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A. Astronomy
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ON ENCKE'S COMET.

ENCKE'S DISSERTATION

CONTAINED IN

No. CCX AND CCXI

OF THE

ASTRONOMISCHE NACHRICHTEN.

TRANSLATED FROM THE GERMAN

BY

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Encke's Comet is undoubtedly one of the most remarkable bodies belonging to our system; and the conclusions which have been derived from its successive appearances are among the most important, with regard to the physics of the universe in general as well as to astronomical science in particular, which the present century has produced. The methods, by which the necessary calculations are made, have never been practically employed in this country, and are little known, even to those among us who are acquainted with the ordinary operations of Physical Astronomy. This Essay is, I believe, the first publication which contains a complete abstract of Encke's theory and its comparison with observation. If by circulating a translation I shall excite the curiosity of one reader to possess himself more completely of the theory and the facts of this singular body, I shall think my trouble well repaid.

The translation is almost strictly literal. The references (except those to Argelander) have all been verified.

Observatory, Cambridge,
Jan. 2, 1832.

G. B. AIRY.

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TRANSLATION, &c.

On the next return of Pons' Comet in the year* 1832, *with a survey of the grounds on which the new elements rest.*

By PROFESSOR ENCKE.

THE approaching return of Pons' Comet imposes on me the duty of giving the hitherto unpublished comparison of the very numerous observations of its last Appearance, together with a prediction of its course in the coming year (1832.) I avail myself of this opportunity to join at the same time a short review of the course I have followed in all the computations respecting this Comet: this will shew with what confidence we may hope to see the calculation agree with observation.

The observations have been compared immediately with the elements which served for basis to the last prediction, by means of an Ephemeris computed from those elements

* Encke's modesty describes the comet, which we justly call Encke's, by the name of its discoverer Pons. EDITOR.

This comet was first seen by Mechain and Messier in 1786, but they observed it only twice, and were therefore unable to determine the elements of its orbit. Miss Herschel discovered it in 1795, and it was observed by several European astronomers. In 1805 Pons, Huth, and Bouvard discovered it on the same day. In 1819 Pons discovered it again. Hitherto it was supposed that the four comets were different, but Encke (Bode's Astron. Jahrb. 1822) not only pointed out their identity, but shewed that an elliptic orbit agreed better with each set of observations than a parabola. In Bode's Astron. Jahrb. 1823 (published in 1820), Encke gave new calculations of the perturbations, &c., and, as there still appeared to be some unknown cause of uncertainty, he gave two ephemerides for its appearance in 1822. This was observed by Rumker in New South Wales: and Encke after discussing his observations in the Astron. Jahrb. 1826, concluded that the supposition of a resisting medium was necessary to reconcile all the observations. The comet was again generally observed in Europe in 1825 and 1828: and the circumstances of the last appearance were particularly favorable for determining the influence of Jupiter's mass and the absolute amount of the retardation, which the other observations had left undetermined. TRANSLATOR.

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with the utmost accuracy. In order to perceive more readily how individual observations vary from the Curve which, *after* using the Perihelion passage of 1828, is taken as the most probable, I subjoin a small table which gives the quantities that must be added algebraically to the errors given below, in order to get the difference from the orbit now considered the best.

Correction of the predicted Ephemeris for the Year 1828.

1828 Mean Paris Time.	Geocentric Right Ascension.		Geocentric Declination.	
Oct. 24,3	+ 2′ 39″,6	8″,5	+ 0′ 27″,3	5″,4
28,3	2 48,1	7,0	0 32,7	5,3
Nov. 1,3	2 55,1	5,5	0 38,0	5,1
5,3	3 0,6	4,6	0 43,1	5,1
9,3	3 5,2	3,6	0 48,2	4,9
13,3	3 8,8	3,3	0 53,1	4,8
17,3	3 12,1	4,2	0 57,9	4,4
21,3	3 16,3	5,4	1 2,3	4,2
25,3	3 21,7	8,4	1 6,5	4,6
29,3	3 30,1	10,5	1 11,1	4,9
Dec. 3,3	3 40,6	13,9	1 16,0	6,7
7,3	3 54,5	16,7	1 22,7	9,5
11,3	4 11,2	18,8	1 32,2	10,8
15,3	4 30,0	17,8	1 43,0	10,4
19,3	4 47,8	15,3	1 53,4	9,4
23,3	5 3,1	8,4	2 2,8	7,5
27,3	5 11,5		2 10,3	

This table shews also how much the former prediction differed from the truth. The error originated chiefly from the time of perihelion, and upon this account increased as the observations came nearer to the time of the perihelion passage.

The most accurate and in every respect the most important observations, whether in regard to the extent of time which they embrace, or the excellence of the instruments and the care with which the same point of the comet was always taken, or the accuracy with which the compared stars were determined, are those made at Dorpat by Professor Struve,

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to whom Astronomy stands indebted on so many accounts. They appear to be the first example of the practicability of observing a comet with the same precision as planets and fixed stars, or at least as nearly so as the extreme faintness of the comet at the beginning of the observations, and its unfavourable position near the horizon at the end, would in any way permit. The original data are already given in this Journal. From them are deduced the following mean places of the stars; the results of the different observations, according to Struve, agreeing very closely.

Compared Stars.

Name.	Mean Right Ascension. 1828.			Mean Declination. 1828.			Magnitude.
w	19^h	1^m	$58^s,45$	$-10°$	$18'$	$1'',1$	7
v		3	15,83	-9	13	14,8	7—8
u		4	52,86	-9	1	15,2	6
t	20	2	44,10	$+0$	33	49,9	8
s		24	59,40	4	51	44,5	7
r		28	17,51	4	53	57,1	11
q		32	41,67	5	34	5,9	8—9
p		37	29,94	6	45	26,7	8
o		47	43,46	8	36	34,6	7
n		51	58,29	8	39	13,9	9—10
m		59	30,56	9	52	35,4	8—9
l	22	21	50,05	21	6	7,0	9
k		25	39,90	21	32	31,5	8—9
h		35	31,47	22	34	19,5	10
A Pegasi		38	15,22	22	39	45,3	
f		57	47,91	24	32	38,1	9
56 Pegasi		58	44,63	24	32	27,8	
e	23	3	27,49	25	7	34,1	8
α		16	32,62	25	59	1,3	8
β		16	40,22	26	9	47,6	8
d		18	13,02	26	4	19,6	9—10
γ		20	10,88	26	17	37,1	8
c		23	6,22	26	17	47,4	9
b		23	16,50	26	25	24,7	9
a		32	4,53	26	56	24,6	7—8

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If the observations are reduced by means of these determinations, the following places are obtained, in which the first values, namely those of Sept. 16, Oct. 2, Oct. 6, Oct. 13, Oct. 25, although nearly agreeing, are not included. The observations are already corrected for Aberration, Nutation, and Parallax, and reduced to the mean Equinox 1829, Jan. 9,72.

I. Observations at Dorpat. (16. Obs.)

1828.	Mean Paris Time.	Observed Right Ascension.	Observed Declination.
Oct. 26	5h 29m 25s	353° 13′ 47″,5	+ 26° 57′ 48″,0:
28	7 48 13	350 41 48,5	26 22 49,2
29	5 15 41	349 36 35,4	26 6 2,7
Nov. 1	5 57 39	345 54 37,4	25 3 10,5
2	9 58 1	344 29 9,4	24 36 9,1
7	5 48 49	338 40 45,5	22 31 19,9
9	5 20 56	336 22 2,1	21 34 37,8
10	5 2 0	335 12 14,3	21 5 52,8
30	5 24 52	314 45 24,0	9 54 40,8
Dec. 2	5 21 52	312 51 27,4	8 41 56,7
5	5 21 53	309 57 40,2	6 49 36,5
7	3 56 14	308 2 27,7	5 34 8,0
8	4 10 14	307 1 19,9	4 53 53,8
14	3 33 33	300 33 2,7	+ 0 36 30,3
25	3 22 11	286 1 14,8	− 9 15 43,0
26	3 4 4	284 35 30,7	10 15 52,0

The following table contains the comparison of these places with the newest orbit as well as with the former Ephemeris.

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1828.	Apparent Error of the Ephemeris. Right Ascension.	Apparent Error of the Ephemeris. Declination.	Apparent Error of the new Orbit. Right Ascension.	Apparent Error of the new Orbit. Declination.
Oct. 26	− 3′ 2″,6	− 21″,1:	−18″,5	+ 8,9
28	2 54,7	39,0	− 6,6	− 6,3
29	2 57,3	28,9	− 7,3	+ 5,1
Nov. 1	2 58,4	34,5	− 3,3	+ 3,5
2	2 55,1	26,9	+ 1,5	+ 12,4
7	2 52,4	43,0	+ 10,6	+ 2,6
9	3 16,0	51,7	− 10,8	− 3,5
10	1 43,2	1 25,6	....	....
30	3 26,4	1 20,6	+ 6,2	− 8,3
Dec. 2	3 37,1	1 21,0	+ 0,5	− 6,2
5	3 38,8	1 28,6	+ 8,3	− 9,4
7	3 47,5	1 26,2	+ 7,0	− 3,5
8	3 58,0	1 29,4	+ 0,5	− 4,5
14	4 25,3	1 43,6	+ 0,1	− 3,3
25	5 14,0	2 9,5	− 5,9	− 2,7
26	5 13,8	2 21,7	− 3,8	− 13,1

The explanation of the single discordant observation of the 10th of November is obvious in itself from Struve's remarks. On this day, Struve compared the comet with a star *l*, which was the brighter and south-following of two stars of the 9th magnitude distant 1′,5 from each other. It is evident that in the determination of *l*, the preceding instead of the following star was taken, and therefore the Æ was obtained about 1′,5 too little, the declination too northerly. It will be easy to rectify this mistake by the observation of the right star.

If we consider the exceeding faintness of the comet in the first half of the observations till Nov. 10, the uniformity of the signs in the second half of the apparent errors of the new orbit, and the circumstance that, as will be afterwards shewn, the orbit is by no means attached to these observations only, but has been derived from four Appearances of the comet to which equal probability is attributed; then may the mean error,

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amounting at the most to 3″, as deduced by Struve from the agreement of the single comparisons with one another, be considered very little too small; and the superiority of Frauenhofer's telescope for such comet observations will be very clearly proved.

The remaining series of observations, (which are already exhibited completely reduced in the *Astronomische Nachrichten*,) for the whole of which every correction is taken into account, give the following Apparent Errors of the Ephemeris.

II. Mannheim. (12 Obs.)

1828.	Right Ascension.	Declination.
Nov. 4	— 3′ 14″,3	— 52″,7
5	3 2,1	17,7
6	3 7,5	32,4
7	3 1,6	....
27	3 1,8	1 16,1
Dec. 2	3 13,4	1 2,0
5	3 22,0	1 14,5
6	3 27,9	1 16,1
11	3 58,4	1 18,9
12	4 0,8	1 26,3
15	4 16,8	1 44,0
19	4 40,0	1 50,6

III. Seeberg. (9 Obs.)

Nov. 10	— 3 36,4	— 42,8
22	2 41,6	1 5,2
24	3 7,2	56,2
25	2 36,4	....
26	3 2,0	57,6
Dec. 2	3 26,6	1 7,4
6	3 46,1	1 20,4
9	3 58,4	1 29,2
15	4 30,2	1 39,9

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IV. Speier. (6 Obs.)

