THE CALCULATION OF AN ECLIPSE OF THE SUN ACCORDING TO THEON OF ALEXANDRIA

A. Rome

In English-speaking countries, Theon of Alexandria, and still more his laughter, Hypatia, are well known through a novel by Charles Kingsley. Whether or not there are still many people who actually have read it, I don't mow. Novels grow old quickly, and that particular one is nearly a centenarian, ıaving been published in 1853. It is also rather difficult to understand for anyone vho is not acquainted at the same time with ancient Greek literature and history, he history of Greek science, and ancient church history, as well as the religious ıistory of England in the Victorian Era, Wiseman, Pusey, Newman, and the ther Tractarians. But I often remarked in England that the name Hypatia apealed to many people there, and I should not be surprised if it were the same here.

Now, I happen to be engaged in the editing of the *Commentary of Theon of 1lexandria on the Almagest of Ptolemy*. In the course of that work, I found that Iypatia had made a revision of the third book of her father's big work, which vas hitherto unknown, and which is now the only text of Hypatia actually ublished. By the way, those 150 pages show that Kingsley, in his chapter ntitled Nephelococcugia (an allusion to "The Birds" of Aristophanes), was erhaps not altogether well inspired when he imagined Hypatia speaking like 'roclus and making mystical speculations on mathematics. The only text we an read of hers is rather elementary, but quite sound, mathematics and asronomy. It is without all the mystical nonsense some people of the second ophistic period were dreaming à propos of still more elementary mathematics.

My editorial work has just reached the Sixth Book of the *Commentary on the lmagest*, which explains the theory of eclipses of the sun and moon. As an xample of the calculation of an eclipse of the sun, Theon has taken the eclipse f June 16, 364 A.D. We shall rapidly examine it. The Greek text of it is not et completely "established" as we classical philologists say. Before going to he printer, it will undergo a few minor changes. But on the whole, I think it s already sufficiently secure to enable me to speak about it.[1]

[1] The members of the Congress present at the address were given an offset copy of the 'hole calculation of Theon, which he made twice: first according to the *Almagest*, and a econd time according to the *Handy tables* of Ptolemy. I took those calculations from differnt places: the first from the Sixth Book of Theon's *Commentary on the Almagest*; the second om the *Small commentary* of Theon on the *Handy tables* (ed. Halma); the third from the 'reat commentary of Theon on the *Handy tables*, which is unpublished. As my text of the ixth Book is not yet established, I shall not publish the full calculation here, for it surely 'ould make the present article longer than it ought to be. I have changed here the text f my address in such a way as to make it comprehensible for a reader who does not have ıe full calculation under his eyes. I note here only that the offset copy corrects a false xplanation I had given in my edition of Pappus' *Commentary on the Almagest* in 1931: . lxix, instead of $c + d' + x' + a$, it should have been $d' + x' + a$.

Theon says that he has observed the eclipse of the sun of June 16, 364, and he gives the times he has recorded. On the other hand, at several places in his Sixth Book, he calculates the different elements of the same eclipse, to show how it must be done, and also to prove that the results of the *Handy tables* are tolerably concordant with the results of the *Almagest*. He explains also the trigonometrical proofs of the *Almagest*, and occasionally adds some proofs of his own. I shall show you one of these trigonometrical computations, selecting a very short one, for the greatest defect of ancient trigonometry is that it is very cumbrous to handle, and generally frightfully long.

Both in the *Almagest* and in the *Handy tables*, the time of the eclipse is determined by seeking first the time of mean conjunction, then the time of true conjunction, and finally, taking the sun's and the moon's parallax into account, the time of apparent conjunction for the given place, which is here Alexandria. Having thus found the elements of the eclipse, he proceeds to compute the circumstances, the magnitude, and the time table, of which he gives two successive approximations. Finally, there is a calculation of the "prosneusis", an azimuthal direction that enables one to find the place on the sun's disc where the first and last contacts can be observed.

Having thus taken a bird's eye view of the calculation, we can now examine some details.

