CONTRIBUTIONS TO AMERICAN ARCHÆOLOGY, NO. 14

MAYA CHRONOLOGY: THE CORRELATION QUESTION

By J. Eric Thompson

With appendices

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II. MAYA PLANETARY OBSERVATION. By Lawrence Roys
III. REMARKS ON THE CORRELATION QUESTION. By R. C. E. Long
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[Issued October 1935]

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PREFACE

This paper was written in abbreviated form in the spring of 1933. On its completion I asked Mr. R. C. E. Long to add his ideas on the subject. This he kindly consented to do, and his comments are given in Appendix III.

During the summer of 1934 the paper was enlarged and virtually re-written. On recasting, it seemed advisable, for the sake of completeness, to incorporate into the main argument several important points raised by Mr. Long, which had not been discussed in the main body of the original text. The most important are: whether the Maya were interested in quadratures or trines; the possibility of the Maya being able to calculate the sidereal revolutions of the planets; and the deductions from modern sources as to whether the Maya maintained an unbroken count in ancient times.

I am indebted to Dr. Carl E. Guthe for criticisms and suggestions on the lunar section; while Mr. Lawrence Roys has been good enough to contribute Appendix II, dealing with the possibilities of the Maya having been able to compute the sidereal revolutions of the planets.

Published views of earlier writers, particularly those of Dr. J. E. Teeple, have been incorporated in the text without specific references, since it is assumed that those interested in this aspect of Maya archæology are already well acquainted with the literature of the subject.

Finally, my thanks are due to Dr. S. G. Morley, dean of Maya epigraphers, for his criticism of the completed manuscript and for his kind permission to use unpublished material collected by him.

Chicago, November 1934 J. ERIC THOMPSON

MAYA CHRONOLOGY: THE CORRELATION QUESTION

THE GENERAL PROBLEM

Several years have passed since the publication of the last detailed discussion of the factors involved in reaching a correct correlation of the Maya calendar with our own.[1] Certain progress can, however, be recorded, and certain limiting factors, to which any correlation must conform, can now be more firmly established. Before discussing the merits and demerits of individual correlations, these limiting factors will be briefly reviewed.

THE 260-DAY COUNT

More progress can be recorded in settling the correlation of the 260-day count with our own calendar than, possibly, in any other line of investigation. A large amount of evidence has been brought forward in confirmation of the correctness of the original Landa correlation. This can be summarized as follows:

1. Spinden's evidence that Landa's calendar represents the year 1553, and that 12 Kan accordingly was the equivalent of July 26 in that year.[2]

2. The modern Jacalteca and Quiché calendars shown by La Farge,[3] Lothrop[4] and Burkitt[5] to be only one day out of conformity with the Landa day count after extending the last to modern times.

3. The Cakchiquel and Aztec 260-day counts shown by Spinden[6] and Long[7] to have been only one day out of conformity with the Landa count.

4. The statement brought to light by Martinez that 11 Chuen 18 Zac fell on February 15, 1554.[8] Accepting Martinez' correction of three days, the Yucatecan equation is only moved three days out of conformity with the Landa count.

5. The correlation of Aztecan and Maya days made by Martinez at the same time. These would make the Landa count two days off.

6. A final equation might be suggested. The revolt of the Maya against the Spaniards started on November 19, 1546. According to the Landa correlation November 18, 1546, was the day 1 Imix. On the assumption that the Yucatecan day commenced at sunset, November 19, 1545, probably corresponded to 2 Ik. Is this just chance, or was the day 1 Imix, the most important day in the count, and the start of the calendar, deliberately chosen for the revolt? In view of the known importance of lucky and unlucky days in the daily life of the Maya, the writer feels that this was not pure coincidence, but that this day was deliberately chosen as the day, or the eve, of the revolt, for although the massacre of the Spaniards started on the morning of November 19, the assembling for the revolt and the appropriate ceremonies, divinatory and propitiatory, must have taken place on the previous day. It is interesting to note that another plot in Maya history was apparently timed for 1 Imix.[9]

[1] Teeple, 1930; Spinden, 1930; Palacios, 1933; and Roys, 1933. The latter two, however, present summaries of the whole problem for the benefit of the non-specialist.

[2] Spinden, 1924, pp. 84–86. It should be noted that all European dates in this publication are given in the Gregorian calendar unless otherwise noted.

[3] La Farge and Byers, 1931, p. 176.

[4] Lothrop, 1928, p. 654.

[5] Burkitt, 1930–1931.

[6] Spinden, 1924, p. 102.

[7] Long, 1934.

