PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A
MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

Two Uses of Ancient Astronomy

R. R. Newton

Phil. Trans. R. Soc. Lond. A 1974 **276**, 99-116
doi: 10.1098/rsta.1974.0012

Phil. Trans. R. Soc. Lond. A. **276**, 99–116 (1974) [99]
Printed in Great Britain

Two uses of ancient astronomy

By R. R. Newton
Applied Physics Laboratory, Johns Hopkins University, Silver Spring, Maryland 20910, *U.S.A.*

Observations from ancient astronomy are useful in studying the non-gravitational accelerations of the Earth and Moon, and recent developments in this study are reviewed. Such a study necessarily involves astronomical chronology and simultaneously shows some limitations in its use. Limitations include lack of veracity in many records, errors in dating events, and uncertainty in calculating the circumstances of ancient eclipses of the Sun. These limitations are studied both quantitatively and by example.

1. Introduction and background

It has been recognized for some time that friction in the tides is gradually slowing down the Earth's rotation about its axis and that it is also gradually decreasing the angular velocity of the Moon in its orbit about the Earth. These effects are often described by saying that the lengths of the day and the month are both gradually increasing. The general way in which tidal friction brings about these effects is described in many places (see Munk & MacDonald 1960 or Jeffreys 1970, for examples).

So far as we know, tidal friction is the only phenomenon that tends to change the length of the month at a rate sufficient to concern us here. However, it is almost certain that other phenomena tend to change the length of the day at a significant rate. Possible phenomena include effects of the Earth's magnetic field and slow changes in the average radius of the Earth because of changes in the average temperature of the Earth's interior.

The terms 'acceleration of the Earth' and 'acceleration of the Moon' are commonly used to denote the rates at which the angular velocity of the Earth's rotation and the orbital velocity of the Moon are changing. Both accelerations have negative values, meaning that both angular velocities are decreasing.

Within the past century, the gravitational theory of the solar system has been brought to a state of high accuracy. This fact allows us to use the observations of ancient astronomy in estimating the accelerations, in the following way: Whenever we find an ancient observation, we start by calculating what the observation should have been on the basis of modern theory. We can then confidently ascribe any discrepancy between the calculation and the observation to the accelerations.

The symbol $\dot{n}_M$ will be used to denote the acceleration of the Moon (with respect to ephemeris time) and the symbol $\dot{\omega}_E$ will be used for the acceleration of the Earth. Values of both accelerations will be given in units of seconds of arc per century per century, written as $''/\text{century}^2$. This is a conventional usage for $\dot{n}_M$. It is not conventional for $\dot{\omega}_E$, but it will be used here in the interest of simplicity.

The main purpose of the work that I have done with ancient astronomy, as well as with medieval astronomy, has been to study the accelerations of the Earth and Moon. However, the work has necessarily brought me into contact with astronomical chronology, and some of the results of that work have significance for astronomical chronology. Thus, in this paper, I shall discuss two and only two uses of ancient astronomy. These uses are in the study of the accelerations and in the study of chronology.

2. The basic idea of astronomical chronology

In many cases, we know the structure of an ancient calendar and thus we know the relative chronology of events that were dated by means of that calendar. Our need is then to be able to relate events in that calendar to those dated in another calendar, and in particular to our calendar. In principle, we can do this if we can establish the date of a single event in both calendars. For the sake of reliability, of course, we should like to have more than one such event.

In using astronomical chronology, we look for an astronomical observation that can be identified uniquely and that is dated in terms of the ancient calendar. We then calculate the date of the observation, using astronomical theory, and thus establish the correspondence between the ancient calendar and ours.

The accuracy with which we can establish the correspondence depends upon the nature of the observation. The position of the Moon among the stars changes by about 13° per day. Thus, if the position of the Moon is given with an accuracy of about 1°, which is easy for an observation made with the naked eye, we can establish the correspondence down to the very day, provided that the necessary data have been preserved. At the other extreme, the same accuracy in the position of the vernal equinox allows us to establish only the century, since the equinox precesses at the rate of about 1.4° per century.

If we are to use an astronomical observation for chronological purposes, then, four conditions must be satisfied:

(1) The record of the observation must be basically truthful.

(2) The observation must have the accuracy that we need for the desired chronological accuracy. The accuracy of calculation from modern theory is usually not a limiting factor.

(3) The observation must be correctly dated in the ancient calendar.

(4) We must be able to identify the astronomical event uniquely. Identification is usually a problem only with eclipses, although one can imagine circumstances in which it is a problem for other kinds of observation.

Discussions of these four conditions will form the subject-matter of the next four sections.

3. The veracity of ancient astronomical records

Many passages in the literature that describe ancient astronomical observations are simply not true. There are several classes of untrue records that will be discussed separately.

(1) *Records that involve a modern, or at least a relatively recent, error.* There are numerous examples of this class, of which only two will be mentioned.

The so-called 'eclipse of Babylon' is an example of this class that has arisen within the twentieth century. This 'eclipse' refers to a cuneiform text that Fotheringham (1920) and others have studied. According to many later writers, Fotheringham concluded that the text describes an eclipse that was total at Babylon. What Fotheringham actually concluded (p. 124) was that 'the phenomenon recorded in the Babylonian chronicle was something other than an eclipse, or, if an eclipse, was total in southern Babylonia and not at Babylon itself . . .'. That is, the one thing that Fotheringham specifically excluded is the thing that he is credited with establishing.

The 'eclipse' that involves the minor Chinese deities Hsi and Ho involves another erroneous reading, in my opinion. Many writers of the past few centuries (see the summary on pp. 62–65, Newton 1970) have tried to identify this with eclipses ranging from −2154 October 12 to −1904 May 12. Aside from the fact that the passage is almost surely a forgery written around +300 or later, it seems to me that the passage does not deal with an eclipse at all. It seems to be concerned with the problem of keeping a lunar calendar in average adjustment with the solar year.

(2) *The 'literary' eclipse.* Mark Twain (Clemens 1889) and Haggard (1886) insert total eclipses of the sun into their action for literary purposes. These 'eclipses' may be taken as archetypes of the 'literary eclipse'. The 'eclipse' described by Anna Comnena (*ca.* 1120, chap. VII. 2) is the earliest example I have found in which a person uses the ability to predict an eclipse in order to confound an uneducated opponent. Plutarch (*ca.* 90) uses an eclipse description for literary purposes, but without using it to confound the ignorant, and he mentions many other 'literary eclipses', such as the 'eclipse of Archilochus', that were already classic in his time.