The date is given according to two systems: the "vague" year of 365 days, and the "Alexandrian" one of 365 days with a leap year every fourth year: the Julian calendar is based on Alexandrian astronomy. In the corresponding explanations, Theon gives a model of the calculations needed to pass from one system to the other. Those models enable us to fix with absolute certainty, if there were any doubt about it, when the leap years were started at Alexandria. The names of the months might seem more puzzling. Every month has 30 days, and after 12 months there are 5 or 6 complementary days. Thoth, Payni, and so on, are the original Egyptian names, which were used by the Alexandrian Greeks. Alexandria was a bilingual town. By the way, you know that the French revolution at the end of the 18th century tried to introduce a new calendar. In fact, it was simply the Alexandrian one, with new names, starting with the autumn equinox, because the "Alexandrian" year started about that time, August 29 or 30. How the French came to that idea is simple to find when one knows that the decimal system of weights and measures was in great part organized by Delambre, the man who made the triangulation of the arc of meridian Dunkirk-Paris-Barcelona, which was originally the base of the decimal meter. Now, that same man was also an historian of astronomy; his history of Greek astronomy is not yet completely superseded, after 150 years. So it is easy to guess where he found the original version of Floréal and Germinal and Messidor.

Theon says he has observed the eclipse. For the first contact he says he is quite sure of the time, and for the last, he says it is approximate. Now, Delambre finds that this result is too beautiful to be true. But Delambre has a knack of casting a doubt on the reality of all sorts of observations made by Greek astronomers. Strangely enough, people do not read Delambre any more (and in this, I think they are wrong), and still, all those hypercritical doubts are lingering about in general histories of astronomy.

First, note that Theon does not give exact figures for the minutes: he observed the first contact on the 24th of Thoth, year 1112 of the era of Nabonassar, $2\frac{1}{2}\frac{1}{3}$ seasonal hours P.M. The last contact, approximately $4\frac{1}{2}$ seasonal hours P.M.[2]

Then if we compare the calculated time tables given by Theon, we see clearly what he means: He finds the first contact according to the *Almagest*, at $3\frac{1}{3}$ equinoctial hours P.M. which he reduces to $2\frac{1}{2}\frac{1}{3}$ seasonal hours P.M., but according to the *Handy tables*, he finds $3\frac{1}{4}$ equinoctial hours, and he reduces it again to $2\frac{1}{2}\frac{1}{3}$ seasonal ones. The maximum phase, according to the *Almagest*, at $4\frac{1}{3}$ equinoctial hours, is taken to be equivalent to $3\frac{1}{2}\frac{1}{4}\frac{1}{20}$ seasonal ones; according to the *Handy tables* it is at $4\frac{1}{4}$ equinoctial hours or $3\frac{1}{2}\frac{1}{6}$ seasonal hours. The last contact, according to the *Almagest*, is at $5\frac{1}{4}$ equinoctial hours, or $4\frac{1}{2}$ seasonal; according to the *Handy tables* it is at $5\frac{1}{6}$ equinoctial or $4\frac{1}{2}$ seasonal. Maybe some of those figures will be corrected when my text is completely "established", but it is quite clear that the objection of Delambre is not justified. In fact, I am under the impression that whenever Theon or Ptolemy uses Egyptian fractions instead of sexagesimal ones, he intends only to give a rougher approximation.

Can we get an idea of their approximations? Ptolemy (*Almagest*, ed. Heiberg, 1st vol., p. 505, 24) speaking of eclipses, considers $\frac{1}{16}$ of an equinoctial hour as negligible. Theon considers as small an error as one that can be reckoned to 10 minutes.

It is interesting to compute our eclipse with modern tables. The concordance is fairly good. With the tables of P. V. Neugebauer, which are Schram's tables corrected to bring them into accord with Schoch's, I found for Alexandria: first contact, $15^h\ 12^m$ true time; maximum phase $16^h\ 25^m$; last contact $17^h\ 20^m$; magnitude 5.5. (Theon finds with the *Almagest*, 4; 39, 18 digits, and with the *Handy tables* 4; 58 digits.) This compares fairly well with the above results in equinoctial hours. If we use Oppolzer, *Kanon der Finsternisse*, we get the first contact at $15^h\ 28^m$ true time at Alexandria; maximum phase, $16^h\,20.88^m$; last contact, $17^h\ 19^m$; magnitude 4.3. Ginzel, *Spezieller Kanon*, speaks of our eclipse, p. 213. I am afraid he has not been able to read the "Egyptian" fractions in the old Basel edition of 1538, and his figures are all wrong.

