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Discussions.

[The contributions contained under this heading are comments on articles in the previous number of the CHURCHMAN. The writer of the article criticized may reply in the next issue of the magazine; then the discussion in each case terminates. Contributions to the "Discussions" must reach the Editors before the 12th of the month.]

ASTRONOMICAL EVIDENCE FOR THE DATE OF THE CRUCIFIXION.

(The "Churchman," June, 1912, p. 469.)

MR. MAUNDER'S article, "Astronomical Evidence for the Date of the Crucifixion," which appeared on pp. 469-472 of the current volume of the CHURCHMAN, is, in form at least, a discussion of a paper by my brother, the Rev. D. R. Fotheringham, with the same title, but it is largely devoted to a criticism of my paper, "On the Smallest Visible Phase of the Moon," which appeared in *Monthly Notices of the Royal Astronomical Society*, 1910, lxx., pp. 527-531, and which Mr. Maunder had previously discussed in the *Journal of the British Astronomical Association*, 1911, xxi., pp. 355-362. Mr. Maunder, in his earlier, but not in his later paper, also discusses my paper in the *Journal of Theological Studies*, 1919, xii., pp. 120-127, in which I applied to the date of the Crucifixion the results obtained in my earlier paper. As Mr. Maunder's papers dispute the validity of the deductions which I drew from Schmidt's observations at Athens, on which my earlier paper is based, and as they also dispute my application of these deductions to the date of the Crucifixion, I think it due to Mr. Maunder's eminence as an astronomer to attempt some reply.

Mr. Maunder's criticisms appear to me to be affected by a certain misunderstanding of the purpose of my earlier paper. I was not there concerned with the conditions of a record observation, and did not wish to discover what was the lowest altitude at which the young moon might ever be observed with a given difference in azimuth from the sun; I merely wished to know what minimum altitude at sunset should be regarded as corresponding to a given difference in azimuth for the purpose of calculating the beginning of a lunar month in a calendar governed by observation. For this purpose I was only concerned directly with the normal conditions of visibility. The importance of abnormal conditions would for my purpose depend on their frequency. I found a formula which satisfied sixty-nine out of seventy evening observations and five out of six morning observations, and I think I was justified in inferring that in the evening at least abnormal conditions are sufficiently rare to permit me to treat this formula as a rule for the calculation of the first appearance of the moon for chronological purposes. This result was confirmed by a comparison of my formula

with one given by Maimonides, who lived among peoples who regulated their calendar by observation, and whose rule would naturally tend to agree with the observations on which the actual calendar was based. He would certainly not be likely to treat the moon as invisible at an altitude at which it was frequently observed. I may add that the minimum distance of 12° from the sun, which I require for the visibility of the moon, agrees with that usually given by Arab astronomers, who were accustomed to a calendar based on observation, though I am not aware that any of them attempted a rule which takes into consideration altitude as well as elongation.

This difference of purpose would help to explain some of the specific objections which Mr. Maunder takes to my paper. In his later paper Mr. Maunder summarizes the conclusions of my earlier paper as three in number, each of which he regards as erroneous.

"First," says he, "he laid down a rule for determining a limit below which the young moon cannot be seen. The limit thus determined is, in fact, that above which the young moon ought to be seen if properly looked for—quite a different matter." Here I may be allowed to remark that the word "cannot" does not occur in my paper. If by "cannot" Mr. Maunder means "can never," I do not accept his interpretation of my paper. My paper actually recorded one morning and one evening observation below the limit defined. My words were: "The following table satisfies all the observations except Nos. 2 and 43." I cannot imagine how Mr. Maunder read this sweeping negative into my words, except on the assumption that he was interested primarily in record observations, and was therefore looking for a line below which the moon can never be seen.

"Second," says Mr. Maunder, "this rule was determined from observations made only in N. Lat. 38° . The latitude, therefore, naturally does not appear in the rule, and Dr. Fotheringham drew the unwarranted deduction, in which his brother has followed him, that the smallest phase of the moon visible is independent of the latitude of the place of observation." Later in the same article he writes: "The problem is analogous to that of the visibility of Mercury, and it is well known that Mercury is much more easily seen in low latitudes than in high—indeed, it is a fact that I have often verified by my own observations. Mercury, generally a difficult object here in England, is not only an easy object, but a conspicuous one, in the latitudes of Athens or of Jerusalem." Now, a reference to my paper will show that I did not assert that the smallest phase of the moon visible is independent of the latitude of the place of observation, but that the solution given in my paper is independent of differences in latitude. The fact is that latitude enters into the problem through its effect on the positions of the sun and moon in relation to the horizon. My solution, being expressed in terms of altitude and azimuth, has taken account of this effect of latitude, and does not need

