —		8	55	9	8	43	44	163	55	49	16	18	26	19	23	13
19 —		10	42	38	9	6	23	165	46	49	15	57	59	19	7	4
25 —		12	23	28	9	30	22	167	31	43	15	40	58	18	50	33

Fig. 8.10 Five calculated positions of Ceres in Nov. 1801 based on Piazzi's elements

1st = 1, when it appeared as an 7th or 8th magnitude star, accordingly the brightness is now in the following ratio

1 Nov	*0.429*	*1 Dec*	*0.569*
7	*0.451*	*7*	*0.606*
13	*0.476*	*13*	*0.647*
19	*0.504*	*19*	*0.692*
25	*0.537*	*25*	*0.741*

On November 19th it appears only half as bright as on January 1st. On February 11th was the Claritas visa = 0.625 and Piazzi saw it for the last time. From the last days of November on it must be visible in dark nights, maybe earlier beyond the meridian; before the end of November it can hardly be seen with a meridian telescope. Regarding the nebula I.7. I am very interested; inform me if this is a lucky discovery, I wish it from the bottom of my heart."

*[*The 7th nebula of class I of Herschel's catalogue is one of the brightest. Mr Herschel observed it on January 23rd 1784, according to his catalogues at RA east 31° 41′ 15″ and 40′ south of 49 Leonis. It follows the longitude 6Z. 3° 20′ 23″ and latitude 11° 22′ 0″ north. Shortly afterwards he lost it and declared it a telescopic comet. I now think that our new planet was at that time in exactly the same region regarding the longitude. Only the latitude is not quite correct. I therefore suspected an error in the difference of the deviation from 49 Leonis and wrote to Mr. Herschel. I received an answer on October 27th that he was uncertain whether 49 or another star had been taken to determine the position of I.7., for that reason it might be wrong. The description and illustration showed that it was a nebula or a comet but not our new planet. In this same letter Mr Herschel goes on: I was very eager to search for the Piazzian planet, comet or variable star, in the region shown by your table. But I am convinced it is nowhere in the vicinity; and were it even 1000 times smaller than Mars I am sure it wouldn't have escaped me. I will continue my search and should I be lucky you will hear about it immediately. I would love to have your star catalogue and hope it will be available soon. (It was published October last year together with my star maps, 20 pages)]*

On November 14th I received another letter from Mr Olbers: Both of Piazzi's letters, which you communicated to me in extracts made my belief in the new planet become shaky.—How? the new planet had 7″ as apparent diameter and resembled only an 8th magnitude star. If Mars' apparent diameter = 7″ it appears as a 1st magnitude star and its surface reflects the sunlight only badly. The distance of the new planet from the sun is to Mars' distance in a ratio of 7:4 and then Ceres should have had in January three times less brightness than Mars with equal apparent diameter? It gets even more remarkable if you compare this alleged–apparent diameter of Ceres with that of Jupiter's satellites and Uranus and then again view the faintness of Piazzi's star. It could only be seen with a 3-inch aperture achromat; another 4-inch achromat (probably 2 or 2.25 inch is meant) was used in the first days in vain. In the first days of January the moon was still nearly full and no night telescope shows an 8th magnitude star then. But in his 3-inch achromat Ceres tolerated the illuminated threads and the moonlight. Piazzi himself observed it on January 23rd when the more than half illuminated moon passed through the meridian only 9′ before it and at most 6° more decl. So close to the moon I barely dare to observe an 8th magnitude star even with an excellent achromat.—If one assumes an apparent diameter = 7″ at the beginning of January, I calculate 15″ for its diameter in the distance of earth from the sun, and its true diameter $\frac{6}{7}$ *Earth diameter. In October November your weather probably did not permit either to search for the new planet. We had only one clear night on the 9th and I used it to record small stars, not mentioned by Lalande, that I found in the region of the expected planet. Until now a constant fog kept me from comparing my drawing with the sky and soon*

1801.		St.	Länge. Z.	G.	M.	Breite N. G.	M.	Abstand.
Dec.	1	16	5	12	37	10	11	2,5994
—	6	16	5	13	45	10	33	2,5331
—	11	16	5	14	46	10	57	2,4650
—	16	16	5	15	41	11	12	2,3989

Fig. 8.11 Four positions of Ceres in Dec. 1801, calculated by Olbers

		In Länge. Sec.	In Breite. Sec.			In Länge. Sec.	In Breite. Sec.
Jan.	1	+ 0,12	— 0,01	Jan	27	+ 0,04	— 5,85
—	2	— 7,28	— 1,12	—	28	+ 5,18	— 2,83
—	3	— 14,93	— 3,20	—	30	+ 5,25	— 2,27
—	4	— 1,47	— 4,44	—	31	+ 3,95	— 2,61
—	10	— 4,59	— 7,47				
—	13	— 18,83	— 10,92	Febr.	1	+ 9,07	+ 1,19
—	14	— 5,95	— 5,42	—	2	+ 7,19	+ 3,75
—	19	+ 1,17	— 4,01	—	5	+ 6,85	+ 3,98
—	21	+ 1,38	— 0,92	—	8	— 6,38	+ 4,20
—	22	+ 6,08	— 3,04	—	11	— 0,03	+ 0,00

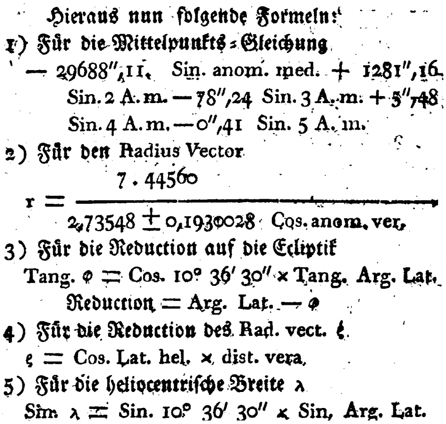
Fig. 8.12 A table of latitude and longitude differences from Jan. 1 to Feb. 11

the moonlight will be a hindrance. Here are some apparent positions of Ceres calculated from my circular elements for December with its distance from Earth: (table of positions for December 1, 6, 11 and 16;) (Fig. 8.11)

In December the planet is closer to Earth than it was on February 11th. As far as I know Lalande and his nephew did not see Ceres among the several thousands of smaller stars they observed and which are mentioned in the first volume of Histoire celeste francaise. It was always in other regions than eyed. On November 16th I received a letter from Freiherr von Zach with the following contents: As promised I continue to keep you informed of everything related to the new planet. This time I am sending you new elliptical orbital elements from Mr. Gauss which are so important because the given position of the star differs by 6 to 7° from any other circular elements and Burckhardt's ellipse. Now the region in question has to be extended further east. And because other astronomers might neglect it I am hurrying to inform you. Gauss' ellipse must inspire confidence when you see the correspondence with Piazzi's observations. The differences in the elements calculated from Gauss' ellipse are (Fig. 8.12)*:*

You have no choice but to call this ellipse perfect. These are the elements:

Epoch	*1801 2Z. 16° 28′ 14″.27 for Palermo time*
Aphelion	*11 0 33 20.00*
☊	*2 21 2 35.00*
Inclination of the orbit	*10° 36′ 30″*
Semi major axis	*2.73548*
Eccentricity	*0.07055535*
Sidereal period	*1642.5 days*
Log. of daily sidereal helio. motion	*2.8944569*



Hieraus nun folgende Formeln:

1) Für die Mittelpunkts-Gleichung

$-\ 29688'',11.$ Sin. anom. med. $+\ 1281'',16.$
Sin. 2 A. m. $-\ 78'',24$ Sin. 3 A. m. $+\ 5'',48$
Sin. 4 A. m. $-\ 0'',41$ Sin. 5 A. m.

2) Für den Radius Vector

$$r = \frac{7.44560}{2{,}73548 \pm 0{,}1930028 \text{ Cos. anom. ver.}}$$

3) Für die Reduction auf die Ecliptik

Tang. φ = Cos. 10° 36′ 30″ × Tang. Arg. Lat.
Reduction = Arg. Lat. $-\ \varphi$

4) Für die Reduction des Rad. vect. ϱ

ϱ = Cos. Lat. hel. × dist. vera.

5) Für die heliocentrische Breite λ

Sin. λ = Sin. 10° 36′ 30″ × Sin. Arg. Lat.

Fig. 8.13 Mathematical calculations on the orbit of Ceres

To calculate the planet's position in November and December, I calculated the data and formulas, which I am sending you for further use (Fig. 8.13)*:*

Equation centri maxima 8° 5′ 22″.8
Dist. Aphel. = 2.9284828
Dist. Perihel. = 2.5424772 log.0.4052571
Log. of daily mean tropical heliocentric motion
2.8945394 = 784″.403 log. of eccentricity in radius Earth = 9.2855635 = 0.1930028

Freiherr von Zach gave me the tables calculated accordingly for November and December. According to these I calculated the geocentric positions of the planet for these two months and drew its apparent orbit in on my map. This apparent orbit was further south and some positions were by 7° further east than all calculated by the circular or elliptical hypothesis of Burckhardt, v. Zach, Olbers, Piazzi and me.

During my search for the planet in the eastern part of the heavens in the early morning hours I followed from time to time its path described by Mr Gauss. At that time I was not aware—like many other especially foreign astronomers—of this young talented man's knowledge and skills in the field of advanced astronomical calculation. And despite the obvious reliability of his elliptical elements of the orbit (Burckhardt and Olbers had not dared to calculate these so exact from Piazzi's small observed arc) many an astronomer believed that the circular hypotheses could not be neglected because they corresponded fairly well to the discoverer's observations—as good as could be expected and seemed a good average.

*[*Even on January 1st this year Lalande gave his opinion on Gauss' ellipse in a letter to Freiherr von Zach: the exact correspondence between Piazzi's observations and Gauss' elements seems to me to prove nothing else than that a small Circulus osculator can be adjusted to any crooked line, if only the time interval is very small. The slightest error is sufficient to describe it this way or that.] One had to go through a sizeable region of the eastern part of Leo and the western part of Virgo to follow all these positions and the search became arduous.*

In addition there were special incidents, apart from the extremely bad weather in November and December last year, that disturbed my search and distracted me. From time to time I received letters from foreign lovers of astronomy who assured they had found the new planet here and there in Leo and who partly gave me their observed positions. So I was forced to have a look myself to be at least able to reply to their announcements and questions, which turned out neither for them nor for me satisfactory. Among others Mr v. Lindener *from Schweidnitz [Ed: now Swidnica, Poland] reported he had observed with a 2.5-foot Ramsden telescope on November 2nd between θl and b or actually between l and η in Leo a star together with three other small stars in a shifted triangle that seemed to have a steady and different coloured light which he considers a planet. He believed to have even noticed after some hours an inherent eastward motion. On the 3rd it had been completely overcast. On the morning of the 4th very misty; Saturn Jupiter and Venus appeared all the same with a tuft of rays and now the telescope showed in the region of the 2nd a body with similar appearance, from which Mr v. Lindener concludes this might have been the new planet or maybe a comet. A very eager lover of astronomy in Schmiedeberg, Mr Arndt, claims to have observed the new planet from November 1st to 14th in Leo with a 7-foot Herschel telescope; but I could not find out more details.]*

*Mr Professor Huth from Frankfurt an der Oder reports on December 5th the following: If I am not mistaken, I succeeded in rediscovering the Piazzian star during the night of the 2nd. It appeared in a 2.5-foot Ramsden telescope with a pale reddish light, completely round and enlarged. It was almost positioned on top of an isosceles triangle, whose base angles were the stars ς and o Leo], thus southwest of these two bright stars in Leo. [*Thus at a completely different position than expected for the Piazzian star, which must have then been already east and several degrees below Denebola in Leo.] In his following letters dated December 15th/21st and January 12th he informed me about his continued observations of this star that moved southwest backwards from 8 and o and that grew smaller and smaller. He observed it on December 3rd, 13th, 14th and 20th and on January 1st at its changing position regarding neighbouring small stars; it had moved meanwhile about 13 degrees. Very probably this heavenly body was a very distant comet, that may have been closer before December 3rd but receded slowly. On January 6th Mr Huth could not discover any trace of this star. Neither did I find it despite all searching with a 3.5-foot Dollond. [* In the astronomical yearbook of 1805 I will give further details of Mr von Lindener's and Mr Huth's observations.]*

On December 8th I received a letter from Mr Piazzi containing among other things the following: Did you find my star? I searched for it on November 2nd by means of azimuth— and zenith distances and during 24 hours I believed to have caught it; but on the 3rd that star was at right ascension 162° 47′ and 16° 19′ deviation north, like the previous evening. Since then rain, winds and clouds have kept me from further observations. None the less I am not easily deterred although I begin to doubt that my small star is related to the famous comet of 1770.

On December 10th Mr Olbers wrote: "The weather seems to have conspired to prevent the discovery of Ceres. Here at least it is constantly grey and only during the night of the 4th I was able to search for it though in vain. Both ellipses [He had already corrected the elements of the first above mentioned.] of Mr Gauss prove the planet nature of Piazzi's celestial body more and more; but, as you remark rightly, the search gets more and more difficult. To search for it with telescopes or achromats at such high magnifications it can be distinguished as a planet, might be very boring."*

The latest volume of commemorative writings of the Paris National Institute, 1801, says of the Piazzian star and its difficult rediscovery after its reappearance in the eastern sky among other things the following: "The variable star discovered by Mr Piazzi in Palermo one year ago has escaped until now all astronomers' inquiries. It seems to be a planet, which orbit requires an orbital period of 4.5 or 5 years. After mid-February 1801 it passed too early in the evening through the meridian to observe its culmination. Mr Piazzi and his assistants searched for it with the best telescopes and utmost effort beyond the meridian and in the evening sky in vain, because they had observed it quite long at the meridian. It is by no means a wonder that nine months later the efforts of all astronomers failed, because to the difficulty of the matter per se an uncertainty about the exact position, where to find the star, was added. For the orbital elements calculated from an arc too small can give the geocentric positions only approximately after such a long time. In addition to that the sky *had constantly been overcast, and to succeed one has to compile an exact catalogue of all 7th or 9th magnitude stars in whose vicinity this small star could be expected. The searches had to be repeated every day to see whether the small planet would reveal itself by its motion. It soon will be at the same position (regarding the sun and earth) where Piazzi had observed it; and if the sky brightens up we can hope to find it. Mr Messier had spent the night of October 3rd searching in vain and to console himself for the failed attempt he watched the beautiful spectacle that took place within the sign of Leo, in which Saturn, Jupiter, Venus and the moon were gathered round the bright star Regulus." –*

From the 18th until the last day of December we had constant dull weather accompanied by snow flurries and frost so that one could not even think of searching for Ceres. On New Year's night it suddenly cleared up and frost set in. I again eyed with my telescopes the region of Leo at midnight, where Mr Prof. Huth had seen his variable star, but in vain. On January 1st at 5 am in clear weather I once more searched through the region, where Ceres was expected, but without any success. The same happened on the night of the 5th. Later on constant dull weather set in again and snowstorms and frost took turns." [ed: here is an inverted comma, but the first one is missing] *Mr Olbers had the obliging kindness to tell me the following important news: [*I received this letter on January 12th.] "With the greatest pleasure I am telling you the pleasant news that I saw the long sought Ceres at last on January 1st for the first time, so exactly on the anniversary of its discovery, and on the 2nd I recognised it by its motion. This morning I could convince myself completely of the accuracy of my discovery. You probably have found this small planet yourself because it can be seen easily now. [*This was not the case.] I am sending you the particular circumstances of my discovery. After many grey days the night of January 1st had been clear. I searched with my excellent comet searcher I searched through the region between β Leonis and ς Virginis in which Ceres must be and entered all small stars not observed by Lalande on my map by visual judgment. On January 2nd it was clear again and to my delightful surprise I noticed that one of the recorded stars not far away from no. 20 Virgo and close above no. 191 of your catalogue had changed its position. At once I compared it at the micrometer with 191 and thus determined its right ascension 185° 9′, deviation north 11° 7′ at 11 h 58′ 36″ mean time Ceres was as bright as a 9th magnitude star. By my Dollond's magnification of 106 times I could not distinguish it from a fixed star. With impatience I was now expecting the next clear night. It has been overcast until 5 am this morning. I noticed Ceres had moved according to theory. It was now positioned below no. 208 of your catalogue. The observations at the micrometer were not all successful, from one I calculated for 5 h 30′ in the morning Ceres' right ascension 185° 43′, deviation north 11° 8′. Thus the existence of the eighth main planet is completely proved and your assumption has been crowned by the lucky success. I offer you my heartiest congratulations. The observed position of Ceres corresponded fairly well to the first ellipse of Mr Gauss."*

On the same day I conveyed my congratulations to Mr Olbers on this important discovery in a letter and reported it to Mr Piazzi in Palermo. On the 14th I made it known to the Academy and publicized it by the Berlin newspapers as well as by writing to foreign academies. I was now expecting fair weather with longing especially since the moon was increasing in brightness. On January 14th the moon shone in slightly hazy air and prevented to observe Ceres. On the 15th the air was clearer with 15 degrees Reaumur [−19C or −2F] of severe frost and despite the bright moonlight I found with the 3.5-foot Dollond on the observatory at 11 o′clock two small stars side by side forming together with star ς and 27 Virginis westwards a long almost right-angled triangle of which the western and brightest seemed to me to be Ceres. Now that it started to thaw hazy weather set in. On the 19th I informed Mr Olbers about my observation of the 15th; and on the 21st I received a letter from him, dated January 16th, by which he sent me his observation of the 15th from which it became evident that I really had discovered Ceres on that day. At last we had on the night of the 23rd to the 24th fair weather. And I now found eastwards in a triangle with ς and 27 Virginis the star which had been on the 15th west of these, and was now missing

	M. Z.	Scheinb. gerade Aufsteigung.	Abweichung nordl.
	U. M. S.	Gr. M. S.	G. M. S.
Januar 2	11 58 36	185 7 40	11 6 30
— 5	17 30 0	185 43 7 dub.	11 7 57 dub.
— 10	12 25 41	186 34 52	11 13 10
— 13	11 53 38	187 1 56	11 18 56
— 14	11 9 3	187 10 11	11 20 57
— 15	12 8 9	187 18 27	11 23 20

Fig. 8.14 A table of observed positions of Ceres from Jan. 2 to 15, 1802, by Olbers

there, which I at once recognised as Ceres; but the moon was more than half illuminated in Virgo and prevented the observation of this faint planet at the circular micrometer of the 3.5-foot Dollond and to determine its position precisely.

In his letter, dated January 15th, Mr Olbers reported: "I continue to keep you informed about our luckily rediscovered Ceres, all the more so, because I seem to be the first who rediscovered it at least in our northern Germany. Here are all my observations until now as good as I could carry them out at the circular micrometer, reduced (Fig. 8.14)*:*

Still Ceres appears as an 8th or 9th magnitude star. On the 10th I could not get a distinct image of it at a magnification of 180 and 240 times, although the weather seemed to be clear (maybe because of the many ice particles in the air, 10 degrees Reaumur frost). It was blurred and pale. The same happened on that night in Lilienthal with a 13-foot telescope, where it was seen at a magnification of 136 times pale and watery, at 288 times not as clear and bright and very blurred. But its disk appeared slightly larger than that of the first Jovian satellite. (It certainly is according to my observation not as bright). Should Ceres maybe possess an appreciable atmosphere reflecting the sunlight only badly? [I have suspected this since long and given this as one reason why this planet has not been discovered earlier.] It is admirable how accurate Doct. Gauss' elliptical elements are, which now give a right ascension greater by ½° and a declination smaller by 12′. This fact honours Mr Gauss' calculations as well as Mr Piazzi's observations. [* I completely agree with Mr Olbers.] I believe I have to respect Mr Piazzi's right to name his celestial body and call the new planet Ceres. Have you already thought about a symbol for Ceres? [Indeed. About this later]. As soon as the elliptical elements are sufficiently known one has to calculate the oppositions of Ceres in the last century and then check whether it had been perhaps observed by any astronomer as a fixed star 2 or 3 months before or after its opposition. Certainly it had not been observed more distant from the opposition; but I very much doubt that one will find any older observation of Ceres."*

*On January 16th Mr Harding from Lilienthal reported on his and Mr Schroeter's behalf the following: On January 11th we, too, had the pleasure to observe Ceres from 3am to 5am. In a 7-foot Herschel telescope it was not yet different from a 9th magnitude star. [*Supposed to mean 6th or 7th magnitude.] At a magnification of 152 times we believed to perceive a diameter; but its light was so pale and the image so blurred that I remained uncertain. But the magnification of 136 times of the 13-foot reflector showed that I was not mistaken. I estimated the size of its diameter that of the first Jovian satellite; it absolutely had the dark red light of Mars and was blurred. West of it were two very tiny little stars at a distance of about 20″ and 35″. At magnification 288 I could make out only the closest of them—the other disappeared completely. Ceres itself had at this high magnification a very pale light and was so blurred that I had to apply a lower magnification again to see it distinctly. To estimate its size approximately I compared it to the first Jovian satellite that was positioned close to Jupiter's disk and immediately saw that this satellite was noticeably* *smaller than Ceres and I thought not to be very mistaken assuming its diameter 2″.5. By mere estimation I found its right ascension at 5am 186° 36′, the northern declination 11° 18′. Since the 11th no further observation of Ceres has been successful here; and now the moonlight is a hindrance, too. –*

After mid-January I received (as usual) from the local Lange bookshop the January issue of the Monthly Correspondence from Gotha, in which Freiherr von Zach already reported that he, after several vain attempts, had observed on the night of December 7th/8th four small stars in the region where Gauss' ellipse allots Ceres' position, of which two are recorded in Lalande's Hist. celeste, vol. 1, of the other two was no trace in this work (one at right ascension 178° 33′ and 11° 41.5′ declination north). The next bright morning should decide on the nature of both stars. But it appeared on the 18th only. The air was not completely clear and when these four stars should have appeared at their culmination in the 8-foot meridian telescope, Mr von Zach though being very cautious with the illumination of the hairs, could neither discover the suspicious two nor the two of Lalande. In this uncertainty persisted when he handed in his article.

On January 23rd I informed Baron von Zach that I had rediscovered Ceres, according to Mr Olbers information on January 15th. On January 25th I had the pleasure to observe Ceres at last not only at the meridian but also at the circular micrometer of the 3.5-foot Dollond; I recognised it at once by its movement since the 23rd. I let it pass three times together with ς and no. 27 through the field of the telescope and calculated from this for 10 h 15′ 56″ mean time its mean right ascension 188° 20′ 25″ and the declination 11° north. Ceres appeared to me to have exactly the brightness of an 8th magnitude star, and was only in absence of the moon and at the highest magnification of ca.150 times slightly different from the neighbouring fixed stars. Also on January 26th, February 3rd and 4th the clouds permitted to observe Ceres at the circular micrometer; at the last-mentioned date it stopped and started to move retrograde westwards with strongly increasing northern declination. After that continuous changeable weather set in and only on February 26th and 27th I could continue my observation of Ceres this way. [I will give the results of these observations as well as the following of the new planet in the yearbook of 1805.]*

On January 26th I again received a letter of Mr Olbers in which he remarks the following: "You indeed saw Ceres on January 15th. I observed the small star accompanying it as well because between these two there was a very tiny little star (at magnification 106) which I thought I had to observe, too. But it was not a satellite of Ceres, but a small common fixed star. Should Ceres, being certainly not larger than our moon, have any satellites?"

"Of the four small stars, which our common friend Mr Freiherr von Zach in Gotha observed on December 7th last year, (cp. p. 74) the first one is missing as I found out yesterday comparing his second, third and fourth with my Dollond to no. 12 Virginis Flamsteed. There is still another star further north but this is not no. 1 of Mr von Zach. But there must be an error in the estimation of the declination. Ceres could not, as I believe, have been at 11° 41.5′ then but must have had about 11° 54′ to 11° 55′ declination.—I admit at least, that I would never have rediscovered the planet if Doct. Gauss had not calculated his ellipses. For I would have never searched for Ceres that far east and I doubt that other astronomers would have extended the labourious search that far. Mr Gauss has sent me for the fifth time elements derived from Piazzi's observations and Mr von Zach's new reduction; but they do not correspond as good to the present observations as the fourth ellipse. On the whole the second corresponds to the newer observations best."

As the 2nd ellipse gives for January 1802 the RA by 20′ too small, the dec. 6′ too large. The fourth ellipse the RA by 20′ too large, the dec. by 14′ too small. The 5th ellipse the RA by 32′ too large and the dec. by 16′ too small.

I have sent Mr Gauss all of my observations and he soon will give us corrected elements, to calculate in advance Ceres' position with the necessary accuracy to expect it at the passage instrument and the Wall quadrant. I will no sooner tackle the ellipse, than I have received the absolutely exact observations. Ceres can always be seen with my comet *searcher very well, with which I rediscovered it. It is now considerably brighter than no. 299 Virginis of your catalogue but still much fainter than no. 27 Virginis Flamsteed." On January 29th I received a letter from Mr Schroeter in Lilienthal:*

I am hurrying to give you my first observations of Ceres. Yesterday I found in hazy air at a magnification of 136.35 times of the 13-foot reflector 2″.813, and at magnification 288 times 2″.774. When I had Ceres on the 25th after 11 pm at a magnification of 136 times of the 13-foot reflector in the field of this telescope it stood in front of me in such perfect, round and steady planetary shape that no doubt is maginable. With a 9.5-inch aperture and a magnification of 136 and 288 times, its appearance resembled that of Uranus in its this time completely white light and was distinct but accompanied by a small nebulosity with its sphere showing through. It had according to my observation, including the atmospheric nebula, a diameter of 2″.51, thus considerably smaller than I had estimated from its appearance. On the evening of the 26th between 10 and 11 o'clock our weather was much clearer and more favourable than on the 25th. Now the planet appeared in comparison to an 8th magnitude star much larger and in a much paler reddish light than a true and distinct planetary disk, which was not the case on the 25th. In the telescope, however, it had at 136 times as well as 288 times magnification again a pale white bluish planet light and showed, despite the much more favourable weather today its disk not in the same way as yesterday, but appeared comet- or nebula-like limited. I could only determine its whole nebulous diameter as 2″.69. Consequently a strange atmospheric change cannot be noticed.

On the first days of February I found in the Moniteur at the entry for January 22nd the first report from Paris on the rediscovered planet which I insert in the original language. It goes: [translated from French:]

*"The new planet is today the most curious thing in astronomy so it is only natural to talk about it; I have to announce first that Baron von Zach in Gotha is the first who has rediscovered it on the morning of … 8th; he had nevertheless not been sure about it until … 31st, [*both times, the month is missing in the Moniteur; it was December] because he had observed four small stars and could not discern which of them was the new planet; as soon as he was certain he wrote to me, sent me the positions; I informed all astronomers of Paris about them on pluviose 5th.*

But citizen Mechain has already observed three or four hundred small stars in the region where this small star is expected; but it is so difficult to see that he would have missed it even if it had been among these: at last he discovered it on pluviose 4th (January 24th) at right ascension 188° 16′ and declination 11° 52′. Citizen Delambre discovered it on the following day, citizens le Francais and Burckhardt observed it on the 6th. Some compare it to an 8th magnitude star, the others to a 9th magnitude star, which proves that it has not two seconds of apparent diameter, thus it has not 600 leagues of true diameter; it is five times smaller than the earth which is the reason it has taken so long to discover it. Furthermore it had only been by fluke that Mr Piazzi made this curious discovery.

And from a later issue of The Moniteur: *"Mr Burckhardt has calculated the perturbation of Jupiter on Ceres and determined anew the orbital elements as follows:*

Mean distance 2.7677	*Eccentricity 0.0791*
Inclination 10° 37′ 4″	*Epoch 1801 2Z 17° 19′ 2″*
Aphelion 10. 26. 8. 42	*☊ 2. 21. 5. 46*
Revolution	*1681.51 days*

On February 27th 13 h 59′ 15″ mean time Le Francais and Burckhardt observed precisely: right ascension of Ceres 186° 58′ 44″ declination 15° 15′ 55″.

Around February 18th I received the February issue of the MC, in which Mr Freiherr von Zach reports that he revealed his discovery of Ceres on December 7th only to His Highness the Duke, Prof Buerg from Vienna, who was with him then, and Mr von Ende from

*Celle but it was only on the last day of December that he could find out for certain that this observed star was nothing else than Ceres; he could not find it at its former position and therefore pointed the passage instrument at the parallel of Ceres and observed three small stars from which one again corresponded to Ceres, according to Gauss' ellipse. The observation even with the passage instrument turned out unfortunately uncertain, because the faint light of the planet tolerated nearly no illumination of the hairs and it could not be determined in the darkness at which hair it had been observed. Therefore the position could only be stated from this observation unreliably. Until January 10th the sky of Gotha again had been overcast. On the 11th the weather cleared up and Mr Baron von Zach found the planet this time at 17 h 3′17″.4 mean time at 186° 45′ 49″.95 apparent right ascension and 11° 15′ declination north, which indicated a change of position in accordance with planetary theory. On the following day he received a letter from Mr Olbers in Bremen, dated January 6th [*Mr Olbers' letter to me containing this important news was of the same date and I received it on January 12th as well.] in which he communicated his discovery of Ceres. "On January 16th the sky permitted, in severe frost, a renewed observation of Ceres in Gotha and Mr von Zach found it at 16 h 26′ 25″.6 mean time at right ascens. 187° 27′ 53″.25 and 11° 26′ north. declination. The latter is usually determined only by mere eyepiece estimation from the positions of the neighbouring stars and by the half-circle of the passage instrument with its scale in whole minutes only, because it was impossible to observe Ceres in the field of the telescope of the meridian quadrant [* The ducal observatory of Seeberg is not equipped with a wall quadrant yet.] but disappeared immediately at the slightest illumination of the hairs due to its faint light. Even in the 8-foot 4-inch aperture passage instrument this small star could only be discerned with great effort."*

*On February 16th I wrote to Mr Gauss in Brunswick, congratulated him on the calculations fortunately come true in Ceres' path and sent him some of my former observations at the circular micrometer. [*Thereupon some made at the wall quadrant, for the purpose of his further calculations.] But before this letter could reach him I received a letter dated February 15th, and this skilled and modest man expresses himself:*

"Excuse me, that I take liberties with writing to you. I believed an announcement of an ephemeride of the planet calculated according to corrected elements, which you would receive otherwise only months later, would not be unpleasant for you....Should you like to report some good declinations, which are still lacking I would be very obliged. I wish, that especially Piazzi would start early to observe it again, maybe you could expedite this or have already done so. A number of observations of this excellent astronomer, who enjoys such a kind climate and incomparable instruments, especially the circle, would contribute to an exact knowledge of its orbit in the near future.

My enclosed corrected elements give at present the right ascension by 7″ too small and the declination by 20″ too large. But still the number of declinations to which I could compare them is too small and do not correspond among each other accurately enough; therefore for a further correction of the elements I have to wait for further observations (Fig. 8.15).

*[*From these elements I further calculated: semi-major axis 2.76997*

semi-minor axis 2.76077 distance of aphelion 2.99546 distance of perihelion 2.54448

log of eccentricity measured in parts of the radius of earth's orbit 0.225492]

Accordingly the following positions of Ceres have been calculated (Fig. 8.16)*:*

*According to my information from Mr Gauss I never missed Ceres in the next days and could follow it in its retrograde and increasing northern declination and observe its culmination at the wall quadrant. They corresponded extremely well to the observations. I then calculated according to his above elements preliminary tables of Ceres' true heliocentric orbit and used them to temporarily determine its apparent geocentric positions for the former months and months to come to be able to observe it further. [*It is remarkable that Ceres according to these tables on November 2nd last year really was around n Leonis, where Mr von Lindener observed a star (cp. p. 62). It is a pity he could not give me its exact positions.]*

Fig. 8.15 Orbital elements of Ceres by Gauss

1801 Palerm. Merid.

Epoche . . .	77° 27′ 31″	
Sonnenferne	325° 57′ 15″	Sider. ruhend
☊ . . .	80° 58′ 40″	vorausgesetzt.
Tägl. mittl. trop. Bewegung	769″,7925	
Logarithm. der halben großen Axe	0,4424742	
Excentricität	0,0814064	
Neigung der Bahn . . .	10° 37′ 57″	
Epoche 1802	155° 30′ 25″	
Größte Mittelpunktsgleichung	9° 20′ 8″	
Tropische Umlaufszeit	1683 Tage 14 St. *).	

Fig. 8.16 A table of positions of Ceres from Mar. 1 through Apr. 9, 1802

Mitternacht.		Gerade Aufsteigung.		Abweichung nordl.	
		Gr.	M.	Gr.	M.
März	1	186	41	15	30
—	4	186	11	15	50
—	7	185	39	16	10
—	10	185	4	16	29
—	13	184	28	16	47
—	16	183	51	17	4
—	19	183	13	17	19
—	22	182	34	17	33
—	25	181	55	17	44
—	28	181	17	17	54
—	31	180	39	18	1
April	3	180	3	18	7
—	6	179	29	18	10
—	9	178	56	18	10

On the night of March 1st I observed Ceres for the first time in our 5-foot Bird wall quadrant. It appeared in the field of its telescope exceptionally clear.

[Its objective was twice ground by Carochez in Paris and had a 2¼-aperture and tolerated even by moonlight the complete illumination of the hairs. Exactly the same was the case when I could compare the planet with a 7th magnitude star.]* [Ed: he refers to Noel Simon Carochez (1740–1813). In the revolutionary period of France he was the foremost instrument maker for the Bureau des Longitudes]

At that time I saw it constantly at a 2-foot Dollond night telescope very distinctly. Some of my friends could even perceive it with the naked eye.

*[*It is inexplicable why Mr Piazzi could not discern Ceres beyond the meridian with his searcher and achromat of considerable aperture. Has this planet an alternating light whose cause is to be sought in the composition of its surface of the atmosphere? Schroeter's observations seem to prove this.]*

Some of my observations of Ceres at the wall quadrant (Fig. 8.17)*:*

*I calculated the positions of Ceres according to the observed difference of its right ascension and declination primarily of θ β and oLeonis. (I will provide further calculations in the yearbook of 1805). The opposition of the planet to the sun occurred according to this in the afternoon of March 17th at 4 o'clock. [*On the day of opposition Ceres did not have its culmination until 12 h 27 min true time, because it was lengthways opposite to the sun. This was in consequence of its great northern latitude and its position at the equinox and for the astronomers the first experience of its kind and thus unique.]*

	M. Z.			Scheinbare gerade Aufsteig.			Abweichung. Nordl.		
	U.	M.	S.	G.	M.	S.	G.	M.	S.
1 März	13	50	12	186	40	46	15	29	40
5 —	13	35	42	186	0	0	15	57	5
14 —	12	49	31	184	15	50	16	52	54
15 —	12	44	44	184	3	7	16	58	24
16 —	12	39	56	183	50	27	17	3	52
19 —	12	25	37	183	12	8	17	18	52
24 —	12	2	16	182	17	12	17	39	26
27 —	11	47	21	181	29	15	17	50	47
1 April	11	23	35	180	26	57	18	2	48
3 —	11	14	7	180	3	13	18	6	14
15 —	10	18	46	177	59	38	18	5	12

Fig. 8.17 A table of observed positions of Ceres from Mar. 1 to Apr. 15, 1802

On March 15th Mr Lalande from Paris wrote as follows:

"Mr Burckhardt has just calculated the perturbations of the new planet, which approach 30 minutes. He has produced tables of its path according to his calculated elements:

Epoch	*1802 5Z 5° 32′ 35″*
Aphel.	*10Z 26° 44′ 37″*
☊	*2Z 21° 5′ 35″*
Semi axis	*2.76587*
Eccentricity	*0.07887*
Inclination of orbit	*10° 36′ 52″*
Annual motion	*2Z 18° 13′ 18″ ***

*[**These elements correspond extremely well to those of Gauss.]*

Herschel has found the planet's diameter only 1″.5. I would like to know in which work and for what reasons you announced the Piazzian planet before its rediscovery in order to mention it in the history of this planet that I am planning to publish." On April 9th I at last received a letter from Mr Piazzi, dated Palermo, March 2nd, in answer to my report of January 12th on the rediscovery of Ceres by Mr Olbers. He writes: "Only on February 23rd [The letter was dated March 2nd and began as follows: I therefore understand February 23rd I rediscovered my planet. Without doubt due to my letter of January 12th which must have reached him around February 20th.] because of the bad weather I could not make a single observation. I would like to thank you for your congratulations and return them because you deserve it so very much. I will follow my planet continuously; and because you asked me for my observations I would like to ask the same of you as well as any information about what becomes known in Germany."*

Within the second half of April Ceres became smaller and smaller; on the 21st its brightness resembled only that of an 8th magnitude star. On the 23rd, 24th and 25th I and Prof. Huth, from Frankfurt an der Oder, visited Mr von Hahn [Count Freidrich von Hahn, 1742–1805; see Lisch, 1856*] in Remplin. We could only spend these three evenings in Remplin, and the sky was very changeable and clouds interrupted and prevented our observations at Mr. v. Hahn's wonderful instruments. On the 23rd it had been clear only for* *one hour during twilight; later on clouds came in from the west and covered the sky. On the 24th it had been clear between 10 and 11 o'clock, then a kind of northern lights occurred and layers of mist passed through the sky; by 11:30 it was overcast again. On the night of the 25th sky was heavily overcast. On our return journey on the 26th and 27th, however, we had the clearest nights. We observed Ceres at the 20-foot Herschel telescope of Mr v. Hahn [* Of the newer, bigger and more powerful Herschel telescope which Mr von Hahn is getting erected (cp. Astr. Yearb. 1804, p. 266) only the tube and parts of the frame are finished.] in which Ceres appeared at a magnification of 240 times in round planet-like shape but we were unable to discover with certainty something like an atmosphere around its sphere due to its extreme small apparent diameter. [*On the 24th I furthermore observed with pleasure at this excellent 20-foot Herschel telescope Saturn with its ring and its shadow and a dark stripe on the sphere of this planet plus three of its satellites; extremely distinct and clear; Jupiter's sphere with its stripes and satellites extremely clear and distinct at considerable magnification; the star ring in Lyra, the uncountable amount of stars around Cygnus and other curiosities of the firmament.] For an observation of Olbers' small planet-like comet the clouds gave us no time.*

From the latest question of Mr Lalande in his letter of March 15th can be told that the astronomers in France have never mentioned in their papers the idea that the unusually large gap between Mars and Jupiter might be occupied by a main planet, and that they did not know the progression of the distances of the planets, publicised by me since 1772, and which has already been nicely confirmed through the discovery of Uranus; or they have ignored it.

And I neither have ever come across any comments of astronomers of other nations on this progression. Professor Lambert noticed this wide space between Mars and Jupiter; but in his Cosmological Letters on page 7 he jocularly invented a comet which had led away the planets formerly situated there. [He speaks of several; of a single one maybe existing in this space and at what distance nothing is said.] And when I sent him, still at Hamburg, at the beginning of 1772 the second edition of my then published Introduction to the Knowledge of the Starry Heavens to Berlin, which was the reason why I was summoned here, the same expresses his view in a reply, dated February 3rd, on the progression as follows: "The comment on the distance of the planets on page 462 of your work would have pleased Kepler, who wrote an entire book about it. It might help you get on the track if the planets are indeed distant from the sun according to a simple law and their masses are not taken into account. Jupiter's satellites show indeed an order in their distance, which is based on the balance among them. Wargentin* [ed: Pehr Wargentin, 1717–1783, Swedish astronomer who discovered the comet of 1750] *dwells on it in Actis Upsaliensibus* [Ed: see Wargentin (1741)]. *Apparently this progression of the planets' distance was not known to dear departed Lambert and it seemed worthy of utmost notice to him."*

*The new planet was discovered by Mr Piazzi in Taurus and observed by him alone before its closeness to the sun, but thereafter rediscovered in Virgo by Mr Olbers in Bremen and then seen by all astronomers and observed in its path; therefore the name Ceres, given by its discoverer, is very appropriate. Ceres was the goddess of harvest in ancient Greece, Taurus in the sky was the symbol for the beginning (ploughing and tilling) agriculture, Virgo or Ceres herself the symbol for the completion of agriculture (harvest and threshing). Most likely the name Ceres will be kept in agreement with the majority of astronomers; [*For the name Juno or Hera, borrowed from such a splendid goddess, seems not suitable for this small nondescript planet.] and it is furthermore now time to think about an appropriate symbol for this new planet. My first idea was to choose an upright positioned oval accompanied by a cross underneath, which could hint at the shape of an ear. But later on I got the idea of a sickle; and because several other astronomers have declared it the most simple and appropriate, it will be universally introduced. Thus this new planet will be represented ⚳, which even looks good among the other planet symbols: ⛢ ♄ ♃ □ ♂ ♁ ♀ ☿.*

*It was a strange coincidence that exactly at that time when Ceres was observed by the astronomers for the first time opposite to the sun, at its Opposition, at the same time Uranus came to the same position, and that with their moving back and forth now the one now the other reached the meridian, often only several minutes apart; or that they both were simultaneously at their culmination, although Ceres due to its great northern latitude of 16°, in the meantime 12–20 degrees, at a greater altitude or further north than Uranus. Because of this circumstance the observations of one or the other had at times to be neglected according to altitude and time, namely then, when it followed it too close in the meridian or was just ahead of it. [*Ceres appeared in the searcher, in the 3.5-foot Dollond, in the achromatic telescopes of the passage instrument as well as in the wall quadrant every time considerably smaller and fainter in brightness than Uranus; particularly since the latter seems to have increased in brightness for some time and is visible to the unaided eye and has nearly the brightness of a 5th magnitude star.]* [Ed: a section here about the discovery of Pallas, which will be in a later volume of this series.]

The true size of the new planet Ceres can not be calculated reliably because its apparent diameter is stated so differently. Mr Piazzi estimated it in January 1801 7 sec., but probably too large. Mr Schroeter gives its diameter in a letter, dated April 21st, as follows:

*On March 30th with blurred shape 4″.6. On the 31st Ceres appeared without nebulosity and a diameter of 3″.2. On April 21st it took on its nebulous appearance again and had a diameter of 4″.7. On the 3rd 4″.3. [*Accordingly Ceres appears with alternating brightness, as several claim to have observed.] Ceres was at that time about 1.70 distant from the Earth (the distance of the Earth from the Sun assumed = 1.00). If I assume on average Ceres' apparent diameter at this distance as 4″, it must thus appear to be 6″.8 at the latter. But the apparent diameter of Venus = 17″.0: thus Ceres would have a true diameter of 17″.0/ 6″.8 = 2.5 times smaller than the earth, or 15 times smaller in physical space. The Moon is 50 times smaller than the Earth; thus Ceres would be only* $3\,{}^{1}\!/_{3}$ *smaller than the Moon and approximately as large as Mercury. But these numbers can still be very unreliable. Mr Herschel found its diameter in January only* $1\,{}^{1}\!/_{2}$ *″.*

According to public news, if it is reliable, Mr Piazzi is supposed to have found a satellite near Ceres; I do not know anything about it yet. But Mr Harding reported already on January 16th from Lilienthal that he had observed at the 13-foot Herschel reflector two very tiny stars near Ceres, at a distance of about 20″ and 35″, whose further observations became not known to me.

On Ceres the sun appears 7.5 times smaller in area and accordingly its light is equally fainter there than on the earth. Seen from the sun, the earth moves away at most between 19.5 and 23° [= the difference in heliocentric and geocentric longitude of Ceres.] westwards or eastwards from it, approximately as far as Mercury from us. In our firmament this small new planet shows due to its great inclination of the orbit of 10 ½° to the ecliptic at times apparent motions, which we are not used to observe at any other planet. When it is in opposition with the sun near Taurus' horns or the arch of Sagittarius or appears at midnight at the meridian it appears near or on the ecliptic. If it (as it was the case in March and April this year during its first opposition observed by astronomers) is in the western part of Virgo in opposition it exceeds by far the boundaries northwards of our old zodiac and reaches a latitude of more than 18°. But if it is on the other hand cast in the water of Aquarius in opposition it exceeds these boundaries southwards and reaches 18° longitude south (it hardly moves 19° in the meridian above the horizon). Its great southern latitude, which can at times even be reached in Capricornus, is the reason that it appears there at best 7° high in the meridian. In general Ceres normally has in the constellations Gemini, Cancer, Leo, Virgo, Libra and Scorpio a northern latitude and in Sagittarius, Capricornus, Aquarius, Pisces, Aries and Taurus a southern latitude. It can only be observed two or three months before or after its Opposition because of its small true and apparent size. If I assume its apparent diameter at its greatest closeness to the earth, i.e., opposition, 4 seconds, thus* *its apparent diameter close to the sun or low or high in the evening sky, or when it is following the sun or is moving ahead of it only about 2 seconds and it can only be seen with very good and powerful achromats or telescopes.*

*Why this planet had not been discovered earlier? One can apply this question to all discoveries. [*The deceased Mr Lichtenberg from Goettingen answered this question after the discovery of Uranus quite naively in the Goettinger Taschenbuch of 1783. Cp. my treatise on Uranus of 1784, p.78.] It was raised with astonishment after the discovery of Uranus, too. In general its answer is the lacking happy coincidence which caused it and many other discoveries of importance. Herschel and Piazzi were not interested in discovering unknown planets beyond Saturn and between Mars and Jupiter, when the former pointed his 7-foot telescope at a region of the Milky Way between Taurus' horns and the feet of Gemini, in order to discover star clusters and nebula or binary stars [*On this strange evening of March 13th 1781, when Mr Herschel discovered Uranus below B in Taurus, he was engaged in the discovery of several binary stars. Philosoph. Transact. 1781] there and the latter caused to observe no. 87 Taurus by an erratum of de la Caille. This fact nevertheless does not diminish the merit and the fame of these two worthy men: For their tireless investigation of the heavens led them to their lucky discoveries. Both, Uranus and Ceres, had been declared comets just after their discoveries. The experience confirms our assumptions: that our new planet, like I have supposed since long, must only be a small body, scarcely reflecting light from its surface, maybe shrouded in an extensive atmosphere, [* Probably the small apparent diameter of Ceres is a consequence of this faint reflection. Its true size may be more considerable than our calculations show. Maybe it has dark patches on its surface and does not reflect from all points an equally strong light. This is commonly assumed of the Jovian satellites, and Schroeter's observations confirmed this, because their shadows on Jupiter's disk appear larger than they are.] and therefore can only be observed for some months before and after its opposition, which occurs every 15.5 yr; and that its orbit might have a considerable and greater inclination to the ecliptic, than that of all other planets, and therefore often exceeds the boundaries of our ancient zodiac, where no planet is expected, or not as often as near the ecliptic where there are neighbouring stars which positions can be compared in order to discover a small undiscovered planet because of its motion. By the way, there is still hope to find Ceres among observations of former astronomers, although due to above mentioned reasons more unlikely than in the case of Uranus.*

Thus we became acquainted in less than twenty years with two more fellow-travellers on our way around the sun, namely the fifth and the eighth main planet, Ceres and Uranus. [The first was discovered on January 1st, 1801 and the latter on March 13th, 1781.] Discoveries, whose epochs will forever remain strange in the annals of astronomy and which are unique in their kind for 2000 years of observations of planetary orbits. Kepler and Newton did not suspect those.—By the discovery of Uranus we have witnessed with astonishment the doubling of the visible solar system and the hitherto gap between Mars and Jupiter being occupied by Ceres.*

We now know eight globes which stroll around the Sun on eternal and harmonic orbits placed behind one another. The admirer of a wise reason of all things will find in this attitude new food for thought. Where solely orbits are possible, there heavenly bodies roll, and where solely beings feel happy, there calculations show that the latter should be approximately twice as distant from the Sun as Mars is!

*[*Because, as already mentioned, this new main planet is exactly there where it is placed according to a well-known progression of the distances of the planets (cp. p. 43), and surprisingly this was the case with Uranus as well: Thus it appears as if except for the known eight main planets there are no further planets in our solar system, because whichever space they might occupy the harmonic law of distances would be ruined, which we cannot explain yet and had only been proven by experience. By which extremely strange coincidence should only the eight planets have come to our knowledge, whose distances are in correspondence to this law and of which two are even scarcely visible to the unaided eye.*

How could Newton's celestial mechanics, improved by de la Place, and the profound calculation of the impact of the mutual gravitational forces of the known planets, correspond so exactly to the observations, if the spaces would be filled above that with several unknown but collaborating planets? Among those could be some of considerable masses, which we cannot see due to their faint reflection of the sunlight. What perturbations on their orbits the smaller planets would not suffer especially by the great gravitational forces of the large and close planets Saturn and Jupiter? And could the comets so unimpededly stroll through the planes of all the planetary orbits?—Disregarding several reasonable objections against this opinion.]

Chapter 9
Schroeter's Book of 1805

In 1805, Johann Schroeter (Cunningham, 2007) published the report of the Celestial Police, entitled *Lilienthal Beobachtungen der neu entdeckten Planeten Ceres*, *Pallas und Juno*. Much of the book deals with the diameter of Ceres, which he overestimated, and the atmosphere of Ceres, which does not exist. This summary translation is the first time Schroeter's book has been published in English. The original paragraph headings, denoted numerically, have been retained (Fig. 9.1).

Schroeter's original book was printed on 378 small pages. Pages 176–241 dealt with Pallas, while pages 242–339 dealt with Juno. Pages 340–378 consisted of a translation of Herschel's paper from English into German. This text treats only the section on Ceres from pages 1–175, which follows a 12-page preface and a 34-page review of the contents to follow. The sections on Pallas and Juno will be presented in other volumes by this author.

Observations at Lilienthal of the New Planets Ceres, Pallas and Juno

Chapter 1

Observations and measurements of the newly discovered planet Ceres Ferdinandea by the astronomer Mr. Piazzi, director of the Royal Observatory in Palermo stem from thorough justification of the same despite doubts about the true proportion of this body and its atmosphere.

On January 6, 1802, Dr. Olbers from Bremen told us the important news that he recovered Ceres between Denebola [β Leonis] and ρ Virgo. This happened on January 1 as well as on the 3rd and 5th. Due to bad weather and health I was only able to do the measurements on January 25. Long before that Inspector Harding devised an excellent, complete and accurate chart of the heavens, through which Ceres travelled in an elliptic path (as calculated by Dr. Gauss). This enabled him to find this small planet immediately on January 11, 1802, as the weather turned better again. With a good 3-foot achromatic telescope Ceres appeared to him a reddish star of magnitude 9. An enlargement of 84 times with the 7-foot Herschel telescope showed her neither considerably larger nor brighter. She did not

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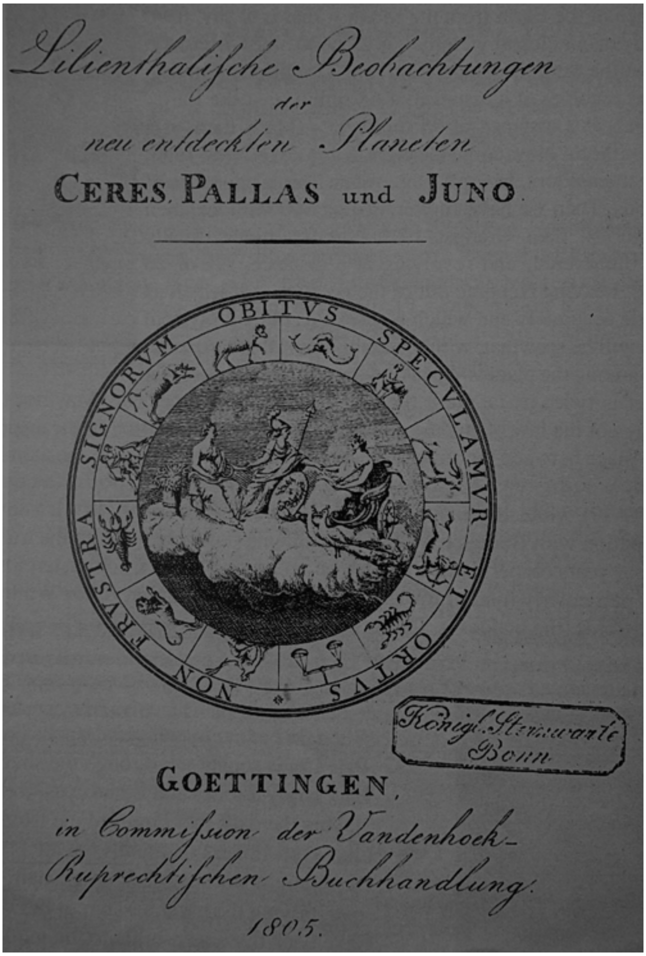

Fig. 9.1 The title page of Schroeter's 1805 book on the asteroids

have a lively central point as other nearby located smaller and larger fixed stars; that appeared to point to her being a planet. With an enlargement of 156 times, which he then tried, he did not receive a clear view. Ceres appeared reddish and foggy. At 15 o'clock 12 min. he tried an enlargement of 136 times with the 13-foot reflector, but she appeared again in reddish light surrounded by fog. West of her sparkled two extremely small stars to which Mr. Harding turned his attention and he estimated them 20″ and 25″ apart. With enlargement of 288 times with the same strong reflector Ceres appeared again unclear. Not sure whether the weather conditions or the planets' dim light caused this he tried again the enlargement of 136 times. The planet appeared more clear but still surrounded by fog.

2

Past 17 o'clock increasing fog in the southeasterly skies prevented further studies. He pointed the 13-foot reflector to the clearly visible Jupiter whose first satellite moved off shortly before. Jupiter therefore appeared as a round little disk without any irradiation.

This proved that Ceres' fogginess was not caused by air or the telescope. He noticed that with the same enlargement Ceres appeared to him at least twice as large as the disk of Jupiter's satellite. According to my calculations and measurements this satellite at the shortest distance of Jupiter from Earth has a diameter of 1″.4. Since Jupiter was not far from Earth Mr. Harding concluded that Ceres has to have a "diameter" of not less than 2″ but not more than 3″. He estimated Ceres to have 2″.5 in her nebulous bounds.

3

On January 25 past 11 pm after a succession of 14 totally cloudy nights I decided to combine my observations with the ones of Mr. Harding. Mr. Harding found Ceres westerly at ρ Virgo. When I looked past ρ Virgo westerly I discovered at once with the 13-foot reflector this new, perfectly round planet. This time it was not reddish but in pure white light, her disk identical to the Georgian planet [Uranus]. *Despite the light of the rising Moon she was surrounded by a very thin cometlike fog. This way I saw her with 136 and 288× as well as in moonlight. This new planet resembled to some extent the comet of 1799 which I described in the 3rd volume of my articles. Only her disk was much brighter and more clearly visible and her atmospheric nebula smaller. When I measured the diameter at 0 o'clock I had the same situation as I had had with that comet. If I measured with the nebula then she came under the 288 enlargement, almost as large as a lit disk of 2 Dec lines and in comparison to a disk of 1.5 lines only of this difference smaller as one of 2 lines with 1834 lines. But when I measured only its disk I found the diameter only to be 1.334 lines. The distance of the projections was the same for both measurements namely 522.5 lines. Therefore the calculation for the diameter of the planet disc*

1.

Log 1.334 Lin	*=3.1251558*
–Log 522.500 –	*=5.7180863*
	7.4070695 = tan 8′ 47″
$= \frac{527''.000}{288} = 1''.850$	

2. For the whole diameter including the nebula

Log 1.834 Lin	*=3.2633993*	
– Log 522.50	*=5.7180863*	
	7.5453130 = tan 12′ 4″	
$= \frac{724''.000}{288} = 2''.514$		

This measurement of the whole diameter was repeated by Mr. Harding at the same distance from the eye. He found the planet 1.750 lines large. For the whole diameter this amounts to 2″.330 only 0″.184 not even 2 decimals a second, or less than 1/14 of the whole diameter. This shows clearly how accurate one can determine the diameter of such small planetary discs at such short distance of the projections from the eye. With such careful measurements a major error is impossible.

4

Already then the difference of these observations from the previous ones showed that strange changes in the atmosphere of this planet were unmistakable. Mr. Piazzi estimated its diameter at 8 sec. Its reddish light was so pale that he could not see it with an achromatic telescope with a 3 inch opening. Dr. Olbers, Freyherr von Zach and Mr. Harding saw it in reddish light. Mr. Harding saw it even with 136× with the 13-foot reflector.

But now it appeared as a sharp defined planetary disc surrounded by a minute nebula. In the 2-foot reflector of 2 inch opening its light appeared white and at least as strong as the light of Uranus.

5

The peculiar changes of the nebular surroundings and of the light were confirmed the following evening on January 26 at 10:45. Ceres appeared in the 13-foot reflector considerably larger than the star of mag. 8 Nr. 299 Virgo of the Bode index, but more matte and like a disc. Enlarged 136 times with this reflector she appeared again in bluish white but soft, matte and relatively bright light and again nebulous. The same results with enlargement of 288 times. But neither with this or the other enlargement could I again distinguish the actual disc from the nebulous surrounding, as I did the night before, despite even better weather conditions today.

6

At about 12 o'clock we measured her whole diameter. After careful examination and comparisons, I found her whole disc to be 2 lines with 288× with the 13-foot reflector. But in comparison with a disc of 1.5 lines it was 1/4 this difference or 0.125 lines smaller. The same findings were made by Harding. The unknown distance amounted to 500 lines. Hereafter the calculation

L. 1.875 Lin.	*=3.2730013*
–L. 500.000 –	*=5.6989700*
	7.5740313 = tan 12′ 54″
$= \frac{774''.000}{288} = 2''.687$	

The whole disc appeared today 0″.173 larger than the evening before. I was unable to see or measure again the actual disc as I did the night before. Since our atmosphere was even more clear today it was probably the atmosphere of the planet that was more cloudy. Ceres did not have the cleanness of 24 hours ago.

7

Mr. Harding took charge of the determination of the planets' rising since he had the use of a circular micrometer.

The results of a thrice repeated measurement at which he compared the planet with Flamsteed n. 27 Virgo and Bode's N 199 Virgo, gave 188° 20′ 5″ at 11 h 59′ 54″ mean time, and northern declination 11° 54′ 43″

On January 26 he compared the planet with the same stars at 12 h mean time, and it resulted in 188° 24′ 22″ and northern declination = 11° 59′ 56″

8

On January 28 amid clear heavens the planet was located near N299 Virgo of Bode's index and appeared through the 13-foot telescope much brighter and like a star of mag. 7. With 136 times enlargement it appeared to be somewhat reddish. Much the same at 11:10 o'clock with 288× but in more reddish light. Whether or not the reason for the planets' reddishness lay within its own atmosphere or our's cannot be answered. At 12:30 I measured

1. *With 288 times; its disc at 1.5 lines but in between slightly larger so that I set for difference of .500 lines = .166 larger.*
2. *With 136 times not as large as 1 line, so that I had to estimate the smaller size at 1/5 and the distance from the eye to be 430 lin.*

Therefore the calculation

1) L. 1.666	*=3.2216750*
–L. 430.000	*=6.6334685*
	7.5882065 = tan 13′ 19″
$= \frac{799''.000}{288} = 2''.774$	

2) L. 0.800 Lin.	*=9.9030900*
–L. 430.000 Lin	*=2.6334685*
	7.269215 = tan 6′ 24″
$= \frac{334''.000}{136.35} = 2''.816$	

These measurements with different enlargements show clearly how accurately one can measure this way, because the average results in 2″.795 which deviates only 21/1000 sec. from each result. During this observation Harding compared the position of Ceres with Nr. 27 Flamsteed, and found for 13 h 32′ 52″ mean time. Apparent RA Ceres = 188° 31′ 22″ and apparent DEC 12° 8′ 54″.

9

The observations continued on January 31 past 11 o'clock. Ceres appeared again a white light, tending towards reddish colour. It was impossible for us to see anything of the actual disc as before as January 25. On the contrary Ceres' disc appeared especially at 288 times very boundless. It was fruitless to make any fairly sharp observations. Our exceptionally clear atmosphere left no doubt that peculiar changes of Ceres' atmosphere were the cause. Under those circumstances was the measurement of her diameter that difficult; it took more than half an hour to do it. After repeated examination she was at 288 times equal with the projection disc of 2 lines. Harding made sure of that as well. The projections' distance was found at 489 lines and therefore the calculation

L. 2.000 Lin.	*=3.3010300*
–L. 489.000 Lin.	*=5.6893089*
	7.6117211 = tan 14′ 4″
$= \frac{884''.000}{288} = 2''.930$	

It was peculiar that the apparent diameter of this planet increased fairly regularly, daily about of a decimal second or 0″.069 during the time from the 25th to 31st of January. Naturally my curiosity arose just like with the Comet of 1799, because I also hoped for peculiar results as soon as Ceres' distance from Earth according to the Gaussian elements was calculated. Olbers undertook this task.