1828.	Right Ascension.	Declination.
Oct. 29	— 2′ 35″,1	— 6,4
Nov. 7	2 43,3	44,5
23	3 5,2	1 12,2
Dec. 2	3 5,8	1 22,0
5	3 23,1	1 16,5
6	3 27,7	1 16,9

V. Göttingen. (1 Obs.)

Oct. 27	— 2 47,6	....

VI. Bremen. (13 Obs.)

Nov. 3	+ 26″,4	— 24″,0
4	— 2′ 27,1	49,8
5	2 4,0	1′ 37,2
9	2 46,3	2 17,3
10	2 17,8	1 22,4
25	2 37,8	36,4
27	2 15,6	....
Dec. 1	3 12,8	39,5
	3 9,6	....
6	3 13,0	1 40,3
9	3 13,6	1 6,9
10	3 49,2	1 31,4
15	3 44,4	1 17,0

VII. Abo. (22 Obs.)

Oct. 29	— 2 44,2	+ 1 9,6	Meridian obs.
30	....	— 1 33,2	ditto
Nov. 8	1 31,2	27,5	ditto
—	3 0,7	....	....
—	2 39,9	....	....
—	3 29,8	1 20,0	Ring-microm.

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1828.	Right Ascension.	Declination.	Instrument.
Nov. 19	— 2′ 14″,3	— 11″,5	Meridian
30	3 12,8	1′ 2,5	Ring-micr.
Dec. 1	3 16,0	1 0,5	Heliometer
—	3 48,0	1 0,6	Ring-micr.
2	2 42,8	42,4	Heliometer
5	3 32,8	1 5,1	ditto
6	3 57,8	1 28,3	ditto
8	3 36,8	1 13,6	ditto
14	4 8,5	1 18,9	ditto
16	4 5,8	1 43,1	Equatoreal
—	4 32,5	1 41,9	Heliometer
21	4 55,1	1 57,0	Equatoreal
—	4 51,2	2 22,6	Heliometer
23	5 24,9	2 40,3	Equatoreal
—	5 8,1	1 55,4	Heliometer
25	5 18,4	2 11,4	Equatoreal

VIII. Prag. (8 Obs.)

Nov. 4	— 28,7	+ 35,6
5	2 17,9	+ 2,8
30	3 11,6	+ 3,7
Dec. 2	2 57,2	— 1 37,1
5	3 25,8	1 2,0
10	3 46,4	1 29,4
14	4 23,0	2 12,4
16	4 15,1	2 1,4

IX. Kremsmünster. (18 Obs.)

Oct. 29	— 2 38,9	— 11,9
31	4 9,0	+ 1 37,5
Nov. 4	1 33,5	+ 2 42,7
5	1 23,3	— 0,1
6	1 21,4	+ 1 27,9
7	1 50,3	+ 1 42,3

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1828.	Right Ascension.	Declination.
Nov. 22	— 2′ 19″,4	+ 1′ 20″,2
23	1 29,9	+ 21,0
Dec. 2	1 58,3	+ 23,5
3	2 35,0	+ 46,4
4	3 25,4	— 1 2,2
5	3 2,8	— 3,9
10	4 37,2	— 1 59,5
11	4 22,8	+ 12,0
12	3 11,4	— 2 11,6
13	3 36,1	— 35,1
15	3 35,7	— 1 24,8
16	3 53,6	— 1 27,6

X. Padova. (13 Obs.)

Oct. 31	— 2 16,4	— 7,2
Nov. 1	4 33,5	46,7
3	3 56,7	27,1
4	2 13,0	57,7
5	2 38,2	43,6
28	3 19,7	30,4
Dec. 2	3 17,4	52,0
3	3 24,4	1 19,8
5	3 42,1	1 18,0
6	3 56,4	1 25,2
7	4 6,9	1 9,2
10	4 18,1	1 2,5
19	4 20,6	2 12,9

XI. Krakau. (5 Obs.)

Nov. 30	— 3 14,6	— 1 16,5
Dec. 2	3 41,2	1 19,5
3	4 13,3	2 16,0
6	4 45,4	1 22,9
7	5 22,9	0 33,6

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XII. Nismes. (17 Obs.)

1828.	Right Ascension.	Declination.
Nov. 2	— 2′ 56″,5	....
10	2 2,0	....
12	2 6,4	....
29	3 2,7	....
30	3 33,0	— 2′ 27″,7
Dec. 2	3 39,1	— 43,1
4	3 19,1	....
5	3 26,0	+ 2,7
10	4 15,7	— 48,8
11	3 37,2	+ 2,8
12	4 23,3	— 2 7,7
13	3 0,3	....
14	4 12,6	— 1 21,4
19	5 5,8	— 51,2
21	5 9,6	— 1 17,0
23	4 58,0	....
24	4 47,6	....

XIII. Berlin. (11 Obs.)

Oct. 27	— 2 9,7	— 32,7
Nov. 4	2 25,6	1 15,2
5	2 11,7	19,4
10	2 16,7	10,9
13	2 32,6	33,1
25	3 5,1	53,5
26	3 16,2	1 35,4
Dec. 2	3 37,0	1 5,6
5	3 43,5	1 55,4
6	3 57,5	1 22,0
16	4 41,2	1 46,1

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XIV. Makerstown. (30 Obs.)

1828.	Right Ascension.	Declination.	Remarks.
Oct. 26	— 3′ 1″,2	— 33,8	(From the *Memoirs of the Astronomical Society.*)
27	1 53,9	— 20,8	
28	3 35,6	+ 48,9	
31	2 32,5	+ 2 6,7	
Nov. 1	3 26,7	— 3 21,9	
2	2 21,2	+ 1 15,3	
3	1 39,5	+ 10,7	
4	....	....	Apparently erroneous.
8	2 45,6	— 24,7	
10	2 32,1	19,8	
11	2 39,8	1 24,7	
12	2 17,0	40,9	
22	3 31,0	39,6	
26	3 0,3	35,9	
Dec. 1	2 57,2	1 20,0	The time altered 1ʰ.
3	3 25,2	1 35,6	
4	3 43,7	1 38,3	
5	3 4,3	1 17,0	
6	3 57,6	1 32,7	
7	3 18,8	1 38,3	
8	3 50,5	1 32,8	
9	4 14,5	1 19,1	
11	3 26,1	1 58,5	The time altered 1ʰ.
14	4 22,5	1 45,4	
16	4 42,0	2 35,9	
17	5 5,6	1 35,9	
18	4 55,7	1 46,5	
19	3 38,6	....	No comp. in Decl.
21	4 49,4	1 58,3	
25	5 27,5	1 55,5	

XV. Greenwich. (11 Obs.)

Nov. 4	— 40,0	+ 5 22,9	(From the *Greenwich Observations.*)
8	+ 1 23,6	— 17,4	
9	— 13,0	— 14,3	
12	+ 17,0	+ 1 41,1	
14	— 4 9,0	+ 2 12,9	
23	3 5,1	— 12,8	
24	2 32,7	+ 5,7	
29	3 53,0	+ 1 51,2	
Dec. 1	3,2	— 13,7	
4	4 15,3	+ 33,7	
11	4 23,3	+ 55,9	

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On comparing the differences for various places on the same day, considerable discrepances appear. Some of these are inexplicable. It can only for example arise from an error on my part (though I have as much as possible endeavoured to secure myself against such) or from one on the part of the Observer, that even in December when the comet was most distinctly visible, the Greenwich declinations differ from those of Struve by from 2½ to 3 minutes. Others may be accounted for, partly at least, by Struve's excellent system (well worthy of imitation) in respect to the point of the comet which he observed. For example, according to his representations, the brightest point in the comet, on which he made his observations, is always at a greater distance from the sun than the center of the nebulous mass. Less powerful telescopes could not perhaps define the first, but might choose the estimated center. According to the place of the comet they must in this case give a smaller Æ, and a more southern declination than Struve: and in fact this is seen by my own observations here whilst the comet was faint. The difference constantly decreases, and in the latter observations of December is imperceptible. The Mannheim observations agree as nearly as possible in every part with those of Struve, as might be expected from the excellence of the instruments and the known accuracy of the observer.

Under these circumstances I have not hesitated to abide by Struve's observations alone for this perihelion passage. The near prospect that several larger telescopes will be in action, and that this remarkable comet will not be thought unworthy of their employment, has confirmed me in my determination. By this means we may hope to obtain observations at different passages which will harmonize together, and to attain in a short time a clearer insight into its true course.

The connection of these observations with the passages of 1819, 1822, 1825, gave an improved system of elements for which the more distant perturbations were calculated. Unfortunately it has not hitherto been possible to continue them fully up to the time of the next passage. They extend to January 1832. As however, from the nature of

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the subject, the part yet wanting can have only a very slight influence upon the geocentric place of the comet, the following Ephemeris will not merely be sufficient for the finding of the comet, but will also give its place with tolerable accuracy; at least for those months in which alone it can be visible in our northern hemisphere, if it is so generally, namely those before its passage through perihelion.

Elements of Pons' Comet for 1832.

Passage through perihelion 1832, May 3,99093, Mean Paris time.
Longitude of perihelion 157° 21′ 2″,4 } Mean Equinox, 1832, May 4.
☊ 334 32 5,2 }
Inclination of the orbit 13 22 12,3
Angle of eccentricity 57 43 6,3
Mean daily sidereal motion 1071″,09598.

