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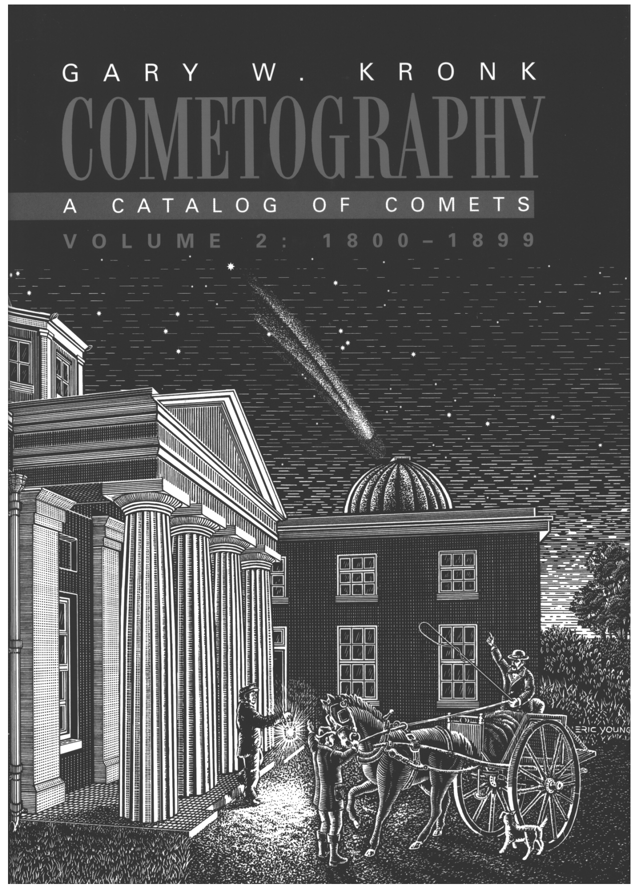
Book Review

Cometography: A catalog of comets. Volume 2: 1800–1899, by Gary W. Kronk. Cambridge University Press, 2003, 837 pp., $185, cloth (ISBN 0521-58505-8)



The first volume in this projected four-volume set was reviewed in 2000 by Walter Huebner in MAPS vol. 35, pp. 1399–1400. Since this second volume is half again as large as its predecessor, yet in describing just the comets of the nineteenth century it covers a timespan that is 25 times shorter, one may idly wonder how much space will be required to address the comets of the past few years.

Certainly, the nineteenth century, here defined as running from 1800 through 1899, was a time of significant change in our knowledge of comets, both observationally and with regard to their orbits. Initiated by Charles Messier around 1760, comet hunting as a competitive "sport" was taken up both by staff members of established observatories and by rank amateurs, with the latter often as a result being elevated to the former. In 1892, one such person, Edward Emerson Barnard, then at the new Lick Observatory in California, made the first discovery of a comet photographically—a technique that continued to compete with traditional visual searches using telescopes in backyards around the world pretty much throughout the twentieth century.

The nineteenth century also saw significantly improved instrumentation, notably in the optical quality of ever larger telescopes, the construction of micrometers and the accurate measurement of time, supported by the compilation of reliable catalogues of positions of stars to which the positions of comets could be referred. This revolution in astrometry was accompanied by one in celestial mechanics, as new methods were developed for the calculation of cometary orbits, the analysis of observational errors, the step-by-step allowance for perturbations by the planets, and an early appreciation that the motion of at least one comet, 2P/Encke, seemed also to be influenced by something other than gravitation. The chance appearance at perihelion during the 1860s of comets with orbits obviously shared by three of the most celebrated meteor streams (despite the fact that the orbital periods range from 33 to more than 400 years) clearly established an association between these phenomena. Sure, nineteenth-century orbit computations had to be done in the absence of the fruits of modern electronic technology with little by way of assistance but a table of logarithms or of products of pairs of multi-digit integers, but many of those works still stand as masterpieces of ingenuity and dedication.