10

Because of bad weather we could continue our observations only on February 5th 10 h 30′ mean time. This time Ceres showed really white light but still slightly reddish. The 13-foot reflector was opened and enlargements were done at 136 and 288 times. At 11 h 30′ and throughout the whole observation time she appeared more defined than ever and *as a round planet. Even through the 7-foot Herschel telescope she stood out for her planetary appearance from the fixed stars. Nevertheless she had nebular boundaries and her actual disc could not be distinguished like on January 25. But this did not prevent us from seeing her distinct planetary form very clearly. The clear picture of the planet made a measurement very easy, and the changes in atmospheric boundary was clearly obvious. At 288 times her disc measured 2 lines and I found the distance from the eye to be 413 lines. Therefore the calculation*

L. 2.000 Lin.	*=3.3010300*
–L. 413.000 Lin.	*=5.6159501*
	7.6850799 = tan 16′ 39″
$= \frac{999''.000}{288} = 3''.468$	

If one compares this measurement with the one from January 25, when Ceres appeared also very clear, it results in an increase of 0.′954 of her apparent diameter within the last 11 days. From January 25 to January 31 or within 6 days this increase amounted to 0″.416. According to this, the increase from January 31 to February 5 (5 days) should have been 0″.342. On February 5 its size should have been only 3.276. The difference between this and the actually found size amounts to only 0″.192, not entirely 2 decimals of a second. To avoid some minor discrepancies which may be possible due to a very small diameter and also due to our own atmospheric influence, we intended to measure Uranus, then not too far from Pallas. But we could not go ahead with it this time as our atmosphere changed to hazy over the easterly skies.

11

Harding obtained with the circular micrometer for the 5th of February 11 h 56′ 33″ mean time Ceres' westerly distance from Nr. 34 Virgo Flamsteed = 35′ 52″.5, and the difference in the declination = 12′ 30″. RA of Ceres = 88° 42′ 34″.5, and the declination = 12° 49′ 46″.

12

On February 10 our weather cleared up again. Despite the moonlight Ceres was still distinguishable at 13× with the 13-foot reflector. At 10 h 20′ I found her situated in an almost right angle above the 27th and 34th star Virginis of Flamsteed. An enlargement of 136 times with this powerful reflector showed southeasterly a minute dim star close by. With 288 times it appeared entirely dark and barely distinguishable. With 136 times as well as 288 times the planets' light was slightly reddish and still quite boundless and nebulous. This was even more peculiar and crucial since the bright light of the half illuminated Moon nearby had to make the farthest part of the nebular boundary invisible. Besides, his light appeared soft, planet-like and white-reddish at fully 9.5 inch opening. Also today we could not see his actual disc like we did on January 25.

13

Judging from Ceres' peculiar atmosphere her weather conditions could not be good. To avoid any optical delusions we went ahead with our intentions and compared her with Uranus. Now we were convinced that the peculiarities were not caused by our atmosphere. Uranus appeared already very clear at 80 times with the 7-foot Schrader telescope.

14

It was this time also very difficult to obtain the measurement of her diameter due to her nebulous boundaries and the bright moonlight. To be more accurate I constructed new projection discs which showed fractions. The measurement with the 13-foot reflector *at 28 times resulted in 2800 lines and the distance from the eye 565 lines. This resulted in the apparent diameter*

L. 2.800 Lin.	*=3.4471580*
–L. 565.000 Lin.	*=5.7520484*
	7.6951096 = tan 17′ 2″
$= \frac{1022''.000}{288} = 3''.543$	

This measurement was also accurate.

15

At 12 h 30′ mean time Uranus was compared with the picture and size of the new planet. Uranus appeared as clear as all other planets, like a small perfect sphere. His light was much whiter, stronger and more beautiful than Ceres'. He was much more eye-catching than all other stars whereas Ceres was barely distinguishable through the same view finder. The difference between these two was striking and the comparison on January 25 showed a cometlike atmosphere surrounding Ceres. In comparison to Uranus Ceres appeared like the Herschel planetary nebula at ν of Aquarius appears compared to Ceres. In order to find out if Uranus' apparent diameter was really larger than that of Ceres, I adjusted the micrometer to about the same distance that I measured Ceres, that is 568 lines from the eye. Uranus appeared a tiny bit smaller than the disc of 2.800 lines. This results in

L. 2.300 Lin.	*=3.4471580*
–L. 577.000 –	*=5.7611758*
	7.6859822 = tan 16′ 41″
$= \frac{100''.000}{288} = 3.475$	

[the figure of 100″.000 in Schroeter's book is incorrect. It should be 1000″.000]

16

On February 12 Ceres appeared again in slightly reddish light with nebular boundaries. This with 136 times as well as 288 times with the 13-foot reflector. Using the micrometer disc I found her diameter at 288 times = 2933 lines and the distance from the eye 580 lines. This results in

L. 2.933 Lin.	*=3.4673121*
–L. 580.000 Lin.	*=5.7634280*
	7.7038841 = tan 17′ 23″
$= \frac{1043''.000}{288} = 3''.621$	

Since the measurement from February 10 at which Ceres was found to be 3″.548 her apparent diameter increased her diameter according to this measurement by 0″.073. Such a slight difference may very well be due to some inevitable errors in estimating, however it shows how accurately such a small diameter can be determined.

17

On February 13 Olbers sent me the requested distances of Ceres from Earth calculated from the Gaussian elements. He included a note, saying "Notable was the comparison of Ceres with Uranus on February 10. I observed both at 106 times and 180 times with my Dolland. Uranus' disc appeared at least twice as large as Ceres' disc, which was barely recognisable at even 180 times." Olbers' remark gave me the reassurance that both our observations confirmed Ceres' peculiar atmospheric nebula. To us also appeared Uranus with his sharp boundaries, for that very reason, noticeably larger than Ceres. Nevertheless the apparent diameter of both planets [see sections 14 and 15] was nearly the same (3/4 of a decimal second difference only). In order to see such a slight atmospheric nebula with its many sloping layers the telescope's opening has to be very large. Olbers could not see the nebula with an opening of 3¾ inches. But we observed it with 6.5 and 9.5 inches. He only saw the brighter nucleus on a disc. He who wants to make sure of this truth can read on about my observations of the comet of 1799 in the 3rd vol. of my articles, pg. 82 and in the annexed Table delta.

18

It stimulated my curiosity to see how the steady, regular increase in diameter, or in my opinion, rather the increasing visibility of the atmospheric nebula would relate to the Earth's distance from it, since it appeared that Ceres' apparent diameter increased far too fast.

The following tables indicate

Date 1802	*1*	*2*	*3*	*4*	*5*	*6*
25 Jan.	*1.9029*	–	*2″.514*	–	–	*von0″.000*
26 Jan.	*1.8927*	*2″.527*	*2″.687*	*+0″.160*	*+0″.160*	*bis0″.160*
28 Jan.	*1.8723*	*2″.554*	*2″.795*	*+0″.081*	*+0″.040*	*−0″.241*
31 Jan.	*1.8421*	*2″.596*	*2″.930*	*+0″.095*	*+0″.031*	*−0″.336*
5 February	*1.7948*	*2.665*	*3″.468*	*+0″.467*	*+0″.093*	*−0″.803*
10 February	*1.7525*	*2.729*	*3″.548*	*+0″.016*	*+0″.003*	*−0″.819*
12 February	*2.7368*	*2.754*	*3″.621*	*+0″.048*	*+0″.024*	*−0″.867*

[Ed. notes on table: the value of 2.7368 is a typographical error in Schroeter's book; it should be 1.7368. In column (2), the last three entries are missing the ″ sign in the original. This table purports to show an increase in the size of Ceres, due to its atmosphere; thus the results given are completely spurious. Column headings: (1) distance of Ceres from Earth; (2) according to this the apparent diameter in proportion to the x measurement should be, where x means first, second, etc.; (3) it was found by measurement; (4) apparent diameter increase; (5) daily apparent increase; (6) sum of the diameter's apparent increase; von = from, bis = to]

19

At viewing these tables it shows that after the 1st and 4th column the apparent diameter increased steadily, that is daily 0′.061 or a good half decimal of a second. These measurements were unbiased, since the product was never surveyed.

In case of any kind of delusion it would be absolutely impossible to measure such a regular increase. In the 4th column of the previous table the gradual increase was always found to be in decimals of the same second, e.g. on January 26, 2″.687 and January 28, 2″.795, and this without any previous calculation. The difference amounted to 0″.008. If the diameter would have been only 0″.220 according to the English measurements, then the difference of the increase would have been only 0″.008. This would give the refutation to the sentence that I am able to measure 8/1000 part of a space second.

This precisely measured increase in diameter and the simultaneously observed sharply outlined disc of Uranus ensure that the instruments were in perfect condition.

After these observations and measurements it is certain that such a peculiar increase would not apply to the solid nucleus itself, but only to the surrounding atmospheric fluid which became more visible over 18 days by 0″.867 than it should have been according to the distances from Earth. It appears to be as certain as with the comet of 1799.

20

After these comments I would like to follow up on the observations and measurements. On February 18 due to an almost full Moon at a distance of about 23° 17′ from Ceres, her less dense outer layers of atmospheric nebula were not recognisable. Therefore her whole apparent diameter should have decreased considerably, instead it increased further. With 136 and 288 times with the 13-foot reflector she appeared cometlike, boundless and with an occasional brighter centre. She followed at about 13 to 14 minutes a star, besides in the position in which Ceres was located according to RA and DEC there was no star visible with which she could have been confused. With the disc micrometer her hazy disc appeared with 288 times considerably smaller than the two discs of 2.33 and 2.50 lines. The unknown distance of the micrometer from the eye amounts to 611 lines. I added to it of the difference of 2,500 lines and 2,800 lines = +0.100

L. 2.600 Lin.	*=3.4149733*
–L. 611.000 Lin.	*=5.7860412*
	7.6289321 = 14′ 38″
$= \frac{878''.000}{288} = 3''.048$	

When the difference of +0.100 is omitted from this calculation then the apparent diameter amounts to only 2.930 seconds.

21

From this can be confirmed Ceres' atmosphere, according to theory, slopes to less and less dense layers, and that the thinner layers are not visible with the same telescope in proportion to the Moon's light. On February 12 Ceres' distance from Earth was 1.7368 and her diameter was found to be 3″.621, which is a bit small due to the Moon's light. However today the distance amounted to only 1.6939 and without any increment her diameter should have been 3″.712. But it amounted to only 2″.930. Uranus and the other planets showed no such changes. The moonlight factors in the greater or lesser visibility of the nebular layers and therefore the planets' apparent diameter changes are shown in the table of chapter 18. On January 27 the Moon was in the last quarter and on February 9 again the first quarter, and it was only logical that as Ceres came closer to Earth her diameter increased far more in the dark nights from January 25 to February 5. This was in fact the case. In the 7th column of the table increased the peculiar proximity up to 0″.803. Therefore it should have been increasing from February 5 to February 12 inclusive 0″.501. But in fact it increased only 0″.064, and at the time when the full Moon was near Ceres, her diameter was again 0″.691 less than at the last observation.

22

On February 26 and 27 we observed Uranus and measured him on the last evening. I estimated him at 288 times to be almost at 3.333 lines but in comparison with one to ¼ of the difference smaller than 3.333 lines. Namely the middle of the difference 0.155 lines smaller. Harding estimated the disc ¼ of the difference only 0.133 lines smaller. According to my estimate Uranus' diameter averaged 3,178, but the distance from the eye was 585 lines. Therefore Uranus apparent diameter was 3″.892. Unfortunately we were out of luck with Ceres on that night.

23

On March 5 Harding located Ceres among many small, only partially known stars and I found her at 186° 0′ RA, and 16° 0′ north DEC. She appeared at 9 o'clock 16′ mean time very small and insignificant, so it was impossible to see her with any kind of enlargement of the 13 foot telescope as a planetlike disc. Occasionally a lighter nucleus sparkled from her centre, but too small to do any measurement. If it was not for her position, she could have been mistaken for a fixed star. That too proved the changing modifications of her atmospheric nebula.

24

On March 6 at 7:30 pm I found Ceres at once with the 13 foot telescope. At 136 times she appeared again as a little planetary disc of a pale reddish light, but very nebulous, almost like a comet. At 288 times and fully opened the reddish light changed somewhat to whiter light. Yet, the nebula was still very visible. At 11 pm I adjusted the disc micrometer until the planet with his nebula was equal to a projection disc of 3.333 lines. At this enlargement (288 times) it sparkled again, like yesterday, a much brighter nucleus. To test the characteristics of the nebula and the nucleus I placed the planet against an illuminated background of the projection micrometer. The planet appeared then not exactly 2800 lines. The distance of the micrometer from the eye was 621 lines. Therefore the apparent diameter of the planet including its whole nebulas amounts to

L. 3.333 Lin.	=3.5228353
–L. 621.000 Lin.	=5.7930916
	7.7297437 = tan 13′ 27″
$= \frac{1107''.000}{288} = 3.843$	

25

If one compares the planet's picture of today with the one 24 hours ago, where it was insignificant and more like a comet, then such a peculiar difference can not be explained either from a 22, 24 or 26 hour rotation, nor from a 12 hour rotation, because the manifold changes of our previous observations seem to contradict. The thinking scientist will rather recognise the manifold changes of these two observations and the one from January 25, which in comparison to the other known planets shows a much more dense and extensive atmosphere, like the one we have seen in the Comet of 1799.

With good reason I believe that the changing modification of that much more expansive atmosphere in which the sphere is shrouded will cause much difficulty in discovering a rotation period.

I will have to admit that those who observed this planet with excellent telescopes but small apertures and low intensity of light will not be able to compare my observations, because they see the planet with sharper boundary, thus smaller and without or with little of the atmospheric extension, so that they are bound to blame these telescopes for the optical delusions. But why can one see with the same telescope with large aperture and light and at the same time and thus with the same modifications of our atmosphere Uranus so beautifully, which has the same apparent size as Ceres?

Does this not only show that it is only the light intensity of larger telescopes that gives us the opportunity to see and distinguish such a slight atmospheric nebula, which is not possible with weaker telescopes I observed at viewing the most recently measured diameter of the surrounding nebula that the measurement was taken on a dark night, which in relation to the measurement on February 18 at full Moon, the diameter now again fell into the old line of measurements, 0″.795 again larger. This proves the influence of the moonlight and the sloping fineness of the nebula.

On March 6th Ceres' distance from Earth was 1.616 AU and thus she should appear to be 2″.960 in proportion to the first measurement. But, she was found to be 3″.843 in diameter and thus the resulting increase in diameter is +0″.883. According to the tables in chapter 18 its increase shows in the 7th column already 0″.867 on February 12, after 18 days. The error in the measurements was thus this time insignificant, and the diameter did not further increase from February 12 up to March 6 in 22 days. It appeared that soon a limit was reached, for the light intensity of the 13-foot reflector due to the developing visibility of the expanding nebula which was caused by the approximation. This was subsequently shown.

26

Also on March 6 we observed Uranus at the same time with the same 13-foot reflector to point to the different properties of Uranus and Ceres. I could see Uranus with the naked eye whereas I could not see Ceres and the nearby fixed star. Uranus appeared with the reflector to have a sharp boundary and compared with Ceres looked like a little Jupiter. Only with the difference that we could not see on Jupiter any flattening on this observation or the previous one. These strikingly different natural proportions which one can see on Ceres only with great light intensity cannot possibly be an optical illusion of the reflector since two observers of different visual faculties perceive identically. In order to measure and compare the diameter of Uranus he appeared with the same distance of the projection micrometer with which we measured Ceres' diameter, namely 621 lines from the eye, about the same size as a disc of 3.333 lines. This with 288 times. But not just as large, namely 0.109 lines smaller, which resulted in a diameter of 3″.739.

27

At this observation we again observed through the nebula Ceres' sparkling nucleus. Thereby I want to remark on a precise determination of the diameter of Ceres without her atmospheric nebula. As remarked in paragraph 24 Ceres appeared 3.333 lines, projected on a dark background, including her nebula. But projected onto a light background the largest part of the nebula disappeared. Only the nucleus and more dense nebula remained. Its size was not fully 2.800, but an average 2.600 lines. The diameter including the whole nebula amounted to 3.333 lines or 3″.843 seconds; the apparent diameter of the nucleus amounted to 3.600 lines or 2″.997.

I compared this measurement with the one from January 25, namely the proportion of the apparent whole diameter of that time to the sphere. Then the whole diameter of sphere and nebula was 2″.514; the sharply bounded sphere alone was only 1″.830. Now however the whole diameter was 3″.843. Now is these two distant measurements were accurately done, then 2″.514 of the whole diameter is to 1″.830 of the sphere's diameter as now 3″.845 of the whole diameter is to 2″.797 to the sphere's diameter. But the present measurement revealed 2″.997 and the whole difference is only = 0″.200 or 2 decimals of a second more. This result had to be, since then only the sharply defined disc without any nebula was measured, but now included a small part of the more dense nebula. This led to a second companion, namely: the proportion of the distances of Ceres to Earth to the measured apparent diameter of only the sphere without nebula.

Since the atmospheric nebula was measured in the progressively larger extension as Ceres came closer to Earth, and therefore the whole diameter was also found progressively larger than it was according to the common ratio. The presently measured whole diameter of 3″.843 was caused by the more and more visible layers of atmospheric nebula. Now, if the ratio found from January 25 to March 6 of the diameter of the sphere to the whole diameter including nebula is correct, then vice versa the distances from Ceres to Earth have to be the same. Also vice versa the whole diameter + the peculiar increase of same as per measurement from January 25 had to be the same for the whole diameter measured March 6. This was a very difficult and delicate trial, which was successful because on

January 25 the distance of Ceres was 1.9029, the measured diameter of the sphere 1″.830, and the whole diameter including the nebula 2″.514. On March 6 the distance was only 1.6109, diameter of the sphere was 2″.997 and with the nebula 3″.843, and the sum of the progressive apparent increase of the whole diameter 0″.883.

28

If the knowledgeable person checks these comparisons with the tables of paragraph 18 then he will see that:

1) *The apparent diameter measured on January 25, 1802 was of a small body, and was as accurate as possible.*

   a) *Both measurements were 40 days apart from each other, and at neither these nor any other measurement taken in between as per paragraph 20 was it possible to know the result beforehand.*
   b) *Ceres came in these 40 days closer to Earth 0.2920. Not only because of that but mainly because of the progressively increasing visibility of the less dense layers of nebula her apparent diameter increased over. But, both distance and the peculiar increase of diameter were included in this comparison and still, both results correspond so unexpectedly.*
   c) *All measurements in between as per paragraph 18 harmonise with the above: the increase of the diameter was partly the result of the approach to Earth and partly because of the increasing visibility of the outer layers of nebula but it was always correspondingly progressive.*
   d) *Most measurements were taken by two observers with different eyesight, and both their measurements and notes compare totally or with only very slight differences, which exclude any optical illusions. The true diameter of Ceres and her atmospheric nebula can be determined, as it has been done (see para 62).*

29

2) *I myself had been unsuccessful in measuring either the atmospheric nebula or the disc itself with out 10-foot achromatic, parallactic telescope of Peter Dolland and 3* $\frac{9}{10}$ *lines opening. Only with the 13-foot reflector was it possible to differentiate and measure the nebula. Many observers have measured the diameter of the planets from Jupiter to Mercury and Uranus and their measurements were very similar, despite different visual faculties. These small measurement errors are normally only* $\frac{1}{14}$ *of the diameter at the most. But these errors are not larger when smaller objects are measured, but rather smaller, since it is more difficult to estimate larger objects than smaller ones. Only theory combined with true practice can decide here. Our measurements of Mercury and Uranus with the Herschel and other telescopes were always correct. Also, the measurements of the Jupiter satellites, done by three observers with various visual faculties and instruments were correct, within a few insignificant hundredths of a decimal of an arc second. Indisputable are the corresponding determinations of notable small planet-diameters like Ceres and Pallas. The later well-known measurements of a fourth observer, namely Herschel, who has again different visual faculties and used different instruments, harmonized well with ours. A theorist would never think that our measurements were not obtained from a strong irradiant fixed star which has a very small diameter of 0″.20 but that we only apply measurements to planetary bodies which appear in a soft light. Also the light of Ceres and Pallas is yet much paler than any other planet, even the little Jupiter satellites.*

30

On March 7th at 9 pm Ceres appeared again in reddish light with strong nebular boundaries. This both with 136 times and 288 times. With the 13-foot reflector, in the viewfinder, she appeared larger and more eye-catching than a nearby star of magnitude 7. With the disc-micrometer I found her to be 3.333 lines at 288 times and therefore the unknown distance from the eye 602 lines. I compared again Uranus, which was as always in weak light but more pale than usual. As per above measurement the calculation for the whole diameter of Ceres including the nebula

L. 3.333 Lin.	=3.5228353
–L. 602.000 Lin.	=5.7795965
	7.7432388 = tan 19′ 2″
$= \frac{1142''.000}{288} = 3''.965$	

It showed that the peculiar increase of the visible nebula remained steadily progressive.

31

On March 8th slight fogginess made observations difficult. Ceres appeared again in reddish light, like a comet more than the known planetary nebula at Aquarius. At about 9 pm I found at 288 times that the whole extension of the nebula was visible in clear atmosphere. Ceres' picture on dark background projected was as large as a disc of 3rd category of 3.333 lines. In comparison with a disc of 2nd category it was at most only a difference of 1/6 of both discs.

If Ceres was visible through a slight fog then her outer nebula disc appeared and she appeared as a disc of 2,333 to 2,500 lines of 1st category with an average of .2416 lines in diameter. One could see the nucleus. The distance of the micrometer from the eye was 589.5 lines.

1) *Therefore according to the correction in which Ceres was slightly smaller than a disc of 3.333 the apparent diameter is*

L. 3.333 – 0.089 = 3.244 Lin.	=3.5110808
–L. 589.500 Lin.	=5.7704838
	7.7405970 = tan 18′ 55″
$= \frac{1135''.000}{288} = 3''.941$	

2) *If the picture of Ceres was as large as 3.333 lines then*

L. 3.333 Lin.	=3.5228353
–L. 589.500 Lin.	=5.7704838
	7.7523515 = tan 19′ 26″
$= \frac{1166''.000}{288} = 4''.048$	

The average of both results even with a one decimal second is also 3″.994. The brighter nucleus amounts to 3″.037. After 11 pm when the observation was repeated (with 288 times) Ceres appeared more clear, but I could not see her with the naked eye, although Uranus, smaller but bright, I could see immediately.

32

On March 14th at 9:15 pm we observed Ceres with the 13-foot reflector, again in reddish light. She was more clearly visible than the last time, even though she was shrouded in a slight fog. Harding tried repeatedly to observe her with our 10 foot Dolland, but even with 500 times he could not distinguish her from a fixed star and see her as a disc.

33

At the same time I measured Ceres' diameter with the 13-foot reflector. I set the disc micrometer back and slid it farther and farther from the eye until I found the planet with his nebula with 288 times at a projection disc of 3.333 lines, and the distance of the micrometer from the eye was 600.0 lines. The calculation

L 3.333 Lin.	*=3.5228353*
−L. 600.000 Lin.	*=5.7781512*
	7.7446841 = tan 19′ 6″
$= \frac{1146''.000}{288} = 3''.979$	

34

On March 14 at 8 h 37′ 54″ Western time Harding determined Ceres' apparent ascent at 184° 16′ 46″ and the apparent northerly dec. to be 16° 52′ 32″.

35

More peculiar was the result of Harding's repeated investigations, that with the best dioptric telescopes of 4-inch openings, Ceres' atmospheric nebula could not be detected, and that the planet could not be distinguished from a fixed star with any enlargement. This was in fact the case with Maskelyne, Zach, Olbers and all other astronomers, who observed with dioptric telescopes. I already pointed out in para 17 that a large opening and lots of light is required. This is evidenced by simultaneous observations of Herschel and von Hahn [Count Friedrich von Hahn], *who both saw Ceres' nebula as well as we did. Our Dr. Olbers can, among others, attest to the excellent virtues of telescopes with large openings and light intensity in studies of all fixed nebulas. Olbers compared some nebulas seen through our 10-foot Dolland and then with the 13-foot reflector. It is really amazing that one cannot see even the slightest trace of the Orion nebula's fine layers. However, they are clearly visible with the 13-foot reflector.*

36

The one doubt remains: why, if Ceres' atmospheric nebula could not be seen through achromatic telescopes when the nebula of the smallest comets can be seen with the insignificant dioptric viewfinders? Every planetary and comet nebulas' visibility depends on the size of its extension. The larger the extension the more dense the nebula around the nucleus which according to the law of gravity declines to less dense layers until it is not visible anymore. Therefore its density on the surface depends on the nebula's extension. The more dense a nebula is on the nucleus of a comet the more light it is able to diffuse. The light might be only reflecting Sunlight. Experience shows that the nebula of any comet is of a large extension in proportion to its nucleus and is often of rather great density and strong light intensity near the nucleus. If we compare now the extension and density, intensity of light and the visibility of the atmospheric nebula of Ceres with the one of any small comet, e.g. the one from 1799, then the extension on January 25 and March 6, 1802 only to 1″.25 and the vertical light only 0″.35. But at the comet of 1799 its smallest radius, observed August 30, amounted to 51″.50 and from the surface of the nucleus which is only 3″.75 in diameter the amount was still 49″.64. How small is now the extension and the density and visibility of Ceres' atmospheric nebula in proportion to a comets' atmosphere.

37

On March 20 at 9 pm we observed Ceres again and her pale light distinguished her from other fixed stars with help of the achromatic finder of the 13-foot telescope. At 136 times and 288 times she appeared as a soft planetary disc despite the almost full Moon only about 28° from her. She was noticeably brighter than at the last observation on March 14. She appeared not in reddish light but in white light. I measured her with the projection disc being still set at 3.333 lines with which I measured her on March 14. But she appeared larger and I had to bring the micrometer closer to the eye. I found her to be 2.50 and 280 = 2.65 lines and the distance of the micrometer from the eye = 581.5 lines.

38

That Ceres' light and visibility was stronger compared to previous observation was certain because I compared it to Uranus. He appeared this time quite pale, which was unusual. I found Uranus' diameter 3.227 lines with 28 times and he appeared to have sharp boundaries. Ceres, however, outside of her faded light nucleus was very vague and comet-like.

39

Diameter

1) *of Ceres*

   a) *including the whole of its visible atmospheric nebulosity*

L. 3.333 Lin.	*=3.5228353*
–L. 581.500 Lin.	*=5.7645497*
	7.7582856 = tan 19′ 42″
$= \dfrac{1182''.000}{288} = 4''.105$	

   Thus

   b) *for the blurred nucleus without the outer nebulosity only 3″.263;*

2) *the diameter of Uranus however which I found on March 6, 3″.843, now 3″.974.*

   If one compared this diameter of Ceres with the one on March 14, the difference is only +0″.126, only $\frac{1}{33}$ *of the diameter and this proved again the accuracy of the previous measurements. Likewise correct is the diameter of the nucleus which was found to be 3″.342 on March 14. On March 20 11 h 36′ 55″ W time, Harding found the apparent RA to be 182° 59′ 56″.*

40

On March 28 about 9 pm Ceres appeared through the 13-foot telescope planet-like, larger but with pale whitish light. I found Ceres with a disc of 3.333 lines at 288 times at 594 lines from the eye. But when her atmospheric nebula was invisible due to some passing clouds, her nucleus appeared only as a disc of 2.333 to 2.500 lines with an average of 2.416 lines. Therefore the calculation

L. 3.333 Lin.	*=3.5228353*
–L. 594.000 Lin.	*=5.7737864*
	7.7490489 = tan 19′ 18″
$= \dfrac{1158''.000}{288} = 4''.021$	

Harding found the apparent RA of 8 h 30′ 26″ true time to 181° 19′ 27″ and the apparent N DEC 17° 52′ 26″.

41

On March 29 at 7 h 45′ no measurement was possible, but Ceres' light was still pale and whitish. No big changes from last observation.

42

These are all our observations and measurements of Ceres in 1802, since Olbers discovered on March 28 the second new planet Pallas. His measurements and observations superceded Ceres observations.

43

Just now, November 30, 1804, I received from Gauss the calculated distances of Ceres, Pallas and Juno from Earth. The following tables do not contain measurements from February 18.

Date 1802	*1*	*2*	*3*	*4*
25 Jan.	*1.903*	*–*	*2″.514*	*–*
12 Feb.	*1.736*	*2″.754*	*3″.621*	*0″.867*
6 Mar.	*1.616*	*2″.960*	*3″.843*	*0″.883*
7 Mar.	*1.613*	*2″.966*	*3″.965*	*1″.005*
8 Mar.	*1.611*	*2″.969*	*3″.994*	*1″.025*
14 Mar.	*1.604*	*2″.982*	*3″.979*	*0″.997*
16 Mar.	*1.602*	*Ceres closest to Earth*		
20 Mar.	*1.605*	*2″.962*	*4″.105*	*1″.143*
28 Mar.	*1.621*	*2″.951*	*4″.021*	*1″.070*
			Mean 3″.984	*Mean 1″.020*

44–46

Detailed explanation of tables.

(1) *Distance of Ceres from Earth*
(2) *According to this the apparent diameter in proportion to previous measurements should be*
(3) *it was found by measurement however*
(4) *Apparent strange increase of the diameter*

In order to give the experts the opportunity to thoroughly compare the observations of me and Harding, the paper of Herschel "Observations on the two lately discovered celestial bodies" may be useful.

Herschel states that

1) *The many measurements of small objects correspond so well with the ones made by the great scientist of the universe, Dr. Herschel. Among many I want to note especially the measurements of Sidus Georgium, the faintest double stars, the extension of Venus' atmospheric light, and the diameter of the small satellites of Jupiter. All these mutual measurements were made with the same method as was done with Ceres and Pallas.*
2) *That Herschel observed these two new planets in the same way as we did and concluded the same as we did.*
3) *Therefore it is even more peculiar that with such consistent observations the diameters of both planets differ so much. English measurements (pg. 6 of Herschel's article) show Ceres only at 0″.351 and Pallas 0″.319. But these present measurements give for Ceres*

3″.482 (see para 62) and for Pallas 4″.504 (see para 95). This makes Ceres 10 times and Pallas 14 times as large.

47

Nobody will doubt that this most exceptional deviation of otherwise consistent observations cannot be found in the telescopes or the visual capabilities of the observers. But at the same time nobody will doubt that this uniquely rare deception happened with the English measurements. And that it is rather impossible to explore and observe such small, dimly illuminated, and in fog shrouded planetary objects of 0″.40 and 0″.22 diameter, but this in fact was made with greatest conformity with the exception of the dimensions. Moon craters and mountains of 0″.22 and 0″.13 I could not 100 % distinguish even with the best telescopes, but Herschel found Ceres and Pallas to have such a diameter. I also have my doubts that such a small "dot", which is also shrouded in fog and very dimly lit could be seen from such a distance of 1.634 and 1.833 even with the best telescopes. Herschel distinguished also the disc from the nebula (see section 9 and 10) observed, as we did, the peculiar atmospheric changes, compared it with the coma of a comet and concluded (see section 12 nr. 6) just like we did, an atmospheric expansion which was out of proportion to the diameter of the solid body. To observe all this was downright impossible if not both planets in their distances from Earth at that time were at least 2″ to 2″.5 in diameter.

48

That both planets must have had such a large diameter at the Slough Observatory is clear to any experienced observer for the following reasons:

1) *We compared continuously with Uranus.*
2) *Harding compared Ceres with the 1st satellite of Jupiter on January 11, 1802 at 17 o'clock, before an exact measurement was taken, and he found Ceres to be at least twice the size of the satellite. But, according to section 222 and the enclosed tables of the 2nd volume of my articles, the diameter of the 1st satellite of Jupiter amounts to an average of 1″.40 with projections = measurement of 1″.39 with certainty. Without Harding knowing what our measurements revealed, he found Ceres' diameter with the help of such a comparison to be about 2″.60 to 2″.80. Without thinking about this comparison, the 1st measurement of January 25 resulted in 2″.51, and in the period of 62 days the diameter increased steadily and progressively.*
3) *How valid this is is also shown here. If Ceres and Pallas were really only 0″.22 and 0″.17 in diameter then they would have appeared 6 and 8 times smaller in diameter, and their visible disc areas would have appeared 36 and 64 times smaller than the 1st satellite of Jupiter. Such a small, matte and fog-shrouded dot, which would be smaller than the smallest of the old satellites of Saturn, would not be visible with the best of viewfinders and would only just be recognised with its nebula with very powerful telescopes. But we found both planets with the viewfinder of the 13-foot reflector with only 13 times. In comparison, the satellites of Jupiter appeared at the same time only as tiny dots, and the two planets appeared at least twice the size of the 1st satellite of Jupiter. Neither planets have the extraordinarily strong light of fixed stars.*

49

Following all these convincing reasons, occurred an extremely rare and unusual deception of the English measurements.

When this honourable observer had the kindness to communicate his very valuable article through the royal Prussian Privy Councillor Huth, who just returned from England, I noticed after careful examination that:

1) Ceres was measured 3 times, Pallas only once, and those measurements were not repeated under different circumstances. I am convinced that the deception, which crept in on the measurements of April 1, 21 and 22, happened.

a) *On April 1, Ceres' diameter was found to be only 0″.40 at 370.42 times of a 7-foot telescope. On April 21 only 0″.38 diameter at 516.54 times of a 10-foot telescope, and on April 22 only 0″.22, almost half the size of April 1st.*
b) *If such an enormous difference is possible, then it is certainly the result of an error in the method of measurement. Therefore every expert will agree that nothing can be said for certain with a measuring method which allows such a deception. At one time it shows the truthful results, the other time discouraging discrepancies.*
c) *Such feelings were also expressed by our honourable observer.*
d) *The measurements of Pallas on April 22, which amounted to only 0″.13, could not improve the error, since the reason for it was the method.*
e) *Despite the measurements of Ceres from April 1 to 21, which resulted in a diameter almost twice as large and therefore showed an obvious error in the method, nobody showed consideration for them. It was not the average of all three results taken (0.40, 0.38 and 0.22) but only the sole results of April 22 were taken as a basis for the apparent diameter of Ceres, namely 0.22 and 0.13 for Pallas.*

51–52

Therefore these articles of Herschel, who otherwise produced very valuable observations, contain nothing in respect to the measurements since the result of only one measurement of Ceres and Pallas and only of one day, April 22, were taken.

At my perusal of the translated articles, I noticed at once that the honourable author applied the projection disc at far too great a distance from the eye. I was always convinced that no micrometer should be placed any farther from the eye than it is possible to read some average large script. Namely for farsighted eyes 5 to 7 feet. Someone with shortsightedness should place it at an appropriate distance and use with the comparing left eye a lorgnette. The thought that one gains greater measure for small heavenly bodies by placing the projection micrometer farther from the eye is plain deception, because a small object can be much easier and more sharply measured with a small scale hand adjusted to ones' visual faculty than with a large one. The greater distance of a projection micrometer one chooses, the more one will be exposed to optical deceptions, and the more difficult and unfavourable will be the measurement itself, because:

a) At a larger distance it is difficult and takes time to hold the micrometer at a right angle with the Newtonian telescope. My projection machine instead can be quickly inserted into the telescopes' right angle, and remains steady all the time. b) It is difficult to align the micrometer exactly to the point at which the e.g. projection disc appears identical in size with the enlarged picture of the observed object. For a single observer it takes considerable running back and forth, which does not aid in an exact measurement.

α) According to the articles of Herschel he used to measure Ceres on April 22 an illuminated projection disc of 1.4 inches diameter which he gradually increased to 1,942 inches or 161 feet, 10 inches. This is an enormous distance from the eye.
β) At the only measurement of Pallas the same evening the disc was 2136 inches or 178 feet from the eye. The perfect picture of Pallas was found which was in proportion to the disc like 2 to 3. But when Pallas appeared in her smallest size the ratio to the disc was only like 1 to 2.

53

In connection with the deception of the micrometer at a large distance there is another main deception to debate. To my knowledge nobody thought about this, and I myself discovered it only after repeated thinking. The following explanation will please Dr. Herschel. All of a sudden I remembered the known truth that the farther one places a shining object (eg a lamp) from the eye, the larger it appears through its irradiation than it actually should. Take for example the Moon. He may appear to the naked eye at various sizes, due to similar deceptions. But at any elevation with an appropriate micrometer it is the same size.

However, with an illuminated to far distanced projection micrometer, the Moon is not seen with its true diameter, but with a notably larger diameter which appears, despite irradiation, like the true diameter of the disc itself.

54

This thought was quite disturbing to us, since I managed numerous observations of the Moon and others with illuminated projection micrometers. The worst thing about this was, that it is not always possible to account for such irradiation and deception, since the decreasing diameter of the projection has to appear proportionally larger. Not only if the distance but also the degree of illumination is greater, which is not easy to determine and on observations which have already been made, both 100 % determinable.

55

While having these uneasy feelings, I thought of a way to explore the proportion of the deceiving, larger diameter to the smaller, true one. I used a projection disc of 1.4 inches—the same with which Herschel measured on April 22, 1802 Ceres and Pallas. On October 12, 1802 I illuminated the disc, which was made of thick, bluish-white Dutch paper, in such a way that it appeared in about the same dim light as Ceres and Pallas. Then I used for my right eye a little tube, located at the sextant, with a normally small opening but without glasses. The left eye, however, I kept open as it is done at measurements. With this light equipment I positioned myself on a vertical line against the illuminated disc, about 150 feet from it, and observed it in two adjacent pictures, also with both eyes naked. Despite the dim light of the projection disc I saw her at this considerable distance with my left eye as a really sizable, well defined disc. With the right eye, however, through the sextant tube, surprisingly I saw her only as a little bright, roundish dot. Conductor Blohm, who was present at this time, also observed this deception and convinced himself of it. I repeated the experiment on October 13 at 5:45 am at full Moon but cloudy sky. Despite the Moon's light and the coming dawn, I saw the illuminated disc through the tube with my right eye only about 1/3 in diameter than with my left eye. I judged that the difference should be even greater at greater darkness, when irradiance is stronger. I measured the distance of the illuminated disc from the eye and found it to be not more than 137 feet or 1,644 inches. Since Herschel distanced his projection disc on April 22 on Ceres to 162 feet and on Pallas to 178 feet, and therefore, at such a large distance, the deception be still greater, I measured from the 1.4 inch disc the distance of 178 feet (his last applied distance) and continued on in the evening. At this distance I saw with my right eye through the tube the illuminated disc barely 1/5 in diameter as with the naked left eye, with which I saw her still as a round, well-defined disc. Therefore I saw her 5 times as large in diameter as I actually should have seen her. Now I came closer to the disc. The difference between the size of both pictures became less and finally at 8 feet distance both pictures became completely identical and fell into one picture. To be absolutely sure I repeated the experiment, changed the tube from one eye to the other. But it was confirmed and stayed at the same proportion, so that I concluded that Pallas' diameter was at least 5 times smaller as the one found at Slough.

56

This experiment revealed also the important knowledge that: regardless of how farsighted I might be, an illuminated projection-micrometer may not be placed farther than 8 feet from the eye. Any larger distance of such micrometers result in a deceptive diameter. But the most unfortunate fact is, that the variety of visual faculties must result in various deceptive results at the same circumstances at such a great distance from the eye.

57

Fortunately I did my observations from the first years entirely within the range of 3 to 7 feet, so that the measurements of my entire 20 years were within uniformity of measure. It would have been impossible to correct these measurements afterwards.

58

It becomes peculiar if, with this knowledge, one now compares the article "On the Diameter and Magnitude of the Georgium Sidus, with a Description of the dark and lucid disk and periphery micrometers" by Herschel, November 7, 1782. The very sharp-witted author invented then the illuminated disc-micrometer in order to better measure the diameter of the new planet, discovered by him. Soon he noticed that two identically-sized discs at the same distance side by side, but one stronger, one more weakly illuminated, appear larger and smaller when compared with each other. Even though the illuminated discs were placed at a reasonable distance of 49 and 57 feet, the great observer noticed at once that the disc-micrometers showed the diameter to be too small. Because his lamp-micrometer resulted in a diameter of 5″.06, the thread micrometer of 5″ 11″ and 4′ 11″ with an average of 4″.36. But the disc micrometer showed only 3″.63 and 3″.67 with an average of 3″.65, and therefore was 1/6 smaller. The honourable author noted the discrepancies caused by the light and irradiation.

59

Due to lack of time and the not yet disclosed observations of Saturn's rings I did not repeat my experiments with any other heavenly body. I can only refer to one experiment which I managed on Juno (discovered by Harding). On September 9, 1804, I found Juno's diameter 2″.611. On September 14, I used the 13-foot telescope, parted a vertical line and placed a machine with a projection disc of 3.5 inches diameter. Measurement was done at 288 times. It took a long time until I could project Juno's image. I estimated her diameter only 2/3 or 36/100 of the diameter of the illuminated projection disc—within 1.2 inches. The distance of the projection from the eye I found 1,720.0 inches, or 143 feet 4 inches. At once Mr. Harding and I went to measure with the projections machine

1) *with 136.35 times and found her to be not quite half as large as an illuminated disc of 2 lines, and not as large as a disc of 1 line but an average of 0.8 lines. The unknown distance from the eye was found to be 459 lines, within only 45 inches and 9 lines, not even 4 feet.*
2) *At 288 times with the same 13-foot telescope I found Juno's disc on average larger than a projection disc of 1.5 lines, and smaller than 2 lines. Average of both sizes 1.75 lines in diameter. The unknown distance from the eye 519 lines, within only 4 feet 3.9 inches. Therefore the calculation*

0.8 Lin./459.0 Lin.	*= tan 6′ 0″/136.35*	*=2″.640*
1.75 Lin./519.00 Lin.	*= tan 11′ 35″/288*	*=2″.413*

But a measurement with a large projection disc of 3.5 inches and at a distance of 1700 inches results in 1.2 inches/1720 inches = tan 2′ 24″ = 0″.50. The small size of only 0″.50 just about agrees with Herschel's from 1st April with an even larger distance from the eye, namely 2131 inches of 0″.40 of Ceres.

60

I am convinced that the honourable author applied too high a magnification namely on April 1st on the 7-foot telescope of only an opening of 6.3 inches at 370×. On April 21 and 22, he used a 10-foot reflector with 516×. We never used with our 13-foot reflector a magnification greater than 288×. On the Comet of 1799 I used the 13-foot reflector with only 80 to 136×, and with the 27-foot only 160 to 170×.

61

I would like to note that it is impossible to recognise a planetlike or cometlike heavenly body shrouded in nebula with the best achromatic viewfinder and larger telescope if it is only 0″.13 to 0″.40 in apparent diameter. This fact I proved already in section 47. An actual experiment of this is the following: Fixed stars of magnitude 1, which at most have a diameter *of 0″.33 are not visible because of their diameter but only because of their irradiating a lively light. Nine years ago I experimented with green soft Dampfglasern (glasses that mute or soften light) on fixed stars of magnitude 1. I applied such a glass to the naked eye and looked for Aldebaran and he disappeared completely. With such a glass in the 10-foot Dolland with only 60×, the especially bright Arcturus appeared as a very tiny dot. With a slightly stronger damping green glass he disappeared completely. That small are the diameters of fixed stars of mag. 1. If then the 3 new planets really have such small diameters as the English measurements state, they should then disappear immediately with the use of even the lighter green glass. On September 10 of this year the newly discovered Juno appeared with two Lalande fixed stars of mag. 8 at 100× of my 5-foot telescope with 6.5 inch opening. This presented the best opportunity for the experiment. We applied the lightest green glass that we had. The fixed stars appeared through it not even half as bright as the planet which kept its diameter. Then we applied a more muted but very light violet glass. Through it, the two fixed stars disappeared completely, however, the planet appeared matte in its eye-catching size as a true planetary heavenly body. Since the planet has only reflected light, not the fixed star or Sunlight, it is a definite proof that the fixed stars, reduced in their diameter to a fraction of an arc second, became invisible. The planet, however, despite its more subdued light, remained visible due to its own size. The planet, therefore, has a diameter of 2″.526, because if it were any smaller in diameter, it would have become invisible. Another obvious example can be found in sections 121 to 125.*

62

As per section 3 the apparent diameter of the solid sphere of Ceres was on January 25, 1802 1.830 arc seconds. Later determinations of March 6 and 28th justify this. But according to Gauss' Elements, if the average distance of Earth to the Sun = 1, and the distance of Ceres from Earth on January 25 1.9029, 1.0000: 1″.830 = 1.9029: 3″.482 = diameter of Ceres' sphere as seen from the Sun = 3.482 sec. Since the true diameter of Earth is 1719 geograph. miles in avg. distance from the Sun, then, as seen from the Sun = 17″.0 the true diameter of Ceres' sphere amounts to 352 geograph. miles.

63

If one compares Ceres' small diameter with other main planets or satellites then we see that

1) *compared with Earth (1719 miles) then 1719/352 = 4.88, or almost 5 Ceres—diameters amount to one Earth diameter.*
2) *compared with Mercury at 608 miles, 608/352 = 1.73.*
3) *The diameter of Ceres' sphere comes close to that of the Moon with 468 geographical miles and the 2nd satellite of Jupiter 465, since the 1st, 4th and 3rd Jupiter moons are markedly larger than Ceres' diameter, namely like 564,570 and 818 to 352(sic) geograph. miles.*

64

As peculiar the slight ratio of proportion of Ceres is to other main planets, the more peculiar are the conditions of her nebula. All the observations result in the conclusion that Ceres' dense atmosphere is of an extremely large extension and density compared to all other larger main planets and satellites. This nebula is at its most dense close to the sphere and decreases gradually towards its outer boundary, just as in the comets. The same conclusion was arrived at by Herschel.

65

At this pt. I would like to comment on the density of Ceres' atmosphere in comparison to other planets.

1) The size of her extension amounted on January 25 and on March 6, 1802 to an apparent diameter of the sphere of 1″.830; however, the diameter of the surrounding nebula *including the sphere 2″.514. If one subtracts, then only 0″.342 for the vertical height remains from the surface to the outer border. But the true diameter of the sphere amounts to 352 miles, and therefore 0″.342 = 65.7 miles. Ceres nebula was visible up to 65 miles vertical height at that distance from Earth. But as Ceres came closer to Earth from January 25 to March 16, her nebula became progressively more and more visible, so that its extension was shown to be 1″.020 larger. The 6 measurements at that time show her diameter on average = 3″.984. However, the distance of Ceres from Earth was on March 16 1.602. Thus her diameter as seen from the midrange distance of Earth from the Sun was 6″.382 and her true size was 645.33 miles. If now the diameter of 352.09 miles is subtracted, then only 146.62 miles remains for the single extension with the 13 foot reflector recognizable atmosphere from the surface to its outer borders.*

66

It is a peculiar example in our Solar System to have such a small body with an atmosphere of such a large extent. Especially if one compares the atmospheres of Earth and our Moon when seen through dawn and dusk.

2) *One can conclude from known theoretical principles from the size of the visible extent of an atmosphere its density. Since Ceres' atmosphere is at its outermost border a vertical height of 146 miles, we can assume that she, like Earth, Moon and Venus, is capable if refracting rays and to spread dusk and dawn over the sphere. The vertical height at which the atmosphere is causing dawn and dusk amounts on our Earth, according to de la Hire, only to 38,000 Toisen* [the toise was a French unit of measurement. One toise is equal to 2.13 English yards or 1.95 metres] *and on Venus to 7,026 Toisen of lightest twilight. On the Moon, however, only 1,313 Toisen, just like on Earth in dark lunar night.*

67

I would like to comment on the Melanderhjelm theorem, regarding the density of an atmosphere. If the theory is true, then the density dependent atmospheric twilight and the vertical height up to which the atmosphere is able to reflect the Sun's rays, has to be in the same proportion. Therefore, the Moon's atmosphere must not only be on his surface 28.4 times thinner than on Earth (square of 5.33 = 28.4) but in proportion of this thinner density it should reflect the Sun's rays up to 28.4 times lesser vertical height. This vertical height I determined to be 1,313 Toisen, and it goes into the 38,000 T of our atmosphere dawn and dusk, as calculated by de la Hire, really 38.94 times. Hence, theory and practical knowledge arrive at a peculiar harmony. If the M theorem holds generally then one would be able to conclude on Ceres from the vertical height of her density. Unfortunately, this theorem presents a number of theoretical flaws. Therefore I made these comments only with respect to future observations.

68

Generally the observations presented help to survey why Ceres' atmosphere is subject to such tremendous modifications, which are on the other planets much less present or only partially present. Ceres' atmosphere is to some extent similar to atmospheres of comets and shows in her example how diverse creation is.

Chapter 10
British Correspondence About Ceres

The Joseph Banks Correspondence

The complete extant correspondence of Banks as it relates to the asteroids follows, showing what great detail was being transmitted to Banks, and from him to Herschel and others. Most of the letters received by Banks were from Baron von Zach, who kept him fully apprised of asteroid observations made by him and others on the Continent. Zach stayed in London from late 1783 through 1784, where he became well acquainted with Banks, Maskelyne and Herschel. Letters from Maskelyne to Banks are in this section, while other letters to and from Maskelyne immediately follow (Fig. 10.1).

On January 14, 1802, Zach wrote to Banks:

I have herewith the Honour of transmitting to you the Information of the Discovery of the new Planet, Discovered first a Year ago in Palermo by Mr. Piazzi, and called by him, Ceres Ferdinandea.

I detected this planet first, in my Observatory at Seeberg December 7th 1801 at 18 h 48′ 10,″3 Mean time. Apparent Right Ascension = 178° 33′ 30,″6 very exact. Declination 11° 41″½ N. only by Estimation, having not observed the Planet with the quadrant, but only with my 8 feet Transit Instrument.

The 31 December, I saw the planet again, and had the certitude that it had changed its place according to an elliptical motion, that suits with a Planet existing between the orbits of Mars & Jupiter, as was supposed immediately after the first discovery made in Palermo 1 Jan. 1801.

The 11 January 1802, I saw the planet again, and had full Conviction, that it really was the supposed Planet Ceres Ferdinandea. I observed the planet again in the Meridian 11 Jan. 1802 at 17 h 3′ 17,″4 Mean time App. AR = 186° 45′ 49,″95 exact to a second. Declin. N. 11° 10′ by Estimation.

These Positions agree to a half a degree in AR, and to 9 Minutes in Declination, with the account of its Motion, which I have printed in my Journal Monatliche Correspondenz December.

My first observation is printed in my Journal for January, but without knowing then, that this star was really the planet sought for. I take the liberty to send you here the printed sheets thereof.

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2_10

Fig. 10.1 Nevil Maskelyne (Courtesy of the National Maritime Museum, Greenwich. Used with permission)

Dr. Olbers in Bremen, discovered the same planet (not having had any Notice of my Discovery) January the 2.d His observations, made in a gross manner, are as follows:

	AR	
1802 Jan. 2. 11 h 58′ 26″ M. time Bremen	*185º 9′*	*Decl. 11º 7′ N*
——— 5. 17 30 0 ——— ———	*185 43′*	*—— 11 8′ —*

Letters from Paris, from Mr. La Lande, and Mechain, as far as 1 January, mention nothing about this planet, it therefore has not been discovered in France at that time. I can't tell, whether it has been seen anywhere before 7 December.

The planet appears to be a star of 9 magnitude. I looked at it with a magnifying Power of 120, but could not discover the least appearance of a Disc; the planet appears to me rather like a tarnished Star.

I leave it to your favor, whether this Discovery is thought worth your while, to give the Royal Society, the English Astronomers Information thereof. For it is very likely that Mr. Herschel, has discovered this planet already for his own part. Notwithstanding I take the liberty, to send you here annexed an Ephemeris of the Position of this heavenly Body [insert image: The ephemeris for Ceres supplied to Banks by Zach on 14 January 1802], to facilitate the Research, if perhaps an unfavorable sky has not permitted to make the Discovery in England. (Zach, 1802a, his underlining)

Banks to Maskelyne, London, January 20, 1802

The Moniteur received this morning informs us that M. Olbers of Bremen has rediscovered Piazzi's Planet on Jan. 2 at 11 h 50′ 36″ mean time at Bremen. It was seen on the wing *of the Virgin at right ascension 185.°9 its north Declination 11° 9′ Jan. 5 at 17 h 30′ RA 185° 43′ north Dec. 11° 8′. About its size is that of a star of the 9th magnitude. It cannot be distinguished from a star with a telescope that magnifies 106 times.* [Banks wrote an identical letter to Herschel on this date.]

Zach's next letter to Banks was penned on 30 January 1802, telling him that only he and Olbers had seen Ceres:

In pursuance of what I had the honor of intimating to you in my last, and in the supposition my notices will not be disagreeable to you, I take the liberty to send you the continuation of my observances of the New Planet Ceres Ferdinandea.

The RA are very exact, the Declinations only guessed at my transit instrument, for the planet appears so faint, that it was impossible to distinguish in the telescope of my 4 foot meridian quadrant, notwithstanding I got twice the zenith distances of the planet, marked with an x Viz., the 25th and 28th January. But I scarcely could see the wires as the little planet permits very little illuminating, and the state of air being very foggy all the time.

Seeberg Obsvy	*Mean time*	*Appar. RA Ceres*	*App Decl.*
1801 7 Dec	*18 h 48 m 10.3*	*178 33 30.60*	*11 41 1/2 N*
1801 31 Dec	*17 38 ::*	*184 44 ::*	*11 5*
1802 11 Jan	*17 3 17.4*	*186 45 49.95*	*11 15*
1802 16 Jan	*16 46 25.6*	*187 27 53.25*	*11 26*
1802 22 Jan	*16 25 23.9*	*188 6 25.80*	*11 44*
1802 25 Jan	*16 14 32.9*	*188 20 39.15*	*11 56 23x*
1802 26 Jan	*16 10 53.7*	*188 24 49.50*	*11 57*
1802 28 Jan	*16 3 29.0*	*188 31 37.85*	*12 9 41.3x*
1802 29 Jan	*15 59 43.7*	*188 34 18.15*	*12m 14*

The Ceres has hitherto been observed by no other astronomer but by me, and by Dr Olbers of Bremen; this latter made a little mistake in reducing his observations, I had the honor to send to you. I make it a duty, so give you here a corrected copy of these observations, this gentleman sent to me in his last letter. The mistakes took place in noting the AR of 20 Virginis, 3 minutes too little, with which he compared the planet. His observations stand thus now.

Bremen	*Mean time*	*App. RA Ceres*	*App. Decl. N*
1802 2 Jan	*11 h 58′ 36″*	*185 7′ 40″*	*11 6′ 30″*
1802 5 Jan	*17 30 0*	*185 43 7*	*11 7 56*
1802 10 Jan	*12 25 41*	*186 34 52*	*11 13 10*
1802 13 Jan	*11 53 38*	*187 1 56*	*11 18 56*
1802 14 Jan	*11 9 3*	*187 10 11*	*11 20 57*
1802 15 Jan	*12 8 9*	*187 18 27*	*11 23 25*
1802 20 Jan	*13 8 0*	*187 55 0*	*11 37 18*
1802 22 Jan	*12 26 40*	*188 5 45*	*11 43 55*

These observations are all made with the circular micrometer which wants no illuminations of wires. It is only a perfect circular diaphragm in the focus of the telescope. The immersions and emersions of the planet, and the star compared with, are obscured. And so AR and Decl. are deduced, but this method of observing gives not a very great precision, especially for Declination upon which 1 or 2 min cannot be depended.

Till 15th January the planet has not yet been discovered in France. A letter from Mr Piazzi, Palermo 9th last December mentions that he had not discovered yet the planet. I long to know, whether the English astronomers had been happier. Perhaps Mr Herschel will discover some satellites in Ceres. Mr Harding[1] *of Lilienthal sends me words, that looking at the Ceres with a Power of 288, he distinguished a little disc, of the size of the I or II satellite of Jupiter; he esteems therefore the diameter of Ceres about 2 seconds. He saw also two lucid points near the planet (11 January) both to the west of the planet, the one about 20″ the remoter 30″ or 35″. Dr Herschel will best tell us, if these lucid pointers are satellites or not.*

Here I have the honor to send you another ephemeris for Ceres, which will agree better than my former, because I corrected it upon my observations. (Zach, 1802b)

Midnight at Seeberg	*RA*	*Decl N*	*RA in time*
1802 30 Jan	*188 37*	*12 16*	*12 34 30*
1802 2 Feb	*188 43*	*12 31*	*12 34 53*
1802 5 Feb	*188 45*	*12 47*	*12 35 1*
1802 8 Feb	*188 44*	*13 4*	*12 34 55*
1802 11 Feb	*188 38*	*13 22*	*12 34 33*
1802 14 Feb	*188 24*	*13 41*	*12 33 57*
1802 17 Feb	*188 16*	*14 1*	*12 33 5*
1802 20 Feb	*187 59. 25*	*14 21*	*12 31 59*
1802 23 Feb	*187 40*	*14 42*	*12 30 40*
1802 26 Feb	*187 17*	*15 3*	*12 29 8*
1802 1 March	*187 51*	*15 24*	*12 27 22*

Two weeks later, on February 16, 1802, Banks (1802) passed to Herschel some of the information in the last paragraph of Zach's letter:

By a letter from Zach [this is the January 30 letter] *I learn that Mr. Harding of Lilienthal looking at Ceres with a power of 288 distinguished a little disc of the size of the I or II satellite of Jupiter whence he concludes the diameter of the planet to be about 2″. He saw also 2 lucid points near the planet both on the west side on the 11th of January. The one about 20″ the other about 30″ or 35″ distant which he suspects to be satellites.*

In fact, Harding's full report is published in the February 1802 issue of the *Monthly Correspondence,* page 170. He used a 7-foot Herschel telescope for these observations. A little over a week after writing his previous letter Zach (1802d, his underlining) sent Banks another missive on February 8, 1802:

I take the liberty to send you here, the first approximated Elements of an elliptical orbit of the new Planet Ceres Ferdinandea, which Dr Gauss corrected upon my first Observation of this Planet Decbr. 7 1801, and 16 Jany. 1802. These Elements will want some farther corrections, but in the mean While they will agree with the Heaven for a considerable time about half a minute.

[1] Karl Harding (1765–1834) discovered the third asteroid, Juno, in 1804. He worked as an assistant to Johann Schröter in Lilienthal at the time this letter was written. He later moved on to a professorship at Göttingen. An account of Harding's observation of January 11 was reported in the *Göttingische Gelehrte Anzeigen* 38 on March 6, 1802, 369–372.

Epocha Mean Long. 1801 for Palermo	*77° 24′ 55,9″*
—— —— —— 1802 —— ————	*55 33 35,1*
Dayly mean tropick heliocentric motion	*770,″7376*
Tropick Revolution	*1681 days 12 h 9 min*
Log. ½ axis	*0.4421189*
Aphelium	*326° 14′ 45″*
Node	*80. 58 55*
Eccentricity	*= 0.08086253*
Greatest Equation of the Center	*9° 16′ 23″*
Inclination	*10. 37 51*

My Observations agree with this elements thus:

Seeberg	*Calculated AR*	*Distance from observation*	*Calculated Decl. N*
Decbr 1801 7	*178° 33′ 33,6″*	*+3″, 0*	*11°47′33″*
Jany 1802 11	*186 46 9,3*	*+19″, 3*	*11 15 41*
Jany 1802 16	*187 28 3,1*	*+9″, 9*	*11 26 40*
Jany 1802 22	*188 6 45,9*	*+20′,1*	*11 45 18*
Jany 1802 25	*188 21 6,5*	*+27″, 3*	*11 56 49*

You'll find my Subsequent Observations of the Planet in the annexed printed Sheets pag. 15. The Planet was stationary from Febr 4th to 5th.

To facilitate the Calculation of the Position of Ceres by the above Elements, I calculated the following formulae

1) *for the Equation of the Center*

$$-33330''.972 \text{ Sin. Anon. med} + 1681''.843 \text{ Sin 2 Anon med} - 117''.670 \text{ Sin 3 Am} + 9''.408 \text{ Sin. 4 A.m} - 0''.8148 \text{ Sin 5 A.m}$$

2) *For the Radius Vector = r*

$$r = \frac{7.61007}{2.767700 + 0.2238032 \text{ Cos. Anom. ver}}$$

3) *for the heliocentric Latitude = λ*

$$\text{Log Sin } \lambda = 9.2659499 + \text{Log Sin. Arg. Latit.}$$

4) *for Reduction to the Ecliptick = ε*

a) $\text{Log. Tang. } \phi = 9.9924811 + \text{Log tang. Arg. Latit.}$

b) $\text{Arg Latit} - \phi = \varepsilon$

Mr Schroeter from Lilienthal measured the Planet's disk

Jany 25 = 1″,815 but the planet appears to him wrapt up in a very great nebulosity. The Diameter with this

Nebulosity was = 2″,514
the 26 Jany ….. = 2,687
Mr Harding found the same to be = 2″,330

Banks received a letter from Gilpin (1802a), dated February 18:

I have the honor to inform you that on Monday night the 8th inst. the sky being clear I obtained an observation of Piazzi's new Planet named by him Ceres Ferdinandea on the meridian, however, at that time it was only conjectured to be such. On the 12th the sky *proved sufficiently clear to allow me to observe it on the meridian again, these two observations of the Planet, with those that had been made on stars near it for the purpose of comparison admits of no doubt of its having altered its situation with respect to those stars with which it had been compared, and that it is the Planet described by Piazzi, the existence of which I understand is still doubtful with some. I shall not at this time trouble you with a detail of particulars of these observations, but reserve them for some future time when I shall have had opportunity of making more, should they then be thought of importance. I shall not fail to communicate them to you; at present I give its place in the heavens deduced from these observations, which may be depended on to a tolerable degree of accuracy.*

Mean time	*AR in sidereal time*	*AR in degrees*	*Declination*
On the 8th 15 h 20′ 32″	*12 h 34′ 41.50″*	*188º 40′ 25″*	*13º 8′ 21″N*
12th 15 4 18	*12 34 0.82*	*188 30 12*	*13 32 36*

Its motion therefore in AR from the 8th to 12th appears to have been 10′ 13” retrograde, and in Declination 24′ 15″ Northward.

