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Küstner's Observations of 1884–85: the Turning Point in the Empirical Establishment of Polar Motion

Dedicated to Professor Friedrich Gondolatsch at the occasion of his 95th birthday.

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Abstract. Since Euler in 1765 predicted the possibility of polar motion, the search for an observational confirmation went on. While some indications were found before, the proof without doubt is generally attributed to Küstner. Ironically, the aim of Küstner's precise observations of 1884–1885 in Berlin was another one, namely an improved value of the constant of aberration. Using the Horrebow-Talcott method, he could conclude that the latitude of Berlin had decreased significantly by $0\rlap{.}''2$ in about one year. His findings promoted the start of international cooperation to observe the polar motion. Measurements from Honolulu in 1891 exhibited the effect in counterphase, thus omitting the last chance for a local explanation.

Karl Friedrich Küstner (Fig. 1) was born on 22 August 1856 in the then Silesian town of Görlitz, today divided between Poland and Germany. He studied since 1875 in Berlin and went in 1876 to Strasbourg, where he got his Ph.D. in 1879 with a dissertation on the lunar radius from a compilation of occultation observations. For 3 years he was an assistant at institutions in Berlin and became an "Observator" in 1882 at Hamburg Observatory (at that time, this was a position next in rank to the director). He and Auwers observed the Venus transit in December 1882 from Punta Arenas in the Straits of Magellan. (The transport of precise time from Montevideo required several dangerous trips!). Since 1884 he was an Observator in Berlin, and it is this time on which we are going to concentrate later. In 1891 he was called to the chair of astronomy and the directorship of the university observatory at Bonn. In Bonn, he was engaged in meridian circle work for improving the fundamental system, basic spectrographic topics and photographic astrometry. His catalogue of 1908 is the first one which extends the fundamental system towards faint stars up to $9\rlap{.}^{m}5$ (Fricke 1983). We are even using today his heritage, since his plates are of extraordinary quality. Because the 5m double refractor is still working, we can use plate pairs with about 90 years epoch difference. On this basis, we have contributed to the extragalactic calibration of the Hipparcos system and determined the proper motion of globular clusters within this system.

In 1910 Küstner received the Gold Medal of the Royal Astronomical Society. In 1916 (in the middle of World War I!) the German astronomer Auwers donated a gold medal named after the British colleague Bradley. Küstner was the only one to receive it since the funds vanished later due to inflation. He retired in 1927 and died on 15 October 1936.

Figure 1. Karl Friedrich Küstner (1856–1935) painted in 1931 by W. Fassbender (Universitäts-Sternwarte Bonn).

In his theory of rotating rigid bodies, Euler (1765) had seen the *possibility* of a free nutation of the body axis around the angular momentum axis. From then onwards astronomers must have been looking to see whether the possibility was "used" by the real Earth. They did not know the order of magnitude of the angles, but Euler had told them at least the period: 305 days, from the observed ratio of the moments of inertia.

Among those who tried to determine pole variations, Bessel was an early one. As a young assistant at Schroeter's observatory in Lilienthal near Bremen, he had in 1808 an eye on the latitude as an *error source* (Erman 1852, I, 182). As a professor in Königsberg, he received in 1814 from Lindenau at Gotha the news of a theory of Poisson describing a free nutation. Obviously he didn't know Euler's work. See the combined review of French academy papers by Lindenau (1810); later, Lindenau (1818) refers to Legendre. This message was enough for him to construct his own theory, coming up with a period of 335 days and clear consequences for observable quantities (in a letter to Olbers of 1814, Nov 7; in Erman 1852, I, 391f.) Olbers asked him to consider the effect of an additional mass increment (*loc. cit.* p. 399f) and Bessel fulfilled that request in a letter of 1817 Sept. 23 (Erman 1852, II, 61) and then in a paper showing that all the human activity (*e.g.* the shipments of goods from the Indies) had no measurable effect (Bessel 1818). Also in 1817, he tried to see the polar motion in azimuth measurements (Ševarlic, p. 17). And still later in letters to Humboldt of 1843/5 (Felber 1994) he expresses the strong suspicion — based on his own observations — that his latitude had varied by $0\overset{''}{.}3$. Only his illness since 1844 probably prevented him from reaching certainty (at his own level of quality).

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Since the whole correspondence is now published, one can follow how Humboldt was attracted by this opening of a new field. He had a great talent and a great interest to obtain such qualitatively new information from specialists, while in turn the specialists (at least of the type of Bessel) were hesitating to publish premature results.