Kronk mentions that the first spectroscopic observations of a comet were made in 1864, the features observed being recognized by William Huggins as emissions of the Swan bands of carbon some four years later. Sodium emission was first identified in the spectrum of the great comet of 1881, while Copeland and Lohse correctly noted iron lines in that of the great 1882 sungrazer. Nevertheless, this information cannot be gleaned from Kronk's book, despite his allocation of a total of 24 pages of dense text concerning how long a tail was measured by this observer on such a date and how difficult it was for that observer to see any trace of the comet in bright moonlight a week later. Furthermore, for all the nineteenth-century knowledge of spectra and orbits, an understanding of the true nature of comets and their place in the universe was almost non-existent. The ideas that comets are largely ice and that most of them are now located at the outskirts of the solar system at any particular time did not begin to take hold until 1950, and the deduction that the likely birthplace of comets of both long and short period is just beyond the orbit of Neptune is even more recent.

How well do Kronk's new efforts compare with those of others? Certainly, there is here much more detail than in his earlier “Descriptive Catalog,” which was widely criticized because there were no references. It is also not surprising that some of what Kronk writes now represents only a slight rewording. When direct quotes appeared in the 1984 work they are usually repeated here, such as the story, now attributed to Joseph Ashbrook, concerning a visitor’s complaint to the director of a recently refurbished observatory that comets were being discovered elsewhere. The director then turned to his assistant and said, “You see, Mr. Wells, you must discover a comet,” which Mr. Wells indeed did within a week. Perhaps understandably, such quotes are quite absent from S. K. Vsekhsvyatskij’s “Physical Characteristics of Comets,” which, despite the prevalence of typographic errors, particularly in the translation from Russian to English, must be considered the definitive descriptive account hitherto of cometary observations—with references—until the 1950s (and later, in supplements not translated into English). But, unlike Pingré’s famous 1783–1784 “Cométographie,” which was certainly not superseded by Kronk’s rather disappointing volume 1, Vsekhsvyatskij’s compilation is now losing significance.

But Kronk is less satisfactory when he describes orbital results. Here, Galle’s “Verzeichniss der Elemente der bisher berechneten Cometenbahnen,” quite unmentioned by Kronk, continues to stand supreme, with its extensive collection of variant orbits for each comet, at least until its 1894 publication date. Like the Pingré volumes more than a century earlier, Galle’s account of the orbital information is masterly and is a model of completeness and reliability for what this reviewer would like to have included in his own published catalogues of cometary orbits.