1832. 0,3010. Mean Paris Time.	Comet's Right Ascension.	Comet's Declination.	Log. Dist. from Earth.	Log. Dist. from Sun.
Jan. 0	343° 43′,9	+ 1° 6′,7	0,3545	0,3162
4	344 35,1	1 23,5	0,3570	0,3073
8	345 30,7	1 42,6	0,3590	0,2980
12	346 30,8	2 4,0	0,3605	0,2883
16	347 35,5	2 27,7	0,3614	0,2782
20	348 44,5	2 53,7	0,3617	0,2677
24	349 57,8	3 22,0	0,3615	0,2567
28	351 15,5	3 52,6	0,3606	0,2451
Feb. 1	352 37,5	4 25,4	0,3591	0,2330
5	354 3,8	5 0,4	0,3569	0,2203
9	355 34,5	5 37,6	0,3540	0,2069
13	357 9,7	6 17,0	0,3504	0,1928
17	358 49,9	6 58,6	0,3461	0,1779
21	0 35,2	7 42,6	0,3411	0,1621
25	2 25,8	8 28,9	0,3352	0,1453
29	4 22,1	9 17,3	0,3284	0,1274
March 4	6 24,9	10 7,9	0,3208	0,1083
8	8 34,4	11 0,9	0,3123	0,0878
12	10 51,6	11 56,1	0,3027	0,0656
16	13 17,5	+12 53,6	0,2920	0,0416

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1832. 0,3010. Mean Paris Time.	Comet's Right Ascension.		Comet's Declination.		Log. Dist. from Earth.	Log. Dist. from Sun.
March 20	15°	53′,0	+13°	53′,1	0,2802	0,0156
24	18	39,1	14	54,6	0,2671	9,9872
28	21	37,7	15	57,6	0,2526	9,9559
April 1	24	50,6	17	1,8	0,2364	9,9213
5	28	20,0	18	6,5	0,2184	9,8827
9	32	7,9	19	10,1	0,1981	9,8396
13	36	17,7	20	10,7	0,1750	9,7911
17	40	50,9	21	5,2	0,1483	9,7369
21	45	48,8	21	47,9	0,1170	9,6774
25	51	4,9	22	10,0	0,0794	9,6162
29	56	24,4	21	58,3	0,0335	9,5632
May 3	61	9,5	20	58,0	9,9782	9,5365
7	64	35,7	19	1,0	9,9151	9,5500
11	66	19,4	16	10,4	9,8487	9,5962
15	66	32,8	12	37,2	9,7831	9,6562
19	65	36,7	8	24,8	9,7201	9,7169
23	63	48,0	+ 3	33,7	9,6602	9,7731
27	61	17,6	− 2	1,7	9,6034	9,8235
31	58	7,8	8	28,3	9,5502	9,8684
June 4	54	12,0	15	55,8	9,5015	9,9085
8	49	12,5	24	28,6	9,4593	9,9444
12	42	37,6	34	0,0	9,4277	9,9768
16	33	30,1	43	56,6	9,4110	0,0061
20	20	23,2	53	11,2	9,4128	0,0329
24	1	56,7	60	0,4	9,4333	0,0576
28	339	52,0	63	4,7	9,4691	0,0803
July 2	319	30,8	62	31,2	9,5147	0,1014
6	304	33,1	59	53,2	9,5654	0,1210
10	294	34,0	56	36,1	9,6177	0,1393
14	287	54,5	53	21,0	9,6692	0,1565
18	283	25,5	50	22,2	9,7194	0,1726
22	280	22,2	47	45,3	9,7672	0,1878
26	278	15,2	45	28,4	9,8127	0,2022
30	276	49,6	43	29,2	9,8559	0,2158
Aug. 3	275	53,6	41	45,3	9,8968	0,2287
7	275	21,3	40	14,7	9,9355	0,2410
11	275	5,2	38	55,5	9,9722	0,2527
15	275	4,2	37	45,3	0,0070	0,2639
19	275	14,5	−36	42,6	0,0400	0,2746

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In order to judge of the possibility of seeing the comet in our northern hemisphere, I have formed the following table, which shews the time of the comet's setting and the time of sunset for the latitude of Berlin, together with the days of 1828 on which the comet's distance from the sun was the same as on the days of 1832 in the first column. For all the circumstances of this comet seem to concur in shewing that the distance from the sun, or, which amounts to the same, the intensity of the reflected light, determines its visibility. The quantity of light, or the apparent magnitude of the comet, has a very slight influence.

1832.	Time of Setting.		Corresponding Days. 1828.
	Comet.	Sun.	
Jan. 0	10^h 24′	3^h 54′	Sept. 7
16	9 44	4 14	23
Feb. 1	9 11	4 43	Oct. 9
17	8 46	5 13	25
March 4	8 31	5 43	Nov. 10
20	8 27	6 12	26
April 5	8 39	6 40	Dec. 12
21	9 11	7 7	28
29	9 23	7 23	Jan. 5
May 7	9 4	7 35	13
15	8 1	7 47	21

From this it would seem that though the hope of seeing the comet is faint, yet it is not to be entirely given up. Struve's first efficient observation in 1828 was on the 26th of October. On the 17th of February 1832, the comet will be at the same distance from the sun as on that day, and will set 3½ hours after the sun. Its distance from the earth however will be four times greater than in 1828, a difference of position which greatly increases the difficulty. About the time of its greatest brightness it sets only two hours later than the sun. Yet as Struve observed it on December 26, 1828,

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though under unfavourable circumstances, only 44^m before its setting, and not two hours after the sun's setting, it may perhaps be again possible to make an efficient observation in April. At least it is very much to be wished that Struve and those astronomers generally who can command instruments of great light, would take the trouble of convincing themselves by a strict examination of the presumed place of the comet, whether a trace of it can be discovered.

After its passage May 4 until June, the comet comes nearer to the earth than it has done upon any former Appearance. Though it does not arrive at the maximum of its possible brightness and of its apparent magnitude at the same time, yet it will be easily found even with the naked eye, if its place is but tolerably known. On May 7 it sets $1\frac{1}{2}$ hour after the sun for a south latitude 34°. Unfortunately, through the return of Mr Rumker, we have lost the prospect of obtaining observations from Paramatta. The chasm however will be filled up by observations made at the Cape of Good Hope by Mr Fallows, to whom we already owe so many excellent series of determinations.

The review of the course which I have hitherto adopted in my calculations, from the first moment in which I was fortunate enough to discover the periodical return of the comet until the formation of this last system of elements, is rendered somewhat difficult by the dispersion of the essays, each of which contained the results which had been obtained at the time of publication, through different periodicals. It appears to me therefore not superfluous to subjoin a short statement comprising their principal results, which may answer the same purpose as this troublesome research.

The first calculations of perturbations applied to the periods 1805—1819 and 1795—1805, led to this surprizing conclusion, which has since received further confirmation, that the magnitude of the semi-major axis of the orbit, cleared from perturbations and reduced to a given instant, is obtained smaller from the later revolutions than from the earlier. (Bode's *Astronom. Jahrb.* 1822, p. 200.) A somewhat more accurate repetition, in which also the return of 1786 was

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included, established this difference. In fact the pure elliptical time of revolution of the comet, cleared from perturbations and reduced to the time of the perihelion passage 1805, appeared to be

from the period	1786——1795...	1208,112 Days...	3 Revolutions
	1795——1805...	1207,879——...	3 ———
	1805——1819...	1207,424——...	4 ———

(Bode's *Astronom. Jahrb.* 1823, p. 215.) The observations of 1786, 1795, 1805, 1819, were strictly examined, and their greater or less uncertainty might perhaps amount to 1 or 2 minutes, but not to so much that there could have been a possibility of errors as great as must have found place if a uniform period of revolution had been assumed. There remains then nothing else for the prediction of future Appearances than the trying a new explanation which may correspond with the earlier observations. (Bode's *Astronom. Jahrb.* 1822, p. 183—196; 1826, p. 128—131.)

The most natural and in fact almost the only explanation which this phenomenon admits of, appears to me, (an opinion in which Olbers concurs) to be afforded by the hypothesis that the comet experiences a resistance in its course, which (as the existence of a perfect vacuum is improbable) may be exercised by the medium extending through all space. Its small density or other circumstances may be the reason that the denser planet-masses are not affected in the same manner. (Bode's *Astronom. Jahrb.* 1826, p. 133.) It is the simplest, since it is evident that the epochs of the perihelion may be connected by a formula including a term depending on the square of the time. Thus the epochs which serve for basis to the numbers above are very nearly represented by

$$1207{,}6564\, n - 0{,}0542\, n^2$$

where n represents the number of revolutions since 1805. It is also almost the only one, because any other, founded on the difference of the comet's constitution from that of the planets, might perhaps give a period of revolution that

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would not harmonize with our sun's mass, or Gauss's constant k*, but would never give a continued variation of periodic time following a regular law (Argelander, *Comet of* 1811.) The conviction of its necessity was in the first years so strong that it was used for the prediction of the Appearance of 1822 (Bode's *Astronom. Jahrb.* 1823, p. 217.)

The event confirmed this hypothesis. The discovery of the comet in the year 1822 by Mr Rumker proved that the mean error of the Ephemeris thus computed was only 5′. (*Astr. Nachr.* Vol. II. p. 38.) Hence the urgency of using for the ground of the hypothesis more accurate constants than the mere assumption of a diminution in the time of its revolution. On the whole it was here of little importance whether the peculiar assumptions corresponded strictly with nature. As the comet is almost always observed at the same part or nearly so of its orbit, and consequently of space, every assumption will sufficiently represent the observations, which was the principal object to be attained; it being premised that the periods of revolution are thereby regularly successively lessened.

Take for unit of force the sun's attraction; for unit of velocity that with which a body would describe the space = 1, (or the semi-major-axis of the Earth's orbit) in time = 1 (or the mean solar day:) put U for the resistance (expressed by these units) which a body of the same external form and density as the comet (both of these assumed unchangeable) would experience if it moved with the velocity = 1 in a

* In Gauss's notation, let $2p$ be the parameter of the conic section described by any planet round the sun, μ the mass of this planet, $\frac{g}{2}$ the area which it describes in the time t: then it follows easily from Kepler's laws that $\frac{g}{t\sqrt{p}\sqrt{(1+\mu)}}$ is the same for every planet in the system. If the value of this expression is taken for the earth, and if the earth's mean distance is taken as the unit of linear measure, and a mean solar day as the unit of time, this becomes $\frac{2\pi}{365{,}26\sqrt{(1+\mu)}}$, $\left(\text{where } \mu = \frac{1}{354710}\right)$. This is Gauss's constant k.
TRANSLATOR.

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medium which had the same density as that at the distance 1 from the sun in the assumed hypothesis. Let the effective resistance be supposed proportional to the density of the medium and the square of the linear velocity directly: and the density of the medium proportional to the reciprocal of the square of the distance fron the sun: then the tangential force at any instant, which is directly opposed to the motion, is

$$U' = U \, . \, k^2 \left(\frac{2}{r} - \frac{1}{a}\right) . \frac{1}{r^2}$$

where k is the constant from Gauss's *Theoria motûs*, a the semi-major-axis, and r the radius vector. Resolve this force into one perpendicular to the radius vector and another parallel to it, and substitute these in the expressions which give the alterations of the eccentricity and the mean daily motion produced by disturbing forces. Thus we obtain

$$\frac{d\phi}{dt} = - k^3 . \frac{2p \cos E}{r^3 \cos \phi} . \sqrt{\left(\frac{2}{r} - \frac{1}{a}\right)} . U$$

$$\frac{d\mu}{dt} = + 3 \, k^4 . \frac{1}{r^2 \sqrt{a}} \left(\frac{2}{r} - \frac{1}{a}\right)^{\frac{3}{2}} . U$$

where ϕ denotes the angle of the eccentricity† in Gauss's notation, p the semiparameter, E the eccentric anomaly, μ the mean daily motion. If the form of the orbit is supposed invariable, the effect of U on the other elements vanishes, since it is periodical, returning to the same values at the termination of one revolution. If the elements are supposed variable, a calculation undertaken for the purpose of examination has convinced me that the part of the effect of U which depends on that variation would be quite imperceptible. The part of the perturbation of the epoch which depends on the variable μ is obtained by the double integration of the last formula.

† In the usual notation, $e = \sin \phi$, or $\phi = \sin^{-1} e$. TRANSLATOR.

