The Moon that Wasn't: The Saga of Venus' Spurious Satellite by Helge Kragh (assisted by Kurt Møller Pedersen)

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This book is an account of certain peculiar telescopic observations from the 1640s through the 1760s, and of the interpretations of them during this early period and later. What was seen was Venus plus an appearance that some took to be a satellite of Venus and others explained as an illusion produced by secondary reflections from the eye and within the telescope, or as a star or planet or other celestial object erroneously identified as a Venus moon.

According to our author, the satellite did not exist, a conclusion which we accept as justified. Before admitting the reality of a putative object, we demand a certain concordance and predictability in its various appearances. In the case of a satellite of a circumsolar planet, we would like to be able to determine the data that a mathematical astronomer especially wants—a repeatedly verifiable mean distance from, and period about, the primary. From these data together with the distance from and period of Venus about the Sun, one could determine the mass of Venus relative to the mass of the Sun; and this value would enable us to determine the gravitational action of Venus on other bodies such as our Moon. Venus perturbs the motion of our Moon by a rather small but nowadays quite detectable inequality amounting to some 14 arc-seconds. The Venus moon would thus have played its role in the project of getting Newton's—or Einstein's—gravitational theory to work. But the putative satellite of Venus confined itself to rare and unpredictable appearances, refusing to be pinned down to a stable, identifiable orbit.

'Why write a book', asks Kragh, 'about something that manifestly does not exist?'

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> The brief answer is that for more than a century the enigmatic satellite—or something taken for it—was occasionally seen, or thought to be seen, and that the object thus became part of the history of astronomy.... By following the discussions of this ghost-like satellite, we address the history of planetary astronomy in a novel way. We get a different insight not only into the world of the astronomers but also into the popular literature concerning the planetary system and other aspects of astronomy. [viii]

The 'different insight' that Kragh mentions here has to be extracted from the various responses generated by the reports of the telescopic sightings of the alleged moon, first in an age when telescopic observations of any kind were novel, and later when observers had gained expertise in the use of telescopes and had accumulated a body of confirmed results which they could refer to with confidence.

Kragh's account begins with a 1646 report by Francisco Fontana, an early constructor of astronomical telescopes. Fontana saw two small dots accompanying Venus and supposed them to be

> her Courtiers and Attendants.... This is a new discovery not yet published in my opinion. But it is true that they do not always appear, but only when Venus is shimmering.... These little dots were... not always seen in the same situation on Venus, but they moved back and forth like fish in the sea. [10]

Fontana's report, Kragh tells us, 'quickly caught the attention of the learned world'. Galileo was dead when Fontana's book appeared; but earlier, he had expressed skepticism about Fontana's observational claims, acknowledging that Fontana's telescopes had greater magnifying power than his own but denying that they could reveal novelties that had not been revealed by his telescopes. Galileo's disciple Torricelli, no less sceptical than he, described Fontana's book as full of insane things—'absurdities, fictions, effronteries, and a thousand similar outrages'. Riccioli, a Jesuit astronomer in Bologna, denied that he or his friends Grimaldi and Gassendi had ever observed on or close to Venus any globules in any telescope. The Jesuit polyhistor Kircher in a book of 1656 agreed with Riccioli that Fontana's report was less than convincing.

Yet if a different note could be struck, you could bet on there being someone to strike it. Andreas Tacquet, a Jesuit situated in

Antwerp, suggested in 1669 that the telescopes of Riccioli, Grimaldi, and Gassendi may have been inferior in quality to Fontana's. Two other authors later in the century, Johann Zahn and Otto von Guericke, looked with favor on Fontana's Venusian moons but like Tacquet made no effort to put Fontana's claim to observational test. Christiaan Huygens, one of the pioneering geniuses of 'the century of genius', on reading Fontana's observation-claim, was open in 1656 to the possibility that Venus had a moon or moons but forthwith proposed searching for it or them. After three years of observing Venus again and again, he concluded in 1659 that Venus was without a companion.