[8] Martinez, 1926.

[9] R. L. Roys, 1933, p. 115.

Such close conformity obtained from sources so divergent in time and distance is almost overwhelming evidence against any correlation that does not depend on an equation which makes 12 Kan the equivalent of July 25 or July 26 in 1553.

It does not necessarily follow that this equation can be carried back without a break to Cycle 9,[1] but the evidence, given on page 81, would very strongly suggest that there was no break, and that the 260-day counts of the different peoples of Middle America were in mutual conformity, with a possible error of one or, perhaps, two days, from their inception, presumably on a pre-Maya horizon.

Naturally, if there was a break, all Sixteenth Century evidence must be discarded, and a correlation must be reached through astronomy checked by ceramics and architecture. In this publication the assumption is made that there was no break.

THE YEAR COUNT

The evidence for correlating 1 Pop with July 25 or July 26 in 1553 is strong, although not so strong as for the 260-day count. The evidence is as follows:

1. Landa's statement that the year started on July 26 in a year that Spinden has shown was 1553. Martinez' equation would move this to July 23.
2. The 11 Chuen 18 Zac double date, as amended by Martinez to equal February 25, leads to July 23, 1553, for 1 Pop.
3. The calendar from the Alta Vera Paz indicates that those months were frozen into the European year in such positions as strongly to suggest that they were formerly in conformity with those of Yucatan.[2]
4. Spinden has shown that certain of the months in the Tzeltal year were also, in all probability, in close conformity with those of Yucatan before they marched in step with the European year.[3]
5. The Pokomchi months given by Father Moran are in general agreement with the months of the Alta Vera Paz calendar.
6. The various Chilam Balams and Aguilar give Maya years starting on July 16. These could hardly have been copied from Landa. Accordingly the Maya year must have been frozen into the European in a year shortly after the Conquest, when 1 Pop corresponded to July 16 (Julian).
7. As we know that 12 Kan coincided with 1 Pop in 1553, and that the former almost certainly fell on July 25 or July 26, and was a year bearer, it is clear that 1 Pop fell on July 25 or July 26, 1553.

Reasons for believing that the Maya year started on 1 Pop, and not 0 Pop are given in Appendix IV. The evidence that the Maya year in 1553 started on 1 Pop, which was the equivalent of July 26 or possibly July 25 is strong. The evidence that the year bearer 12 Kan also fell on July 26 or possibly July 25 in 1553 is even stronger.

THE KATUN COUNT

Little new material has come to light in recent years that materially aids in solving the problem as to when the Katun 13 Ahau of the Conquest fell. The

[1] Technically the date 9.15.0.0.0, for example, occurs in Cycle 10, but as the average person finds it difficult to adjust himself to this terminology, the practise is here followed of naming a Cycle from the Cycle coefficient. Thus 9.15.0.0.0 is considered in this publication as falling in Cycle 9.

[2] Gates, 1931, and Thompson, 1932a.

[3] Spinden, 1924, pp. 87–88.

matter has been exhaustively treated by Morley,[1] who reaches the conclusion that the Katun 13 Ahau of the Conquest ended between December 1536 and February 1537. Spinden, however, ends this Katun on April 22, 1536, basing this date on the Landa typical year.

Since the evidence has been so thoroughly investigated by Morley, it is not intended to examine afresh in this publication all the statements that can be used for correlating Katun endings with events that are fixed in the European chronological system, but rather to attempt to weigh the value of the various statements according to the reliability of the writings in which they are found. Morley and Spinden place great reliance on the various statements on the correlation to be found in the Chilam Balam of Chumayel, Mani and Tizimin, but in this summary these statements are discarded as original material, being used only as confirmatory evidence.

The reason for this step is to be found in the undoubted fact that many errors have crept in and additions been made to the various Chilam Balams by the numerous copyists. The Chilam Balam of Chumayel contains many such errors or additions, which are on several occasions clearly self-contradictory. For example on page 17 we read that Campeche was seized in the year 1513 in Katun 13 Ahau, while on page 21 we read that Katun 11 Ahau began in 1513, and that Merida was begun in 1519.[2] On page 86 we read that the Spaniards came to Merida in Katun 9 Ahau, whereas on page 80 we are told that the Spaniards first arrived in the first Tun of Katun 11 Ahau.

Before taking up the evidence from other sources, it would be advisable to discuss one statement in the various Chilam Balams containing obvious errors, but one of great importance, as it has been used as evidence for more than one correlation. This is the reference to the massacre of the rain bringer.

