Olbers to Gauss

Bremen

October 10th, 1802

Translated from the German by Tarrajna Dorsey

The adjustment of Méchain's observations of ⚴ by your element IV, which you have sent to me, has given me much satisfaction. It seems entirely unnecessary to me for you to expend still more effort in finding more exact elements for the time being. I consider these to be accurate enough for pure ellipses, and the not inconsiderable effort of determining variable elements, will, it appears to me, be rightly deferred to the point in time where a greater arc can give these elements more certainty. The elliptical elements already calculated so exactly by you are adequate to find the small planet again in future years, in my opinion; and since they indicate the positions exactly, in all probability within $\frac{1}{2}^{o}$, the telescope can always be situated in a position where one is sure to find the planet somewhere in the field of the same, and then it depends only upon recognizing it amongst the many small stars. — This will never be easy work; for in the very region where we have to search for ⚴ in the coming year, in or near the Milky Way, there is a vast, innumerable quantity of smaller stars.

Should I fail to see the star observed on March 1st, 1797 according to the *Hist. Cél.*, I will inform you straightaway. On October 6th, at 4 o'clock in the morning, since I had to awaken an invalid, I have looked over the western region of the parallels, but failed to see any star, the eastern standing too low still, and the twilight too near.

I have finally observed the small comet on October 2nd. Since I do not know if you would like to make use of some of the observations, all of which, however, are not very exact, I thus impart them to you here from the 19th of September on.

		Times	Apparent Right Ascen.	Apparent Declination
Sept.	19	$8^h\ 30^m\ 15^s$	$255^o\ 33'\ 12''$	$26^o\ 4'\ 26''$
"	20	$8^h\ 6^m\ 20^s$	$255^o\ 49'\ 24''$	$26^o\ 52'\ 35''$
"	21	$7^h\ 48^m\ 4^s$	$256^o\ 4'\ 41''$	$27^o\ 40'\ 34''$
"	23	$8^h\ 4^m\ 2^s$	$256^o\ 35'\ 50''$	$29^o\ 11'\ 54''$
"	24	$8^h\ 52^m\ 38^s$	$256^o\ 51'\ 24''$	$29^o\ 52'\ 39''$
"	25	$8^h\ 53^m\ 0^s$	$257^o\ 8'\ 27''$	$30^o\ 34'\ 51''$
"	29	$7^h\ 16^m\ 20^s$	$258^o\ 13'\ 5''$	$33^o\ 6'\ 7''$
"	30	$8^h\ 43^m\ 38^s$	$258^o\ 29'\ 33''$	$33^o\ 42'\ 23''$
Oct.	2	$11^h\ 54^m\ 30^s$	$259^o\ 6'\ 16''$	$34^o\ 51'\ 23''$

An error appears in the last right ascension, which is however ascribable perhaps to the compared star in part. — For the sake of honor, I will calculate the orbit again, whether it is seen to be of no greater use. For since the elements which I have derived up to the 13th from the observations without all corrections still give an error within 5′ to 6′ for September 30th, the dimensions of the orbit are thus determined sufficiently exactly in order to recognize the comet again in a future appearance, and to be able to sufficiently estimate its latest course, distance from the Earth and ⊙, and mass, etc., and this however is indeed the actual aim of our parabolic comet calculation.

Herschel has had the kindness to send me a special copy of his treatise, *Observations on the two lately discovered Celestial Bodies.* His amplification was 516 times. — Next he gives ⚴ $110\frac{1}{3}$ miles in diameter, and says it were 31,000 times smaller than ☿ (remaining numbers were corrected thus, with ink).

Have you obtained Oriani's observations yet?

You, my good friend, have shown me the greatest courtesy with your explanation and commentary upon your method. My small doubts, objections, and hesitations are now lifted, and I now believe myself to have pretty much entered into the same spirit. Again I must repeat, that the more I become familiar with the whole operation of your method, the more you amaze me. — How you will aid us all, my most dear friend, if only you look after your health!