In 1672 and again in 1686, Jean Dominique Cassini (Cassini I) saw what he considered might be a Venusian moon. This first of the astronomical Cassinis had been invited in 1669 to migrate from Bologna to France and become the director of the new Observatoire de Paris; he would remain in that role till his death in 1712. On 28 August 1686 at 4:15 a.m., while observing Venus, he saw, at a distance of $\frac{3}{5}$ of Venus' diameter, a luminous appearance that seemed to have the same phase as Venus, which was then gibbous on the western side. The diameter of this object was about $\frac{1}{4}$ that of Venus. He had seen a similar phenomenon on 25 January 1672 from 6:52 to 7:02 a.m., when Venus was horned and the luminous appearance was of the same shape and distant from Venus' southern horn by a diameter of the planet.

> I was in doubt whether it was or was not a satellite of Venus, of such a consistence as not to be very well fitted to reflect the Sun's light... But in spite of some research I have done from time to time after these two observations, in order to complete a discovery of such great importance, I have never succeeded in seeing it except these two times; and this is why I suspend my judgment.

The foregoing report of the observations of 1672 and 1686 was first published, according to Kragh, in 1730, which does not explain how David Gregory came to refer to Cassini's two observations in his *Astronomiae physicae et geometricae elementa* of 1702. In the English edition of this work, published in 1736, Gregory expressed the opinion that Cassini's results gave 'more than a bare Suspicion to incline us to believe that Venus has a Satellite'.

Cassini's observations are the first of several in which the supposed satellite is seen with the same phase as Venus. The parallelism of phase was to be expected if the luminous appearance was indeed a satellite, since the satellite and Venus were being illuminated by the Sun's rays at very nearly the same angle. But the parallelism was also to be expected if the luminous appearance was a secondary reflection of the primary image arriving at the ocular. Kragh tells us [122], that in 1881 the British astronomer William Frederick Denning directed his telescope at Venus and saw two crescents. Denning immediately thought of the reputed observations of a satellite of Venus and readily came up with an explanation in terms of reflections in the telescope. A similar explanation had already been put forward in 1765 by the Jesuit astronomer Maximilian Hell in his *De satellite Veneris*, as Kragh reports [80ff].

Another observation of the putative satellite sharing a phase with Venus was due to James Short, a Scotsman who had settled in London and made a reputation as a manufacturer of reflecting telescopes. On the morning of 3 November 1740, while observing Venus, he saw a luminous object some $10^\circ 02'$ to the west of the planet:

> ... I put on a magnifying Power of 240 times and, to my great surprise, found this Star put on the same Phasis with Venus. I tried another magnifying Power of 140 times, and even then found the Star under the same Phasis. Its Diameter seemed about a Third, or somewhat less, of the Diameter of Venus; its Light was not so bright and vivid, but exceeding sharp and well defined [31, quoting from *Philosophical Transactions* 41 for 1739–41].

Short looked for the object during the following mornings, 'but never had the good fortune to see it again'. The French astronomer Lalande, visiting with Short in March 1763, concluded that Short at that time did not believe in the existence of a Venus satellite [33]. Perhaps the failure during the Venus transit of 1761 of the putative satellite to show itself as a black dot against the bright background of the Sun had destroyed any lingering hope Short may have had that the satellite would prove itself genuine.

The elusive satellite was next sighted by Andreas Mayer of the University of Greifswald in Pomerania (Northern Germany), on 20 May 1759 at $8\text{h}45'50''$. Above Venus, Mayer saw

> a little globe of far inferior brightness, about 1½ diam. of Venus from herself. . . . The observation was made with a Gregorian telescope of thirty inches focus. It continued for half an hour, and the position of the little globe with regard to Venus remained the same, although the direction of the telescope had been changed.

The diameter of the little globe, Mayer later reported, was about ¼ that of Venus. At the time of the Venus transits of 1761 and 1769, he continued his observations but evidently did not see the putative satellite again. In 1762, he wrote: 'Whether or not this satellite belongs to Venus, I do not dare to claim.'

