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On the use of statements of ancient Solar eclipses for correcting the elements of the Moon's motion, with special reference to Prof. Ginzel's „Specieller Kanon der Finsternisse“.

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This subject is of such fundamental importance both in chronology and astronomy, and Prof. Ginzel's investigations as set forth in his recent work are so exhaustive, that the principles involved in the case are worthy of more discussion than they have received. I may add that the empirical corrections to the motions of the lunar elements on which both Oppolzer and Ginzel have based their respective kanons are so radical, even revolutionary, that I hope to see the whole question more fully considered and discussed than it has yet been. Such discussion is of special importance in view of the readiness of chronologists to accept astronomical results as conclusive. I begin by briefly setting forth my own views of the proper method of treating the subject.

An eclipse depends on the motion of three elements of the lunar orbit; the node, the perigee and the mean longitude of the moon. The great precision with which the motions of the node and perigee have been determined by modern observations, and the agreement of these motions with theory, show that there is no reason to doubt the correctness either of the gravitational theory on which they are based, or of the motion derived from modern observations. An error of 3" or 4" in the centennial motion of the node, and of 10" or a little more in that of the perigee, is all we have to fear. Errors of this amount would be without any appreciable effect on the paths of ancient eclipses. We have therefore left only the mean longitude as the element to be corrected.

The mean motion of the moon, referred to the measure of time given by the solar day, is subject to changes some of which can be determined only by observation, and may therefore be regarded as more or less empirical. We have, first, the secular acceleration, compounded of the actual secular acceleration of the mean motion and of the effect of the tidal retardation of the earth's rotation. The latter can be determined only by observations of the moon; its effect therefore appears in the form of an empirical correction to the moon's mean longitude, varying as the square of the time, to be determined by observation.

Besides this we have certain inequalities of long period, amounting to only a few seconds, but introducing an element of uncertainty in the actual mean motion of the moon at our time, of which the probable amount may be $\pm 5''$

per century. This uncertainty we have to admit as one of the unavoidable errors incident to the subject. Ancient observations throw no light on inequalities so small.

If this view is correct no attempt should be made to determine the motion either of the perigee or node from ancient eclipses; but the corrections derived from them should be confined to the secular acceleration. Both Oppolzer and Ginzel have used these eclipses to correct the motions of the node and perigee. It is therefore greatly to be desired that the reasons for doing so, and thus preferring the results of ancient eclipses to modern observations, should be discussed and set forth.

We now approach another question which is equally worthy of discussion. To what extent, if at all, we are justified in determining the secular acceleration of the moon from the supposed solar eclipses recorded by ancient historians? On this question I stated my views in my »Researches on the motion of the moon«, but may be allowed to add the following brief résumé.

Since the exact moment of such an occurrence can never be determined, we have only the path of the shadow to depend upon. In order that such a datum may be useful for our purpose we must be assured that, during a well identified total eclipse, the shadow of the moon passed over a definitely known point of the earth's surface.

On searching the literature of the subject I found only one case in which this condition could be considered as satisfactorily fulfilled. This was a supposed eclipse mentioned by Herodotus and Aristides. Prof. Ginzel gives the following translation of the narrative of Herodotus:

»Mit dem Frühjahr brach das Heer gerüstet aus Sardes auf, um nach Abydos zu ziehen. Als es aufgebrochen war, da verliess die Sonne ihren Sitz am Himmel und war unsichtbar, obwohl es nicht wolkig, sondern ganz klarer Himmel war, und es ward aus Tag Nacht.«

Aristides is yet more specific in stating that an eclipse of the sun happened in the east, the region of its rising, as the king was going against Greece.

I now wish to call especial attention to the fact that these statements are the most explicit of all that we can find in classical writings. It is therefore a most remarkable and suggestive fact that no total eclipse of the sun could have occurred on the occasion in question, and that the

only possible eclipse would have been an annular one occurring two years later, about noon.

The two cases which come next in the order of certainty are the Assyrian eclipse of -763, which we might suppose to have been total at Nineveh, and an eclipse about the year -431 which Thucydides describes as total. In the first case the southern edge of the shadow passed about 100 miles north of Nineveh, and in the second case there is only an annular eclipse, the path of which did not come within 500 miles of Greece.

My own conclusions from this failure is that we cannot conclude with any certainty that the statements of unusual darkness made by Herodotus and other ancient historians really refer to total eclipses of the sun. More important yet, if an eclipse was referred to, we cannot in any instance be sure that it was total at any given place. If this view is correct the eclipse of Agathocles remains as the only one which can be used for the purpose in question. Here, the shadow path was so wide and the position of Agathocles at sea so uncertain that all we can conclude from this eclipse is its entire agreement with the received theory of the moon's motion.