Probably no one would be more surprised than these writers (with the possible exception of Anna) to find that their literary eclipses were being used as valid astronomical observations by modern scholars, but this has often happened. Sometimes the 'eclipse' is first identified as to date and the result used to date the composition of the literary work. Sometimes the work is dated independently and the result used to 'identify' the eclipse. Analysis of this sort is based upon the tacit assumption that the writer wrote the passage immediately after seeing a total eclipse. This is equivalent to the assumption that the writer had neither imagination nor memory.

Here of course we come to a matter of opinion. I cannot prove that the writer's inspiration was not based upon recently seeing a total eclipse of the Sun. Other students of the subject are entitled to opposite opinions. However, I do not see how these opinions can be given the rank of precise astronomical data. In particular, I do not see how a single opinion of this sort can be given more weight than all the surviving observations made by professional astronomers in all of ancient and medieval times, yet this is what several modern scholars have done. For further discussion of this point, see § 8 below.

A special type of record that we may call 'literary' is a calculated result that forms part of the astronomical or astrological literature. There are many results that were calculated in preparing ephemerides (Neugebauer (1955), for example) or in preparing astrological predictions, for which there are an enormous number of examples. Occasionally we can use calculated results as representative of smoothed observations, if we can be sure when the underlying observations were made. Usually, though, we must avoid anciently calculated results for present purposes, although they may be highly valuable for other purposes.

(3) *The 'assimilated' eclipse.* The human memory is fallible and a large eclipse of the Sun is a dramatic event. It is likely that a person has a tendency to bring two important or dramatic events together in his memory after the passage of time. Thus a writer may unintentionally displace an eclipse to make it coincide with some other event, or vice versa. I use the term 'assimilated eclipse' to designate a record in which an eclipse is displaced in time in order to assimilate it to some other event. The account of the eclipse of 1133 August 2 in the *Anglo-Saxon Chronicle* (*ca.* 1154) is probably an assimilated eclipse. The eclipse is put in 1135, where it is used as a precursor of the death of Henry I, but all details except the year are kept correct. An assimilated eclipse may also be literary.

(4) *The hoax*. It is well known that eclipses, comets, and other astronomical happenings were widely regarded as portents. When we see an eclipse, for example, being used as a portent, we should be suspicious that it is a hoax that has been invented for the occasion. It is possible that the 'eclipse of Stiklestad' (Snorri, *ca.* 1230) is such a hoax, invented as a bit of religious propaganda, although it may be a genuine misunderstanding by Snorri. It seems certain that the eclipse put on the day that Xerxes left Sardis (Herodotus, *ca.* −446) to begin the invasion of Greece is a hoax, at least in the way that Herodotus uses it. There was no eclipse at that time, and events could not have happened as he describes them. It is not clear whether Herodotus was the hoaxer or the hoaxed. Dubs (1938) suggests that the questionable Chinese record that he lists under −183 May 6 was fabricated as an omen for political reasons.

An eclipse that is used magically may none the less be genuine. For example, an eclipse that happened within the year before a person's death is often called an omen of the event. There is a reasonable probability that a solar eclipse will be visible at a given spot within a given year, and hence there are many 'correct' omens. We can only proceed with caution with eclipses that are used magically. If little or no conforming detail is given, and if superlatives are used in the description, we are probably safe in ignoring the record. However, if detail is correctly given, and if there is no exaggeration, we can probably accept the record. For example, the Byzantine historian Gregoras (*ca.* 1359) says that a solar eclipse was an omen of the death of the emperor Andronicus II Paleologus. It occurred as many days before his death as there had been years of his life. Indeed we find an eclipse that was readily visible in Istanbul within a day of the date specified in this way, and I believe that Gregoras has recorded a genuine observation of the eclipse of 1331 November 30.

Unfortunately, we must cope with hoaxes in the writings of professional astronomers. Al-Biruni (1025) quotes the astronomer Abu Sahl al-Kuhi, who worked in Baghdad around the year 990, and whose work appears to be lost. Al-Kuhi claimed that he had made careful measurements to find the obliquity of the ecliptic, and that he found it to be 23° 51′ 20″. Al-Biruni is profoundly suspicious of al-Kuhi's claims, pointing out that his value agrees with Ptolemy's to the second of arc but that it disagrees by about 15′ with other contemporary determinations. Al-Biruni doubts that al-Kuhi made the claimed measurements at all, and he is certainly justified in his suspicions.

It is ironic that al-Kuhi's probable hoax was committed for the purpose of 'confirming' an older probable hoax. Ptolemy (*ca.* 152, chap. I.12) describes two instruments for making the measurements needed to find the obliquity. He then says that he observed the solstices for several years, presumably with one of the instruments described. He found the obliquity to have the value that Eratosthenes had found four centuries before, namely 23° 51′ 20″. The obliquity in Ptolemy's time should have been about 23° 41′. Further, as al-Biruni (1025) points out, the data which Ptolemy quotes would actually lead to 23° 51′ 15″. I think that there is little doubt that Ptolemy's 'measurement' of the obliquity is also a hoax.

I shall not treat the famous question of how Ptolemy obtained his star table. Instead, I shall mention briefly his solar data, which, it seems to me, are unquestionably a hoax.† Ptolemy (*ca.* 152, chap. III.2) gives, to the hour, the times of two autumnal equinoxes, one vernal equinox,

† I published this conclusion in 1969 (Newton 1969). I remarked that J. P. Britton, in an unpublished work, had reached the same conclusion in independent and slightly earlier work. I have since found that Delambre (1819) had already published it. I overlooked this part of Delambre's work earlier because, oddly, it is not in his *Astronomie Ancienne* but rather in his *Astronomie du Moyen Age*. The conclusion may be one that is often discovered and as often forgotten.

PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

and one summer solstice. He says that these times were measured with great care. However, the errors in them are more than a day, whereas Hipparchus three centuries before him had made such measurements with errors of only 2 or 3 h. On the other hand, the data agree exactly, to every numerical digit written down, with what we would calculate from Hipparchus's data and his value for the mean motion of the Sun. It is almost impossible that such errors and such agreement could happen by chance.

In sum, most ancient astronomical data are probably genuine, but a disturbingly large fraction are probably hoaxes. We must beware of accepting data without independent confirmation.

4. The accuracy of ancient measurements of position

There are a few sets of data that allow us to estimate the errors made by ancient astronomers in the measurement of a celestial position.

In an earlier work (Newton 1970, chap. VIII), I analysed about twenty observations of conjunctions of Venus with other bodies, made by Islamic astronomers between 858 and 1003. In five instances, the astronomers said that the latitude difference between Venus and the other body was zero, and I found that the standard error in this statement was 0·061°. I did not calculate the standard deviation of the longitude difference at the time of the stated conjunctions. However, it is easy to determine from the results given that the standard error in the longitude determination was about 0.15°. It was interesting that all errors greater than 0.1°, with one exception, were made in the evening. The accuracy of the morning observations is about the same as that of the latitude differences.