From the observations of the Astronomer Royal on the 3rd and 4th inst. its motion in AR in that time was 0′ 57″ direct and its declination had increased 4′ 40″ Northward, but Mr. Aubert who observed it on the meridian on the 7th found its AR less than the Astronomer Royal had one on the 4th by 1′ 13″, hence it appears to have been stationary about the 5th inst.

The Planet appears to me to be of about the 8th magnitude, its color something like that of the Planet Mars, and has not that sharpness or brilliancy that a star of the same magnitude appears to have.

The observations were made with a Transit Circle 18 inches in diameter, the telescope is two feet focal length, and 1¾ inches aperture, and magnified about 50 times. Altitudes of the same object made with this instrument differ more than 5″ but in general they are much nearer.

Then on February 20, Zach (1802e, his underlining) sent Banks his latest observations of Ceres:

I had the honor to send to you my observations of the new Planet Ceres Ferdinandea made in January: here I take the liberty to send the continuations of them made in February.

1802	*Mean time in Seeberg*	*Apparent RA observed*	*App. Declin. Obs*
Febr 3	*15 h 40′ 35.8″*	*188° 42′ 13″.05*	*12° 40′ 5″N*
Febr 4	*15 36 41.4*	*188 42 36.30*	*………………*
Febr 5	*15 32 45.1*	*188 42 31,15*	*12 50 25*
Febr 9	*15 16 43.7*	*188 38 3.90*	*13 14 18*
Febr 19	*14 34 46.7*	*187 58 27.90*	*14 20 3*

Dr Gauss has corrected his elliptical Elements of the orbit upon my observations. Here is what he has found since my last letter to you.

Epocha for the Beginning of the Year to the Meridian of Seeberg	*77º 27′ 36″.5*
Epoch 1801 for the Seeberg Meridian	*77º 27′ 36″.5*
Mean diurnal mot. helioc. and tropical	*769″.792 log 2.4463726*
Log. semi major axis	*0.4424742 number 2.769965*
Eccentricity	*0.0814064*
Aphelion 1801 stationary	*325º 57′ 15″*

(continued)

Node	*80 58 40*
Equation of the orbit	*9 20 8*
Inclination	*10 37 56.6*

With these Elements of the orbit, all the observations made by Mr Piazzi in Palermo from Jany 1 till Febr 11 1801, agree perfectly well, and within a few seconds. And my observations are represented by them thus:

Seeberg Observy	*RA calculated*	*Difference*	*Declin. calcul.*	*Dif′*
1801 December 7	*175° 33′ 29″.2*	*–1″.4*		
1802 January 11	*186 45 47.6*	*–2.3*		
1802 January 16	*187 27 38.8*	*–14.4*		
1802 January 22	*188 6 18.2*	*–7.6*		
1802 January 25	*188 20 37.2*	*–2.0*	*11° 56′ 58″.4*	*+35″.4*
1802 January 26	*188 24 37.0*	*–12.5*		
1802 January 28	*188 31 25.7*	*–12.1*	*12 9 55.6*	*+14.3*
1802 January 29	*188 34 14.1*	*–4.0*		
1802 January 30	*188 36 38.4*	*–5.5*	*12 19 19.8*	*+19.1*
1802 January 31	*188 38 38.3*	*–7.1*	*12 24 15.3*	
1802 Febry 3	*188 42 7.8*	*–5.2*	*12 39 53.6*	*–11.4*

As these elements agree hitherto so well with the heavens, the following ephemeris calculated upon them for the next month will probably do the same, so I annexe it here, to point to our English observers the place, where they have to look for the Ceres.

This planet will come in opposition with the Sun March the 17th afternoon. In the same time this heavenly body will be in its greatest proximity to the Earth = 1.6025 and therefore the most favourable time, to look for its satellites, if there are any. About this time the planet will also be in the greatest geocentric Latitude = 17° 9′, and a little later, he will have his greatest retrograde motion, about 13 min in Right Ascension per day. The North Declination will increase till to the beginning of April, about the 9th of the same month, the motion in Declination will commence to be South.

Position of the Ceres for the Midnight Mean time in Seeberg Observatory

1802	*RA in degrees*	*Decl. N*	*RA in time*
March 1	*186° 41′*	*15° 30′*	*12 h 26′ 45″*
March 4	*186 11*	*15 50*	*12 24 45*
March 7	*185 39*	*16 10*	*12 22 36*
March 10	*185 5*	*16 29*	*12 20 18*
March 13	*184 28*	*16 47*	*12 17 53*
March 16	*183 51*	*17 4*	*12 15 24*
March 19	*183 13*	*17 19*	*12 12 50*
March 22	*182 34*	*17 33*	*12 10 15*
March 25	*181 55*	*17 44*	*12 7 40*
March 28	*181 17*	*17 54*	*12 5 7*
March 31	*180 39*	*18 1*	*12 2 37*
April 3	*180 3*	*18 6*	*12 0 12*
April 6	*179 29*	*18 10*	*11 57 54*

It appeared to me that the Ceres has some change of light. I imputed it first to our hazy atmosphere this winter, but Mr Schröter from Lilienthal, and Mr Olbers from Bremen sent me words, that they have observed the same, and they believe that it is the planet, which is subject to such changes of light. Mr Herschel will tell us best, whether it is so.[2]

I have some hopes to find the Planet, in ancient Catalogues of Stars. Mr Messier[3] *was very near it in the Year 1779. The famous comet of this year ran just over the northern Wing of Virgo, as now, and the new planet was not very far. If the comet had two months sooner reached the wing of Virgo, Mr Messier must infallibly have observed the Ceres then, because he determined all the little stars in the vicinity of the Comet; the Planet had been in the way of the comet, and so of course he would have caught this little planet in 1779.* [This point is given some more detail in a letter by Olbers, published in the March 1802 issue of the *Monthly Correspondence*.]

If my informations are acceptable to you, Dear Sir, only a little hint, and I shall continue with pleasure to give you further intelligence.

Banks replied to this letter on March 12, 1802, but it is no longer extant. Zach also sent a copy of this letter to Edward Troughton, and subsequently it was published in *A Journal of Natural Philosophy* (Nicholson's Journal; 1802b). Zach's next letter to Banks is dated March 15, 1802:

I had the honor of your letter 12th inst. and was very much flattered to see, that you accepted so well, and pay'd some attention to my letters, which I took the liberty to address to you concerning the Ceres Ferdinandea. This very kind reception imbolden's me to send you here, the continuation of my observations of this new Planet. Very good observations of this heavenly body, are still very scarce, and I hope the English astronomers will find the mine so. There are hitherto only three places, where Ceres is observed with great accuracy, in Greenwich, in Paris, and in Seeberg. All other astronomers in Europe have either not the means, and the power to observe this delicate Planet with great precision; or the intelligence of the discovery of this heavenly body, has not reached them yet. So Mr. Oriani astronomer in Milan sent me words Febry 10th, that to this date, no tidings of Ceres came to Italy, and yet I sent to him the intelligence in the begining [sic.] *of January, but very unfortunately, Mr. Oriani was not in Milan by this time, he was then as Deputy of the Cisalpine Republic in Lyons, so my first letter to him was delayed. From Palermo neither no news; so I can't tell when Mr. Piazzi (who is very able to make excellent observations) began first to observe the Ceres Ferdinandea. Mr. Mechain was so kind, as to send me two observations of Ceres, of the Astronomer Royal, and I was much satisfied, to see, that my observations agree'd perfectly with the Doctor's. But as my friend Mr. Mechain sent me words, that the Doctor wished not, to have his observations published, so I have made no use of them in the Monatliche Correspondenz, tho' a greater, and a divers set of observations woul'd have been very acceptable to the calculators of the Planet's orbit. I do not communicate to you either the observations from Paris, because Mr. Mechain assured me in his letter, that he has already done it. Here are all my observations made in the present Month of March, they prove, that the ephemeris of the Planet's Track, which I took the liberty to send you, still agrees tolerably well with the Heaven.*

At the bottom of this table is an * with another observation made on March 15. To the right of the table, Zach wrote this paragraph:

**An observation just made the night before this letter was sent off, which will set a puzzling some astronomers, for the Ceres fell just in, with a star of 7 magn. and it was difficult to*

[2] Any changes in the light of Ceres observed by Zach, Schröter and Olbers were mistaken. A combination of poor optics and atmospheric conditions misled them. Its true amplitude is only 0.04 magnitude, far too small for visual detection.

[3] Charles Messier, the famous French astronomer. He co-discovered comet Bode on 19 January 1779.

distinguish, which was the Planet. I observed them both, and so I found out, that the Planet followed the star 7″.52 in time, and was the northern most. The mean position of the star 1 January 1802 is AR 184° 0′ 21″.23 Declinat 16° 56′ 46″.2 N.

After this aside, Zach now resumes the letter:

I shall take the liberty, to send you in my next (letter), the first proof of a celestial map, which is engraving just now, & upon which I delineated the Track of the new Planet during its visibility in the present Year. This map will be of great utility to astronomers, in observing the planet out of the meridian, for in May the Twilight will not permit this kind of observation, and we must take recourse to equatorial sectors, or parallactic instruments, in comparing the little Planet with fixed stars in its Parallel. In order to do this with great accuracy, I have set down in this map, all the little stars, even the telescopicks, which come in the Planet's Way, and which are not lay'd down in Mr. Bode's great maps he published last year.[4] *I have to this Purpose not only calculated the positions of these little stars, observed by the nephew of Mr. de LaLande,*[5] *in his Histoire celeste francaise, but as he has only observed the zenith distances, I have determined with great care, with my 8 feet transit instrument of late Mr. Ramsden, the mean Right Ascensions of them. And as I took to my standard, the late corrected positions and AR of Regulus, β Leonis, β Virginis and Spica, from the Royal Astronomer, communicated to me by Mr. Mechain; and as referred in No. 1 Jan.y 1802 p. 60 of my Monthly Correspondence, I hope all these little stars, are determined' as exactly, as in the Doctor's Catalogue of the 36 principal stars.*[6] *If the Astronomer Royal will take the trouble to examine how far my attention deserves credit, I put down here some of my determinations, that will enable him to judge, whether these AR, will be to any service to astronomers, in comparing the new Planet with them.*

The following two paragraphs were written to the right of the table of star positions that are reproduced in Figs. 10.2 and 10.3.

All the time, I observed the new Planet, I also observed carefully the Sun, in order to find out the error of our solar tables, on purpose to get the heliocentric places of the planet as pure and unmixed with errors of our own motion, Planet as pure, and unmixt with the errors of our own motion. This caution is by no planets more necessary, as by Ceres and Mars, as astronomers, who aspire to modern delicacy in practical astronomy will understand best. For our best solar tables, can yet be erroneous to a quarter of a minute, in some cases. Some equations of the Sun's longitude depending on the Planet's actions, and on the higher powers of the eccentricity of their orbits, had been neglected hitherto, tho' the joint actions of these neglected terms can amount to 10 or 12″ in space. Mr. de LaPlace, wrote to me lately, that he had also found out, that the mean solar motion has a secular diminution of about 20″.[7]

To correct now the elements of the orbit of Ceres, the action of Jupiter and Mars upon this little planet must needs be computed, for these Perturbations will be considerable enough. I expect only the observation of the opposition, and as soon, as I shall have done it, I'll immediatly [sic.] *set about this calculation.* (Zach, 1802g, his underlining)

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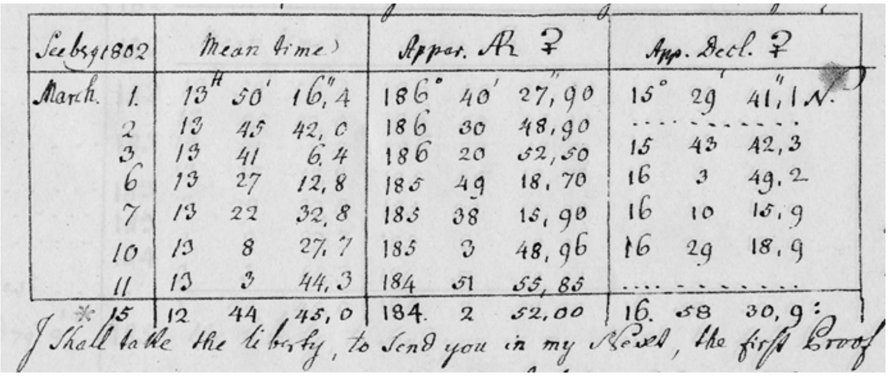
[4] Zach refers here to Bode's famous *Uranographia* star atlas (see Appendix D).

[5] Michel Lefrançois. He was a cousin, not a blood nephew of Lalande, but was always referred to as Lalande's nephew. Michel married Lalande's daughter Amelie. He began working with Lalande in 1781.

[6] Maskelyne's 1790 catalog gave the proper motions of 36 stars. From these data, Herschel determined the Sun's motion in space.

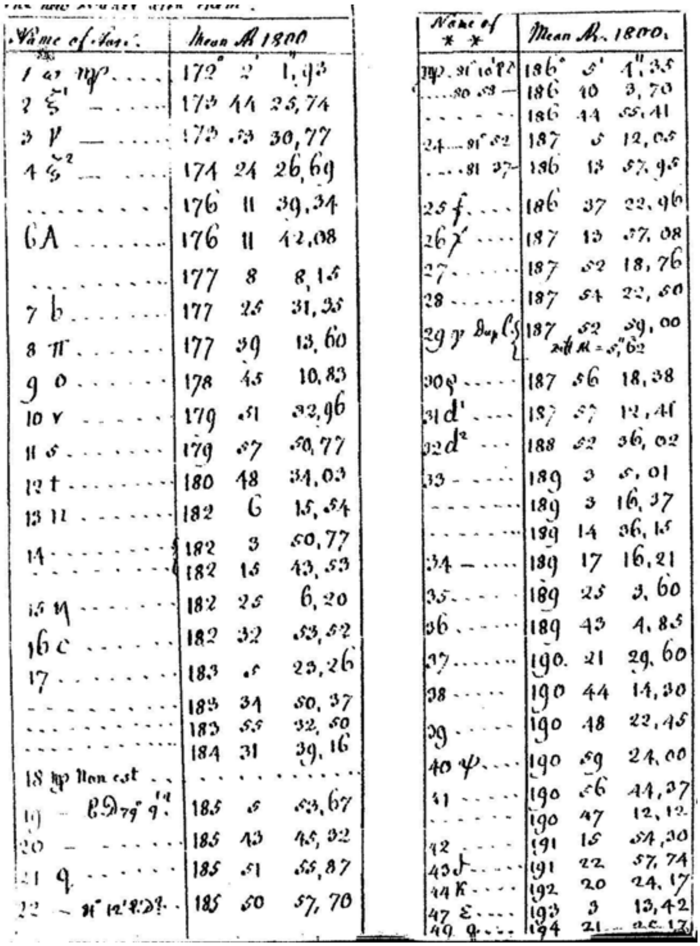
[7] William Herschel wrote about this matter: On the Quantity and Velocity of the Solar Motion, *Philosophical Transactions of the Royal Society,* 95: 231 (1805), and 96: 205 (1806).



Seebg 1802	Mean time	Appar. AR ⚳	App. Decl. ⚳
March 1.	13h 50' 16",4	186° 40' 27",90	15° 29' 41",1 N.
2	13 45 42,0	186 30 48,90	. . . .
3	13 41 6,4	186 20 52,50	15 43 42,3
6	13 27 12,8	185 49 18,70	16 3 49,2
7	13 22 32,8	185 38 15,90	16 10 15,9
10	13 8 27,7	185 3 48,96	16 29 18,9
11	13 3 44,3	184 51 55,85	. . . .
* 15	12 44 45,0	184. 2 52,00	16. 58 30,9:

I Shall take the liberty, to Send you in my Vessel, the first Proof

Fig. 10.2 Zach's table, March 1802 positions of Ceres



Name of Star.	Mean AR 1800
1 a ♍	172° 2' 1",93
2 ξ¹ —	173 44 25,74
3 ν —	173 53 30,77
4 ξ² —	174 24 26,69
. . . .	176 11 39,34
6 A	176 11 42,08
. . . .	177 8 8,15
7 b	177 25 31,35
8 π	177 39 13,60
9 o	178 45 10,83
10 ν	179 51 32,96
11 s	179 57 50,77
12 t	180 48 34,03
13 h	182 6 15,54
14	182 3 50,77
. . . .	182 15 43,53
15 η	182 25 6,20
16 c	182 32 53,52
17	183 5 23,26
. . . .	183 34 50,37
. . . .	183 55 32,50
. . . .	184 31 39,16
18 ♍ Non est . .	. . . . . .
19 — B.D. 79° 9' 14"	185 5 53,67
20 —	185 43 45,32
21 q	185 51 55,87
22 — 81° 12' 8.D? .	185 50 57,70

Name of * *	Mean AR 1800.
♍ 81° 10' ??	186° 5' 1",35
. . . . 80 53 —	186 10 3,70
. . . . . .	186 44 55,41
24 . . . 81° 52	187 5 12,05
. . . . 81 37 —	186 13 57,95
25 f	186 37 22,96
26 γ	187 13 57,08
27	187 52 18,76
28	187 54 22,50
29 γ Dup. Cs	187 52 39,00 Diff AR = 5",62
30 φ	187 56 18,38
31 d¹	187 57 12,41
32 d² . . .	188 52 36,02
33 -	189 3 5,01
. . . . . .	189 3 16,37
. . . . . .	189 14 36,15
34 —	189 17 16,21
35	189 25 3,60
36	189 43 4,85
37	190. 21 29,60
38	190 44 14,30
39	190 48 22,45
40 ψ	190 59 24,00
41	190 56 44,37
. . . . . .	190 47 12,12
42	191 15 54,30
43 δ	191 22 57,74
44 κ	192 20 24,17
47 ε	193 3 13,42
49 g	194 21 [illegible]

Fig. 10.3 A table of stellar positions by Zach

Febr. 1802	AR ☉ in time	Longit. ☉	Errors of my Tables.
Febr. 27	22h 39' 25",75	11s 8° 12' 17",9	+ 8",0
28.	22 43 11, 11	11 9 12 26,1	+ 11, 5
March 1	22 46 55, 96	11 10 12 32,9	+ 14,4
2	22 50 40, 91	11 12 12 47,7	+ 8,0
3	22 54 24, 88	11 12 12 53,1	+ 9,4
6	23 5 34, 44	11 15 13 5,3	+ 5, 4
7	23 9 16, 68	11 16 13 4,4	+ 4, 9
8	23 12 58, 22	11 17 12 56,9	+ 8, 9
17	23 45 58, 75	11 26 29 46,5	+ 5, 2
18	23 49 37, 57	11 27 10 22,8	+ 3, 2
19	23 53 15, 87	11 28 9 51,7	+ 5, 6
20	23 56 54, 39	11 29 9 22,2	+ 5, 1
21	0 0 32, 56	0. 0 8 52,4	+ 2, 8

Fig. 10.4 Zach's observations of the Sun in Feb. and Mar. 1802

Zach's next letter to Banks is dated March 30, 1802:

I promised you in my last letter, that I shall have the honor to send to you, the observation of the opposition of the new Planet Ceres Ferdinandea, which as you know, is of a great moment for the Theory of this new heavenly body, and as I was so lucky to get these observations with full success, I take the liberty to communicate to you the results of them.

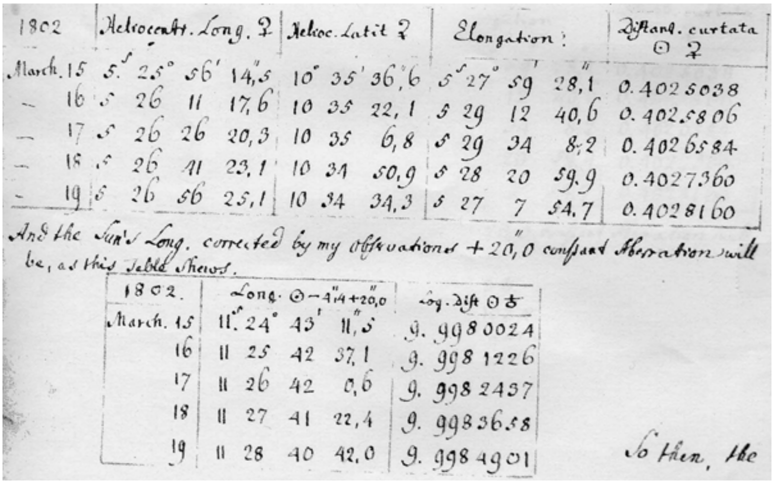
The 15, 16, 17, 18, 19 March the sky was very favorable; these days include exactly in their middle the opposition of the Planet with the Sun. I had the honor to tell you in my former letter that my purpose first was, to find out the errors of our Solar Tables, upon which a nice observer cannot rely; to attain which, I made the following observations of the Sun, which I compared with my own Solar Tables, published in Gotha 1792, and corrected hereafter in 1798. These observations stand thus (Fig. 10.4)

The mean error from 17 to 21 March is therefore +4″,4 which my Solar Tables give the longitude of the Sun too great; and to be employed in the calculations of the opposition of Ceres Ferdinandea. As to the planet, I compared' my observations with the Elements VII of its orbit, as referred in my journal, March page 272, viz:

Epocha of long. 1801 Seeberg meridian	*77º 27′ 36″,5*	
Mean daily heliocentric tropical motion	*769″.7924*	
Log. ½ great axis	*0.4424742*	
Eccentricity	*0.0814064*	
Aphelion	*}*	*325º 37′ 15″*
	}1801 stationary	
☊	*}*	*80 58 40*
Greatest equation of the orbit	*9 20 8*	
Inclination	*10 37 56,6*	

Epoch 1802	Mean time	Apparent AR ♀ observed	Apparent Declinat. observed. North corrected by Parallaxe	Long. ♀ observed	Latit. ♀ observed N.	Parallaxe in Altitude	Aberration in Long.	in Latit.
March 15	12h 44' 45",0	184° 2' 52",0	16° 58' 34",0	5s 26° 44' 22",7	17° 8' 24",5	+5",1	+7",5	+0",0
— 16	12 39 58,8	183 50 19,0	17 4 2,0	5 26 30 35,6	17 8 23,0	+3,1	+7,5	+0,1
— 17	12 35 12,3	183 37 34,8	17 9 9,5	5 26 16 45,8	17 7 58,9	+3,1	+7,5	+0,2
— 18	12 30 25,5	183 24 50,7	17 14 8,4	5 26 3 1,1	17 7 27,1	+3,1	+7,5	+0,3
— 19	12 25 28,3	183 11 58,6	17 18 53,9	5 25 49 15,3	17 6 40,0	+3,1	+7,5	+0,4

Fig. 10.5 (**a**, **b**). Error of the elements and a further list of Zach's observations



1802	Heliocentr. Long. ♀	Helioc. Latit ♀	Elongation	Distance curtata ☉ ♀
March 15	5s 25° 56' 14",5	10° 35' 36",6	5s 27° 59' 28",1	0.4025038
— 16	5 26 11 17,6	10 35 22,1	5 29 12 40,6	0.4025806
— 17	5 26 26 20,3	10 35 6,8	5 29 34 8,2	0.4026584
— 18	5 26 41 23,1	10 34 50,9	5 28 20 59,9	0.4027360
— 19	5 26 56 25,1	10 34 34,3	5 27 7 54,7	0.4028160

And the Sun's Long. corrected by my observations +20",0 constant Aberration will be, as this Table shews.

1802	Long. ☉ −4",4+20",0	Log. Dist ☉ ♁
March 15	11s 24° 43' 11",5	9.9980024
16	11 25 42 37,1	9.9981226
17	11 26 42 0,6	9.9982437
18	11 27 41 22,4	9.9983658
19	11 28 40 42,0	9.9984901

So then, the

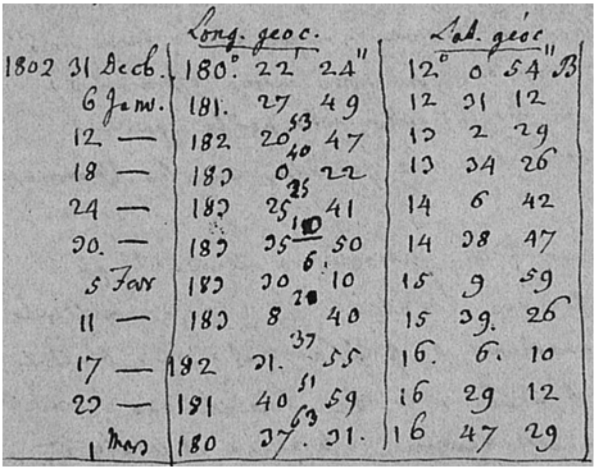
Fig. 10.6 Heliocentric elements of Ceres in Mar 1802

According to these Elements my observations are represented thus, as in the following (Fig. 10.5.)

It appears therefore, that the mean error of the elements of the orbit is –31″,1 in Longitude and +27″,2 in Latitude, excluding the error of Latit. of the late observation, marked doubtfull [sic.]. The heliocentric Elements of the calculus for the above observations will stand as follows (Fig. 10.6)

With these data I then found, that the Ceres Ferdinandea came into opposition with the Sun, March ye17th at 4H 18′ 0″ mean time in Seeberg. For this moment, the true corrected Longitude of the Sun by –4″,4 and +20″,0 from appar. Equinox is = 11Z 26° 21′ 26″,6

The geocentric. long. of Ceres corrected by +31″,1 is	*=5 26 21 26, 5*
The geocentric. long. of Ceres by the elements is	*=5 26 21 7, 7*
	___________
Error of elements in long	*–18″.8*



	Long. geoc.	Lat. geoc.
1802 31 Decb.	180° 22′ 24″	12° 0′ 54″ B
6 Janv.	181. 27 49	12 31 12
12 —	182 20 47	13 2 29
18 —	183 0 22	13 34 26
24 —	183 25 41	14 6 42
30. —	183 35 50	14 38 47
5 Febr	183 30 10	15 9 59
11 —	183 8 40	15 39. 26
17 —	182 31. 55	16. 6. 10
23 —	181 40 59	16 29 12
1 Mars	180 37. 31.	16 47 29

Fig. 10.7 The Ephemeris for Ceres supplied by Banks to Zach on Jan. 14, 1802

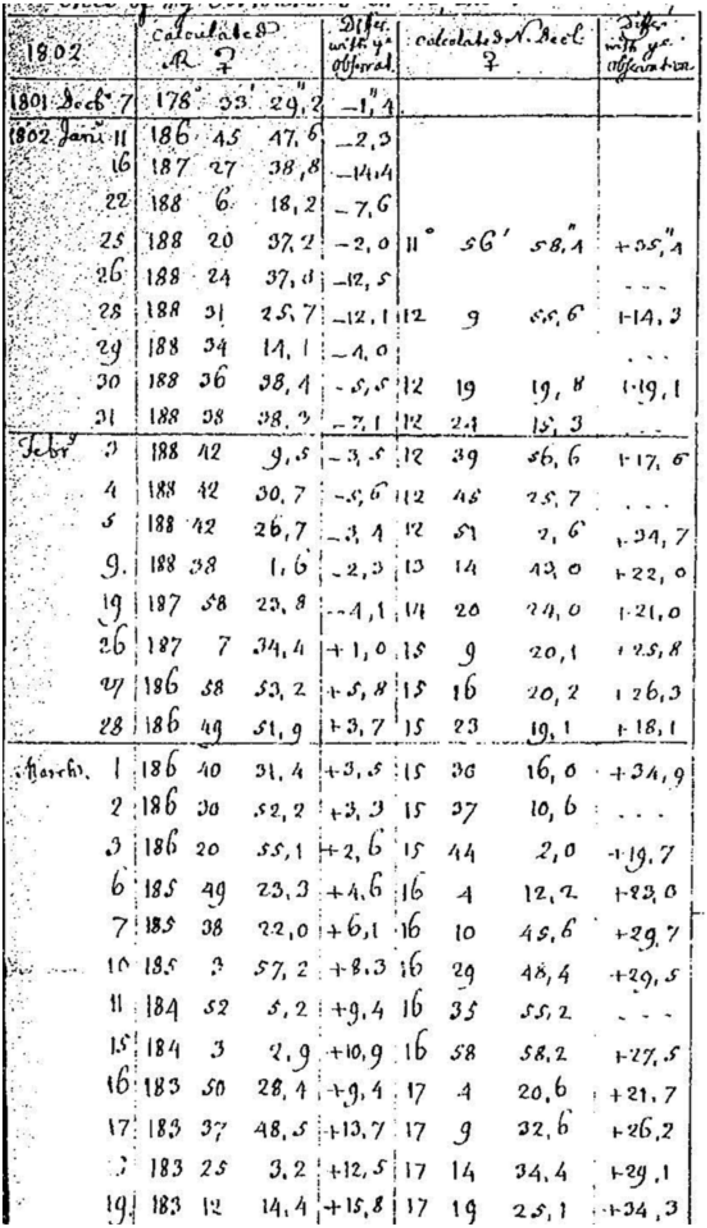
The geocentric. Latit. of ⚳ *corrected by –27″.2 is*	*=17º 8′ 9″,0 North*
Hence the heliocentric latit. will be	*=10 34 54,8*
The same by the elements calculated	*=10 35 12,2*
	______________
Error of Elements in latit.	*+17″,4*

So then, the main result is: That Ceres Ferdinandea was in opposition with the Sun 1802 March 17th at 4 h 18′ 0″ mean time in Seeberg in geo- and heliocentrick Longitude from the

Apparent equinox	*=5Z 26º 21′ 26″,55*
In geocentr. latitude	*=17 8 9,00 North*
In heliocentr. latitude	*=10 34 54,80 North*

All my observations shall agree with the above Elements of Dr. Gauss. The comparison which he has made, with the whole series of my observations in AR, and Declination, is as annexed here: (Figs. 10.7 and 10.8)

By inspection of this Table it appears, that all the negative Errors in AR, turned into positives, and are increasing, and very likely will continue so: But there is no correction of Permanence to hope, till the Equations of Perturbation shall be calculated. The Planet Jupiter has a very great action upon our little Planet, and Mr. De la Place wrote to me, that the summ [sic.] of all the Perturbations, to which Ceres is liable, amount to half a Degree. My Disciple Dr. Burckhardt, has already done this (?). He sends me Words, that all Equations of the Perturbation come chiefly from Jupiter, and amount to 27 minutes. Mars produces not 1 second, and Saturn as much as nothing. Taking therefore, these quantities in Account he finds the following corrected Elements of the Orbit.

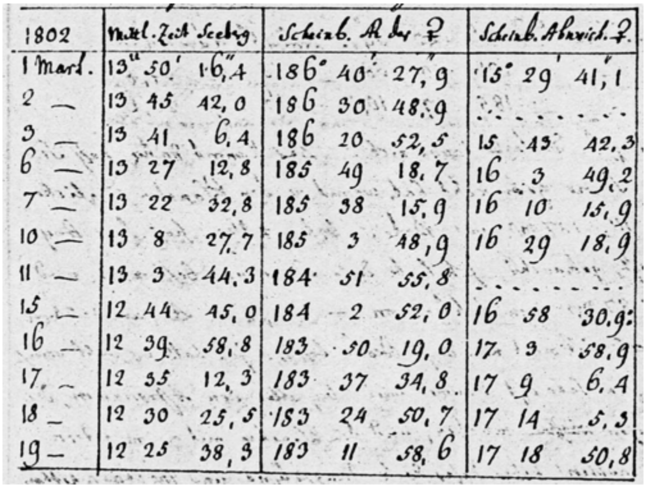


1802	Calculated AR ⚳			Differ. with ye observat.	calculated N. Decl. ⚳			Differ with ye observation
1801 Decbr 7	178°	33'	29",2	-1",4				
1802 Janr 11	186	45	47,6	-2,3				
16	187	27	38,8	-14,4				
22	188	6	18,2	-7,6				
25	188	20	37,2	-2,0	11°	56'	58",1	+35",1
26	188	24	37,0	-12,5				...
28	188	31	25,7	-12,1	12	9	55,6	+14,3
29	188	34	14,1	-4,0				...
30	188	36	38,4	-5,5	12	19	19,8	+19,1
31	188	38	38,3	-7,1	12	24	15,3	...
Febry 3	188	42	9,5	-3,5	12	39	56,6	+17,6
4	188	42	30,7	-5,6	12	45	25,7	...
5	188	42	26,7	-3,4	12	51	2,6	+34,7
9	188	38	1,6	-2,3	13	14	43,0	+22,0
19	187	58	23,8	-4,1	14	20	24,0	+21,0
26	187	7	34,4	+1,0	15	9	20,1	+25,8
27	186	58	53,2	+5,8	15	16	20,2	+26,3
28	186	49	51,9	+3,7	15	23	19,1	+18,1
March 1	186	40	31,4	+3,5	15	30	16,0	+34,9
2	186	30	52,2	+3,3	15	37	10,6	...
3	186	20	55,1	+2,6	15	44	2,0	+19,7
6	185	49	23,3	+4,6	16	4	12,2	+23,0
7	185	38	22,0	+6,1	16	10	45,6	+29,7
10	185	3	57,2	+8,3	16	29	48,4	+29,5
11	184	52	5,2	+9,4	16	35	55,2	...
15	184	3	2,9	+10,9	16	58	58,2	+27,5
16	183	50	28,4	+9,4	17	4	20,6	+21,7
17	183	37	48,5	+13,7	17	9	32,6	+26,2
[illegible]	183	25	3,2	+12,5	17	14	34,4	+29,1
19	183	12	14,4	+15,8	17	19	25,1	+34,3

Fig. 10.8 The calculated figures and observed differences in the positions of Ceres

Epocha Longit. ⚳ Meridian Paris	*77º 19′ 17″*
Aphelium in 1801	*326 42 32*
Annual Motion	*+2 5*

(continued)



1802	Mittl. Zeit Seeberg	Scheinb. AR ⚳	Scheinb. Abweich. ⚳
1 Mart.	13h 50' 16",4	186° 40' 27",9	15° 29' 41",1
2 —	13 45 42,0	186 30 48,9	. . . .
3 —	13 41 6,4	186 20 52,5	15 43 42,3
6 —	13 27 12,8	185 49 18,7	16 3 49,2
7 —	13 22 32,8	185 38 15,9	16 10 15,9
10 —	13 8 27,7	185 3 48,9	16 29 18,9
11 —	13 3 44,3	184 51 55,8	. . . .
15 —	12 44 45,0	184 2 52,0	16 58 30,9:
16 —	12 39 58,8	183 50 19,0	17 3 58,9
17 —	12 35 12,3	183 37 34,8	17 9 6,4
18 —	12 30 25,5	183 24 50,7	17 14 5,3
19 —	12 25 38,3	183 11 58,6	17 18 50,8

Fig. 10.9 Zach's observations of Ceres made in March 1802

Ω *1801*	*81 5 35*
Annual Motion	*as much as nothing*
Inclination	*10 36 52*
Half great–Axis	*2,76587*
Eccentricity	*0.0788725*
Tropick Revolution	*1679 days, 84*
[To the right of these elements, Zach writes the following]:	

Mr. Burckhardt has already constructed planetary Tables, upon these principles, but I shall not insist upon, as I suppose, that Mr. Mechain, will aquaint, the Astronomer Royal with this matter. For the present Moment Dr. Gauss Elements are more then sufficient to point out the Planet, and so I send here the continuation of an Ephemeris (⚳) Dr. Gauss has calculated at my Request.

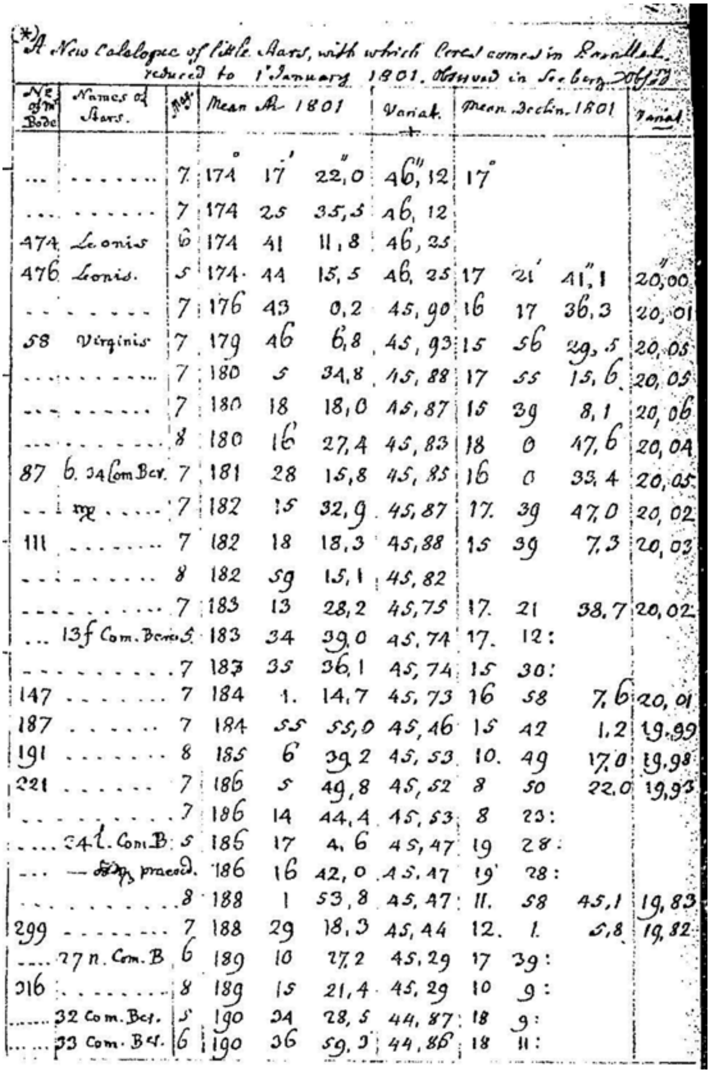
The letter then continues below.

Here I have the Honor of Sending you the Continuation of my Observations of the Ceres Ferdinandea (Fig. 10.9)

To the left of this table, Zach writes the following:

As Several Astronomers wished for the Position of Stars, with which they had compared the Ceres, I upon this occasion have constructed the following Catalogue of Stars (*) which are new, or had been laid down very erroneous I hope the competent Judges will find these Positions very exact (Fig. 10.10).

From Mr. Piazzi I received lately two letters from Febr 2th and Febr 17th. He just tells me in a Postscript, that he received by newspapers the intelligence of the discovery of his New Planet, but exspects [sic.] now, to hear by his correspondents, where to look for the Ceres Ferdinandea. He wishes not, that the name he has given to the Planet in honor of his



(*) A New Catalogue of little Stars, with which Ceres comes in Parallel. reduced to 1st January 1801. Observed in Seeberg 1801

No. of the Bode	Names of Stars	Mag.	Mean AR 1801 °	′	″	Variat.	Mean Declin. 1801 °	′	″	Variat.
...	....	7	174	17	22,0	46,12	17			
...	....	7	174	25	35,5	46,12				
474	Leonis	6	174	41	11,8	46,25				
476	Leonis	5	174	44	15,5	46,25	17	21	41,1	20,00
...	....	7	176	43	0,2	45,90	16	17	36,3	20,01
58	Virginis	7	179	46	6,8	45,93	15	56	29,5	20,05
...	....	7	180	5	34,8	45,88	17	55	15,6	20,05
...	....	7	180	18	18,0	45,87	15	39	8,1	20,06
...	....	8	180	16	27,4	45,83	18	0	47,6	20,04
87	6. 34 Com Ber.	7	181	28	15,8	45,85	16	0	33,4	20,05
...	♍	7	182	15	32,9	45,87	17	39	47,0	20,02
111	....	7	182	18	18,3	45,88	15	39	7,3	20,03
...	....	8	182	59	15,1	45,82				
...	....	7	183	13	28,2	45,75	17	21	38,7	20,02
...	13 f Com. Beren.	5	183	34	39,0	45,74	17	12:		
...	....	7	183	35	36,1	45,74	15	30:		
147	....	7	184	1	14,7	45,73	16	58	7,6	20,01
187	....	7	184	55	55,0	45,46	15	42	1,2	19,99
191	....	8	185	6	39,2	45,53	10	49	17,0	19,98
221	....	7	186	5	49,8	45,52	8	50	22,0	19,93
...	....	7	186	14	44,4	45,53	8	23:		
...	24 l. Com. B.	5	186	17	4,6	45,47	19	28:		
...	— praeced.		186	16	42,0	45,47	19	28:		
...	....	8	188	1	53,8	45,47	11	58	45,1	19,83
299	....	7	188	29	18,3	45,44	12	1	5,8	19,82
...	27 n. Com. B	6	189	10	17,2	45,29	17	39:		
316	....	8	189	15	21,4	45,29	10	9:		
...	32 Com. Ber.	5	190	24	28,5	44,87	18	9:		
...	33 Com. Ber.	6	190	36	59,3	44,86	18	11:		

Fig. 10.10 A new catalog of little stars with which Ceres comes in parallel

King might be altered, but it seems, that the French prefer the name of Juno. Mr. LaPlace in all his letters to me, always used the name of Juno. Mr. LaLande employed the name, Planéte de Piazzi, as he does with, Planéte de Herschel. In my journal Monthly Correspondence I applied to it the symbol ⚳, till a better shall be found out. The symbol of Saturn Δ, represents a scythe, so the symbol of Ceres ≥ may represent a sickle, as Ceres is the Goddess of corn and tillage.

But it is time to finish my epistle, in offering you of the highest Regard & Esteem. (Zach, 1802i, his underlining)

The confusing layout of Page 2 of this letter is reproduced here (Fig. 10.11).

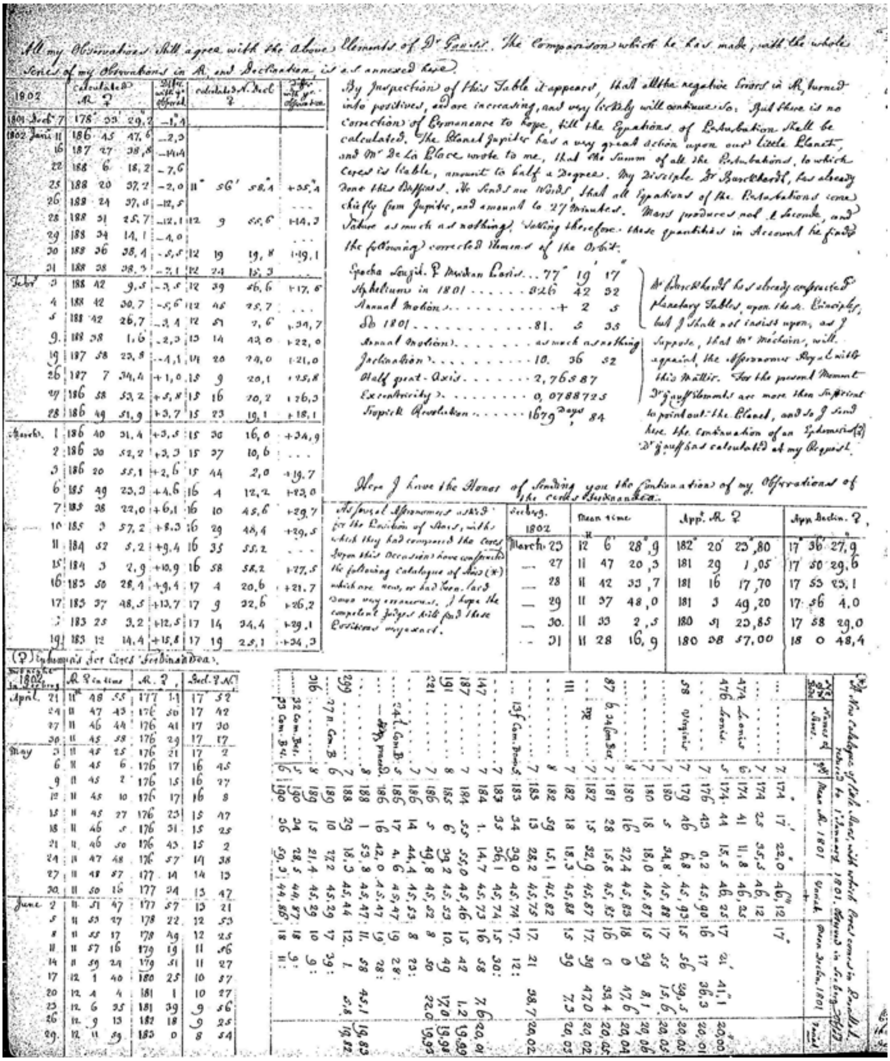


Fig. 10.11 The layout of Page 2 of Zach's letter

The Nevil Maskelyne Correspondence

This section intersperses letters to and from Nevil Maskelyne with his personal logbook notes about his asteroid observations. The February 3 entry is the night Maskelyne first saw Ceres:

Observing logbook note, February 3, 1802

M. Méchain observes in his letter to me of Jan. 27 that his observation of Jan. 24th gives the Declination 17′ more than Dr. Zach's computation. Hence Feb. 2 at 15 h RA might be *probably 188° 41.′5 and Declination 2° 31.′5 N and on Feb. 3 188° 43′ = 12 h 34′ 52″ and Declination 12° 36′ 40″N.*

This night I found it and observed it at about 13 h 8′ in RA 12 h 34′ 57″.5 and Declination 12° 38′ 11″N. The RA is less than the calculated one by only 1/2″ and the Declin. 1′ 31″ more than the calculated one. 12 h 34′ 57″.5 = 188° 43′.

Observing logbook notes by Maskelyne, February 4, 1802

This was a very fine night. The planet appeared with a well defined disc; and so did the 34th of Virgo, a star of 6th magnitude. The planet also appeared smaller, as well as more distinct than the night before. The planet appeared much less than the wire, which subtends an angle of 14″.8 and the star appeared the least possible larger then the wire, which covers 14″.8 in the heavens.

George Gilpin wrote Maskelyne on February 4, 1802:

Your servant being here I embrace the opportunity of acquainting you that the President [Sir Joseph Banks] *has received a Letter from Mr. Zach dated 14th Jan. 1802, acquainting him that he had detected the new Planet on the 7th December last; when it appears by his observation at 18 h 48′ 10″.3 Mean time its appt. RA was 178° 33′ 30″.6 very exact and its Decl 11° 41½′ N by estimation– not having observed it at the Quadrant but by the 8 feet transit. On the 11th January 1802 he observed it again at 17 h 3′ 17″.4 M. time its RA 186° 45′ 49″.95 very exact to a second and its decl. 11° 10′N by estimation-he mentions having letters from Paris as late as the 1st January but no mention was made of their having seen it, therefore he concludes that they had not at that time. Mr. Zach's letter is to be read tonight at the* [Royal] *Society.* (Gilpin, 1802b)

George Gilpin was Secretary of the Royal Society. Zach's letter that mentions Ceres was read to the Royal Society on 4 February, along with one from Maskelyne saying he had also observed Ceres. Gauss wrote Maskelyne on February 20, 1802:

Your kindness, and the common interest of science will I hope excuse a stranger's intruding upon You with this letter. I flatter myself, that You would not be displeased with the communication of an Ephemeris of the New Planet, which is constructed upon elements corrected after some new observations of Mr. de Zach's, and may perhaps contribute to facilitate farther observations of this faint-lighted Planet. The elements themselves are to be printed in the Monthly Correspondence, March. I hope, this ephemeris shall not deviate above One minute of degree from the true places.

Mr. de Zach has already furnished me with a considerable number of very precise Right Ascensions, but hitherto he could get but very few exact declinations. You would lay me under the greatest obligation, if you would hereafter honour me with the communication of some exquisite unparalleled excellence of the Greenwich-Instruments. If You should please to grant me this favour, I certainly will make of it the best use I am able. (Gauss, 1802a)

Memorandum by Maskelyne, February 23, 1802

Looked at planet Ceres with telescope of equatorial sector in the time from its passing the meridian till a quarter past ten with powers 50 and 200. The planet and star which preceded it 25″ was about 5′ N of it, seemed nearly of equal size colour and brightness; the planet rather the least possible larger brighter and whiter. The planet nearly white very little reddish. There was the least possible reddish cast. A star of Virgo appeared tolerably well defined, not perfectly so. The sky was tolerably clear, but the moon light strong. The star appeared in the telescope of the transit instrument rather smaller than the wire or less than 2″ in diameter; as the planet appeared rather larger with the telescope of the equatorial sector, it may be reckoned 2″ in diameter. T. F. [Thomas Firminger] *from the observations he has made it with the quadrant, reckon it 2″ in diameter. Perhaps in a very fine clear air it may appear larger than this morning when at the time of passing the meridian the air was hazy and overcast with thin clouds.*

Observing logbook notes by Maskelyne, March 6, 1802

At 9 h 50′ planet appeared excessive faint, with an aperture of 1/2 inch in diameter. I saw 34 Virginis very faint with 1/3 inch aperture, but not so faint as the planet before with 1/2 an inch aperture. Then looked at with an aperture of .3 inch and it was scarcely distinguishable and much fainter than the planet appeared before. This was a fine night, and stars of 6 magnitude were visible. Perhaps with an aperture of 1/3 inch, the star 34 Virginis might have appeared equally faint as the planet with an aperture of half an inch. According to this the light of the planet is to that of the star as 9 to 25 or near three times less. The planet appeared of 8th magnitude.

Maskelyne replied to Gauss on March 11, 1802:

On the 7th of this month I received your favour of the date 20th last month, for which I am much obliged to you, particularly for the ephemeris of the place of the new planet, which we call Ceres Ferdinandea according to the discoverer Mr. Piazzi; and apply to it the symbol  *expressive of the discoverer till a better shall be found out. Agreeable to your desire, I have sent you some of my observations, and will do myself the pleasure to send you more occasionally if you shall desire it. The first was taken with the equatorial instrument of 5 feet, the telescope having an aperture of 4.1 inches, by differences from 34 Virginis, whose place I have since settled by the meridian instruments. The three others were taken on the meridian. Taking your meridian to be 10° 52′ E of this, I find the RA corresponding to my observation should by your calculations be 185° 48′ 52″ and declination 16° 3′ 50″ which agree perfectly with your calculation. The Astronomers of Europe, and, I may say, Astronomy itself, is much obliged to you for having taken the pains to investigate elements of the orbit of the planet from Mr. Piazzi's observations of only 6 weeks, sufficiently exact to find the lost planet by, without which I fear it would not have been found again soon, for neither Mr. Piazzi's circular orbit nor Dr. Burckhardt's elliptic orbit were nearer enough to the truth; and I have undergone much labour in searching for it, but all in vain. It appears to me in a fine clear night to be of 8th Magnitude, and of an indifferent night of the 9th Magnitude. I divide the stars less than the 6th magnitude, which are visible to my equatorial of 4.1 inches, into 6 classes or gradations from 7th to 12th Magnitude, the last of which is to the telescope what the 6th Magnitude is to the naked eye. On February 4th I observed the planet to have a well defined disc; but at the same time 34th Virginis had the same. This appearance I see, tho' but rarely, in the stars, on a very fine night. Its diameter appears to me about 2″. On the 6th being a very fine night at 3 hours distance from the meridian, it was just ready to disappear with an aperture of half an inch, while 34 Virginis a star of 6 M. required an aperture of 3 inch to reduce it to the same degree of faintness as that of the planet with the half inch aperture. Hence the light of the planet is about 3rd of that of a star of the 6th M. It will require a good ephemeris to follow the planet when it is approaching to its conjunction with the Sun, as it will be very liable to be confounded with small stars. I have found it to be the case now with respect to stars of the same magnitude as itself. It is a pity a person so capable as Mr. Zach, should not have better wires put to his instrument. I use wires of 1/1000 inch in thickness; they might be made much smaller in one direction by flattening them with a stroke of a hammer.* (Maskelyne, 1802a)

Maskelyne sent Herschel a letter on March 16, 1802:

I have the pleasure to send you some places of Ceres Ferdinandea sent me by Dr. Gauss, true to a minute. I see you acknowledged my letter about the planet to the Royal Society. Hope you will determine the app. diameter of it with your telescope, which will show it the smallest. [Maskelyne means here that Herschel's large telescopes will hopefully be able to resolve Ceres, even though it is the smallest of the planets. He includes an ephemeris from March 16 to April 18.]. (Maskelyne, 1802c)

Memorandum by Maskelyne, Greenwich, March 27, 1802

The planet (Ceres) was in opposition the 17th of March, 1802, of the size and appearance of a star of 8th magnitude, its apparent diameter about 2″. At the time of opposition to the sun it was near its northern limit of latitude, and about 30o past its perihelion, and was nearly in as favourable a position to for being seen as it ever will be. Its apparent diameter at the distance of ϕ (Earth) at opposition = 3″.16. Its diameter probably 1/7 of that of the earth, and its bulk 1/343 of that of the earth. Subtracting 1/2″ for imperfection of the telescope, its apparent diameter in March = 1″.5 : at mean distance of ϕ at opposition = 2″.37. Its diameter diam ϕ :: 1:77.36 and its bulk 1/398.7 of that of ϕ or 1/400 of that the earth. Dr. Gauss to whom we are indebted for the recovery of the planet is a young man of extraordinary mathematical genius, who was taught mathematics under Zimmerman, at the expense of the Duke of Brunswick. He had read Newton's Principia at 18, and is now only 22; and has published a very learned treatise on the higher parts of the arithmetic, written in a very perspicuous style, in good Latin, which he has dedicated to his patron the Duke of Brunswick.

Gauss wrote Maskelyne on April 3, 1802:

Your kind letter of March 11th and all the interesting communications You have honoured me by, have caused to me the greatest pleasure, and I say You my most respectful thanks for them. Of Your observations of Ceres Ferdinandea I have compared two with my last elements (signed VII in Mr. Zach's Monthly Corr. March); the 6th March the error of the computed place was +5′0 in RA and +34″0 in Decl. (both being too great by this difference). But Your RA of February 19 which You make 187° 44′ 17″6 does not agree neither with the elements nor with the cotemporal observations of other Astronomers, if I may presume, that the minute only is erroneously written and ought to be 58′, this RA will agree exactly with Mr. Zach's observation of the same day 187° 58′ 27″9; the RA calculated for Your observation was 187° 58′ 13″2, for Mr. Zach's obs. 187.58.23″8, therefore error of the elements −4″4 by Your, −4″1 by Mr. Z's observation [ed: the figure 187° 44 17″6 was wrongly converted from time to space. 12 h 31′ 53′ of time corresponds to 187° 58′ 15″.3]. *The error in decl. of the same day was by Your obs. +35″3. (I must remark, that when I computed my VIIth elements, I had not yet any declinations and of consequence could not adapt the elements to them; as notwithstanding this the error of the elements in decl. is only ½ minute, this certainly is the most convincing proof of the high exactness of Mr. Piazzi's observations). As without doubt, no other observations can more effectually contribute to the farther correction of the elements (which I am already going to make preparations for, though I shall not entirely finish it, till no observations more may be made before the Conjunction of this year), I shall be extremely happy to receive the farther Greenwich observations You are so kind as to promise to me.*

I take the liberty to join hereunto a continuation of the ephemeris of the new planet till ultim. Jun., constructed after the same (VII) elements and for the same meridian of Seeberg 42′ 54″ time E. of Greenwich. As the orbit of Ceres is not an exact ellipsis, the effect of the perturbations caused by Jupiter being considerable, I do not doubt, but this ephemeris will not conspire with the true places equally exactly during this whole time, as it has done hitherto. However I trust, this circumstance will not lessen the utility of it; for as the error calculi must, by necessity, increase only by slow degrees, one may always previde [sic.] *it with all necessary exactness, by comparing the preceding observations with the ephemeris. The error calculi in RA was on the 19 March according to Mr. Zach's obs. +15″, the error in Decl. appears to be nearly constant. I should hardly imagine, the error might go upwards of 5 minutes in the month of June.*

I am informed, the French Mathematicians have already calculated the perturbations of Ceres by Jupiter, corrected the elliptical elements and constructed tables thereby. But, in my opinion, this is a pain not yet very necessary. For as it is at present of very great importance with respect to the certainty and precision of the calculated orbit, whether the series *of observations taken for basis of it, be several months longer or shorter, those tables will enjoy but a short life. For it will be absolutely necessary to make a new computus, when the observations of this year shall be entirely finished. Then I also, but no sooner, will neglect no pains, to determinate the elements as accurately as it shall be in my power.*

According to Mr. Schroeter's mensuration at Lilienthal, the true diameter of Ceres in only the fifth part of the diameter of earth; of consequence the planet is much smaller than our Satellite. But, comparatively, its atmosphere is prodigiously high. Certainly one may not wonder, that this puny celestial body has escaped so long to the astronomers.

[Gauss encloses an ephemeris of the equatorial coordinates of Ceres dating from April 21 to June 29, 1802, calculated for midnight on the meridian of Seeberg.]. (Gauss, 1802c)

The William Herschel Correspondence

It is fortunate that Herschel could rely on a large network of friends to keep him acquainted with Continental discoveries, as official publications often came out weeks or months afterwards. In the case of Ceres, this was particularly acute because Piazzi wanted to keep the discovery to himself, in flagrant breach of astronomical protocol. Piazzi waited an extraordinarily long time to inform his British colleagues of the discovery. Francisco Sastres (died 1822), the Neapolitan Consul in London, informed Herschel of the discovery in a letter dated May 2. Herschel was closely associated with Naples, as it was the first Italian city to host a telescope made by him (Gargano, 2012), so he and Sastres were acquainted. Bode gave positional details in a letter on June 6, but Herschel had to wait until a letter written on September 1 to get the news from Piazzi himself! Curiously, Piazzi sent his observations and calculated elements to Karl Seyffer in Germany a month earlier, on August 4, 1801. Why did he wait another month before informing Herschel? Perhaps his sense of shame finally overcame his reluctance to share the results with one of the greatest observational astronomers of the age. Here is the letter from Sastres to Herschel:

I yesterday received a letter from Palermo, acquainting me that Father Piazzi had discovered a comet, and that he requested of you the favour of informing him, through me, whether you have observed the same. (Sastres, 1801)

The June 6 letter from Bode added a lot of detail:

I would like to ask you to give the Royal Society of Sciences on my behalf the following astronomical news. On March 20 I received a letter, dated January 24, from Mr. Piazzi in Palermo.

While reading the letter I immediately noticed the peculiar appearance and motion of this alleged comet and after the first observations I considered the assumption justified that this little star is not a comet but rather a planet and to be precise that one of our solar system that has been announced by me since 1772 and has not been discovered yet. If I assume its distance from the sun approximately 2.90, the accuracy of Mr. Piazzi's communicated observations and the fact that it came to a standstill on January 11, it corresponds extremely well to this assumption. On March 23 I wrote Mr Piazzi and asked for some more observations, but until today I have not received those or a response to my letter of March 23. The other day I got a second letter from Mr. Piazzi, dated April 10 but he only mentions he had followed the comet discovered in January until February 11. But then he fell ill and had not fully recovered yet, but he intended to send me as soon as possible the orbital elements.

If Mr. Piazzi had determined the star's position for February 11, its orbit could be derived already and my assumption of it be confirmed. As soon as I have some more observations I will inform the Royal Society about the results of my observations. (Bode, 1801c)

William Herschel to his sister, Worcester, August 25, 1801

I want very much to look over the ecliptic again, to see for another planet, and to find whether Piazzi's star is a real planet, and if so whether it be furnished with satellites. For this reason my 10 feet telescope shall have all possible distinctness.

Many months after he had informed other astronomers, Piazzi finally wrote to Herschel:

Piazzi to Herschel, September 1, 1801:

I hope you haven't forgotten me: certainly I will never forget the kindness you showered upon me during my stay in England. January 1st I discovered a star, which by its motion greatly resembles a planet: enclosed you will find a little memo on this discovery. With your great skill, and your great methods, I would very much like it if you would search for it. For me, for want of instruments, I can concern myself only with its passage at the meridian. Time is still needed, and I don't know if I will be lucky enough to see it again. Help me then to verify this discovery, which doubtless will interest you as much as it does me.