But, writing this, I see already some of the readers preparing an objection:

[2] This somewhat puzzling way of writing the fractions as a sum of elementary fractions each having a numerator equal to unity (with the exception of 2/3 which had a sign of its own), was taken by the Alexandrian Greeks from the Egyptians. An alternative way of writing fractions, which gave any degree of accuracy that might be wanted, was the sexagesimal system.

The "seasonal" (day) hour is the calculated 12th of the time between sunrise and sunset, neglecting refraction. The "equinoctial" hour is practically the same as our true solar time.

can we use a modern table to control the accuracy, to a few minutes, of an observation by Theon? I think we may not. At any rate, we may not always. The astronomers who make tables of eclipses choose the coëfficients of their formulae to fit observations that they deem interesting. Oppolzer took a minimum of them, 7 from antiquity, 3 from the middle ages, with the result that his *Kanon der Finsternisse* does not fit exactly the observations from 1650 to our time. Ginzel gave the maximum weight to the eclipses of +71 and +1385. For the rest, he considered only 3 eclipses of antiquity (all after Plutarch, 2nd century A.D.) and all the medieval ones; but he, too, overlooked all the modern observations.

Schoch tried to fit in the modern observations, too. P. V. Neugebauer has shown, for instance, how Schoch's tables give an excellent description of the sun eclipse of April 17, +1912: For the point chosen, Senlis in France, the curve of centrality calculated with Schoch's tables is not farther from what most probably happened, according to observation, than the curves given by the current almanacs, which are all slightly different from one another. But in addition to this, Schoch took into account all the sun eclipses of antiquity he could know, and all the eclipses of the Middle Ages. For Greek antiquity, he depends on Fotheringham's *A solution of ancient eclipses of the sun.*[3] Now Fotheringham has examined also the eclipse of June 16, 364. You see that if we try to control Theon by means of Schoch's tables, we are arguing in a circle.

I would make some remarks about that article of Fotheringham.

He calls his number 8, *The eclipse of Hipparchus*. When he wrote his article, he had to rely upon a partial edition of a few pages of Pappus, by Hultsch. Now the full text of Pappus' Fifth Book is printed. But there is much hesitation about the date of that eclipse, and I dare not say anything about it.

Fotheringham's number 11 is just our eclipse of June 16, 364. He took all the particulars about it from the only edition available, the Basel edition of 1538. There, the observed times of Theon are recorded distinctly as being equinoctial hours. The consequence of this was that the time supposed to be recorded by Theon could not be brought into line with the other eclipses recorded, and with the theory. Finally, Fotheringham admitted that "The time of Theon was 37, 32, and 38 minutes slow at the three observed phases of the eclipse."

First, I showed you that Theon did not give the time to the minute. Then, letting alone the question of night determination of time, which is of course irrelevant here, I am afraid it is impossible to admit that Theon had such wretched sundials at his disposal. But there is another thing: although my text of the Sixth Book is not yet definitively established, I am quite sure that the original text of Theon did not speak of equinoctial hours; in the best manuscript, the Medicaeus 28, 18, the word equinoctial is absent. Besides, Theon says that the calculated hours conform to the observed ones. But the conformity exists only with the calculated seasonal hours, and of course they cannot be mistaken for equinoctial ones, as the whole calculation is given. If the hours recorded by

[3] Schoch has been the assistant of Fotheringham at Oxford. Reciprocally, in his article, Fotheringham has used a first redaction of Schoch's tables.

heon are seasonal, they fit fairly well with the hour angles called "set *A*" by otheringham, which are obtained by combining Newcomb's tables with rown's movements of the moon and Fotheringham's obliquity of ecliptic.