Thus it is reasonable to say that the standard error in an observation of stellar position made with the naked eye is about 0.1° or 6′. It must be recognized that the accuracy depends somewhat upon the circumstances. For example, astronomers frequently measured the time by measuring the altitude of a star and calculating the time from the stellar coordinates. Such a measurement must be made quickly, and it is plausible that the error in altitude would be considerably more than 6′. I have not found any data that allow separating this error from other errors.

Another example concerns measuring the time of an equinox passage of the sun by measuring its declination. I have studied two main bodies of equinox data (Newton 1970, chap. II). Hipparchus measured the times of 20 equinox passages between −161 and −127. The analysis of his data indicates that he had a bias of about 4.5′ in the establishment of his equatorial circle. After the bias is removed, the standard error in his times is 2 h or less. A sample of eight Islamic equinoxes between 830 and 882 shows errors about half this size. Since the declination of the Sun changes by about 1′ per hour near the equinox, the precision of measuring the solar declination was about 2′ or less. Thus solar observations, perhaps because of the large amount of light available, seem to be somewhat more accurate than stellar or planetary observations.

The remarks about solar observations probably apply only to observations made with circles or other devices having no obvious bias other than refraction. There is evidence (Beer *et al.* 1961) that solar measurements made with a gnomon may be subject to considerable bias, because the observer does not judge correctly (or may not know that he should judge) where the centre of the penumbra is.

Thus the standard error in an ancient observation of position should allow establishing chronology to a day if the quantity observed changes as rapidly as the longitude of the Sun, and, of course, if the other requisite conditions are met. However, the standard accuracy is not always present. To illustrate this, let us pretend that we do not know the date of the Council of Nicaea, and let us try to estimate it by astronomical chronology.

It is often said that the Council of Nicaea fixed the rules by which Christians determine the date of Easter. There is no evidence that the Council in fact did so (Jones 1943; Newton 1972*a* chap. II), but it is certain that the observance of Easter was one of the main subjects on the agenda of the Council, and hence it is plausible that the rules were established within a short time after the Council.

One of the factors that enters into determining the date of Easter is the convention that the date of the vernal equinox is to be taken as 21 March in every year and for every meridian. From this, we may conclude that the equinox occurred on 21 March at the time of the Council. However, because of the difference between the true tropical year and the average length of a Julian year, the date of the equinox moves steadily earlier in the Julian calendar. Thus we should be able to date the Council by finding when the equinox came on 21 March in that calendar.

We must choose a place whose local time is to be used in making the necessary calculations. The most plausible choices are Alexandria and Jerusalem. Since local times at these places differ by only about 20 min, it does not matter much which one is used, and I shall choose Alexandria, since it was the leading centre of astronomy in the Roman Empire. I shall take 3 h as the difference between ephemeris time and universal time at the period of the Council.

In a group of four successive Julian years, the equinox comes earliest in the leap year. It comes later by about 6 h in each succeeding year until the intercalary day in the next leap year causes it to move earlier again. If we are to be justified in saying that the equinox comes on a particular day of the year, the equinox must come on that day in at least two successive years out of a set of four.

In the set of four years beginning in 140, the calculated equinox comes on 21 March in the first two and on 22 March in the last two. This is probably the first occasion on which the equinox came on 21 March in two successive years, and hence about 140 is the earliest possible time for the Council. In the set of years beginning in 292, the equinox comes on 20 March in the first two and on 21 March in the last two. This is probably the last time when two equinoxes came on 21 March. Hence we have determined that the Council of Nicaea occurred between about 140 and 292.

This conclusion is wrong. The Council of Nicaea was in 325. The explanation is probably that the astronomers who fixed the rules for Easter used poor data for the equinox. Elsewhere (Newton 1972*a*, § II.3), I have speculated that the error, which still survives in the ecclesiastical calendar, was a consequence of using the fudged solar data given by Ptolemy (*ca.* 152).

5. Dating errors

In a recent study (Newton 1972*a*), I analysed 629 records of solar eclipses found in medieval European and Byzantine sources. Table 1 is a summary of the errors in the years given for these eclipses, after we make due allowance for differences in conventions about when the year began. There are also many errors in the month and in the day of the month, but I have not analysed them. There are several notable features about the table.

First, the probability that the year is wrong is almost exactly one in four. The probability shows some tendency to improve with the later records, but I have not attempted to study this matter carefully.

Table 1. A summary of dating errors in medieval records of solar eclipses

From Newton 1972a. Reproduced by permission of the Johns Hopkins Press.

error, years	no. of records with this error	error, years	no. of records with this error
0	472	9	2
1	88	10	1
2	21	12	1
3	16	16	1
4	6	33	1
5	5	99	1
6	3	533	1
8	1	550	1
unidentifiable	8		

Secondly, in slightly more than one record in a hundred, the data are so badly garbled that we cannot identify the eclipse that is meant. These records pose a serious hazard. It often happens that we know that the eclipse cannot be identified because we know the chronological system used. If we did not know the system, we might think that we could 'identify' the eclipse, but we would be wrong.

Thirdly, there are the errors of 533 and 550 years. These errors have a specifically Christian origin and might not arise in records from other cultures. They result in part from a cycle of 532 years that occurs in the medieval ecclesiastical calendar. However, other cultures may have analogous cycles. For example, there are circumstances in which an Egyptian record might be displaced by one 'Sothic' cycle of 1460 years.

Table 2. Dating errors in records of solar eclipses in the annals of the former Han dynasty

errors, years	number of records with this error
0	41
0.5	5
1	6
2	1
3	4
unidentifiable	2

Chinese records have a pattern similar to that of the European records. Dubs (1938) has analysed 59 records of solar eclipses found in the annals of the former Han dynasty (approximately −200 to +20). Table 2 summarizes the dating errors in these records as Dubs gives them, rounded to the nearest year or half-year, as the case may be. In 13 cases out of 59, not quite 1 in 4, the rounded error is 1 year or more. In two cases out of 59, the data are so badly garbled that Dubs did not venture an identification.

There seems to be a difference between the Chinese and European records in that table 2 shows no errors greater than 3 years, except for the unidentifiable cases. However, this is probably a consequence of Dubs's conventions rather than a real difference. In all but the two unidentifiable cases, Dubs assigned a date for purposes of discussion, and the date he assigned was that of the eclipse nearest to the date of the record that was visible at all in any part of

China. The differences used in preparing table 2 are those between the recorded date and the nearest eclipse, which was not necessarily the eclipse that gave rise to the record.