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By means of this formula a value was obtained for U (by a first calculation) which, supposing the perturbations as they were assumed in the earlier computations, appeared to correspond best with the epochs of 1786, 1795, 1805, and 1819. The determination of U evidently depends on the assumptions for the planet-masses, especially on that of Jupiter. The earlier perturbations were calculated with Laplace's value of Jupiter's mass $= \frac{1}{1067,09}$: under this assumption, which consequently must be retained for the future, the value of $U = \frac{1}{752,73}$ did not appear to correspond exactly with observations, (for this purpose the earlier calculations of perturbations were not strict enough on account of their great extension), but rather to be, among such as could be taken, that which united the earlier epochs with sufficient accuracy.

It was from 1819 that the calculations of perturbation were conducted from the very beginning so accurately and extensively that the results obtained could be used for a perfectly rigorous determination of the orbit. To this end, the time was divided into intervals of 4 days, in the proximity of perihelion, until the comet was beyond the sphere of attraction of Mercury. Then followed intervals of 12 days till it passed the sphere of attraction of the Earth and Venus. The remaining time was divided into intervals of 36 days for computing the perturbations of Mars, Jupiter and Saturn. Uranus alone was excluded. The differential quotients computed for these points of time were integrated by mechanical quadrature. When the comet was at a considerable distance from a planet (namely, for Mercury, at the termination of the 4-day intervals, and for the Earth and Venus, at the termination of the 12-day intervals) the perturbation caused by this planet was taken into account by referring the elements of the comet's orbit to the centre of gravity of the system, formed by the Sun, Planet and Comet. Using this process it was allowable to venture on applying very long intervals. For the first period 1819—

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1822 the perturbations in respect to the center of gravity were calculated for very great distances; but as they were imperceptible, the computation was discontinued. The whole calculation admitted of the most certain and severe examination by the regularity of the differences. The formula for the reduction to the center of gravity may be found, as given by Bessel, in Argelander, *Comet of* 1811.

The changes produced by U were also obtained by this method, which appeared to be the most accurate, and at the same time the shortest, as the coefficients were already given. After each section the elements were corrected for the perturbations already arrived at.

The calculations of the perturbations, besides the value of U above, gave by the connection of the two series of observations in 1819 and 1822 that system of elements by which the perturbations were continued till 1825, and from these the Ephemeris computed for 1825 was deduced. (Schumacher's *Astr. Nachr.* Vol. IV. p. 126.)

In the mean time, before yet the event had decided upon the correctness or incorrectness of the elements, two foreign enquiries were published, one of which questioned the correctness of the formulæ, the other the necessity of the whole hypothesis.

In Zach's *Corresp. Astron.* Vol. IX. p. 194, I had stated the hypothesis without giving the particular values, but merely this result: that the changes which μ and ϕ undergo during a revolution would have this relation:

$$\frac{\Delta \phi}{\Delta \mu} = -35{,}236.$$

The distinguished geometer in Turin, M. Plana, continued the same enquiry under the same hypotheses. He integrated the differential formulæ by the help of elliptic transcendents, and found

$$\frac{\Delta e}{\Delta \mu} = -18{,}463.$$

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(Zach's *Corres. Astron.* Vol. XIII. p. 354.) The great difference of the results appeared to him inexplicable. It is clear from the notation explained above that the difference originated merely in a misunderstanding. Plana understood the ϕ used by me to mean the eccentricity e expressed in seconds; my assumption however was, $e = \sin \phi$; and for $\phi = 58° 3'$, (the value that Plana uses)

$$35{,}236 \cos \phi = 18{,}646:$$

thus the results agree as nearly as from the difference of elements could reasonably be expected.

The Baron Damoiseau at Paris had also calculated the perturbations of the comet in the same manner, and had thus connected the epochs of 1805, 1819, and 1822. He found that, according to his numerical values for these three epochs, the mean daily motion might be regarded as uniform; and he determined by the elements thus found the course of the comet for 1825 and 1828. The result of his labours, and the report presented to the Institute of France, are given in the *Connaissance des Temps* for 1827. If a mean uniform motion had been obtained from three epochs, the introduction of a new hypothesis would have been not merely unnecessary, but even prejudicial.

The calculations of Damoiseau are not on the whole to be considered unworthy of confidence. But here enters the circumstance which afterwards, though in a different manner, affected my own calculations: namely, that the period of 1819—1822, which he has made use of, is unfavourable, and inapplicable for the purpose of deciding upon the necessity of the hypothesis, principally because in it the perturbations of Jupiter are so excessive, that a correction which can only be deduced from the difference of two periods, cleared from all perturbations, is not determined from this period with perfect security. It depends in this case too much upon the assumed mass of Jupiter, and the particular values used in the calculation. According to Damoiseau's numbers, if ΔM represents the correction of the mean anomaly for perturbation, the following values hold:

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From 1805—1819, 4 periods, $\Delta M = + 15436$; $\Delta\mu = + 3,091$

1819—1822, 1 .... $\Delta M = - 9858$; $\Delta\mu = - 7,363$.

A slight change of the assumed masses, or a somewhat different calculation, by its very unequal operation, in the proportion of 4 to 1 in respect to the number of periods, and of 3 to 2 in respect to the perturbations of epochs, will in fact annihilate the possibility of deducing a small quantity from the difference of the two intervals. It appears from the above that it was only the earlier epochs 1786—1819 which rendered a new hypothesis desirable. Of this Damoiseau was not ignorant, but he considered the observations of 1786 and 1795 so useless that they ought to be entirely disregarded. (*Connaissance des Temps*, 1827, p. 228.) That this has no foundation has been already shewn; and the general principle, of wholly setting aside older or less accurate observations, could be attended only with prejudicial consequences. In reference to this subject, in 1826 I did myself the honour of entreating M. Damoiseau to extend his calculations if possible to 1795 and 1786, as an independent examination by two wholly different methods must be of the greatest value. It was not clear from his answer whether this additional labour could be expected from him: but as he mentions this circumstance in his indication of the comet's course for 1828 (*Connaissance des Temps*, 1830,) without refuting my assertions, I suppose I may conclude with tolerable certainty that he has found results perfectly consistent with mine.

It would have been superfluous to renew this subject, especially as the later observations of 1825 and 1828 can in no way be reconciled with the assumption of a uniform mean motion (*Astron. Nachr.* Vol. IV. p. 159. Vol. VII. p. 118.) if in some recent works on Astronomy, Damoiseau's results and the report from the *Conn. des Temps.* 1827 had not been literally repeated in their principal parts, and an explanation of the difference of our views rendered necessary. The argument by which the hypothesis is chiefly supported from the beginning, namely the impossibility of representing by one

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unaltered orbit all the observations from 1786—1819, is not noticed in the report, much less confuted. And the argument which seemed to have some weight against the hypothesis, from the possibility of combining the epochs 1805, 1819, and 1822 in the same orbit, under the supposition of the perfect correctness both of the calculations and of the assumed masses of Damoiseau, might still, after the foregoing remarks, have been considered doubtful, even if the observations which followed had not confirmed the hypothesis.

The observations of 1825 answered to the computed Ephemeris with surprizing exactness. The mean error amounted to about 2′, (Schumacher's *Astron. Nachr.* Vol. VI. p. 38.) This may be accounted for partly because an error in the time of perihelion-passage had a small proportionate influence upon the geocentric place.

The three series of observations of 1819, 1822, and 1825 now before us, combined by means of accurate values of the perturbations, gave a new determination of the true magnitude of *U*. But as the excessive perturbation of Jupiter in the years 1819—1822, which has before been mentioned, exercised its influence here in the same manner, it was necessary to include in the equations of condition a small correction of Jupiter's mass. Three fundamental places were determined from the observations of 1818—1819, three from those of 1822, and six from those of 1825. Equal weight was given to all, as less depended on the harmony of individual places, than on that of the three epochs on the whole. After elimination by the method of least squares, the smallest sum of the squares of the remaining errors, exhibited as a function of a correction of *U*, and of Jupiter's mass, deduced from 24 equations, was as follows:

To avoid large numbers, let ♃′ and *U*′ denote the new possible values of Jupiter's mass and of *U*, and make

$$U' = U.\ \frac{100+\mu}{100}$$

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$$♃' = ♃ \; \frac{100+v}{100}$$

then the 24 equations of condition of 1819, 1822, 1825, give the expression

$$5039{,}8 + 286{,}280\;(v - 2{,}887)^2 \qquad \text{(I)}$$
$$+ 8{,}763\;(\mu + 10{,}267\,.\,v + 2{,}945)^2$$

The term $(v-2{,}887)^2$ shews that a larger mass for Jupiter would better represent the observations. By virtue of the term $(\mu+10{,}267\,.\,v+2{,}945)^2$, a diminution of *U* is necessarily connected with it. But if Jupiter's mass remains $\frac{1}{1067{,}09}$ then $v = 0$, and $\mu = -2{,}945$. As this latter correction very little diminished the sums of the squares, and had in itself no great weight, it appeared from the three epochs that, Jupiter's mass being retained, *U* might also, for the present, remain unaltered. (Schumacher's *Astron. Nachr.* Vol. VI. p. 38,)

Retaining therefore the former values, the perturbations were carried on until 1828, and a perfectly accurate Ephemeris computed before the return of the comet, and distributed among the greater number of observers. The return in 1828 was the more interesting, because in this year an error in the time of perihelion-passage produced an unusually strong effect upon the geocentric place. Besides the possibility of being able to diminish the influence of the period 1819—1822 by means of the new observations, there was the prospect of establishing with much greater certainty the determination of *U*.

The errors of the Ephemeris were this year greater than in 1825, principally towards the end, where the influence just mentioned more particularly appeared. My impatience to try what values would be given by a new combination would not allow me to wait for the collection of all the observations, but as soon as those made at this place were concluded, I formed from them alone 5 normal places. Though a correction of these was certainly to be

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expected, yet even their incomplete accuracy was sufficient to decide with safety upon the principal points.

The application of the same values, according to the same method, gave thus for 34 equations of condition, the final expression for the sums of the squares of errors

$$6679{,}7+868{,}440\,(v-2{,}317)^2$$

$$\text{(II)}\qquad +207{,}092\,(\mu+2{,}5613.\,v+12{,}200)^2$$

The comparison of the two values (I) and (II) shows at first sight, that for $v=0$, or the mass of Jupiter hitherto used $\frac{1}{1067{,}09}$, the values of μ come out very different. If we substitute in (I) the value of μ obtained from (II) by making $v=0$, which on account of the factor 207,092 has so much greater weight in (II) than in (I), the sum of the squares of errors will indeed be very much increased. It would however always be an unsatisfactory circumstance that a single additional series could so violently alter the value of μ, and our expectation of an exact prediction of future appearances would be greatly shaken.