Mayer's was the last sighting of the alleged satellite before 1761. In June of that year, Venus was predicted to transit the Sun's face—the first Venus transit since 1639 (when a transit predicted by Kepler was observed by Horrocks and Crabtree)—and a second transit was to occur in 1769. The transits were important, Halley had announced, for making possible the determination of a more precise value of the Sun's horizontal parallax, that is, the Sun's distance in terms of the Earth's radius. During 1761, the astronomical interest in Venus was high, and Kragh finds that during that year the putative Venus satellite was sighted some 19 times. It was sighted nine more times in the period 1764–1768 and then, Kragh tells us, 'it was over': the sightings ceased.

Observing in Marseille, the Jesuit astronomer Louis Lagrange (not to be confused with the mathematician Joseph-Louis Lagrange) sighted the putative satellite three times during the period 10–12 February 1761. He was using a six-foot refracting telescope made by Short. The object exhibited no phase, such as seen by Cassini and Short. Also, it appeared to be following a path perpendicular to the ecliptic, a result so surprising to Lagrange that, according to Lalande, 'he did not find it difficult to abandon all the consequences which he had drawn from these observations.' In letters later written to Maximilian Hell, Lagrange made it clear that he did not believe that what he had seen was a satellite of Venus.

Another French astronomer, Jacques Montaigne, importuned by a young civil servant named A. H. Baudouin to make a search for the satellite, set about looking for it from his station in Limoges. He succeeded in sighting it four times between 3 May and 10 May 1761. On

3 May at 9h30m he saw it as a faint crescent situated as was Venus' crescent, about ¼ of the latter's size, and about 20′ from Venus. Baudouin submitted a report to the Académie des Sciences claiming that Montaigne's observations constituted a genuine and important astronomical discovery, thus confirming the original discovery by Cassini. Baudouin had not himself seen the moon but was confident that it existed and predicted that it would be seen moving across the disk of the Sun on 6 June 1761, the day of the transit. On that day, Baudouin observed the Venus transit with Charles Messier, a well known discoverer of comets, but they saw no satellite.

A number of astronomers observing the transit on June 6 looked for the satellite and reported their failure to see it. They included Lacaille in Paris, Cassini de Thury (Cassini III) observing with Liesganig in Vienna, Pingré with his assistant Thuillier on the island of Rodrigues in the Indian Ocean, the Swede Bengt Ferner observing near Paris, Samuel Dunn observing near Chelsea, and John Winthrop at Harvard College.

On the other hand, the amateur astronomer Abraham Scheuten, observing in Crefeld, Germany on 6 June 1761, claimed 15 years later that he had seen both Venus and its satellite on the Sun [55]. Similarly, an anonymous Englishman observing in St Neots (west of Cambridge) claimed to have seen the satellite on the Sun during the Venus transit [55–56]; and so did a Danish amateur astronomer, Friedrich Artzt, observing on Zealand [62–63]. The St Neots observation was reported right away [55–56] but the Artzt claim was set forth only 52 years after the event. The long delays in the Scheuten and Artzt reports detract from the confidence that we may be inclined to have in their truth; nor is the amateur status of Scheuten, Artzt, and the St Neots observer reassuring. We acknowledge these contrary reports, but is there anything we can do about them? Kragh maintains a noncommittal attitude toward not only these claims but all the observation-claims which he reports. Science, however, has to move on and cannot be brought to an indefinite standstill in the face of reports for which there is no direct way of confirming or disconfirming.

After the transit had occurred, an assistant at the observatory of the University of Copenhagen, Peder Roedkiaer, believed that he saw the Venus moon on several occasions between 28 June and 1 December 1761. The director of the observatory, Christian Horrebow,

did not report these observations, perhaps doubting that Roedkiaer was actually seeing the satellite. Three years later, on 3 and 4 March 1764, Roedkiaer again saw the satellite, and on the following March 11 Christian Horrebow himself saw it. He believed that he was seeing exactly what Cassini had seen in his two observations of 1672 and 1686. He made tests, seeking to ensure that the appearance was neither a star nor an illusion due to secondary reflections. Again, in January 1768, Horrebow and two assistants once more saw the satellite. But this was the last time.