We should like to assign a standard deviation to the dating error in table 1, but we are hampered in doing so by the unidentified eclipses. If we ignore them, the standard deviation is 31.5 years. This value is dominated by the two errors of more than 500 years. If we ignore these two errors, but assign a nominal value of 20 years to the unidentifiable cases, the standard deviation is 3.0 years. I suspect that the latter value comes closer to representing the typical situation.

There is no apparent reason why the errors in dating eclipses should be different from the errors in dating other events.

6. The identification of solar eclipses

When there is a considerable amount of detail in a record of a solar eclipse, and particularly when the date is given in a chronological system that we know, there is usually no difficulty in identifying the eclipse. Difficulties begin to arise when we can date the record only to a general historical period. When this happens, we begin by calculating the local circumstances, at the point of observation, of all eclipses within the allowed historical period.

If we are lucky, there is only one possible eclipse within the allowed period. We then have a tentative identification, but we should avoid accepting it as certain if there is no corroborative detail. Our main trouble arises when there is more than one possibility within the allowed period.

When this happens, many people have had recourse to considerations about the magnitude. There is a strong tendency to assume that a recorded eclipse was total. For example, Fotheringham (1920) says with regard to the 'eponym canon' eclipse from Assyria: 'As the eclipse is the only eclipse mentioned in this Chronicle, which covers an interval of 155 years, there can be no reasonable doubt that it had been reported as a total eclipse'. Unfortunately, an inspection of annals and chronicles does not support Fotheringham's contention. There are too many counter-examples. To give but one which is familiar to a British audience, the *Anglo-Saxon Chronicle* (*ca.* 1154) records only one solar eclipse between the years 733 and 878. That is the eclipse of 809 July 16, which probably did not attain a magnitude of more than about 3/4 anywhere in England. (Magnitude is used here to mean the fraction of the solar diameter that is covered by the Moon at the centre of the eclipse.)

The usual procedure when there is more than one possible eclipse is to play what I have called the 'identification game' (Newton 1969, 1970). In this game, the player starts by assuming that he knows the accelerations of the Earth and Moon within small uncertainties. Using his assumed accelerations, he calculates the magnitude of each possible eclipse and 'identifies' the eclipse as the one that yields the largest calculated magnitude. Since any eclipse can be made total at a given point by some values of the accelerations, the procedure of the identification game is equivalent to choosing the accelerations that are closest to the ones assumed. The player then completes the game by using the choices just made in making new estimates of the accelerations.

This is, of course, reasoning in a circle. One way to see that the method has no validity is to note that it works just as well if the 'historical periods' and 'places of observation' are chosen with the aid of a table of random numbers.

Quite apart from this error in logic, the identification game is not valid because it is based upon choosing the maximum calculated magnitude. This choice seems to be based upon the assumption that the magnitude must be large if an eclipse is to be recorded. It is instructive to examine the magnitudes in the large sample of European and Byzantine records that was mentioned above. The sample used in studying dating errors contained 629 records. In studying the magnitudes, we can use only those records which, with high probability, are independent and which can be assigned to a particular point of observation rather than to a general region. There are 202 records in this sample.

Table 3. Calculated magnitudes of 202 solar eclipses recorded in medieval European and Byzantine sources

magnitude range	no. of records lying in this range: if $\dot{\omega}_E = -1068''$/century^2	if $\dot{\omega}_E = -1424''$/century^2
$\geqslant$ 1.00	23	26
0.98–1.00	16	22
0.96–0.98	24	13
0.94–0.96	17	11
0.92–0.94	12	16
0.90–0.92	21	20
0.88–0.90	13	11
0.86–0.88	18	15
0.84–0.86	4	13
0.82–0.84	8	13
0.80–0.82	12	3
0.78–0.80	6	3
0.76–0.78	2	7
0.74–0.76	4	2
0.72–0.74	3	5
0.70–0.72	4	4
< 0.70	15	18
smallest value	0.30	0.31

Note: The acceleration of the Moon was taken to be $-42.44''$/century^2.

The centre time for this sample is about 1000. For this epoch I have estimated (Newton 1970, p. 272):

$$\dot{n}_M = 42.3 \pm 6.1''/\text{century}^2, \quad \dot{\omega}_E = -1068 \pm 170''/\text{century}^2.$$

The centre column in table 3 gives the number of eclipses whose calculated magnitudes, when calculated with these accelerations, lie within the indicated ranges. (I changed $\dot{n}_M$ slightly for convenience in preparing table 3.) The range of magnitude for those which were total is roughly equivalent to a range of 0.02, the range used in counting the eclipses that were less than total. Thus we see that the number of eclipses in each range is roughly constant from totality down to a magnitude of about 0.85 or perhaps less. Only about one eclipse out of nine recorded was total, and the smallest magnitude in the sample was about 0.30.

The last column in table 3 is calculated with $\dot{\omega}_E$ equal to 4/3 of the best estimate. This change in $\dot{\omega}_E$ changes the values for individual eclipses considerably but it makes little change in the general nature of the distribution. Hence the distribution calculated with $\dot{\omega}_E = -1068$ should be close to correct.

Thus, if the record of an eclipse gives no specific statement about the magnitude of an eclipse, we have no warrant to assume that the eclipse was total or nearly so. We are entitled to a reasonable presumption, say about 11 chances out of 12, that the magnitude is greater than 0.7. In playing the 'identification game', when there is no explicit statement about the magnitude, we must keep all calculated magnitudes greater than, say, 0.7 if we wish to have reasonable confidence in the 'identification'. Yet Ginzel (1899) and others have rejected calculated magnitudes as great as 0.96, even in cases where we do not know the place of observation.†

Even if an ancient record should state explicitly that an eclipse was total, we cannot take this statement at face value. The reason for this is connected with the existence of annular eclipses. If an eclipse occurs when the apparent diameter of the Moon is less than the apparent diameter of the Sun, the lunar disk is unable to cover the Sun completely even for an observer on the shadow axis, and the eclipse is called annular. Out of all eclipses for which the shadow axis strikes the Earth, more than half (about 56 %) are annular.

The Cairo chronicler Ibn Iyas (*ca.* 1522) furnishes an example of the erroneous reporting of an eclipse as total. He clearly states (at least in the cited translation) that the eclipse of 1473 April 27 was total in Cairo and that complete darkness lasted for an appreciable time. However, calculation shows that the eclipse was certainly annular and that it was not total anywhere.