In the mean time the great difference between the coefficients of v, in the terms in which v is united with μ, leads of itself to the investigation of that value of v, which in both equations gives the same value for μ. For this purpose, make

$$2{,}5613\,.\,v+12{,}200=10{,}267\,.\,v+2{,}945$$

thus we obtain $\qquad v=1{,}20108$

and for a new mass of Jupiter

$$♃'=\frac{1}{1054{,}4}$$

almost exactly agreeing with that which Gauss obtained from Pallas, and Nicolai from Juno, $\left(\frac{1}{1053{,}924}\right)$ and which, according to Heiligenstein's calculations and my own for

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Vesta and Ceres, corresponds better to observations than that adopted by Laplace. The latter rests chiefly on the measures of the elongations of the satellites, (a repetition of which with more accurate instruments is greatly to be desired); and Bouvard's new mass $\frac{1}{1070}$, founded upon the perturbations of Saturn by Jupiter, differs but little from it. But as Nicolai's value is also supported by several planets, and as the comet observations likewise intimate an increase of mass, I think I shall not have too hastily introduced a new value into these calculations of perturbations, if for the future I assume also for the comet Nicolai's determination

$$♃' = \frac{1}{1053{,}924}$$

With this we get from (I), $\mu = -\ 15{,}771$

from (II), $\mu = -\ 15{,}399$

or the new value of U from (II),

$$U = \frac{1}{889{,}76}$$

with which the determination from (I) would have completely harmonized if the mass of Jupiter had had the new value from the beginning. The probable error of the new value of U, if Jupiter's mass is considered free from error, will be, as deduced from (I), about $\frac{1}{24}$ of the whole; as deduced from (II), $\frac{1}{120}$ of the whole.

These relations were not at first clearly understood, on which account the indications obtained from the first conclusion of the calculations, though they were deduced from the equation (II), gave an erroneous representation (Schumacher's *Astron. Nachr.* Vol. VII. p. 183.). It was not then known how much the harmony was assisted by an increase of Jupiter's

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mass, or a positive value of v. Besides, the same calculations there, which here are the basis of equation (II), are made under the assumption of the old mass of Jupiter, as will be seen by the substitution of $v=0$. The least sum of the squares of errors, as it would have appeared if we had supposed the comet to move without suffering a resistance, or if $U=0$, will be found by substituting the value of $\mu=-100$. The thoroughly inexplicable number which will be thus obtained, proves directly the necessity of the hypothesis, as an actual calculation has shewn that the equations of condition are accurate enough for this purpose.

The results thus far obtained appeared to me of such importance that before their final introduction I resolved once more to subject the earlier assumptions to examination. I compared therefore once more the whole of the observations of 1819 and 1822, in order to choose the normal places as judiciously as possible. In the same manner I was induced by the alteration of the values of perturbation with the new mass of Jupiter, to adopt those masses for the other planets, (especially for Venus) which, according to the other astronomical calculations, as well as by Bessel's precession, appeared the most probable. I will now in the first place give the normal places for the four periods 1819—1829 (I). Though some small alterations are possible in another selection, yet I have reason to be firmly convinced that if these normal places correspond to the calculation, the whole series of observations will also be represented. Only it will be necessary to use Bessel's corrections to the Solar Tables, as in 1819 they alone alter the geocentric place in one situation about 20″. Then follow the values of the perturbations according to the assumed masses, and the value of U, which is obtained at the conclusion of the investigation (II). To this are added the elements, as strictly deduced from the normal places, in which, as above, equal weight is given to all the observations (III). Lastly the comparison of the normal places with the elements (IV).

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I. Normal places of Pons' Comet.

(1) Perihelion of 1819. The places are referred to the mean Equinox of 1819, Jan. 0.

Mean Paris Time.	Comet's Right Ascension.	Comet's Declination.
1818. Dec. 22,25	326° 18′ 33″,4	+ 2° 54′ 24″,4
1819. Jan. 1,25	323 11 45,3	+ 0 14 53,8
12,25	315 34 33,7	— 5 36 2,6

(2) Perihelion of 1822. The places are referred to the mean Equinox of 1822, May 24.

Mean Paris Time.	Comet's Right Ascension.	Comet's Declination.
1822. June 2,85	93° 46′ 49″,5	+16° 52′ 29″,9
12,85	103 15 28,1	+ 7 6 20,8
22,85	115 45 43,6	— 9 7 38,2

(3) Perihelion of 1825. The places are referred to the mean Equinox of 1825, Sept. 16,3.

Mean Paris Time.	Comet's Right Ascension.	Comet's Declination.
1825. Aug. 12,6	100° 57′ 40″,6	+31° 32′ 30″,6
17,6	110 23 36,0	30 14 24,8
22,6	120 16 49,2	28 3 26,5
27,6	130 23 56,7	24 54 53,3
Sept. 1,6	140 32 55,7	20 48 15,0
6,6	150 39 7,8	15 46 55,2

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(4) Perihelion of 1829. The places are referred to the mean Equinox of 1829, Jan. 9,72.

Mean Paris Time.			Comet's Right Ascension.			Comet's Declination.		
1828.	Oct.	28,3	350°	48′	41″,8	+26°	23′	7″,4
	Nov.	8,3	337	26	19,1	22	1	23,3
		30,3	314	41	9,3	9	52	0,4
	Dec.	7,3	307	54	16,3	5	28	45,0
		14,3	300	22	27,3	+ 0	29	27,2
		25,3	285	47	24,1	— 9	25	24,9

In these normal places every correction is already taken into account. They must be compared with the elements which hold for the following epochs.

II. Values of Perturbations for the Periods 1819—1829.

i ...... Inclination of the orbit
☊ ...... Ascending Node
ϕ ...... Angle of Eccentricity ($e = \sin\phi$)
ϖ ...... Longitude of perihelion
μ ...... Mean daily sidereal motion
M ...... Mean Anomaly

} for 1819, Jan. 27,25

The elements for the other perihelions are found by the expressions

$$i' = i + \Delta i$$

$$☊' = ☊ + \Delta ☊$$

$$\phi' = \phi + \Delta \phi$$

$$\varpi' = \varpi + \Delta \varpi$$

$$\mu' = \mu + \Delta \mu$$

$$M' = M + \mu t + \Delta M$$

where t is the interval of the epochs. The Precession is yet to be applied to ☊ and ϖ.

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The values of the masses are

$$\text{Mercury} = \frac{1}{2025810}$$

$$\text{Venus} = \frac{1}{401839}$$

$$\text{Earth} = \frac{1}{357500}$$

$$\text{Mars} = \frac{1}{2546320}$$

$$\text{Jupiter} = \frac{1}{1053,924}$$

$$\text{Saturn} = \frac{1}{3512}$$

$$U = \frac{1}{890,852}.$$

This last value of U was obtained from the connection of the above normal places for 1829 (Struve's) with the others.

(1) 1819, Jan. 27,25 – 1822, May 24,0. t = 1212,75 Days.

Prec. = 2′ 50″,525 from 1819 Jan. 0.

	Δi	$\Delta \Omega$	$\Delta \phi$	$\Delta \varpi$	$\Delta \mu$	ΔM
Mercury	− 0″,142	− 1″,948	+ 1″,049	+ 0″,756	−0″,033340	− 36″,434
Venus	+ 0,267	− 0,446	− 2,939	− 1,164	+ 0,108817	+ 98,132
Earth	+ 0,111	+ 0,979	− 0,756	+ 1,021	+ 0,039976	+ 26,764
Mars	+ 0,069	− 0,114	+ 0,006	+ 0,236	− 0,000614	− 0,935
Jupiter	−977,779	−650,898	−1564,628	+573,598	− 7,573441	−10083,841
Saturn	− 15,141	− 7,965	− 25,752	+ 4,471	− 0,041944	− 68,019
U			− 3,574		+ 0,100860	+ 63,502
Sum	−992,635	−660,192	−1596,594	+578,917	− 7,399686	− 9989,811

(2) 1819, Jan. 27,25 – 1825, Sept. 16,3. $t = 2424{,}05$ Days.

Precession = 5′ 37″,108 from 1819, Jan. 0.

	Δi	Δ☊	$\Delta\phi$	$\Delta\varpi$	$\Delta\mu$	ΔM
Mercury	– 0″,118	– 2″,205	– 0″,084	+ 0″,332	+0″,000068	– 28″,205
Venus	+ 0,129	– 2,957	– 3,349	– 2,065	+ 0,122426	+ 260,955
Earth	+ 1,469	– 10,722	+ 0,032	+ 4,030	– 0,018181	+ 19,229
Mars	+ 0,055	– 0,291	+ 0,001	– 0,457	– 0,000660	+ 4,731
Jupiter	–912,889	–661,514	–1386,704	+578,844	– 6,836044	–18891,768
Saturn	– 15,985	– 10,298	– 27,527	– 2,188	– 0,046068	– 79,219
U			– 7,074		+ 0,198788	+ 244,495
Sum	–927,348	–687,987	–1424,705	+578,496	– 6,581731	–18469,872

(3) 1819, Jan. 27,25 – 1829, Jan. 9,72. $t = 3635{,}47$ Days.

Precession = 8′ 23″, 706 from 1819. Jan. 0.

	Δi	Δ☊	$\Delta\phi$	$\Delta\varpi$	$\Delta\mu$	ΔM
Mercury	– 0″,113	– 2,″177	+ 0″,088	+ 0″,735	–0″,004294	– 43″,152
Venus	+ 0,063	– 1,975	– 2,432	– 0,956	+ 0,068691	+ 386,292
Earth	+ 0,546	– 17,402	+ 2,212	– 2,206	– 0,133318	– 10,688
Mars	– 0,015	– 0,481	– 0,009	– 0,291	– 0,001012	+ 3,572
Jupiter	–967,834	–701,659	–1500,080	+619,272	– 7,287609	–27888,628
Saturn	– 10,903	– 10,735	– 14,960	– 2,071	– 0,023442	– 124,340
U			– 10,571		+ 0,297231	+ 544,940
Sum	–978,254	–734,429	–1525,742	+614,483	– 7,063753	–27131,854

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III. Elements.

Epoch: 1819, Jan. 27,25 Mean Paris time.

$M = 359^0\ 59'\ 46'',41$

$\mu = 1076'',92072$

$\phi = 58^0\ \ 3'\ 39'',8$

$\varpi = 156\ 59\ 46,4$ } referred to the mean equi-
☊ $= 334\ 33\ 19,5$ } nox 1819, Jan. 0.

$i = 13\ 36\ 58,4$

Comparison of the Elements with the Normal Places.

	Excess of Calculation above Observation.	
	$\Delta\ \alpha . \cos\ \delta$	$\Delta\ \delta$
1818. Dec. 22,25	+ 33'',8	+ 31'',5
1819. Jan. 1,25	+ 8,4	+ 13,9
12,25	— 20,1	— 8,0
1822. June 2,85	+ 40,7	+ 22,8
12,85	— 14,3	— 2,0
22,85	— 0,5	— 3,5
1825. Aug. 12, 6	— 21,5	+ 13,8
17, 6	— 15,1	+ 6,4
22, 6	— 3,1	+ 0,5
27, 6	— 0,3	— 6,4
Sept. 1, 6	+ 15,0	— 9,6
6, 6	+ 10,8	— 12,8
1828. Oct. 28, 3	— 9,9	+ 2,7
Nov. 8, 3	+ 0,1	0,0
30, 3	+ 6,3	— 8,4
Dec. 7, 3	+ 6,5	— 3,8
14, 3	0,0	— 3,7
25, 3	— 4,7	— 3,2

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From this follows the Sum of the Squares of Errors.