Spinden has already pointed out that two events are almost certainly confused here. One of these was the massacre of the water bringers at Otzmal in 1536 and the other was some event that took place in 1545 or, perhaps, 1546. Since the Calendar Round date 9 Imix 18 Zip actually falls in a year 4 Kan, ritualistically associated with the east, this part of the statement is undoubtedly correct, and must refer to a year July 1545–July 1546 corresponding to the year bearer 4 Kan. According to the Landa correlation 9 Imix 18 Zip in a year 4 Kan corresponded to the autumnal equinox (September 23). Is this pure coincidence, or did the massacre occur in connection with some annual pilgrimage or ceremony in celebration of the equinox?

Perhaps a later copyist, knowing only of the massacre of Otzmal added that it was the water bearer who was killed and the year was 1536.[3]

On the other hand, the statements in the Chilam Balams of Mani and Tizimin that the event took place six years before the end of Katun 13 Ahau appear to

[1] Morley, 1920, Appendix II.

[2] R. L. Roys, 1933, p. 84. In a very interesting foot note Roys suggests that the writers of the Eighteenth Century, believing a Katun to have consisted of twenty-four years, put in some of these statements.

[3] Nakuk Pech mentions the sorcerers "Etzob" in connection with the revolt of 1546. A Maya word for sorcerer is Ahpulyaah, while the word used for water bringer is Ahpulha. Is it possible that the original text was written Ahpulyaah, but that this was incorrectly copied as Ahpulha, and the year 1536 added to clarify the statement?

belong to the original statement giving the Maya date. If 1545 was six years before the end of Katun 13 Ahau, the Katun ended in 1551 or 1552, but this is contrary to all the evidence. Mr. Ralph Roys, whom I consulted as to the possibility of emending the text so as to make it read as six years after the close of Katun 13 Ahau, suggests that the latter may well have been the original text, but perhaps a copyist, connecting the statement with the massacre of Otzmal, and knowing that that took place in 1536 before the end of Katun 13 Ahau, associated with the foundation of Merida, changed the sentence to read that the massacre took place six years before Katun 13 Ahau. If, however, the original statement was six years after the end of the Katun, the Katun ended in 1539 or 1540.

This case, treated at some length, tends to show how unreliable are the various Chilam Balams as at present constituted.

The earliest written reference to the end of a Katun occurs in Landa's history of Yucatan. Landa's statements on the Maya calendar having been proved correct in almost every detail, one must consider any statement of his on the end of a Katun as highly reliable. He tells us: "The Indians say that the Spaniards had just arrived at Merida in the year of our Lord 1541, which was exactly the first year of the era of 11 Ahau . . . and they arrived in the very month of Pop, which is the first month of their year."

Actually the Spaniards first arrived at Merida toward the end of 1540, but the actual incorporation of the city did not take place until January 6, 1542. If Landa's statement refers to the actual first arrival of the Spaniards at Merida, then Katun 13 Ahau ended and Katun 11 Ahau began somewhere between the latter part of 1539 and the latter part of 1540.

Certainly the Spaniards did not arrive in the month of Pop. They did, however, defeat the Maya in the month Kayab (June 11, 1541), and the subjection of the country might have been considered to have been completed by the month Pop, five or six weeks later. If Landa's statement refers to the subjection of the surrounding country, Katun 13 Ahau ended between 1540 and 1541. It is unlikely that the actual ceremony of incorporation of the city would have been marked by the Maya.

Despite a certain ambiguity in Landa's statement, one can draw the conclusion from it with some certainty that the Katun ended between the fall of 1539 and the fall of 1540.

Landa's statement receives confirmation from Roys' translation of a passage in the Chilam Balam of Chumayel. He translates a sentence of the third chronicle to read: "It was in the first tun of 11 Ahau, that was the Katun, when the Spaniards first arrived in our land."[1] This must refer to the occupation of Merida, since the first arrival of the Spaniards in Katun 11 Ahau is contradicted by the great mass of evidence from all sources. The statement is followed by another to the effect that Christianity was introduced in the seventh Tun of Katun 11 Ahau. If Katun 13 Ahau ended between the fall of 1539 and the fall of 1540, the seventh Tun of Katun 11 Ahau coincided with the period between the fall of 1545 and the fall of

[1] R. L. Roys, 1933, pp. 142–143.

1546. It was at this time, according to Brinton, that the first priests seriously to set about the spiritual conquest of the Maya arrived. The two statements in this sentence corroborate each other, and at the same time support the Landa statement. A final remark in this paragraph to the effect that the year was 1519 is clearly incorrect. However, Roys has suggested that the dating of the beginning of Katun 11 as 1513 may have been due to Eighteenth Century calculations that a Katun was twenty-four years long. By those calculations the seventh Tun of Katun 11 Ahau would have coincided with 1519.