Ibn Iyas was probably not an expert astronomer. We might asign this as the reason for his failure to discriminate between annular and total eclipses except for the fact that many experts also failed to make the distinction. For example, in the eclipse tables of Ptolemy (*ca.* 152), the minimum apparent diameter used for the Moon is so large that annular eclipses cannot occur. Thus it seems almost certain that annular eclipses were unknown to Ptolemy and to earlier astronomers. That is, ancient astronomers probably did not distinguish between total and annular eclipses, and they may have thought that the corona and the annulus that remains visible in an annular eclipse were the same thing, merely varying in brightness from one eclipse to another. In fact, I have not yet found an ancient or medieval record that unmistakably refers to the corona (Newton 1972*a*, § XVII.6).

In summary, I have not found a single instance in which the identification game yields a valid identification when there are two or more visible eclipses within the historical period allowed by a record. Unfortunately, Fotheringham, Ginzel, and others have based their conclusions heavily upon the use of the identification game.

We do not have the evidence needed in order to be sure why people have used the identification game. I suspect that acceptance of it rests upon two main factors: (1) The person who did the 'identifying' was usually not the person who used the 'identification' in estimating the accelerations, and thus the circular nature of the game was not as patent as I have made it here. (2) The people who did the identifying probably over-estimated the accuracy with which we can calculate the magnitude of an ancient eclipse. This latter topic will now be discussed.

7. The accuracy of calculating ancient eclipses

For reasons that I have described elsewhere (Newton 1972*a*, chap. XVIII), the magnitude of a solar eclipse depends less upon the individual accelerations $\dot{n}_M$ and $\dot{\omega}_E$ than upon the parameter D'' to be defined in § 9 below. Estimates of D'' for various epochs within the historical period are shown in figure 1. Here we are interested in the accuracy with which we know D''. From

† Ginzel did this with the so-called 'eclipse of Archilochus', which is a passage of poetry. I see no reason to assume that this is more than a literary eclipse. Even if we assume that the passage is a genuine record, we do not know the place of observation. See table 4 and the accompanying discussion below.

figure 1, it seems reasonable to conclude that we know D'' with an accuracy of about 2″/century² for times since about −700.

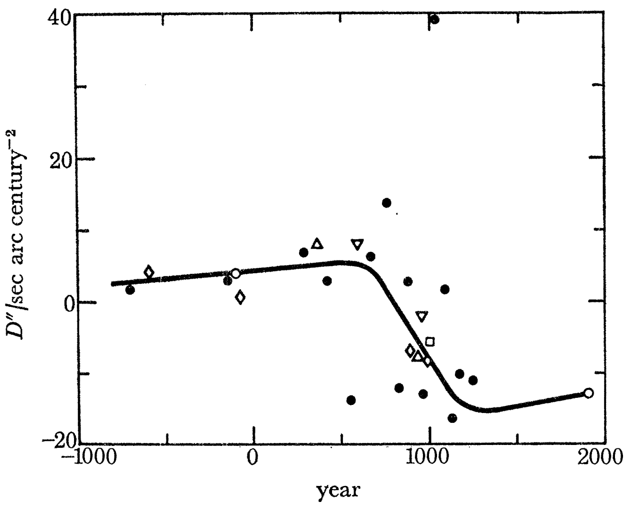

FIGURE 1. The acceleration parameter D'' as a function of time from about −700 to the present. The solid line has been drawn by eye to fit the plotted points. The points are based upon various kinds of observation, as follows: ●, the occurrence of solar eclipses; ◇, measured times of lunar eclipses; ○, measured times of lunar conjunctions or occultations; △, measured times of solar eclipses; ▽, measured magnitudes of solar eclipses; and □, the position of the Moon derived from an astronomical table.

In calculating the circumstances of a solar eclipse, it is convenient to assign values to the individual accelerations. Since the circumstances depend mostly upon D'' rather than upon the individual values, we must use individual values that are consistent with the estimates of D''. For convenience, we may pick any reasonable value of $\dot{n}_M$, say, but we must then calculate the value of $\dot{\omega}_E$ from its definition in §9.

As an example in calculating ancient eclipses, I shall use the various possibilities for the 'eclipse of Archilochus' that has already been mentioned. (See Fotheringham (1920), or Newton (1970), pp. 91–93.) The approximate historical period for this 'eclipse' is the first half of the −7th century. From figure 1, we see that an appropriate estimate of D'' is $2 \pm 2''$/century². For $\dot{n}_M$, I shall use −41.6 (Newton 1970, p. 272). Thus we must use

$$\dot{\omega}_E = -1288 \pm 59.$$

For convenience, I take the uncertainty in $\dot{\omega}_E$ to be 64, or 5%.

The calculations are summarized in table 4. The first column lists seven possible dates that have been proposed for the 'eclipse'. Most discussions take Paros and Thasos as equally likely possibilities for the 'place of observation'. Thus the next two columns give the magnitudes calculated for these places, using $\dot{\omega}_E = -1288$. A magnitude greater than 1 means that the eclipse was total and remained so for a measurable length of time. The last column gives the magnitude calculated for Thasos when we change $\dot{\omega}_E$ by 1 part in 20 to −1224.

Table 4 shows explicitly the fallacy involved in the identification game. Suppose we had not previously used this 'eclipse' in estimating the accelerations (and indeed I have not and shall not use it), but suppose that we want to use it. For purpose of illustration, let us further suppose that we know $\dot{n}_M$ and that we are concerned only with estimating $\dot{\omega}_E$. If we take −1288 as the best estimate of $\dot{\omega}_E$ on the basis of other evidence, we conclude from table 4 that the eclipse was

Table 4. Calculated magnitudes of various possibilities for the 'eclipse of Archilochus'

date		calculated magnitude at Paros ($\dot{\omega}_E = -1288$)	Thasos ($\dot{\omega}_E = -1288$)	Thasos ($\dot{\omega}_E = -1224$)
−710	Mar. 14	0.88	0.93	0.84
−688	Jan. 11	0.92	0.96	0.94
−661	Jan. 12	0.83	0.75	0.81
−660	June 27	0.95	0.94	0.89
−656	Apr. 15	0.99	0.94	1.02
−647	Apr. 6	0.96	1.01	0.97
−634	Feb. 12	0.91	0.88	0.92

Note: $\dot{n}_M$ was taken to be −41.6.

−647 April 6 seen at Thasos. When we in turn use the 'identified eclipse' in estimating $\dot{\omega}_E$, we necessarily find a value near −1288. However, we are just as well entitled to use −1224 as our initial estimate of $\dot{\omega}_E$. When we do so, we conclude that the eclipse was −656 April 15 seen at Thasos. When we then use this 'identification' in estimating $\dot{\omega}_E$, we find a value near −1224.