to be corrected for a change in latitude. When Mr. Maunder says that Mercury is much more easily seen in low latitudes than in high, he does not mean that Mercury is seen lower on the horizon in low latitudes, or that it is seen there with a smaller depression of the sun below the horizon, but that Mercury is seen in low latitudes on evenings or mornings when it is not seen in high latitudes. The reason for this is that in low latitudes the ecliptic is more nearly perpendicular to the horizon than in high latitudes, and the more nearly perpendicular the ecliptic is to the horizon, the greater the altitude of Mercury at a given depression of the sun, and therefore the greater the ease with which Mercury can be seen. We need to know the latitude in order to calculate the altitude and depression of Mercury and the sun respectively, but the same formula expressed in terms of altitude and depression ought to suit all latitudes. It seems to me that Mr. Maunder's exception only goes to prove my rule.

"Third," says Mr. Maunder, "he drew the conclusion, which he strongly emphasized, that it is also independent of the atmospheric conditions. This is manifestly absurd, and was only reached by including a great number of irrelevant observations, and by disregarding those which were relevant but inconsistent with the conclusion sought. In effect, Dr. Fotheringham committed the solecism of asserting that the young moon could not possibly be seen under conditions when the observations he was discussing stated that it had been seen. It has been since easy for me to collect other well-authenticated instances in recent years of similar 'impossible' feats having been successfully performed."

This indictment sounds rather appalling, but I hope that a little explanation may show that I have asserted nothing so absurd as Mr. Maunder supposes. Perhaps I may begin by a mild protest against the quotation marks in which Mr. Maunder has placed the word "impossible." The word does not occur in my paper; it belongs to what I hope I have shown to have been an erroneous interpretation of that paper. Nor did I even assert that the smallest phase of the moon visible is independent of the atmospheric conditions. What I did assert was—"That there should be only one discordant evening observation among so many is remarkable, and seems to show that, given a clear sky, *the problem is almost purely astronomical, and not atmospheric.*" What I meant was that the conditions on a clear evening at a given place do not differ so much from day to day as to make it necessary to consider anything but the relative positions of the sun, the moon, and the horizon. I introduced the word "almost" because I recognized that instances do occur when the uniformity of the conditions governing the visibility of the moon is broken, perhaps because of some extraordinary clearness of the atmosphere, such as, I am told, practised observers have noticed from time to time. Mr.

Maunder asserts that my brother refutes my conclusion by claiming "that an observation in the morning might be made at a smaller distance from the sun than in the evening, on account of the better atmospheric conditions of the morning air." I fail to see the inconsistency. I had, as may be seen by reference to my paper, confined my general proposition to the evening because of the weakness of the evidence for morning conditions. When I assert an approximation to uniformity in the conditions obtaining on clear evenings, I do not assert any identity between evening conditions and morning conditions.

The criticism quoted above of my inclusion or neglect of different observations, which is more fully elaborated in Mr. Maunder's earlier article, does not appear to me to be justified. I had approached the investigation, expecting to find, not a dividing-line, but a dividing-belt, on one side of which all the observations would be positive and on the other side negative, with mixed positive and negative within the belt. Having determined that the series of observations recorded by Mommsen was one which was likely to suit my purpose, I examined the whole series without making any selection, and found that the observations gave me, not a belt, but a line, with two isolated positive observations on the negative side of the line. The inclusion of observations well on the positive side of the line showed at least that uniformity prevailed on one side of the line. An occasional negative observation there would have reduced the value of my rule. And it must be remembered that all the positive evening observations on the list were first observations of the new moon. The fact that the first observation was not made till the moon was on the positive side of the line is of value. It implies a failure to observe the moon on the previous evening; but I have not used such negative observations except where Schmidt included them in his list of negative observations, because I cannot otherwise be sure that observation was possible, or that a careful search was made. Finally, the inclusion of these easy positive observations is evidence of the relative frequency of the exceptional positive observations on the negative side of the line. Altogether my list contained sixty-six months in which the young moon was looked for, and in sixty-five of those months my formula held good. This suggests that it might be expected to fail once in five years, if observation were possible every night. I also pointed out that the one evening exception belonged to the small group of five observations where a difference of more than 20° in azimuth was combined with an altitude of less than 10° . It does not break the uniformity of the far more numerous evening observations where the difference in azimuth is small, and the reader will observe that among the new moons which I have treated as invisible in my discussion of the date of the Crucifixion, the difference in azimuth nowhere exceeds 6.6° . Nor can I accept Mr. Maunder's view that the negative observations on my list were "nearly all merely chance-misses, where the astronomical conditions rendered the observation