In consequence, if tables of syzygy were to be calculated again on the prin- ples of Schoch, there would be some change in two or three of the equations.[4]

One might ask how they proceeded to observe an eclipse of the sun. Up to now, don't think a text has ever been discovered describing the method of observing , when the sun is not quite close to the horizon. Probably the ancient astron- ners found that detail too trivial to mention. Many systems have been sug- ested, but I venture to add that there is a possibility which all historians always em to overlook or to discard. When making glass, the trouble is not in getting coloured, but in getting it white. If we now consult the archaeologists, we shall e that the glassmakers were able to produce glass (and transparent glass, too) practically all shades, up to black. Alexandria was especially renowned for s blue glass, from dark to light blue. On the other hand, if we look at the *Optics* Ptolemy, we shall find that he could cut and polish glass to plane or cylin- rical forms. The blue Alexandrian glass was exported as a half-finished product the form of cubes, which were used by glassmakers very far away, up to the hine. I think it was not at all difficult for an astronomer at Alexandria to find tolerably plane screen that would do nicely to observe the sun. And it is not travagant to suppose that they had noticed it was possible to look at the sun rough that screen. Think also of the famous emerald of Nero.

This suggestion that they might have resorted to glass screens is of course a ere hypothesis, but I think that hypothesis is not worse than the others gen- ally advanced in that connection.

Ginzel points out that screens seem to have been used only since the 17th ntury. This does not prove that the ancient Greeks did not know them. On e other hand, it would be a grave exaggeration to say that, as they had coloured ass and could not possibly not see that it was an excellent screen, they surely ed it. When you have in hand all the elements of the solution of a problem, u are not yet saved if you don't realize that you have found the solution. Of is I shall give you a clear example when I show you a trigonometrical solution.

Next we may perhaps find some interesting things, if we look at the calcula- ons of mean conjunction according to the *Almagest* and the *Handy tables*.

Ptolemy says distinctly in the *Almagest* that the mean conjunction might be mputed simply by using the tables of the sun's and moon's mean motion. In ct, in the *Handy tables*, there is no table of mean syzygies. As the moon epi- cle's center at the time of mean syzygy must be on the apogee of the eccentric, find the time of mean conjunction the only thing we have to do, when we have e moon's position for the beginning of the month we are inquiring about, is to ek in the tables for the epicycle's center quantities that will complete the 360 grees of a circle exactly. The days and hours corresponding to those arcs, are

[4] I say three because I think that, upon closer examination, the famous eclipse of Archil- hos should receive a weight of "nil".

the sought days and hours of the mean conjunction, and the only thing we nee to do is to fill in the other columns for the same days and hours to get all th required information.

In the *Small commentary on the Handy tables,* we find an alternative wa which is interesting, because it is exactly the method that is followed to dete mine the date of Easter in the church calendar. Even the word "epact" is use by Theon. I say Theon, although I cannot yet guarantee that this place is no interpolated. But it seems prima facie to be authentic. The Ptolemaic astronom has surely been used by the experts who tried to solve the vexed question of th Easter date. The council of Nicea took place in 325, just about the time whe Pappus wrote his *Commentary.* I think the date of the equinox was fixed on th 21st of March, because the tables of the sun in the *Almagest* point to that dat for 325 A.D. Tannery supposed that the council relied upon an observatio of the equinox, but as the council took place somewhere in May, it was to late to make such an observation. It was much easier and more in accord wit the habits of the period, too, to have it computed from the *Almagest* or fro the *Handy tables.* So the system of epacts might also derive from the Ptolema School.

In the *Almagest,* Ptolemy has found it useful to make special tables of syzygie one for new moons and another for full moons. He starts from the 1st of Thot of year 1 in the era of Nabonassar. From the elongation at this moment, an the table of moon's mean elongation, he can find the time of the first new moo of that year. From there he starts his cycles of 25 years.