In other words, 'eclipses' identified by means of the identification game always tend to confirm the values of the accelerations used in making the identifications, and such 'identifications' contain no new information.

If we consider tables 3 and 4 together, I do not see how one can reject any date listed in table 4 as a possibility for the 'eclipse of Archilochus'.

The changes in magnitude produced by the change in $\dot{\omega}_E$ range from 0.02 to 0.09. The reason, aside from rounding error, is that the change depends upon the geometry of the eclipse path and the relation of a point to that path. The root-mean-square change is about 0.06 and we are justified in using this value as the typical uncertainty in calculating the magnitude of an eclipse near −700.

The base values of the astronomical parameters used in calculating the magnitude (other than the accelerations) derive from data obtained near 1900. Thus, for a given uncertainty in D'', the uncertainty in the magnitude varies with the square of the time measured from 1900. In addition, figure 1 shows that D'' is far from being constant in time. Therefore, as we extrapolate backward in time from about −700, we must also increase our estimate of the uncertainty in D''.

Table 5. The uncertainty in calculating the magnitude of an ancient eclipse

year	uncertainty in magnitude
0	0.03
−700	0.06
−1300	0.18
−2000	0.54

It seems conservative to double the uncertainty in D'' if we go back six centuries before −700, and it seems conservative to double it again if we go back to, say, −2000. For still earlier dates, it seems pointless to speculate on the basis of present information. The resulting uncertainty in the magnitude, as a function of time before the beginning of the common era, is shown in table 5. The values in this table differ considerably from the conclusions of Stephenson (1970), who says that we can calculate the magnitude for eclipses near −1400 with an uncertainty that does not exceed 0.01.

8. The accuracy of ancient measurements of time

We are now almost ready to turn to the consideration of the accelerations. Before we do so, we must decide upon the types of data that we will use in estimating them.

Many workers have used only observations that a particular solar eclipse was seen at a particular place, and they have usually tried to confine themselves to eclipses that, in their opinions, were total or nearly so. (In doing this, they may have overestimated the degree of totality, as we discussed in §6 above.) Two beliefs seem to underlie this course of action: (1) An observation of a total eclipse is considered to be a highly precise observation. (2) An ancient observation that involves the measurement of time is considered to have very low precision. These beliefs will now be discussed.

The first belief may arise from the fact that the duration of totality at a particular place is at most a few minutes. However, this would matter only if the time of the eclipse were recorded and used, and the fact would be useful only if the time measurement had the accuracy that is denied it by the second belief. Therefore, the belief requires that we use the eclipse without reference to the time of occurrence, even when the time is available.

A particular eclipse observation gives us a functional relation between the acceleration of the Earth and that of the Moon. In order to determine this function, we can start by assuming a value for the lunar acceleration and calculating the path of totality on a sphere that rotates uniformly with the present angular velocity of the Earth. We usually find, for an ancient eclipse, that the place of observation does not lie within the path limits on this sphere. In order to reconcile the calculation with the observation, we calculate the extra rotation needed to bring the point within the path, and we impute the extra rotation to the acceleration of the Earth. The precision of the observation, when used in this way, depends upon the angle through which the Earth can be turned and still have the point lie within the path. That is, it depends upon the width of the path in longitude, measured along the parallel of latitude that passes through the place of observation.

The width of an eclipse path, measured perpendicular to its length, is rather narrow. However, most paths extend nearly in an east–west direction and their width in longitude may be considerable. I measured this width for about forty eclipse paths used in an earlier work (Newton 1970) and found that its root-mean-square value is about 8°. This is equivalent to 32 min of time. If we estimate the acceleration by using the centre line of the path, the maximum error is equivalent to about 16 min, and the standard deviation of the error is almost exactly equivalent to 10 min. This, and not the half-duration of totality, should be taken as the measure of precision of an observation that an eclipse was total at a known point.

In order to find the functional relation that the observation imposes upon the accelerations, we repeat the calculations with other values of the lunar acceleration. It has always been sufficient to use two such values.

The second belief is probably connected with the general idea that ancient and medieval time keeping was poor in accuracy. This may be right so far as the general public was concerned, but astronomers are a different matter. Ancient astronomers had a time keeper whose accuracy was not exceeded until this century. This time keeper is the celestial sphere. The important question is: Did ancient and medieval astronomers use it?

We know that Islamic astronomers used celestial observations in order to measure time because, in a considerable fraction of the Islamic records that have been analysed (Newcomb 1875; Newton 1970), the data needed to calculate the time have been preserved along with the rest of the record. We have somewhat less information about Hellenistic time measurements. Ammonius and his brother Heliodorus say explicitly that they measured the time astronomically in an observation† made in 503. Ptolemy found the time astronomically in an observation made in 138, and in fact he recorded the data used. We do not know whether this was the common Hellenistic practice or not. So far as I am aware, we have no clues as to how the Babylonian astronomers measured time, except for the fact that their time units correspond to rotation of the heavens by 1° and by 30° (one sign of the Zodiac). This suggests but does not prove that they measured time astronomically.

Table 6. Errors in ancient and medieval measurements of time

description of the measurements	historical period years	standard deviation of the measurement min
Babylonian measurements of lunar eclipse times	−720 to −490	31
Hellenistic measurements of lunar and solar eclipse times	−200 to +364	26
Hellenistic measurements of lunar occultations	−294 to +98	32
Islamic measurements of lunar eclipse times	854 to 1001	15
Islamic measurements of solar eclipse times	829 to 1004	9

In any event, we have several samples from which we can estimate the accuracy of old time measurements. These samples are summarized in table 6. The standard deviation for a sample in table 6 is not calculated from the mean for that sample. Instead, all standard deviations are calculated using the variation of D'' shown by the solid line in figure 1. We see that the timing accuracy of the Islamic astronomers is about 10 min. This is the same as the time-equivalent precision of a solar eclipse, and hence an Islamic time observation is entitled to the same statistical weight as an observation of a total solar eclipse at a known point. The timing accuracy of the Babylonian and Hellenistic astronomers is about 30 min and hence one of their observations should receive about one-ninth of the weight given to a total solar eclipse, other things being equal. This certainly does not say that we are entitled to ignore their time measurements. In fact, we should use them even if they had much lower precision yet because, as everyone knows who has tried to measure a physical quantity, it is important to measure it in a variety of ways in order to lessen the effects of bias that are inherent in almost every method of measurement.

† This observation does not appear in any formal work. Apparently Ammonius and Heliodorus noted some observations in their copy of Ptolemy's *Almagest*. Their copy was then used as the base for further copies, into which their notes were also copied. See the introduction to the cited edition of Ptolemy (*ca.* 152).