Prof. Ginzel, in his researches, derives empirical corrections to the moon's motion from ancient solar eclipses alone. I have not found in his work any discussion of the general reliability of this basis. It therefore seems very desirable that the questions which I have raised should be discussed. I cannot claim to have gone over exhaustively every citation which he makes in his extended work; but so far as I can find on a cursory examination there is no case besides those I have mentioned, in which it could be assumed from the narrative that an eclipse was total at any specified place.

Of the mediaeval eclipses it may be said in a general way, that the statements that the sun was eclipsed at a given time are sufficiently explicit and reliable, but that I have not found a case in which it was specifically stated that the eclipse was total, or that the stars were visible, at a specified place. It seems to me that the question whether any result to be relied on can be derived from such data as this, is worthy of special discussion. It is of the greatest importance that both astronomers and chronologists should be made acquainted with the reasons for using such data.

Let us now consider the question from another point of view. Are Prof. Ginzel's empirical corrections compatible with astronomical observations? The observations with which they may be compared are divisible into three classes; those of lunar eclipses recorded by Ptolemy in the *Almagest*; the observations of beginning and end of solar and lunar eclipses made with instruments by the Arabs between the years 800 and 1000, and modern observations from 1600 to the present time. The results of these observations to 1870 I have discussed in the researches already referred to.

It will be of interest to include the corrections derived by Oppolzer. Those of mean longitude are given by both authors in the form of corrections to the times of mean conjunction of the moon with the sun, expressed in decimals of a day; but may be reduced to mean longitude by multiplication by the factor 43900". Their general values are:

$$\Delta T = -0.000192s + 0.0000247(1 + 1/1000s)s^2 \quad (\text{Ginzel})$$

$$\Delta T = +0.0006s + 0.00009s^2 + 0.0000009s^3 \quad (\text{Oppol.})$$

s being the time from 1800 in centuries. The corrections to the elements of mean longitude are

	Ginzel	Oppolzer
Corr. of mean long. (1800)	0".00	0".00
» » mean cent. motion	+8.4	-26.3
» » secular acceleration	-1.08	-3.95

The results of these corrections compare with those of ancient and modern observations as follows: In the first column we give the date; in the second the correction to Hansen's tables derived from observations, then the empirical corrections derived by Ginzel and Oppolzer respectively. Lastly we have the residual differences. In the case of modern observations it is a question whether we should free Hansen's mean longitude from the erroneous term of long period depending on the argument $13v - 8E$ before making the comparison. This I have done.

For the most part the corrections are not computed from the formulae I have quoted, but are taken as tabulated by the two authors.

Comparison of the corrections to Hansen's mean longitude of the moon given by ancient and modern observations with the empirical corrections of Ginzel and Oppolzer.

I. Ptolemy's lunar eclipses.

Date	Obs. minus Hansen	G. - H.	Opp. - H.	Obs. minus G.	Opp.
-687	-11'	-14'	-29'	+3'	+18'
-381	-27	-12	-21	-15	-6
-189	-20	-10	-17	-10	-3
+134	-16	-7	-11	-9	-7

II. Arabian observations.

850	-4.4	-2.9	-1.8	-1.5	-2.6
927	-1.1	-2.6	-1.2	+1.5	+0.1
986	-4.8	-2.3	-0.8	-2.5	-4.0

III. Modern observations.

Date	Obs. min. Hansen	Term of long per.	Obs. min. H.	G. - H.	Opp. - H.	Obs. minus G.	Opp.
1650	+39"	-21"	+18"	-15"	+30"	+33"	-12"
1700	+21	-5	+16	-9	+22	+25	-6
1750	0	+19	+19	-4	+12	+23	+7
1800	0	+15	+15	0	0	+15	+15
1850	0	-11	-11	+4	-14	-15	+3
1900	-22	-20	-42	+7	-30	-49	-12

For the epoch 1750 the direct comparisons of Hansen's tables with observation are so few that we only know the correction to be very small, and the same may be inferred for the epoch 1800 from Hansen's statements.

The deviations from uniform progression in the last two columns arise from the unexplained variations in the moon's mean motion shown by observations since 1650.

I need not dwell either on the discrepancy between the two authorities, or the incompatibility of Prof. Ginzel's

positive correction to the mean motion with 250 years of observations which show the correction to be negative.

Both authorities also give corrections to the arguments depending on the perigee and node of the moon, involving changes in the motion of these elements. These changes are equivalent to the following corrections to the longitudes of the node and perigee.

$$\begin{aligned}\Delta\Omega &= +36''.1 s - 1''.26 s^2 \quad (\text{Ginzel}) \\ \Delta\Omega &= +42'' s - 2''.5 s^2 \quad (\text{Oppolzer}) \\ \Delta\pi &= -78''.0 s - 2''.95 s^2 \quad (\text{Ginzel}) \\ \Delta\pi &= -26'' s - 14''.7 s^2 \quad (\text{Oppolzer})\end{aligned}$$

Washington, 1900 Dec. 5.

Of these corrections it may be said that the centennial motions are incompatible with observations during the last 200 years, and that the terms depending on s^2 are incompatible with gravitational theory.