Especially in observational sciences, the progress happens as *gradus ad parnassum.* It is difficult, and sometimes impossible, to distinguish one person as *the* one who made the greatest contribution. We are glad if the path to knowledge was not too erratic and looks more like a smooth arc tan-curve between zero and full understanding. It is in this sense that I use the word turning point in the title of my talk: whereas many highly suspicious indications of polar motion were known before Küstner, it was his work, according to many voices, which brought the topic to general acceptance as a truly existing effect. His finding met with great public interest, and among the scientific community the matter got into motion. To give a reference from this country and just before Küstner's work, I may quote Nobile (1885): "In questi numeri ed in questi curve á impossibile non riconoscere una determinata tendenza della latitudine a crescere verso il principio della seconda metà dell' anno ed una tendenza a diminuire verso la fine dell' anno."

As a recent witness, I cite Ševarlic (1957) who wrote "Küstner a ensuite remarqué que ces discordances pourraient disparaître si l'on supposait que les latitudes de Berlin, Poulkovo et Gotha varient périodiquement. Son mérite consiste en ce qu'il proclamé le premier d'une maniére irréfutable que les variations de latitude observées proviennent des variations de position de l'axe de rotation de la Terre dans sa masse á cause de la nutation libre, ...". Markowitz (1976) wrote "The variation of latitude was finally found by a German astronomer, F. Küstner, who wasn't looking for it ... Küstner's announcement opened up the field of polar motion study." Lambeck (1980) did not name the scientist in charge, but the meaning is clear : "Euler, in 1765, concluded from his analysis of the rotational motions of rigid bodies that the Earth's pole of rotation could undergo a 305-day oscillation with respect to the crust. Lambert *et al.* (1931) have reviewed the unsuccessful search by astronomers for evidence for such a variation in astronomical latitude. Only in 1884–1885 did the observations yield evidence for it: observations taken at Berlin indicated minute changes in the astronomical latitude."

Exemplary contemporary quotations include the review of Küstner and Nobile in the *Bull. Astr. V* (1888); Bauschinger's hope that more observations should become possible (*Vierteljahresschrift der Astr. Ges.* 23 (1888) 251); the report in "Die Fortschritte der Astronomie" (No. 14 1889, p. 39–44); and the popular but quite serious review in *Sirius* (Neue Folge Bd. XVII (1889), 64).

Among the work stimulated by Küstner's findings:

1. A confirmation was sought and found by independent observations in Europe.

2. Others in Honolulu made observations that finally excluded any local explanation (the original instrument used by Dr. Marcuse in the Hawaii expedition still exists in Potsdam at the Geodätisches Institut).

3. International cooperation achieved more and more of an institutional character (in this respect, Küstner's teacher Wilhelm Foerster may have been the predominant figure).

So what were the roots of Küstner's success in 1884/5? First, he used the Horrebow-Talcott-method with a Universal Transit at Berlin Observatory. (Markowitz prefers the second name alone, since only the use of levels and not relying on circle graduations specifies the method according to him; otherwise the method may even be retraced to Ole Rømer.) His original aim was an improvement of the constant of aberration. The Talcott method uses pairs of stars culminating near to, but on opposite sides of, the zenith. The instrument is turned by 180° between the two coherent observations. (It seems therefore appropriate that the photographic portrait of Küstner has also reversed sides in the paper of Markowitz (1976). In such a way the influence of refraction is minimized. Moreover, Küstner decided to execute a purely *differential* treatment. Besides the correction of the aberration, of instrumental constants and of terms due to stellar parallaxes, a kind of lump sum-unknown combined the correction of the declinations of the pair components and a possible latitude *variation.* Küstner was not looking for it but was forced to accept such a variation and obtained for the latitude change of Berlin

$$\Delta\phi(spring\ 1885 - -spring\ 1884) = -0\overset{''}{.}204 \pm 0\overset{''}{.}026(p.e.)$$

Küstner noticed the time-scale of the variation (order one year) but he insists that as a first step the *existence* had to be shown. Therefore he attributed the greatest weight to the difference of the *two spring values*, since differential effects of aberration, parallax and annual meteorological phenomena — *e.g.* in the refraction — should be negligible (Küstner 1890). This is valid for the errors due to proper motion in declinations at any time, since the proper motions are derived from observations with equal accuracy but much larger epoch differences. In retrospect, he could interpret Pulkovo observations analysed with somewhat contradictory results by W. Struve and Nyrén. Doing this, he commented on the Eulerian period of free nutation and on other causes of polar motion. In his view, the 10-month period does not exist at all, or is masked by other motions caused by meteorological mass shifts. (He quotes W. Thomson (1876).) This discussion (Küstner 1888, p. 52–54) disproves Markowitz's statement on p. 101 that Küstner "... did not remark on any period."