There is an element of irony in the belief that an observation of a total solar eclipse is more precise than other types of observation. The irony arises from the fact that the belief, though often advanced with vigour, is irrelevant. It would be relevant only if early records of total solar eclipses actually existed. There is a record (Newton 1972*a*, p. 399) from which we may conclude, with reasonable confidence, that the eclipse of 840 May 5 was total at the German town of Xanten. This is the earliest record I have found from which we may reasonably infer that an identifiable eclipse was probably total (or else annular and central) at a known place. By 840, contemporaneous Islamic astronomers were making time measurements of the same precision as an observation of a total eclipse, as we see from table 6. Thus, at every stage of history, measurements of time are at least comparable in precision to the observations of solar eclipses that are known to us from that stage.

This statement may surprise the reader, because the literature is full of claims about records of identifiable total eclipses seen at known places. I have investigated every such claim that I have found. In every case, the claim rests upon assumptions that are easily disproved by counter-example; an example of such an assumption is contained in the statement that Fotheringham (1920) made about the 'eponym canon' eclipse, which I quoted above. Even the Xanten record of the eclipse of 840 May 5 is not entirely satisfactory.

Of course, the eclipses in some earlier records may have been total at a known point, and a person may assume as a matter of opinion that they were. However, it seems to me that he is not justified in elevating the opinion into a heavily weighted scientific datum. In particular, he is not justified in doing so when the report is a 'literary' or 'magical' eclipse, as many of the early 'total eclipses' are. Certainly table 6 provides no justification for giving more weight to a single literary eclipse that has been identified by the identification game than is given to all the known measurements of time made by ancient and medieval astronomers.

Time measurements, as well as occurrences of solar eclipses, should be used in the estimation of the accelerations.

9. The acceleration parameter D''

The acceleration $\dot{\omega}_E$ is subject to considerable variation on time scales from a few months to a few centuries, and $\dot{n}_M$ is subject to variation on at least the scale of a century. As a result, it is necessary to distinguish carefully at least three types of averaging or smoothing of the accelerations. The nature of the variations, and suggested definitions of useful kinds of averages, are given elsewhere (Newton 1972*b*). Here I shall present only the kind of average that is of most interest in the study of ancient civilizations. This is the kind that has been called the 'epochal average' and that has been denoted by putting angle brackets around the symbol for the acceleration. Here, for simplicity, the term 'epochal average' and the brackets are omitted.

The linear combination
$$D'' = \dot{n}_M - 0.033862\dot{\omega}_E$$
means the second derivative of the lunar elongation D, taken with respect to solar time rather than to ephemeris time. D'' is well determined by a large body of data with dates ranging from about -700 to the present. We know considerably less about any acceleration parameter independent of D'' than we do about D'' itself. In the interest of brevity, I shall discuss only D'' in this section.

Twelve estimates of D'' based upon about 370 observations of the occurrence of solar eclipses appear in Table XVIII.11 of Newton (1972*a*). Each estimate is based upon eclipses recorded within a particular half-century or century ranging from the fifth through the thirteenth century.

Thirteen more estimates of D'' are given in Table XIV.4 of Newton (1970). Each of these estimates is based upon a particular kind of observation made within a fairly short time interval and within a limited geographical region. The estimates are divided with regard to the kind of observation as follows: 25 observations of the occurrence of solar eclipses divided into three sets, 40 measurements of the times of lunar eclipses divided into four sets, 21 measurements of the times of solar eclipses divided into two sets, 10 measurements of the magnitudes of solar eclipses divided into two sets, and one set containing eight observations of the times of lunar conjunctions or occultations. In addition, there is a value deduced from the tabular mean longitude of the Moon at the epoch 1000 November 30, mean solar noon at Cairo; we know rather well the period of the observations upon which this value (from the Hakémite tables) was based. There is a 14th value of D'' for the epoch 1050 that is based upon the occurrence of solar eclipses. Most of the observations that went into this value were later used in a more detailed study (Newton 1972*b*). Hence this value should not be used as an independent one.

The reader should notice that the quantity tabulated in Newton (1970) is not D'' but rather the quantity $-D''/1.6073$.

Finally, Martin (1969) has analysed about 2000 telescopic observations of lunar occultations made between 1627 and 1860. This analysis allows us to estimate D'' at an epoch near the present.

These 26 estimates of D'' are plotted against time in figure 1, which identifies each value according to the kind of measurement upon which it is based. We see that all kinds of measurement agree as well as could be expected, and that the points based upon the occurrence of solar eclipses have more scatter than the other points. The agreement is particularly interesting since the occurrences of solar eclipses are taken entirely from records made by people who were not professional astronomers, while the other values are based upon measurements of time or magnitude made by professionals.

The solid line shown in figure 1 represents about the best estimate of the time history of D'' that we can make in the present state of knowledge. Since we have neither a theoretical model nor a phenomenological basis for making a formal statistical study of the variation of D'', I have merely drawn the line by eye. It is plausible that the line gives D'' as a function of time since -700 with an uncertainty of about $2''/\text{century}^2$, and I used this uncertainty in the considerations of §7 above. However, we have no estimates for epochs between 1300 and the present, and the error for that period may be greater than $2''/\text{century}^2$.

The most striking feature of figure 1 is the rapid decline in D'' from about 700 to about 1300. When we remember that the values plotted in figure 1 represent the average between any epoch and 1900, this decline means (Newton 1972*b*) that there was a 'square wave' in the osculating value of D'', and that the osculating D'' during the period 700–1300 had a value around $40''/\text{century}^2$ or more. Such changes in D'', and such values, are incapable of explanation by present geophysical theories.

The small value of D'' during the period of classical antiquity (before about 500) should also be noted. From -700 to $+500$, the mean D'' was probably smaller in magnitude than it has been at any time during the past 1000 years. This fact may account for the relative success of nineteenth-century calculations of ancient eclipses and lunar phenomena, which were made in almost complete ignorance of the accelerations.

10. Summary

Ancient and medieval astronomical data allow us to form 25 independent estimates of the important acceleration parameter D'', at various epochs from about -700 to $+1300$. These estimates, combined with modern data, show that D'' has had surprisingly large values and that it has undergone large and sudden changes within the past 2000 years. It even changed sign about the year 800. The uncertainty in the value of D'' at any epoch from -700 to 1300 is about $2''/\text{century}^2$.