Now those cycles of 25 years have been quite recently studied by Professor Neugebauer and van der Waerden. Egyptian texts published by Professo Neugebauer use just the same lunar cycles of 25 years, and suppose that 2 "Egyptian" years are exactly as long as 309 synodic months. Ptolemy knows more accurate equivalent, viz, 309 months minus 0; 2, 47, 5 days.

Did Ptolemy know Egyptian texts of that type? In more than one place i the *Almagest,* he assumes a distinctly polemical tone, to say that people thinkin they can make tables valid for all times to come cannot be considered as seekin the truth. It is tempting to suppose that the Egyptian texts in question ar just based upon those "eternal tables."

On the other hand, the *Commentary of Theon* on the Sixth Book makes anothe hypothesis which can be interesting, too. The table is supposed to have 4 lines. That number of 45 lines plays a great role in the *Almagest.* A whole lo of tables are expressedly mentioned as having 45 lines. Such a mention gave very quick way of controlling whether the copy was accurately done. Ther are many places in the *Almagest* and its commentators where the reader i given practical hints on how to detect errors in his manuscript. That was c course the great danger to avoid when using tables. 45 lines is nearly the highes number of lines found on Greek literary papyri.[5] Publishers were consequentl bound to use the largest usual size of papyrus rolls when they copied the *Almo*

[5] Cf. Kenyon, *Books and readers in ancient Greece and Rome,* Oxford, 1932, p. 56.

st. Now, with 45 lines, and periods of 18 years, as in the other tables of the *lmagest*, we come only to 810 of the era of Nabonassar (63 A.D.). Hipparchus orked about 625 Nabonassar. But the time of Ptolemy is about 885 Nabonasar. Ptolemy has obviously kept the disposition of Hipparchus' tables for the ean motions of sun, moon, and planets. When using those tables after 810 abonassar, it is quite simple to put down first the data for 810 and then add a ifficient number of 18 year periods to reach the given dates. But that does not ork with the table of syzygy. Accordingly, he took cycles of 25 years, which rought him up to 354 A.D., quite enough for him, but not for Theon. But tolemy was not a believer in "eternal tables." His successors, disregarding his arnings, because they were no more able to do original research work, simply ut appropriate prolongations in their manuscripts. There are even manuscripts here one can see by different hands the successive prolongations of the Byzanne period, many centuries after the period of validity of the tables.

In the *Handy tables*, 25 year cycles are used everywhere, which, further, start om the first year of Philippus, not of Nabonassar.

The table of syzygies is also an exception in the *Almagest* for a second reason: iere the cycles are tabulated under the ordinal number of the year, and not as erywhere else under the number of years elapsed since the beginning of the a. This last way of tabulating is the cause of many distractions. I have had veral opportunities of experimenting with it. In the *Handy tables*, the cycle guments are given as in the table of syzygies.

In fact, you see that Ptolemy has probably kept in the *Almagest* the disposion of Hipparchus' tables. He says expressly how he corrected them. But in ie table of syzygies, which probably did not exist in Hipparchus' works, he ied another disposition, which he adopted completely in the *Handy tables*. hat table of syzygies marks thus a transition between his two great astronomil works.

We might now have a look at a trigonometrical calculation. As I told you, I iall take the shortest possible case, because ancient trigonometry is always ery simple (all problems of spherical trigonometry are solved by application one theorem, always the same; and all problems of plane trigonometry are lved as I shall show you); but it is nearly always very long. That is the reason hy I am sure that Ptolemy had a whole regiment of calculators, perhaps slaves, his disposal. Otherwise, he could not possibly have made all the tables that e published under his name: I don't think it was possible for a calculator orking very rapidly and quite used to the Greek methods, to find one single itum of the fourth column in the *Handy table* of parallaxes in less than two hours. he practical way of doing it is explained by Theon. I had to use it, with the elp of a machine, to find a missing figure. It took me twice as much time as iat. The existence of those calculators can even be detected, I think, in the table trigonometrical ratios.