1819......6	Equations of condition	2861,6
1822......6	....	2393,9
1825.....12	....	1565,7
1828.....12	....	314,2
36	Equations of condition	7135,4

The last divisors, and the resulting apparent errors for each element, supposing the probable error of each datum of observation in round numbers = 10″, are found for

M	122,36 ;	Probable Error=0″,9
$\frac{1}{1000}.\mu$	62,63	0,0013
ϕ	86,02	1,1
ϖ	12,98	2,8
☊	0,15	25,6
i	11,21	3,0
$\frac{1}{100}.\frac{\delta U}{U}$	159,38	$\frac{1}{126}$ of the whole value of U.

On the review of the still remaining errors, a tolerably regular succession of signs shews itself in a greater or less degree, in all the Appearances. However if it is recollected that we have not attributed to the observations of 1828 the value which is properly their due, and that the places of 1825 are the mean out of a great number of different observations between which there is a constant, and sometimes

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considerable difference [as for instance, the Neapolitan observations, (Schumacher's *Astron. Nachrichten*. Vol. VI. p. 39.) an excellent series in themselves, especially in the beginning, exhibit a difference of 40″ or 50″ from the others; and certainly the algebraic sign of the difference between them and the mean of the others during the whole time of visibility, is the same as the sign of the difference between the mean of the whole and the place computed from the elements] then it will not appear, that the magnitude or progress of the errors in the two last Appearances, give room for such an objection. The same circumstance has taken place with comets, which have been seen only once, but have then remained visible a longer time; for instance, that of 1811; and was so much the more difficult to be avoided here, as the supposition of an unchangeable external form is in itself improbable: but even supposing this, the very different circumstances under which the observations of different years were made, would hardly allow the possibility of observing always the same point in the body of the comet with extreme accuracy.

From the Appearance of 1822, in which the greatest error is found, it is clearly seen that the elements correspond with observations as nearly as is at all possible. As soon as I had received the observations, I deduced from those of 1822 alone the orbit which corresponded best with them alone, and I compared it with the individual places. The orbit and comparison may be found in Schumacher's *Astron. Nachr.* Vol. II. p. 39. If the errors given there, are compared with those exhibited here (the signs are the same) there appear similar magnitudes and similar signs in every part. There is therefore in this respect no objection to be made against the accuracy of the elements.

The great errors of 1818 and 1819 admit of less explanation. It appears to me improbable, that Nicolai's observations, upon which the first normal place principally rests, should have been so very erroneous, or should have appeared in the beginning so different from the end. In the mean time this circumstance deserves consideration, that the perturbations which connect

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the Appearance of 1819 with the other three, are so very excessive, that they must be directly computed with elements which differ considerably from the true ones, and that on this account in the numerical expressions for this Appearance in particular, a want of uniformity may find place which cannot be removed by the equations of condition.

I think I may consider it as an additional proof of the near correctness of the elements, that the three Appearances of 1819, 1822, and 1825, treated alone, give very nearly the same result as all four, 1819, 1822, 1825, and 1828, (namely within the limit which is prescribed by the sums of the probable errors of the two determinations.) For if we take the fundamental equations of condition for the first three series only, we obtain $M = 359^0 . 59' . 47'',84$: $\mu = 1076'', 91788$: $\phi = 58^0 . 3' . 46'',6$: $\varpi = 156^0 . 59' . 21'',5$: ☊ $= 334^0 . 32' . 13'',9$: $i = 13^0 . 36' . 59'',8$: $U = \frac{1}{860,8}$. The uncertainty of U is by this determination $\frac{1}{24}$ of the true magnitude: and that of ϖ* explains fully the only remarkable discrepance. The error of the prediction for 1828, with these elements, would have amounted to 70″ in Right Ascension, and 36″ in Declination, almost uniformly during the visibility.

It only remains for me to shew that as the Appearances *before* 1819 led first to the necessity of the hypothesis, and also gave the formula, as well as the approximate numerical value, so on the other hand, the more accurate numerical value deduced from the Appearances *after* 1819, corresponds equally with the earlier perihelions.

* The meaning of this clause is rather obscure. The whole sentence stands thus in the original "Die Unsicherheit von U ist dabei $\frac{1}{24}$ der wahren "Grösse: so wie auch die von ϖ die einzige grössere Abweichung, völlig "erklärt." The value of ϖ is very nearly the same as that obtained by the use of the four Appearances (see p. 37.): the only element on which there is a material difference is ☊.

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The values of perturbations here required, are already drawn up in Bode's *Astron. Jahrb.* 1823, p. 213, &c. They are there expressed in days. For the present purpose I will restore them to the original form of Correction of the mean anomaly; with which the given number of days nearly agrees if they are multiplied by the value of μ corresponding to that time, or about 1075″. I have also carried back the perturbations to a certain specified time, not to the instant of perihelion-passage.

By the earlier calculations I found

1)1786. Jan. 30,9.........1795. Dec. 21,5 . t = 3611,6 days.

	$\Delta\ \mu$	$\Delta\ M$
♃	+ 0″,43074	+ 1977″,6

2)1805. Nov. 21,5......... 1795. Dec. 21,5 . t = − 3622 days.

(The calculation was here carried backwards.)

	$\Delta\ \mu$	$\Delta\ \mu$
♃	+ 3″,67404	− 1694″,1
♂ ♀ ☿	− 0,22436	+ 355,5

3)1805. Nov. 21,5 ....1819. Jan. 27,25 . t = 4814,75 days.

	$\Delta\ \mu$	$\Delta\ M$
♃	+ 3″,63325	+ 16048″,1
♂ ♀ ☿	− 0,49084	− 623,9
♄	+ 0,08344	− 83,9

For these values, the old mass of Jupiter as assumed by Laplace, is the basis. If we introduce Nicolai's mass, and then take the sums, we find

1786 − 1795. $\Delta\ \mu$ = + 0,43612......$\Delta\ M$ = + 2002″,3

1805 − 1795. $\Delta\ \mu$ = + 3,49558......$\Delta\ M$ = − 1359,8

1805 − 1819. $\Delta\ \mu$ = + 3,22124......$\Delta\ M$ = + 15540,8

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To leave no doubt upon the method of applying these, I denote by μ_0, μ_4, μ_7, μ_{10}, the μ which corresponds to the epochs of 1819, 1805, 1795, 1786; the index representing the number of revolutions before 1819. A similar explanation applies to M_0, M_4, M_7, M_{10}. Then by virtue of these values of perturbations the following equations hold:

$$\mu_7 = \mu_{10} + 0'',43612$$
$$\mu_7 = \mu_4 + 3,49558$$
$$\mu_0 = \mu_4 + 3,22124$$

$$M_7 = M_{10} + 3611,6\,\mu_{10} + 2002'',3$$
$$M_7 = M_4 - 3622,0\,\mu_4 - 1359,8$$
$$M_0 = M_4 + 4814,75\,\mu_4 + 15540,8.$$

By means of these expressions all the μ and M can be so transformed, as to be exhibited as functions of M_0 and μ_0 only. Thus,

$$\mu_{10} = \mu_0 - 0'',16178$$
$$\mu_7 = \mu_0 + 0,27434$$
$$\mu_4 = \mu_0 - 3,22124$$

$$M_{10} = M_0 - 12048,35\,\mu_0 + 8858'',2$$
$$M_7 = M_0 - 8436,75\,\mu_0 + 10276,2$$
$$M_4 = M_0 - 4814,75\,\mu_0 - 31,3.$$

To avoid large numbers and multiples of 360°, let

$$\mu_0 = 1077'',0 + \delta\mu,$$

and the last three equations become

$$M_{10} = M_0 - 12048,35\,\delta\mu_0 - 7214,75$$
$$M_7 = M_0 - 8436,75\,\delta\mu_0 - 4103,55$$
$$M_4 = M_0 - 4814,75\,\delta\mu_0 - 1517,05.$$

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In these equations the hypothesis of the resisting medium is not yet introduced.. The epochs of the different M lie so near to the time of passage through perihelion, that for a first examination all the M may be considered = 0. Consequently without the hypothesis, we must determine $\delta\mu_0$ so as to make all the equations = 0. A mere glance shews, both that this is not generally possible, and that it does not particularly affect at the bottom the conspicuous errors, since the last equation for M_4 is by far the most accurate. But the value which follows from it corresponds least to the two others.

If we wish to introduce the hypothesis, we must place everywhere at the right side of μ, a term which is proportionate to the number of revolutions, and at the right side of M, a term which is proportionate to the square of that number. If we denote the number of revolutions from 1819, reckoned backwards, by n, the former values of perturbations according to which

μ is increased, from 1819 to 1829, about + 0,29723

M................................. about + 544,94,

give for the co-efficients of n in the equations above, the approximate values:

$$-0'', 099 . n$$
$$+60, 6 . n^2,$$

with which the equations become

$$\mu_{10} = \mu_0 - 1'',152$$
$$\mu_7 = \mu_0 - 0,419$$
$$\mu_4 = \mu_0 - 3,617$$
$$M_{10} = M_0 - 12048,35\ \delta\mu_0 - 1154,75$$
$$M_7 = M_0 - 8436,75\ \delta\mu_0 - 1134,15$$
$$M_4 = M_0 - 4814,75\ \delta\mu_0 - 547,45,$$

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and if we here substitute the values according to the new elements above deduced from the period 1819 – 1829, namely,

$$\mu_0 = 1076'',92072$$

$$\delta\mu_0 = - \quad 0,07928$$

$$M_0 = 359^0 \; 59' \; 46''41,$$

we find

$$\mu_{10} = 1075'',769 \ldots\ldots M_{10} = 359^0 \; 56' \; 26'',85$$

$$\mu_7 = 1076,502 \ldots\ldots M_7 = \quad 52 \quad 1,13$$

$$\mu_4 = 1073,304 \ldots\ldots M_4 = \quad 57 \quad 0,67,$$

or according to the elements, as deduced from the observations of 1819 – 1829, on the supposition of a resisting medium, the times of perihelion-passage will be for the earlier perihelions,

1786. Jan. 31,10 Mean Paris time.

1795. Dec. 21,94

1805. Nov. 21,67.

From the observations, assuming a semi-major axis which differs very little from the true one, these times of perihelion-passage were immediately found (*Astron. Jahrb.* 1822, p. 186, 190, 196:)

1786. Jan. 30,88 Mean Paris time.

1795. Dec. 21,45

1805. Nov. 21,51,

so that the respective differences amount to – 0,22, – 0,49, – 0,16 days. If it were the object to represent the geocentric observations, then indeed must these differences be called very great. But if we consider the incompleteness of the computations of perturbation (as already pointed out) in respect both to the method of calculation, and to the number of the planets, especially in the two first perihelions; the

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amount of the remaining errors in the time of perihelion-passage, (at the maximum about 500″ in the mean anomaly), will appear very little when compared with the magnitude of the disturbances, and will sufficiently prove the agreement of the hypothesis with observation. This circumstance also comes in, that even the perihelion-passages deduced from observation cannot be considered perfectly accurate; and that, with a periodic time deduced from *three* revolutions only, we have gone backward with our calculations to 1786, that is through *ten* revolutions, reckoned from 1819.