possible." With few exceptions, these observations were made by Julius Schmidt, a very expert naked-eye observer, who made daily observations, when weather permitted, of the first appearance to the naked eye of stars of different magnitudes and of the zodiacal light, and who was able on most clear evenings when a first magnitude star was in the zenith to see it with his naked eye before sunset, after first finding it with a telescope. If Mr. Maunder will turn to Schmidt's paper in *Astronomische Nachrichten*, 1868, Band 71, pp. 201 ff., and to the discussion in Mommsen's *Chronologie*, 1883, pp. 69-80, he will find that in a large proportion of the negative observations, observation was made with a telescope as well as with the naked eye, and in many of these the moon, though invisible to the naked eye, was seen through the telescope. There was no chance here. Schmidt's observations were careful observations, and a moon which eluded Schmidt's vigilant watch would be likely to elude an ancient observer also.

I now come to what appears to me to be Mr. Maunder's most valuable contribution to the subject—his addition of eight evening and one morning observation to those recorded in Mommsen's *Chronologie*.

At the time when I analyzed the observations recorded by Mommsen, I was aware of those contained in the *Journal of the British Astronomical Association*, but I deliberately excluded them from consideration—partly because they contained no records of failure to observe the moon, and partly because they only consisted of observations which the observers regarded as remarkably early. These observations are naturally of interest to anyone interested in the phenomenal rather than in the normal, but it would have been misleading to combine them with the data published by Mommsen, which included negative as well as positive, late as well as early, observations. Still, these picked observations are of interest as indicating the extent to which exceptions to my formula may be expected. Five of the eight additional evening observations are satisfied by the formula. I have recomputed the three exceptions, and my result agrees very closely with Mr. Maunder's.

They are as follows :

No.	Observer.	MAUNDER.			FOTHERINGHAM.		
		Altitude.	Difference in Azimuth.	Distance below Dividing-Line.	Altitude.	Difference in Azimuth.	Distance below Dividing-Line.
		Degrees.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
79	Denning	11·5	1·3	0·5	11·4	1·2	0·6
82	S. J. Johnson	11·2	5·6	0·6	11·4	5·8	0·4
85	Horner	4·3	8·8	7·2	4·3	9·2	7·2

The most striking of these is No. 85, but it must be remembered that this observation was not made without the help of instruments. Mr. Horner first found the moon with opera-glasses, and afterwards

with the naked eye. As I have remarked above, Schmidt was able to observe first magnitude stars with the naked eye before sunset after finding them with a telescope. I do not know of anyone who has observed them before sunset without using a telescope. The modern Mussulman watching for the new moon of Ramadan makes his observation first with a telescope and then with the naked eye; but the ancient had neither telescope nor opera-glasses, and therefore Mr. Horner's observations, though interesting to students of the transparency of the atmosphere, will need corroboration from purely naked-eye observations before it is used for chronological purposes. The other observations establish that the moon may sometimes be seen half a degree below my dividing-line. It would be surprising if this were not the case.

The additional morning observation falls $1^{\circ}8'$ below my line. As Mommsen's list contained only five positive morning observations and only one negative morning observation, and that one doubtful, it is difficult to make much of it. One of his five was $3^{\circ}1'$ below my line. It was not one of Julius Schmidt's observations, but was reported to him by Friedrich Schmidt, doubtless because it was remarkable. The addition of another remarkable morning observation does not even prove that observation is easier in the morning than in the evening, though I am assured that that is the case.

I come now to the application of my astronomical formula to the new moons in the procuratorship of Pontius Pilate. The moons which I treated as invisible all lie at least 1° below my line, and are therefore unaffected by the evidence of Nos. 79 and 82. If any of these was seen, the observation was more remarkable than on any known to have been achieved by the unassisted eyesight. Even if we were to allow a margin of 2° below my line, the only moons which we should have to add to the list of the possibly visible are those of 26 March 8, 27 March 27, 32 March 30, and 34 March 9. But the two latter of these do not enter into the question, because they would place Nisan 14 on a Sunday and a Tuesday respectively. If the moon was visible on 26 March 8, Friday, March 22, in that year might be Nisan 14, and might therefore be made to accord with the Johannine date for the Crucifixion; and if the moon was visible on 27 March 27, Friday, April 11, in that year would be Nisan 15, and would therefore accord with the Synoptic date. The moon of 29 March 4, which Mr. Maunder proposes to regard as possibly visible, lay even nearer to the setting sun than the moon observed by Mr. Horner, though at a slightly higher altitude. If the moon was seen that evening without the aid of opera-glasses, then we must add one more to the list of miracles in that eventful season.