We shall take as an example the table of eclipses in the *Almagest*. There is table for the maximum distance of the moon, and another for the minimum distance; and finally, a table of corrections for intermediate positions. Tha table of corrections is made on the assumption that the digits of the eclips and the length of the moon's path during the eclipse vary proportionally to th distance of the moon from the earth. The same table is used also for parallaxes Accordingly, the calculation I shall show you is not to be found in the Sixtl Book of the *Almagest*, but in the fifth (ed. Heiberg, p. 433), where Ptolem explains how he has computed the seventh column of his table of parallaxes

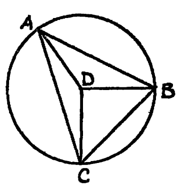

FIGURE 1

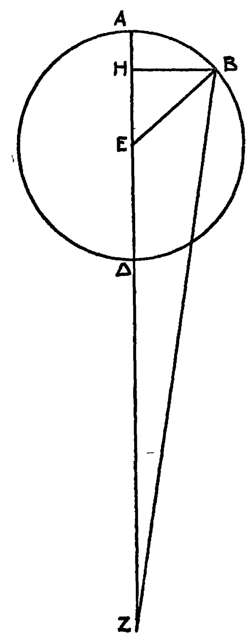

FIGURE 2

The solution is based on the following principle: if we have inscribed triangl ABC, and if we have made a table of chords of all the arcs of a circle, takin the radius as unity, it is clear that $CB = \text{crd } 2\, CAB \times DB$.

You see at once that all the triangles can be solved, with a little patience You see also that the solution is general and applies to any triangle. Well, th Greeks never saw it. They applied it only to rectangular triangles, and for th others always took the way around of cutting them into two rectangular tri angles. This is the example which I promised to show you of the fact that whe all the elements of a solution are known, there may still remain the need t realize that the solution is found.

If we now call Z the center of earth, E the center of the epicycle, with A a apogee and Δ as perigee, let B be any position of the moon on its epicycle. Th value tabulated is $ZA - ZB$ for $A\Delta = 60$, that is to say, taking $A\Delta$ as a unit, sinc we are in the sexagesimal system. For want of a symbolic notation like ours, th

ncients were bound to give their general solutions under the form of a numeri-
al example. Let us take AB = 60 degrees.

We shall now proceed to solve the triangle HEB.

If we circumscribe a circle around the triangle HEB, the diameter will be B, the chord of 2HEB = HB = crd 120 degrees, and HE will be the chord of HBE = crd 60 degrees.

The Greeks used to express that by saying: having angle HEB = 60 parts of hich there are 360 in a circle, we write down the same angle in parts of which iere are 360 in a half circle, that is, angle HEB = 120 degrees.

That way of speaking blurs somewhat the logical principle of the solution, hich I explained a moment ago.

Anyhow, we get now, taking the value of chords in the table of the first book ι the *Almagest*,

$$BH = \text{crd } 120 \text{ degrees} = 103;\ 55$$
$$EH = \text{crd } 60 \text{ degrees} = 60;\ 0$$
$$EB = 120,$$

B being the diameter, and the radius of the circle being 1. If Ptolemy had sed the sexagesimal notation consistently, he would have written something ke 2, 0; 0 = EB. But he does not use sexagesimal notation for the entire part his numbers,[6] and he writes EB = 120, just as he writes BH = 103; 55 and ot 1, 43; 55.

Now, as EB is the radius of the epicycle, and ZE the distance of the center the epicycle from the earth at the time of the eclipse, he has calculated else-here that ZE/EB = 60/5; 15. Reducing proportionally the above equalities, e have:

$$EB = 5;\ 15$$
$$BH = 4;\ 33$$
$$EH = 2;\ 38$$
$$EH + EZ = 62;\ 38$$
$$ZH^2 + HB^2 = ZB^2$$
$$ZB = 62;\ 48.$$

Now

$$ZA = 65;\ 15$$
$$A\Delta = 10;\ 30$$
$$ZA - ZB = 2;\ 27 \qquad \text{if } A\Delta = 10;\ 30$$
$$ZA - ZB = 14 \qquad \text{if } A\Delta = 60.$$

onsequently, the value tabulated for the point B will be 14.