Herschel's friend William Watson was particularly scathing in his opinion of Piazzi, and in a letter dated October 21, 1801, sarcastically supposed that Piazzi did not communicate sooner was because he was deceased:

I wish to be informed whether there is any foundation for the rumour that the deceased Mr. Piazzi of Palermo has discovered a new planet between the orbit of Mars and Jupiter. If that should be the case the astronomers will be the more intent to make a fresh scrutiny of the Heavens particularly of the zodiac to see whether there may not be many more. (Watson, 1801)

Herschel replied to Watson on October 27, 1801:

By the same post that brought me your letter I received one from the deceased, as your letter styles him, Mr. Piazzi, relating to his discovery. He wishes me to search for the lost planet, comet, or moving star which he observed last January. Before his letter came I had already examined that part of the heavens where, supposing it to be a planet, it should be found; but hitherto no astronomer, I hear has had any success in the rediscovery of the erratic phenomenon. Mr. Bode now thinks it might be the comet of Mr. Lexell. I have also anticipated your surmise that "astronomers will be more intent to make a fresh scouting of the Zodiac." This has been my employment since our return from north Wales. (Herschel, 1801b)

In this letter to Bode, written the same day, Herschel hedges his bets as to whether or not Piazzi's object is a planet:

I must now apologize for not having answered before, the letter which you acquainted me with Mr. Piazzi's discovery. I communicated its contents immediately to Sir Joseph Banks, Dr. Maskelyne, Mr. Cavendish, Sir Henry Englefield and other members of the Royal Society, and the letter would have been read at the meeting, if I had not received it too late, the session being over for the season.

I have been on the look out for the planet, comet, or moving star, about the place pointed out in the tables you have been so good to send me, but I am pretty well assured that it is no where in that neighbourhood. Were it one thousand times less in bulk than Mars I think it could not have escaped me; I shall however continue my research, and should I have any success you will immediately hear of it. (Herschel, 1801c)

Herschel (1801d) replied to Piazzi on October 29:

I have been searching for the new star that you discovered for two months now, hoping that it might be a planet, but until today I have not been fortunate to see it. I can assure you that I am very much interested in it. What a pity that I did not have news as soon as you discovered it; maybe we would not have lost it out of sight so early. However, I will be searching for it as soon as the weather permits us to see the sky that is almost constantly covered in clouds. I am very obliged for your Resultati delle observazioni della nuova stella *etc. which I read with the greatest pleasure. In case you find that star again, do me the pleasure to inform me first.*

Less than 2 weeks later, on November 10, Herschel (1801d) wrote to Lalande:

I have been searching for the star that Piazzi discovered and of which he announced to me in his Risultati delle observazioni della nuova stella*, but until now I have not seen any trace of it.*

Lalande replied to Herschel from Paris on November 26, 1801:

I also have received the observations of Piazzi, but I do not believe at all the period of four years. The observed arc is very small, and the degree of the attraction cast too many doubts it may be a comet. (Lalande, 1801)

Seyffer to Herschel, Goettingen, January 4, 1802

Some months ago I sent you the attachment, but I am assuming you did not get it. You probably know that Ceres has been refound. If you observed it as well I would appreciate it if you could communicate your observations or other remarks about it since I am planning on a second edition of the booklet. It would be my honour to print your observations or remarks.

May I ask you for your latest paper? Maybe via courier? I would appreciate other observations of Ceres from England as well.

Sir William Watson to Herschel, Bath, January 15, 1802

After reading your letter I entirely gave up Piazzi's planet and much was I surprised to find in the Philosophical Magazine an account that a paper had been read at the Royal Society from Dr. Maskelyne which gave the observations hitherto made by Piazzi, and two observations among them made so late as November last, giving at the same time the places of the planet at the times of observation. This you will readily believe excited in me many doubts which you can easily solve, and as doubtless you will by this time have received intelligence about this matter I request the favour of another letter, which will gratify not only myself, but all your philosophical acquaintance at Bath.

Herschel to Banks, Slough, January 30, 1802

While I return you many thanks for the favour of your agreeable communication of the discovery of Mr. Piazzi's planet, I ought at the same time to mention, that I have lately been engaged on the subject of a paper which I am preparing for the Royal Society, and which has unavoidably engaged my time in observations of a most interesting nature. So that, on Dec. the 7th I found myself obliged to drop the pursuit of the planet, being otherwise well assured that, if it had an existence it could not possibly escape the vigilance of other astronomers, who were in search of it. I shall now, however, take the first opportunity to get a view of it, in order to obtain as much information as I can of its magnitude or other circumstances, which my instruments may enable me to discover; and, as soon as I have had an observation, will send you an account of all the particulars I can collect.

Bode to Herschel, Berlin, January 30, 1802

What do you say to the rediscovery of the new planet which its discoverer Mr. Piazzi wants to name Ceres? Dr. Olbers was, as you might know, the first to see it again on January 1.

I have been searching for it in vain. On the fifteenth I saw Ceres in a triangle with two stars. On the 23rd I saw none. On the 25th and 26th I was able to determine its position at the circular micrometer.

25 Jan 10 h 15 56 m t. apparent right asc.	*188 20 25 Aber.*	*11 55 12*
26 Jan 10 48	*188 24 26*	*11 54 52.*

Sir Joseph Banks to Herschel, February 1802

By a letter from Zach I learn that Harding of Lilienthal, looking at Ceres with a power of 200, distinguished a little disk of the size of the 1st or 2nd satellite of Jupiter, hence he concludes the diameter of the planet to be about 2″. He saw also two lucid points near the Planet both on the west side, which he suspects to be satellites.

Maskelyne to Herschel, Greenwich, February 4, 1802

This morning about four o'clock on Feb. 3 16.11 m.t. I observed a star of 8th M. in RA 188° 43′ and Declin. 12° 38′N, which agrees exactly with the RA it should be in Méchain's observations, according to a letter which I received from him yesterday, and only 1′ 1/2 more northerly in Declination. Its motion, according to Dr. Zach's journal, is at present 7′ in RA in 6 days, and 31′ northward in Declination. I shall be happy if this shall enable you to get a view of its appearance thru your fine telescopes, with a considerable magnifying power, to show its disc, which may be hoped for, as its apparent diameter probably exceeds that of the Georgian Planet.

Aubert[8] to Herschel, February 9, 1802

I make (have) no doubt but you have seen the Ceres Ferdinandea. Dr. Maskelyne saw it the 3rd instant at 16 h 11′ meantime as follows RA 188° 43′ and Declin. north 12° 38.

I observed it last at 15 h 24′ 42″ mean time (or vulgarly called Monday morn at 3 h 24′ 42) as follows: RA 188° 41′ 45″ and Declin. (within a minute or two) of 13° north.

So it diminishes in RA a very little and increases a little in Dec. N. It appeared to me like a star of the 7th mag and very dingy with a great power. Let me know how you find it. [Aubert, 1802a; A similar Feb. 9 letter was sent to Banks]

Banks to Herschel, London, February 16, 1802

By a letter from Zach [this is the January 30 letter] *I learn that Mr. Harding of Lilienthal looking at Ceres with a power of 288 distinguished a little disc of the size of the I or II satellite of Jupiter whence he concludes the diameter of the planet to be about 2″. He saw also 2 lucid points near the planet both on the west side on the 11th of January. The one about 20″ the other about 30″ or 35″ distant which he suspects to be satellites.*

Herschel to Banks, Slough, February 17, 1802

I have the honour of your letter, and in a paper which I sent to you this morning you will find that I have not been inattentive to the subject of the information you have been so good as to give me. I think, however, that my determination of the magnitude of the new planet must be much more accurate than that of Mr. Harding at Lilienthal, both on account of the object with which I compared it, and of the magnifying power of my telescope, which was more than double that of the former gentleman.

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[8] Alexander Aubert (1730–1805), an English amateur astronomer, constructed observatories at Austin Friars, Lewisham and Highbury. Aubert was educated in Geneva, Leghorn and Genoa for a mercantile career, but his main private passion was astronomy. He built his own observatory on Loampit Hill, near Greenwich, equipping it with instruments by Bird, Dollond, Ramsden and Short. From there he also observed the transits of Venus and Mercury across the Sun in 1769 and 1786. He was a Member of the Royal Society from 1760 to 1778.

I should have sent my paper sooner, especially as my good friend Dr. Watson to whom I showed it here last Sunday, offered to bring it to town on Monday morning; but in hopes of having an additional observation with a 20 foot telescope, I kept it till this morning.

Gauss to Herschel, Brunswick, February 20, 1802

I hope that you, dearest Doctor, will kindly accept earlier than expected my small ephemeris of our new planet. This ephemeris was calculated according to once improved elements and will hopefully be correct to the minute in March and April. So it might contribute to facilitate the observation of this faint star. The elements themselves will be published in von Zach's M C; they do need further improvement: but I hope to be quite precise with the observations made until now and those until the next conjunction. And then it can soon be decided whether Ceres Ferdinandea shares the fate of Uranus Georgius—being observed earlier but not recognised. I am very curious to learn whether your observations will bring us this time already Ceres' satellites or other physical peculiarities of this planet and I would very much appreciate it if you could advise me in such a case. (Gauss, 1802b) [Ed. The table Gauss appends is the same as the one printed by Zach in his February 20 letter to Méchain.]

After Ceres had been discovered, on February 20, 1802, Herschel's friend Patrick Wilson asked him to help with the preparation for an article for *Tilloch's Journal:*

I have this moment, just at post hour, received a note from Mr. Tilloch Editor of the Philosophical Magazine, very respectfully entreating <u>*some short account of*</u> <u>*Gauss' paper*</u> *about the new Planet, to appear on the 1st March—and such an account as may appear to be communicated by one who attended the meeting—he says it he receives it on Monday it will be in time for insertion—so I have acquainted him that probably I may get something drawn up that would be correct and particular; so as to do justice to Gauss' paper—I wish much however that you yourself would draw up such an abstract and send it to me for tomorrow's post, and which I could* <u>*transcribe*</u> *for Mr. Tilloch; so as you should not appear.* (Wilson, 1802b, his underlining)

Just 6 days later, another close friend, William Watson (1802a), held out great hopes for Herschel's investigations of Ceres:

You sent me word in your last [letter] that you were diverted from the pursuit of the new planet by some other object of attention, and that you had met with great success. I am informed also you have sent an account of the new planet, as has also Dr. Maskelyne and Mr. Aubert. If it has any moon or moons you will be the first to discover them.

Despite his deliberate neglect of Herschel in 1801, Piazzi on March 2 (1802b) then had the unmitigated gall to ask Herschel to keep him informed about any discovery Herschel might make about Ceres:

In your very kind reply to my letter about the new planet, you had indicated your wish to be informed if I found it. It is to fulfil this obligation that I take the liberty to write you. I have no doubt that by now you have seen it more than once, and possibly have even improved on my discovery. So I found it on January 23, having not been able to make any observation before then. Its position was fairly in keeping with the ellipse provided by Mr. Gauss. I will follow it continuously until its immersion into the rays of the sun.

Please make sure, with the goodness that is so natural to you and so much to your credit, to keep me informed of everything that you discover about this new star with your great means and your great intelligence.

Even though outwardly Herschel maintained cordial relations with Piazzi, one can only imagine what he thought of such effrontery at the time. On March 9 Wilson (1802c) also communicated to Herschel about a visit to Sir Joseph Banks:

Last Sunday's evening I was at Sir Joseph's rooms when he inquired kindly about you, and expressed some hopes of hearing farther from you as to the new Planet by Thursday, in consequence of our having lately, some intervals of a clear starry heavens.

PS. I have received back Piazzi's and Baron Zach's schedules upon the new Planet—pray shall I send them out to you in a parcel by coach? Mr. Tilloch has been greatly obliged by them.

Three days later, Alexander Aubert wrote Herschel:

I saw the Ceres Ferdinandea Saturday night the 6th instant, it had at 9 h 17′ mean time 185° 50′ RA and 16° 3′ Dec. N.

The 9th March near midnight it had 185° 55′ RA and 16° 22′ Dec. N., so it alters very little in RA and much more in Dec. I hope you received my line advising you of my first sight of it. (Aubert, 1802a)

Maskelyne to Herschel, Greenwich, March 16, 1802

I have the pleasure to send you some places of Ceres Ferdinandea sent me by Dr. Gauss, true to a minute. I see you acknowledged my letter about the planet to the Royal Society. Hope you will determine the app. diameter of it with your telescope, which will show it the smallest. [Maskelyne means here that Herschel's large telescopes will hopefully be able to resolve Ceres, even though it is the smallest of the planets. He includes an ephemeris from March 16 to April 18.]

Chapter 11
Letters Between Piazzi and Oriani

These letters, covering the period 1801–1805, describe in vivid detail the discovery and investigation of Ceres. The two friends wrote more than 200 letters to each other between 1791 and 1826, the year of Piazzi's death. They were published in the original Italian by Cacciatore, G. and Schiaparelli, G. V. (eds.), 1874; their Roman numeral numbering of the letters is adopted here. Sections of the letters dealing exclusively with Pallas and Juno will be published in other volumes. Extracts of the letters dealing with Ceres are given here (Fig. 11.1).

Letter XL, Piazzi to Oriani, January 24, 1801

Although the current political circumstances have interrupted all our correspondence, I hazard nevertheless to write to you, impatient as I am to give you news, which you will not find unpleasant. On the first day of January, I observed in Taurus an 8th magnitude star, which the following night advanced approximately 3′ 30″ towards North and about 4′ towards Aries. By verifying my observations on the 3rd and 4th of January, I found more or less the same movement. On the 5th, 6th, 7th, 8th and 9th of January the sky was overcast. I again saw the star on Jan. 10th and 11th and subsequently, on the 13th, 14th, 17th, 18th, 19th, 21st, 22nd and 23rd of January. Its RA in my first observation was 51° 47′—with a declination of 16° 8′ N: from the 10th to the 11th, from a position of retrograde motion it became direct, and on the 23rd of January I observed RA 51° 46′ with a declination of 17° 8′. I have announced this star as a comet; but the fact that the star is not accompanied by any nebulosity and that its movement is very slow and rather uniform, has caused me many times to seriously consider that perhaps it might be something better than a comet. I would be very careful, however, about making this conjecture public. When I have gathered a greater number of observations I will then attempt a calculation of the elements.

In the meantime it would please me very much if you took it upon yourself to observe it, informing me of your thoughts on it and of any one else who may have seen it.

Letter XLI, Piazzi to Oriani, April 11, 1801

I wrote to you in February [probably refers to January 24] giving you some information about a comet I discovered the 1st of January, and kept observing with some interruptions till the 11th of February. Often I was very sick and am not yet well. A few days ago Lalande asked me for some of my observations and I sent him the one about the comet. I send you the same information, but I beg you not to make them public. If you can calculate the elements

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2_11

Fig. 11.1 An 1833 painting of Barnaba Oriani by P. Narducci (Courtesy of Milan, Pio Albergo Trivulzio. Used with permission)

please send me the results. It has already been three years, and I still don't have your Ephemerides. Now that Italy is peaceful, I hope I will have them soon. Please arrange to send them to Naples.

Letter XLII, Oriani to Piazzi, April 15, 1801

I received your letter dated January 24 only at the beginning of this month [note: that January 24 letter has the post office stamp of March 20, Milano]*, so you can imagine how possible it was to find the new star you discovered. We have had very bad weather for five months and for the past 10 days it has been raining continuously. I congratulate you on your discovery. I don't think anybody else discovered it. Due to its faintness, most astronomers will not have seen it anyway. I believe you continued to observe the star until it reached conjunction with the Sun, and by now you will be in a position to decide if it is a new planet. I can hardly wait for your answer. In the meantime, I sent to the astronomer of Gotha [Zach] your letter so that it may be published in the journal.*

From the two observations you told me exactly that: the 1st of January it was RA 51° 47′, Declination 16° 8′ and the 23rd of January RA 51° 46′, Declination 17° 8′ and due to the fact that on the 10th of January the retrograde motion became direct I deduced that the star, if it is a planet, has its orbit between Jupiter and Mars, and its distance from the Sun is about three times the semi-major axis of the Earth's orbit. In 100 days its movement around *the Sun is 18° 19′ 7″; on the 31st of December 1800 at noon its heliocentric longitude was probably 66° 54′. Its ascending node was 3 seconds 8.5°, and the orbital incl. 3° 50′.*

These determinations are very uncertain because of having only two observations, of which I was not told the time (hour and minutes), and further they are based on the assumption of a circular orbit, whereas it is very probable for this planet to have an orbit with eccentricity as the other planets have. Only you can erase such an uncertainty by communicating your further observations and results. So I really count on you to satisfy my impatient curiosity, and that of other continental astronomers.

Letter XLIII, Piazzi to Oriani:

[Regarding this letter, Oriani wrote as follows: "*Afterward I received another letter from Piazzi dated May 7, 1801, in which he told me of his bad health and replied to me about his doubt that the comet he discovered could be a planet.*" The original of this letter was sent to Zach, in Gotha, on June 20, 1801. In fact, in the MC vol. 4, pg. 165–166, we can read: "*Professor Piazzi, according to Oriani, is finding reason to doubt that it is a planet because of the lack of any reasonable ratio between the retrograde arc he observed, and the movement during the day.*" This statement was in Zach's journal of August 1801: "*It seems that in the same letter Piazzi stated the intention to name the new star Ceres, assuming it was a planet.*" That is at least the logical conclusion, considering what Oriani is writing to Piazzi in the next letter, or perhaps that was in a letter of June 30, 1801 that Piazzi claimed he wrote but is not found today.]

Letter XLIV, Oriani to Piazzi, July 25, 1801, Milan

After receiving your observational data I calculate the orbit of the star you found, supposing it is a parabola. I found the following:

Ascending node	*2Z 21° 48′*
Inclination	*9 33*
Long. of perihelion	*4 10 16*
Perih. distance	*2.1045*
Time of perihelion	*1801 June 21.07*

These elements satisfy many of your observations, but they do not obey all of them. The latitudes deviate a lot, and I think there is an error in reducing the declinations, not to mention that you told me the first time (January 24) the declination was 16° 8′ the 1st day of January, and in the more detailed quotation of the observations the declination is 15° 37′ 43″.5. I did not publish your observations or the parabolic orbit I just mentioned to you, but from Paris has been sent to Gotha's astronomer the calculation according to Burckhardt, which was immediately printed in the MC vol. 4 pg. 58–61. I am sending to you a few papers I received just today in which you will see for yourself everything done to date on your planet. I must tell you that the name Hera or Juno has been given universally by all of Germany, for which it will be very difficult now to rename it Ceres.

Letter XLV, Piazzi to Oriani, August 8, 1801

I wrote to you the 10th of April [on the letter XLI is written April 11] *and the 30th of June* [this letter does not exist anymore. It is possible Piazzi got confused and the letter mentioned is the same as the one dated 7 May numbered XLIII]. *With the latter one I sent to you a review copy of my observations pertaining to the new star* [it was the letter of April 11]. *I have not yet received your reply; I hope you are not sick. I like to think rather that either your letter or mine has been lost. During June and part of July I have been busy calculating my observations. I am finding that the movement of the star can be represented by a parabolic arc. I chose the observations of 1 and 19 January and 11 February in order to combine the first and the last with the mean one. I had to guess the difference of the two* *radii of January 1 and February 11 being 0.26. That done, I obtained a parabola of which the elements are:*

Perihelion	*4Z 5° 28′ 36″*
Passage of perihelion	*184.6487*
Ascending node	*2Z 19° 43′ 0″*
Inclination	*10° 34′ 0″*
Perihelion distance	*0.3713077*

Before trying to find a second parabola I preferred to investigate the correspondence to that hypothesis of the other observations, but I had the feeling that they will never fit into any kind of parabola. I methodically scribed the parabola, and I interpolated on it the observations, and I tried to represent at its best the nature of the arc eventually representing them. I saw it was a circle. So I assembled in a circle the 1 and 19 January and 11 February observations. After a few hypotheses, I found two radii: exactly 2.7067 and 2.6862, which represented the other observations very well, there not even being the difference in longitude of one minute and not 30″ in latitude. The intermediate radii, as proven through the same observations, satisfy it as well. It seems to me I can conclude it is an ellipse with very little eccentricity, so the precise calculation of such a curve will always be uncertain despite the good observations. Considering how small the arc is in between the first and last observations, the RA from January 10 to February 11 has been made contemporaneously to the circle and to the passage instrument. The difference between the two obs. is less than 2 of a second. Therefore in order to try to recover this star I think it was sufficient to calculate its elements in a circle.

The results are:

Distance	*2.6862*
Epoch 1801	*2Z 8° 46′ 41″.4*
Mov. in 100 days	*21 43 50*
Ascending node	*2 20 46 48*
Inclination	*10 51 12*

The average distance deducted from the station time is 2.9352. This radius seems too big, unless I ran into some mistake during calculation. The elongation concerning the time of the station is 4sec 4°. Sidereal Revolution 1656.653. Opposition close to March 1, 1802. Since in the first observations I judged the stars' apparent diameter about 7″ (after the 23rd of January the diameter and its luminosity kept decreasing such that in a few of the last observations, setting the star under the filament, the star was totally covered so I thought it was a comet) so I concluded its ratio to Earth's volume to be 1.33. I think we cannot discern this star before December. This will be the time I will try to find it again.

This is the abstract of the small memorandum I had to present to the delegation which decided to publish it. [Results of a new star discovery the 1st of January at the Royal Observatory of Palermo by G. Piazzi, Director, submitted to the study delegation in Palermo 1801. At the Royal Publisher]. The rituals of this country did not allow it to be published. I hope it will be soon. Then I will forward it to you immediately. Don't forget my request for your ephemerides and other books.

[What Piazzi refers to by the "rituals" that did not allow his work to be published is not clear. However he had already roused the ire of the Dominicans, who were in charge of the Inquisition. After its reorganization in 1542, the Inquisition assumed supervision of printing projects in Italy. It was not until 1822 that publication of books on modern astronomy was allowed.]

Letter XLVI, Piazzi to Oriani, Palermo August 25, 1801

Your letter dated July 25 arrived just as I was publishing my memorandum. I was on the point of adding the elements of your parabola with the abstract of the two German notes you sent me. If the Germans think they have the right to name somebody else's discoveries they can keep calling the new star the way they want, for we will always call it Cerere. I will be very glad if you and your colleagues will do the same [the name Hera was proposed in 1786 by the Duke of Gotha for the unknown planet. The Germans never meant to deny Piazzi the right of naming his own planet. In fact, they were the first to use the name Ceres]. *What interests me is to be able to see it again. Unfortunately I can't look for it other than when it is passing the meridian because I don't have either an equatorial or parallactic machine. You have it, so you'll find it before I will. The ellipses by Burckhardt do not satisfy me. The period of revolution seems too short. Due to your knowledge you can calculate it very easily, a big advantage for me and yourself in finding the star. You can consider the observations precise and good. The German note told me to reexamine the reductions, but I found only very small differences which you'll find on the enclosed sheet. It would be for me a very big consolation to have a friend and fellow Italian to check my discovery. If you solve the ellipses, send me the results with the positions of the star on 15 and 30 November. Keep me informed what others are doing in the matter at the present time. I receive only letters from you and Bode.*

Letter XLVII, Piazzi to Oriani, Palermo November 6, 1801

I hope you received my letters of July and August with the memoirs. I sent in Sept [note: the two letters dated August 8 and August 25 arrived in Milan the 4th and 18th September as per stamps]. Since the weather was very good on the 3rd and 4th of this month, I tried to locate the star. Because I don't have the equatorial or parallactic machine, I used the Azimuth and polar distances from Burckhardt and mine. From Burckhardt's calculations I found a little star that for 24 hours made me think it was mine, but through many observations she maintained the same RA and declination: 10h 51m 8″ and 73° 50′ 40″ as a polar distance. Did you find anything? I begin to be doubtful. For the time being, in order to try everything, I calculated its RA and polar distances for the complete month of December, in which the meridian should pass one hour earlier than sunrise. If you have all the data rectified, please send it to me immediately so I don't waste time.

Letter XLVIII, Oriani to Piazzi, February 1802

I just arrived from Lione [note: Oriani was a member of the Committee of Lione until the end of January 1802, so he could arrive in Milan only in February. That's how this letter was dated] where I bored myself for two months. I find here two letters from the astronomer of Gotha for you and the other for me. I read immediately the two to you and I transcribe the observations on your planet Ceres done in February:

1801 Dec. 7 18h 48m 10s,3 m.t. RA app. ?	*178° 33′ 30″,60 Dec. north*	*11° 41′ ½*
1802 Jan. 11 17 3 17,4	*186 45 49,95*	*11 10*
Observations of Olbers in Bremen		
1802 Jan. 2 11 58 36 t.m.	*185 9*	*11 7*
1802 Jan. 5 17 30 0	*185 43*	*11 8*

It has not yet been seen in Milan, or in Paris either, I believe. In the meantime, I congratulate you for the discovery which is now no longer in doubt, and I hope you will transmit the observations done so far. In two days I will send you the books received from Lalande, along with other things.

Letter XLIX, Piazzi to Oriani, Palermo, March 2, 1802

I have not received anything from you since July. I don't know why. I don't want to think I have lost your friendship. Afraid that some letters got lost, I send you this one by means of a merchant to which you can give the books for me. Don't forget the ephemerides and booklets I have been missing for a long time. On the 23rd of February I started to observe again my planet. It is my intention to follow it till it is visible at the meridian, intending to calculate afterwards its orbit based only on my observations. To do that, would it be too much if I ask you to give me suppositions and formulae most useful to me without having to go back to the gravitational theory? Mr. Zach calculated from the RA I published an average time different than mine. The mistake is on his side because he did not take into account the time equation from noon hours to the moment of the observations. Despite that I feel very grateful to him because he trusted me like one of his best friends. Dr. Seyffer has translated in German my memoirs on the new planet, to which he added a very wise appendix. He sent me a few copies—one is for you.

Letter L, Piazzi to Oriani, Palermo, March 12, 1802

I just now received a letter from you. I didn't receive any for a long time and did not know of your trip to Lione so I was really worried. Now I am happy to know that you are back and in good health.

Of the two letters by Zach you told me you sent I received only one, the one on which you wrote the observations of Observations of Zach in Seeberg Zach and Olbers.

From December 26 to February 23 I could not do any observations. The 23rd of February we finally had a beautiful night, during which, having searched for Ceres with Gauss' elements, I found it immediately. I confirmed with the observations of the 25th, not being able to observe the night of the 24th because of bad weather. Since the 26th I have observed it many times at its passing the meridian and I will continue to do so for as long as possible. The 10th of March, having a beautiful sky, after its meridian passage I tried to follow it and continue the observations after I took off the light and changed the eyepiece with one of 170 power. Everything went OK, and I observed it for about half an hour. Its colour was no longer like Jupiter, but almost reddish. The size was equal to the stars of 6th or 7th magnitude. What really struck me most was a small glittering point visible only when it was in the middle of the field. This pt. was located east of Ceres 4′ to the N. I am very anxious to know if it was a little star but I have not able to determine that because just this morning the sirocco wind [a hot wind from the Libyan desert] *started to blow and it usually brings fog.*

It is my intention to calculate Ceres' orbit based only on my obs—the ones done up till now, and the ones I will make until May. Can you make this job easy for me by giving me the simplest and most trustworthy formula. At the present time I am too tired to resume the gravitational theory. In case you want to do it I can send you all my observations, if they are of any help to you.

Zach found many mistakes in one table of my Memoirs. One is very gross—I am ashamed and don't know how it happened. In order to simplify the conversion of the sidereal time in average, I used the apparent ascension of the Sun according to the Nautical Almanac. Instead I applied the equation of time directly to the RA of moonhour. I applied it to its difference with the right ascension of the planet: hence I introduced the correction pertaining to the time interval from noon hour to the time of the obs. But the correction has no meaning because it was not dark, but I only realised that after I received Zach's data from you. So don't be surprised about anything I wrote in my last letter. Regarding the other mistakes I think Zach could let them go, considering they were writing errors, not impairing any results by me or Gauss. Furthermore, I wrote that by memory against my will because I was convalescent from a terrible sickness. Only this winter can I say I feel better.

Letter LI, from Oriani to Piazzi, April 10, 1802

I received two of your letters almost at the same time, through the usual way by Mr. Gnecchi of Genoa. I thank you for your interest in my health. I wish the same for yours, so you can continue longer and promote the beautiful science of yours. In Leon I talked about *you to Bonaparte who was very excited to know you were born in Ponte di Valtellina. He was talking about your planet, at that time not yet seen for the second time*[1] *and about Laplace's statement saying he believed in its existence more than Lalande.*[2] *This old astronomer made the same insult about your obs. of last year, and when he received your last corrections he stated you did a 3rd edition of your obs., accusing them of being chimerical. You have to understand an old man so close to the end who would like to see everything to end with him.*[3]

I calculated the perturbations of Ceres, and I send you a copy [this was pub. in the MC, vol. 5, pg. 586]. *If you want to find the exact elements of its orbit, based only on your obs., it is sufficient for you to send me only the ones done this year. In case you don't want to do this, you can follow the same method I used for Uranus, which you can find in our ephemerides of 1790 and 1791. On February 24 I too observed Ceres but the bad weather did not allow me to verify the obs. other than on March 10; since then I observed it continuously and I hope to see it for a long time by means of the equatorial sector. Too bad you have not received all the letters from Zach. It has been a long time since he wrote to me, but recently he sent me some pages of his journal that I send to you immediately. If I get any news about Ceres, I will send it to you.*

Letter LII, from Piazzi to Oriani, April 15, 1802

I thank you for the obs. of Ceres by you and others. Here are mine up to today—I forwarded the same to Zach, but who knows when he will get it. The mail is in trouble. If you think Zach will enjoy them I beg you too to send them to him. The king ordered a medal to honour the new deity, Ceres. When it is produced you will have one. How big is Ceres, in your opinion? It seems to me about 4″—I am not able to match the diameter I obs. to the one Zach tells me was observed by Schroeter. I can't convince myself it is surrounded by a dense atmosphere. At the end of March, the sky being very clear, the planet appeared to me to shine at mag. 7 or 8. I did not get the books you sent in June.

Letter LIII, Oriani to Piazzi, May 8, 1802

I received your obs. of Ceres, and I sent a copy of them to Gotha. I send you the ones done in Germany and somewhere else with Zach's original. I am not sending to you the one I did because it was not as precise as yours since I don't have an equatorial sector. You have probably received Ceres' perturbations by me. I am also waiting for those of Ceres.

Letter LIV, Piazzi to Oriani, June 4, 1802

[first part of letter is about Pallas]. *I thank you for the beautiful job you did on Ceres' perturbations. Now it would be convenient to calculate its orbit, which I will not do since you have already begun. Here are all my obs. After May 23 it was not possible to see its meridian passage. I have just published a booklet on my discovery and the king gave me a pension of 200 ounces. Will you please find out if name Ceres Ferdinandea has been properly given. I send you through Genoa 4 copies, 3 for you and your colleagues. The last one*

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[1] Napoleon arrived in Leon for the consulta on January 11, 1802. His conversation with Oriani took place shortly after.

[2] Regarding the conversation about Ceres between Napoleon and Laplace there are some reports in the MC vol 5, pg. 280. Laplace wrote Zach: "Bonaparte to whom I talked about the new planet a few days ago, to understand he is an old man close to the end of his life, who would like to see everything."

[3] We have to admit that Piazzi corrected his obs. many times; the first one he sent to Oriani in Letter XLII were very far away. Lalande was not so wrong after all. He apologized in the best way because he wrote Zach, very proud of being Piazzi's teacher. "I will never allow to be removed from that planet the name of my student Piazzi, and replace it with Ceres, which means nothing to me." MC vol. 5 pg. 280: The strong vilification by Oriani has to be seen as a temporary break of friendship towards Piazzi, and not considered serious.

please give to my nephew. If he is not in Milan please send it to my brother in Valtellina. I have not yet received the cases of books from Genoa. I would like to have them in order to see the 50,000 stars of Lalande before I publish my catalogue, which is almost ready.

Letter LV, from Piazzi to Oriani, July 2, 1802

I hope you won't be sorry if I transcribe a letter recently received from Herschel. [Herschel to Piazzi, May 22, 1802]. *What do you think? It looks to me. (1) Whatever the name given to this new star doesn't really matter. Are they moving stars? You can call them planetoids or cometoids, but not asteroids. (2) For me the only difference between comets and planets is their eccentricity and inclination. Consequently Ceres is a planet and Pallas a comet. (3) Ceres' diameter is certainly not less than 5″ at our distance from the Sun; therefore it has to be much larger than 162 miles. (4) If we call Ceres an asteroid so we must call Uranus an asteroid. If you decide to insert this letter into the Opuscoli it is fine with me. I have not received the books from Genoa yet. I am sorry not to see the catalogue of stars by Lalande before giving the last touch to mine.*

P.S. I don't know if I told you, that before the king's departure to Naples I was able to have the money for the medals spent in purchasing an equatorial sector. So I wrote to Maskelyne to make one of 6 foot which will be totally paid by Royal account.

Letter LVI, from Oriani to Piazzi, September 1, 1802

A few days ago I received your Memoirs about Ceres and I gave everyone the copies as per your instructions. I have repeatedly looked for the two cases of books, and every time they tell me they would be sent shortly. The delay was due to a lack of ships to Palermo and to possible African piracy. I hope by now you have received them. Herschel's ideas about the new planets are quite crazy. No astronomer can find them useful and Zach, in his journal, has rejected them for the same reasons you did. I observed Ceres till August 5 and Pallas till August 8; I saw Pallas again August 17 & 18, but it was so small and dim that I could not do any obs. Now I am very busy in reorganising the formula to calculate the perturbations of Pallas; the very large inclination and not so small eccentricity require new and complicated formula not even available in Laplace's Mechanics.

You did the right thing to convert the money for the medal in buying the equatorial sector; with it your speculae is totally complete.

Letter LVIII, Oriani to Piazzi, November 29, 1802

In a few days I will publish my astronomical bazzecole [publications of secondary importance] *in which you will find, besides obs. of Ceres and Pallas, some new formula, much more complete than the Mechanique Celeste by Laplace for calculating the two new planets' perturbations.*

I will write to Zach soon and will tell him to keep up with the correspondence to you. I have not yet received the tables by Burckhardt which you mentioned to me. I send to you an abstract on Ceres and Pallas positions for the year 1803 which is from the Gotha journal of October.

Letter LIX, from Piazzi to Oriani, December 24, 1802

Your letter was dear to me and at the same time made me sad. You have given me the best chance to rest and to finish my days with a happy, quiet, honoured life inside my fatherland. Yes, my dear friend, I feel all the advantages of the honoured position you are proposing to me and I can see how great is your friendship to me. But because of duty and gratitude I have to decline it. This observatory is my creation. It is not perfected yet. I am writing for an equatorial sector from London and of a circle from Paris. If I leave everything is lost, and perhaps forever. Astronomy in Sicily does not yet have deep roots. On the other hand the King always recognised and honoured me, and has been a great benefactor. I will tell you only one point that will never be erased from my soul. When he came from Naples without notice, he made everybody leave, even the Viceroy himself. I was the only one who was allowed in the room by the express order of the King. Would it be proper now for me to forget what happened with the King? You yourself I am sure could not approve my decision to leave.

Letter LXII, Piazzi to Oriani, May 4, 1803

I sent to Genoa a box for you containing 9 copies of my catalogue: one for you, for your observatory, one for Cagnoli [Antonio Cagnoli, 1743–1816, diplomat of the Venetian Republic] *and one for Prof. Cossali* [Pietro Cossali, 1748–1815, taught physics and astronomy at the Univ. of Parma]. *The other ones please send to Vienna to Triesnecker and ask him to distribute them.*

Baron von Zach wrote me two very nice letters. He sent to me the article of his MC in which was my answer concerning the professorship at Bologna which was offered to me. I have not yet replied because I am waiting first to observe Pallas and Ceres at their meridian passage which I hope to do around the middle of this month.

Thank you for the tables of Ceres' perturbations which I received yesterday.

PPS. On May 13 I think I saw Pallas and Ceres. Ceres was about at the vertical of 48° of Sagittarius. Pallas was very small.

Letter LXIII, Oriani to Piazzi, May 19, 1803

The small stars not visible in further obs. were probably Ceres and Pallas. You know Olbers hopes these two planets to be fragments of a bigger one that used to exist between Mars and Jupiter. Who knows how many thousands of these fragments are in the sky? Your discovery gave us Pallas and for sure will give us many others so that probably it will become fashionable to look for other planets and it will be considered vulgar to discover comets. I have seen Pallas again on March 22 and Ceres on April 17. The obs. are constantly interrupted by clouds.

Letter LXIV, Piazzi to Oriani, June 4, 1803

In the middle of this month I will resume the obs. of Ceres and Pallas which I interrupted because of my usual troubles.

Letter LXVI, Piazzi to Oriani, July 15, 1803

I should tell you a few things about the last obs. of Ceres and Pallas but having this letter sent to you in a different way I didn't want to make it too long.

Letter LXXIX, Oriani to Piazzi, October 3, 1804

Harding of Lilienthal has discovered a third little planet on September 5 while he was putting together the catalogue of all the little stars that are in the zodiac of Ceres and Pallas. This little planet is similar to a star of mag. 8, or if we want to say it better it is similar to your Ceres. According to its movement it shows an orbit which could be in between Jupiter and Mars, which consequently belongs to the same family as Ceres and Pallas. [more of this letter in other volumes in this series]

Letter LXXX, Piazzi to Oriani, October 11, 1804

At the present time I am observing Ceres at the request of Triesnecker, who wants to calculate again the orbit and develop the tables of it. I was going to do this but I don't dare challenge such an astronomer and on the other hand I am very busy. Gauss' elements need to be corrected because the declinations we have from them are 15′ less than the ones I observed.

Besides the Paris prize I received the appointment as foreign member from the Royal Society of London with a very honourific letter. Shortly I will receive from London the equatorial sector, already finished, paid for and sent, so what is missing at the observatory is only a better astronomer more active than myself.

Letter LXXXIV, Oriani to Piazzi, November 28, 1804

At the Ceres opposition of September 27, 1804 I found the elliptic elements corrected by Gauss last year—8 times—together with equations of the Jupiter perturbations show an error in the geocentric long. of –4′ 2″.5 and in the geocentric latitude of –6″.5. So it seems that these elements are not too far from reality.

I am very happy for you for the well deserved honour with the Royal Society of London and the savants of Paris attributed to you: and again I am happy for all the instruments you are buying and I wish you a very strong health so you'll use them for a long time.

Letter LXXXV, Piazzi to Oriani, December 26, 1804

I cont. obs. Ceres and Juno till the fog moved in. In the enclosed envelope you'll find the last one. They cover the period up to December 10. I sent to Zach all the previous ones and the others of Ceres done in 1802–1803.

Dear friend, you forgot my insistent request. Considering I have a very small hope of seeing you again, I would like, at least, your picture in my room with the one of Lalande, Herschel and Ramsden. You have been so nice to me many times—please be once more—send me you curriculum with birthdate. I am not a painter otherwise I would do it myself. If you keep your silence on this matter I will be forced to have it done by description. [They are printed in Vol. 11 of the MC, pg. 290 and 475.]

Letter LXXXVI, Oriani to Piazzi, February 1805

I sent to Seeberg in Gotha to adviser Lindenau, who is substituting for Zach, your last obs. of Ceres and Juno. Mine, done with the equatorial sector till the 4th of this month, are completed as well. I am sending to you the picture of me you asked for. Cesaris told me I look too fat and old, but it is a good likeness. I was born July 17, 1752. Now do me a favour and send me your picture and curriculum with all your positions in science.

Chapter 12
Letters Between Olbers and Gauss

Beginning in 1802, Gauss and Olbers exchanged hundreds of letters. These were originally published in German by Schilling (1900), and his numbering system is retained here. Only their correspondence dealing with Ceres is translated here. Their letters about the other asteroids will be featured in subsequent volumes of this series. For an overview of the Olbers-Gauss letters, see Oestmann and Reich (2001). This chapter also includes three letters from Bode to Gauss dating from early 1802. Bode, in his book, alludes to the first of these from February 16 (Fig. 12.1).

No. 1, Gauss to Olbers, Brunswick, January 18, 1802

I have arranged my latest calculation so that I am able to correct the elements from one single accurate observation without much effort and without doubt they would become accurate, to such a degree that one could expect the planet always with certainty according to them in the passage instrument until its next disappearance after the next conjunction. With pleasure I would like to take on the task of calculating such an ephemerides, because in my restricted situation I have to confine myself to theoretical work and refrain from observing. According to my latest results, published in v. ZACH's Monthly Correspondence, I once more calculated the orbit, taking into account partly the newly calculated Sun positions and the newly reduced longitudes and latitudes according to the new inclination of the orbit of 23° 28′ 5.3″ and partly also the parallaxis. These elements, calculated in this way and as much corresponding to all of PIAZZI's observations as possible, together with an ephemerides calculated until the first of March this year, I had already sent Mr von ZACH at the beginning of this month; until now I have no further news from this splendid astronomer. The fair weather during some nights of this month seemed to be quite common throughout our regions. The position of the planet on December 31st according to these 5 elements deviates only by 12′ in longitude and 17″ in latitude. Because these calculated positions are fairly close to your observation, as far as I can see from the information in the Hamburger Zeitung [Zeitung = newspaper], *I take the liberty of enclosing the elements together with the ephemerides, because they might serve for a far more easy rediscovery after minor interruptions and at least show the path which the planet must take. It is true luck that the inclination of the orbit to the ecliptic is so great; without this circumstance a tolerably accurate determination of the orbit from such a small number of observations would have been impossible; would the orbit coincide with the ecliptic, the observations could still have been represented very probably quite well by a parabola, and then we would have been groping about much longer until one would have got on the planet's track again.*

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2_12

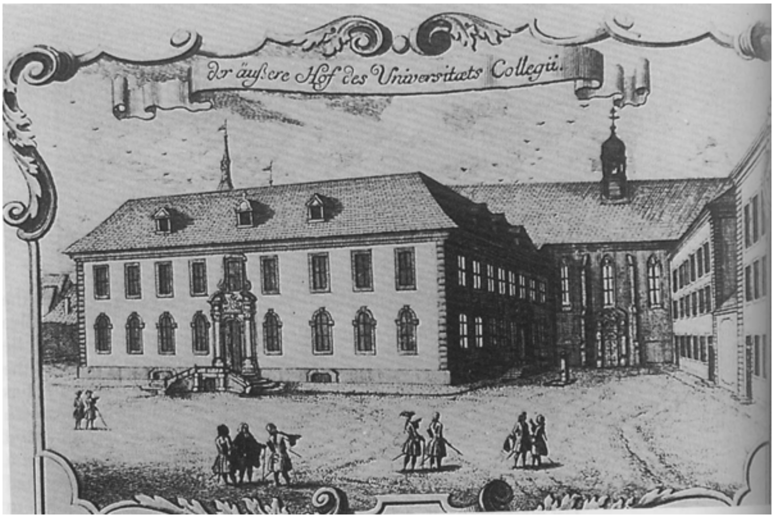

Fig. 12.1 An early view of Goettingen University, where Gauss worked

Elements of Ceres	
Daily mean tropical motion	*763″.95*
Epoch 1801, Palermo meridian	*78° 5′ 17″*
Epoch 1802, Palermo meridian	*155° 32′ 38″*
Perihelion} both for 1800 Dec. 31 and	*324° 37′ 11″*
Nodes} assumed as sidereally stationary	*80° 59′ 12″*
Eccentricity	*0.08791104*
Max equation of the centre	*10° 4′ 58″*
Semi-major axis	*2.7840721*
Inclination of the orbit	*10° 37′ 10″*
Tropical period	*1696½ days*

[Follows ephemerides of Ceres of Dec. 18th, 1801 until March 1st, 1802]

No. 2, Olbers to Gauss, Bremen, January 22, 1802

You are asking me for my observations on Ceres and who else had the right than you? We owe the rediscovery of this new planet only to you, my dear friend, at least here in Germany. I, at least, as Mr v. ZACH has probably told you (for I instructed him to do so, and admit it willingly) would have hardly searched for Ceres that far east, if your elliptical elements had not been calculated. I had been induced, like you may have read in M.C., by the great correspondence between the circular elements and PIAZZI's observations, to the erroneous conclusion the elliptical elements could not be derived with any certainty from those; and for a long time I considered the positions calculated from BURCKHARDT'S ellipse nearly as the outermost boundaries of my search eastwards. I even believed myself justified to the completely wrong assumption Ceres must have been during PIAZZI's obser-

vations near the line of apsides. Again a new lesson for me, that you should not rely on an awkward assumption where certainty can be obtained from calculation.—Fortunately you were not prevented by such assumptions from calculating from PIAZZI'S observations the elliptical elements as accurately as possible. The success does your calculations and PIAZZI's observations equal credit and brought Ceres back to us, which we would never have found again using the circular elements.

Here, my dearest friend, are all my observations, as good as I had been able to make and reduce them. For the most part they were made at the circular micrometer, except for the last two, which I carried out in moonlight at the micrometer. RA and decl. are apparent and have to be corrected by aberration and nutation, if you consider this correction necessary. For with leniency you might remember those observations were made at the passage instrument and circle. I consider the major part of the RA very accurate (if the RA of the compared stars is accurate enough); I cannot give the declination as reliable. In my opinion the observ. of Jan. 15th might be one of the best. [The table that follows is the same as the one printed in the MC for Feb. 1802, see Zach, 1802z.]

You might find by comparison that your elements, given in your latest letter, state the RA too large by about 35′ and the decl. too small by 16′. Truly, after an entire year an absolutely minor error! I can easily imagine, like you said, that your calculations until now make it easier to find the corrected elements of the ellipse.

Certainly, you, my admirable friend, are so kind as to give me the results of your calculations, as I will not miss to communicate my observ. I would like to ask you this favour.—Friend ZACH has praised your method so much that I am very eagerly looking forward to getting to know it. Ceres, according to theory, is strongly increasing in brightness.

Certainly, it was on the 20th brighter than an 8th magnitude star, only still a little more inconspicuous than no. 27 Flamst., to which it was close. Certainly PIAZZI's estimate gives the apparent diameter too large; but I believe that it will be maybe visible to the unaided eye at its opposition in the meridian in fair weather.

As soon as you have corrected Ceres' orbit one still has to do many calculations: I mean all oppositions of Ceres during the last 15 years and its positions regarding RA and decl. about every 15 days, 2 months before and 2 months after each opposition, in order to see whether Ceres is mentioned among Lalande's 50,000 stars. Only today, Jan. 22nd, I received a letter of Mr v. ZACH, dated January 17th, who is reporting he had observed Ceres already on Dec. 7th. There is no doubt about that and the observ. was published, as he is writing and can be seen from some enclosed papers, already in the January issue of M.C., which has not yet arrived here.

With no-one else would I like to share the small honour of Ceres' rediscovery better than with my honourable friend ZACH. I say share: for ZACH had sighted it indisputably earlier, but he seems not to have recognized it with certainty much earlier than me, and thus at least a shy claim for participation might be justified. Until now I have not heard anything from foreign astronomers yet; but I hardly doubt that it has not been found in France and Italy now that it is so conspicuous. Your remark that only the considerable inclination of Ceres' orbit made it possible to calculate the elliptical elements with some certainty, is both astute and correct. Forgive, my most admirable friend, my rhapsodic writing.

P.S. I just observed Ceres again, it was: [position on Jan. 22] *After all Ceres was considerably smaller and fainter than no. 27. At a magnification of 180 times I could not identify it with certainty as a planet.*

No. 3, Olbers to Gauss, Bremen, January 23, 1802

I just discovered in the reduction of my observ. of Ceres a fatal mistake. I had actually assumed the RA of Virgo 187° 54′ 57″ because I got 4 and 7 mixed up, consequently the RA is 187° 57′ 54″. As a result all my RA of January 10th given to you, are too small by 3′. Here they are corrected:

Jan. 10	186°	34′	52″
13	187	1	56
14	187	10	11
15	187	18	27
20	187	55	0
22	188	5	45

I would be very sorry, if I had caused you some useless calculations.

No. 4, Gauss to Olbers, Brunswick, January 26, 1802

I just reduced three of your observ. (with 23° 28′ 5.3″ inclination of the orbit).

Jan 10	*Longitude*	*6Z*	*1° 30′ 25.3″*	*Latitude*	*12° 53′ 58.4″*
15		*6*	*2 6 15.3*		*13 20 41.4*
22		*6*	*2 41 16.0*		*13 58 14.6*

The longitude in my ephemerides was therefore on the 10th approximately too large by 34′ and on the 22nd approximately by 38′; latitude now by about 2′ too small. Out of this follows how much my ephemerides must be changed in advance, which will be absolutely sufficient to find the planet until March conveniently. About that time we hopefully will know the elements exactly enough to be mistaken only by parts of minutes in calculating in advance. I also received a message from Mr v. ZACH several days ago; together with three very exact RA; until then he had been unable to take any decl. because the faint light of the planet did not tolerate the illumination of the hairs. Doubtlessly, you already know from himself, that it has been confirmed that the star observed by him on the 7th (no. 1, January of M. C., p. 92), was Ceres. *I have already calculated those three RA according to my 5th elements; my calculation proves them:*

On Dec 7	*24′ 25″}*	*too large; aberration is neglected in this calculation*
Jan 11	*31 0}*	
Jan 16	*31 58}*	

I have been willing to correct my elements according to this data; but now your decl. will serve me well, especially for a more accurate determination of the inclination. By the way, my 4th elements correspond much better to the observ. than the 5th; by now this fact appears to me to be rather a coincidence or maybe a consequence of the perturbations (in so far as those already were already modifying the elliptical orbit during PIAZZI's observ., especially the latitudes, which had an exceptionally tremendous impact on the elements); for I am certain, that the elliptical calculation according to the 5th elements corresponds slightly better to PIAZZI's observ. than the 5th (although the difference is at most 3″ to 4″).

No. 5, Gauss to Olbers, Brunswick, January 29, 1802

I have already started a new calculation of Ceres' orbit and will send it to you, I hope already next Monday, as soon as I have finished and checked it with your and von ZACH's observ. (I am hoping to receive new observ. from von ZACH by today's post). As far as I can see from now PIAZZI's and your observ. and v. ZACH's three RA (Only von ZACH's assumed declination on Dec. 7th appears to me after a rough estimate by several minutes too small.) can be combined quite well with an ellipse; in advance I estimate the eccentricity at 0.079, aphelion approximately 327° longitude; the semi-major axis will come fairly close to that of the 4th elements.

No. 6, Gauss to Olbers, Brunswick, February 1, 1802

My first attempt to correct Ceres' elements, produced the following results: [these are the 6th elements]

I based this calculation on the most distant observ. of PIAZZI, which are free of the most probable observational errors (as I had concluded from my previous calculations) and von ZACH's two RA of Dec. 7th and Jan. 16th, and it turned out that PIAZZI's observ. were accurately represented by the elements found this way. Because the positions for PIAZZI's intermediate observ., calculated according to these elements, vary only by a few seconds from those according to the previous elements, one can be certain that by this all PIAZZI's observ. can be represented quite well. But I could not have expected from this first approximation that the error in the new observational data would be at once reduced from ½° to 0.

When I had calculated from the 6th elements the two RA on which they are based the differences were smaller than expected; in the first the error was +3″, in the second +10″. But unfortunately ZACH's RA of Jan. 16th seems to be too large by about 10″—if anything can be concluded from the comparison of the other observations. I am presenting here the comparison of the calculations with those observ., which I had already calculated, namely a complete from you and 5 RA from ZACH. The great correspondence to the decl. in your observ. surprised me pleasantly; because it depends mainly on the inclination and because in the calculation of the elements only 2 PIAZZIAN latitudes close to the node are used, one could not demand accuracy of the inclination and no correspondence of the decl. That it is nevertheless existent to such a degree (even if it should be slightly smaller in the other observ.) confirm the excellent quality of PIAZZI's zenith distances.

	Calculated RA	*Error*	*Calcul. decl.*	*Error*
Dec. 7	*178° 33′ 33.6″*	*+3.0″*		
Jan. 11	*186 46 9.3*	*+19.3*		
15	*187 18 52.8*	*+25.8*	*11° 23′ 35.9″*	*+10.9″*
16	*187 28 3.1*	*+ 9.8*		
22	*188 6 45.9*	*+20.1*		
25	*188 21 6.5*	*+27.3*		

It would be indeed easy to adjust the elements to the now already available observ.; they certainly would nevertheless require considerable corrections. It appears advisable to me, to wait for further observations, particularly since these elements are absolutely sufficient to find it again until the next opposition. I hope the error will not exceed 1′ until the next opposition, because from Dec. 7th until Jan. 25th, within these 49 days, it has not increased by half a minute. This is the reduction of your 6 observ.:

	Longitude	*Latitude*
Jan 10	*181° 30′ 25.3″*	*12° 53′ 58.4″*
13	*181° 52′ 55.3″*	*13° 10′ 1.1″*
14	*181° 59′ 40.4″*	*13° 15′ 8.7″*
15	*182° 6′ 15.3″*	*13° 20′ 41.4″*
20	*182° 34′ 7.6″*	*13° 47′ 55.2″*
22	*182° 41′ 16.0″*	*13° 58′ 14.6″*

Has Mr Harding's supposition of satellites of Ceres already been confirmed?

No. 7, Olbers to Gauss, Bremen, February 2, 1802

I continue to send my observations to you. But you, dearest friend, seem to consider them more accurate than they really can be. It would be utterly unnecessary, to take the parallaxis into account; even applying aberration and nutation means nearly overvaluing it. You might leniently remember those are only observations made at the circular or cross-hair micrometer. Such observ. are accurate enough for comets, but for planets one actually has to make observations at a passage instrument and quadrant or a circle. My observed RA will be quite accurate, if the compared stars were correctly determined. The decl. might be slightly more inaccurate. Here are my observ. since my last letter:

		RA	*Dec.*
Jan. 25	*11h 36m 0s*	*188° 19′ 50″*	*11° 54′ 43″*
26	*11h 2m 0s*	*188° 23′ 50″*	*11° 59′ 56″*
28	*11h 21m 0s*	*188° 31′ 15″*	*12° 8′ 43″*
31	*10h 44m 30s*	*188° 38 ′29″*	*12° 25′ 8″*

I consider the observ. of the 28th the best. On the 25th the decl. are very dubious.—The observ. of the 31st did not correspond as exactly as those of the 28th.

With longing I am looking forward to your promised determination of the orbit, and then I am very curious as well how v. ZACH's star of Dec. 7th will correspond. It is quite strange, that this experienced observer was mistaken in his estimated decl. by 1/5° or even 1/4°; because according to a rough estimate Ceres must have had 11°54′ to 11°55′ northern decl then.—By the way I searched the place in the sky where Baron v. ZACH observed his 4 stars on Dec. 7th. Now there is no star where his no 1 was; his no 3 is very faint and inconspicuous. It appeared strange to me that no 2 was preceded by another star, much brighter and more conspicuous than no 3, which I compared on January 25th as well as on the 31st with no. 2 and no. 4. I find its RA for 1800 178° 45′ 31″ and north. decl. 11° 42′ 50″, the latter not very reliable.—I am surprised that Baron v. ZACH had not observed this star, too, and that its declination correspond so exactly to his estimate.

But this is all among us. Time will show whether his RA, which he considers very accurate, *corresponds so* exactly *to your ellipse; for it might not be completely impossible that my star inadvertently had become his no. 1, by counting the clock and then of course his no. 1 could not be found again in the heavens. That might be as it will, but I believe, that you, my dearest friend, would be well advised not to use ZACH's RA of Dec. 7th for a determination of the orbit, but check it first by an orbit determined in another way.*

SCHROETER has observed our Ceres with his 13-foot, but not with his 27-foot telescope. It appears to him always as shrouded in a thin atmospheric nebulosity, almost like something between a planet and a comet. I, too, can never receive in my Dollond at high magnifications a distinct image of it; I blame this fact partly on the constantly turbulent air. On Jan. 25th and 26th SCHROETER found through repeated measurements the diameter with atmosphere = 2.5″, without about 1.8″. I asked him to measure both Ceres *and* Uranus; *thus one will be able to judge better how accurate this great observer can determine such extremely tiny objects, which are nearly unmeasurable, according to his method.* Ceres *seems to be smaller than our Moon.—You probably will not rely on PIAZZI's estimate of the diameter of 7″ by comparison of the hairs of his passage instrument: the irradiation and the diffraction of the rays at the hairs render this estimation erroneous. Even a fixed star is not entirely covered by the hair and if the hair is tiny and it seems to stick out at both sides.*

I have not received any observ. on Ceres, *than that of Baron ZACH of Jan. 22nd, only RA, which he probably has relayed to you. BODE merely observed* Ceres *on the 15th. The* Moniteur *of Jan. 20th says expressly that* Ceres *had not been rediscovered in Paris then. BODE communicates an excerpt from PIAZZI's letter of Dec. 8th, in which PIAZZI almost seems to doubt the rediscovery.—Those, who did not know your ellipse then could hardly have rediscovered it.*

Bode to Gauss, Berlin, February 16, 1802

I would also like to congratulate you on your elements of the elliptical orbit of our new planet that you calculated and that correspond so well with the observations. It will be my pleasure to deservedly publicize the proofs you gave herewith of your knowledge and diligence, if you were inclined to communicate your current and future elements calculated from the observations of Ceres as well as the reasons and formulae to calculate these for the next volume of my astron. yearbook, that has been existing in Germany for 30 years as the astron. Journal. I would like to know in particular how you were able to come up in advance with such an accurate result for the aphelion and eccentricity of the orbit based on Piazzi's observations of only 41 days. Neither did I, for my part, spare any effort since September to find Ceres again, but was not as successful as Mr. v. Zach and Dr. Olbers. I saw her for the first time on Jan 15, then again on the 23rd and several time afterwards. I attempted to determine her location at the circular micrometer of the 3½ ft. Dollond as accurately as possible: [Bode here printed his observations of Jan. 25, Jan. 26, Feb. 3 and Feb. 4]

No. 8, Gauss to Olbers, Brunswick, February 23, 1802

I made a second attempt to correct Ceres' elements. Instead of combining von ZACH's RA of Dec. 7th with that of the 25th I combined it with that of Jan. 16th. I would not have needed that of the 7th, if I had had your advice earlier; the success seems to prove beyond doubt that the star of Dec. 7th had really been Ceres. If I combined v. ZACH's RA of Jan. 11th with that of Feb. 7th [apparently means Feb. 9] *it would nearly result in the same orbit, because the error in both observ. nears almost automatically =0. These elements are*: [these are the 7th elements.]

These elements give, compared to ZACH's observ. the RA without exception too small; but the differences are so small and its path so little determined that I am not absolutely certain whether the error in—or decreases; the latter seems to be the case according to the latest observ. and maybe the differences will turn positive after some time. The decl. are slightly too large, as far as I can conclude from the small number of reliable observ., by about 20″. Truly an extreme minor error, if you consider, that for the calculation of these elements only PIAZZI's latitudes, which are so close to the ☊, were used. The accuracy of PIAZZI's observ. and the possibility to predict from them with certainty appears in a very favourable light through this circumstance. Here is the comparison of these elements with ZACH's observ., which I label VII:

The decl. of Jan. 25th and Feb. 5th are described as very uncertain, that of Jan. 30th on the other hand as good and that of Feb. 9th as very accurate. That of Feb. 3rd is not commented; the comparison with the rest of ZACH's and yours, which are all smaller than those according to the calculation, seems to prove sufficiently that this decl. must be considered too large by ½ minute. It would be easy to find an ellipse which corresponds as accurately as possible to the observ.; but because the errors are that small and only of the dimension of the equation of the perturbation, it would be dubious whether you could come any closer to the true orbit. I therefore consider it wiser to wait and to continue to compare the observ. to those elements for the time being. Meanwhile I have calculated the ephemerides according to these elements, which you find enclosed. [ephemerides of Ceres in 1802, March 1 till April 18]

In our region one will probably not be able to observe Ceres *much later than the end of April in the meridian; but you are most likely to observe it for a while around midnight at the micrometer, when the long twilight time has made observations at the meridian impossible. Due to the great confidence I have in the accuracy of PIAZZI's observ. I am hoping that this excellent observer might have already started to observe his planet again. You probably know already that MECHAIN was the first to rediscover* Ceres *in Paris on Jan. 22nd; V. ZACH sent me two* Parisian *observ. I received from BODE 4 observ. at the micrometer as well; but they are not very exact, even the RA seem to be too large by 30″ to 40″, maybe due to an inaccurate determination of the compared star (which he did not name). It* *would be very helpful if some observations from* Greenwich *could be obtained*; *I dared to ask MASKELYNE for some and have sent him the recent ephemerides*.

Bode to Gauss, Berlin, March 2, 1802

I am very grateful for the newly calculated orbital elements of our Ceres that you kindly sent me and the ephemerides calculated based on these. You probably have received my letter of 16 February at the same time.

After 18 days of snow or rain, *we have been enjoying fair weather for three days now.*

After having observed the planet at the circular micrometer of the 3½ft Dollond a couple of times, *I observed at the 5 ft mural quadrant by Bird last night for the first time its culm.*; *I believe I was successful*; *I compared it to β Leo because there was no other well determined star on its parallel. Based on this*, *I calculated the app. right ascension 186 40′ 46″ and dec. 15 29′ 40″ at 1 h 50′ 12″ m.t.*; *I am admiring how exact the right asc. and decl. are described by your elements*; *truly*, *you will always be remembered for this. The corr*[*ection*] *of the aberr*[*ation*] *and nut*[*ation*] *is indicated for β. Ceres culm. only 4′ 48.5″ after Uranus*.