Mr. Maunder argues that the rule by which, if any two witnesses saw the new moon, a new month was to be inaugurated, justifies us in

assuming "that if the conditions approached those under which in modern times the moon has ever been seen, no matter with what difficulty, it would have actually been seen and employed for their calendar by the Jews in the time of our Lord." Now, the Mohammedans have preserved this rule of the two witnesses, yet their astronomers assert that the moon is not visible till 12° distant from the sun. The inference is that the two witnesses were not more successful than Julius Schmidt in observing the young moon, and the formula which satisfies his observations should satisfy theirs. If a naked-eye evening observation should turn up well on the negative side of the line, as a morning observation has done, it must be taken as evidence of an occasional abnormal clearness of the atmosphere, not as evidence that the observers whose observations are on record had frequently missed the moon when she was visible.

The story about Rabbon Gamaliel, which Mr. Maunder cites from the Mishna, does not seem to me to be proof positive that the beginning of the month was fixed two days too early. It is conceivable that this was an occasion of abnormal clearness of the atmosphere. At the beginning of Tishri the altitude of the moon at sunset increases but slowly from day to day, and it might happen that the moon was seen one evening but not the next, although the weather was clear. But that such an observation was quite exceptional is proved by the opposition of Rabbi Yehoshua and Rabbi Dosa, who regarded such a phenomenon as impossible. They, at least, had not heard of such an anticipation of the normal date of the beginning of the month.

J. K. FOTHERINGHAM.

Mr. Maunder's article in the July number of the *CHURCHMAN* is interesting, but really it does not carry us much farther. Let it be remembered that for a century at least chronologers have been asking astronomers for some working rule whereby they may determine the commencement of the lunar month. This perfectly legitimate request has been so consistently ignored by astronomers that chronologers can hardly be blamed if errors creep into their system. Yet attempts *have* been made to satisfy their inquiry; and among such attempts the highest honour is due to Schmidt's series of observations at Athens. The results were communicated to Mommsen, but no practical rule was deduced. But in reality all the observations can be reduced to rule, and a line of limiting visibility can be drawn accordingly. Strictly speaking, of course, it is not a mathematical line, but a thin band—about a degree in width—on the surface of which visibility is doubtful. The band is so thin, however, that no doubt whatever will affect the solution of the problem in the case of any of the critical months connected with the Passion of our Lord. And the line (or band) having been drawn, the remarkable discovery is made that this is no new rule enunciated for the

first time in the twentieth century, but an ancient and forgotten rule familiar to the astronomers of bygone years, in which the lunar calendar was in common use. It is idle for Mr. Maunder to quarrel with such a rule, just as it is idle for him to cite observations dependent on the telescope or an opera-glass, or other artificial aids to sight. Nor even with such aid can he find any instance parallel to that demanded by Colonel Mackinlay and the chronologers who follow Clinton's date. Their date is simply impossible, and must be dropped.

Mr. Maunder is quite right, however, in drawing attention to the abnormal date assigned to the Crucifixion by the same chronologers in placing it before the spring equinox. Once again we may simply say this date is impossible. Such a full moon as fell in March, A.D. 29, would not be that of Nisan, but of Veadar.

D. R. FOTHERINGHAM.



Welsh Disestablishment and Canon Law.

IT is a strong, and on the whole healthy instinct, which impels us to find modern political problems in our studies of past history. But, like many other strong and healthy instincts, it needs to be carefully watched. Mr. Ogle assures us that he had long ago found strong reasons against Maitland's theory, "without any thought that a sudden turn of political controversy might make it expedient to produce them in however imperfect a form."¹ We believe him; his rather lengthy preface is temperate, dignified, and therefore impressive. He there deals directly with the Disestablishment question; we could wish that his whole book had dealt with it equally directly. But the main body of the work purports to be a scientific historical discussion of one of the best known among all Maitland's historical works. In this discussion Mr. Ogle rapidly loses the self-control which had served him so admirably so long as he openly faced the political question alone. The sense that the Downing Professor is often unfairly used as a political stalking-horse has bred a very pardonable irritation in his mind. He is conscious of attacking Maitland, at this particular moment, only because other far less worthy adversaries are sheltered behind that great name. And we seem to trace a gradual discovery, not the less irritating for being subconscious, that, when it comes to serious gnawing, Maitland is an even harder file than he seemed at first sight. In any case, there is a steady rise of temperature, and the hopes raised by Mr. Ogle's preface are succeeded by a growing sense of disappointment, to use no stronger word. So long as we were on frankly political lines, all was well; but now that the time is come for scientific research, we find ourselves wading through a political pamphlet.

¹ "The Canon Law in Medieval England," by Arthur Ogle, M.A., Rector of Otham. John Murray, 1912.