The same table of corrections is used, as I told you, in the table of parallaxes here it is the seventh column. It is easy to see that the data for AB = 12 degrees, degrees, 36 degrees ··· have been directly calculated, and that in our table

[6] Must I add that he does not use Arabic ciphers nor signs equivalent to our symbolical ιtation?

of corrections, one value of ZA − ZB has been interpolated between each pa of calculated data, while in the table of parallaxes, two such values have bee interpolated. The fact is immediately detected by forming all the differenc between all the data of both tables.

It happens often that tables of the collection contained in the *Handy tabl* have been obtained by interpolation. These interpolations are simply propo tional, but whenever there are remainders to distribute, that distribution is n made at random. Ptolemy had instinctively, if not explicitly, the sensation th it was better to reproduce more or less in the interpolated terms the variatio of the calculated terms. Theon, in his *Great commentary on the Handy table* calls the attention of his readers to that point. Another curiosity of the *Hand tables* is that, in order to save labour, Ptolemy sometimes uses a table that w not originally meant for the problem he is solving, but which gives a numeric answer to a sufficient degree of approximation. We met a curious example that process in the calculation of the moon's speed according to the *Hand tables*: take in the Προκανόνιον the number corresponding to the datum calle "moon's center," divide by 10 and add 0 degrees 30′: you have the moon speed with an approximation of to within 1 minute.

We might finish our very incomplete exploration of the Sixth Book of Theo by having a look at the calculation of prosneusis. As it is always difficult t see the first and last contacts of an eclipse, it may be useful to know beforehan in which part of the sun's disc it will happen. Our modern astronomical almana always mention it. The astronomers of the Ptolemaic school used to calcula the point where a great circle passing through the sun and moon's centers shoul cut the horizon. If they put one of the circles of a spherical astrolabe at the sam time on the sun's center, and on the azimuth so defined, that circle was also o the point of the sun's disc at which to look. That was, I think, the use of th prosneusis computation. I don't think it had any astrological significance. A any rate, I could not find, either in the *Tetrabiblos*, the great bible of astrolog nor in the catalogue of Cumont, any text showing an astrological significan such as would, for instance, point out to what country the omen would appl If there is no astrological significance in the prosneusis, I don't see any possib use for the prosneusis except the one I mentioned.

Delambre pretends it was not used for that purpose, because he thinks th ancient astronomers did not care to observe the first contact accurately. I ha spoken already of this hypercritical opinion.

It would have been possible to compute the prosneusis for each case ver accurately. But, as Greek trigonometry was very cumbrous to handle, th computation would have been very long. Accuracy on that point was not r quired. Ptolemy has, consequently, given in the *Almagest* and in the *Hand tables* approximate methods that are said by Theon to give concordant result In fact, if we try them both on the eclipse of June 16, 364, we get with th *Almagest* a prosneusis of 16°51′ from W in the northern direction, and with th *Handy tables*, a prosneusis of 21°41′ from W in the northern direction. If th ircle of the astrolabe were put exactly on the center of the sun, and on the points f the horizon indicated by those calculations, this shifting of 4° on the horizon rould of course have an influence on the location of the point where the first ontact was to be looked for. In our particular case, the circle of the astrolabe rould indicate two points situated at 4° from one another on the circumference f the sun. But as the diameter of the sun is seen under an angle of 1/2°, that rc of 4° on the circumference of the sun would have been seen under an angle f less than 0°1′, which could not be appreciated with naked eyes; in both cases, ıe circle would point exactly to the same spot of the sun's disc. Thus it can be kplained why Theon calls both results equivalent.

This conclusion is at variance with the conclusion of the article that I pubshed in 1948 on the prosneusis. And I am not sure that I shall not change my ıind again before I publish the fourth volume of the commentaries on the *.lmagest,* where those notes will be printed. In fact, some of the Greek texts 'e have been exploring now together are not quite ready for publication, and ›me of them are not even ready at all. But I thought you would prefer an kploration of this kind, even with its hesitations and mistakes, to looking at ıings which are better settled but already published everywhere.[7]

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