Seyffer to Olbers, Goettingen, March 11, 1802

I am endlessly grateful for your observations on Ceres. I have observed Ceres a couple of times myself. Now if you could—just for my private use—communicate your latest observations.

No. 9, Olbers to Gauss, Bremen, March 16, 1802

I think, *you can be content for the moment with your nice elements no. VII*, *until more time is between the outermost observ.*; *and then above all the perturbations of Jupiter must be calculated. I do not know whether you read the* Moniteur *and therefore am relaying BURCKHARDT's elements mentioned there*, in whose calculation the perturbations of Jupiter are determined in advance. [The table of elements here are the same ones printed in Bode's book, following the sentence "And from a later issue of the Moniteur."]

On Ventose *8th* (*Feb. 27th*) *at 13h 59m 15s mean time Le Francais and BURCKHARDT observed very accurately RA 186° 58′ 44″*; *decl. 15° 15′ 55″* (*I found those on Feb. 27th at 8h 9m 0s mean time through 254 Virgo BODE and 36 Virgo FLAMST. RA 187° 0′ 53″*; *decl. 15° 13′ 58″*).

My further observ. on Ceres, *which I eagerly continue*, *can be of no interest for you*, *because they are*, *compared to ZACH's or the Parisian meridian observ.*, *not accurate enough. Yesterday*, *on the 15th*, *I compared the planet to no. 147 BODE*, *with which* Ceres *was in a close conjunction that night. At 7h 59m 10s mean time* Ceres *was only 17.5s more eastern and only 0′ 12″ more southern*; *BODE states for no. 147*, *according to LALANDE for 1801 RA 183° 59′ 36″* (*45.6*), *decl. 16° 57′ 7″* (*–20.0″*). *No. 147 is a seventh magnitude star*. Ceres *was just as bright. The star appeared to me slightly more reddish. I took advantage of the fair weather and the closeness of that star to observe both of them at high magnifications of 180 and 240 times. But it was impossible for me* to notice any difference between the star and the planet, or any distinct disk of the latter, *because I discerned* Uranus *as a very distinct disk.—I really can conclude nothing else from my observ. than that Ceres is not 2″ in diameter and that which SCHROETER discerns does not show in my telescope.—The same happened to Mr ENGLEFIELD*, *who could not perceive* Ceres *even at a magnification of 400 times as a distinct disk* (*on Feb. 13th*).

MASKELYNE first observed the planet on Feb. 3rd. He probably did not have your ephemerides then. He is not quite communicative about his observations but he is unlikely to withhold those from you.

The star no 100 Fl. *8*, *which FLAMSTEED observed on Jan. 1st*, *1700*, *which cannot be found in the sky nowadays*, *attracted my attention at first. And really the longitude is quite matching*, *but the latitude is 61/3° southern*, *where it should be northern for* Ceres. *And*

Ceres' ☊ *is moved, regarding the fixed stars, backwards only by 27.4″ from Jupiter for every revolution of Ceres.*

Bode to Gauss, Berlin, March 30, 1802

I am sending several observations of Ceres, that I made at the 5 ft. mural quadrant by Bird. I compared the planet to θ, β *and* o[Leo]; *the comparison showed a good correspondence of the r. asc. and dec. and I therefore consider the observation quite accurate. Maybe you could use it for calculating the orbit; in return, I am asking for the results. I wanted to include in the Yearbook 1805, that will be published this year on St. Michael's day, Ceres' course in 1803/04 and 05: If you are inclined to calculate some tables according to your elements, I would like to ask for those for publication in the yearbook and in order to save some time, I wish to calculate the tables according to your elements myself*: [Bode here prints his positional data for four dates from Mar. 15 through March 27] *I hope I did not miscalculate the longitude and latitude.*

PS: I have just received a letter from Mr. de la Lande of March 15. He announces that among others, Mr. Burckhardt had already calculated the perturbation of the new planet and found that the same is up to 30 min. He has calculated tables with new elements; he finds:

Epoch 1802	*5z 5° 32′ 35″*
Aphelion	*10z 26° 44′ 37″*
Node	*2z 21° 5′ 35″*
Annual motion	*2z 18° 13′ 18″*
Mean distance	*2.76587*
Eccentricity	*0.07887*
Inclination	*10° 36′ 52″*

[These figures differ from the elements derived by Burckhardt given in Chap. 3.]

Herschel is said to have found the diameter to be only 1½″. These elements differ considerably from yours. The latter correspond well to my observations.

No. 11, Gauss to Olbers, Brunswick, April 9, 1802

I have not calculated any tables for Ceres *according to my VII. elements yet, and I am not planning to do so, because these elements should merely be a help for this years' observ. I am not intending to correct the elements either, neither to take the perturbations into account, until the observations are concluded this time. To do so earlier appears to me to be an unnecessary effort, because for the accuracy of the results it is extremely important to have observ. which are some months apart, and I would have to do the corrections again. The error in the VII. elements seems to be positive in RA and considerably increasing, according to von ZACH's recent observ. (around the end of Feb. it was 0, on March 15th +15″); Here is the continuation of the ephemerides until the last day of June*: [Ceres ephemerides from April 21st to June 29th, 1802]

I enclose a copy of the ephemerides and the logar. of the true distances from the earth, which I would like to ask you to send to Mr. SCHROETER. The distances until March 19th are taken from my calculation of the Seeberger *observ. and therefore apply to the time of culmination there, the others are from the ephemerides for midnight.*

No. 12, Olbers to Gauss, Bremen, April 13, 1802, midnight

BODE sent me 3 further observ. of Ceres, *which I am relaying, in case you might not know them.*

Mean time		*RA*	*Decl.*	*Longitude*	*Latitude*
March 27	*11h 47m 21s*	*181° 29′ 15″*	*17° 50′ 47″*	*5° 24′ 2.14″*	*16° 54′ 54″N*
April 1	*11h 23h 35s*	*180 26 57*	*18 2 48*	*5 23 0.42*	*16 41 12*
April 3	*11h 14m 7s*	*180 3 13*	*18 6 14*	*5 22 37.41*	*16 34 55*

No. 48, Gauss to Olbers, Brunswick, December 3, 1802

I am calculating now a table for the perturbations of Ceres mentioned in the November issue of the M.C. partially varying the usual procedure. The tables for the perturbations depending only on Ceres-Jupiter of the longitude, radius vector and the latitude are already finished. Soon I will be writing more details.

No. 49, Gauss to Olbers, Brunswick, December 21, 1802

I am planning to send my now completed tables of the perturbations on Ceres by Jupiter to Zach; if they are not specific enough for the M.C. I will send you a copy. To you I can describe and explain the variation in few words. Regarding the longitude all equations, whose argument is a multiple of Ceres-Jupiter form one table; the sum of all others depending on the simple eccentricity can be expressed by A sin (B-Ceres) so that A and B are functions of Ceres-Jupiter only; these values A and B form a second table for all values of Ceres-Jupiter from degree to degree. And there are as well two tables for the radius vector and one for the latitude, altogether five tables where one otherwise would need forty or at least 30 tables for all the equations I need, if you left out those nearing 2″.

At least to me it is far easier to make these easy trigonometric calculations than forming so many arguments, using so many tables, and making so many additions. And the use of my table could easily be more facilitated by adding for instance instead of A log A; or by assuming a constant value =2M, being not smaller than any value of A, or A/M = cos C: thus the sum of the equations depending on the eccentricity is

$$= M \sin(B - C - ⚳) + M \sin(B + C - ⚳)$$

Then one included the values of $B-C$ and $B+C$ for each value of Ceres-Jupiter and added another table including M sin φ for all values of φ. By the way, in my table I did not use the angle B, but for

The longitude	$B - 2(Ceres - Jupiter)$
The latitude	$B - 3(Ceres - Jupiter)$

or I gave the sum of the equations the form of

The longitude	$A \sin(B' + Ceres - 2Jupiter)$
The latitude	$A \sin(B' + 2Ceres - 3Jupiter)$

Thus, the B' always stays within certain limits when Ceres-Jupiter is going through all the values, whereas B would circulate once or twice during one period of Ceres-Jupiter.

No. 51, Gauss to Olbers, Brunswick, January 4, 1803

According to my preliminary tables of Ceres perturbations I have already made several calculations and could convince myself of its excellent usefulness and how it makes the work easier. But I am not quite satisfied with the elements VIII published in the November issue of the M.C. I had only used meridian observations for it that were thus represented very well and much better than by the elements VII. But now that the tedious—without tables—calculations of the perturbations are no longer a hindrance I have already compared even the last of Oriani's observations and to my astonishment found the error considerably larger than according to elements VII. The ascensions are fair enough but the declinations are approximately by 40″ too small so that I assumed the average error in longitude +18″ and in latitude −37″; the error of elements VII is only approximately half as large. I believe this confirms my opinion, previously mentioned, that the unreliability, which is a consequence of too few observations, might have a worse influence on the predictability of the future path than neglecting the perturbations. I therefore would like to polish them and adopt the elements better to all observations available. Almost all calculations which I have to do for this purpose I had to do anyway this year 1803 for using future observations; thus the work is not in vain though not necessary for a rediscovery in the eastern sky; I think the ephemerides calculated by Triesnecker and Bode will be accurate enough.—What prevents, by the way, precision regarding the elements is mainly the short duration of Piazzi's observations in 1801. For an exact determination, if the mean motion cannot be derived from distant observations as it is the case here, it is essential that one has four observations A, B, C, D which are distant enough and the interval between B and C possibly much larger than that of A, B and C, D. Furthermore, one has no other choice but to use Piazzi's first and last observations, which are only 41 days apart. Thus, if only one of Piazzi's observations is necessary for A (which is rendered far more accurate by comparison with neighbouring ones than a single one can be) one can expect a higher degree of accuracy; so 1803

where A *of 1801*
B 1802 (or Zach's observations of December 7, 1801)
C 1802
D 1803

can be used. But a true accuracy can only be expected after the opposition of 1804, when A, B, C, D can be taken from so many different years and 1806 when one can use four oppositions, then—hopefully—the orbit of Ceres will be as well known as that of all other planets. In the case of Pallas we will have to wait one more year for everything.—But if we found—and we do not want to give up this hope yet—the one or the other among older observations everything is to happen faster.

No. 60, Olbers to Gauss, Bremen, April 2, 1803

[first part of this letter, about Pallas, will be in a later volume] *Von Ende, the head appeal counsel* [Ferdinand von Ende], *and Harding, who just visited you in Celle, have again located Ceres on March 22 near 1 and 2 Sagittarius, and on March 23, 24 and 25 saw it recede. Your observations with the circle micrometer turned out so unsatisfactorily (probably because the telescope wasn't steady enough) that the position of Ceres couldn't be determined with any certainty. I have not disregarded your repeated entreaties not to waste my time on Ceres for I'm convinced that it's been found and observed in Paris, Milan, etc. already long ago. Since Ceres will now be about 9th magnitude and its position is uncertain to within at most a few minutes, then there shouldn't be any difficulty in finding it on every moonless and clear night. Any possible observations of mine of Ceres would probably not have any use to you because they're not exact enough to help improve upon such an advanced theory.*

No. 96, Gauss to Olbers, Brunswick, September 7, 1804

At long last I've for the first time had the pleasure to observe Ceres here in Brunswick. On August 29 at 11h 18m 2s it led 32 Ceti by 23.5s, based on the mean of three consistent observations, and at 11h 31m 11s was 1′ 3″ northwards. Hence I find:

	Difference from ephem.
11h 18m 2s t.m. apparent RA 15 0′ 32″	*−7′ 32″*
Apparent Dec. 9 55′ 23″ south	*+4′ 3″*

I'm very curious how your observation of the same day will agree with this one.

I observed Ceres again the day before yesterday; only the interrupted observations didn't agree well. After one observation at 11h 1m it trailed 28 Ceti by 22s and led 30 by 78s and after the second at 11h 16m trailed 28 Ceti by 19s and led 30 by 81s.

The mean Dec. of both observation is 10° 36′ 52″. But this one is still doubtful precisely because the difference in Dec. was very incompatible with the diameter of the field of view.

[last part of this letter, about the Paris prize, will be in a later volume of this series]

No. 122, Gauss to Olbers, Brunswick, January 25, 1805

My new elements of Ceres, or as Lalande says, of Vidal, follow. [For a full explanation of Vidal's planet, see a later volume of this series.]

	Epoch	*Aphelion*	*Eccentricity*
1801	*77° 17′ 0.1″*	*326° 19′ 59″*	*0.0784929*
1802	*155 27 34.2*	*326 22 0*	*0.0784871*
1803	*233 38 8.3*	*326 24 2*	*0.0784814*
1804	*312 1 33.5*	*326 26 3*	*0.0784754*
1805	*30 12 7.7*	*326 28 4*	*0.0784700*

Logarithm of the semimajor axis	*0.4420004*
Daily tropical movement	*771.0524″*
Node 1801	*80° 54′ 46″*
Inclination 1801	*10° 38′ 13″*

These elements, with which my old perturbation tables must still be linked, are based on the 3 oppositions and Piazzi's observations of 1801. Meanwhile, the latitudes spanning 4 years don't reflect the discovered secular change in the ascending node and inclination (I noted the first already last year); and, in order to achieve agreement between the observations and my perturbation tables, I must apply

A daily displacement of 0.243″ to the nodes and a daily displacement of 0.025″ to the inclination, both much larger than that given by the calculation. Without doubt this is simply due to the incompleteness of the periodic equations for the radius vector and the latitude, and will in future prove itself by detailed calculation of the perturbations.—Harding already has the ephemeris for Vidal 1805 and 1806 and is already on his chart. Bode's ephemeris in the 1805 and 1806 almanac gives the position around the time of opposition about 2° too far west, a consequence of accepting the too small mean motion from the VII Elements. [The remainder of this letter, about Pallas, will be in a later volume of this series.]

No. 128, Gauss to Olbers, Brunswick, March 25, 1805

I've now begun to occupy myself with the perturbations of Ceres but there certainly won't be any quick progress. This is, so to speak, only a trial for which I'm using a specific method. Someday I might be able to apply it to Pallas and Juno if it fulfills my requirements. There is indeed much work to it, but the way in which I've approached the problem will not neglect the 11th power of the eccentricity and inclination. Meanwhile, the by far longest part of the calculation is so routine that I'll certainly be able to use outside help with it. Perhaps in future I'll take advantage of Mr. Bessel's readiness to oblige. He has recently sent me his results concerning the comets of 1618[1] *which, along with his skill with calculus, are continually increasing my grasp of the problem.*

No. 130, Gauss to Olbers, Brunswick, May 10, 1805

I have again, after all, given up the method with which I had begun calculating the perturbations of Ceres. The overwhelming routine and dull calculations which I foresaw discouraged me; moreover, even if all calculations, which I could have delegated to others, had been undertaken by Mr. Bessel and Mr. V. Lindenau (who likewise kindly volunteered his support with this same project), still much more than my patience would have allowed would have remained for me.

In the meantime I've explored a different method, which is as promising as the former, but necessitates considerably fewer—although more mathematically elegant—calculations.

I've already seriously begun applying it to Ceres, though initially I'm limiting the scope by including only terms to the fifth power in the eccentricities of Jupiter and Ceres. This method has all the more appeal for me since with it I can fortunately make use of many previously conducted in-depth investigations into special kinds of transcendental functions. I'll subsequently try to give you an idea of them. I also hope to be able to arrange things in such a way that I might considerably lighten my workload through outside support. Indeed, this won't be necessary for the present calculation with Ceres (since I myself have already advanced beyond precisely that point where the help was needed), but surely when I repeat the same one, which will be necessary, since without doubt the expanded perturbation equations will themselves be linked with sizable modifications in the elements—or also when I some day undertake this task for Pallas and Juno, where it will be substantially more extensive.

No. 132, Gauss to Olbers, Brunswick, July 2, 1805

Unfortunately, I have not yet finished the calculation of the perturbations of Ceres. Partly because I have not worked on it continuously, partly it took more effort than I believed initially, now and then more than was necessary. But the most efficient practicing of a method can only be learned by application. I started with the perturbations of the latitude. I omitted everything depending on the eccentricities in my first calculation; Oriani considered only one member. I considered all equations, which I found over 1″, among which are some dependent on the products of the eccentricities, consequently, (since anyway in all latitude equations the inclination enters) are of the order 3. From this work I have quasi already harvested some fruit. You remember that I have already been complaining since 1803 that the latitudes in the opposition of 1803 can no longer be combined with those of 1801, found according to the motion of the ascending node found from theory, and that I was forced to move the descending node 1803 by 3′. Also the latitude of the opposition 1804 did not match that of 1802; the inclination had to be reduced far more than theory indicated.

In my latest elements, I had to give the ascending node a diurnal tropical motion of 0.241″ and the inclination a diurnal decrease of 0.0243″ in order to combine the observations, so that

[1] Three notable comets appeared in the year 1618. See Drake and O'Malley (1960).

Ascending node	*1801 Jan 1*	*80° 54′ 46″*	*Incl.*	*1802 opposition 10° 38′ 1″*
	1803 opp.	*80 58 28*		*1804 opposition 10 37 38*

To my great pleasure this is no longer necessary, and the observations agree with the latest latitude equations quite well. The per se minor, remaining differences of several seconds can very well be explained by the fact that the radii vectors employed still need improvement, because the perturbation equations have not been applied entirely. Here are the latitude perturbations

⚴ Mittl. Länge der ⚴ ω Sonnenferne
♃ „ „ des ♃ ω' „
☊ Länge des aufst. Knotens der ⚴ auf der ♃-Bahn = 78° 35′.

Knoten der ⚴ 1801	80° 53′ 56″
Tägl. Bewegung	+ 0,004 05″
Neigung 1801	10° 37′ 29,9″
Tägl. Abnahme	— 0,001 195″

Breitengleichungen:

A.

$-11{,}19'' \sin(\text{♃}-\text{☊})$ $+\ 2{,}78'' \sin(\omega-\text{☊}) \cos(\text{♃}-\omega')$	$-\ 9{,}65'' \sin(\text{♃}-74^\circ 13')$
$+14{,}17'' \sin(\text{⚴}-2\text{♃}+\text{☊})$	$+14{,}17'' \sin(\text{⚴}-2\text{♃}+78^\circ 35')$
$+27{,}47'' \sin(2\text{⚴}-3\text{♃}+\text{☊})$	$+27{,}47'' \sin(2\text{⚴}-3\text{♃}+78^\circ 35')$
$-\ 4{,}62'' \sin(3\text{⚴}-4\text{♃}+\text{☊})$	$-\ 4{,}62'' \sin(3\text{⚴}-4\text{♃}+78^\circ 35')$
$-\ 0{,}01'' \sin(4\text{⚴}-5\text{♃}+\text{☊})$	$-\ 0{,}01'' \sin(4\text{⚴}-5\text{♃}+78^\circ 35')$
$+\ 5{,}51'' \sin(2\text{⚴}-\text{♃}-\text{☊})$	$+\ 5{,}51'' \sin(2\text{⚴}-\text{♃}-78^\circ 35')$
$+\ 0{,}99'' \sin(3\text{⚴}-2\text{♃}-\text{☊})$	$+\ 0{,}99'' \sin(3\text{⚴}-2\text{♃}-78^\circ 35')$

B.

$\underline{-\ 2{,}71'' \sin(\omega-\text{☊})}$	$+\ 1{,}91''$
$\underline{+\ 3{,}44'' \sin(\text{⚴}-\text{♃}-\omega+\text{☊})}$ $\underline{-\ 3{,}33'' \sin(\text{⚴}-\text{♃}+\omega-\text{☊})}$ $\underline{+\ 2{,}25'' \sin(\text{⚴}-\text{♃}+\omega'-\text{☊})}$ $\underline{-\ 0{,}24'' \sin(\text{⚴}-\text{♃}-\omega'+\text{☊})}$	$+\ 8{,}61'' \sin(\text{⚴}-\text{♃}+95^\circ 26')$
$\underline{-\ 4{,}59'' \sin(2\text{⚴}-2\text{♃}-\omega+\text{☊})}$ $\underline{+\ 3{,}59'' \sin(2\text{⚴}-2\text{♃}+\omega-\text{☊})}$ $\underline{-\ 2{,}93'' \sin(2\text{⚴}-2\text{♃}+\omega'-\text{☊})}$	$-10{,}31'' \sin(2\text{⚴}-2\text{♃}+94^\circ 10')$
$\underline{-\ 2{,}26'' \sin(3\text{⚴}-3\text{♃}-\omega+\text{☊})}$ $\underline{+\ 3{,}90'' \sin(3\text{⚴}-3\text{♃}+\omega'-\text{☊})}$	$-\ 6{,}16'' \sin(3\text{⚴}-3\text{♃}+112^\circ 26')$
$\underline{+\ 4{,}17'' \sin(2\text{♃}-\text{☊}-\omega')}$ $\underline{-10{,}41'' \sin(2\text{♃}-\text{☊}-\omega)}$	$-\ 13{,}69'' \sin(2\text{♃}-57^\circ 21')$
$\underline{-\ 1{,}65'' \sin(3\text{♃}-\text{☊}-\omega-\omega')}$	$-\ 1{,}65'' \sin(3\text{♃}-56^\circ 9')$
$\underline{+\ 2{,}72'' \sin(3\text{♃}-\text{⚴}-\text{☊}-\omega')}$ $\underline{-\ 4{,}56'' \sin(3\text{♃}-\text{⚴}-\text{☊}-\omega)}$	$-\ 6{,}77'' \sin(3\text{♃}-\text{⚴}-61^\circ 25')$
$\underline{-\ 2{,}81'' \sin(2\text{⚴}-4\text{♃}+\text{☊}+\omega')}$ $\underline{+\ 1{,}82'' \sin(2\text{⚴}-4\text{♃}+\text{☊}+\omega)}$	$+\ 4{,}30'' \sin(2\text{⚴}-4\text{♃}+72^\circ 24')$
$+13{,}75'' \sin(3\text{⚴}-5\text{♃}+\text{☊}+\omega')$ $-10\ 56'' \sin(3\text{⚴}-5\text{♃}+\text{☊}+\omega)$	$-22{,}51'' \sin(3\text{⚴}-5\text{♃}+70^\circ 27')$

With this Oriani's calculation in Dec. 1802 of the MC is to be compared. All red underlined equations are omitted by him. The numerical value is

of the equation named A	*of the equation named B*	*of all*

		von den A bez. Gleichg.	Von den B. bez. Gleichg.	Von allen
Jan. 1.	1801	— 47,03″	— 17,14″	— 64,17″
Febr. 11.	1801	— 45,88″	— 17,56″	— 63,44″
8	1802	— 25,78″	+ 27,20″	+ 1,42″
8	1803	+ 0,46″	+ 41,53″	+ 41,99″
8	1804	+ 22,87″	— 33,60″	— 10,73″

From this it is clear that the neglecting of the equations B, rendered the sole use of A useless.

One can get a parenthetic idea of the latitude perturbations of Pallas, that all in the ratio of the greater inclination of the orbit have to become

more accurate $\frac{\text{sin incl. orbit. ⚴ ad orb. ♃}}{\text{sin incl. orbit. ⚳ ad orb. ♃}}$ *and those members, which contain* ω, *must be increased withal in the ratio of the greater eccentricity; I believe, that all equations of longitude, latitude and radius vector in the case of Pallas can rise to 500, and maybe several days are necessary to calculate one single position. By similar, but of course more numerous tables, like my older ones for the perturbations of Ceres, the work can be facilitated considerably on the other hand, but the calculation of such tables will also take months, after the formulae have been developed.*

I have not been observing for some time at all, but soon I will start to look for Pallas and Ceres in the mornings; since the latter will pass its ascending node again, I am extremely curious, to what extent the obs. will confirm my equations and the indicated place of the ascending node.

I have not heard any news about Mr. von Zach's arrival yet and I doubt, that he will give any signals from the Brocken this summer. Your opinion on Mr. von Lindenau is probably very justified. Some time ago he put the suggestion forward, "since the mutual perturbations of two planets (mainly those of longer periods) are in a similar ratio like the products of the masses and the square roots of the semi major axis or more correct like $m\sqrt{a}$: $-m'\sqrt{a'}$ *to make the calculation shorter by searching for the perturbations of the greater by the smaller (Jupiter by Ceres), where the smaller perturbing mass would allow the neglecting of a lot of members, in order to deduce from the above ratio the perturbations of the smaller by the greater."*

Chapter 13
Letters Between Olbers and Bode

Most of the letters in this chapter are between Olbers and Bode. These are supplemented with letters from Bode to Oriani, Ende and Seyffer to Olbers, and Olbers to Mayer (Fig. 13.1).

Bode to Oriani, Berlin, May 10, 1801 (approx.)

Seriously, my dear friend, I also consider Piazzi's behavior most reprehensible. The first letter he wrote me, dated January 24, was so tangled and incomprehensible that it seemed to be dictated by a charlatan or a Cagliostro [impostor] *who wanted to communicate a piece of news without enabling the reader to verify it. I do not doubt at all the truth of his observations, but they have to be either badly reduced or carelessly made for they cannot be represented by a parabola, especially in latitude, as Burckhardt remarks rightfully. By the way, the other day Piazzi sent me improvements that I am sending you here as originals newly addressed* [this refers to a letter from Piazzi to Bode dated May 1, 1801.] *But despite his improvements and those that he plans on sending me, I am determined not to make any calculation on account of this little planet until its reappearance in the autumn; for now it is not worth the effort. Then we will talk about it again. In the meantime you can see that the elements of a parabolic orbit obtained by Olbers' method vary little from those Burckhardt found and it is to assume that he will not have used precisely the three observations of January 13 and 30. This agreement between the elements is so great that it was perhaps not found calculating the orbit from three of Piazzi's latest observations. I am very grateful for the Latin verses and the map of Hera's path, and the pages of your nice journal.*

I will send the whole thing to Palermo in order to tell Mister Piazzi (for he is not Sicilian but from Valtelline,[1] *almost German so to speak) that his semi-confidentialities are made fun of and that his exclusive plans have leaked out.*

[This letter was written in French, probably by someone employed by Bode, who translated his words from German into French. The letter resides in the Bremen University archives, and is stamped archive number 9. Presumably Bode had two copies of the letter made—one went to Oriani and a copy was sent to Olbers.]

Since the origin of Piazzi himself was a matter of discussion here, it will not be amiss to print how Lalande (1802a) described Piazzi in an address he gave in Paris on April 5, 1802:

In publicising such a peculiar observation [the discovery of Ceres] *it is only natural for the public to ask who is this lucky astronomer to whom we owe it!*

[1] Valtelline is in northern Italy, bordering Switzerland.

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
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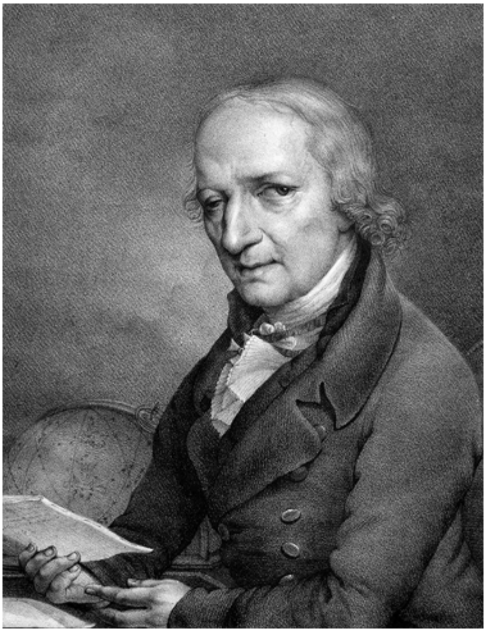


Fig. 13.1 Johann Bode engraving

Joseph Piazzi was born in Ponte in Valtellina in 1746. He took the habit of the Theatines in 1764 and was professor of mathematics in Malta in 1770 and in Palermo in 1781. He sparked in Prince Caramanico, viceroy of Sicily, the idea to use an old tower of the Kings' Palace in Palermo to establish an observatory there. In order to attract the best he saw the need to visit other great observatories to see the most experienced astronomers [History of Astronomy, *1789*]. *He went to Paris on January 28, 1787, and worked with us in a manner that made us regret his departure. In 1788 he went to England and ordered beautiful instruments there; he had already published two volumes of excellent observations. He prepared himself to measure a degree in Sicily* [geodetic work] *and I had already sent him some instruments for this work.*

Ende to Olbers, Celle, June 8, 1801

I was very surprised by the news about the new planet. At first I considered it a spoof with little accuracy. And this belief was encouraged by von Zach's utter silence in his MC regarding this important incident. But Mr. Harding has just relayed yours, Zach's and Bode's indicated elements. So there can no longer be any doubt about the matter itself but is it really proven that this new star is a planet? Or that it must exist as a planet between Mars and Jupiter? I do not deny that the existence of a planet between Mars and Jupiter has always seemed unlikely to me.

The whole idea is based on our notions of order and it is far from being known if the Creator has the same conception. At least He liked to place the two suspected satellites of Uranus which were discovered by Herschel, at a totally different position than our conjectured symmetry did.—If I consider the smallness of the new planet; how could it be that it resists the mainly attracting forces of Jupiter if not its density was unequalled and thus had a considerable mass.—The perturbations of this planet by Jupiter must be, I believe, so great that hardly any regular revolution occurs. These doubts are the reason why I am still *not sure whether the new star is a planet between Mars and Jupiter. But could it not also be a comet? Did not Lexell* [Anders Lexell, 1740–1784] *and Pingre* [Alexandre Pingre, 1711–1796; in 1784 he published a comprehensive compilation of the knowledge of comets at that time, Cometographie] *assign a similar period of revolution of 5 years and several months to the comet of 1770? This would at least prove the possibility of it. But it is not for me to determine whether the new planet is a comet and even that of 1770 since I am lacking data and observations on both. It would be peculiar if the planet of 1770 was observed as a comet and vice versa.* [The comet of 1770 was discovered by Messier, but is named comet Lexell].

Bode to Olbers, Berlin, June 9, 1801

I cannot wait any longer to announce that I consider the moving star discovered by Mr. Piazzi in Palermo on January 1 in Taurus of the 8th order of magnitude, which he considers a comet, for the planet existing between Mars and Jupiter, being heralded by me since 1772. Mr. Piazzi's letter was dated January 24, I received it on March 20, answered it immediately on the 23rd and asked for more observations than those given for January 1 and 23; I have not received an answer yet. But I received a letter of April 10, in which he informs me he had followed the comet (as he stills calls it) until Febr 11, but became ill afterwards but wanted to write me as soon as possible the orbital elements. I wanted to advise but most likely you have read already in the papers my article concerning this matter. I am still hoping that the observations I asked Mr. Piazzi for will arrive. If you assume the distance 2.9 or 3 Piazzi's known observations match my assumption very well.

on Jan 1	*RA*	*51° 47*	*Decl. north*	*16°.8*
on the 23rd Jan		*51 46*		*17.8*

On the 11th the star came to a standstill since it was previously retrograde. It is moving to its node which must be in Taurus. The inclination of its orbit might be 6°, period of revolution 4 years 9 months.

Ende to Olbers, Celle, July 11, 1801

It is truly not right that Piazzi is withholding his observations on the new planet and thus causing not only uncertainty for the astronomers but also robs them of the ability to search for it. This secrecy is intolerable. But if fraud had taken place it would be a considerable blow to astronomy in the eyes of the Saxons and they wished they had not talked about it so early.—But the exact correspondence of the calculated elements crushes any kind of suspicion. But let me tell you a funny anecdote regarding this planet. The other day I had Prof. Droysen from Greifswald [J. F. Droysen, 1770–1814, teacher of mathematics] *visiting on his journey to Paris. He came from Berlin and told me: Bode had a new planet up his sleeve—called Mira. I laughed hard at this news then: You know well our good Bode is always busy with Mira.—But now I am certain that my rapporteur, although professor of physics and astronomy, was only listening with one ear and Bode was talking most likely of Hera and the listener turned the unknown Hera into the well-known Mira.*

[Ed: The variable star Mira had a very bright maximum of magnitude 1 in November 1779, making it almost as bright as Aldebaran. As a result, Mira attracted a lot of observational interest in the late 18th century. In Bode's atlas of 1782, the discovery date of Mira—1596—is indicated.]

[Ed: After returning from Paris, Droysen paid a visit to Zach in Gotha on Oct. 19, 1801. In a book about his travels (Droysen, 1802, p. 447), he relates this about Ceres: "The new planet busies the active astronomers, and even if Mechain and Lalande still very much doubted its existence, Zach indeed considered it as more than probable."]

Bode to Olbers, Berlin, September 5, 1801

I was already looking for the new planet at Mr. Hahn's with his excellent instruments on August 22, 23, and 25 in the early hours but the region where it was supposed to be did not develop well. Now we have since eight days our typical Berlin weather, meaning changeable air, rain, very stormy and moonlight at dawn. As soon as the moon has vanished I will eagerly watch out. I did not even think of any further calculations regarding the orbit of the planet and this would have been a needless effort since this task is in such good hands like de la Place's, Olbers', Burckhardt's and v Zach's. Let me express my gratitude for your readiness and kindness to send me your results of your precise and astute studies and calculations on the orbit of the planet. I am delighted and will publish those with pleasure in the yearbook 1804. Piazzi is asking for the results of other astronomers but he himself has not even undertaken a preliminary calculation in order to falsify or rectify his statement it was a comet.

Actually it will be very difficult to find the small star in the morning sky at its present great distance, but we must try.

Bode to Olbers, Berlin, September 19, 1801

*So I am retracting what I wrote you in my last letter regarding the untimely discovery of the new planet and confided to your kindness. I have been strangely misled because I suspected at the greatest magnification of my 3foot Dollond that some stars which stood like this ** (and others which I had found the previous day with my searcher close to them) had moved; it now appeared, since the previous day, that the larger one * * had moved until it appeared above the smaller one. This happened on the 14th and 15th. And other circumstances occurred which enforced my error. The larger star had a pale reddish light and remained visible in the field of the telescope (Dollond) in bright dusk, when its neighbour, a 7th magnitude star, was long invisible. Furthermore in the morning of the 15th dusk surprised me and I did not have the time to muster through all regions. I should have waited until the next morning, which showed my error, but since it was post day, I wanted to inform you of my pleasant expectation. You kindly relayed your studies and calculations of the orbit of this heavenly body. Since then I have been searching every clear morning until the break of dawn, and also this morning. But I could not notice any change of the position of any star compared to neighbouring ones and neither found one showing a planet-like appearance. I fear that searching for the star will be difficult if it is still possible at all. Thus I am waiting for letters from you and other astronomers. Mr. Piazzi is responsible for many sleepless nights. Why did he not let his assistant observe the planet in March, April and May—we could indicate its current position with more accuracy. I am hoping, dearest friend, that you did not tell anybody else than Mr. Schroeter of my untimely announcement.*

Ende to Olbers, Celle, September 22, 1801

Probably, you did not find any trace of Hera either. We chased her in vain at Seeberg. I do not deny that I have problems with the entire matter and Piazzi's mysterious behaviour is unworthy of an astronomer, the cause for all kinds of suspicion. Now we learn that he has an assistant and he is to blame for the faulty RA's in the reductions. Why did Piazzi not supervise this assistant when he fell sick? Why did he not inform at least some astronomers about his discovery and why did he postpone it until nobody was able to follow Hera any more? If Piazzi, as an honourable man, had not acted like a charlatan, we would have been done with Hera and knew what to think. I am afraid that we might only get hold again on this peculiar star by mere chance and it is lost without it.

Bode to Olbers, Berlin, October 3, 1801

I would like to express my gratitude for your kind letter with your studies on the new planet. I made a similar sketch of the region our stranger is roaming through in order to facilitate the search and now I am able to compare yours and mine, and naturally I will inform you immediately when I am convinced I have found it. Sincere thanks for not having *publicised my premature announcement of the planet. During the last weeks I got two letters from Mr. Piazzi and I regard it as my duty to relay their contents. On August 1 he wrote: "I believe that you are right in taking it for a real planet. The latest observations during which it has lost lots of brightness misled me. I tried to show its path as a parabola, but in vain. A circular arc corresponds best and is sufficient to find it again. The covered path is still too small for a more accurate calculation of an ellipse. I calculated the following orbital elements: diameter 2.6862, node 2Z 20° 46′ 48″, motion on the orbit from Jan 1 through Febr 11 9° 2′ 39″.7, inclination 10° 51′ 12″, epoch 1801 23.8° 46′ 41″, motion in 100 years, 22° 6′ 33″.7 Sidereal Period 16 28 27 days, apparent diameter at the earth's distance from the Sun 19″. Size 1.33 diameter of the Earth. These elements are taken from a treatise which is soon to be published and which I will send you via Dr. Triesnecker. The observations were made with a telescope with a magnification of 50 times and an aperture of 3 inches. I estimate the diameter of the star to be 7″. At first I tried to observe it with a night telescope and another achromat of 4 in. aperture but it was impossible to discern it. I am embracing you with all my heart since you were the first to announce my planet that I would like to name Ceres Ferdinandea. I do not believe it possible to find it again before November." Fortunately, I was able to publish the content of this letter in my yearbook 1804 since I had the last proof sheet at hand, but I received Mr. Piazzi's second letter too late, it was dated August 25 and contained a printed table of his accurately calculated observations. The latter are the same as in Mr. von Zach's Correspondences where the decimals of the seconds differ and that one of Febr 11 is now indicated with a right ascension smaller by 15″. The obtained distance of the planet from the Sun differ the geocentric longitudes and latitudes, the first by max. 64″ from the observed, usually less. The latter max. by 28″. I will calculate its geoc. longitude and latitude for October and November and add its apparent orbit to the charts accordingly. The second letter contains only also the following: "Since I am convinced that my star is a real planet you can imagine how impatient I am to find it again, but due to a lack of adequate instruments I have to await its passage through the meridian. Pray, agree on the name Ceres Ferdinandea, which I gave her." Here we have now Piazzi's upfront plea and his own efforts to calculate the orbit of the new planet. I will send the printed table and the calculation of Piazzi's observations to our friend v Zach; the latter do not quite correspond to his regarding the position of the Sun etc. Piazzi assumed a different distance regarding the geocentric longitude and latitude.*

PS: Piazzi also wrote that in order to obtain an even greater accuracy one had to subtract 1″.5 from the first four right ascensions and of those of Febr 5 and 8″.3, nothing from the last, and to add 1″.5 to all others.

PS: I believe that the nebula class I.7 in Herschel, which he observed on January 23, 1784 in the northern wing of Virgo and was unable to find it later again, and thus called it a comet, is our new planet. The longitude and the period of revolution both match excellently with your calculation. Only the latitude cannot be brought into correspondence by 10 days, (?) 1½°, unless Herschel wanted to understand the right ascension and decl. of 49 Leo northern and not southern and this is a typo. I did not write because of this. You will find the details in the yearbook of 1804.

Bode to Olbers, Berlin, December 1, 1801

The bad and rainy weather of this October and November makes the search for Ceres almost impossible. During the last three weeks there were only two or three clear moments that I unfortunately could not successfully use. The other day Mr. von Zach sent me his elliptical orbital elements of Ceres by a certain Dr. Gauss from Brunswick. I calculated accordingly the geocentric locations for November and December and found that the planet should appear almost 8″ further east and 2° south. The matter becomes critical and if it does not show its planet-nature in a powerful telescope or achromat, I am afraid it might escape. I have received letters from aficionados, e.g. today from Schneidenburg: "On the 16th at the 6th hour Mr. Arend from Schneidenburg saw the appearance again below θ

Leonis. I am saying again since he saw her, as I did on the 1st, 6th, 8th, 10th and 14th and on the 2nd and 4th without knowing of me. He believes it was Ceres, too." Nothing more, no positions of these days, not even if a motion, which is crucial, was noticed. The position of the 16th would be correct. I believe this time the nebulae at the back of the Lion are taken for Ceres. The information is very incomplete. But Mr. Arend is said to possess a 7 foot Herschel and a reflector.

Bode to Olbers, Berlin, December 19, 1801

Now something new: Several weeks ago I had Prof. Huth from Frankfurt an der Oder visiting. When leaving he asked for a copy of my drawing of the region through which Ceres is supposed to pass in November and December which I gladly gave him and he promised to keep his eyes peeled. On December 5 he wrote that he had discovered with his 2½ f. Dollond a star in the Lion close to θ with a pale reddish light, completely round, planet-like (larger than α Leonis to the naked eye). I asked for a more information on this observation, particularly whether he had noticed a motion. On December 15 I received another note that he had observed the star again on December 3, 13, and 14 west in a rectangular triangle with θ and λ and compared to two neighbouring small stars he had noticed a backward motion of it according to the enclosed sketch. What is your opinion? Is this Piazzi's star? It is possible, but then our assumption it is the expected planet is not since that one cannot be retrograde at its elongation from the sun (Mr. Huth noticed an increased retrograde motion). I am waiting impatiently after this second letter for fair weather but the last two nights were overcast in order to convince myself from the existence and motion of this body. Nobody is expecting Ceres at that position.—I do not know yet what to make of it, is it a distant comet? It must be retrograde itself since the motion of the earth cannot make it appear so. Time will tell. After constant bad weather we have frost now and since the wee hours I have been eagerly searching for Ceres with my 3½f. Dollond everywhere, even there where Gauss' elements put her, but in vain. I am impatiently waiting for clear skies and news from Mr. Huth. I will keep you posted.

Ende to Olbers, Celle, January 9, 1802

My dearest friend, you are a true lucky devil! I have not been sleeping through the night since December 18, got up every hour to search the sky and could see nothing but a huge nebula taking up the entire firmament. And why? To search for Ceres in the even region where you found it on January 1. I <u>believed</u> to have found a trace of a phenomenon but was unable to verify it until today.—I saw a tiny star surrounded by a small nebulosity. If I assumed the RA as uniform my phenomenon <u>could have been</u> Ceres. But the nebulosity does not match. Consequently, I gave up my assumption. Thank god, you found Ceres! And although I would have been pleased to announce this to you, I am very thankful for the communicated piece of news. Ceres is going to touch the boundaries of my celestial region very soon: I will send you the catalogue of my prefecture as well as the charts to save you the effort. My region begins with 194° RA. And I will also collect the stars from 185° to 194° for you from decl. 12° to 2° and send you. Bode is incomplete for this region. I have more than 120 stars taken from the Conn. des tems in my prefecture that are missing in his catalogue.

Ende to Olbers, Celle, January 12, 1802

Please find enclosed, my admirable friend and colleague, a small chart and a catalogue of the stars in Ceres' neighbourhood. I feel the imperfection of both and they are only meant as a proof that I am not idle. The planet itself, I did not see it. It needs a clear night that I would like to use for observing Ceres and inform you about the success. Since Ceres is nearing my departement *(the French term) it might be of interest for other astronomers to get acquainted with it. I have more than 120 stars that are missing in Bode and more than 400 up to the 7th order of magnitude.*

Bode to Olbers, Berlin, January 12, 1802

With the greatest of pleasures I heard from you that you were so fortunate as to discover on January 1 Ceres and I am hastening to congratulate you from the bottom of my heart. Your information regarding the variable position of the planet on January 1, 2 and 6 decides without doubt on the correctness of your discovery. Indeed, I am very pleased that my assumption has been proven and the existence of the planet is no longer subject to doubt. Within twenty years two new main planets—what a discovery! Searching for Uranus in 1781 was not as sour for me as it is now for Ceres. Uranus was discovered within two nights but I have been searching with much effort and during many nights for the latter since August in vain due to awful weather and other obstacles. On New Year's Eve it cleared up around midnight and I searched until 1o' clock, but did not find anything special or was not as fortunate as you were to hit the position where Ceres was. I am glad she is here and will definitely find her in the next free night in the neighbourhood of ρ Virginis. I will publish this important news in the next newspaper issue, notify the Academy, and send it to Mr. Piazzi in Palermo with the next mail and do my best to publicise it everywhere. I have just read your letter to the Naturwissenschaftliche Gesellschaft*, the news is a sensation. Prof. Huth and Mr. von Linde in Schneidenburg claim to have seen it first west in a triangle with θ and λ, and then later south of θ planet-like in December, so I had to look. Those two gentlemen misguided me. As soon as I have found the planet I will notify you. It is excellent that Gauss' ellipse is so accurate.*

PS: I am humbly asking for your observations. You have also informed our friend v Zach about your discovery.

Seyffer to Olbers, Goettingen, January 18, 1802

I want to congratulate you from the bottom of my heart on your discovery of Ceres, and would also like to express my gratitude for communicating this interesting piece of news. Mr. Mayer immediately brought me your letter. You will find an article by him in our newspaper. I am offering my humble service for the future either regarding our newspaper or any other matter. As you know, I do not possess an instrument with a micrometer to observe Ceres beyond the meridian; and until <u>*today*</u> *I have not had favourable weather during the culmination. Since she appears of the 9th order of magnitude she must remain visible in my mural quadrant. If you have been able to make any observations or determinations that would* <u>*facilitate*</u> *my finding her I would like to ask for those humbly and without many words.*

Bode to Olbers, Berlin, January 19, 1802

I wanted to congratulate you once again on your discovery of Ceres and at the same time advise you that I also believe to have seen this goddess on the night of the 15th in bright moonlight at 11½ h with a 3½ f. Dollond. You can best judge whether I saw correctly: with ρ Virginis and its northern neighbour (I do not find it binary as Herschel does), but west in an almost rectangular triangle, two little stars a and b, a slightly brighter than b and I consider a for Ceres. Thirty minutes later it became hazy again and ever since the air has been so thick that neither moon nor sun shine through. I would have loved to confirm the correctness of my observation by noticing a motion. According to the elements of Gauss' ellipse, which adorably place the planet where you found it, it must have been on the 15th where I found it. According to these elements I preliminarily calculated the further geoc. motion of the planet. It will be stationary by the end of this month, then moves backwards, is around mid March at opposition, moves backwards until May and is then moving again with greatly decreasing latitude, passes β Leonis closer south than in April north and passes south-eastwards. Prof. Huth in Frankfurt an der Oder gave me the positions of his observations of his star of December 3, 13, 14, 20 and January 1 which he sketched only according to visual judgement in comparison to small neighbouring stars westward with θ and δ Leonis, soon it decreased in brightness and is most likely a very distant comet, I could not find any trace of *it. But this friend was the unblameable reason that I was thrown off Ceres' scent. Mr. v Zach observed, according to the January issue, on December 8 a small star precisely there where Gauss' ellipse places Ceres, which he was unable to find again afterwards. What do you think of this? While searching I often sketched stellar groups among which I suspected Ceres, but was never so fortunate to hit the right one or changeable weather interrupted my search.—Dr. Gauss must be a devilish calculator and thinking head since he indicated the aphelion and eccentricity with such an accuracy that was almost impossible taken the small observed arc by Piazzi. I did not trust the apparent orbit calculated according to his elements to the deserved extent since also the circular hypotheses corresponded quite well to Piazzi's observations. Our honourable Schroeter and his assistant will probably now determine Ceres with his large telescopes. Send my respect to our common friend and wish him all the best for his studies on this goddess. I informed Piazzi about your discovery already with the last mail. Yesterday I received a letter from him of December 8, in which he wrote: "Did you find my star? On November 2 I searched for it by means of azimuth and zenith distances during 24 hours, I congratulate myself on finding it. But on the third it had the same RA and decl. Since then rain, wind and clouds have prevented any further studies, but I do not give up so easily; anyway, I commence to doubt that my little star can be the sister of the famous comet of 1770."—So even Piazzi is searching in vain and has been prevented from observing by bad weather under his usually bright skies.*

Bode to Olbers, Berlin, January 30, 1802
Thank you kindly for the relayed observations on Ceres, I was especially glad for the observation of the 15th from which can be taken that I really was the first who saw Ceres on that evening, as you can see from my letter. Only on the 23rd I was able to look again. I found her east of ρ and 27 Virginis. On the 24th it was completely hazy again and only on the 25th and 26th I succeeded in finding the planet with the 3½ f. Dollond. I could even discern it with the 2 f. searcher, and I saw it also yesterday evening with both telescopes. But a recurrent inflammation of the eye paralyses my usual tasks and refrained me to observe its position more accurately yesterday and I am writing this letter only with difficulties.

On the 25th and 26th I was able to observe Ceres at the circular micrometer and let her pass together with ρ and 27 Virginis through the field of it and calculated on average as accurately as this method permits the following:

	m.t.	*App. Right Ascension*	*Decl.*
On the 25th	*10 15 56*	*188 20 25*	*11 55 18*
26th	*10 48 12*	*188 24 26*	*11 59 58*

I wanted to inform you about my observations on our new planet. I have sent your observation together with your regards to Mr. Piazzi.

I am hoping to observe Ceres at the passage instrument and mural quadrant without difficulties. I think even now Ceres resembles a 7th magnitude star. What are the English and French astronomers are going to say to Ceres' rediscovery—they still doubt her existence.

Bode to Olbers, Berlin, February 27, 1802

I observed Ceres only at the circular micrometer of the 3½ f. Dollond as good as possible. I found at 11 pm 8′ 36″ m.t. its right ascension 187° 8 18 and declination n. 15° 8 15. It stood between 253 and 254 Virginis approximately 14′ north of the latter. The calculation showed that this star was Ceres and nothing else. North-east between 253 and 254 with a pale white light, I am almost surprised that Ceres appears as a 7th magnitude star, brighter than 254. I could not distinguish it, even at the highest magnifications (200) from a fixed star. I also think that the above location corresponds to Gauss' ellipse very well.

Consequently, I must have chosen the correct star, the following night will tell—Next month I am going to start to observe Ceres at the mural quadrant, but I can easily imagine and am willing to believe that others can and will determine its position more accurately. I wrote twice to Mr. Piazzi concerning Ceres (and did not forget your discovery) but I do not have any news from him; is the discoverer still searching for it in vain? Maybe my letter is the first announcement of Ceres' rediscovery.

Please congratulate Dr. Gauss on my behalf for his fortunate calculation of the elliptical orbit of Ceres. I had the pleasure to receive a letter with the newly calculated orbital elements and Ceres' positions for March.

Thank you for your kindly relayed observations of Ceres, I will gladly publish these together with your remarks in my yearbook. I will observe the three mentioned stars at the wall quadrant and Pl as soon as weather and other circumstance permit my doing so. The same will happen to the star that you and von Zach found differing by 13′ in decl. and which the latter took for Ceres on Dec. 7. So our common friend von Zach had really found the planet already in December, why did he not notify me of his discovery, or at least on the December 31, when he convinced himself of the correctness.

If I had not been disturbed by several correspondents in my search for Ceres, who claimed to have found here and there in the Lion a moving star, who asked for answers, thus assuming I had searched, I would have had more time to follow Gauss' ellipse. Prof. Huth claims to have seen a retrograde moving star together with θ and δ Leonis, he sent me a sketch of its path for December and indicated its position but with the end of December it became invisible. —I will gladly give you my further observations of Ceres at the wall quadrant and I will carefully try to be accurate. I will take its culmination at our nice 3 f. Dollond and the altitude at the wall quadrant with neighbouring stars. Ceres is already very beautifully visible at the 2 f. searcher, I believe a good eye will soon be able to see it unaided. Our honourable Schröter observes Ceres diligently with his huge telescopes. He has sent me his notices.

Seyffer to Olbers, March 15, 1802

I am very thankful for the second part of your observations. I also was able to observe Ceres myself several times; but I would appreciate it very much if you could send me your latest observations for my personal use. I publicised your observations nowhere since I consider it not right without your explicit consent. Did Zach see the star earlier than you? Piazzi promised to notify me of his discovery but until today now word from him. How do the new observations correspond to your elements? When is the opposition? I for my part do not yet have the November issue of Zach's Correspondence which is said to contain the history of his rediscovery. I wish to receive new observations from you even if you do not have time to write more or answer my questions.

Olbers to Tobias Mayer, Bremen, January 6, 1802

The following article appeared in the GGA, dated January 23, 1802:

In a letter to our Privy Councillor Mayer of January 6, Dr. Olbers in Bremen reports to the Royal Society of Sciences that he had found Ceres Ferdinandea, discovered by Piazzi on January 1, 1801, on its first anniversary—January 1, 1802, and recognised it on January 2 by its movement and finally convinced himself on the morning of the 6th of the rediscovery of this new planet.

Seyffer to Olbers, January 18, 1802

I am congratulating you on the discovery of Ceres from the bottom of my heart and thank you honestly for sharing the news. I do not possess an instrument with a micrometer as you know to observe Ceres at the meridian; and until today I have not had clear skies during the culmination. Since it appears of the 9th order of magnitude it will be visible with my mural quadrant. If you have made any observations or determinations which facilitate the rediscovery, I would like to humbly ask without many words for those.

Ende to Olbers, Celle, March 25, 1803

I have been mulling over whether I shall tell you, my dearest friend and colleague, of the discovery I made in the morning two days ago of which I am convinced but have not yet verified to publicise it. But I have since long freed myself from any false shame to restrain myself if broadening our knowledge is possible and rather than confine it I would risk a denial. The matter is regarding Ceres which I believe to have found on the 23rd in the morning (22nd astronomical) together with Mr. Harding when he was visiting on his way to Goettingen. I suggested using the extremely clear night for searching Ceres especially since a few days ago he mustered the region of Sagittarius with the 10 f. Dollond in Lilienthal and got acquainted with the stars of that region. On the 22nd (astronomical) at 15h 30′ m.t. we noticed close to $\nu 1$ $\nu 2$ *of Sagittarius two stars which Harding believed to have never seen before. I am naming these stars a and b as they appeared in the telescope.*

Harding let these stars pass at a 3.5 f. Dollond whose field of view was very badly limited. After three passages of the Sun I determined according to your method the value of the entire field of view 43′ 6″.4.

	Harding fangt. Eintritt	Austritt		Eintr.	Austr.
ν^1	16. 28. 29	16. 33. 42	ν^1	16. 42. 54.	16. 47. 54.
a.	28. 32	33. 44.	a	42. 56.	47. 52.
ν^2	29. 30	34. 35.	ν^2	43. 16.	48. 41.
b.	30. 1.	35. 18	b.	43. 53.	49. 8.

b. was of 8th or 9th magnitude. I discerned it clearly with my Ramsden searcher and even in bright dusk when a. had already vanished. In the morning of the 23rd at 15h 15′ I believed to notice even at a brief glance through the searcher, that b. had changed its position and its declination south had increased. The stars appeared in the following constellation:

a showed itself in the 7f. Reflector as a fine binary star, b. like yesterday without nebulosity. Harding let ν2, b and d pass at the 7f. Reflector (whose field of view I am going to determine this evening). Here are his observations:

	Eintr	Austr	Eintr	Austr	Eintr	Austr
ν^2	16.21.23.	23.18.	16.28.6	30.3	16.34.15	36.3.
b.	23.27.	25.11.	30.11.	31 35	36.11.	37 58.
d.	24.52.	26.37.	30.55	33 17.	37 33.	39 28.

All these observations are not very reliable and mostly not made with a circle but badly limited field of view. But they all show the motion of the star b, which is sufficient for my purpose.

*Since Gauss' calculation points at exactly the position where we saw the moving star, it appears to me more likely that this was Ceres and not a new planet. Since I could not perceive with the reflector any trace of a nebulosity but discerned b. clearly as 8th or 9th magnitude the assumption of a comet is invalid. This morning (24th astronomical) I wanted to rectify the * but two nights awake had weakened my already affected health so that I did not dare to stay awake for another entire night and unfortunately the guard woke me too late and only at 16h m.t. when twilight had already set in. I saw the star through haziness and believed to notice a further motion but this is just an assumption and no certainty. Harding claims this was No 168 Bode. But this is certainly not true since this star is farther east. It is the same no. 757 Mayer which was also found in Paris on July 19, 1795 (Hist. Celeste p. 175) and Δ RA together with ν2 Sagittarius 6′ 34″ in time.*

I thus believe that no. 164 Bode, because he borrowed it, is not correctly determined. Tomorrow morning I will be up soon to either prove my discovery true or false. I will drop you a line tomorrow evening. I regarded it as my duty to draw your attention immediately to this phenomenon so you can examine it yourself.

On the 23rd we could only see ν2 with the 7f. Reflector; b had moved considerably. This makes my assumption even more likely especially since all other noticed stars of that region were recognised again.

Ende to Olbers, Celle, April 2, 1803

I would love to communicate, my dearest friend, good and reliable observations on Ceres but: the spirit is willing but the flesh is weak! All I can tell you is: I think to have convinced myself completely of the discovery or I must assume a very curious and inexplicable deception. Each day I observed a small star moving closer to no. 757 Mayer, but the one perceived the previous morning was missing.—That is all I can tell you and it is impossible to give you one single proper observation.—Those made by Harding have most likely already shown you that it is impossible to be successful without a proper micrometer.

Chapter 14
Zach's Ceres Correspondence

The bulk of these letters were written by Baron Franz von Zach. The recipients were Lalande and Mechain in Paris, Gauss in Goettingen, David in Prague and Oriani in Milan. Also included are letters from Olbers to Zach, Lalande to Oriani, and Buerg to David (Fig. 14.1).

Zach to Lalande, Seeberg, May 24, 1801
You probably know better than I do, that Piazzi's planet is a planet between Mars and Jupiter. Mr. Oriani has sent me elements that seem faulty to me, but I have calculated from these three meagre bits of data that he sent:

January 1	*RA 51° 47′*	*6° 8*
10 –	*Stationary*	
23 –	*RA 51. 46*	*17.8*
Annual mean motion	*= 2z 6° 54′ 25″*	

Epoch of long. 1801	*= 2 6 55 40*
Long of ☊	*= 1 25 43 44*
Orb. inclin.	*6° 14′ 0″*

Dist.	*⊙ in a circle 3.071*
Revol. Sider.	*5.3817 years*
Revol. Synod	*5.3806 years*
His elongation while stationary was 7^{z} 26° 41′ 41″	

I do not understand how Oriani could find the long.	*☊ 9^{z} 8° 32′*
Orb. inclin.	*3° 50′*

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2_14

Fig. 14.1 A 1794 engraving of Franz Xaver von Zach

	Geoc. long.		Geoc. lat.	
I find on Jan 1 hel. long.	*2^{z} 7^{o} 9′ hel. lat.*	*1z 58 ¾*	*2^{o} 23′ 30*	*2° 37′*
10	*2 8 49*	*1 24 ½*	*1 23 15 ½*	*1 47 1/6*
23	*2 11 12*	*1 29 ¼*	*1 22 49*	*1 39*

It is extraordinary that Lexell and Pingre have found for the comet of 1770, dist. 3.08, annual revolution 5.4 years. Precisely like our alleged planet. Would this not be the return of that comet? From 1770 to 1801 = 30 years = 5.6 or 6.5 thus, it would be its 6th or 5th return. It is due to its smallness that we did not see it at all. In 1770 the circumstances were extraordinary. Schröter observed an augmentation in the comet of 1799 and a diminution of light that bear no relation to the distances. It is true that the ☊ and the inclination of my elements do not match those of 1770 but Lexell has already suspected that the orbit of this comet could have been completely changed by Jupiter's action. This body can be something even more extraordinary than a planet!! and torment geometers and astronomers.

If this new star is a planet between Mars and Jupiter, it is necessary that this law:

Mercury	*4*
Venus	*4 + 3 = 7*
Earth	*4 + 2 + 3 = 10*
Mars	*4 + 2 + 2 + 3 = 16*
Juno	*4 + 2 + 2 + 2 + 3 = 28*
Jupiter	*4 + 2 + 2 + 2 + 2 + 3 = 52*
Saturn	*4 + 2 + 2 + 2 + 2 + 2 + 3 = 106*
Uranus	*4 + 2 + 2 + 2 + 2 + 2 + 2 + 3 = 196*

where in general $a + (2^{n-2} . b)$ sticks to the universal gravitation and Kepler's laws, and it is necessary for C. Senator La Place to explain this.

Lambert was the first to speak of a planet between Mars and Jupiter. See lettres cosmolog. by Dorquier and Utenhove p. 51. [see Lambert, 1761, and a revised edition, 1801]

Oriani did not send me the times of Piazzi's observations, assuming those are meridian observations, here is how I reduced them.