The second statement which clearly has not been tampered with or altered by copyists is that a Katun 3 Ahau was running its course when Fathers Orbita and Fuensalida reached Tayasal late in October of 1618.[1] This statement, as Morley has shown, is of vital importance, since it shows that the calendar of Tayasal was in agreement with that of Yucatan so far as the numbering of the Katuns is involved. It has been suggested that the Katun count might have broken down in Yucatan during the period of confusion and strife following the fall of Mayapan. However, the Itza of Tayasal had left Yucatan prior to this period and, therefore, before their calendar could have been affected by the unrest in the north. Since the Tayasal Katun endings are in agreement with those of Yucatan, one can assume that there was no break-down in the count of the Katuns in Yucatan in the period subsequent to the fall of Mayapan. Yet this is the period, if any, when one would most expect a break-down in the count, for it coincided with the greatest disintegration in pre-Spanish Maya history.

If a Katun 3 Ahau was running its course October 31, 1618, the Katun 13 Ahau of the Conquest ended somewhere between July 22, 1500, and December 24, 1539. This is in agreement with the deduction from the Landa statement that the Katun ended late in the fall of 1539.

Page 66 of the chronicle of Oxkutzcab has been discussed in detail by Morley, who has clearly shown that the only conclusion that can be drawn from it is that Katun 13 Ahau ended in 1539. Although this page can not be classed as original material, its value is greater than that of the various books of Chilam Balam. It has, apparently, not passed through the hands of several copyists, but was translated directly fron hieroglyphs. There are, therefore, less probabilities of errors having found their way into the text. However, the numerous references to year bearers and Tun endings are arranged in tabular form, and it is accordingly a simple matter to rectify an error in one entry by other entries. In this way it is possible to restore the document to its original purity.

Accepting the statement that a Tun 13 Ahau ended in 1539 on the day 8 Xul, and applying the Landa correlation to this, Tun 13 Ahau, which must have also been the end of a Katun, ended on November 12 or November 13, 1539. This is in agreement with Landa's statement as to the end of the Katun, and is also in agreement with the Tayasal count.

[1] The fathers reached Tipu on their return from Tayasal five days after leaving the lake. Their arrival at Tipu was at the beginning of November, so the memorable conversation must have taken place near the end of October. According to the 12.9.0.0.0 correlation Katun 3 Ahau began in 1615; according to the 11.16.0.0.0 correlation the Katun began on September 20, 1618, but the 11.3.0.0.0 correlation makes the Katun start in 1621.

An original statement which may refer to the end of a Katun is supplied by Father Avendaño. He tells us that he was thoroughly conversant with the Maya calendar, and that when he was at Tayasal, about the middle of January 1696, he discussed it with the leaders of that city. He writes that at first they denied all knowledge of the matter, but eventually agreed with him that some four months were lacking until the time appointed by ancient prophecy for them to accept Christianity. One suspects that Avendaño did most of the talking, and that the Itza leaders rather sullenly agreed with his arguments.

This statement strongly suggests that Avendaño calculated that a Katun should end about May 1696. Since Katuns ending on the day 8 Ahau appear to have played such a part in shaping Itza history, it is not improbable that Avendaño knew of some prophecy to the effect that the Itza of Tayasal would embrace Christianity in a Katun 8 Ahau, and with this in mind chose the date of his visit to the Itza so that it would be close to the start of this fateful period.

If Katun 8 Ahau began in May 1696, the Katun 13 Ahau of the Conquest ended in August or September of 1538. While agreeing generally with the evidence that Katun 13 Ahau ended between 1536 and 1542, it disagrees with Landa's statement, leading to the conclusion that the Katun ended late in 1539.

It may be that Avendaño did not have in mind the start of a Katun. He may have calculated that July 19 corresponded to 13 Kan 1 Pop in 1541, the year of the Conquest of Yucatan. Without taking the Gregorian reform into account, he would reach June 8, 1696, as again 13 Kan 1 Pop. The recurrence of 13 Kan 1 Pop, which originally had signalized the submission of Yucatan, might seem to him and to the Itza a suitable time for Tayasal to come into the Christian fold. A return to Tayasal four months after his first visit (*i. e.* not earlier than the middle of May) would have given him a short time for instruction prior to the mass conversion on 13 Kan 1 Pop.