Your proof, that $D = \pm 2KL\cdot$ Area $\Delta\ P, P', P''$, and thus D is six times as large as the pyramid $KPP'P''$, I have been able to follow quite well, and this all the more easily, since the attractive theorem, that the square of the area of a plane figure is the sum of the square of the area of the three projections onto three arbitrary planes, perpendicular to one another, had appeared to me shortly before in my reading of Montulca, and I had sought out the proof myself as well. Montulca ascribes this theorem to Monge, or much more, to Fiuseau. But my dearest friend, the phrase

$$D = 6 \times \text{Pyramid } KPP'P''$$

seems to me to not be entirely *identical* to the one in your first treatise, "that $\varpi\varpi'\varpi''$ is the sixfold volume of a pyramid, whose apex falls in the midpoint, and whose vertices [*Winkelpunkte*] of the base fall on the surface of an inscribing sphere of radius one, such that they correspond to the three geocentric positions of p." Rather, I find that if the volume of this latter inscribed pyramid is called π, then

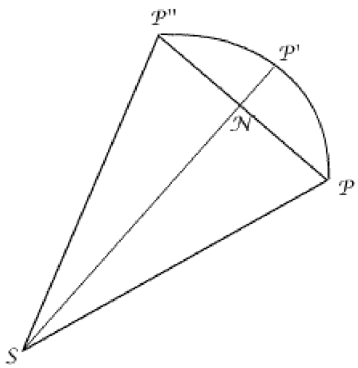
$$[\varpi\varpi'\varpi''] \neq 6\pi,$$

but rather

$$(\varpi\varpi'\varpi'') \cos\beta \cos\beta' \cos\beta'' = 6\pi.$$

If I am in error, please correct me. — By the way, I now understand your further statement as well, that the remaining signs express very similar things, and I know how to picture all of the pyramids.

You are entirely correct, I maintain, that your method were only then conveniently applied, if r' and R' differ greatly, and thus, that the apparent orbit is very curved, *was incorrect.* Though here, the error of Q depends only on the *arithmetic ratio*, not on the *geometric ratio.* However, it remains always a convenience to your method, as you yourself noted as well, if r' is much greater than R', and thus too great an arc of the Earth's orbit belongs to only a small arc of the comet's orbit.



But your method demands much more exact observations than my so much more imperfect comet method. I have sought to make this clearer to myself by making a small translation of your formula (7). If S is the sagitta $P'N$ of the Earth's orbit, s the sagitta of the comet's orbit, then with all rigor,

$$\frac{F + F' + F''}{F'} = -\frac{S}{R' - S}$$

$$\frac{f + f' + f''}{f'} = -\frac{s}{r' - s}$$

Now the assumption

$$\frac{1}{\cos\frac{1}{2}(v'' - v)} = 1, \text{ and } \frac{1}{\cos\frac{1}{2}(L'' - L)} = 1$$

is entirely analogous, just as much as when I set

$$\frac{S}{R' - S} = \frac{S}{R'} \text{ and } \frac{s}{r' - s} = \frac{s}{r'}.$$

Therefore,

$$F'(f + f'') - (F + F'')f' = F'f'\left(\frac{S}{R'} - \frac{s}{r'}\right)$$

and $\frac{F+F''}{F'} = 1 + \frac{S}{R'}$ (or to be wholly precise $= \frac{R'}{R'-S}$), consequently, equation (7) is called

$$\left(\frac{R'}{R'-S}\right)\frac{[\varpi\varpi'\varpi'']\delta'}{[\varpi P'\varpi'']R'} = \frac{S}{R'} - \frac{s}{r'} \;\ldots\ldots\; \text{(I)}$$

Thus, that which stands on the left hand side expresses the difference of the proportions of the sagittas to the middle *radius vectors*.