1 Jan 1801 8 h 43′ 14.″6 m. t. of Palermo	*23 Jan 7 h 16 40.″61 m. t. Palermo*
RA = 51° 47′ Decl. 16° 8′	*North RA = 51° 46′ Decl. 17° 8′ North*
Long. 23° 29′ 40″ Lat. 2° 37′ 5″ South	*Long. 1ᶻ 29° 43′ 40″ Lat. 1° 38′ 50″*
Long. ⊙ + 20″ = 9ᶻ 11° 1′ 40″	*Long ⊙ + 20″ = 10ᶻ 3° 22′ 28″*
Log R = 9.9926158	*Log R = 9.9932351*

Zach to Oriani, Seeberg, May 29, 1801

Thousand thanks, my dear and estimable friend, for the important news you kindly shared. Three days before receiving your letter (dated April 7, 1801) I got one from Mr. Bode (dated April 14, 1801) who told me first that Piazzi's comet was the planet between Mars and Jupiter. He told me that Mr. Piazzi wrote him from Palermo <u>on January 24</u> (please note the date of the letter) that he had discovered <u>a comet</u> and sent him the same two positions of Jan 1 and 23 such as you have sent them to me, with the same note that on January 11 <u>the comet</u> was stationary. <u>Bode</u> adds that these observations immediately struck him as very special and he made a small calculation and recognised on the spot that the comet was a planet and that it was the one he had long since suspected between Mars and Jupiter. You, my dear friend, who has calculated several orbits of planets, and are experienced, tell me, I am begging you, how can someone immediately tell from two positions they are planetary? I shook my head reading Bode's letter and said to myself this is fishy. Sixteen years ago I printed Lambert's cosmological speculations, as you can see in the ephem. of Berlin 1789, p. 162. I looked in my papers and found for the distance 2.82 and for the sidereal revolution 4 ys 9 ms and I further saw that Piazzi's observations on Jan 1 and 23 as well the stationary period of Jan 10 and 11 match rather well. When I was in Berlin in October 1785, I lodged at Bode's place in a sealed envelope, my elements by dreamed analogies, and he told me he still had them. [For more about Zach's dream see Chap. 1.]

Three days after Bode's letter, I had the pleasure of receiving yours. You, my respectable friend, simply told me that <u>Piazzi himself suspected several times that the comet could just as well</u> be a planet. And Piazzi even had a good reason to suspect it since its motion is very slow and he always saw the star without any nebulosity. Thus Piazzi had good reason to suspect a planet. You, my good friend, immediately calculated the orbital elements in a circular orbit and thus saw that the planet had its place between Mars and Jupiter. You can easily calculate all this since Piazzi had already told you that he considered his comet a planet. But how could Bode immediately tell from a handful of numbers, even after a calculation, it was a planet? Credet Judaeus Appella, I do not believe it! [A common Latin saying meaning "Let the Jew Apella believe it; not I". The phrase means, roughly, tell it to someone else, not me.] *It is not Bode's way to throw himself at cometary observations and to calculate orbits. What made him do so this time? The same reason you made yourself immediately calculate, it was Piazzi who told him, as he told you, that he suspected that the star was a planet but Mr. Bode concealed this part of Piazzi's letter to be the first to announce the discovery of a planet, for your letter from Piazzi, and that to Bode bear the same date and are both written on January 24. Is it likely he mentioned his suspicion of a planet to you and not to Bode when all other details of the discovery are identical? No, no, it is not likely and already Bode's hurry to publicize this news and to adopt the first idea of a planet between Mars and Jupiter show that he is guilty. But anyway, in my journal I do* *justice to Piazzi who was the first to notice that this planet was placed between Mars and Jupiter at three times the semi-major distance of the earth's orbit. Bode did not know that I had news of the new planet as soon as he and of an equally good source. I responded that actually I agreed with him that Piazzi's star might very well be the hidden planet between Mars and Jupiter and that you had not only found out the same thing but that already Piazzi had found out that this star could not be a comet. Despite this he published in several newspapers of Berlin, Hamburg and others that Piazzi had announced to him a comet but that he immediately had suspected it was the new planet between Mars and Jupiter. In one of the papers it is said that he instantly responded to Piazzi and asked for further observations to confirm the planet. In others (probably after having received my letter) he says that you, Piazzi, and I as well, agree with his opinion that the star of Piazzi is a planet. But in my journal I assigned to everyone the honour of their discovery and I placed everything in order. How do you like this small deception? My dear friend, I calculated according to your elements the planet but it was already too late and the star was already lost in the rays of the sun and the mists of the horizon. I searched for it in vain on several clear evenings. Bode told me that he was just as unlucky. Thus we have to watch out carefully for it when it reappears around August and September. I hope that Mr. Piazzi will be more communicative and will send all his observations and that you have till then the best elements to be able to find the star more certainly when it is far enough away from the sun to be observed with a good parallactic telescope. I am begging you to send me more precise elements when you have them for it will be difficult to find such a small star in the dusk if its position is so badly determined and we will have to wait too long to observe it at the meridian, I would like to get hold of it as soon as possible. I have calculated a circular orbit myself from the observations you sent me from the standstill on the 10th till Jan. 23. But I believe that in the great hurry you are at fault regarding the place of the Ω and the inclination, you find the first 3^z $8° 32'$ and I found 1^z $25° 43' 44''$. And the second $3° 50'$ and 1 $6° 14'$. I find the annual motion $6° 54' 45''$, sidereal revolution 5.3817 years, epoch of the mean long. (aphelion) 1801 1^z $6° 55' 40''$. You told me that you found the planet's elongation at the time of its standstill to be 7^z $16° 58'$, and I find 7^z $26° 41' 45''$. But otherwise I reduced like you Piazzi's observations assuming that those are meridian observations. Here is what I found*: [Zach here prints Piazzi's first five observations of Ceres]

But most likely you know all this much better than I do at the moment, and I would be endlessly grateful if you could send me Piazzi's other observations for you can easily imagine that this first news created utter impatience. A very singular and peculiar thing is that the famous comet of 1770, which so tormented the geometers and astronomers, and which Lexell could only reduce to an ellipse of 5.5 ys and to a distance from the sun = 3.08, what Burckhardt confirmed with his praised work on this comet, could very well have been the star of Piazzi or vice versa the star of Piazzi the comet of 1770. I will not bore you with hypotheses on this matter. But why can a planet not have a tail as Saturn and Uranus have rings? Do the elements of the comet of 1770 not resemble those of Piazzi's star? I answer this with Lexell's words that Jupiter's influence has totally changed the orbit of this comet. Why was it not seen any earlier and more often? Schröter and Herschel have proven that comets can be visible in a certain period and not in another. They noticed changes in satellites on the extension and intensity of the light. For cometary bodies it is possible they are not always in a state of phosphorescence and thus visible. We know celestial bodies which have periods of light of different duration, it is for instance suspected that the star of Tycho in Cassiopeia has a period of 300 years. The comet of Lexell can always have existed in its ellipse of 5.5 years without being visible or better has reappeared in the form of a star only visible with a telescope as Piazzi, I suppose, has just found it. If Piazzi's star is the planet in question and at its perihelion it is possible that it disappears completely at aphelion, one more difficulty for the discovery. Maybe Piazzi's star has increased in brightness since January 24. The nebulosity or a tail has appeared, during the appearance of the comet of 1770, Messier could for a long time not see a tail, it appeared at the end, V. Mem, de Paris.

1776 p. 597. Anyway, it is peculiar that these two extraordinary stars of 1770 and 1801 meet in dist. and revolutions. An interval of 30 years, the period being 5.5 years, would result in the 5th or 6th return, or rather visibility since; the rest of the time the star was invisible. All these are only dreams and tales. But the great gap between Mars and Jupiter that Lambert observed first in 1761 in his cosmological letters, would then also be a dream. I do not think so. On the contrary, the planet Uranus and that of Piazzi if Fabula vera est *[if the tale is true], seem to confirm in my opinion, at least as approximation, that a fairly regular progression exists which the planets observe in their respective distances from the sun. Although not being supported by any theory, it is up to you other great geometers to tie this law, like those of Kepler regarding the universal gravitation and to show us that it must exist.* [Zach here prints the table of numbers showing the progression from Mercury to Uranus.]

Our famous German philosopher Kant has conjectured that the proximity of Jupiter has absorbed in his sphere all the matter of which the planet in question must have been formed, that is to which he attributes the enormous and disproportionate mass of Jupiter, the smallness of Mars and the lack of satellites. Please note that it is a philosopher who is speaking. I need to tell you another joke of Bode about the new planet. Since he wanted to steal the glory from Piazzi being the first one to recognise the planet and to appropriate it, he deserves a little teasing. He wrote me confidentially that he had already thought about a name for the new planet and that it should be Junon. But since I have been talking about this planet for 16 years now and been hoping to find it working on my zodiacal catalogue, the Duke has already jokingly baptised this new hidden planet Hera or γρα, which means Junon in Greek. Thus I did not mention anything of Bode's nice idea in my journal since he told me the secret, I only said that 16 years ago the Duke of Saxe-Gotha gave this planet between Mars and Jupiter the name Hera and that it absolutely and necessarily must be Hera and not Juno. Here is the demonstration: 1. the new planet cannot be called Juno since this name is already consecrated to Venus. Pliny Hist. Nat. *Lib. II chap. VI said: Below the Sun walks the great star some call Venus…others call it, however, Juno. L. Apuleius said at the beginning of de Mundo: Juno, which esteems to be the star of Venus, is ranked as the fifth. St. Augustine* De Civitate Dei *Lib. VIII c. 15 calls Venus Stellam Junonis. Hence it is against the rules to give this name to the new planet. 2. It must be Hera because Hera is the mother of Vulkan who resides in Sicily. This city of Hera is named Hybla Minor of which Cicero talks* ad Atticum *II.2. and in Pausanias in Elis Lib. VI c. 6; it is also treated in the Itinerario Antonini; this will conserve, perpetuate and bequeath at the same time the discovery made by a Sicilian astronomer in Sicily to posterity. 3. It must be the Greek name Hera and not the Latin Juno, because Herschel's planet also has a Greek name—Uranus, it should be Coelus in Latin, but it is very good, all the ancient planets will have Latin names, the modern Greek ones, this distinguishes them at a glance, so if a new planet beyond Uranus will be discovered, it needs a Greek name. And here is my poor Baudet (as La Lande called him writing to Gotha) fleeced of the honour to be the parent of the new planet, as well of the honour to have recognised the planet and to have said it was the one between Mars and Jupiter for it belongs to two fine Italians and not to a heavy German like Baudet.* [in French, baudet means donkey] *In Germany we founded a society of 24 astronomers, our patron is the Duke, our president Mr. Schröter at Lilienthal, I am the secretary, our goal is to search for this planet, we have divided the zodiac into zones of 15° of long. and of 8° latit. north and south. Each member inspects his department, at the moment we are perfecting the catalogues. Mr. Piazzi has nicked the planet but we would have found it this way. You and Piazzi are on the list of this astronomical society which was born in September 1800. I am fulfilling my duty to ask you to accept, La Lande, Messier, Méchain are members, when writing to Piazzi, invite him on behalf of the society, which by declaration charged me with this, I will tell you another time about the statutes etc. of this astronomical society which is scattered all over Europe.*

Zach to Lalande, Seeberg, June 4, 1801

Friend, that I will always love, esteem, respect, despite your little pinpricks as they are never backstabs. I hurry to rectify two errors in my last letter I have calculated.

$$\frac{\underline{X + \textit{Dist. Hel. long.}}}{2} = \textit{sin}\ \frac{\overset{\textit{1. obs}\quad\textit{2 obs.}}{\underline{(\textit{Lat. hel} + \textit{Lat. hel})}}}{\textit{sin. Lat hel} - \textit{Lat. hel.}}\ .\ \textit{tang}\ \frac{\underline{\textit{Dist. long. hel.}}}{2}$$

From this I have taken X = 14° 51′ 50″
Hel. long. 1 Jan 1801 2ᶻ 7 9 0

Long ☊ = 1 22 0 50	*I subtracted x, but it should have been added*
so ☊ = 2ᶻ 22° 0′ 50″	*this must be corrected in the elements of the new planet that I have sent you*
The second error is in the inclination of the orbit	
Log sin. lat hel. 1° 59′ 38″ = 8.5414907	
— sin X 14 31 50 = 9.4091275	

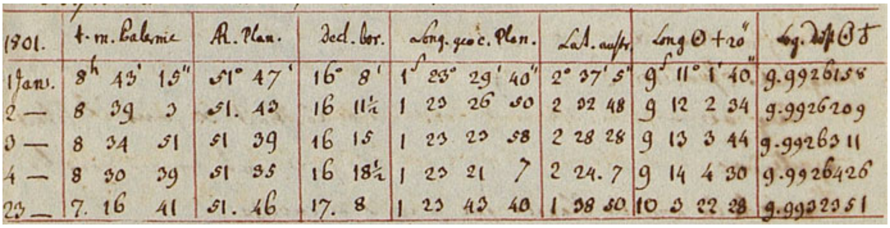
Lg. sin 9.1323632
Orbit. inclinat. 7° 47′ 40″ and not as I have given you 6° 14′

You feel that these elements can only be very rough and I do not even hope to find the planet by them when it will leave the rays of the sun, but before the month of August, we will know something more certain. I am surprised that you did not tell me anything about it. Piazzi, who notified you of the comet, did he not write you about the planet, he notified Bode and Oriani as soon as January 24. Would not at least the latter have notified you? In the meantime, I have calculated the celestial region where we need to search for the new planet, all August it will cross the constellation Cancer. On August 1, we need to search the sky between ω, μ of Cancer and κ Geminorum. (On 27 July [illegible]) the planet will be on the parallel of a star, that you have in your great catalogue (Conn. de t. for the year VII p. 288) at RA 117° 6′ 50″ Decl. 24° 8′ 57″ north. The star will follow the planet which is only at RA 116°. 82 Flamsteed of Gemini will precede the planet; I have very well determined the position of 82 ♊ for 1800 RA = 114° 8′ 42″.32; you certainly have the declination. On August 31 the planet will be close to γ ♋ or Asellus Borealis.

The planet will not be advanced by more than half a degree than the star. In any case, this could be enough for the comet snoopers and the Messiers and the Méchains; will easily find it, but I think that Piazzi's observations of Jan and Feb March April will give us a better orbit than mine, calculated based on such petty and poor observations. Here is how I made good use of the notes in Oriani's letter, where it is only more or less, while waiting on better ones, I have concluded the following observations of Piazzi (Fig. 14.2).

1801 m.t. Palermo RA plan. Decl. north Geoc. long. Plan. Lat. South Long☉ + 20″ Log. Dist. ☉♁

You probably know all this better than I do. Mr. Bode has not known it for long, I hope this very minute to learn something for certain. Bode, in the meantime, has baptized the new planet Junon. But this is not right, because Venus already carries this name. See Pliny in book II ch. VI of his Hist. natur. *who says there Below the Sun walks the great star some call*



1801.	t. m. Palermo	AR. Plan.	Decl. bor.	Long. geoc. Plan.	Lat. austr.	Long ☉ + 20"	Log. dist ☉ ♁
1 Jan.	8h 43' 15"	51° 47'	16° 8'	1s 23° 29' 40"	2° 37' 5"	9s 11° 1' 40"	9.9926158
2 —	8 39 3	51. 43	16 11½	1 23 26 50	2 32 48	9 12 2 34	9.9926209
3 —	8 34 51	51 39	16 15	1 23 23 58	2 28 28	9 13 3 44	9.9926311
4 —	8 30 39	51 35	16 18½	1 23 21 7	2 24. 7	9 14 4 30	9.9926426
23 —	7. 16 41	51. 46	17. 8	1 23 43 40	1 38 50	10 3 22 28	9.9932351

Fig. 14.2 Piazzi's data from Jan. 1801

Venus…others call it, however, Juno. L. Apuleius in On the Universe p. 252 (Bipont Edition) says right at the beginning Juno esteems to be the star of Venus. St. Augustin in his work City of God Lib. VII Ch. XV calls the planet Venus Stellam Junonis, and so do other authors as well. Therefore, the name Junon is not suitable for the new planet. I have been working on this chimera for 16 years now, as one can see from the Ephem. de Berlin 1789, p. 163 printed in 1785. Consequently, the Duke of Gotha had already given a name to that planet, he called it Hera, the Greek name of Juno. This name seems to be more suitable. 1) Because it is Greek, like the name of Uranus, also a new planet discovered in our times, so, the new planets are distinguishable from the old planets by Greek and Latin denominations. If one had wanted to give Herschel's new planet a Latin name, that would have been Coelus, but in naming the new planet Hera all the new planets would have Greek names, and if we discover one beyond Uranus, it would consequently have to be a Greek name. The name Herschel that you, my renowned Friend, gave this planet, is actually not suitable. I have well noticed how angry you were with poor Wurm to have called it Uranus in his Historia novi Planetae [1791], *and in the tables of DeLambre that you kindly sent him in sheets. But he is to be excused indeed because all of Europe calls this planet Uranus, except for you and the English. The Germans, Italians, Suedes, Danes, Russians, Spaniards, Portuguese &…call it Uranus. Senateur La Place, in the immortal works by not calling it anything else but Uranus. All the Ephem. astronom. except for Berlin, Vienna, Milan, Bologna, and Amsterdam call it Uranus, only in the Conn. de t. you can find Herschel, this name can not be found in any treatise on astronomy, Bode, Melanderhielm, Schubert &…call it Uranus. Even in Darquier's translation of Lambert's Lettres cosmolog., Utenhove always uses Uranus, p. 51, 58 &…. ullula cum Lupis [who keeps company with wolves]. 2) The name Hera is also suitable, because Hera is also the name of a town in Sicily, of which Pausanias speaks in Eliacis Book VI ch. 17. Cicero in the Letter to Atticus Book II Lt. 1 it is mentioned in the itinerary of Antonin. This town was afterwards called Hybla minor. This carries on, conserves, transmits to posterity at the same time the place of the discovery and of the Sicilian astronomer who found this planet. Furthermore: Hera is the mother of Vulcan, who has his seat and place on Mount Etna on Sicily. In mythology, allegory goes even further by placing Hera between Mars and Jupiter, the latter his father and the ancestors, above him and his spouse and the children below him. This article might anger you because I know how much you care about the name Herschel; but you cannot swim against the current. The English do not even call the planet Herschel but Georgian Planet. How very odd! So, the new planet must be called Piazzi and he himself calls the planet Ferdinandea!*

The elements I have calculated 16 years ago according to the law of analogy or chimera, if you want, were filed 16 years ago in a stamped envelope at Bode's place in Berlin. The distance given there is 2.889385. The eccentricity 0.14089, the greatest after Mercury, but these are nothing but dreams, like those of Kepler, Lambert, Kant, we will soon see, if I correctly gave the distance, it already seems to be the case. This unique law of distances $a + (2^{n-2} \cdot b)$ *becomes quite remarkable, I hope that C. La Place will show this to us.*

Zach to Oriani, Seeberg, July 6, 1801

I am responding immediately to your very amiable letter which you wrote to me on June 17th, and which I received on July 5th. I am ever grateful to you for Piazzi's observations which you were kind enough to send to me. But I had to laugh more than once about your Jesuitical letter and your mental reservations. [Zach seems to be using "Jesuitical" in apposition to mental reservation....so he's using it as an adjective with the meaning it has in all Western languages: devious, not quite telling it "like it is."] *You laugh at us poor heavy Germans because we are squabbling over the new planet Hera. This reminds you, perhaps, of the story of the two spouses who were arguing over what country they would have had their embryo adopt, if it had been born alive, and all the while it (the embryo) was in a glass filled with wine spirits. You do well in not telling us your feeling, of what you think now about the new planet. You were the first, however, to announce it to us as such. You want to fill me with new strength for my attacks, and you push complacency to the point of sending me Piazzi's original letter, but my weapons have not become rusty for that. Piazzi asks your opinion; he—of the planet, but you still keep an Ahlam Silentum Jesuiticum. I cannot, in consequence, direct my anger against Bode; I will turn it, therefore, against Piazzi. It is truly unpardonable that this Sicilian has made a secret of his discovery for such a long time. If he had notified the astronomers in time, we would know what to hold on to, for one would have been able to observe his planet, his star, his comet, his chimera during the entire months of March and April, and we would know at present what it is. All the astronomers found his conduct and secrecy very reprehensible. The astronomers are very angry and I have just received a letter from Senator Laplace who thinks the same. Such puerility to have wanted again to keep a secret of his observations until he had calculated an orbit! While waiting, the pot of roses is discovered and my friend Burckhardt calculated, based on the ensemble of Piazzi's observations, an elliptical orbit of the so-called planet and sent it to me for my journal. Of course, I hastened to publish it, not only so that I could, but I made a plot engraved on a small map on which the future motion of the planet can be found until the month of September.*

Since Piazzi's irrational jealousy has deprived us of information which would have allowed us to reach about the nature of this star, all that remains to be done, therefore, is to go on an exact hunt and to put all astronomers and amateurs together in a campaign to catch the pseudo-planet on its return from the sun. This planet has the face of a comet, like toads resemble frogs. If it was only up to Piazzi, never will we find the poor Hera, for since he does not want his observations to be published, nor the calculations that would have been done, and he, still weak and ill. We will never know, therefore, where to look in order to find the path of such a faint star. There are some astronomers who are beginning to doubt the very existence of this star. Burckhardt suspects that his observations are very faulty; actually, he [Piazzi] *gave you and Bode a false declination by a half-degree. Burckhardt says there are many others. Now I cannot conceive how an experienced observer such as Piazzi, armed with the best instruments—an entire wall quadrant and a Ramsden meridian telescope—could commit such similar mistakes in the meridian observations? I say ahead of time that I will lose time searching for Hera. Truth be told, I am already disgusted with this.*

I have, however, prepared an equatorial Ramsden, and good parallactic telescope from Dollond with which Mr. Buerg (who is presently with me for half a year) and I will hunt for this little star, but I fear that we will find nothing. [Johann Buerg 1766–1834]. *Hera may never be tracked down, and if one never finds this comet or planet anymore, there will be much reproach which will rain down on Signor Piazzi, for having been mysterious. Even injurious conjectures will follow and each will allow himself to say what he wants. One will rank this discovery with Cassini's planet, discovered in 1787, or with the satellite of Venus, with the comet of Koenig in Mannheim, etc. Whatever the case may be, I will tell you here, while waiting, the clock, the wall quadrant and Burckhardt's ellipse with my map, therefore, go and look for Hera, you three astronomers in Brera, you cannot miss it.*

Zach to Lalande, Seeberg, July 13, 1801

We start being suspicious of Piazzi's comet. Why is he acting so mysteriously? Why does he sometimes call it a planet and sometimes a comet? Friend Burckhardt suspects some errors in his observations. You did not want to share them with me, but I have them all the same and maybe more than you have received, I have sent them on to Burckhardt. Piazzi is still earning serious reproaches for having hidden his star that attracts so much attention and sparks the curiosity of all astronomers. Did he puzzle us by chance? Why did he send faulty positions to Oriani and Bode? The observations I forwarded to Bdt [Burckhardt] *do not seem to be free of copying or observation errors either.*

Zach to Lalande, Seeberg, Aug. 1, 1801

Also Mr. Fuss believes in the planetism of Piazzi's star as well as Prosperin [Eric Prosperin, 1739–1803, astronomer to the King of Sweden], *who wrote me that his calculations give him, if not a proof then at least a strong suspicion that the orbit of Piazzi's star is an ellipse. Fuss thinks that this star could very well be the comet of 1770. He assures me that his father-in-law, the famous Euler, has always insisted that the orbit of this comet must have been completely changed by Jupiter's action. This is also the opinion of Senateur La Place. We must hope that time will shed light on all this, in the meantime we are chasing this fugitive. It is unforgivable, and this is a guilt that Piazzi will never be able to whitewash himself of, to have been so secretive about his observations and to not have informed other astronomers in time, without this petty-mindedness, we would maybe know more about this star, but due to how he treated this discovery, we risk to not be able to find it again, at least it will be much more difficult. By the way, the glory to have met this star is pretty small and we are forever grateful for an error in immersion. Piazzi, busy drawing up his ✷ catalogue, was looking for star 87 of Tob. Mayer in the catalogue of Wollaston, but he did not find that star in Mayer, actually this star is not in Tob. Mayer, it is in de la Caille (Eph. des Mouv. céleste 1765–1775 p. 1 XVII) it is also on Bode's chart, but I could not find it in your catalogue, and it was while searching for this star that Piazzi stumbled over this said planet.*

Olbers to Zach, Bremen, Aug. 1801

I continue, my dear friend, to send you some results of my studies on the new planet made during my apprenticeship at Rehburg. Actually they follow from much easier calculations than the previous and consequently do not deserve your attention but they can strengthen our belief in the planetary structure and our hope to find it again next September. Since I had convinced myself that Piazzi's observations could not be associated with any parabola, it was now important to see how much or how little they varied from a circle hypothesis. Provided that Piazzi's star had described a circle, I calculated from the observations of January 1 and February 11 the following elements:

Long of node	*$2^z\ 20^o\ 28'\ 20''$*
Inclination of the orbit	*11 0 48*
Heliocentric elongation of the long in the orbit of the 1st observation	*$11^o\ 49'\ 9''.4$*
Radius of circle	*2.724415*
Sidereal period	*1642.5 days*
Diurnal heliocentric motion	*$13'\ 9''.408$*
From these circular elements for January 19 was calculated	

Calculated longitude	*$1^z\ 23°\ 28'\ 49''$*	*Calculated latitude*	*$1°\ 53'\ 9''$*
Observed longitude	*$1^z\ 23°\ 29'\ 24''$*	*Observed latitude*	*$1°\ 53'\ 35''$*
Difference	*$-0\ 35''$*	*$-0\ 26''$*	

This correspondence is very extraordinary and certainly as great as the one found initially for Uranus with the circle hypothesis and the observations are maybe not more accurate than shown by the circle hypothesis. Thus I believe to draw the conclusion that 1) Piazzi's star really moves on an ellipse only slightly different from a circle and thus is really a planet. 2) Even in this, it's not very eccentric orbit, it can not have been very far from the line of apsides during the observations, i.e. either close to the perihelion or aphelion for otherwise the observations could not have been in such accordance with the circle hypothesis. 3) It seems hardly possible to derive anything certain from observations which differ so little from the circle hypothesis about the dimension and position of the real ellipse. At least if also, as Dr. Burckhardt had found according to his elliptical orbit, the heliocentric velocity had slightly increased during the observations and the distance from the Sun had slightly decreased, it will be impossible recognise whether Piazzi's star went shortly before January 1 through its aphelion or after February 11 through its perihelion. In both cases the heliocentric motion increases and the distance decreases and the difference between both cases is certainly too small to be distinguished by an arc of 8° 57′ by Piazzi's observations. And I am saying it a second time—it is not an advantage for the determination of elliptical elements that the observations include the planet's standstill so that the apparent geocentric motion is so exact and thus very small errors in the mean observations have a great influence on the elliptical elements to be derived. I admit it, Burckhardt has found an ellipse which corresponds very well to the observations in which the aphelion occurs on January 1. But I am convinced a sufficient ellipse with the perihelion on February 11 could just as easily be found. 4) This uncertainty whether Piazzi observed his star close to its aphelion or perihelion has an influence on the positions which are given in advance in order to find the star again. If the new planet had passed its aphelion before January 1 its heliocentric velocity increases and also its geocentric longitudes must be greater in August and September than according to the circle hypothesis. But if it passed through its perihelion in February its heliocentric velocity has decreased and its geocentric longitudes must be smaller in August and September than according to the circle hypothesis but in the latter case they will differ less from it than in the first. Since we cannot know which case will occur it is safer to base a future discovery on the positions derived from the circle hypothesis which cannot be very different from the true ones and which are an average between both possible cases. Consequently, I calculated the following positions of the planet for Palermo noon.

Aug 13	Longitude 4^{z} 3° 21′ 1/2	Latitude 4° 55′ north
19	4 5 50	5 6
25	4 8 18	5 17
Sep 1	4 11 10	5 13
7	4 13 36	5 44

You see, my dear friend, that these positions deviate from Burckhardt's in longitude not even by 2° and in latitude only by a few minutes. I believe we will find the new planet the easiest if we start from these points determined by the circle hypothesis and search a few degrees forward and a few degrees backward from those under the given latitude and examine all small stars on this parallel.

But this rediscovery will certainly take place before the beginning of September. After August 19 the new planet, according to the circle elements, rises exactly with the beginning of the astronomical twilight (2 h 23′) at Bremen. It must be very high in complete darkness if we want to see it for it will be very tiny in September since it resembled on January 1 an 8th magnitude star. On January 1 its distance from the earth was only 1.968, it will be on August 19 = 3.645 and on September 7 = 3.536. Before August 23 most likely the twilight and until September 6 the moonlight will make it impossible to see this extremely small star. On the small chart which you kindly gave me, there is close to the tongue of the Lion a 6th *magnitude star. Is this star really in the sky? What a shame that most likely due to a lack of time you did not draw this chart in a larger scale and map all known stars in order to make it even more useful for finding Piazzi's star again. If I am awake around 2 or 3 I will look for it and gladly leave the first rediscovery for others. Your friendship vouches for immediate information if this—for astronomy—glorious rediscovery definitely confirms the existence of the new planet. I received his* [Piazzi's] *newly improved observations with great pleasure. Fortunately, the observations on which I based my calculations remain unchanged. I like the name Ceres since it reminds of Sicily. Piazzi has certainly earned the right to name the new planet. But the affix Ferdinandea will meet with as little luck as Herschel's George's planet. With amicable honesty I have to admit that I do not consider your assumption which you still have not given up, that Piazzi's star might be identical with the comet of 1770, likely at all. Certainly the comet of 1770, surrounded with an enormous atmosphere, could appear as an 8th magnitude star without any nebulosity. But the altitude of the comet of 1770 may be changed by Jupiter and consequently cannot have declinations that correspond to those derived from Piazzi's observations.*

Zach to Oriani, Seeberg, September 7, 1801

With one foot in the stirrup in order to leave for Goettingen I received two letters that might be of interest to you. One is from Mr. Schubert in St. Petersburg about the perturbations of Mars and the other from Dr. Olbers in Bremen about the new planet. You will see what Schubert sent regarding the equations of Mars, please examine it. Olbers calculated new elements according to the new observations that you sent me.

Radius of the circ. orbit	*2.730185*
Long. ☊	*2Z 20° 23′ 45″*
Inclin.	*11 3 36*
Revol.	*1647.75 days*
Diurn. helioc. motion	*13′ 6.″528*

These elements are calculated from the observations of Jan 1, 19, Feb 11. The intermediate observations correspond up to 1 min in long. and to 1/2 min in latit. Consequently, Olbers believes that we must search the new planet on

Sept 1	*Long 4Z 11° 10′*	*Lat. 5° 31′*
7	*4Z 13 36*	*5 44*

Zach to Duke Ernst II, Seeberg, September 23, 1801

I am hastening to tell Your Highness the good and pleasant news that Hera was found again. Mr. Bode wrote from Berlin that he believed to have found this new planet again, but he is not certain because he only saw it and did not observe a star that appeared to his naked eye to have changed its place. He gave me its place and that of the group of stars where he had seen the star on September 15. He asked me to verify his assumption and not to talk about it until it is certified. That is why I did not talk about it yesterday—I wanted to verify the fact first today. And despite the bad appearances of yesterday evening, I got up at 3 am this morning. But instead of observing the planet I found it was pouring rain so I started to calculate Bode's rough position and to combine it with older observations made by Piazzi at the beginning of this year. The result of my calculation is that I do not hesitate one minute to announce that the star seen by Bode is the new planet Hera and thus I dare to give Your Highness this news as a certain fact.

Bode's rough position coincides exactly with the position I indicated on my small chart, Hera's path, in the July issue of my journal, between two stars ε and λ in the Lion's head. I am now so certain that I am undertaking to find the planet at my first attempt with the telescope when the weather is fine again and I hope, if the sky is favouring me, to obtain the first good observations of it before closing my October issue. Waiting for this small planet's appearance, I considered it my duty to inform Your Highness first thing and laying myself respectfully at Your Highness' feet.

[Of course, Hera or Ceres had not yet been located as Zach so confidently asserted, although at least some Italian astronomers also believed the search was over. Antonio Cagnoli, in Modena, wrote to Angelo DeCesaris at Brera Observatory on on October 18: "I'm eager to hear we have come to an end in the discovery of Piazzi's planet. Let me have its place when you have fixed it."]

Zach to Lalande, Seeberg, Oct. 17, 1801

I am endlessly obliged for the elements of the new planet of Piazzi that you so have kindly sent me. Piazzi also sent them to Bode along with a printed page of his work he is publishing. You do not send me two other elements that he has sent Bode and of which I do not understand how he was able to find them. He says that the planet's diameter at the distance of the ☉ was 19″ and its volume 1.33 of that of the earth. He estimated the diameter of the planet 7″. What does all this mean? I do not understand. How can he find the planet's diameter to be 7″ when saying that it appeared like a star of the 9th order of magnitude? What a contradiction! So, this planet is greater than Herschel? It seems, he did not send you anything of this galimatias. Mr. Olbers has likewise calculated circular elements.

Dist. 2.730185 ☊ 2^z 20° 23′ 45″ Incl. 11° 3′ 36″ Epoch 1801 2^z 8° 36 51.″5 Revol. sid = 1647.75 days. Daily hel. Motion 13′ 6.″528. It seems likely that the planet at the epoch of its discovery was close to the line of apsides. Burckhardt has placed it in his elliptical elements at the aphelion, but it can just as well be the perihelion, but it is quite difficult to untangle with such a small arc of 9 2′ 29,7 that this planet has made in its orbit since January 1 to February 11. In this case, it is necessary to use these two hypotheses when searching for this planet. Know: If the star passed on Jan 1 its aphelion, its heliocentric motion is augmenting and consequently its geocentric longitudes will be greater in August and September than Piazzi's circular elements indicate. But, if on the other hand the planet was at its perihelion in February, the heliocentric motion is decreasing and the geocentric long. will be smaller in August and September than the circular elements indicate. Since we do not know which of the two cases will take place, we need to search for the planet a few degrees before and after the position that the circular elements of Piazzi indicate and search for it all over on the parallel of the latitude that matches best. Incidentally, the positions calculated after Burckhardt's ellipse match well regarding the latitude and there is only a difference of a couple of degrees for the longitude with Piazzi's elements. But I am afraid that despite these precautions, we will not find this planet in complete darkness and when we can observe it at the passage instrument only in November. The planet must appear extremely small at present. On Jan 1 its distance from the earth was 1.968 on August 19 3.645, on Sep 7 3.536, on Oct 25 3.091. On January 1 it appeared as a 9th magnitude star and since its light, or its claritas visa diminishes at the inverse ratio of the square of the distance, judge for yourself how small this planet must now appear, it must be at least of 16th order of magnitude and almost invisible. Since the first notification I have thought about this circumstance as you can see in my first announcement of that planet that I made in my Journal in the June issue, page 601.

On September 19, Mr. Baudet [Bode] *raised the alarm, he wrote me that he had found the planet, indicating the position that matched well enough the elliptical elements of Burckhardt, he asked me at the same time not to publish anything before I had verified this* *myself. David, Bürg and I, we at once searched that part of the sky with my beautiful parallactic telescope by Dollond, but we did not find anything.*

We were all disappointed, believing that it was due our clumsiness, as Baudet indicated this discovery as certain, but with the next courier he took it back and I reprimanded him insultingly for having alarmed us. His error proved that he mixed up two stars, which by chance were exactly at the distance of the diurnal motion of the planet. Despite the slim chances to find this planet before the end of November, I always search for it in the morning weather permitting in order to not waste my time pointlessly and when the dusk no longer allows me to chase the 8th order of magnitude stars. I am entertaining myself by comparing Jupiter to Regulus, this gets my parallactic telescope going and I will have lots of good observations when I catch the budding planet.

Zach to David*, Seeberg, October 30, 1801

I am enclosing the ephemerides of the orbit of the new Piazzian planet that you can use for finding it. It probably roams in the parallels of γ, θ, and β Leonis. I calculated the brightness by setting the luminosity = 1 on January 1st when Piazzi first discovered the planet and estimated it to be of 8th magnitude; on February 11th, when he stopped observing it, the planet was only 0.625, almost half as bright. It will appear equally bright at the beginning of December.

[*Martin Alois David (1757–1836) Director of Prague Observatory from 1800–1836. David observed Ceres for the first time on March 15, 1802. Zach appends an ephemeris from November 1 to December 31. This is very similar to the one published in the MC, November 1801 issue.]

Zach to Méchain, Seeberg, October 30, 1801

Since my last letter I have been calculating the positions of the new Piazzian planet according to his elements, which I have sent you. It is a small part of the ephemerides that I am sending you to facilitate the search for this star. But I recommend to always use the parallel of the latitude; the elements give the latitude well enough but the longitude may be faulty up to ±2°. This throws a great uncertainty on the declination, one must not trust it. The planet will go through the parallels between γ, θ, and β Leonis.

I have also calculated the visible brightness of the planet, supposing that on January 1 when Piazzi discovered it and considered it an 8th magnitude star, the brightness was = 1. On February 11 it was = 0.625 already and only close to December 7 that it had the same brightness and it almost appeared to be of the 8th order of magnitude.

Zach to Gauss, Seeberg, November 10, 1801

I received your esteemed letter of November 2nd together with the parcel and the following letter of November 5th on the same day, the ninth. Please accept my sincere thanks for all the tempting and wonderful things, which you were so kind to send me. Especially your studies of Piazzi's planet gave me great pleasure and I very much regret that I have not received your important letter eight days earlier, before finishing the November issue of the M.C. Consequently, your important remarks will be made public four weeks later, which means quite a period of time and is relevant in searching for the heavenly body, but I informed some of my astronomical friends, hunting for the planet carefully, that you extended the examination area further and thus might look farther east. I myself will be following this suggestion and send you daily news if I should be so fortunate and find this wanderer again. With pleasure I will relay any observation of this planet, mine or of other astronomers, so you can correct your ellipsis accordingly. In the meantime, your Honour, you will see yourself that in your ellipsis there is much hypothesis and that for example the aphelion and eccentricity might very well move away from the truth. Dr. Burckhardt might have adapted his ellipsis to the observations if he wanted to, but he did not think it worth the effort and it might just as well be as easy to find a suitable ellipsis for the case that one *wanted to date the perihelion February 11th. But carefulness is always advisable and in order not to mislead anybody one must not deny the possibility of more than one possible ellipsis through Piazzi's points: For instance, your Honour finds strong proof, or at least importance and trust in your elements that you deny the enormous observational error of 20″, assumed by Dr. Olbers, even Dr. Piazzi admits an error of 15″ in his observation of Feb. 11th (Nov. issue p. 572). But because you based your calculations on these incorrect observations it was only natural you found the error 0″, in this case your reasoning was* Petitio Principii *[begging the question]. This small detail does not change your ellipsis considerably and is hardly worth mentioning; your work stays nice and thorough, first because Piazzi's observations are so well presented by your ellipsis, at least because they show a* <u>*possible*</u> *path on which our new guest might just as well wander as on Burckhardt's path or any other, and thus serve as a clue for an observer, and the more so since all our previous reasonings assured us (falsely though) and truly, without your calculations no astronomer would have hit on the idea to extend his examined celestial region to the east. For this reason I very much regret that your letters arrived only after the Nov. issue. On the other hand I do not believe that this celestial body will be easily found again, partly because of the general bad weather but more because of the brightness with which this heavenly body can be seen these days; and according to my calculation of the magnitude (Nov. issue p. 581) your Honour can see that approximately on Dec. 10th this planet will appear as bright as on Feb. 11th when Mr. Piazzi found it to be so faint and ceased to observe. Thus, I hope the studies of your Honour will be published early enough in the December issue of the M.C.; and because I have to close this issue because of the register earlier, I am hurrying to send you with this post some remarks and propositions.*

First of all I took the liberty of sending your Honour a special copy of my continued history of the new planet. Therein you find a true excerpt of Piazzi's treatise, but mainly on page 10 a new revised print of all his observations. I leave the correspondence between these observations and a circular orbit for your own contemplation, but it shows how careful we have to be with ellipses. I am sending you the original sheet of Piazzi's treatise with his observations to avoid mistakes and writing errors. Now my urgent suggestion would be: Your Honour would correct his elliptical elements according to these last revised Piazzian observations, and it is up to you to apply the corrections of page 18; in order to leave also here no room for doubt I am sending you also the original where these corrections are on page 24. I must remind you there are differences between Piazzi's calculated longitudes and latitudes and mine, the error will be easily found. The data of the solar tables is also not correct, which is probably caused by Piazzi's use of DeLambre's Sun tables. The writing error in the longitude of the Sun on Jan. 10th has already been published in the Oct. issue. Our mean solar times are equally wrong, but because Piazzi observed his RA in the meridian, I think I must defend my time reductions, but because it is possible that the passage instrument is not completely in the meridian and Piazzi thus applied only undetermined observations it is very advisable to keep to Piazzi's times. When your Honour has finished the correction of your elements and if you could send them to me before Nov. 20th, I could arrange to publish those in preference to the ones already sent in the Dec. issue of the M.C. Since I understood from your letter you are not in possession of my solar tables, I took the liberty of sending you those accompanied with the plea to regard this as a small token of great respect.

As soon as I had received your letter I planned to calculate an ephemerides of Ceres' orbit according to your elements for Dec. 6 like I did with Piazzi's circular elements. While devising the formulae necessary, I found minor differences to your results: You state e.g. the difference between the true and the mean anomaly 8° 5′ 19″, but I find it from your eccentricity 0.0705553 to be 8° 5′ 22″.8. And accordingly for the difference of the true and mean anomaly

–29088″.11 sin An mean + 1281″.1 sin 2 An mean –78″.24 sin 3 An mean
+5″.48 sin 4 An mean –0″.41 sin 5 An mean

And for the radius vector

r = 7.44560/2.73548 ± 0.1930028 cos. true anom.

[According to Kepler's equation, the Mean anomaly = E –e sin E, where the mean anomaly is measured from perihelion, e is eccentricity and E is the eccentric anomaly, also measured from perihelion. This was the angular position, as viewed from the center of the ellipse, of a point moving in a circle circumscribing that ellipse. Zach himself, when writing in English, used the phrase "greatest equation of the orbit" to denote the "mean anomaly." See his letter to Banks of March 30, 1802.]

Since in the case of the helioc. motion the tropical and not the sidereal has to be used, I found the log. of the daily tropical mean helioc. motion = 2.8945394.

Since your Honour will now probably find a slightly different ellipsis, I postponed the plan for calculating the ephemerides until having received your new elements. If your Honour would like to undertake this task yourself, I would be very grateful. I am not answering the other points of your substantial letter, which I enjoyed very much, because I have to hurry to finish this one in order not to miss the next post, because otherwise the time would be too short to publish the desired material in the M.C. And another thing—but between you and me—can you believe that our good and honourable Kluegel [Georg Simon Kluegel, mathematician, 1739–1812] *takes Ceres Ferdinandea for nothing more or less than a distant meteor!*

Zach to Oriani, Seeberg, November 20, 1801

A thousand thanks for Piazzi's dissertation which you were good enough to send me. I am taking the liberty of addressing a letter to you for Piazzi, which I beg you to send him. I left it open for you so that you would become familiar with the elliptical elements calculated by Dr. Gauss in Brunswick, and which marvellously represents the observations from Palermo. This may, at the same time, guide your investigations of the new planet because without this, if you only search based on the circular orbits, you risk moving too far away from the real path. The positions of Gauss' ellipsis show the planet Ceres at 6° farther east than the circular orbit. I will send your remarks to Mr. Schubert in St. Petersburg with all the articles from your Theory of Mars.

Zach to Lalande, Seeberg, Nov. 26, 1801

I am sending you here, my dear friend, the elements of an elliptical orbit by Dr. Gauss, that represent—there could not be any better—Piazzi's observations. He has sent me corrected elements since, that are even better and the error, be it in longitude, be it in latitude, never exceeds 5″.

Here you are.

☊ 81° 0′ 44″

Inclin. 10 36 57

Aphel. 326 27 38

Eccentricity 0.0825017

Log. Semi-major axis = 0.4420527 (2.784)

Epoch 1801 = 77° 36′ 34″

Motion cal[culated] 78 5 16

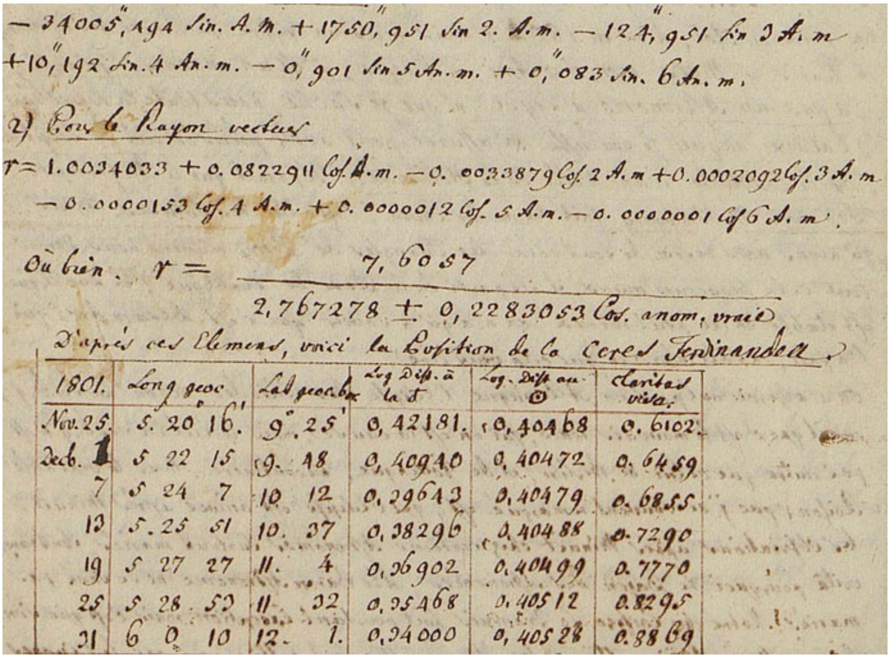
From there greatest eq. of the centre = 9° 27′ 41″

Hel. diurnal and tropical movement 12′ 50.″914

In Piazzi's memoir and in his observations on Jan 13 and Feb 8 the long. of ⊙ is too great by one minute. In Table II it must be geoc. latit. South, not North. Piazzi also used an obliquity of ecliptic too great by 10″.

According to the above elements, in order to make calculating the place of the planet easier, I have calculated the following equations (Fig. 14.3).

1) For the equation of the centre



— 34005″,194 Sin. A. m. + 1750″,951 Sin 2. A. m. — 124″,951 Sin 3 A. m
+10″,192 Sin. 4 An. m. — 0″,901 Sin 5 An. m. + 0,″083 Sin. 6 An. m.

2) Pour le Rayon vecteur

r = 1.0034033 + 0.0822911 Cos. A. m. — 0. 0033879 Cos. 2 A. m + 0.0002092 Cos. 3 A. m
— 0.0000153 Cos. 4 A. m. + 0. 0000012 Cos. 5 A. m. — 0. 0000001 Cos 6 A. m.

Ou bien. $r = \dfrac{7,6057}{2,767278 \pm 0,2283053 \text{ Cos. anom. vraie}}$

D'après ces Elemens, voici la Position de la Ceres Ferdinandea.

1801.	Long geoc	Lat geoc bor	Log Dist. à la T	Log. Dist au ☉	Clarités visa.
Nov. 25	5. 20 16	9°.25′	0,42181	0,40468	0.6102
Dec. 1	5 22 15	9. 48	0,40940	0,40472	0.6459
7	5 24 7	10 12	0,39643	0,40479	0.6855
13	5.25 51	10. 37	0,38296	0,40488	0.7290
19	5 27 27	11. 4	0,36902	0,40499	0.7770
25	5 28. 53	11. 32	0,35468	0,40512	0.8295
31	6 0 10	12. 1.	0,34000	0,40528	0.8869

Fig. 14.3 Calculations relating to the equation of the center and a table of Ceres' positions

Here is the position of Ceres Ferdinandea according to these elements. We were astonished by the great inclination of the orbit of this planet. But analogy might not prove anything here, one could leave this, by employing it, like Senateur La Place did with the satellites of Uranus and the solar equator. The inclination of Ceres' orbit against this equator is not as great as that against the ecliptic, it is the terrestrial orbit that digresses the most. An ellipse can so nicely represent Piazzi's observations; how can you still doubt that this is a planet?

If it were a comet it would be even more unusual than that of 1770. But why do we not find it?

I am searching for it every time the weather permits my doing so, but this occurs rarely at present. [Red ink: I am letting you into the secret], for I have nothing to hide from you, I am telling you that Olbers has just written me from Bremen on Nov 20, that he had found a suspicious star on Ceres' parallel that cannot be found in any of the catalogues not even, in your Hist. Celeste. But because the sky has not been clear since then, he had not been able to verify if the star of the 8th order of magnitude is still at the same place, the first beautiful morning when he will be able to verify this, he will notify me and I will certainly not miss to send you a courier at once. You probably agree that currently this news is not ready to be published and Dr. Olbers would be very angry if this happened before he could have verified his suspicion there are many more 8th magnitude stars that have not yet been observed. What do you think of the manner in which Piazzi observed and assessed the diameter of this planet 7″. By the way, these $\underline{7''}$ do not match the $\underline{19''}$ that he gives for the distance = 1.

Zach to Gauss, Seeberg, November 29, 1801

Just a few minutes before the post will be collected, I received the actual galley proof and I only have the time to send it to your Honour in haste. With the next post I will have the *honour and be able to send you all of Piazzi's observations, reduced, with location of the Sun and Log diff. Piazzi has been wrong with several reductions, especially with the mean times. I do not send you these 6 renewed reductions because they were necessary for your current studies—your last calculation has almost exhausted the matter—but only for that case that you want to connect these observations of Piazzi with any in the future. With the last mail I received a note from a German astronomer* [probably Johann Huth] *that he saw on November 10th a suspicious star on exactly that parallel where Ceres must be, and which cannot be found in any star catalogue and is of the same expected size; but since then the sky has been overcast and he has not been able to verify his supposition. I am expecting every day now the confirmation, whether it really was Ceres or an unknown star. Once the planet is found it can get away no more and then I hope to provide you with very accurate observations. The post is burning at my fingertips, let me close by assuring you of my heartfelt admiration.*

Zach to Oriani, Seeberg, December 1, 1801

I am only writing you two words extra, my dear friend, since the enclosed letter for Mr. Piazzi is not only for him but for you as well, that is why I am sending it open so you could read what you judge to be of interest to you. I am asking you to close it and to send it to Palermo, if the postage is considerable please add it to my bill that we will settle at the time of the great payment.

Zach to Gauss, Seeberg, December 2, 1801

Today it is my pleasure to be able to send you the newly reduced observations of Piazzi. It is hardly worth the effort to match your elliptical elements to them, because those can hardly change. I am sending you these carefully calculated observations only for the case if they can be combined with future observations, then they will remain the basis for all future calculations. I cannot understand how Piazzi could be so wrong on average in reducing his times; there are errors of 12 time seconds, like on Jan 3. Probably he used DeLambre's Sun tables of LaLande's Astronomy, but even then the difference should not be any larger than at most 0″.2. For calculating the longitudes and latitudes I used the multiplication–latitudes and latest apparent inclination of the ecliptic (23° 28′ 5.″3) observed by Méchain and DeLambre in Paris on the summer solstice of June 21. For calculating the position of the Sun I used my Sun tables together with the corrections mentioned in AGE vol. IV p. 481. I decreased my epoch by 7.″25, increased the longitude of the apog. by 2′ 27″, omitted the perturbation equation for Mars and consequently my solar tables match the sky except for some seconds, at least for now.*

*In 1792 he published Tables du Soleil, de Jupiter, de Saturne, d'Uranus et des satellites de Jupiter in the third edition of Lalande's book *Astronomy*.

Zach to Lalande, Seeberg, December 6, 1801

We do not yet know anything of Ceres Ferdinandea. It is too early to be hoping to find her with my beautiful meridian telescope and my parallactic telescope is not as good, the star is still pretty small, and it will only be around the end of this month that it will appear with the same sparkle as during its discovery on January 1, 1801. Within 15 days, I hope to be able to view 8th magnitude stars in the meridian; the time when Ceres is to culminate according to our hypothetic elements, Schröter, Olbers, Bode have not found anything either. And the great huntsmen of France, Méchain, Messier, nothing either? In the meantime, I have sent you, as I had promised, Piazzi's reduced observations of the new star. Piazzi observes very well but calculates carelessly. The reductions of the observations of his planet are a strong proof of that. He badly converts the sidereal time, there are differences up to 10″ in time for instance on January 3. The locations of the sun are often faulty by half a minute. I send you these observations scrupulously reduced for the future, and to combine them with the observations when this coquette will be found again, for the first observations of Piazzi will always serve as basis and foundation of all our future calculations, unless we *do not find this star in some former star catalogue. It does not seem that C. Le Français de La Lande has it in his immense collection of observations for you will see that by an oversight he unfortunately only ever observed those zones in which this new star never was.*

Zach to Oriani, Gotha, December 18, 1801

What is Ceres Ferdinandea doing? Nothing has been found yet, neither in France, nor in Germany. One begins to doubt. The haughty jokesters are already having fun. But what the devil is Piazzi doing then? Lalande wrote me that he has changed his observations again and he gave them a new edition. What does it mean? Lalande adds in his letter "Here is why I do not believe in the planet." But I think that this new edition of observations do not show any others than the ones he has already given. He had made calculations and impressions, especially about the average time, the locations of the sun and I like to believe that this is only what Piazzi would have changed. I noticed it, and that is why my calculations do not agree with yours. I hope that he did them again and that we will agree with what I submitted to my journal. I beg you to send these printed sheets to Piazzi with my compliments. I would like very much to see his new edition of observations.

That devil of a planet torments me so! I assiduously observe all the little stars from its latitude, but the bad weather contradicts me furiously. I already believed to have caught it by the four corners several times, but it was always nothing. At this moment, I have it in petto [literally: secretly] *but at the first light, it will be nothing again. What is most tormenting about the whole affair is that I am often obligated to wait eight and fifteen days to verify a new star, as much of the sky is overcast here this season.*

Since the 7th of December, I have not had a clear sky. On that day, I observed several unknown stars, which I could not find in any catalogue, not even in the one by Bode in folio which has just appeared and which portrays all the stars. Here are their right ascensions observed:

No. 1	*11 h 54′ 14.″04*
No. 2	*11 h 55′ 57.″44*
No. 3	*11 h 57′ 8.″43*

But #1 I found nowhere. I have only to visit it, and that I have not been able to do since the 7th of December. It is very disquieting!

On the 16th of December, there was a break in the sky. I had observed several small stars. When #1 should have passed to the meridian, it did not appear. Great joy! I was starting to believe that I had caught the coquettish little Ceres, but the joy did not last even a minute for I no longer saw #2 nor #3. A light fog was hiding them from me. And that is where I am in my hunt for Ceres. See if you find #1 again, perhaps it is the planet, Gauss' ellipsis gives this position. Note once again that a very small star, almost nebulous, preceded #3. I did not see it, but only noted it.

I hope that in your next letter I will have better news of the planet than what I have given you.

Zach to Lalande, Seeberg, December 24, 1801

No news on Piazzi's planet.

Zach to Lalande, Seeberg, January 7, 1802

Still no news regarding Piazzi's new planet, but the weather has been abysmal here, in 31 days only two fairly clear nights, how can we find such a small star?

Zach to Méchain, Seeberg, January 13, 1802

I am pleased to tell you that I discovered the new planet on Dec 7 1801 at 18 h 48′ 10″.3 m.t., RA = 178° 33′ 30″.6, Declin. 11° 41′, estimated as you can see from the enclosed printed pages. I saw the planet again on Dec 31, then again on Jan 11, 1802 at 17 h 3′ 17″.4 m. t. RA = 186° 45′ 49″.95 Decl. n. estimated 11° 10′. There can be no doubt that this is Ceres Ferdinandea. The ellipse I have enclosed does not stray 30 min from RA and 8 to 9 min from the decl.

Mr. Olbers discovered the planet too, but later than I did.

On January 2, 1802 at 11 h 58′ 36″ m.t. RA 185° 9′ Decl. 11° 7′

Jan 5 17 30 185 43 11 8

His observations were mostly made at a mobile telescope with a circular micrometer. My observed RA's of Ceres are very accurate, one could not obtain better, but my declinations are only estimated from sight. I could no longer see the planet at the telescope of my circle; the threads are too delicate, I could not illuminate them without losing the planet, but I will change this. Here are the ephemerides in order to find the planet but you have to subtract 30′ of the calculated RA and add 9′ to the declination. It is now hardly possible to miss the planet it is almost of 10th magnitude and dull without any kind of disc. Soon I will write you in more detail, this letter all in a haste. Heart and soul, your eternal friend and very devoted Zach.

Zach to Lalande, Seeberg, January 14, 1802

It is my honour to announce that I was lucky enough to discover the new planet of Piazzi on Decbr 7 of last year. Here is the story of my discovery and the reason why I did not inform you any earlier, although my observation of that planet of December 7 was published in print already 3 weeks ago.

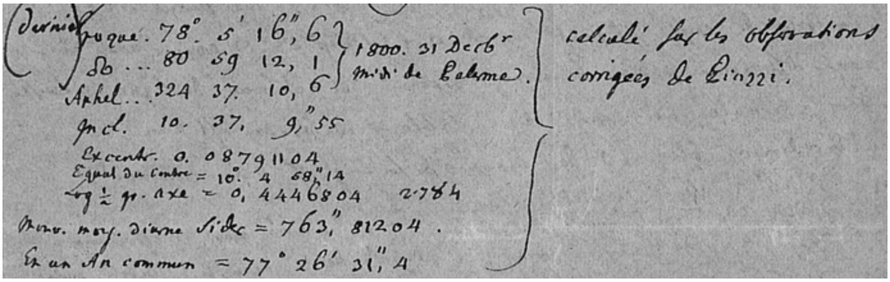
The weather has been awful throughout November here. Decb^{r} 7 was the first morning in five weeks where I could chase this little planet. I observed all 7th, 8th and 9th order of magnitude stars that were on Ceres' parallel according to the ellipse of Mr. Gauss after which I calculated the ephemerides that I sent you. That day, I had four stars that were not in any catalogue, but searching in your precious Histoire céleste, *I found two of them, page 225 that C. Le Français observed on April 6, 1796. Two remained unidentified, You will find these four stars on page 3 of the enclosed sheet of my Journal. Decb^{r} 17 was the first clear night that followed but there were whitish stripes in the sky that prevented observing the small stars. I did not see my 4 small stars at all. The sky was overcast just until Decb^{r} 31. That night, I found your two small stars no. 2 and no. 4. But no. 1 was not there. So, this was the new planet of Piazzi. But I was not sure and wanted a third observation before making any noise, I only shared my secret with the Duke, God and Friend Bürg. Imagine my impatience until the first beautiful night, it came only on January 11, my suspicion turned to certainty, and I found my little planet at once. I can with all certainty announce my discovery. My observation of Ceres on Decb^{r} 7 is excellent and as I have published it on Decb^{r} 27 + in the January issue 1802 of my*

(FN by Zach) + I did not know yet that it was the planet, and I could only verify it on Decb^{r} 31.

Journal: 7 Decb^{r} 1801 at 18 h 48′ 10″.3 (11° 41 ½ N) mean time Seeberg Right asc. of Ceres 178° 33′ 30.″6 because I was not searching for the planet, I did not at all observe these small stars at the quadrant, therefore, I only guessed the declination by sight to be 11 41 ½ north. That very night I also observed the planet Herschel 19 h 17′ 32.″7 m.t. RA = 185° 55′ 18.″2 Decl. 1° 47′ 12.″3.

On Jan 11 I observed Ceres at 17 h 3′ 17.″ 4 m.t. RA = 186° 45' 49.″95 (11° 10′)

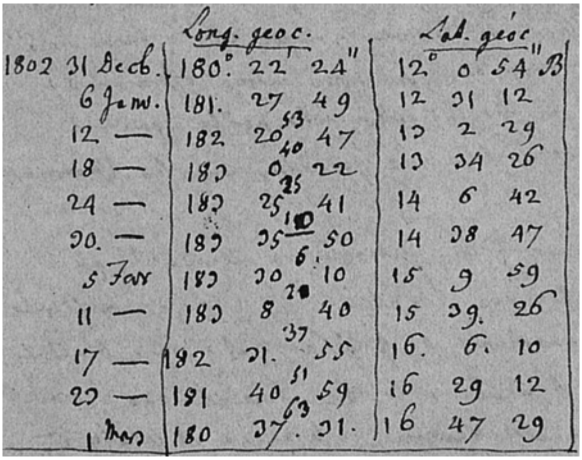
I was not able to see that little planet in the telescope of my quadrant; unfortunately, I use some spider silk threads. I did not see them in the darkness and when I illuminated them, the little planet disappeared, so I could only estimate by sight in my passage instrument the declination to be approximately 11 10′. This progress corresponded perfectly with the planet between Mars and Jupiter, of whose existence can there be no doubt, and which



Dernier Epoque. 78° 5' 16",6 } 1800. 31 Decbr midi de Palerme.
Do... 80 59 12, 1
Aphel... 324 37 10, 6
Incl. 10. 37. 9",55
Excentr. 0. 08791104
Equat du Centre = 10° 4 58",14
log ½ gr. axe = 0, 4446804 2·784
Mouv. moy. diurne sidér = 763",81204.
En un An commun = 77° 26' 31",4
calculé par les observations corrigées de Piazzi.