The chronicle of Nakuk Pech has unfortunately not as great value as might be expected, since the original is missing, and only copies exist. Nakuk Pech tells us that Aguilar was eaten[1] in 1517 by Ah Naum Pat, and that in that year the Katun ceased to be taken. This statement, as Morley has pointed out, is clearly wrong, for Aguilar was not eaten or fattened up in 1517, but was rescued by Cortez in 1519.

It would seem that since Pech could have known nothing about the European calendar before 1541, the association in Pech's mind was not of the Katun and 1517, but Aguilar and the year a Katun ended.[2] That must have been one of the ways the Maya associated events, for the end of a Katun was a very important event in their lives, marked, as it was, by an elaborate ritual that probably included human sacrifices all over the country.

On this assumption Pech knew that Aguilar was making history in the closing year of a Katun, which, by inference, must have been Katun 2 Ahau. Only two

[1] Martinez, 1926a, suggests the translation "fattened up" in place of "eaten." R. L. Roys in an oral conversation makes the suggestion that *hantabi* may be a copyist's error for *antabi*, which would mean aided.

[2] Just as a farmer might tell you that his daughter was married in the year of the great drought, or an old soldier of the British army would tell you that he enlisted the year Queen Victoria died.

events of outstanding importance connected Aguilar with the Maya. The first was his arrival on the coast of Yucatan in 1511 as one of a group of castaways; the second was his dramatic rescue by Cortez in 1519. The first of these dates is clearly too early to coincide with the end of the Katun, so one must conclude that Pech associated the rescue of Aguilar by Cortez with the year in which Katun 2 Ahau ended. In that case the statement indicates that the Katun ended between the beginning of March 1519 and the beginning of March 1520, and accordingly Katun 13 Ahau ended between the middle of November 1538 and the middle of November 1539 (Old Style). This agrees with the statements of Landa and others already discussed.

The second reference made in the Nakuk Pech documents to the Katun count occurs in an obscure passage connecting the arrival of the Spaniards at Merida with Katun 11 Ahau. However, the reference to Katun 11 Ahau is qualified by the expression *u hotzuc*, which has generally been translated as "fifth division." Morley takes this to be reference to Tuns, believing that the whole should be taken as a statement that the Spaniards reached Merida and established themselves there in the fifth Tun of Katun 11 Ahau, or after five Tuns had passed.[1]

It is difficult, however, to believe that *hotzuc* can refer to five Tuns or fifth Tun. There are many references to sub-divisions of Katuns in Maya writings, but the word Tun is invariably used. Furthermore, *tzuc* appears to convey an idea of heap or pile, a term scarcely applicable to the Tun. It is possible that *ho* is not numerical, but the word in the original text may have had some connection with *hoth*, a word meaning to carve, perhaps with reference to the carving of the Katun stone.

Outside of the various books of Chilam Balam there do not appear to be other references to Katun endings. As already stated, the references in the various books of Chilam Balam are in general agreement that Katun 13 Ahau ended between 1536 and 1542. One or two of the statements, however, that appear to have been correctly given are open to more than one interpretation.

The statement in the Chilam Balam of Mani, for instance, to the effect that the Spaniards first passed and came to Yucatan in Katun 2 Ahau is open to more than one interpretation. It has been taken to refer to the washing up of Valdivia and his few companions on the east coast of Yucatan in 1511.

It is true that they were the first Spaniards to come to Yucatan, but the arrival of a few castaways, who, with two exceptions, were eaten or died in attempting to escape, can scarcely have made a very great impression on the great mass of Maya not residents of the east coast. These first Spaniards were a sorry band with little, apart from their physical appearance, to awe the Maya, who were already accustomed to castaways from the West Indies,[2] and who must have known of the white man since Columbus's fourth voyage.

What must have remained in Maya memory was the first arrival of the armed forces of the Spaniards in their outlandish boats. Tidings out of the New World

[1] Morley, 1920, pp. 476-477.
[2] Bernal Diaz, Chapter VIII.

must have stirred the quiet villages of Estremadura to a fever of excitement, but what of the effect of the invaders on the natives of Yucatan? What an impression must have been made on the Maya when in 1517 they first viewed the invaders under Hernandez de Cordoba with their biting swords of Toledo steel, their armor, and, not least, their strepitous muskets.

Of Valdivia and his ill-fated comrades it could scarcely be said that they passed and came to the land of Yucatan, but this term is certainly applicable to the forces of Hernandez de Cordoba, as his force, after a landing on the east coast, coasted the peninsula, making landings at Campeche and Champoton, and fighting a pitched battle at the latter place.