The first extended critique of the observations of the presumptive satellite was undertaken by the Hungarian-born Jesuit astronomer Maximilian Hell (Miksa Höll in Hungarian) in his *De satellite Veneris* of 1765, mentioned previously. Hell had carried out a series of optical experiments. He found that, under certain conditions, he could always produce a spurious satellite, not only of Venus but also of Mars or Jupiter. The conditions included a special position of the eye relative to the eyepiece of the telescope tube and as well, a slow, careful motion of the eye. The image was formed, Hell believed, by a twofold reflection, first from the convexity of the cornea and then from the concave face of 'the meniscus lens or . . . the eyeglass' (Kragh does not explain exactly where these lenses are in the telescope; a diagram would have been helpful). Hell was able to see the satellite through two Gregorian telescopes, but never through two much better Newtonian reflectors. According to Kragh [83], Hell's analysis was widely accepted in the literature of the late 18th and the 19th century. Doubters there were, though few.

Hell passed through Copenhagen in May 1768 on his way to Vardø to observe the Venus transit of 1769 and took the occasion to have a conversation with Horrebow, who proved agreeable to the conclusion that the satellite was an illusion [67]. Whatever faith Horrebow may have had in his observations of the satellite in 1764 and 1768 had either evaporated in the meantime or been blown away by Hell's strong contrary conviction.

The Jesuit Roger Boscovich, in a treatise of 1767, explained the sightings of the Venus satellite in the same manner as Hell, that is, as caused by reflections from the eye's cornea and from a lens in the ocular of the telescope. Boscovich writes as if entirely unaware of Hell's treatise, although he and Hell used the same publisher [84–86].

An entirely different explanation was proposed by J. J. d'Ortous de Mairan (1678–1771), a late Cartesian who figured importantly in the affairs of the Paris Académie des Sciences in the middle years of the century. In the 1730s, de Mairan had published an account of the *aurora borealis*, explaining it in terms of a solar atmosphere extending out to the Earth. In 1764, he proposed accounting for the non-appearances of the Venus satellite as due to a thickening of this same atmosphere [77–79].

J. H. Lambert in three publications in 1773, 1775, and 1776, without assuming as certain that the Venus satellite existed, determined from the data supplied by Baudouin, Roedkiaer, and Horrebow a mean distance from and period about Venus, and calculated that on 1 June 1777 it should be possible to see the satellite on the face of the Sun, although Venus would be passing above the Sun at a distance of 15′. Observers in Berlin, Vienna, Paris, Stockholm, and Copenhagen looked for the satellite on the appointed day but it failed to appear [87–93].

William Herschel, discoverer of the planet Uranus, looked for the Venus satellite in November 1789 and concluded that if it existed, it must be less bright than a star of the 8th or 9th magnitude. J. H. Schröter, with his 27-foot telescope at his private observatory of Lilienthal, observed Venus for 15 years but never,

> in spite of all attention, found the slightest trace of either a real satellite or, in any telescope, a deceptive secondary image such as the late Father Hell thought [supposed] in his treatise of 1766. [96–97]

Fully a third of Kragh's book is devoted to the 19th-century authors who found the subject still of interest, some sure that no satellite existed, others urging that the earlier observations could not all be attributed to illusion.

Passing over most of the 19th-century writers whom Kragh gives an account of, we mention the endeavor of John Craig, an amateur astronomer and retired country clergyman, who in 1852 erected in Wandsworth Common, London, the world's largest achromatic telescope, 85 feet in length. Craig proposed using the telescope for the study of Saturn's ring system and to settle 'the old question of Venus' moon'. The telescope proved a costly failure and was dismantled a

few years later without contributing to the knowledge of Saturn's rings or the Venus moon question [108–109].

We mention also F. Schorr's *Der Venusmond und die Untersuchungen über die früheren Beobachtungen dieses Mondes* [1875]. Schorr was confident of the existence of the satellite:

> The Venus moon belongs to the citizens of our solar system; new observations, more precise than the earlier ones, will eventually prove its existence without doubt and provide means to determine its orbit with such accuracy that [as] is required by the present state of science. [112–113]

Schorr's explanation for the moon's not having been observed over the preceding century was that it reflected very little light. What could he say now, 135 years since the publication of his book, during all which time the satellite has still not reflected enough light to be seen?