Fig. 14.4 The ellipse by Gauss

Fig. 14.5 Ephemerides for Ceres in Jan. and Feb. 1802



	Long. geoc.	Lat. geoc.
1802 31 Decb.	180° 22' 24"	12° 0 54" B
6 Janv.	181. 27 49	12 31 12
12 —	182 20 47	13 2 29
18 —	183 0 22	13 34 26
24 —	183 25 41	14 6 42
30. —	183 35 50	14 38 47
5 Fevr	183 30 10	15 9 59
11 —	183 8 40	15 39. 26
17 —	182 31. 55	16. 6. 10
23 —	181 40 59	16 29 12
1 Mars	180 37. 31.	16 47 29

I was the first to find again (to date to my knowledge) this famous planet in which you would not believe in. But I persevered and I congratulate myself to have maintained that belief in my Journal to have made calculations and ephemerides, without which I do not think it would have been possible to have this star so quickly again, at least it was not found in France until the epoch of January 1, which is the date of your last letter, in which you still doubt the existence of this planet. I ignore what has been done in Italy and if Piazzi has found it. In Germany it was Olbers who found it, but in my opinion he only saw it on January 2 for the first time, notwithstanding he deserves the merit to have found it himself, for he did not know about my observation of Decbr 7 and my January issue in which the observation is published, was in Bremen only on January 2, it was not distributed until December 31. The issue had already been printed, when I first discovered in the night between Decbr 31 and January 1 that the little star no. 1 that I had observed on Decbr 7 was missing and had changed its place.

Mr Olbers wrote on Janur 6 from Bremen, that he had found Ceres again on Jan 2 1802 at 11 h 58′ 36″ m.t. RA 185° 9′ Decl. 11° 7',

then again on Jan 5 ---- at 17 30 --- --- RA 185° 43′ – 11° 8′

But these observations are inaccurate and made at a mobile Dollond telescope with a circular micrometer. My RA possesses greater accuracy and you can count below to the second. A calculation made me see that Gauss' ellipse give the position of the planet to half a degree in RA and to 8 or 9 minutes close in declination, a little more advanced than the *observation, which is really amazing, and proves that we have not badly reasoned and calculated.*

Here is the latest ellipse by Gauss (Fig. 14.4).

According to these elements. Here are the ephemerides for Ceres (Fig. 14.5)

But we need to take 30′ off the right ascension calculated in order to get closer to the observation, at least in January, for afterwards this ellipse must move increasingly away. I hope to be able to send you in my next letter some elements corrected to my own observations as well as several other observations of Ceres. Last night I was unable to see the planet, the fog was too heavy. Ceres appeared extremely small, I would say almost of the 10th order of magnitude, I saw her with a magnification of 120 times, she had not changed in appearance no trace of a disk, it is a dull star and is indistinguishable.

Zach to Oriani, Seeberg, January 14, 1802

I am informing you with great haste that I found the planet Ceres Ferdinandea on the 7th of December of last year. I had already printed this observation in my journal, January 1802 edition without knowing then that it was the planet, but I doubted it. On December 31st, I verified the thing and my suspicious star had changed its position. On January 11th, I observed it for the third time (the season here being frightful), and I was certain of my findings, which I have the honour of announcing to you. Here is my observation:

	Apparent RA	*S. Declination*
1801 Dec. 7 18 h 48′ 10″.3 m.t.	*178° 33′ 30″.6*	*11° 41*
1802 Jan. 11 17 h 3′ 17″.4 m.t.	*186° 45′ 49″.95*	*11° 10*

The RA were very exact; one cannot ask for better, but the declinations are only a rough estimate. I have not yet been able to see Ceres with my wall quadrant. It was not with C's instrument that I was able to see it. The planet is 9th magnitude in size, maybe even less, with 120 magnification and I was not able to see the slightest trace of a disc.

Mr. Olbers discovered the planet Ceres on his side in Bremen, but later than me, on January 2nd. I say, "on his side," because he had actually made the discovery like me, for I had not communicated my observations to him, which was being kept secret until after January 11th in order to be certain of my discovery. Olbers' observations are [same as the figures in the Zach-Méchain letter of Jan. 13].

I hoped that Piazzi or you other gentlemen astronomers, in the beautiful climate of Italy, would have found the planet before me. In France, we had not yet found it on January 1st. That is the last date of my letters from Paris where, in general, the existence of the planet Ceres is in doubt.

If you write to Mr. Piazzi, ask him about my observations. They differ about 1/2 degree in RA and 9 minutes in declination from Gauss' ellipse. In order to find this body, here is a small ephemeris to which the correction must be applied. [Zach also wrote about Ceres to Joseph Banks on this date.]

Zach to Gauss, Seeberg, January 17, 1802

It is my honour and pleasure to inform you that on Dec 7th last year I was so lucky to discover Ceres Ferdinandea. That star, observed by me on Dec 7th and suspected and reduced prophetically as a planet, mentioned in the January issue of the M.C. p. 92, was nothing else but Ceres. Now I have three good observations of this planet in RA, but unfortunately I was unable to observe it at the wall quadrant, since it is so faint and the observation does not agree with it, so I cannot discern the threads. At my Passage Instrument with its big aperture I can see it clearly and my RA are excellent and accurate. Here they are:

	Mean time	*RA app.obs.*
1801 Dec 7	*18 h 48′ 10″.3*	*[Ceres] = 178° 33′ 30″.60*
1802 Jan 11	*17 3′ 17″.4*	*[Ceres] = 186 45 49. 95*
Jan 16	*16 46 25″.6*	*[Ceres] = 187 27 53. 25*

Dr Olbers found Ceres on Jan 1st. His obs. that he has sent me are [same as the figures in the Zach-Méchain letter of Jan. 13]:

His observations are not very accurate since made at a circular micrometer only. But I hope to be soon able to send you all obs. I would like to thank you for your diligent studies—without them we would not have been able to find Ceres so soon and I promise to promote your merits. So much in haste. Soon more and better.

Zach to David, Seeberg, January 22, 1802

Finally, I was lucky and found Ceres on December 7th last year. I am hurrying to advise and am sending you the ephemerides, in order to find it immediately. Please inform the K. Akademie der Wissenschaften on my behalf. [printed next is a table of positions from Jan. 24 to February 5]

[Although Zach terminated his relations to the K.b.G.d.W. (Königlich böhmische Gesellschaft der Wissenschaften = Royal Bohemian Society of Sciences) in Prague in the course of a quarrel about Emery′s chronometer, he considered it correct as its foreign member (from 1792 on) to inform the Society about his discovery.]

Olbers to Zach, Bremen, January 23, 1802

Finally today, my dearest friend, a word from you—your letter of January 17 with the enclosed pages of the January issue of the MC, which we have not yet received. I have been waiting impatiently for this letter since I was afraid my letter of January 6 might have gone lost. With pleasure I saw that you, dearest friend, were so lucky to find and observe Ceres already on December 7. Without doubt, you are the first astronomer who found this small planet again. If you want to grant me a <u>*small*</u> *share of the honour of the rediscovery, I only owe it to the bad weather in December. If you had, as I did, only two consecutive clear nights, the matter would have been decided at once.*

Now, after reading your letter, I imagine the things to have happened as follows: Only on December 31 you were completely certain that you had found Ceres <u>*on the 7th.*</u> *But you were not so certain whether you saw that Ceres on December 31 again. And naturally, this could not be otherwise or you must have known all telescopic stars of that region in advance. But I am actually doubting whether you (but I am only judging according to your letter) saw Ceres on December 31. The given RA of 12 h 20′ 16″ matches much better Bode's N191 than Ceres, which must have had 1 1/4 minute less RA. Anyway, the uncertainty regarding Ceres' position prevented you to publicise your, most certainly made observation of Ceres on the 7th, at least as a certain observation of Ceres. By this I am explaining your silence against me. You wanted to wait until you were able to give us also accurately Ceres' position reliably but the terrible weather postponed this until January 11. Enough, dearest friend! Yours is the honour of the first rediscovery of the long sought after Ceres and I want to congratulate you from the bottom of my heart. Mine is only the small honour of being the first who announced, favoured by good weather, its rediscovery. In the reduction of my communicated observations I indicated the RA of 187° 54′ 57″ which due to a slip of the pen should have been 187° 57′ 57″. Therefore, all communicated RA's of Ceres from January 10 on, when I was still comparing it to the stars of Virgo, are too small by 3′. Here they follow together with two later ones, corrected by these 3′:* [these are his observations from Jan. 10 to Jan. 22]

Ceres is strongly increasing in brightness and certainly is brighter that an 8th magnitude star. Last night I observed it at a magnification of 180 times but could not distinguish *a distinct disk. Why does your excellent meridian telescope not have also such a device that one could determine through a micrometer small differences in declination? Would this in any way interfere with its main use?—I would believe not.*

Lalande to Oriani, Paris, January 22, 1802

Having learned that my dear colleague Olbers has seen Piazzi's planet again, and since you were the first who had made it a planet when Piazzi considered it only a comet, I would like you to tell me how you were able to do so having only two or three observations and the differences being so small.

Here are the elements of Mr. Gauss: [These are printed in the January 24 paper by Burckhardt]

Here are the obs. of Mr. Olbers [these are the Jan 2 and Jan 5 observations, the same as those in the Zach-Gauss letter of January 17, 1802].

[Note here that Lalande is giving credit to Oriani, not Bode, for the recognition of Ceres as a planet.]

Zach to Gauss, Seeberg, January 23, 1802

I have been waiting in vain to be able to send you a complete observation. Now that it is thawing and with westerly winds we have to wait quite some time for clear skies. But I would like to continue sending you news about Ceres that were sent to me.

Until now only Dr. Olbers and I have been able to observe the new planet. A rough estimate shows that we have to subtract approximately 33 min of the RA and 15 min decl. which follow from your recently sent elements (V). But with time this divergence will increase; but certainly we will be able at any time with this correction to find Ceres even if the sky will be overcast for a fortnight. It probably is quite easy for you to correct your almost matching orbital elements so that I am hoping to publish in the March issue of the M.C. very accurate tables according to your elements. Too bad that the February issue is almost closed otherwise I would have included your elements.

You probably have received my three observations. I could send you one of Dec 31st, but unfortunately I had the planet at only two threads of the passage instrument and forgot in haste to recognise which of the five they were. I will be able to decide this matter when I have an exactly calculated position of Ceres, the error can only be ±1 or 2 intervals of the threads. Meanwhile I am giving you again my accurate obs. of the RA [same as the figures in the Zach-Gauss letter of Jan. 17].

Dr Olbers has reduced his observations more accurately, here they are. He considers the RA very good. He could not give the declination as exact.

Jan 2	*11 h 58′ 36″*	*RA 185° 7′ 40″*	*Decl. 11° 6′ 30″N*
5	*17 30 0*	*185 43 7*	*11 7 56*
10	*12 25 41*	*186 31 52*	*11 13 9*
13	*11 43 38*	*186 58 56*	*11 18 56*
14	*11 9 3*	*187 7 11*	*11 20 57*
15	*12 8 9*	*187 15 27*	*11 23 25*

From this I can see that Bremen has better weather than we have at the foot of the Thuringian Forest—here it was completely overcast on the 13th, 14th and 15th of January. I have to add that on Dec 16th, Jan 1st and 6th I observed the Sun in order to determine the error of my solar tables. Namely:

1801 Dec 25	*+5″.6*	*The longitude is given too big by*	*+3″.0* [red ink]
1802 Jan 1	*+6. 4*	*The epoch has been diminished in the calculation by*	*+3. 8* [red]

″ Jan 6	*+3. 9*	*7″.2 decreased The apogee by*	*+1. 3* [red]
	2′ 27″	*and the equation of Mars was neglected*	

but if you take it into account, the errors are like the red numbers at right. Before this letter was collected I discovered Ceres again, but unfortunately not at the wall quadrant.

On Jan 22nd 16 h 25′23″.9 mt RA = 188° 6′ 25″.80 Decl. 11° 40′ (estimated).

[The term "apogee" is conventionally used for geocentric orbits; the corresponding term for heliocentric ones is "aphelion".]

I am very much looking forward to receiving your ways of calculation and problems which you were so kind to promise me, and your future attempt to solve the Ceres problem. Dr. Olbers asked me to send you his great respect and admits that without you he would not have been able to rediscover Ceres. He would not have searched that far east at least.

Zach to Méchain, Seeberg, January 25, 1802

I have enclosed, my dear friend, my observations of the new planet Ceres and hope that you have observed it already several times. Here are my observations: [These were published in the MC.]

I could not observe the planet with my quadrant, the aperture is too small and with illuminating the hairs the planet disappears but increases in brightness and I hope to be soon able to observe good zenith distances. Here are Dr. Olbers' observations at the circular micrometer, his observations are a wee bit erroneous. [These were published in the MC.]

I hope to send you soon new elements of Ceres' tables but today I am in haste since the post leaves in an hour.

Zach to Gauss, Seeberg, January 26, 1802

I am hurrying to send you once again an observed RA, but unfortunately again no exact declination. I cannot discern the planet at my quadrant after illumination of the threads. Furthermore, Ceres seems to change—it now appears like a star of the 9th order of magnitude and then of the 7th, this must be due to the foggy air. On January 22nd it was so bright I considered it to be of the 7th order of magnitude and tonight it seemed like 9th. This fact almost lets me question whether I really had observed Ceres on Jan. 22nd. The field of my passage instrument is star-studded, it is even difficult to find Ceres especially in hazy air, so I could have observed a star instead of the planet. The differences are quite alright and it would be a strange coincidence if close to Ceres of all places was a star of 8th or 9th order of magnitude. These days her path is unsteady. Here follows today's observation

Jan 25 16 h 14′ 32″.9. RA = 188° 20′ 39″.15	*Decl. 11° 51′*
	Estimated
	Very uncertain

Zach to Gauss, Seeberg, January 27, 1802

Dr Olbers has made a mistake in the reduction of his observations. I just received this information and am hurrying to relay it. Dr Olbers' observations should be:

[This table is the same as that published in the Feb. 1802 MC.]

My observations remain as they were

[This table is the same as that published in the Feb. 1802 MC.]

Ceres' brightness is increasing but the weather is foggy and on the 26th I could see the planet only with great effort. Letters from England of Dec 27th and France of Jan 15th say nothing of a discovery of this planet.

Zach to Oriani, Seeberg, January 30, 1802

Here is further news of Ceres which I ask you to pass on to His Highness.

[Zach here prints his observations from Dec. 7 through January 29, and those of Olbers from January 2 to January 22.] *The astronomers of Lilienthal observed the disc of Ceres to be 2″.*

Zach to Gauss, Seeberg, February 4, 1802, 4am

I just observed Ceres again, I will reduce it and mail it immediately. Here are my recent observations [The numbers in this table are somewhat different from those Zach published in the March issue of the MC]:

1802 Jan 30 15 h 55′ 57″.5 RA	*=188° 36′ 43″.95*	*Decl. 12° 19′ 0″.7*
Jan 31 15 52 9.7	*188 38 45. 45*	
Feb 3 15 40 35.8	*188 42 13. 05*	*12 40 5.0*

With the next mail you will receive the January and February issue of the M.C. I would like to send it to you regularly. I should have done that for a long time and must apologise that it has not happened. The sets for 1800 and 1801 will be yours with the next mail. Please take this as an insufficient sign of my respect in which I always remain.

Laplace to Zach, Paris, February 4, 1802

I am hoping that we will have the elements of this new planet in a few weeks, which are precise enough to determine the perturbations and to know even better its elliptical elements. Its perturbations have to be considerable. Having calculated the perturbations and the new elements you will have excellent tables for the motion of this new planet, which will be known just as well as the others.

Olbers to Zach, Bremen, February 6, 1802

Thank you very much for your very interesting letter of January 31, the beautiful bust of Ceres and the Conn. des tems for Schroeter and me. Please convey my special thanks to G. de la Lande. But I can only say to you, my dearest admirable friend, that all those many, many tokens of your friendship and attachment touch my heart and I am at a loss for words to express my feelings. I am experiencing the same problems with Ceres. It is so variable in its apparent size that it is difficult to recognise on some evenings. Here are my latest observations: [These observations from Jan. 25 to Feb. 5 are in the March issue of the MC.]

The declination of the observations of January 25 and 31 is very disputable. The declination of both observations of February is maybe awkward because the declination of 34 Virginis is uncertain. I derived it from the zenith distances of Lalande's Hist. celeste *because I did not trust Flamsteed's determinations. My further observations on Ceres do neither deserve your attention nor a publication in your MC. They might have been of interest as long as they were the only complete ones. But now also you see it with your quadrant: Citizen Méchain found it on Jan 24, de Lambre on the 25th and Burckhardt and Le Francais observed it on the 26th. Bode plans on observing at the passage instrument and mural quadrant in February. With this every use of my observations is gone since they are not accurate enough for a planet observed beyond the meridian and made at the circular or hair micrometer and often depending on an uncertain position of one single star. I am, as you know, rather equipped for observing comets and not accurately planets. For my own pleasure I will follow the small planet until its opposition, though.*

My dear friend! You completely got me wrong, if you believe I had the slightest doubt that you really were certain of the existence of Ceres on Dec 31. No! You only found N1 on December 7 no longer at its position and thus you were unsure whether you had discovered Ceres on December 7. I only meant, and still believe (since this explains your silence against all your friends) that you could not at once recognise on December 31 the Ceres *certainly observed on the 7th among all the stars observed on that day, or you ought to have known all telescopic stars of that region in advance where it was on December 31 and you wanted to await this second recognizing before you publicized your discovery. And really the star of which you gave me the RA 12 h 20′ 16″ = 185° 4′ was not Ceres: but I see now that you also observed on December 31 a star in 184° 44′ and this actually was our planet. Certainly you must have observed on this December 31 several other small stars of that region or rather all small stars and that you initially took the one further south (185° 4′: most likely N191 Bode) for Ceres might be caused by the fact that your* <u>*estimated*</u> *declination on December 7 is too small by about 13′.—This is my hypothesis and explanation for your silence: and you see that it neither doubts the certainty nor your priority. But as soon as you as you tell me that this is not the fact and that you observed on 31st Ceres again at its former position, I give up my hypothesis at once, I am assuring you.*

So you are, my dearest friend!, let me repeat it, undoubtedly the first who rediscovered the long sought after Ceres. I am saying this with utter conviction for I must tell you honestly that the great error in your estimated declination perplexed me since your estimate indicated only 11° 41′ 1/2 and Ceres must have had on Dec 7 a north. declination of 11° 54 to 11° 55′. You might find the reason for the error in your estimate in your diary: <u>*but your N1 was without doubt Ceres*</u>*. The observed RA matches the now improvedly calculated ellipse by Gauss in such a way one could not wish for a better correspondence. On the 12th he expressed his delight about the rediscovery of Ceres and communicated on the 15th his first observation of Ceres of which he became certain when he received my observation of the 15th. According to a quote of one of Piazzi's letters of Dec 8, 1801, to Bode who shared it with me Piazzi had not found it then: "But I do not give up so easily; although I begin to doubt strongly that my small star could be the dear sister of the famous comet of 1770". I am asking you not to publicize the text in brackets. You, my dearest friend, have earned the honour of the first rediscovery of Ceres! But then also our dear Gauss deserves justice since it is to him we mainly owe this fortunate rediscovery. I for my part at least, admit it willingly and honestly, I would not have searched for Ceres so far east.*

Zach to Gauss, Seeberg, February 7, 1802

It is my pleasure to send you all of my observations of Ceres. It is a very peculiar paradox but true though, that I have troubles observing Ceres <u>*because my instruments are too good*</u>*. Such a weird thing has only happened to me once in the past 30 years of observing—it was the very inconspicuous comet of 1799. I then (AGE vol. IV, p. 265) complained, too, that any illumination does not agree with my 2 first threads. When the threads were visible, the comet disappeared and when I saw the comet the threads were invisible. That is now the case with Ceres. I then remarked, too, that the fine threads—otherwise the advantage of my excellent instruments—were a disadvantage. But there is something else that makes it more difficult this time: the high magnifications impede a clear image of the planet. These high magnifications are the reason why Ceres appears so faint and dim, especially at the quadrant, overkill is possible! I saw the planet with my comet searcher and small telescope much better and more distinct than with any of my large and excellent instruments and unfortunately I only have high magnifications and smaller are not easily or quickly obtained. Thus I had to resort to thicker silver threads and I put some in my quadrant. I hope to cover the planet completely at zenith distances and not need any illumination at all. But I consider my observed RA of Ceres* <u>*very accurate*</u> *and I cannot let a single second being taken away from it. Your Honour believes in the last letter of Jan 31st that if only one could decrease the RA of Ceres of Jan 16th by 10″, the differences would correspond better. But I revised my observation and am reassured and must insist on my numbers. This matter might be solved if you will have polished your elements, right now it cannot be decided. But your calculations of my five observations from December 7th until January 25th show that those will most probably be even better according to your improved elements.*

More difficulties mark the planet's observation on December 21st; the stars appeared especially flickering and scintillating that night, the threads' illumination had to be the faintest so that I recognised the passage at the threads only because I fixed my gaze at the *stars and realised the glare the threads were causing when the star passed. I had to eliminate all light and write in the dark just as I had described the observation of the comet of 1799 in the AGE. How misleading such an observation can be is easily understood. But since you want to try your luck with it, here it is, maybe you can ascertain the true thread. I wrote*

A = 12 h 18′ 56″ true sidereal time and

B = 12 20′ 16″ true sidereal time

At first I considered A to be the middle meridian thread and then B. But it probably is the thread B and the minute is wrong and must be 12 h 19′ 16″. Otherwise the threads' interval would be wrong, it is 20″.

If I add those 20″ to A, 12 h 18′ 56 + 20″ = 12 h 19′ 16″, and thus A and B are correct if I take 19 min instead of 20′ with B. You will see what is right. You could also assume that the minute had passed in A and thus would be 12 h 19′ 56″ and A was the middle thread. In any case the minute must have passed in this or the other observation, because the difference of 1′ 20″ between both observations cannot happen, because the difference of the threads is only 20″ or 1 minute too big. Neither can I can guarantee the precise second because it was only a mere feeling that the star passed the thread and the error could easily be ± several seconds. Anyway, the observation was sufficient to determine the star almost to the minute so that I concluded it matched your ellipses. Dr Olbers believes it was star no. 192, but it does not match the observation because it has 185° 8′ RA and declination 11° 51′. But my heavenly body's declination was 11° 4′ or 5′.

I will not advise you on how to improve your elements, since what could I say that you would not know? Like I already said: Lead us to the goal, by all means. Please advise—do you possess La Place's Mécanique céleste*? I have ordered the second volume for you and have the first at hand. And do you have Callet's* Tables Log. Stereotype*? I will send you suggestions regarding the calculations of the perturbations for Ceres. It is enough for today, I am already late and I do not want to miss the collection. Soon more. Devoted as always*

Zach.

Observations of Ceres at Seeberg Observatory

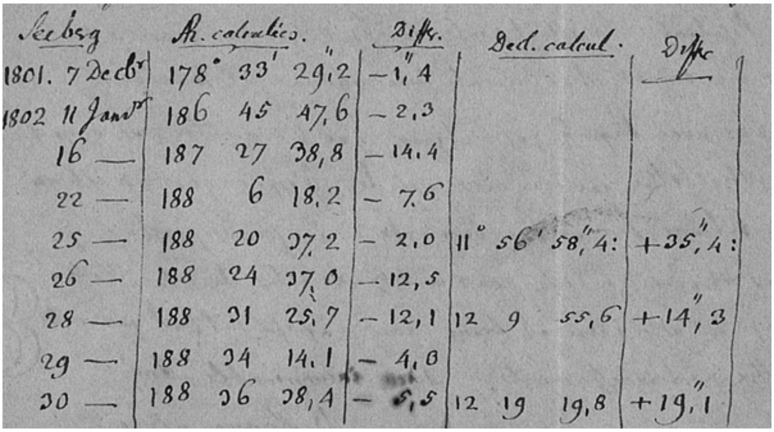
[This table is the same as the one in the February issue of the MC. He indicates that the Jan. 25 observation was made with the wall quadrant, but may be unreliable. The Jan. 28 observation is indicated as a "better observation."]

Zach to Lalande, Seeberg, February 10, 1802

I have just received your precious letter of Feb. 1. You demand presto, prestissimo the corrected elements of Piazzi's planet, the mail will leave in half an hour, so I only have enough time to satisfy your request in a haste, so that this letter arrives before you will have finished your history of the planet. Here are the latest elements, corrected to my observations and which also represent Piazzi's observations.

Epoch 1801 for Seeberg meridian...	*77° 27′ 36.″5*
Motion daily helioc. tropic mean...	*769.″7924*
Log ½ major axis	*0.4424742 (2.7699)*
Eccentricity	*0.0814064*
Aphelion (1801)	*325° 57′ 15″*
☊ (1801)	*80 58 40*
Equat. of the orbit	*9° 20′ 8″*
Inclination	*10° 37′ 56.″6*

As you can see here, my observations correspond to these elements.



Seebg	AR. calculée	Diff.	Decl. calcul.	Diffe
1801. 7 Decbr	178° 33′ 29″,2	−1″,4		
1802 11 Janvr	186 45 47,6	−2,3		
16 —	187 27 38,8	−14,4		
22 —	188 6 18,2	−7,6		
25 —	188 20 37,2	−2,0	11° 56′ 58″,4:	+35″,4:
26 —	188 24 37,0	−12,5		
28 —	188 31 25,7	−12,1	12 9 55,6	+14″,3
29 —	188 34 14,1	−4,0		
30 —	188 36 38,4	−5,5	12 19 19,8	+19″,1

Fig. 14.6 Zach's observations of Ceres from Dec. 7, 1801, to Jan. 30, 1802

I believe to have sent you all of my observations of Ceres Ferdinandea in my previous letter, here are my observations of this month (Fig. 14.6)*:*

Zach to Gauss, Seeberg, February 10, 1802

Finally, I am able to obtain very accurate declinations. I now have silver threads in my quadrant and a smaller magnification and this almost immediately resulted in a wonderful observation of the declination on Feb 9.

At 15 h 16′43″.7 RA = 188° 38′ 3″.90 Decl. 13° 14′ 18″.0 N.

I think I have sent you my previous observations; but let me repeat them anyway:

Feb. 3	*15 h 40′ 35″.8*	*RA = 188° 42′ 13″.05*	*Decl. 12° 46′ 5″*
4	*15 h 36′ 41″.4*	*RA = 188° 42′ 36″.30*	*Decl.*
5	*15 h 32′ 45″.1*	*RA = 188° 42′ 30″.15*	*Decl. 12° 50′ 25″*
(the last Decl. is disputable)			

At last some news from Paris has arrived. Méchain saw the planet only on Jan 22nd, on the 25th at 13 h 22′ he observed it at RA 188° 20′ 15″ Decl. 11° 55′ 59″. From this you can see if my questionable observation of Jan 25th is worth anything. Burckhardt observed the planet on Jan 26th at the meridian and compared it to ε Virginis—4° 40′ 7″ at RA and 0′ 20″ more southern than ε Virginis. From this I calculated the RA app. ε Virginis = 193° 4′ 56″.97 − 4° 40′ 7″ = RA of Ceres 188° 24′ 49″.97 App. Decl. ε Virginis = 12° 1′ 31″.44 − 20″S = Decl. 12 1 11.44 mt 16 h 10′ 48″.2 mean Parisian time.

In 12 days I must finish the March issue. I am longing to see a small Ephemerides for the months of March and April according to the improved elements from your hand.

Zach to Méchain, Seeberg, February 10, 1802

Here are the elements of Ceres improved according to a series of my observations which also represent Piazzi's observations of last year.

Epoch 1801 for the Seeberg Meridian	*77° 27′ 36″.5*
Mean diurnal mot. helioc. and tropical 769″.792	*log 2.4463726*
Log. semi major axis	*0.4424742 number 2.769965*

Eccentricity	*0.0814064*
Aphelion 1801 stationary	*325° 57′ 15″*
Node	*80 58 40*
Equation of the orbit	*9 20 8*
Inclination	*10 37 56.6*

My observations go with the elements as follows:[This table is the same one Zach published in the March issue of the MC.]

I believe to have sent you all of my observations; anyway here are those of this month [also published in the MC].

Zach to Méchain, Seeberg, February 20, 1802

I continue to send you my observations of the new planet, showing the desire to receive yours in exchange. I am pleased to send you those made in January, here they are:

Day of obs.	*Mean time*	*Apparent RA*	*Apparent Dec.*
Feb. 3	*15 40 35.8*	*188 42 13.05*	*12 40 5*
4	*15 36 41.4*	*188 42 36.30*	
5	*15 32 45.1*	*188 42 30.15*	*12 50 25*
9	*15 16 43.7*	*188 38 3.90*	*13 14 18*
19	*14 34 46.7*	*187 58 27.90*	*14 20 3*

[Compare this with the table published by Zach in the March issue of the MC. Some numbers differ.]

Did you notice at all that the planet is changing? At first I attributed it to the state of our atmosphere, but Mr. Schroeter and Mr. Olbers observed the same thing—that the planet's light changes from day to day. I often consider it of the 7th order of magnitude but the next day it seems to be of the 9th order. Moreover, as you know (and I think you have sent it), the planet is nebulous. Mr. Schroeter found the core diameter on January 25 = 1″.815 and that of the entire star including its nebulous atmosphere = 2″.514. This heavenly body will bring us more peculiar phenomena. While waiting we are observing and calculating flat out. Mr. Gauss has improved his elliptical elements that guided me so well, according to my observations, on my search. Here follows what I found lastly. [Zach prints here Gauss' Elements VII; see his March paper in the MC].

These elements represent all of Piazzi's observations of the last year and here are now mine: [Zach prints here his data from Dec. 7, 1801, through Feb. 3, 1802.]

These elliptical elements can already be used to calculate the secular and periodic perturbations of this planet and they are needed if you want to go back and search the planet in Mayer's and Flamsteed's catalogues, both have observed 7th magnitude stars, Mayer even 9th magnitude stars. In 1779 Messier came very close to it. If the comet of that year had touched Virgo's left wing two months earlier, planet and comet would have met and since Messier observed all stars in the neighbourhood of that comet the planet would not have escaped him. Unfortunately he was only at the doorstep of this discovery. As you have observed masses of small stars at this occasion too and during the appearance of other comets it might also be that you caught it; this merits the troubles of the search, because this find would be very precious to establish the mean motion, which would take quite some time without. There are many small stars I observed to set up my catalogue of zodiacal stars that today are missing in the sky and I will see if I can find Ceres or if they *are errors caused by copying or calculating. Ceres was once favourable to me and she could be so a second time.*

Here is an ephemeris of Ceres' motions for the month of March according to the below elements. I hope they agree well enough to find it again. [This is the same table as the one in the Zach-Banks letter of Feb. 20].

On March 17 the planet will be at its opposition. Slightly before that it will be at its closest to earth = 1.6025. At this time it will also be at its greatest geocentric latitude = 17° 9′ and its greatest movement retrograde at 13 min RA per day.

I rather preferred to talk (in this letter) about the new planet because I am certain you will provide us with good observations. I am closing my letter in haste in order not to miss the collection, assuring you that you know better than I can express, you are in my heart and soul.

Zach to Gauss, Seeberg, February 21, 1802

Do I owe you an answer to your dearest letters of the 7th and 15th? I was hoping every single day to be able to send you some observations of Ceres, but this month has been most unfortunate and from the 9th till the 19th I did not see one inch of blue sky! But on the 19th I was able to make an excellent observation and obtained a fantastic RA and declination. I think I have sent you my complete observations of February already, but here they are again [This is the same table as printed in the Zach-Méchain letter of Feb. 20.]

I received your elements VII, the comparison with my observations and the small ephemerides for March and I would like to express my gratitude. You will find all this in the M.C. and soon I will have the honour to send you special prints of the March issue. Maybe I will be able to obtain some observations until then although it is most unlikely. This rather bad weather happens at an inopportune moment. Jupiter and Saturn are at opposition. Luckily, I got Jupiter the day of its opposition on Feb. 19th and the day after tomorrow Saturn is at opposition. The Thuringian climate is sorely testing my patience. If I did not live in my instruments, so to speak, and profited from each and every hole in the clouds, I would not be able to do anything at all. But let us return to Ceres. I agree with you to carry on with the comparison of the observations and your elements VII. For a long time, or even till its invisibility, there is no danger that we could lose Ceres. I believe your elements VII present it accurately, except several minutes, for the next six months. In adapting those elements more and more to the observations you do not get better elements because this planet must suffer severe perturbations, thus they cannot be purely elliptical. Of course we have to make sure we get mean elliptical elements before we begin calculating the perturbations. But for this we have to await the planet's first opposition which I am planning to observe very carefully and I want to give you the positions of the Sun most thoroughly observed. Then we certainly will obtain a pure heliocentric position of Ceres. Since then you can content yourself with your elements VII; but I, esteemed doctor, would like to suggest a different thing, which is rather urgent.

Now that we have caught Ceres it must be in our interest to observe it as long as possible. Until mid-April this might be possible in the meridian but then dusk will impede this best way of observing and then we will have to manage with equatorial or parallactic instruments or telescopes with micrometer hairs etc… These kinds of observations are not very exact because the observer does not always have well determined stars on the parallel of the planet, which he uses for a comparison so he has to take the star that is there, and you know for yourself how badly those small stars of 7th, 8th, or 9th order of magnitude are determined for this purpose. My idea is the following: You calculate Ceres' apparent geocentric path till it is lost in the rays of the Sun. I will map all those small stars that will be in Ceres' neighbourhood and determine them now, while I still can, very accurately; they still culminate at this time of year in dark night. And I have to hurry before the days get longer. (I should have thought of it earlier, but the better thoughts always come after the *feast). When I will have finished my little star catalogue it will be quite easy for me and all other astronomers who will be observing Ceres in the western sky in April, May and June at parallactic instruments or telescopes to obtain good observations, and we will get quite a bit of its path till its future reappearance.*

The mean motion of the planet is of course a thing, which will make difficulties, if we are not so lucky to find Ceres in older star catalogues. La Lande writes, he hoped to find it among his 50,000 stars—and not only once. That would be excellent! I gave him your elements VII for finding Ceres. I myself hope to find Ceres in my zodiacal catalogue. I observed several small stars between 1788 and 1800 which are now missing in the skies; we will see whether Ceres is among them or if those disappearances—or better, the existence of those stars at certain positions—are errors in observations, writing or calculations, as it has been so often the case with Tob. Mayer, Flamsteed and others. Dr Olbers has most likely told you that Messier in the year 1799 in the case of the comet of that year was close to Ceres. I do hope that we will find Ceres somewhere, even in Flamsteed it is not impossible as he observed stars of the 8th magnitude. But if we go back with your elements this far it would be very uncertain, partly because of the mean motion and partly because of the secular and recurring variations of Ceres' motion. This planet surely suffers immense perturbations from Jupiter, those of Mars will probably be considerable again; but I believe Venus and Saturn do not affect it.

I do not expect inhomogeneities of long periods in the case of Ceres, due to the big incommensurability of its mean motion with that of Jupiter and Mars. Meanwhile the secular equations must be considerable and even the periodic ones most likely account to many seconds and we definitely will have to take into account the squares and products of the eccentricities in their calculation. But we can only talk of this after the opposition. La Place agrees and believes that the perturbations have to be taken into account before we can obtain the true elliptical elements, but it is still too early. He is also writing in this letter that he had talked with Bonaparte about this new planet, who rather wished to call it Juno than Ceres, and he is adding: "and I agree with him on this." Of course you are not allowed to have your own opinion in France than that of the highest consul; yes, even in Germany you have to comply sometimes; meanwhile we want to continue to call our planet Ceres, until Juno has officially deserted the peace congress at Amiens and has been accepted in our Reichstag. [The Peace of Amiens was signed on March 25, 1802, ending hostilities between England and France.] *La Lande really wants to call the planet Piazzi. The March issue of the M.C. speaks of this and La Lande's language has been modified by me—he was so angry and upset by the names. He wrote: "Uranus and Ceres are only stupidities;" he absolutely wants Herschel and Piazzi.* It is not for us to settle such a quarrel *[Virgil's Ecloga III] If they only give us the planets, they can argue about the name as much as they want to, I will remain passive and take no part in this stupidity.*

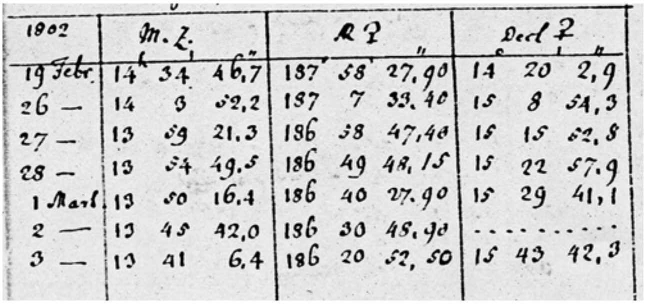
Buerg to David, Seeberg, February 23, 1802

I heard from Mr. von Zach that you did not find Ceres. We can see her now without any difficulties with the quadrant. I think you will not miss her for her light has increased considerably compared to the beginning when even Mr. von Zach sometimes could not find her with his quadrant when at the same time I was observing her with a meridian telescope.

Zach to Oriani, Seeberg, February 25, 1802

At last, my dear friend and illustrious colleague, I have at least learned what has become of you. I believed you to be on the new planet, Ceres. Your letter from Milan of the 10th of February, which I received yesterday, tells me that you were in Lyon.

You ask me eagerly for my observations of the planet Ceres, my dear friend. Here they are in A and in B you will find the elements with which all goes so well. In C are the ephem-



1802	M. Z.			AR ⚳			Decl ⚳		
	h	'	"	°	'	"	°	'	"
19 Febr.	14	34	46,7	187	58	27,90	14	20	2,9
26 —	14	3	52,2	187	7	33,40	15	8	54,3
27 —	13	59	21,3	186	58	47,40	15	15	52,8
28 —	13	54	49,5	186	49	48,15	15	22	57,9
1 Mart	13	50	16,4	186	40	27,90	15	29	41,1
2 —	13	45	42,0	186	30	45,90	. . .	. . .	. . .
3 —	13	41	6,4	186	20	52,50	15	43	42,3

Fig. 14.7 Zach's observations of Ceres from Feb. 19 to Mar. 3, 1802

eris which will hardly differ a minute until April. Thus, they will serve to find the planet again easily. In D are the observations of Dr. Olbers; I give them to you "curiositatis gratis" but they are hardly exact, having been made with a telescope with a circular micrometer, especially for the declinations. I beg you to communicate all this to Mr. Piazzi. Senator Laplace writes me that Bonaparte would like the new planet to be called "Junon." Lalande wants to call it "Piazzi." As for me, I will continue to call it Ceres while begging Mr. Piazzi to dispense with "Ferdinandea," which is a bit long.

Tables sent separately:

A: Observations made at Seeberg Observatory and compared to the above Elements

B: Average elliptical elements of Ceres' orbit from Mr. Gauss compared to my observations above, and which represent at about the same time the observations from Palermo of last year, made by Mr. Piazzi

C: Ephemeris on the position of Ceres, for midnight at the meridian of Seeberg.

D: Observations of Ceres from Dr. Olbers made in Bremen with a circular diaphragm.

Lalande to Oriani, Paris, March 4, 1802

Mr. Burckhardt has calculated the perturbations of Piazzi's planet which exceed 30 min and he derived elliptical elements. [These elements are in the letter from Zach to Gauss, March 20, 1802.]

Zach to Gauss, Seeberg, March 7, 1802

Your Honour,

will probably have already seen from the March issue that the continuation of your ephemerides came in time to be included. Here are my continued observations of Ceres (Fig. 14.7)*:*

At last, the French have handed over their observations. It is quite funny to see how they act and want us to think that they had observed Ceres earlier, if they had had the large telescope, if the weather had been fine, if one had looked more to the right, the other more to the left, the other higher or the fourth further down! But all this is according to the French proverb: "Without the ifs and buts Paris would be in trouble." All the while they are arguing who was the first Frenchman who saw Ceres. Méchain can take credit for being the first to observe the planet on Jan 24. He wrote: "he had cried hard in front of the premier consul." In the presence of the minister for internal affairs he had "cried like an eagle" and had told him "They will say that all astronomers of Paris with their large telescope that is not finished are always outdone and preceded by strangers, so quickly, give us all the money needed to get that telescope."

Here are Méchain's observations of the Observatoire National: [This table was printed in the MC, April 1802.]

Sir Joseph Banks writes from London that only on February 3rd they had started to observe the planet. All astronomers were working on it. Herschel studied his diameter and satellites. Maskelyne's observations are as follows:

Febr 3	*16 h 11*	*RA Ceres m time*	*12 h 34′ 50″.8 Decl*	*12° 37′ 23″*
4	*17 h 26*		*12 34 52.5*	*12 44 7*

Your elements are still in perfect correspondence with the sky. On Dr Olbers' advice I will publish in the April issue a map of Ceres' path as long as it will be visible, where I will chart all sorts of small stars, and I have already started their determination. Thus I am longing for your ephemerides until May and mid June. So much for today in a hurry, soon more about the instruments, the artificial horizon was finished yesterday.

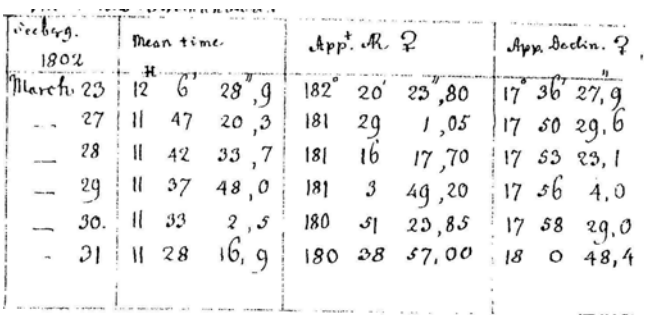
PS: Oriani was in Lyon as delegate; upon his return home, he wrote, that Reggio [Francesco Reggio, 1743–1804], *the only astronomer who had stayed at home, had not even looked for the planet, because he did not believe in it. Now I am expecting observations from Milan and Palermo every day, to where I have sent your ephemerides. Last night, on the night of March 6th, I had Ceres for another time, but did not reduce the observation. I will send it the next time.*

Oriani to Zach, Milan, March 17, 1802

The first time, my estimable friend, when the clouds and fogs let me see the sky, since my return from Lyon, that means February 24, I saw Ceres but without recognising it because bad weather set in and I could only see it again on March 10, 11 and 13. My colleagues saw it on the 11th after my indication and I believe it is still unknown within the rest of Italy. The elliptical orbit calculated by Dr. Gauss gives its position so close to the true one that I am surprised it is still being misread. It appears as a star of the 6th or 7th order of magnitude. I tried to illuminate the hairs of the micrometer, and I was able to observe it further. Unfortunately the sky was overcast after the 13th and only today could I see it again. I calculated the perturbations of the other planets on Ceres and I am waiting for its opposition to rectify better the orbital elements.

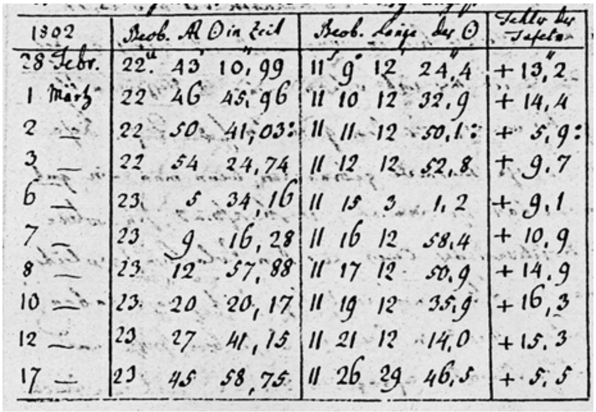
Zach to Gauss, Seeberg, March 20, 1802

First of all I would like to express my gratitude for your letters of March 4th and 14th. But before I start to answer all those interesting items, I would like to take out my Ceres. I wanted to await the planet's opposition and I succeeded in obtaining an excellent observation in the most wonderful weather. Here are all my observations of March (Fig. 14.8)*:*



Seeberg. 1802	Mean time.	App. R ⚳	App. Declin. ⚳
March 23	12 6′ 28″,9	182° 20′ 23″,80	17° 36′ 27,9
— 27	11 47 20,3	181 29 1,05	17 50 29,6
— 28	11 42 33,7	181 16 17,70	17 53 23,1
— 29	11 37 48,0	181 3 49,20	17 56 4,0
— 30.	11 33 2,5	180 51 23,85	17 58 29,0
- 31	11 28 16,9	180 38 57,00	18 0 48,4

Fig. 14.8 Zach's observations of Ceres in Mar. 1802



1802	Beob. AR ☉ in Zeit	Beob. Länge des ☉	Fehler der Tafeln
28 Febr.	22 43 10,99	11 9 12 24,4	+13,2
1 März	22 46 45,96	11 10 12 32,9	+14,4
2	22 50 41,03:	11 11 12 50,1:	+5,9:
3	22 54 24,74	11 12 12 52,8	+9,7
6	23 5 34,16	11 15 3 1,2	+9,1
7	23 9 16,28	11 16 12 58,4	+10,9
8	23 12 57,88	11 17 12 50,9	+14,9
10	23 20 20,17	11 19 12 35,9	+16,3
12	23 27 41,15	11 21 12 14,0	+15,3
17	23 45 58,75	11 26 29 46,5	+5,5

Fig. 14.9 Zach's observations of the Sun

In order that you get the heliocentric position of the planet as purely as possible, here are my observations of the Sun with the errors of my improved tables (Fig. 14.9).

1802 Obs. RA Sun in time Obs. longitude Sun error of tables

The error is that of my improved solar tables, it can certainly be assumed for the entire period the mean of +11°, by this my solar longitudes are too short.

I believe I have sent you Méchain's Parisian observations from January 24th until February 10th. Since then I received the news (March 1st) that they had bad weather for 20 days and did not observe the planet. No news from Milan and Palermo, but most likely they are observing the planet by now and I am expecting good observations from there. And at last, Triesnecker in Vienna found Ceres after many unsuccessful attempts on March 3rd, here are his positions as he sent them.

		Apparent RA Ceres	*Decl.*
March 3	*13 41′ 11″ mt*	*186° 20′ 59″.3*	*15° 43′ 44″.8*
″ 4	*13 36 35*	*186 10 58.0*	*15 50 34. 8*
″ 7	*13 22 39*	*185 39 10.7*	*16 9 25.6*

The first observation is good, the last is not much use. The reason is that Triesnecker used single positions of stars, with which he compared the planet. He used no. 87, 111, 187 Virgo of Bode's last star catalogue of his sky maps; I have determined these stars anew and found for instance an error of –42″.2 in RA and +34″.4 in Decl. for no 87 Virgo. With this correction the observation corresponds much better. I had expected exactly this and very often, with all astronomers who make differential observation and have to compare the planet with a star in the parallel, what even Maskelyne and Méchain did, who did not need to. Until now I am the only one who made immediate meridian observations of Ceres. I do not care about all those small stars that appear in the parallel of Ceres, I only observe them because of other astronomers, for if I did not, their observations would be useless. In May I will have to use this method myself, and that is the reason why I am already now making a star catalogue, since you were so kind to send me the geocentric path of Ceres for these months; but to employ that method when there is an excellent passage instrument is pure barbarism. I wanted to deduce Ceres' positions from the polar star. March 15th has probably embarrassed many an astronomer for very close to the planet there was a star of equal *size and look. At first I was startled too, but fortunately I had 20″ to think and I got the planet. My friend Buerg at the quadrant was not so lucky, he took the star for Ceres—that is the reason for the disputable declination of that day. The star was no. 147 Virgo in Bode. And yesterday again the planet came close to a very small star that can be found in every catalogue, it was of the 9th order of magnitude and I determined its apparent RA 183° 14′ 43″.6 Declination 17′ 21″ 5″.0. A good map and a new star catalogue of the areas where Ceres roams is badly needed, otherwise a lot of nonsense is produced, as you can see. The copper engraver left me in the lurch with my April issue and I will only be able to publish the map in the May issue. It will be more detailed and anyway, I received only the day before yesterday Ceres' path until May and June, which you were so kind to calculate, but will it correspond until then so nicely as it does now? La Place wrote that the sum of all perturbations approximates 30 minutes. The third volume of his* Mécanique célèste *has been sent to press, which contained the theory of perturbations for the Moon and the planets, that of Ceres is planned for another volume.*

And Burckhardt writes that he had started to calculate the perturbations for Ceres, and found that the perturbations of Jupiter amount to 27 minutes, but that he had neglected those dependent on the square of the eccentricities. Regarding the latitudes they amount to 1 1/2 minutes. Mars is not important, neither is Saturn. The equations for long periods can be several seconds, but are not yet to be determined because we do not know the mean motion of Ceres. The annual motion of the aphelion is +2″.5. That of the ascending node is very small. Burckhardt has already calculated new elements after having applied the perturbation equations, here they are:

Epoch 1801 for Paris	*77° 19′ 17″*
Aphelion 1801	*326 42 32*
Ascending node	*81 5 35*
Inclination	*10 36 52*
Semi major axis	*2.76587*
Eccentricity	*0.0788725*
Trop. Revolution	*1679.84 days*

According to Burckhardt they are not very exact but present four observations very well, which are far apart, to a few seconds. He did not send me his equations, but promises to send the tables soon. La Lande has sent an observation, which was made by his nephew on February 27th on the Champs de Mars. 13 h 59′ 15″4. m.t. RA 186° 58′ 44″.1 Decl. 15° 15′ 54″.8. Matches my observation well. It appears the French are ploughing with other calves than their own. They calculated with your elements the perturbations and with my observations the tables; and I bet they mention neither—à la manière francaise [in the French manner]. *The one will cry in the presence of Bonaparte, the other will cry like an eagle at the minister's but neither will tell the truth. Forget it, if they only did not distort it! This minute I received a copy of no. 15 of the* Décade philos[ophique]. Litt[eraire].et polit[ique]., *where I found on page 375 an extract of a letter from Mr Zach to Méchain… "…elliptical elements of the new planet, improved by Mr Gauss according to the most recent observations: trop. motion heliocentr. 770″.7376 Tropical revolution 1681 daily 12 h 9′."* [the same letter was printed in England in A Journal of Natural Philosophy, 1802, 318] *That's it. And nothing else. How disgraceful! Are those your elliptical elements? Did I only write this and nothing else to Méchain? Are we Germans even too stupid to know what elliptical elements are; or are the French responsible for this stupidity? If they publish their elements, they will also print only daily motion and tropical revolution? I have sent all of your parts of the path, I wrote to Méchain "improved according to my recent observations" and not "according to the recent observations." The editor of the Décade does not understand such astronomical nuances, those could only have been made by a Frenchman and professional astronomer,* *who wants to cry his eyes out in front of Bonaparte like an old whore because he was not the first to rediscover the planet. It is incredible how ridiculously these citizens behave: they consider it a disgrace that has been brought over the grand nation that they neither observed nor calculated the planet first. But still they make a great fuss, stretch their backs, speak in a high voice: "let us do it, we arrange all this." La Lande really wrote: "soon* <u>*we*</u> *will have all satisfaction. And the name Juno is being used. The senator La Place uses it exclusively." Méchain plays the diplomat and is still manoeuvring. He neither writes Juno nor Ceres, but only "the new planet"; it is ridiculous to see how anxiously and* <u>*world-wisely*</u> *he tries to avoid the* nomen proprium *[proper name]. La Lande who is French, too, with all his heart but still a respectable and honest soul with his own head, is different as he writes: "To me, it will always be Piazzi and nothing else, if someone wants to steal his treasure, I do not want to be part of this injustice." That is great! But incompatible with the court and an affront to Bonaparte, who calls him (Lalande) his grandpa.* [d'Agelet, the unlucky astronomer who died with La Perouse was Bonaparte's teacher for mathematics at the Ecole militaire, and d'Agelet, as it is well-known, was a student of La Lande, that is the reason for the grandfatherhood.]

I told Méchain, who usually is a good soul, very crudely my opinion on the extract of my letter. I like the Frenchmen, they have a head and de l'esprit *(what does not always mean common sense) but the heart is oftentimes worth nothing, especially among the inhabitants of Paris. I appreciate their knowledge and deeds and have to if I do not want to be injust, but under certain circumstances they are beyond all bearing and like Molière once said: "We and our friends, we alone have esprit." The word bète (fool) is fast spoken and also the phrase "a big German, a heavy German!" That is in the language of "A German war" and is a very old proverb and in the entire Adelung there is no translation.* [Zach is referring here to the German dictionary by Johann Adelung, 1732–1806.]

Sniadecki to Zach, Cracow, March 22, 1802

By your indefatigable zeal you worked so much on the astronomers in your journal that you almost forced them to search for Piazzi's Star. I searched for it in vain at the end of November and during December, from β Leo, to η Virgo, examining that region of the sky with the greatest attention but without any success. In January and at the beginning of February almost each day was foggy. About the end of February I received your journal and the prints you were so kind to send me in a letter. And having learned that this pygmy planet does not tolerate any illumination of the hairs in the telescope, on February 28 the light close to the telescope was almost out, I saw in my passage instrument a very tiny star below the 9th order of magnitude whose position I noted; on March 1 I convinced myself that this was the Star of Piazzi, it had changed position and slightly

	Temps moy[en] [à] Cra[covie]	A[scensio] R[ecta] Ceres	Décl[inaison] bor[éale]	Étoiles de comparaison
1802, mars 1	13h 50′ 24″	186° 40′ 17″	16° 30′	
2	13 45 30	186 31 30	16 36 43″,8	167 ♌ 304 ♌ 407 ♌
15	12 44 53	184 2 56 ,7	16 57 34	107 ♌ 147 ♍
16	12 40 8	183 50 10	. . . .	269. 141,69. 107 ♌
17	12 35 22	183 37 34 ,5	17 9 8	269 10,69 190 ♌ 147 ♍
19	12 25 47	183 12 27	17 18 53	125 ♌ 190 ♌ 476
20	12 21 1	182 59 14 ,3	17 23 47	12 ♌ 84 ♌ 125 ♌ 190 ♌

Fig. 14.10 Sniadecki's observations of Ceres in Mar. 1802

increased in brightness. From a suspended lamp at a certain distance behind me I let fall a soft light on the reflector of my meridian telescope; this brought a very slight illumination of the hairs without decreasing the light of the star, but this measurement became useless all the same since the planet appeared as a 7th magnitude star. During my last observation I was at least able to see it perfectly clear and very distinct with the normal illumination of the hair of the micrometer. Here are my observations that the weather allowed me (Fig. 14.10)*:*

Mean Time in Cracow RA Ceres Declination n. Comparison star

The declination of the first was only observed at the semi-circle of my meridian telescope. On the 16th the passage through the meridian of the planet 2′ in time before the emersion of Saturn at the lighted edge of the moon, my vision was so hindered by the brightness of the light of the moon that it was impossible to seize the planet with the hair of the micrometer of my quadrant. And furthermore I rejected this observation as suspicious while guaranteeing the exactness of the others, where I used all my carefulness and attention. I wanted to defer the communications of these observations until the opposition of the planet in order to send you also the calculation of longitude and latitude but the sky got overcast and I could not do anything else that night. If however the weather is fine I will not miss to follow this pygmy planet and have the pleasure to tell you everything I will do.

Zach to David, Seeberg, March 29, 1802

I was extremely pleased to learn that you had found Ceres at last. But unfortunately you have come to the wrong hands and the stars you took from Bode are determined extremely inaccurately; the same thing happened to Triesnecker in Vienna. If you had sent your data to me I could have reduced your observations; but I was unable to complete them since you had sent me only half of them. Thus, I could not use your observations in my MC, April issue. I mentioned, however, that you had found and observed Ceres.

On March 15th you observed no 147 (Virgo) Bode. But you must know, dearest friend, this star is badly determined in Bode and you can by no means cope with it. According to me on March 15th

The RA app. Ceres	*184° 2′ 52.0″*
Decl.	*16° 58′ 30.9″*

But if you ask me to, I will help you out. According to me is

No 147 RA 1801	*184° 1′ 14″.7*	*Decl. 16° 58′ 7″.6*
According to Bode	*183 59 36.0*	*16 57 7.0*
Error	*+1′ 38″.7*	*+1 0″.6*

I had the star the very same night, namely March 15th and if you would like to use my apparent position you will probably find something reasonable; here is my position:

1802, March 15 apparent RA No. 147	*184° 0′ 58″.8*
Apparent Dec	*16 57 33.9*

On March 19th you used no 476. Equally bad! Want to help you once again and lead you out of the hole into which Bode has led you. According to me

	David in Prag	Zach in Seeberg	Differ.	[Motus diurnus
Den 19. März AR Cer. app.	183° 11′ 59,60″	183° 11′ 58,6″	—1,0″	12′ 52,1″
Diff. AR 51″ t. m.	+12 47,09			
Den 18. März AR Cer. ...	183° 24′ 46,69″	183° 24 50,7″	+4,0″	12′ 44,1″
Diff. AR 49″ t. m.	+12 17,01			
Den 17. März AR Cer. ...	183° 37′ 3,70″	183° 37′ 34,8″	+31,1″	12′ 44,2″]*)
Diff. AR 47″ t. m.	+11 46,93			
Den 16. März AR Cer. ...	183° 48′ 50,63″	183° 50′ 19,0″	+1′ 28,4″	

Fig. 14.11 The difference between Zach's and David's observations of Ceres in Mar. 1802

No 476 RA 1801	*174° 44′ 15″.5*	*Decl. 17° 21′ 14″.1*
According to Bode	*174 44 33.0*	*17 20 56.0*
Error	*–17″.5*	*+18″.1*

If you want to use my apparent positions, they were on March 19th:

1802, March 19, appar RA No. 476	*174° 45′ 36″.7*
Appar Dec.	*17 20 42.5*

Now let's see what we can do with your observations. Actually you sent me just one reasonable and useful one; namely you write, RA appar. differ. of Ceres with 476 Ω in mean time 33′ 40″.

Now this is	*8° 26′ 22″.9*
My apparent RA of no 476	*174 45 36.7*
RA appar. Ceres……	*183 11 59.6 David in Prague*
RA appar. Ceres…	*183 11 58.6 Zach in Seeberg*
Differ	*1″.0*

That corresponds magnificently! Gratulator, gratulator; but now comes the snag (Fig. 14.11)*:*

The differences are useless. I am asking you therefore to send me the decl.—differ. By the way, if you intend to send me any observations in the future, please send all data, so I am able to reduce the observations myself, compare them to my own und thus use them. That way, everything is too late. Soon more of that. This obviously was in a great hurry. All of your local friends send you their kindest regards, but I remain with my steadfast attitude, known to you—yours, Zach.

Zach to Oriani, Seeberg, April 27, 1802

I have sent you the perturbation formulae of Wurm. He has written me since and improved and reformed some, of which I must inform you, always supposing that you are interested. If I am mistaken, too bad for both of us: I am wasting my time with writing you and you with reading my stuff. But I have my reasons which I would like to tell you another time, why I would like you to give us Ceres' perturbations soon, since there is a boutique in this base world which thinks that it is the only one that can practise this trade and since I know that this is not true, and that in general I lower every monopoly, each exclusive privi-

lege, I would like to give this boutique a dementi where they would like to sell us these things, like Orvietan, making mysteries and secrets. But my God, in this base world the sun shines for everyone! Please hurry, my good friend, and send me very quick your Orvietan, so that you too can say anch'io sono apothecario!* [This curious passage refers to Zach's hatred of the French attitude towards the calculation of the perturbations of Ceres. The "boutique" he is referring to is the small group of French astronomers, led by Lalande, who think they are the only ones who can do the work. Zach does not believe they have a monopoly on celestial mechanics, and is urging Oriani to beat them to it by publishing first.]

I am sending you here the continuation of my observations of Ceres and some of Pallas as well, of which we already know what to do with. These observations do not fit into an circular orbit, Olbers believes it must be an ellipse. This star appears to me as decreasing in brightness but it always has a very distinct appearance and no nebulosity at all. Have you found and observed this singular body? But I informed you on time.

* Orvietan or Venice treacle, once believed to be a sovereign remedy against poison. From Orvieto, a city of Italy, where it is said to have been first used. See New Scientist no. 2640, Jan. 26, 2008 issue, pg 52.