Should that be the case, Katun 2 Ahau was still ruling at the beginning of March 1517, and Katun 13 Ahau had not ended in the middle of November 1536.

In this same Katun 2 Ahau smallpox broke out according to the books of Chumayel and Tizimin. Here again one must decide whether to blame Valdivia and his party or one of the expeditions of 1517–19 for its introduction. It is possible that among the boatload of approximately ten castaways there was a case of smallpox, but the possibility can not have been very great. On the other hand Bernal Diaz tells us that a negro in the army of Narvaez was responsible for its introduction into Mexico in April 1520. It would seem, then, that the forces of Cortez could hardly have introduced the plague in Yucatan, and failed to do so in Mexico.

It is, then, most probable that the disease was introduced by the earlier expeditions, or spread from the Vera Cruz area.[1] If it was introduced by the early expeditions, Katun 2 Ahau could not have ended before March 1517, and if it spread from Mexico, Katun 2 Ahau must have ended after April 1520.[2]

The re-examination of certain elements of the Katun evidence confirms the general consensus that the Katun 13 Ahau of the Conquest ended between 1536 and 1542, but the best evidence shifts the indicated year from 1536 to 1538–40. In short the evidence indicates that among the Maya of Yucatan and the Itza of Tayasal a Katun 13 Ahau certainly ended between 1536 and 1540, and of these years the most reliable evidence suggests that 1539 or 1540 witnessed the actual close of the Katun.

In order to exhaust all possibilities, let us suppose that the Katun of the Conquest ended between 1536 and 1542, but for some obscure reason it was not named for the day on which it ended. Two assumptions will, however, be made. The first is that 12 Kan 1 Pop fell between July 25 and July 27 inclusive in 1553. The second, for which ample justification will be given later, is that 9.16.4.10.8 coincided with new moon or was not more than one day after or two days before new moon.

One Calendar Round is 18980 days, which, in turn, are equal to 643 moons less 8.17 days. Since 11.16.13.16.4, 12 Kan 1 Pop at July 12, 1553 makes 9.16.4.

[1] By July 1520, smallpox had spread like a wild fire over Central Mexico.

[2] Narvaez' fleet reached San Juan de Ulua April 23, 1520 (O. S.). We have no reason to believe that any landings were made in Yucatan. However, it is possible smallpox spread from Panama.

10.8 a new moon, Calendar Rounds can only be added or subtracted from this in such multiples as will convert the surplus of 8.17 days into even moons.

The only possible subtractions are seven and eleven Calendar Rounds. These produce 10.18.4.15.4, 12 Kan 1 Pop and 10.7.13.17.4, 12 Kan 1 Pop as the Long Count positions of Landa's typical year of 1553. The first equation would make the Katun of the Conquest fall in 1530, but this is outside the limits 1536–42. The second equation would make 10.7.0.0.0 6 Ahau 8 Tzec fall in 1539, thus conforming with much of the evidence already presented as to the year in which the Katun ended, but this Katun ends on 6 Ahau, not on 13 Ahau.

Actually 6 Ahau 8 Tzec is just twenty days before 13 Ahau 8 Xul, on which date, according to the Chronicle of Oxkutzcab, a Tun, and, by inference, a Katun ended in 1539. Thus, if one is prepared to accept the possibility that the Katuns were named not for their closing days, but for days one Uinal later, a new correlation in conformity with Sixteenth Century evidence is possible.

There is no evidence, however, that Katuns were not named for their ending days.[1] Furthermore, although 9.16.4.10.8 thereby becomes a new moon date, this new moon can not be the date of an eclipse, for it is in the wrong 260-day group. The correlation is also in the wrong Calendar Round for fitting the Venus data. The suggested correlation must, therefore, be rejected.

THE VENUS COUNT

There is little to add to the deductions made by Teeple with reference to the bearing of the Venus data on the correlation question. He has shown that the only correlations based on Sixteenth Century data which will agree with 9.9.9.16.0, 1 Ahau 18 Kayab as the approximate date of an heliacal rising of the planet Venus are those which make the Katun 13 Ahau of the Conquest fall on 10.10.0.0.0 or 11.16.0.0.0. The 12.9.0.0.0 and 11.3.0.0.0 correlations show Venus to have been over 250 days from heliacal rising at 9.9.9.16.0, 1 Ahau 18 Kayab.

A little fresh light, confirming Teeple's original elucidation of the matter, can, perhaps, be thrown on the system employed by the Maya for correcting the cumulative error in the Venus count.