As an example of where Kronk’s account is lacking, consider the comet now known as 27P/Crommelin. It appears in Kronk’s volume 2 in the guise of both comet P/1818 D1 = 1818 I and comet P/1873 V1 = 1873 VII = 1873g. Although Pons was given credit for the 1818 discovery, there would have been no publication of observations without the assistance of von Zach (unmentioned by Kronk but known to have attempted to apply adjustments to the very rough information by Pons, who apparently gave the wrong date for one of the four observations anyway) and Encke (whose complete failure to make any sense of the data Kronk does acknowledge). Cycle forward now to 1850 and Pogson’s computation of a parabolic orbit. What Kronk does not say is that Pogson acknowledged that his computation was done from a new reduction by Hind of three of Pons’ data points and, further, that the orbit was very similar to that of the comet of 1772, by then known to be an early appearance of comet 3D/Biela. Next in historical sequence came an 1868 Viennese paper in which Weiss suggested that the 1818 comet was a third fragment of the double comet 3D/Biela, the latter pair known to have been positioned in the 6.7-year-period orbit a full year away from where the 1818 object would have been. It was an 1872 discussion of the Weiss paper by A. S. Herschel in England that prompted his countryman, Hind, to complete the work Pogson had done in 1850 from Hind’s own data reduction. Kronk indeed refers to the resulting paper by Hind, whose quote about “no possible connexion” with 3D/Biela should be viewed in the light, not that (as Kronk implies) it was initially Hind’s idea that there was a connection but that he had considered Weiss’ suggestion of another fragment. It is quite incorrect to attribute the additional fragment idea to Weiss in 1873 because Weiss’ 1873 paper (for which Kronk incorrectly cites a later paper published by Weiss early in 1874) was written after the 1873 rediscovery, by Coggia and Winnecke one day apart, of the 1818 comet. In fact, as Kronk notes under his entry for the 1873 comet (which, at the time, was credited in most publications only to the first discoverer, Coggia), it was clearly Weiss and Hind who were the first to suggest the identity of the two, already on the basis of the first three nights of accurate measurements of the latter. Curiously, when Crommelin wrote about this in 1929, he incorrectly gave first credit for this to Argelander (who made the connection several days after Weiss and Hind on the basis of an orbit by Fabritius), then referred only to the later 1874 paper by Weiss—and gave no credit to Hind at all. The significance of Weiss’ later paper is that this astronomer first computed elliptical orbits for the comet, making representations of the 1818 observations on the assumption that the 1873 observations could be satisfied using either the full 55.82-year interval between them as the orbital period or one-eighth of this (or 6.9775 years); the latter was a complete guess that would, of course, be more representative of a typical short-period comet (like 3D/Biela). Weiss also fully expected that observations made a month after the 1873 discovery would restrict the choices. Most unfortunately, as in 1818, the observations were terminated after rather less than one week. Kronk correctly notes that, by late 1875, Weiss was favoring the idea that there were not eight revolutions between 1818 and 1873 but nine, or perhaps also three, or just one. Not mentioned by Kronk is Berberich’s suggestion of ten revolutions (or a 5.58-year period) and his provision of a predicted ephemeris for early 1885 (the intervening return having placed the comet behind the sun). Kronk mentions the calculations on the comet by Schulhof (for which he consistently used the name Coggia-Winnecke), but he really does not give justice to this immense work, published between 1885 and 1892. Contrary to what Kronk says, Schulhof did consider that the number of revolutions between 1818 and 1873 might be any integer from one to ten, and while recognizing the enormity of the task of checking out all these possibilities in full detail, he did make an attempt to judge what planetary perturbations the comet would experience in each case. This included the two-revolution period that is now known to be correct. Although he was obviously hasty in rejecting such a solution, Schulhof clearly demonstrated a rationale for his choices, which favored nine, six, one, and three revolutions. Ultimately, of course, his rationale failed, but Kronk's curt dismissal that "the 1818 and 1873 observations were just too rough" (the 1873 observations were not "rough," they just did not cover enough time) and that Schulhof "never derived the correct period" (could anyone else have done so?) is unfair. Moreover, Kronk's statement that a 55.8-year period would allow the comet to be linked to one observed in January 1457 is patently incorrect. By an interesting coincidence, some remarkably accurate drawings of that comet by Toscanelli depicting its changing positions with respect to identifiable stars had just come to light in 1884, allowing Celoria to compute an orbit—as Kronk mentioned in his volume 1. What Kronk did not mention there was that it was Schulhof who quickly recognized a similarity to the 1818–1873 orbit and, indeed, considered identity (using an appropriate period) long before Crommelin got into the act. What did bring Crommelin into the act was the further accidental rediscovery of the 1818–1873 comet by Forbes in 1928, for it was he, and also Smiley, who suggested the identity. This time there were enough observations made to establish in a matter of weeks that the period was half of the interval since the 1873 passage, i.e., 28 years—a result that, in his 1873 entry, Kronk appears to attribute for the first time to Yeomans and Chodas in 1986. It was Crommelin who introduced the name Pons-Coggia-Winnecke-Forbes for the comet, and in his own extensive work on this comet, he was careful to acknowledge the contributions of Schulhof, who did not have the fact that there were two revolutions between 1818 and 1873 handed to him on a plate. Crommelin could obviously provide the first successful prediction for the comet's return, in 1956, by which date the International Astronomical Union had renamed the comet for him. The 28-year period made it reasonable for Crommelin to consider that the 1457 comet belonged (with 13 revolutions between then and 1818), and he also felt that there were some observations of the comet in 1625. Kronk discusses that 1625 comet in his volume 1, correctly quoting this reviewer as "impressed that the apparitions in 1818, 1873, 1928, and 1956 could be fitted without nongravitational forces." He mentions his own calculation of a resulting "gravitational" perihelion date that misses the observed 1625 date by three months, but he does not mention running the calculation back to 1457. This reviewer did both calculations many years ago and found an even larger discordance in 1457, suggesting, as a consequence, that both identifications are invalid. This was before the comet's 1984 recovery showed that nongravitational forces were detectable (another point Kronk does not mention). Certainly, the forces are small, but that could change with time, and one should perhaps not be too hasty to drop the proposed linkages.

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