It is easier to find portraits of Küstner than of his instrument (Fig. 2). It had a focal length of 129 cm, an objective diameter of 115mm. It was designed by W. Foerster and built by C. Bamberg in Berlin. The principles were outlined by Foerster (1878/80). At the end of the second part of this theoretical essay, Foerster speaks of a possible third part on the practical aspects, saying that the new Universal Transit Instrument had been installated at the Berlin Observatory a few months ago. That 3rd part of the essay has probably been replaced by the detailed description of the instrument (Foerster 1880). In his memoirs (Foerster 1911 p. 307), he gives a paper as something like an extended third part: "Diese Zusammenfassung steht in nahem Zusammenhang mit der Praxis eines Instrumentes, welches ich im Jahre 1878 für die Berliner Sternwarte hatte bauen lassen, eines sogenannten Universal-Durchgangsinstrumentes, dessen Anwendung durch den Assistenten der Sternwarte, Dr. Küstner, die ersten s i c h

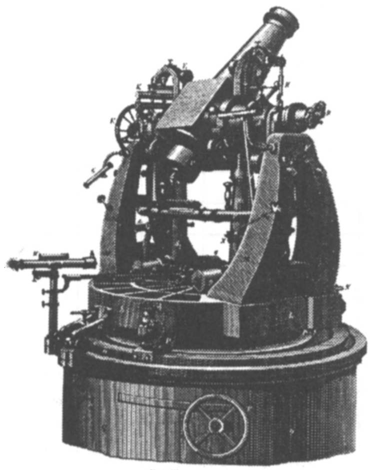

Figure 2. The instrument used by Küstner.

e r e n Anzeichen der kleinen Vernderungen der Lage der Erdachse im Erdkrper erkennen ließ... ."

Foerster had sent his collaborator Dr. A. Marcuse to Honolulu to observe in 1891/2 whether the latitude changes appear there in counterphase (Fig. 3), as they should if the one observed in Berlin were not only a local phenomenon (Foerster 1911, p. 205f.). The activity of Foerster for the foundation of an international service to observe the Earth's axis was successful (Foerster 1911, p. 191, 240f., 283) and the Geodätisches Institut in Potsdam was the first center of it. It is then completely understandable that Küstner, having followed the call to Bonn, did not try to compete with Potsdam and left this area of science to his former colleagues. He received and used, however, fresh values of the polar motion for the reduction of fundamental astrometric observations in Bonn. This can be seen, *e.g.*, in the dissertation of Carl Wirtz (1898), who became later the forerunner of Edwin Hubble in the detection of the expansion of the universe.

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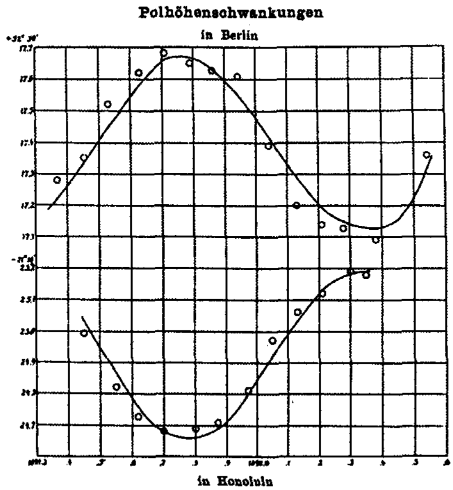

Figure 3. Antiparallel latitude variations in Berlin and Honolulu (from Przybyllok).

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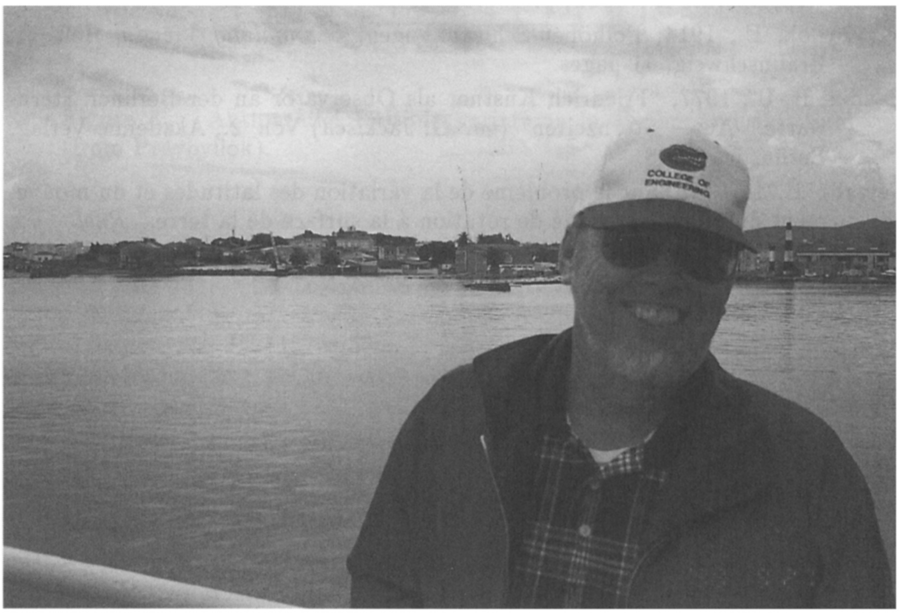

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