The second row of figures on page 24 of the Dresden Codex undoubtedly deals with this matter. The sums of the columns in this row, reading from right to left, are: 9100, 33280, 68900 and 185120. Teeple has already shown that the second and third columns record 57 Venus years with a correction of eight days and 118 (57 and 61) Venus years with a correction of 12 days. As he has shown, the second column leads to the new base of the Venus cycle, 1 Ahau 18 Uo, while the third column leads to the later cyclic base, 1 Ahau 13 Mac.

The total of 9100 days in the first column, as it stands, has no apparent connection with the Venus year, and appears to be wrongly written. Since the calculations on this page, with the sole exception of the Initial Series, are in multiples of 260 days, one would suspect that 260 days or some multiple of this number have been added or subtracted from the total through a mistake in calculation.

[1] R. L. Roys, 1933, p. 167 "On 13 Ahau the Katun will end."

By adding a unit of 260 days, a total of 9360 is reached. This is 16 days short of 16 Venus years, which in turn is the amount to be deducted from 260 Venus years according to the Maya system of slicing off the final 4 years and 4 extra days from a Venus cycle of 65 years.

The fourth column of 185120 represents 317 Venus years minus 8 days, but after the lapse of this interval one would expect a correction of 24 or 25 days. However, if the amended total of 9360 for the first column is subtracted from the total of the fourth column, the result is 301 (57+61+61+61+61) Venus years with a correction of 24 days. In other words the Maya scribe appears to have wished to record that 16 Venus years and 16 days have been sliced off from the total of four cycles (260 Venus years), but as this is not quite enough a further cycle, from which 8 Venus years and 8 days have been sliced, is added to the total, thereby producing 301 Venus years with a total correction of 24 days.

This sum of 301 Venus years less 24 days leads from 1 Ahau 18 Kayab to 1 Ahau 8 Chen as the base of a new Venus cycle. The position 8 Chen is recorded on page 47 of this same codex immediately below the position 18 Uo reached by the second column, and the position corresponds to heliacal setting of the planet.

It has generally been assumed that the Venus year ended and started at heliacal rising, but of the six base dates for the starts of Venus cycles, as calculated from page 24 of the codex, three (1 Ahau 18 Uo, 1 Ahau 18 Pax, and 1 Ahau 8 Chen) are recorded on page 47 of this codex as heliacal settings whereas the other three (1 Ahau 18 Kayab, 1 Ahau 13 Mac, and 1 Ahau 3 Xul) are recorded on page 50 as heliacal risings.

In view of the known desire of the Maya to record completion, it would, perhaps, seem more probable that the astronomers would have been more interested in the heliacal setting of the planet than in its heliacal rising. In this connection it is interesting to note that should the day 1 Ahau be ritualistically associated with heliacal setting, the day Lamat automatically becomes associated with heliacal rising eight days later. This is an interesting connection in view of the well-known association of Lamat with the planet Venus. On the other hand if 1 Ahau is ritualistically associated with the heliacal rising of Venus, there is no reason for associating Lamat any more than Kan, Eb, or Cib with the planet Venus.

Page 24 of the Dresden Codex indicates that there was an accumulated error of eight days at the close of the 1 Ahau 18 Kayab table. This may have been shown by the placing of 1 Ahau 18 Kayab at heliacal rising instead of heliacal setting, and then correcting the table by subtracting eight Venus years and eight days so as to bring the start of the new cycle back to heliacal setting at 1 Ahau 18 Uo as recorded on page 57 of the codex. The date 1 Ahau 18 Kayab thereby becomes heliacal rising in the ninth year of the new cycle, this ninth year having started with heliacal setting on 6 Eb 10 Kayab. The numeration on these pages based on heliacal risings might have been written just before the introduction of the 1 Ahau 18 Uo cycle to demonstrate that, as a result of the accumulated error of eight days, 18 Kayab, the start of the old cycle, which was supposed to coincide with heliacal setting, had at the close of the cycle actually come to coincide with heliacal rising. Alternatively, the numerical matter may have been added or altered by a later copyist who associated 18 Kayab with the start of a cycle, yet realized that it had slipped eight days to become heliacal rising in a later cycle. In favor of this thesis is the undoubted fact that the compilation of the Dresden Codex was subsequent to 9.9.9.16.0.

The start of the Venus year at heliacal setting can not, of course, be considered as having been proven, but enough has been written to show that this must be seriously considered as an alternative to the generally accepted idea that the Venus year started with heliacal rising. Heliacal risings were, of course, of very great importance, for it was on these occasions that the planet harmed mankind. The emphasis on heliacal risings in the Venus tables may, therefore, have been due to the divinatory importance of these phenomena, whereas the astronomical count may have started from heliacal setting.