We can also use ancient astronomical data for chronological purposes, but with some limitations. An uncomfortably large number of ancient records are either untrue or are in error by amounts larger than those expected from the technical ability of the times. Further, the recorded dates are often in error by serious amounts, even in terms of the calendrical system used by the observer. In a sample of nearly 700 records of solar eclipses, the year is wrong in about one record out of four. The errors range up to 550 years. In addition, the dating is so badly garbled in at least ten cases that we cannot tell which eclipse is meant. These records are a particular hazard. We happen to know that they are unidentifiable only because we know the chronological system used in dating them. If we did not know the system, we would not necessarily know that the data have been garbled and we could easily be led to a false identification.

Solar eclipses can be useful under restricted circumstances. As an approximate rule for the usefulness of eclipses, we can use them only if we already know the date within about a decade; occasionally we can tolerate a larger uncertainty. If the *a priori* uncertainty in date is appreciably larger, we frequently find that more than one eclipse meets the conditions of the record. Multiple possibilities in identification have often been resolved by choosing the largest eclipse, but this procedure is usually unjustified, for two reasons. First, we cannot calculate the magnitude of an ancient eclipse with the necessary accuracy. The error in the calculated magnitude of an eclipse near -1400, for example, can be 0.20 or more, even when we use all available information about the accelerations of the Earth and Moon. Secondly, we cannot usually assume that the eclipse was large. Only about one recorded eclipse out of nine was total. The probability that an eclipse was recorded is reasonably independent of the magnitude for all magnitudes greater than about 0.8 or 0.85. Even if a record says that an eclipse was total, we cannot assume true totality. Apparently astronomers did not learn to distinguish annular eclipses from total eclipses until about the year 1000.

I wish to thank R. E. Jenkins of the Applied Physics Laboratory, Johns Hopkins University, for many helpful discussions and for a critical review of this paper. I also wish to acknowledge the support of this work by the Department of the Navy, through its contract N00017-62-C-0604 with the Johns Hopkins University.

References (Newton)

al-Biruni, Abu al-Raihan Muhammad bin Ahmad 1025 *Kitab tahdid nihayat al-amakin litashih masafat al-masakin*; there is a translation into English, using the title *The determination of the coordinates of positions for the correction of distances between cities*, by Jamil Ali, American University of Beirut, Beirut, Lebanon, 1967.

Anglo-Saxon Chronicle *ca.* 1154 There is an edition of the six main Anglo-Saxon texts by Benjamin Thorpe in *Rerum Britannicarum medii aevi scriptores*, 1861, no. 23, v. 1, London: Longman, Green, Longman, and Roberts.

Anna Comnena *ca.* 1120 *Syntagma rerum ab Imperatore Alexio Comneno gestarum*; there is an edition by L. Schopen, in 2 volumes, in *Corpus scriptorum historiae Byzantinae*, 1839, (ed. B. G. Niebuhr), Bonn: Weber's.
Beer, A., Ho Ping-Yu, Lu Gwei-Djen, Needham, J., Pulleyblank, E. G. & Thompson, G. I. 1961 *Vistas in astronomy* **4**, 3–28. Oxford: Pergamon Press.
Clemens, S. L. 1889 *A Connecticut Yankee in King Arthur's Court*, chap. 6. New York: Harper and Bros.
Delambre, M. 1819 *Histoire de l'astronomie du moyen age*. Paris, Chez Mme. Ve. Courcier.
Dubs, H. H. 1938 *Osiris* **5**, 499–522.
Fotheringham, J. K. 1920 *Mon. Not. r. astr. Soc.* **81**, 104–126.
Ginzel, F. K. 1899 *Spezieller Kanon der Sonnen- und Mondfinsternisse*. Berlin: Mayer and Muller.
Gregoras, N. *ca.* 1359 *Romaikes istorias*; there is an edition by L. Schopen, in 3 vol., in *Corpus scriptorum historiae Byzantinae*, 1829 (ed. B. G. Niebuhr). Bonn: Weber's.
Haggard, Sir H. Rider 1886 *King Solomon's mines* chap. 11. New York: Harper and Bros.
Herodotus *ca.* −446 *History*. There is a translation by George Rawlinson, first published in 1858, reprinted by Tudor Publishing Co. New York, 1947.
Ibn Iyas *ca.* 1522 *Chronique*. There is a translation into French by Gaston Wiet, in 4 vol., under the titles *Histoire des Mamlouks Circassiens* (2 vol.) Imprimerie de l'Institut Français d'Archéologie Orientale, Cairo, 1945, and *Journal d'un bourgeois du Caire* (2 vol.), École Pratique des Hautes Études, Paris, 1960.
Jeffreys, Sir Harold 1970 *The Earth*, 5th ed., Cambridge University Press.
Jones, C. W. 1943 *Bedae opera de temporibus*, Menasha, Wisconsin: George Banta Publishing Co.
Martin, C. F. 1969 A study of the rate of rotation of the Earth from occultations of stars by the Moon, 1627–1860, a dissertation presented to Yale University; intended for publication in *Astronomical Papers Prepared for the Use of the American Ephemeris and Nautical Almanac.*
Munk, W. H. & MacDonald, G. J. F. 1960 *The rotation of the Earth*. Cambridge University Press.
Neugebauer, O. 1955 *Astronomical cuneiform texts*, in 3 vol. London: Lund Humphries.
Newcomb, S. 1875 Researches on the motion of the Moon, in *Washington Observations*, U.S. Naval Observatory, pp. 1–280.
Newton, R. R. 1969 *Science, N.Y.* **166**, 825–831.
Newton, R. R. 1970 *Ancient astronomical observations and the accelerations of the Earth and Moon*. Baltimore, Md: Johns Hopkins Press.
Newton, R. R. 1972*a* *Medieval chronicles and the rotation of the Earth*. Baltimore, Md: Johns Hopkins Press.
Newton, R. R. 1972*b* Astronomical evidence concerning non-gravitational forces in the Earth-Moon system, *Astrophys. Space Sci.* **16**, 179–200.
Plutarch *ca.* 90 *De facie quae in orbe lunae apparet*; there is an edition by H. Cherniss in *Plutarch's Moralia*, v. 12. Cambridge, Mass.: Harvard Press, 1957.
Ptolemy, C. *ca.* 152 *'E mathematike syntaxis*; there is an edition by J. L. Heiberg in *C. Ptolemaei opera quae exstant omnia*, B. G. Teubner, Leipzig, 1898. There is a translation into German by K. Manitius, Leipzig: B. G. Teubner), 1913.
Snorri (Sturluson) *ca.* 1230 *Noregs konunga sogur*; there is an English translation by L. M. Hollander, University of Texas Press, Austin, Texas, 1964.
Stephenson, F. R. 1970 *Nature, Lond.* **228**, 651–652.