Now a small arc is known to be very nearly

$$s = \frac{SR'^2}{r'^2} \;\ldots\ldots\ldots\; \text{(II)}$$

and therefore will that formula

$$\left(\frac{R'}{R'-S}\right)\frac{[\varpi\varpi'\varpi'']\delta'}{[\varpi P'\varpi'']R'} = R'^2 S\left(\frac{1}{R'^3} - \frac{1}{r'^3}\right).$$

It is *perhaps* possible, that your formula (7) might have lost *something* of your precision. I only did it to see more clearly what are actually the determined magnitudes in your formula. For any given δ' you thus find from (I) the proportion of the sagitta of the comet orbit to the *radius vector*, or $\frac{s}{r'}$; r' is therefore determined by δ', and thus (II) gives the value of s from r', and consequently from (I) the value of r'. Therefore the great convenience of having a *parabolic* orbit is made clear. Here r' or δ' can be determined from the *chords*; if, however, a is not ∞ and is thus unknown, then the *sagittas* must be used. For any given r', the geocentric observations give the chords, and likewise, from the time intervals and r', the theory of gravity gives the chords *for the parabola*, and consequently, from the chords, an equation for r'. However, if a is unknown, not the chord, but only the sagitta can be found from r' and the time interval, and accordingly these can thus only be compared with those found from the observations. Now the chords are magnitudes of the first order, and the sagittas are only magnitudes of the second order, and so this case demands far more exact observations than the former.

If according to this, as you so correctly expressed, no *motivated* intention is had to determine the orbit independently of a hypothesis regarding the nature of the conic section which it forms, if, rather, the orbit can rightly be regarded as a parabola, then it seems to be *more convenient* to choose the determination from the chords. Admittedly, your method should be applicable to actual comets as well, if the observations are not too rough, just as with La Place's formula, which is so analogous to yours. This analogy lies, as far as I see, in this: you determine r' from the sagitta, or the distances of the middle positions of the Earth and the comet from the chords; La Place determines it from the distances of the three positions from the tangents.

Speaking candidly, I confess to you, that I do not believe the connection of your method with mine will facilitate the latter very notably; for the value of the unknown magnitudes are found very quickly by you as well, and with few attempts. — But how excellent, how indispensible your method is for such planetary bodies as *Pallas*! Look sometime in the September issue of the *M.*

C., at how the poor Burckhardt had toiled, before he could *guess* this orbit. I would not have fared any better either. — For ⚳ the case was something else to a certain extent; because the circular hypothesis could be used for it, which was completely unuseable for ⚴, except to find the distance and the position of the orbit approximately.

You have completely satisfied and instructed me that r' may not be used, and that Newton's method were not applicable in the case which you have in mind. I thank you very much; for I confess to you, that I had really not considered the matter from this *correct* viewpoint up until now, probably because I have always used only Newton's method when the triangle between three positions of the comet[1] was too large.

The more I consider your method to find the parameter from vv'', rr'', the more easy and attractive I find it to be; you have especially made the calculation far more convenient still in your last letter. — No, a series for a or k etc. can certainly never be as useful for the calculation. — As soon as I have the time, as well as the occasion, I will attempt to calculate an example according to your method, purely for my own practice; for at the beginning it may go for me similarly as with Scanderberg's sword[2]. Truly, the certainty and precision with which *you* now calculate according to your formula is very difficult for me. I miscalculate very easily, especially in small matters, which do not immediately stand out in the result.

Forgive me, my dear kind friend, for this long-winded epistle! Do not think that I fancy myself to have said something new to you about your method. I merely wish to show, that I have carefully studied it, and that you have a student who is eager to be taught in your friend.

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[1] "Δ *inter tria loca Cometae*" stands in the original. — [TAD]

[2] A well-known saying of the time, which goes something like: "Scanderbeg's sword must have Scanderbeg's arm," alluding to the story of the 15th century Albanian prince Gjergj Kastrioti, or, as known to the Turks, "Iskander-beg" (Prince Alexander), who sent a scimitar as a gift to his enemy, the Turkish Emperor Mahomet. However, the Emperor could not so much as lift the sword, and furiously sent it back, viewing it as an attempt to inspire fear. The prince calmly responded that he had merely sent his own sword, but not the arm which had wielded it in victorious war against the Turks. (Martim de Albuquerque's *Notes and Queries*, 1853.)