What I have here given appears sufficient for the present, to shew the path which I have followed in this enquiry, and to give the means of estimating the stability of the grounds upon which the distant predictions are supported. A more copious collection will appear in the *Transactions of the Berlin Academy.*

If I may be permitted to express my opinion on a subject which for twelve years has incessantly occupied me, in treating which I have avoided no method, however circuitous, no kind of verification, in order to reach the truth as near as lay in my power; I cannot consider it otherwise than completely established that an extraordinary correction is necessary for Pons' Comet, and equally certain that the principal part of it consists in an increase of the mean motion proportionate to the time. Another question which is properly more physical than astronomical (as in strictness the determination of future appearances from past observations is the chief object of Astronomy) is this; whether the hypothesis of a resisting medium gives the true or probable explanation; though hitherto no other appears to me to have equal weight. In conclusion, I think I may venture to express my belief that future Appearances will ever more and more establish the near correctness of the values here exhibited. To state at once my opinion of their accuracy, I will take the uncertainty of the determination of U as scarcely greater than that of most of our Planet-masses; if this circumstance may be taken as a ground for estimating the latter, that the mass which is the most powerful, and in different ways the

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preponderating one, namely, that of Jupiter, is yet doubtful to the amount of its eightieth part. Mean time it will be my endeavour, as it has been hitherto, to conduct the proof of the correctness in the least exceptionable method, namely, by a complete and exact prediction of its actual course. For 1832 I hope to be able to fulfil this pledge.

ENCKE.

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Perhaps the following theorems, equivalent at least to those which Encke has employed in the theory of this Comet, and a short statement of the reasoning on which he relies as proving the existence of some cause whose effects are the same as those of a resisting medium, may not be misplaced here.

In an inquiry of such delicacy, it is necessary to take into account the perturbations produced by the Planets. This is done by estimating the alterations which they produce in the elements of the Comet's orbit. In the ordinary theory of the planetary perturbations, the excentricity and inclination being small, it is convenient to expand the expressions into infinite series of cosines of multiples of the *mean* longitudes, the coefficients proceeding by powers of the excentricities and inclinations. But in the case of a Comet, where the excentricity and the inclination are considerable, a finite expression must be used; and this can be obtained only by keeping the expression for R,

$$\text{or } -\frac{m'}{\sqrt{\{(x'-x)^2+(y'-y)^2+(z'-z)^2\}}}+\frac{m'(x'x+y'y+z'z)}{\{x'^2+y'^2+z'^2\}^{\frac{3}{2}}},$$

in the form of a function of the *true* longitudes and radii vectores. Put a for the semimajor axis, e for the excentricity, ϖ for the longitude of perihelion, ι for the inclination, ☊ for the longitude of the ascending node, r for the radius vector, v for the true longitude, n for the mean motion in the unit of time, and ϵ for the epoch of mean longitude, in the Comet's orbit; and the same letters with accents for the

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similar quantities in the disturbing planet's orbit. The longitudes are supposed to be measured upon the fixed plane (the Earth's orbit for instance) to the node, and then upon the plane of the Comet's or planet's orbit. Put also S for the sum of the masess of the Sun and Comet, m' for the mass of the disturbing planet. Then

$$R = -\frac{m'}{\sqrt{\{r^2 + r'^2 - 2(xx' + yy' + zz')\}}} + \frac{m'(xx' + yy' + zz')}{r'^3}$$

where $xx' + yy' + zz' = rr' \{\cos(\Omega - \Omega') . \cos(v - \Omega) \cos(v' - \Omega')$

$$+ \sin(\Omega - \Omega') . \cos\iota' . \cos(v - \Omega) . \sin(v' - \Omega')$$

$$- \sin(\Omega - \Omega') . \cos\iota . \sin(v - \Omega) . \cos(v' - \Omega')$$

$$+ \cos(\Omega - \Omega') . \cos\iota' . \cos\iota' . \sin(v - \Omega) . \sin(v' - \Omega')$$

$$+ \sin\iota . \sin\iota' . \sin(v - \Omega) . \sin(v' - \Omega')\}$$

(an expression which the experienced computer will generally be able to simplify, according to the circumstances of the case.)

And $$\frac{da}{dt} = -\frac{2na^4}{S}\sqrt{(1 - e^2)}\left\{\frac{e . \sin(v - \varpi)}{a(1 - e^2)} . \frac{dR}{dr} + \frac{1}{r^2} . \frac{dR}{dv}\right\}$$

$$\frac{dn}{dt} = -\frac{3}{2} . \frac{n}{a} . \frac{da}{dt}$$

$$\frac{de}{dt} = \frac{1 - e^2}{2ae} . \frac{da}{dt} + \frac{na\sqrt{(1 - e^2)}}{Se} . \frac{dR}{dv}$$

$$\frac{d\varpi}{dt} = \frac{1 - e^2}{r . e . \sin(v - \varpi)} . \frac{da}{dt} - \left\{\frac{2a}{r . \sin(v - \varpi)} + \frac{1}{e}\cot(v - \varpi)\right\} . \frac{de}{dt}$$

$$- \frac{na}{S\sqrt{(1 - e^2)}} . \tan\frac{\iota}{2} . \frac{dR}{d\iota}$$

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$$\frac{d\Omega}{dt} = -\frac{na}{S\sqrt{(1-e^2)}\cdot\sin\iota}\cdot\frac{dR}{d\iota}$$

$$\frac{d\iota}{dt} = -\frac{na}{S\sqrt{(1-e^2)}}\cot(v-\Omega)\cdot\frac{dR}{d\iota}$$

$$\text{or} = \frac{na}{S\sqrt{(1-e^2)}}\left\{\frac{1}{\sin\iota}\cdot\frac{dR}{d\Omega} + \tan\frac{\iota}{2}\cdot\frac{dR}{dv}\right\}$$

$$\frac{d\epsilon}{dt} = -t\frac{dn}{dt} - \frac{r^2\cdot\sin(v-\varpi)\cdot\{2+e\cos(v-\varpi)\}}{a^2(1-e^2)^{\frac{3}{2}}}\times\frac{de}{dt}$$

$$+\frac{d\varpi}{dt} - \frac{r^2}{a^2(1-e^2)^{\frac{1}{2}}}\cdot\frac{d\varpi}{dt} - \frac{r^2 n}{Sa(1-e^2)}\tan\frac{\iota}{2}\cdot\frac{dR}{d\iota}.$$

When the values of a, n, &c. are found by integrating these expressions, $nt+\epsilon$ is to be taken as the mean longitude of the Comet moving in an orbit of which a, e, and ϖ are the semiaxis major, excentricity, and longitude of perihelion, and Ω and ι the longitude of node and inclination; and with these elements its place is to be calculated exactly as if it was not disturbed. In the calculation of $\frac{da}{dt}$ &c. the variable values of the elements ought in strictness to be taken, but it is generally sufficient to take the values which they had at some near epoch, for instance the beginning of a revolution. Sometimes, however, sensible errors may arise from this difference; and this appears to be Encke's meaning in page 40, line 2.

The accurate integration of these expressions does not appear possible; and we are therefore driven to the method of integration by quadratures. For this purpose the values of the differential coefficients are calculated for small invervals of time, each is multiplied by the length of that interval (expressed by the number of units of time which it contains) and all the products are added. In page 24, Encke has described the intervals &c. which he used in the calculations

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for the comet; the only point which he has omitted to mention is, the extent which he assumed for the sphere of activity of each planet. This is a mere assumption of convenience, distinguishing the parts of the orbit in which the attraction of each planet is considerable from those in which it is small, and in which therefore a less accurate calculation is sufficient.

The effect of the resisting medium may be thus found. The resistance being $V\left(\frac{ds}{dt}\right)^2$, and x and y being measured in the plane of the orbit, the resolved part in the direction of x is $-V.\frac{ds}{dt}.\frac{dx}{dt}$, and that in y is $-V\frac{ds}{dt}.\frac{dy}{dt}$.

Hence the equations of motion are

$$\frac{d^2x}{dt^2} = -\frac{Sx}{r^3} - V\frac{ds}{dt}.\frac{dx}{dt}$$

$$\frac{d^2y}{dt^2} = -\frac{Sy}{r^3} - V\frac{ds}{dt}.\frac{dy}{dt}.$$

From these, $\left(\frac{ds}{dt}\right)^2 = C + \frac{2S}{r} - 2\int_t V.\left(\frac{ds}{dt}\right)^3.$

But in an ellipse, $\left(\frac{ds}{dt}\right)^2 = \frac{2S}{r} - \frac{S}{a}.$

As the same expressions ought to apply to the instantaneous ellipse in a disturbed orbit as to an undisturbed ellipse,

$$C - 2\int_t V\left(\frac{ds}{dt}\right)^3 = -\frac{S}{a};$$

whence $\frac{da}{dt} = -\frac{2Va^2}{S}.\left(\frac{ds}{dt}\right)^3 = -2a^2V.S^{\frac{1}{2}}.\left(\frac{2}{r} - \frac{1}{a}\right)^{\frac{3}{2}}.$

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Again $\frac{d}{dt}\left(x\frac{dy}{dt} - y\frac{dx}{dt}\right) = -V\left(x\frac{dy}{dt} - y\frac{dx}{dt}\right) \cdot \frac{ds}{dt}$;

or as $x\frac{dy}{dt} - y\frac{dx}{dt}$ in an ellipse $= h = \sqrt{Sa(1-e^2)}$,

$$\frac{1}{a(1-e^2)} \cdot \frac{d}{dt}(a.\overline{1-e^2}) = -2V\frac{ds}{dt}.$$

Using the value of $\frac{da}{dt}$ above,

$$\frac{de}{dt} = 2V.\frac{1-e^2}{e}\left(1-\frac{a}{r}\right).\frac{ds}{dt}$$

$$= 2V.\frac{1-e^2}{e}\left(1-\frac{a}{r}\right)S^{\frac{1}{2}}.\left(\frac{2}{r}-\frac{1}{a}\right)^{\frac{1}{2}}.$$

The value of $\frac{d\varpi}{dt}$ is

$$\frac{1-e^2}{r.e.\sin(v-\varpi)}.\frac{da}{dt} - \left\{\frac{2a}{r.\sin(v-\varpi)} + \frac{1}{e}\cot(v-\varpi)\right\}\frac{de}{dt}.$$

On substituting for $\frac{da}{dt}$ and $\frac{de}{dt}$ it will be seen that there are negative terms for values of $v - \varpi$ greater than 180^0, corresponding to positive terms for values of $v - \varpi$ less than 180^0 by the same quantity, and *vice versâ*, and therefore on integration for a whole revolution the change of ϖ will be 0.