In conclusion it seems to me that both astronomers and chronologists should appreciate very highly the historic value of Prof. Ginzel's minute and exhaustive researches. It will not, I trust, diminish this appreciation to raise the question whether corrections to the moon's elements can be based upon them. It seems to me that this question cannot be answered in the affirmative without a better basis than has yet been found.

Simon Newcomb.

Leoniden und Bieliden

beobachtet auf der Sternwarte in Odessa im Jahre 1899.

Die Beobachtungen der Leoniden auf der Sternwarte in Odessa wurden während der Nächte 13.–17. November von einer Anzahl Professoren und Studenten ausgeführt. Zum Einzeichnen der Sternschnuppenbahnen wurde eine Karte der betreffenden Himmelsgegend (gnomonische Projection, Radiant in der Mitte) lithographirt. Die Studenten wurden nach Gruppen eingetheilt — einige sollten sich nur mit der Abzählung der Leoniden befassen; andere, welche mehr mit dem Himmel vertraut waren, sollten die beobachtete Bahn in die Karten eintragen.

Es wurden auch Versuche angestellt, um die Sternschnuppenbahnen zu photographiren, und zwar mit unbeweglichen Cameras und mit Cameras, die durch Uhrwerk der täglichen Bewegung folgten; als Objective wurden benutzt: Doppel-Anastigmat, Görz, 29 mm Oeffn.; photographisches Objectiv von Cook, 6 Zoll Oeffn. und 170 cm Brennweite; Orthostigmat Steinheil, 31 mm Oeffn. (2 Exemplare); Antiplanat Steinheil, 64 mm Oeffn. Trotz aller Sorgfalt der Entwicklung konnte auf den Platten keine Spur von Sternschnuppenbahnen wahrgenommen werden. Eine nachträgliche Prüfung dieser Objective und einer Anzahl anderer, die zum Photographiren der Sternschnuppen nicht angewandt wurden, ergab, dass das Portrait-Objectiv von Voigtländer, 80 mm Oeffnung, trotzdem es in manchen optischen Eigenschaften den oben erwähnten Objectiven nachsteht, an Lichtstärke alle diese Objective übertrifft und zum Zwecke des Photographirens der Sternschnuppenbahnen von den geprüften Objectiven am geeignetsten zu sein scheint.

Bei den Beobachtungen betr. das Zählen der Sternschnuppen und Einzeichnen ihrer Bahnen in die Karte haben sich die Studenten *Okulitz, Lebedintzew, Chado, Sakelari, Olkin, Kononowitsch, Wasiliew, Kachnew* und der frühere Officier der Handelsmarine, Herr *Steffanowsky*, betheiligt. Ihre Namen sollen im Folgenden durch Ok, L, Ch, S, Ol, Ko, W, Ka und St bezeichnet werden; vereinzelte Beobachtungen dieser Art haben auch der Astronom der Pulkowaer Abtheilung in Odessa Herr *Orbinsky* und der Astronom an der Universitäts-Sternwarte Herr *Babitschew* (im Folgenden durch Or und B bezeichnet) gemacht. Die Herren Babitschew und Steffanowsky

haben die Berichte der verschiedenen Beobachter der Zeit nach zusammengestellt und die Coordinaten der Endpunkte der gezeichneten Bahnen gemessen.

In der Nacht am 13. November fing der Himmel erst um 16^h 15^m an sich aufzuklären, auch von da an bedeckte er sich zeitweise mit Wolken; am 14. fing er um 14^h 30^m an sich aufzuklären und um 15^h war es ganz klar, um 18^h kommen kleine Wolken, die Bilder der Sterne sind unruhig; am 15. November war es klar von 12^h an, Reif auf den Karten, sehr gute Bilder der Sterne; am 16. November war es klar nur zwischen 17^h 20^m und 18^h 10^m; am 17. November war es klar während der ganzen Nacht.

Die Zahl der beobachteten Leoniden während je einer halben Stunde war folgende:

M. Z. Odessa		Zahl der Leoniden
14. November.		
Von 15 ^h	bis 15 ^h 30 ^m	8
» 15 30 ^m	» 16 0	10
» 16 0	» 16 30	11
» 16 30	» 17 0	20
» 17 0	» 17 30	37
» 17 30	» 17 46	6
15. November.		
Von 13 ^h 0 ^m	bis 13 ^h 30 ^m	5
» 13 30	» 14 0	4
» 14 0	» 14 30	7
» 14 30	» 15 0	14
» 15 0	» 15 30	9
» 15 30	» 16 0	20
» 16 0	» 16 30	17
» 16 30	» 17 0	19
» 17 0	» 17 30	24
» 17 30	» 17 54	18
16. November.		
Um 14 ^h 50 ^m	(aus den Wolken)	1
Von 17 ^h 20 ^m	bis 17 ^h 56 ^m	11