Cossali* to Oriani, San Vincenzo (Piacenza), April 29, 1802

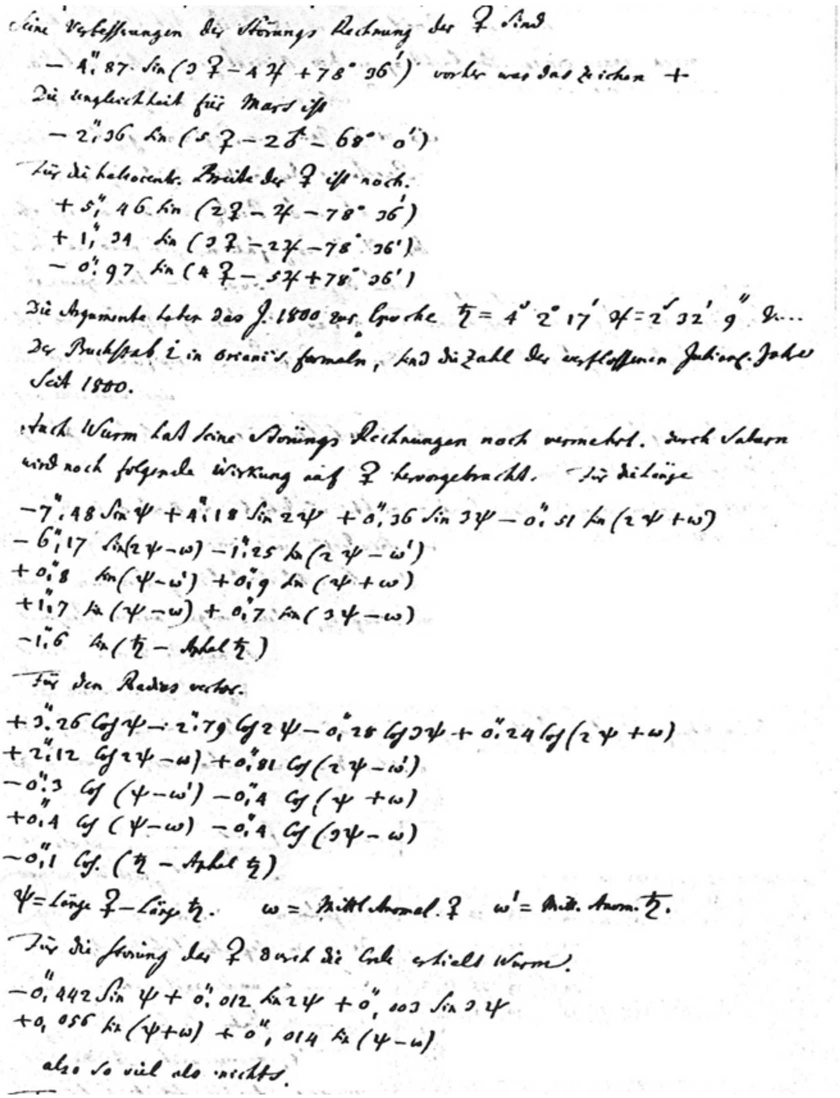
I have to beg a new favour, for myself and also a stable of amateurs of astronomy under the management of the clever and very good abbe' Veneziani. Having here a parallactic machine we wish to find Piazzi's planet. Hence it would be useful if you could observe Saturday night the RA and Decl. of the planet and write me promptly together with its approximate daily motions and directions. So that when your letter arrives Sunday morning we could prepare the machine and Sunday night try to see it.

*Pietro Cossali (1748–1815) was a Theatine abbot who showed a precocious genius for mathematical research. In 1786 Ferdinando of Borbone gave him a teaching post for experimental Physics at Parma University, then transformed in to Astronomy, Meteorology and Hydraulic. He was well known for the fundamental book about Italian history of mathematics. In 1806 he moved to Padua university where he taught "sublime calculus".

Sniadecki to Zach, Cracow, May 24, 1802

I have just received observations for you made at Vilnius with the best meridian telescope of 5.5 feet and of 4 inches of aperture and a mural quadrant made by Ramsden with 8 feet of radius. Mr. Poczubut has addressed them to you and signed them and without having touched them—it is my pleasure to send those to you. This amiable old man who lives for astronomy only, aroused my pity with his letter in which he complains about his deteriorating strength which does not allow him to participate in observing with such an assiduity that equals his zeal. While constantly and with great determination searching for Ceres using my observations and then those Mr. Bode sent him, frustrated by the bad weather, he has succumbed on several occasions until the disappearance. His letter is dated Vilnius, 8th May. He has not yet found Pallas although I sent him your three or four first observations and, eight days later, mine. It seems to me he will not find it any more. I saw it in the meridian on the 12th for the last time. Since then the sky has been overcast, rainy and cold. The Reaumur thermometer does not pass +6° and at the end of April it was already at 20°; it has heavily snowed in the mountains and here at the foot of the Carpathian Mountains the effects can be felt. One single evening was clear but very cold. I searched for Pallas beyond the meridian and without having seen it, I caught an infection of the throat and lungs which confines me to my room. My lungs have been ruined by teaching for 24 years and unfortunately I am very susceptible to illnesses that sometimes go as far as haemoptysis.

But let's get back to the observations from Vilnius. It seems to me that Mr. Poczubut, in deducing two declinations of Ceres of the meridian distance to the zenith did not take the refraction into account—I am judging according to other observations he has sent me sev-


eine Verbesserungen der Störungs Rechnung der ⚳ sind
− 4,"87 sin(3 ⚳ − 4 ♃ + 78° 36′) vorher war das Zeichen +
Die Ungleichheit für Mars ist
− 2,"36 sin (5 ⚳ − 2 ♂ − 68° 0′)
Für die heliocentr. Breite der ⚳ ist noch.
+ 5,"46 sin (2 ⚳ − ♃ − 78° 36′)
+ 1,"34 sin (3 ⚳ − 2 ♃ − 78° 36′)
− 0,"97 sin (4 ⚳ − 5 ♃ + 78° 36′)
Die Argumente haben das J. 1800 zur Epoche ♄ = 4ˢ 2° 17′ ♃ = 2ˢ 32′ 9″ &c...
Der Buchstab i in Oriani's formeln, sind die Zahl der verflossenen Julianischen Jahre seit 1800.

Auch Wurm hat seine Störungs Rechnungen noch vermehrt. durch Saturn wird noch folgende Wirkung auf ⚳ hervorgebracht. Für die Länge
− 7,"48 sin ψ + 4,"18 sin 2ψ + 0,"36 sin 3ψ − 0,"51 sin (2ψ + ω)
− 6,"17 sin(2ψ − ω) − 1,"25 sin(2ψ − ω′)
+ 0,"8 sin(ψ − ω′) + 0,"9 sin(ψ + ω)
+ 1,"7 sin(ψ − ω) + 0,"7 sin(3ψ − ω)
− 1,"6 sin(♄ − Aphel ♄)
Für den Radius vector.
+ 3,"26 Cos ψ − 2,"79 Cos 2ψ − 0,"28 Cos 3ψ + 0,"24 Cos(2ψ + ω)
+ 2,"12 Cos(2ψ − ω) + 0,"81 Cos(2ψ − ω′)
− 0,"3 Cos(ψ − ω′) − 0,"4 Cos(ψ + ω)
+ 0,"4 Cos(ψ − ω) − 0,"4 Cos(3ψ − ω)
− 0,"1 Cos. (♄ − Aphel ♄)
ψ = Länge ⚳ − Länge ♄. ω = Mittl. Anomal. ⚳ ω′ = Mittl. Anom. ♄.
Für die Störung der ⚳ durch die Erde erhielt Wurm.
− 0,"442 Sin ψ + 0,"012 sin 2ψ + 0,"003 sin 3ψ
+ 0,"056 sin(ψ + ω) + 0,"014 sin(ψ − ω)
also so viel als nichts.

Fig. 14.12 The first page of Zach's letter to Gauss

eral times in the same state. If my supposition is correct his results do not differ much from yours and mine. He must have been very busy when he wrote to me for he does not give any details of the observations that he usually does. To clear up this mystery I will write him and ask for the state of barometer and thermometer during the observations that he usually never fails to note down. You will know his answer without delay but because of the appalling arrangements of the posts it sometimes takes one month to get a response from Vilnius. While waiting you might want to use his observations in the way he sent them, it will then be easy for astronomers to reduce and improve them. He loves to write Latin verses and *sometimes quite good ones. You will find at the beginning of his observations two Latin verses about the distinctive character of Ceres. And something about Pallas:*

Falx Cereris signum esto; tu ut tueare labores

Sideribus sacros, aegida Pallas habe.

Before having improved my observations of Ceres I redid the calculation of its opposition and here are the results. (Here I put the calculation of the opposition of Ceres Sun, like I redid it recently and sent it to Vienna.)

Finally, I would like to say a few words regarding the fact that my observations of Ceres cannot always be reconciled with yours. In March I compared the planet to a lot of stars in Leo and Virgo of which I only mentioned a few. In the calculation of the observation there were stars that gave me very inhomogeneous results. I regarded them as suspicious and rejected them. Your letters convinced me that I was right. If I was upset by the bad weather that did not permit me to make a large number of meridian observations I confined myself to a small number of stars and their errors in the positions which influence my results the most, which are those you said. Generally, in March before knowing of Bode's errors there were stars that I used only for RA and others only for the declination and still others for this or that element, thus it was necessary to have my entire calculation at hand in order to duly make corrections.

Zach to Gauss, Seeberg, May 30, 1802

[This mathematical section is reproduced here; translation of relevant passages into English follows (Fig. 14.12).]

His improvements of the calculation of Ceres' perturbations are $-4''.87$ *sin (3 Ceres—4 Jupiter* $+ 78° 96'$*), the sign was +*

Mars' inequality is

1 line

For the helioc. latitude of Ceres:

[3 lines]

The arguments are based on the epoch 1800

The letter i in Oriani's formulae is the number of Julian years gone by since 1800.

And Wurm, too, has multiplied his calculations of the perturbations. He detected the following effect of Saturn on Ceres. For the longitude

[5 lines]

For the radius vector

[6 lines]

ψ *= longitude Ceres – longitude Saturn,* ω *= mean anomaly Ceres,* ω' *= mean anomaly Saturn*

For Ceres' perturbations by the Earth Wurm's result is:

[2 lines]

thus almost nothing.

For Mars he neither finds anything; that Oriani found $2''.3$ *is because he used a too large mass for Mars; Wurm gives it 1/3833869 of the solar mass. In the June issue there will be a very interesting article by Wurm about the masses of the planets.*

Calculations of Ceres' perturbations by Oriani, Milan, obtained by another hypothesis than that of the mean distance.

D *= mean longitude of Ceres – mean longitude of Jupiter.*

A' *= mean anomaly of Jupiter*

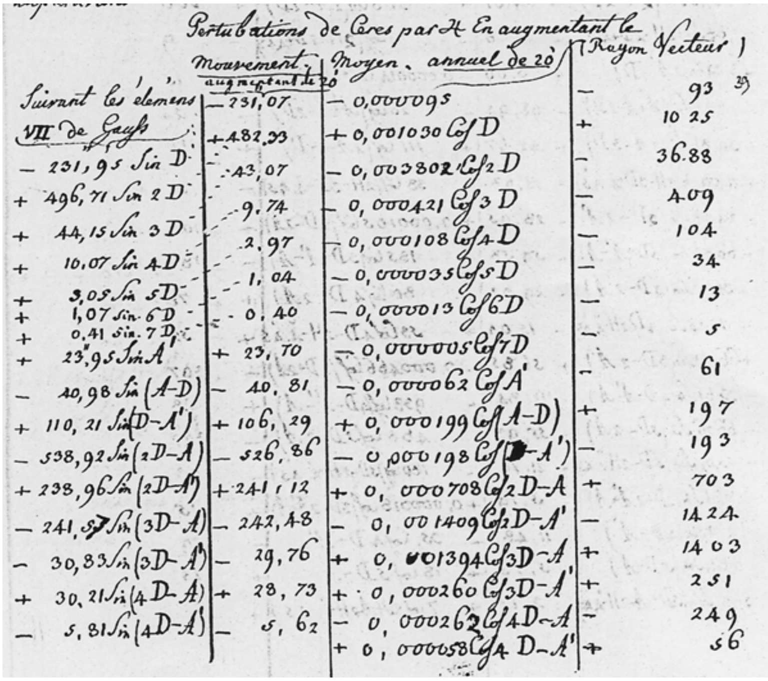
A *= mean anomaly of Ceres*

H' = mean longitude of Jupiter – mean longitude ascending node of Jupiter

H *= mean longitude of Ceres – mean longitude ascending node of Ceres.*

It has to be said that the perturbations have been obtained with a different eccentricity = e in multiplying the terms that contain A with e/0.081406

those containing 2 A with (e/0.081406)2



Perturbations de Ceres par ♃ En augmentant le Mouvement Moyen annuel de 20' (Rayon Vecteur)

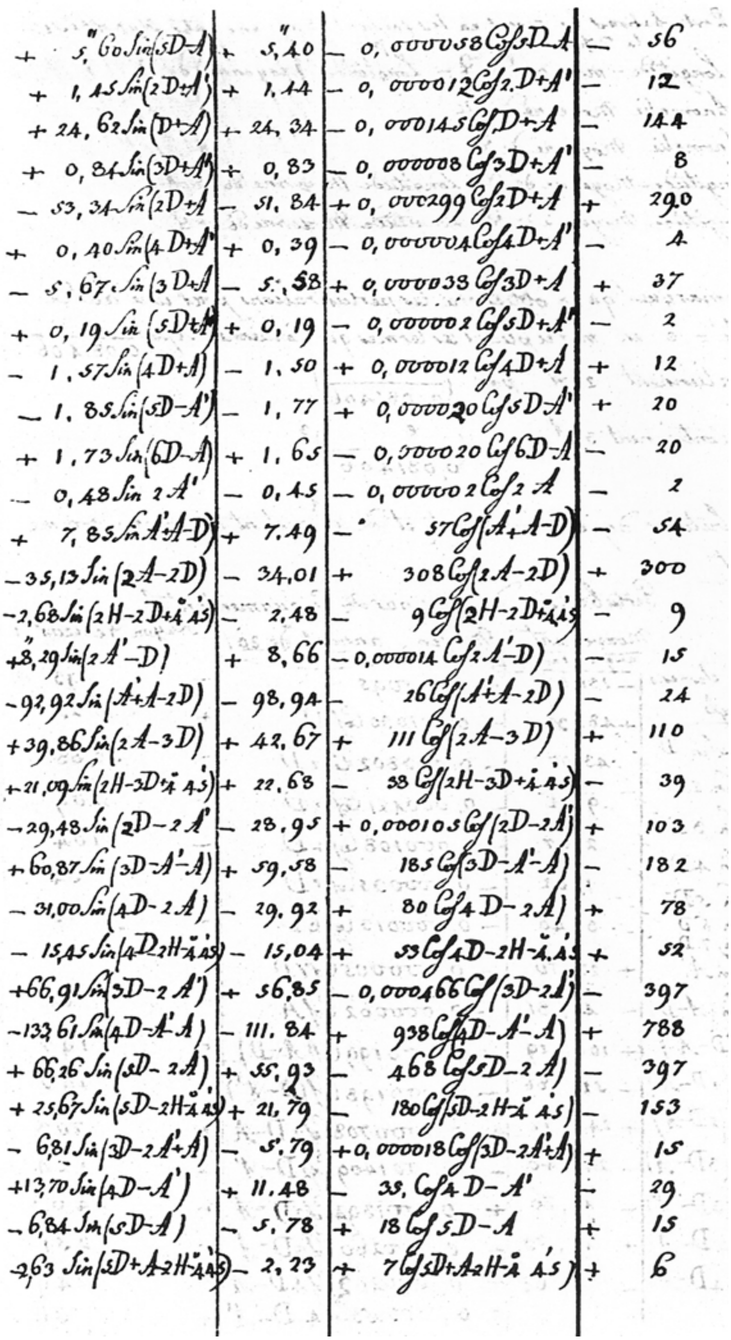
Suivant les elemens VII de Gauss	augmentant de 20'		
− 231,95 Sin D	−231,07	− 0,000095	− 93
+ 496,71 Sin 2D	+482,33	+ 0,001030 Cos D	+ 10 25
+ 44,15 Sin 3D	−43,07	− 0,003802 Cos 2D	− 36.88
+ 10,07 Sin 4D	−9,74	− 0,000421 Cos 3D	− 409
+ 3,05 Sin 5D	−2,97	− 0,000108 Cos 4D	− 104
+ 1,07 Sin 6D	−1,04	− 0,000035 Cos 5D	− 34
+ 0,41 Sin 7D	−0,40	− 0,000013 Cos 6D	− 13
+ 23,95 Sin A'	+ 23,70	− 0,000005 Cos 7D	− 5
− 40,98 Sin (A−D)	− 40,81	− 0,000062 Cos A'	− 61
+ 110,21 Sin (D−A')	+ 106,29	+ 0,000199 Cos (A−D)	+ 197
− 538,92 Sin (2D−A)	− 526,86	− 0,000198 Cos (D−A')	− 193
+ 238,96 Sin (2D−A')	+ 241,12	+ 0,000708 Cos 2D−A	+ 703
− 241,57 Sin (3D−A)	− 242,48	− 0,001409 Cos 2D−A'	− 1424
− 30,83 Sin (3D−A')	− 29,76	+ 0,001394 Cos 3D−A	+ 1403
+ 30,21 Sin (4D−A)	+ 28,73	+ 0,000260 Cos 3D−A'	+ 251
− 5,81 Sin (4D−A')	− 5,62	− 0,000262 Cos 4D−A	− 249
		+ 0,000058 Cos 4D−A'	+ 56

Fig. 14.13 Perturbations of Ceres due to Jupiter

and those containing 3 A with (e/0.081406)3
The dependent perturbations of Saturn and Mars remain almost the same
[Translation of the text in the upper part of the following table] (Figs. 14.13, 14.14 and 14.15):
Perturbations of Ceres by Jupiter. In increasing the
mean annual motion by 20′
according to the elements
VII by Gauss by increasing by 20′ (vector ray)
[Translation of the text in the table above]:
Perturbations of Ceres in helioc. latitude
according to the elements VII by Gauss/by increasing the annual mean motion by 20′
The arguments can be calculated according to the known astronomical tables.

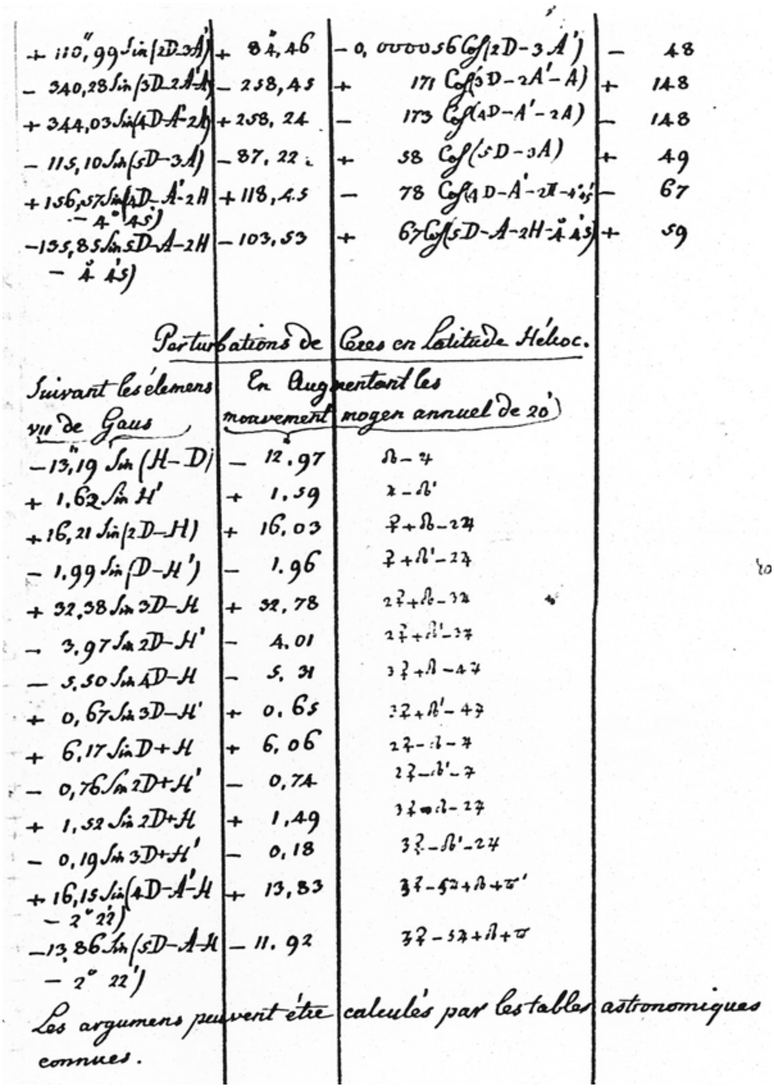
Sniadecki to Zach, Cracow, June 24, 1802

I have not been mistaken regarding the observations of Vilnius that the declinations of Ceres are considerably affected by refraction. Mr. Poczubut wrote me that he would have liked it better to let the astronomers choose which system of mean refraction they preferred, he has sent me the reading of the barometer and thermometer Reaumur for each day of observation. And I have the pleasure to send you the rest of the observations of Ceres. He could not find Pallas, my advice came too late, although I did not hesitate to send it to him as soon as I received your letter. Since my last letter it has been cold here from time to time and constantly overcast. I have only seen Ceres twice around sunset but I could not find Pallas again after my last meridian observation, having lost it out of sight during 15 days of bad weather.



+ 5,″60 Sin(5D−A)	+ 5,″40	− 0,000058 Cos 5D−A	− 56
+ 1,45 Sin(2D+A′)	+ 1,44	− 0,000012 Cos 2D+A′	− 12
+ 24,62 Sin(D+A)	+ 24,34	− 0,000145 Cos D+A	− 144
+ 0,84 Sin(3D+A′)	+ 0,83	− 0,000008 Cos 3D+A′	− 8
− 53,34 Sin(2D+A	− 51,84	+ 0,000299 Cos 2D+A	+ 290
+ 0,40 Sin(4D+A′	+ 0,39	− 0,000004 Cos 4D+A′	− 4
− 5,67 Sin(3D+A	− 5,58	+ 0,000038 Cos 3D+A	+ 37
+ 0,19 Sin(5D+A′	+ 0,19	− 0,000002 Cos 5D+A′	− 2
− 1,57 Sin(4D+A)	− 1,50	+ 0,000012 Cos 4D+A	+ 12
− 1,85 Sin(5D−A′)	− 1,77	+ 0,000020 Cos 5D−A′	+ 20
+ 1,73 Sin(6D−A)	+ 1,65	− 0,000020 Cos 6D−A	− 20
− 0,48 Sin 2A′	− 0,45	− 0,000002 Cos 2A	− 2
+ 7,85 Sin(A′+A−D)	+ 7,49	− 57 Cos(A′+A−D)	− 54
− 35,13 Sin(2A−2D)	− 34,01	+ 308 Cos(2A−2D)	+ 300
− 2,68 Sin(2H−2D+A″A′S)	− 2,48	− 9 Cos(2H−2D+A″A′S	− 9
+ 8,″29 Sin(2A′−D)	+ 8,66	− 0,000014 Cos 2A′−D)	− 15
− 97,92 Sin(A′+A−2D)	− 98,94	− 26 Cos(A′+A−2D)	− 24
+ 39,86 Sin(2A−3D)	+ 42,67	+ 111 Cos(2A−3D)	+ 110
+ 21,09 Sin(2H−3D+A″A′S)	+ 22,68	− 38 Cos(2H−3D+A″A′S)	− 39
− 29,48 Sin(2D−2A′	− 28,95	+ 0,000105 Cos(2D−2A′)	+ 103
+ 60,87 Sin(3D−A′−A)	+ 59,58	− 185 Cos(3D−A′−A)	− 182
− 31,00 Sin(4D−2A)	− 29,92	+ 80 Cos 4D−2A)	+ 78
− 15,45 Sin(4D−2H−A″A′S)	− 15,04	+ 53 Cos 4D−2H−A″A′S	+ 52
+ 66,91 Sin(3D−2A′)	+ 56,85	− 0,000466 Cos(3D−2A′)	− 397
− 133,61 Sin(4D−A′−A)	− 111,84	+ 938 Cos(4D−A′−A)	+ 788
+ 66,26 Sin(5D−2A)	+ 55,93	− 468 Cos 5D−2A)	− 397
+ 25,67 Sin(5D−2H−A″A′S)	+ 21,79	− 180 Cos(5D−2H−A″A′S)	− 153
− 6,81 Sin(3D−2A′+A)	− 5,79	+ 0,000018 Cos(3D−2A′+A)	+ 15
+ 13,70 Sin(4D−A′)	+ 11,48	− 35 Cos 4D−A′	− 29
− 6,84 Sin(5D−A)	− 5,78	+ 18 Cos 5D−A	+ 15
− 2,63 Sin(5D+A−2H−A″A′S)	− 2,23	+ 7 Cos 5D+A−2H−A″A′S)	+ 6

Fig. 14.14 Perturbations of Ceres due to Jupiter



+110",99 Sin(2D−3A)	+84,46	−0,000056 Cos(2D−3A')	−48
−340,28 Sin(3D−2A'−A)	−258,45	+171 Cos(3D−2A'−A)	+148
+344,03 Sin(4D−A'−2A)	+258,24	−173 Cos(4D−A'−2A)	−148
−115,10 Sin(5D−3A)	−87,22	+58 Cos(5D−3A)	+49
+156,57 Sin(4D−A'−2H −4°45')	+118,45	−78 Cos(4D−A'−2H−4°45')	−67
−135,85 Sin(5D−A−2H −4°45')	−103,53	+67 Cos(5D−A−2H−4°45')	+59

Perturbations de Ceres en Latitude Helioc.

Suivant les élemens VII de Gauss	En augmentant les mouvement moyen annuel de 20'	
−13,19 Sin(H−D)	−12,97	[illegible]
+1,62 Sin H'	+1,59	[illegible]
+16,21 Sin(2D−H)	+16,03	[illegible]
−1,99 Sin(D−H')	−1,96	[illegible]
+32,38 Sin 3D−H	+32,78	[illegible]
−3,97 Sin 2D−H'	−4,01	[illegible]
−5,50 Sin 4D−H	−5,31	[illegible]
+0,67 Sin 3D−H'	+0,65	[illegible]
+6,17 Sin D+H	+6,06	[illegible]
−0,76 Sin 2D+H'	−0,74	[illegible]
+1,52 Sin 2D+H	+1,49	[illegible]
−0,19 Sin 3D+H'	−0,18	[illegible]
+16,15 Sin(4D−A'−H −2°22')	+13,83	[illegible]
−13,86 Sin(5D−A−H −2°22')	−11,92	[illegible]

Les argumens peuvent être calculés par les tables astronomiques connues.

Fig. 14.15 Perturbations of Ceres due to Jupiter

Zach to Gauss, Seeberg, June 24, 1802

Oriani mentions the following error in his perturbations formulae of Ceres
231".95 sin D................228".78
The perturbations by Mars are
According to Gauss' elements VII. Mars-Ceres = d a = anom. med. Mars

+0.54 sin (3A − 2d)	*+0. "38*
−1.17 sin (2A + a − 3d)	*−0. 82*

+0.82 sin (A − 2a − 4d)	*+0. 59*
−0.19 sin (3a − 5d)	*−0. 14*
+0.28 sin (A + 2H − 12° 29′ − 2d)	*+0. 20*
−0.17 sin (a + 2H − 12 29 − 3d)	*−0. 12*

if increase the mean annual motion of Ceres by 20 min.

These perturbations can be regarded as minor and thus neglected. A change of 20′ of Ceres' annual motion does not result in different perturbation formulae of Saturn. You mentioned Schubert's perturbation formulae; would you please advise? [Ed: see Schubert, 1805.]

Dr Burckhardt has calculated the perturbations of Pallas for the times observed, but only those of Jupiter and only according to the first powers of the eccentricity. He finds perturbations

In longitude	*In latitude*	*For log. rad. vector.*
4 April −122.″0	*−174.″4*	*−0.0017641*
17 ----- −18.0	*−184. 9*	*−0.0016756*
20 May +77.6	*−194. 6*	*−0.0015837*

Testa to DeCesaris, November 17, 1802

Calandrelli pays his respects, and asks you to communicate to him the results of the apparition of Ceres observed by you, and also some geocentric longitudes and latitudes of the same planet. If you have some good observation made by Piazzi last year please communicate it together with its elements published by Piazzi or by others or by yourself.*

*Giuseppe Calandrelli was born in Zaragola on May 22, 1749. He was Professor of Mathematics and Director of the observatory at the Collegio Romano. He died in Rome on December 24, 1827.

Zach to Gauss, Seeberg, February 5, 1806

Of Ceres, Harding sent only one observation. Pasquich's results, obtained in Buda, reached me through Lindenau. I do not think these results are very reliable, they strongly disagree with the ephemeredes calculated by you, especially with regard to the data on declination. The method of calculating their average values should be improved.

Appendix A
Description of the Ramsden Circle (Fig. A.1)

A Description of the Ramsden Circle used by Piazzi to discover Ceres, as written by Pearson (1829).

1. *The first astronomical circular instrument that was made by Ramsden, was that with which the late eminent astronomer, Piazzi of Palermo, took those series of observations, from which the declinations in his much esteemed catalogue were computed and published, first in 1803, and then in an improved state in 1814. Piazzi informs us, that Ramsden twice undertook the construction of this instrument, and as often abandoned it; but at length in January of the year 1788, he entered upon the work in earnest, and finished it in August 1789.*
2. *The vertical axis of the instrument is composed of various parts, which, being firmly united together, constitute a frame that revolves in one piece on two pivots at the extreme ends; the cone, that terminates the lower extremity, tapers from a diameter of 14.2 inches to 5, where it is made fast to the horizontal circle, on which the azimuthal angles are measured, of three feet diameter. This circle has ten tubular radii of a conical form, and is divided into two semicircles, figured into 180 degrees, each of which is subdivided into spaces of 6'; it revolves with the frame composing the vertical axis. The base of the inverted cone is firmly attached to an oblong plate of metal, which may be called the lower stage, and into which four long vertical tubes are fastened, as so many pillars, to support the upper stage, of the same dimensions as the lower one, namely 25.3 by 16.8 inches. These four pillars are each 6.5 feet long, and 3.5 inches in diameter, and, together with the two stages, constitute the frame holding the vertical circle. This circle is five feet in diameter, divided into four successive quadrants, in which each degree is figured with an Arabic numeral, and each tenth degree with larger numerals of the Roman characters; and, as the reading microscopes have small fields of view, the subdividing strokes, including spaces of 6′, are known by single, double, triple, etc points made contiguous to them on the graduated face. The circumscribing portion of the vertical circle is composed of two flat rings, standing parallel to and concentric with each other, by means of cross bars that unite them, like so many rounds of a ladder; which plan gives strength, without adding materially to the weight of the structure. The graduated face is that which is presented to the eye in the perspective figure.*
3. *The horizontal axis of the vertical circle consists of three pieces, a central cylindrical hollow piece, and two inverted hollow cones, all of brass, compactly fixed together; at the extreme ends of which two steel cylindrical pivots are made fast, one to the apex of each cone, which bear a portion of the circle's weight. The eight conical radiating*

C. Cunningham, *Discovery of the First Asteroid, Ceres*,
DOI 10.1007/978-3-319-21777-2

Fig. A.1 A photo of the author with the Ramsden Circle at Palermo Observatory

tubes of brass are fixed to the central part of the axis at their bases, and at the remote ends to the middle of certain cross bars, connecting the two rings of this circle: the telescope passes through the said central portion, instead of two more radii, and is made fast at both ends between the rings of the double circle, so as to prevent any tendency to flexure in its tube. The focal distance of the telescope is equal to the diameter of the circle; and five direct eye-pieces are supplied, magnifying the linear dimensions respectively 50, 75, 100, 130 and 170 times; besides which there is a prismatic eye-piece of the description mentioned in the sixth paragraph of our fifth section, which performs the office of a reflecting diagonal eye-piece. This eye-piece has two powers, one of 75, and the other of 130, and is principally used for viewing stars near the zenith. The illumination is effected by transmitting the light of a small lamp through the hollow axis, the inclined reflector, in the middle of which, is exactly similar to that of a transit-instrument: the light, however, is limited to suit the object viewed by a parallelopiped composed of three pieces of glass; of which the middle one is white, and the two extreme ones green; they are contained in a frame that has an adjustable motion by means of pulleys, visible at the remote end of the axis, behind the back pillars. The reason of two green wedges being used is, that the lines in the focus of the eye-piece may not appear double, by passing through two glasses of unequal refracting powers; the second green glass being made to correct the refraction occasioned by the first. The pulleys are acted upon by a long handle terminating with a Hooke's joint. The whole length of the vertical circle's axis, including the pivots, is about two feet. Besides the four long pillars already noticed, there are two shorter ones, ascending, at the distance of eleven inches from each other, from the face of the lower stage, up to the middle of *the inverted cones of the horizontal axis; these hollow pillars are each three feet three inches high, and are braced near their upper ends to their adjoining long pillars; a strong rod passes through each pillar, and supports a small frame, containing a pair of rollers, side by side, one pair of which may be seen under the left hand cone of the axis. Each cone has a circular edge-bar made fast round it, that rests on the Y formed between the pair of rollers, and a pair of adjusting screws acting on the vertical rods, bearing the rollers, force them against the circular rings of the cones, and support any required portion of the circle's weight, to relieve the pressure on the steel pivots. One of these rods has its adjusting screw under the lower stage, but the other is acted on through a small frame interposed between the two halves of the tube, for some reason that is not obvious. The lower stage is strengthened by several brackets descending from its lower surface to the inferior end of the inverted vertical cone, to which they are made fast.*

4. *The two large metallic pillars, that ascend from the two opposite corners of the square, composed of a marble floor, are each seven feet high, and four inches in diameter, and are matched by a similar pair, that do not appear, but which ascend from the other two opposite corners of the same square floor. These two pillars have arched tops of brass. The arch that connects the first pair of pillars will explain the structure of the second, which is exactly similar, and is placed at right angles to the other. A cross of four straight bars connect the arched portions, that rest on the superior ends of the four pillars, to which they are made fast; and a circular hole, at the place of crossing, receives the tubular pivot at the superior end of the vertical axis, and is firmly fixed to the middle of the oblong opening, that nearly severs the upper stage. This opening allows the telescope to view stars near the zenith without obstruction, the bars, connecting the two halves of the stage, being thinner than the diameter of the object-glass.*
5. *The lower support of the vertical axis consists of three concentric circular plates of iron, laid over one another with attached rollers under the second and third; the uppermost circle bears the conical pivot, on which the axis turns, and the other two have their respective adjustments at right angles to each other; one being moved in the direction of east and west, and the other of north and south: each motion is produced by a horizontal screw, by means of a handle with an universal joint, of which one is seen standing up rather obliquely, to the right of the three circles. The manner of each screw's action may be easily understood, if we conceive one of them made fast to one of the lower circles and the other made fast to the other, with their tapped ends entering the sides of the uppermost, which may thus be pushed forwards or drawn back in either of the assigned directions. A ring of mahogany, three feet two inches in diameter, and three inches thick is laid over, and made fast to the uppermost circular plate; and forms the basis of a balustrade, having a ring of metal above and another below, connected by twenty cylindrical rods, or small pillars of brass, each thirteen inches long. This balustrade preserves the azimuthal circle from injury, and supplies the means of clamping the vertical axis with a tangent screw of slow motion, which is turned by the handle seen above the balustrade towards the left: it also holds the reading microscope in its proper place, over the divisions of the graduated horizontal circle, that the position of the telescope, and of the vertical circle, may at any time be indicated by it, when once adjusted so, that zero will show the meridional position. The microscope is seen in the place of a connecting pillar, towards the right hand side of the balustrade, and carries an inclined circle of silver at the object-end, to throw light on the divided face of the circle. It is furnished with a micrometer not essentially differing from that of the reading microscope, except that the lines are fine wires.*
6. *The vertical circle has its divisions indicated by two reading microscopes placed diametrically opposite each other, to correct for any excentricity (sic) that may exist in any of the positions of this circle, as it regards the telescope's elevation; the construction*

and adjustments of these microscopes are similar to those of the microscope that reads the azimuthal angles; but, though we know that the vertical circle is graduated into four successive quadrants, it is not quite clear from the original account, whether altitudes or zenith distances are read, or both in the reversed positions; though it is probable that zenith distances were generally indicated, since this is the denomination in which the observations are registered, as exhibited in Cacciatore's late valuable publication. The situation of the superior microscope is a little under the upper stage, where a frame of parallel bars connects the upper ends of the two front pillars, as seen in the plate, and affords the means of making the proper adjustments for distinct vision, for the value of the screw, and for bisecting the circle, as the position regards the lower microscope. The situation of the latter microscope is regulated by a similar frame, screwed to the lower parts of the same pillars, above the inferior stage, and has the same adjustments and value of its micrometrical screw, as the other microscopes; each of which will read separately to the accuracy of a single second of a great circle.

7. *When the zeroes of the two microscopes are adjusted to the zero points of the circle, separating the semicircles by an imaginary vertical line, the circle may be turned half round, or the telescope inverted; and if the same zeroes again coincide, then the microscopes are properly opposed to each other, and also the circle is properly divided, in the direction of that diameter, into two equal semicircles. When there is a difference indicated between the two semicircles, after the inversion of any given position has taken place, one half of the difference, shown by one of the microscopes in the second position, will be the error belonging to each semicircle, which error will have the sign + in one semicircle, and − in the other; but one fourth of the said difference will be the error belonging, with its proper sign, to each quadrant; on which account the whole error thus observed is called by Troughton the quadruple error; and if the maximum of this error is small, when the circle has been examined by opposite microscopes in all diametrical directions, the circle may be said to have but little excentricity, and also equal divisions; which is the most desirable property a graduated circle can have.*
8. *Besides the two reading microscopes just described, which are employed solely for reading the subdivisions of the circle, a pair of smaller or secondary ones are placed on the same frames respectively, for viewing a plumb-line, suspended from a small adjustable cock, placed above the upper frame; which plumb-line descends down a square pipe of wood, attached to the right hand pillar, and carries a weight immersed in a water-vessel, standing on a small stage that may be raised or lowered at pleasure, by a vertical screw, for regulating the depth of the immersed weight; and as this small stage is fixed upon the larger one, that revolves with the vertical axis, it is evident that this plumb-line may be used in any azimuthal position that the telescope can take; and therefore that the axis may be adjusted by it into a position that will be perfectly vertical in all azimuthal directions; and, what is very important, will watch this adjustment at all times, by preserving its own vertical position, and exhibiting any deviation that may take place in the cock of suspension carried by the vertical axis; and consequently in the perpendicular direction of the axis itself. When any of the least inclination of the vertical axis towards a given point in the horizon has been detected by the plumb-line, it must be re-adjusted by the screws acting on the circular plates, supporting the lower end of the axis: when this inclination is towards a point lying in the middle between the two adjusting screws, they must both be turned an equal quantity in the same direction; but if it be directly towards one of the screws, that screw only will require to be turned: in general that particular screw must be most turned in making the adjustment, towards or from which the inclination or reclination is greatest.*
9. *But there is a second useful purpose to which the plumb-line is applied; the horizontality of the vertical circle's axis is thereby insured, as often as any inclination is detected in it. If we suppose the forked measuring bar there used with the microscope, forming the ghost apparatus, to be applied to the plane of the vertical circle, so as to measure*

its distance from the plumb-line at two points, successively taken in a vertical diameter, the equality of the measures will show that the circle's plane is parallel to the plumb-line, in the same way that the telescope of the transit-instrument was shown to be parallel to its plumb-line when properly adjusted; and as the circle was formed in the lathe upon the pivots of its own axis, its plane stands by construction at right angles to the line passing through the axis, that joins the centres of the pivots; and therefore when the plane of the circle is adjusted to become vertical, its axis necessarily becomes horizontal. It is affirmed that in this way an error of a single second of inclination in the axis may be detected: but as the transits taken by this circle, are intended to be only approximate, to identify the body observed, and as its principal use is to measure correct zenith distances, a very nice adjustment of the horizontal axis is not so material, as that of the vertical axis, on which the accuracy of the observation depends. Instead of measuring from the plane of the circle itself to the plumb-line, Ramsden however fixed a small bridge to the object end of the telescope, in which was inserted a pin, from which the measurement by the forked rod was taken alternately above and below, after the circle had been turned half round, which mode of measuring, he thought, insured the motion of the telescope to be in a vertical circle in the heavens, without considering the question of its parallelism to the plane of the vertical circle; and this was precisely the plan adopted by [William] *Cary. Whenever Piazzi rectified the superior and inferior microscopes of his vertical circle, and of his plumb-line, he was accustomed to use the zero points of his circle, as the points that bisected it most perfectly into two equal semicircles; and, as a reason for such preference, he affirms that these points did not deviate more than a quarter of a second from their true places. The clamp of the vertical circle and the tangent-screw of slow motion are made fast to the left-hand short pillar, carrying one of the pairs of rollers, and the handle seen depending near its lower extremity, and parallel to it, communicates the slow motion, by taking hold of the screw's arbor with the hollow squared end of its universal joint.*

10. Adjustments—*When the telescope has been brought to distinct vision of a terrestrial object, the first adjustment will be that which makes the vertical axis perpendicular in all directions, which may be performed by means of the plumb-line, by halving the error, partly by the screws of the subjacent circular plates, and partly by the adjusting screws of the cock of suspension, or by turning round the excentric disc of mother-of-pearl forming the object of the compound microscope, or ghost apparatus, which, for small quantities, is a more convenient operation. When the vertical axis is adjusted, the microscope, reading the azimuthal circle which is now perfectly horizontal, must be so placed and rectified, that it may view the dividing strokes of the limb, and the wires in the common focal point, distinctly at the same time, and also make just six revolutions in measuring one space. The relative positions of the balustrade, that carries this reading microscope, and of the zero of the azimuthal circle, must also be so situated, that when the telescope is brought truly into the meridian, the zero of the microscope is capable of being made coincident with that of the circle, which may be effected by turning the ring carrying the balustrade a little round the pivot of the vertical axis. The collimation in azimuth must be rectified by turning the vertical axis half round till the microscope reads the opposite zero, and by observing a distant mark in both positions, before and after the telescope has been reversed, when the error will appear; which must be done away by continual halving, partly by the proper screws in the eye-piece that move the vertical wires, and partly by turning the axis a little, and altering the reading on the scale of the microscope, till the distant object is bisected in both of the reversed positions: after which the telescope may be finally placed in the meridian, and the zeroes of the microscope and of the circle's limb be made again to coincide. The error of collimation in zenith distance may lastly be adjusted, either by a distant meridian mark, or by the pole star, at its meridian passage; for if the reversed positions of the circle and inversion of the telescope give the same zenith distance, no error exists;*

but when there is an appreciable difference it may be made to disappear by repeated halving, partly by the contrary screws in the eye-piece, intended for this purpose, and partly by displacing the scale of one of the vertical circle's reading microscopes, till the observation is the same in the inverted positions of the telescope, and reversed positions of the circle; after which the second microscope must be put to correspond to the position, exactly opposed to that of the first; and the instrument will be fir for use, provided that, in making these adjustments, the plumb-line still shows that the axis of the vertical circle is horizontal, for adjusting which it has the usual vertical screw at the Y bearing the front pivot.

11. *With respect to the mode of making the observations, suppose of zenith distances; if the instrument had no error in collimation, the two readings of the microscopes would at once give a mean, when the star is seen crossing the middle of the field of view upon the horizontal wire; but where there are so many pillars and arches of metal exposed to different strata of air, not precisely of the same temperature, it is found from experience, that the adjustments will not be permanent, and that good results may be obtained, particularly with this instrument, rather from the application of known corrections, than from a dependence on the continuance of perfect adjustments for any considerable time. When the vertical axis is truly perpendicular, and the error of collimation in zenith distance known, this error applied to a mean of the readings will be sufficiently correct; but the change in the temperature of the internal air, that is continually taking place from the opening and shutting of doors and shutters, notwithstanding every precaution, will always render such single observations, made in one position, doubtful; and therefore corresponding observations of the same body, made in reversed positions, should always be preferred; and when several of these are taken, in which there exists but slight discrepancies, a mean of the whole is most to be depended on. If the reversed observations, with the face of the vertical circle alternately placed to the east and west, be made on the same evening, the circumstances affecting the adjustments are most likely to be similar, and the error in collimation will disappear, by having contrary signs in the different positions, though in this case the body observed cannot be on the meridian at both instants of making the observations; but the tables of reduction to the meridian will, with a little trouble, remedy this inconvenience. Yet the circle's vertical axis must have its position correct at both instants, or there will exist an error arising from the inclination towards either the north or south, that will affect both observations alike, for which there is no correction, but what must be estimated from the situation of the plumb-line, that has no scale for indication. If the star is observed on the meridian on two successive nights, or after an interval of some days, a slight change will have taken place in the zenith distance itself from precession, aberration, and nutation, and also the instrument may not be in the same state of adjustment, as to collimation, and perpendicularity of the axis, that it was at the first period: hence difficulties present themselves in either case which minute attention and delicate management alone can overcome. The instrument could not have been in better hands than those of Piazzi, and of his highly gifted assistant Cacciatore, who, fortunately for astronomy, succeeds him in the Observatory at Palermo.*
12. *The late proprietor of the instrument (Piazzi) we have here described, has enumerated eight advantages which it possesses over its quadrantal predecessors, which are as follow; first, the graduated circles are not encumbered with verniers, so as to have their divisions defaced, or steadiness molested; secondly, the subdivisions are read by microscopes that magnify nine times, so that the least quantity may be appreciated; thirdly, the vertical circle has its plane made by revolving on its own axis, and also its circular lines struck therefrom; consequently a deviation of the plane, and en excentricity of the divided circles are both avoided; fourthly, the compound circle preserves its figure much better than it would have done, if it had been cast in one solid piece; fifthly, the observations may be reversed with respect to both zenith distances and*

azimuths; therefore a mean of two reversed observations of a zenith distance will correct the errors of the limb arising from excentricity, and also the error of collimation in zenith distance, which will be plus in one position, and minus in the other; sixthly, the instrument may be clamped to the balustrade, and used as a transit-instrument; seventhly, it gives zenith distances and azimuths at the same time, and therefore is particularly useful in single observations of a comet, or other temporary phenomena; lastly, the refraction of the atmosphere, corresponding to a given temperature, may be experimentally determined by comparing an observed zenith distance of a known star, with its computed zenith distance, in a known latitude, when the azimuth has also been observed. Indeed it was in this way that this zealous and persevering astronomer determined the mean refractions of stars.

An Italian-language description of the Ramsden circle is at this website: www.astropa.unipa.it/museo/sezioni/cerchio.htm

Appendix B
The Events of 1801

Jan. 1: Discovery of Ceres
Jan. 24: Piazzi sends letters to Bode and Oriani
Feb. 11: Piazzi makes his last observation of Ceres
Feb. 26: Lalande reads about the discovery in the Journal de Paris
Feb. 27: Lalande writes to Zach and Piazzi about the discovery
March 20: Bode receives Piazzi's discovery letter
March 26: Bode reports the discovery to the Royal Academy in Berlin
April 2: Oriani receives Piazzi's discovery letter
April 7: Zach is sent notice of Piazzi's discovery in a letter from Oriani
April 14: Bode sends a "deceptive" letter to Zach with Piazzi's positional data
May 6: Notice of discovery in the Jena newspaper
May 12: Notice of the discovery in the Berlin newspaper
May 13: Notice of the discovery in the Hamburg newspaper
May 31: Lalande receives Piazzi's positional data from Piazzi
June: The first paper about the discovery is published in the MC
June 6: Bode writes Herschel about Piazzi's discovery
Aug. 4: Piazzi notifies Seyffer of his discovery
Aug. 7: Notice of the discovery in a London newspaper
Sept. 1: Piazzi notifies Herschel of his discovery
Sept. to Oct: Gauss develops an ephemeris to aid the recovery of Ceres
Oct. 15: Piazzi's printed account of his discovery reaches Zach
Oct. 22: Maskelyne receives a letter from Piazzi with details of his discovery
Oct. 25: Piazzi's printed account of his discovery reaches Lalande
Dec. 7: Zach makes the first sighting of Ceres since February

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Appendix C
The Historical Development of the Orbital Elements of Ceres

This account is excerpted from Publications of the Lick Observatory, vol. 19 (Leuschner, 1935).

The following abbreviations are used in the citations: BJ (Berlin Yearbook); MC (Monthly Correspondence); CT (Conn. des Tems); AJ (Astronomical Journal); AN (Astr. Nachrichten), MN (Monthly Notices), JBAA: Journal of the British Astronomical Association, BIA Len: Bulletin Inst. Astronomy Leningrad.

Preliminary orbits of Ceres were published:

Basic Dates	*μ*	*Author*	*Reference*
1801 (5 obs.)	*859.″05*	*Burckhardt*	*BJ 1804 255*
January 1, February 11	*786.528*	*Olbers*	*BJ 1804 256*
January 1, February 11	*795.937*	*Piazzi*	*BJ 1804 259*
January 2, 22, February 11	*781.355*	*Gauss*	*MC 4, Gauss Werke VI 200*
January 1, 21, February 11	*784.2543*	*Gauss*	*MC 4, Gauss Werke VI 200*

From 22 meridian positions covering 9° of heliocentric arc near the stationary point several astronomers found that the planet moved in a nearly circular orbit. Olbers reobserved the planet January 1, 1802 (from Gauss' second orbit above) 0.°5. Gauss computed Elements A by including this position, O-C 1802 February +7" -20", BJ 1805 94, MC 5 263, Werke VI 207. Burckhardt obtained Elements B, including perturbations, BJ 1805 96, CT 1804/05 453. Oriani computed an orbit by Laplace's method, MC 6 (1802 December). Gauss derived Elements C, MC 6 492, Werke VI 229, and formed tables of perturbations, Werke VII 375. In 1805 he developed the perturbative function in the manner later used by P. A. Hansen, Werke VII 401.

Encke based an ephemeris on Gauss' Elements XIII (1809), Gauss' tables of perturbations, and an empirical correction $\Delta M = -14'$ determined from the latest positions, BJ 1830 118, 245, O-C 15'. He derived Elements D from oppositions 1820, 21, 22, 25, including special perturbations in the elements by Jupiter (1053.924) O-C 6' 6', and published elements osculating for oppositions 1831-50, BJ 1831 275, 250, AN 27 179. Encke and J. P. Wolfers continued special perturbations by Jupiter and published ephemerides, BJ 1831 248, 118, 1837-71. O-C increased in 28 years to 3 20".

1827 -2" 0" 1829 -27" -11"

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A. Heiligenstein computed Elements E from 7 oppositions 1818-27, including special perturbations in the elements by Jupiter (1053.924), O-C ΔL 10", AN 7 413, and published ephemerides, BJ 1832-36. 1830 -6" –10"

M. C. Damoiseau computed many terms in the general perturbations, CT 1846: Supp 32, but G. W. Hill (1896) found them to be rather inaccurate, AJ 16 57.

E. Schubert derived Elements F from 250 positions in 14 oppositions 1832-54, with the perturbations of Encke and Wolfers and a correction for the secular variations of the obliquity of the ecliptic, O-C 22″ 8", AJ 3 153, 162, NA 1837, BJ 1838 286. Schubert corrected an error in ϕ and obtained Elements G from 4 normals 1853-57, including special perturbations by Jupiter and Saturn, O-C 0."4. AJ 5 73. O-C increased in 23 years to 6 40". W. Godward (1878) derived Elements H from 15 oppositions 1857-76, using the residuals from the ephemerides in NA, including perturbations by Venus, Earth, Mars, Jupiter, Saturn, MN 38 119. O-C from NA ephemerides increased in 36 years to 2 10". 1913 +2.8 -20" 75° 1915 +2.1 +16" 272°

To illustrate a modification of Hansen's method of general perturbations, Hill computed first order perturbations by Jupiter with Schubert's Elements F, AJ 16 57. The osculating μ differed so much from the mean μ that he used an arbitrary value in the integration. He derived mean Elements I from 10 normals 1802-90, including secular perturbations by Mars, Jupiter (1047.355), Saturn, with Gauss' method, and periodic perturbations by Mars and Saturn from Damoiseau's tables, O-C heliocentric ΔL 40", geocentric ΔB 20". Abandoning his intention to improve the theory, Hill published a collection (incomplete) of positions 1801-97 in 75 normals, AJ 21 51.

Using Hill's Elements I, C. J. Merfield computed secular perturbations by the 8 major planets with Gauss' method as given by Hill, MN 67 551 (see Merfield, 1907). Wolf developed p by Gyldén's theory (1890, Stockholm) [see editorial note below]. *In 1916 M. A. Vil'yev computed general perturbations with Hansen's method, Bull Pulkova 7, Merton computed elements with positions in 1922 and one in 1923, and an ephemeris for 1923, JBAA 33 226, 295.*

N. Komendantov, starting with the elements in NA 1913, based an orbit on 12 normals 1913-22, including Jupiter's action with V. Numerov's method, O-C 0.7 8", AN 219 275, JO 6 57. 1923 -0.6 -2" 153°

Cripps computed an empirically corrected ephemeris, BAAH 1924. Komendantov derived Elements J from 16 normals 1920-23 (0.6), including the action by Jupiter and Saturn, O-C 0.3 3", BIA Len 1 10, JO 7 92, VRI 45 102. He published accurate ephemerides, JBAA 34 295, BAAH 1925-26. 1924 +2.0 +15" 250° 1926 +5.7 -20" 350°

He obtained Elements K from 5 oppositions 1920-26 (no Jupiter conjunction), including the action by Jupiter and Saturn, O-C 1924-26 0.1 0", BIA Len 1 163, 2 18, KP 1927–. He published accurate ephemerides, BAAH 1927–.

El	Epoch		M	Eq	ω	Ω	i	ϕ	μ
A	1801	S	291°30'21.″5	1801	64°58'35"	80°58'40"	10°37'56.″6	4°40' 9.″9	769.″6536
B	1802	.	8 47 58	....	65 39 2	81 5 35	10 36 52	4 31 25	771.363
C	1801	S	290 45 57.9	1801	65 38 38	80 54 59	10 37 56.0	4 31 17.8	770.6567
D	1822 Jan 22.0	B	339 59 46.6	1810	66 55 2.6	80 41 55.0	10 38 7.7	4 31 18.0	770.72468
E	1818 Oct 15.0	Gö	240 19 38.2	1818	67 13 41.9	80 48 32.2	10 38 21.7	4 31 5.2	771.2273825
F	1854 Jan 0	W	113 22 25.1	Ep	68 4 32.6	80 50 50.8	10 37 8.5	4 24 38.4	769.63875
G	1854 Jan 0	B	113 18 22.4	Ep	68 5 32.6	80 50 31.1	10 37 4.8	4 24 29.4	769.62476
H	1854 Jan 0	W	113 22 11.7	1854	68 4 55.5	80 50 31.1	10 37 5.8	4 24 29.6	769.64746
I•	1850 Jan 0.0	G	161 1 59.9	1850	67 40 26.9	80 48 5.6	10 37 6.2	4 29 57.8	770.718276
J	1921 Jan 3.0	U	310 20 25.1	1925e	131 12 12.6	23 26 59.3	27 8 6.3	4 32 57.9	771.38881
K	1925 Dec 6.0	U	336 41 4.0	1925e	129 51 16.1	23 26 30.2	27 8 53.6	4 33 56.0	770.94898

[Ed: In 1881 Hugo Gyldén, Director of the Observatory at Stockholm, began the publication of a new method for calculating the motions of celestial bodies. His pupils made the method of practical importance by its application to the asteroids.

A technical study of this (with detailed references) is in the Report of the Annual Meeting of the British Association for the Advancement of Science, 1900, vol. 69, 138–144].

Appendix D
Star Atlases

The role of star charts in the search for asteroids in the early nineteenth century is considered by Staubermann (2006). These three star atlases featured prominently in the early work devoted to the study and recovery of Ceres:

Wollaston's Star Catalog

A *Specimen of a General Catalogue*, Arranged in zones of north polar distance, and adapted to Jan 1, 1790: Containing a Comparative View of the Mean Positions of Stars, Nebulae, and Clusters of Stars, Together with a Proposal for Setting on Foot Some Regular Method of Observing the Heavens, through the Concurrent Assistance of Astronomers in All Nations … London: for G. and T. Wilkie, 1789. Large folio, pp. xvii, [i] blank, [254]; with three engraved illustrations.

An uncommon and practical star catalogue compiled by Francis Wollaston, and a precursor to his more ambitious work of 1800 *Fasciculus Astronomicus*. The present volume was considered to be of great value by contemporaries and was utilized by Herschel, Lalande and Piazzi. The arrangement in Zones of North Polar distance was an innovation of Wollaston, who collected the stars from various catalogues, from Hevelius to the present day. It is a substantial collection of comparative catalogues with a preface announcing Wollaston's plan and discussing the many previous catalogues on which he based his coordinates. Wollaston long entertained the hope that astronomers might collaborate on a general plan for improving star catalogues and drafting them in a way that would facilitate the measurement of small stellar movement.

The volume includes *An Index to the Stars in the British Catalogue*, Flamsteed's *British Catalogue,* and Nicolas De La Caille's *Southern Catalogue* of 1763 arranged in the order of right ascension for Jan 1, 1790 and *A Zodiacal Catalogue* as well as information taken from the Astronomer Royal, Nevil Maskelyne's own catalogues. The three illustrations found in the advertisements at the end of the work are also of

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interest. These engravings "from which any number of impressions may be had" were intended to be used by the reader to represent both "the field of view of a telescope" and to "assist in making such a plan of a small portion of the heavens," suggesting that the catalog was intended for both the professional and the 'gentleman astronomer'.

Wollaston's 1811 atlas, *Portraiture of the Heavens,* was the first major star atlas published after Bode's *Uranographia* of 1801. Wollaston's maps are much simpler, and cleaner. The constellation figures are faint, and in outline only. The number of constellations has been sharply reduced; Wollaston rejects all of Bode's innovations, and indeed there are no constellations here that were not used by Flamsteed. Nebulae are not depicted at all. Instead, the emphasis is on the stars. Special attention is given to variable stars, and each plate has marginal annotations at left and right that refer to changes in the fixed stars.

Bode's Star Atlas: *Uranographia*

Large folio (25¼ × 19½ in.). 3pp. letterpress preface, 1p. index, otherwise engraved throughout. Double-page engraved title and 20 double-page celestial charts. *Allgemeine Beschreibung und Nachweisung der Gestirne.* Berlin: for the author, 1801. Title and text in German and French. Bode's *Uranographia* marks the high point of pictorial star atlases, and surpassed all of its predecessors by listing over 17,000 stars and containing, for the first time, the nebulae, star clusters and double stars discovered by William Herschel. The *Allgemeine Beschreibung* was the most extensive star catalogue to date. It was intended as an accompaniment to the atlas, but the two volumes are now very rarely found together. The successor to the works of Johann Bayer (1572–1625), Hevelius and Flamsteed, the *Uranographia* was the last great atlas of its kind. The clear and vigorously engraved celestial charts set new standards for accuracy and completeness, and for the first time the mapping of the heavens was carried beyond the limits of naked-eye visibility. The vast number of stars included which lay beyond the traditional constellations led to Bode's innovative idea of constellation boundaries. Five new constellations are introduced: Felis (suggested by Lalande), Globus Aerostaticus (proposed by Lalande in honour of the Montgolfier brothers), Lochium Funis, Machina Electrica, and Officina Typographica (in honour of the 350th anniversary of the invention of movable type).

Flamsteed's Star Atlas

John Flamsteed was the first Astronomer Royal of England and presided over the building of Greenwich Observatory. He was a dedicated observational astronomer, and his *British Catalogue* of stars, finally published in 1725, well after his death, brought stellar astronomy to a new level. His star atlas, published 4 years after the catalogue but in development for over 20 years, was based on Flamsteed's new, more accurate observations, and that fact, coupled with its impressive size (it was the largest star atlas that had ever been published) immediately vaulted it into the select ranks of the great celestial atlases.

One of Flamsteed's principal motives in publishing the atlas was to correct what Flamsteed felt were serious errors in Bayer's depiction of the constellation figures. Bayer (in his 1603 atlas *Uranometria*) had reversed many of the figures, showing them from the rear instead of the front, and these new positions contradicted the traditional star descriptions (i.e., Ptolemy's "star in the right shoulder" of Orion had become, in Bayer's rendering, the star in the left shoulder). Since most stars were still referred to by their Ptolemaic labels, Flamsteed objected to Bayer's revisions as introducing unnecessary confusion into stellar astronomy, and so all his figures follow the traditional descriptions exactly.

The Flamsteed atlas was welcomed because of its unprecedented accuracy, but it did suffer from some deficiencies. It was almost too big to use, with its 24-by-24 in. plates, and it lacks the aesthetic quality of both the Bayer and Hevelius atlases. While Andromeda is graceful enough, Aquarius is almost grotesque, especially when compared with the artful Aquarius in Bayer's *Uranometria.* What Flamsteed's atlas needed most was a new edition, with reduced and more pleasingly drawn plates. Astronomers in France filled this need in 1776, and the French version of the Flamsteed atlas, the *Atlas celeste,* immediately became the standard in the field.

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AJ: Astronomical Journal

AN: Astronomische Nachrichten

BAJ: Berlin Astronomical Yearbook

JAHH: Journal of Astronomical History and Heritage

JHA: Journal for the History of Astronomy

MC: Monthly Correspondence

MNRAS: Monthly Notices Royal Astronomical Society

Phil. Trans.: Philosophical Transactions of the Royal Society, London

RAS: Royal Astronomical Society, London

S&T: Sky & Telescope

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