The expressions for $\frac{da}{dt}$ and $\frac{de}{dt}$ it appears were integrated by Encke by the method of quadratures: and on the assumption $V = \frac{U}{r^2}$. This appears to be the only defect in generality in the whole investigation. Perhaps with the assumption of a different law of density the relation between Δa and Δe produced by the resistance might be very different, and the effect on the perihelion position of the comet

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might be sensible. Perhaps however future observations on the comet will enable us to determine whether this is or is not the law of density.

The elements of the comet's orbit being known with sufficient accuracy for a calculation of the perturbations and retardation with assumed values of the planets' masses and of U, the place of the comet was computed for certain times in the years 1819, 1822, 1825, 1828. Now each of the elements might be erroneous to a small amount: the mass of Jupiter might be erroneous (the effect of the other planets is so small that the errors in their masses are not worth considering): and U might be erroneous. For the computation therefore it was assumed that

semiaxis-major $= (a) + \delta a$, (a) being a numerical value.

excentricity $= (e) + \delta e$, (e) being a numerical value.

&c.

Mass of Jupiter $= ♃ + \frac{♃}{100} v$, ♃ being a numerical value.

Coefficient of retardation $= U + \frac{U}{100} \mu$, U being a numerical value.

With these elements the heliocentric and geocentric places of the comet were computed, and the latter were given in Æ and declination. Each computed Æ therefore was obtained in this form

$$(A) + k \cdot \delta a + l \cdot \delta e + \&c. + p \cdot v + q \cdot \mu$$

where (A), k, l, &c., p, q, are all exhibited in numbers. Similarly each computed declination was obtained in this form

$$(D) + k' \cdot \delta a + l' \cdot \delta e + \&c. + p' \cdot v + q' \cdot \mu$$

where (D), k', l', &c., p', q', are also numerical values. On subtracting these from the observed Æ and declination, A and

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D, the apparent errors of observation are found (subject to uncertainty, as δa, δe, &c. are not yet determined.) The error in Æ is not a measure of the error of observation in one direction *in space:* but it becomes so if multiplied by the cosine of declination. Thus we obtain for the errors of observation

$$\cos D . \{(A) - A\} + k \cos D . \delta a + l \cos D . \delta e + \text{\&c.}$$

$$+ p \cos D . v + q \cos D . \mu$$

and $\{(D) - D\} + k' . \delta a \quad + l' . \delta e \quad + \text{\&c.} \quad + p' . v + q' . \mu.$

It must be observed that the observed Æ and declination used here were not places actually observed, but were what Encke calls Normal places, namely places concluded *immediately* from groups of observations, and probably much more accurate than any of the observations taken separately.

Now the question is, How shall the quantities δa, δe, &c. be determined so as best to satisfy the numerous observations, (Encke used 34, see page 30, line 4,) or how shall the error of each observation be determined so that the whole system of 34 errors is the most probable? The method generally received is this: Determine them so that the sum of the squares of errors of observation shall be minimum with respect to the variation of each of the quantities δa, δe, &c.: this process will give simple equations equal in number to that of the quantities to be determined. It is readily seen that the equations thus formed are

$$0 = \Sigma . k \cos D \left\{ \cos D \{(A) - A\} + k \cos D . \delta a + l \cos D . \delta e + \text{\&c.} \right.$$

$$\left. + p \cos D . v + q \cos D . \mu \right\}$$

$$+ \Sigma . k' \left\{ \{(D) - D\} + k' . \delta a + l' \delta e + \text{\&c.} + p' v + q' \mu \right\}$$

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(where Σ denotes that the sum for all the observations is to be taken) and a similar equation for each of the other quantities.

In this manner all the quantities δa, δe, &c., v, and μ, might be found at once. For clearness however in regard to some of the important points, Encke preferred omitting for the present the values of v and μ, and therefore expressed δa, δe, &c., in terms of numerical values and multiples of v and μ. Thus he got

$$\left.\begin{array}{l}\delta a = a' + s \,.\, v + t \,.\, \mu \\ \delta e = e' + s_{,} \,.\, v + t_{,} \,.\, \mu \\ \quad \&c.\end{array}\right\} \quad \text{(a', s, and t, \&c. being numerical values)}$$

and upon substituting these, each of the errors of observation had the form

$$E + F \,.\, v + G \,.\, \mu$$

and the sum of all their squares, which is of the form

$$H + Kv + L \,.\, \mu + M \,.\, v^2 + N \,.\, v\mu + O \,.\, \mu^2$$

may be put in the forms of page 29, line 4, and page 30, line 4. Thus from the observations of the years 1819, 1822, 1825, Encke found this expression for the sums of the squares of the errors (taken in seconds of space)

$$5039{,}8 + 286{,}280\,(v - 2{,}887)^2 + 8{,}763\,(\mu + 10{,}267 \,.\, v + 2{,}945)^2$$

and from those of 1819, 1822, 1825, 1828,

$$6679{,}7 + 868{,}440\,(v - 2{,}317)^2 + 207{,}092\,(\mu + 2{,}5613 \,.\, v + 12{,}200)^2.$$

These will be the smallest possible when the quantities within the brackets are made equal to zero. Thus from the three years' observation Encke was entitled to conclude as most probable

$$v = +\,2{,}887$$

$$\mu = -\,32{,}59\,;$$

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and from the four years' observations

$$v = +2{,}317$$

$$\mu = -18{,}135;$$

and with these the sum of the squares of errors were

for three years 5039,8

for four years 6679,7.

As the results differed considerably, it became a matter of judgment to determine what ought to be adopted. The principle adopted by Encke will be found in page 30, line 20; some doubt may be entertained in regard to its soundness, as in strict conformity with the method he ought undoubtedly to have taken the values deduced from the 4 years' observations. Probably one reason was that the mass of Jupiter thus obtained agreed nearly with that obtained by Nicolai and others. However if we take the sums of the squares of errors with the values $v = +1{,}201$, $\mu = -15{,}399$ (adopted by Encke) we obtain

for three years 5853,7

for four years 7764,4.

These are not much greater than the others; and perhaps Encke is perfectly justified by Nicolai's determination in making the choice that he has made. With these values of v and μ he has found for Jupiter's mass and the coefficient of resistance the values in page 31. By the use of Struve's observations instead of his own, the value of μ was afterwards slightly altered, (see page 35.)

Now we may discuss the question: Can we by any probable alteration of Jupiter's mass and any probable errors of observation avoid the supposition of a resistance? For this purpose we must make $U + \frac{U}{100}\mu = 0$, or $\mu = -100$. On

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substituting this, the sum of the squares of the errors of observation for the four years becomes

$$6679{,}7 + 868{,}440\,(v - 2{,}317)^2 + 207{,}092\,(2{,}5613\,.\,v - 87{,}800)^2.$$

If we consider $v = +\,1{,}201$ (which as we have seen has been adopted partly because it agrees with other phænomena) we find for the sum of the squares

$$1494310.$$

As this is the result of 34 observations, the probable error of any one (in the method usually received) would be found by dividing this by 34 and extracting the square root. Thus we get for the probable error (not of a single observation, but of a normal-place)

$$209'',6.$$

How unlikely this is, will be best seen from an examination of the observations, and from the circumstance that Struve considered the probable errors of his single observations to be only 3″.

We may however find the mass of Jupiter which will make the sums of the squares a minimum. For this purpose the expression must be put in the form

$$2226{,}99\,.\,v^2 - 97164\,.\,v + 1607742$$

$$\text{or } 2226{,}99\,(v - 21{,}815)^2 + 547909.$$

This is minimum if $v = 21{,}815$, or if the mass of Jupiter is increased by more than $\frac{1}{5}^{\text{th}}$: a quantity which is inconsistent with the other phænomena of perturbation (especially with those of the small planets.) And the probable error, with this violent change, is only reduced to 126″,9: which like the other is beyond all credit.

The probable error of observation with the values that Encke has adopted for ♃′ and U' is

$$\sqrt{\left(\frac{7764{,}4}{34}\right)} = 15'',1,$$

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which is not unreasonable, being perhaps too great for the later places and too small for the earlier ones.

These values, it is to be observed, are obtained entirely from the observations of the Appearances of 1819, 1822, 1825, and 1828. But it will be well to examine how far the Appearances of 1786, 1795, and 1805, confirm the hypothesis of resistance.

The magnitude of resistance supposed by Encke is such (see p. 41, and the tables p. 35, &c.) that if the comet's place were calculated with the mean motion belonging to any time, its mean longitude would at the end of one period (1208 days) be found about *one minute of space* in advance of that calculated mean longitude; at the end of two periods it would be *four minutes* in advance, &c. Let n be the number of periods reckoned from any one, suppose that of 1786; then if there were no resisting medium, the mean longitude at the end of n periods would be $A + Bn$; if there were a resisting medium, it would be $C + Dn + 1' \times n^2$. The difference would be $(C - A) + (D - B)\,n + 1' \times n^2$. Now between 1786 and 1828 there were 13 revolutions, and between 1786 and 1805, 6 revolutions. If then the place and motion in 1786 were the same in both suppositions, the difference of their results in 1805, would be 36′, and in 1828, 169′. If $D - B$ was so determined that for $n = 6$, $(D - B)\,n + 1' \times n^2 = 0$, and if $C - A = 0$, then the suppositions would give the same places for 1786 and 1805, but for 1828 they would differ by 91′. If they were altered so as to coincide for 1805 and 1828, they would make the difference in 1786, 78′. If 1785 and 1828 were made to agree, the difference in 1805 would be 42′. They might however be so chosen as to make the differences + 21′ for 1786 and 1828, and − 21′ for 1805, and this is the smallest difference that the differences of the suppositions will admit. There is no difficulty then in determining which supposition corresponds best with observations. Now Encke has stated (page 45) that his hypothesis, which represents all the later observations within a few seconds, does also represent the earlier observations within about eight minutes: and a part of this he thinks is due to the inaccurate cal-

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culations of perturbation. Consequently the supposition of no resistance must be enormously in error for some of the Appearances: and there can therefore scarcely be a doubt that the hypothesis of a resisting medium, or something which produces almost exactly the same effects, is the true one.

It will be observed that these conclusions depend entirely on calculations made by Encke, and which have not (I believe) been repeated by any other person. As far however as the skill and experience of one calculator can remove all doubts upon the accuracy of the results, they may be considered as perfectly certain. And I cannot but express my belief that the principal point of the theory, namely an effect exactly similar to that which a resisting medium would produce, is perfectly established by the reasoning in Encke's memoir.

For the convenience of those who may wish to construct the orbit of this comet, I subjoin the values of the elements which Encke has omitted.

Perihelion distance = 0,3435 } The Earth's mean distance
Aphelion distance = 4,101 } from the Sun being 1.

Semiminor axis of orbit = 1,187

Periodic time = 1210 days.

The other elements will be found in page 17. The place of perihelion, it will be seen, coincides nearly with the descending node. All these elements are considerably altered by perturbation.

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The Syndicate of the University Press have with great liberality undertaken to defray the whole expense of printing this Treatise. I have great pleasure in taking this opportunity of publicly expressing my obligations to them.

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