Closure came, according to Kragh, in the 1880s with the publication of Paul Stroobant's 'Études sur le satellite énigmatique de Vénus' [1887]. Stroobant reproduced the central parts of the relevant sources from Fontana to Horrebow in their original languages and discussed all the hypotheses systematically. He stated his main conclusion as follows:

> In brief, we can say that the satellite of Venus does not exist, and when there was no false image or optical illusions, we find for the best observations a star corresponding almost exactly to the different observed positions.

For example, Roedkiaer on 4 August 1761 saw an object which he assumed to be the Venus moon, then noticed another star-like object nearby which he judged a better candidate. Stroobant by comparing the positions of the two objects with Argelander's star catalogue was able to identify them as 64 *Orionis* and 62 *Orionis*.

Kragh describes Stroobant's conclusion as 'somewhat cavalier', but adds [131]:

> since it was accepted by nearly all astronomers this is beyond [beside?] the point.

Kragh's endeavor in this book is to give an account of the Venus-moon affair that is as complete as possible. His examination of the

literature has been extensive and it seems unlikely that he has failed to mention any incident or person relevant to the putative Venus moon. He provides an extensive bibliography, identifying the many primary sources which he has consulted. He includes biographical sketches of the 23 chief characters in his story.

The present reviewer is restive with the restrictions Kragh sets for himself:

> We have not tried to determine what really was seen in the various observations, that is, to determine whether Mairan, Hell, Stroobant, Thirion or other post-1760 commentators were right or not; nor do we believe that such an undertaking would be historically fruitful (it may be of a certain astronomical interest, but that is a different matter). [147]

But, to this reader, it seems awkward and artificial to divorce the history of an episode in the pursuit of scientific knowledge from the scientific knowledge that is being sought.

Kragh in his avoidance of the scientific questions that engaged his characters does not seem aware of some of the consequences of the premises which he accepts. If no Venus satellite exists, then whenever the putative satellite appeared as sharing a phase with Venus, the possible explanations of the appearance become quite limited. The only celestial objects that are observable from Earth and show phases, aside from Venus itself, are the Moon, Mercury, and Mars; and Mars can exhibit only a gibbous phase, never appearing halved or crescent. The periods of all three of these bodies have been well known for centuries and their whereabouts at any time is readily ascertainable, so that their possible role in any appearance of the supposed satellite sharing a phase with Venus can easily be eliminated.

The satellite was seen sharing Venus' phase by Cassini in 1672 and 1686, by Short in 1740, by Andreas Mayer in May 1759, by Jacques Montaigne in May 1761, and presumably by Christian Horrebow in March 1764. These instances can only be accounted for, I believe, by an explanation of the type put forward by Maximilian Hell and Roger Boscovich.

The kind of explanation that Stroobant proposed seems appropriate where shared phases are absent and we find an observer like Roedkiaer choosing first one star then another as his candidate for the

satellite. In Stroobant's conclusion quoted above, he explicitly allows that some of the appearances are to be explained as illusions, presumably in the manner of Hell's and Boscovich's analysis. In the Roedkiaer case cited above, Stroobant's explanation is not merely a 'favorite' but the best explanation, given what we know of the circumstances.

Scientists are fallible human beings, bringing with them in their quest opinions and tendencies that may later prove wrongheaded. *Humanum est errare.* But the glory of it is that now and then evident progress is made. I suspect that it is so in the present case, where Maximilian Hell proposed accounting for certain misleading images by secondary reflections and Stroobant showed how certain other appearances of the presumptive satellite were better explicable in terms of 19th-century knowledge of the stars.

It would be good to have a detailed explanation of how the illusion was produced in Cassini's telescope (a refractor with two convex lenses?) and in Short's telescope (a Gregorian reflector?). But, alas, that would require experimentation—and a lot more work!

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