A suggestive Venus inscription at Naranjo is discussed in Appendix I.

LUNAR DATA

The work of Guthe on the lunar inscriptions of the monuments seems to establish that the Maya lunar count during the period of uniformity might have been based on computation rather than on observation.[1] This would appear to favor the Ludendorff-Spinden thesis that the Maya lunar count was not based on observation, but was computed, the error in the computation having amounted to four days in the interval between 7.0.0.0.0 and the Great Period.

Although there is evidence for a computed calendar of the Dresden Codex type during the period of uniformity, yet the lunar calculations in the earlier period, whether based on observation or computation, were in some cases incorrect and apparently were adjusted at intervals. Evidence from various monuments indicates an accumulating error which soon would have been very great unless a correction were made. Examples of this are given below.

1. Stela P at Copan records 9.9.10.0.0 as 9D 3C, while Stela 13 at the same city gives the moon age of 9.11.0.0.0 as 5D 3C. From this we deduce that the Maya calculated 10,804 days as the interval here between new moons. Actually the interval between the new moons should be 10,808.66 days. That is to say, the Maya reckoning is almost five days too short in a period of thirty Tuns.

2. Stelæ D and Y at Pusilha record 9.10.15.0.0 as 3D 3C. Stela H, at the same site, gives the moon age at 9.11.0.0.0 as 4D 3C. The interval here between Maya new moons is 1799 days, while computing from the length of the average moon it would amount to 1801.83 days. In other words the Maya reckoning is almost three days too short. Doctor Guthe, to whom I submitted this section for his advice, suggests that a straight alternation of 29 and 30 day lunar months may have been used at the period in question (30x30 plus 29x31 equals 1799). Alternative explanations are that the interval is the result of incorrect observations or is due to a correction in a computed calendar.

3. Stelæ O and P, at Pusilha, both give the moon age of 9.7.0.0.0. The former gives the moon age as 5E 6C; the latter records it as 3E 3C. The first stela was erected at 9.7.0.0.0, and the lunar count is therefore either based on observation or computation

[1]Guthe, 1932.

made at that time; the second stela was erected at a later date, probably at 9.10.15.0.0, and the lunar data, therefore, were almost certainly the result of computation.

If one accepts the Ludendorff-Spinden postulate that there was a single computed lunar count unmodified by correction, it should be perfectly simple to find out the age of the moon at some date in the past by computation, and one should get exactly the same result as was obtained when the date in question was present time, for at the later date one is merely subtracting what one has added by the same formula during the interval. Thus, if the moon age of 9.7.0.0.0 at Pusilha was computed at 5E 6C at that date, the same moon age of 5E 6C should be reached when a computation is made backward to the same date some seventy-five years later. This is not the case.

There are three explanations. Firstly, the 5E 6C moon age may have been based on observation at 9.7.0.0.0, but when the moon age of this same age was calculated back from 9.10.15.0.0 the position 3E 3C was reached. Secondly, both results for 9.7.0.0.0 may have been computed, but in the interval between 9.7.0.0.0 and 9.10.15.0.0 the method of calculation was altered, and consequently on computing backward by the new method a different result was obtained for the moon age at 9.7.0.0.0. Thirdly, an error in carving the glyphs or in making the computation may have occurred. One can scarcely employ the third to bolster up some preconceived theory.

4. The double dates that usher in the period of lunar uniformity appear to indicate a correction having been made at Naranjo and Piedras Negras,[1] since the same dates are recorded with the D and E glyphs one day apart, but this discrepancy might be due to a shift in the start of the lunar count from the disappearance of the old moon to new moon.

5. There also were, it would seem, corrections at the close of the period of uniformity. Stela S, at Quirigua, gives the lunar age of 9.15.15.0.0 as 5D 4C, while Stela F, at the same site, records the lunar age of 9.16.10.0.0 as 3D 6C. The interval between new moons is here by Maya calculation 5402 days, while based on the average moon it would be computed as 5404.1 days. That is to say, the Maya interval is two days too short. The interval of 5402 days can not be calculated from the Dresden Codex. Therefore if the Dresden Codex pattern was used during the period of uniformity, it was not used afterward unless some adjustment was made.

On my raising this point with Doctor Guthe, he replied: "These two monuments and similar evidence at other cities indicate that an adjustment in the lunar count was made about this time, which in itself would explain the apparent error of two days. It is reasonable to assume that towards the end of the period of uniformity the count was not in perfect accord with the phenomena it professed to record, otherwise there would have been no adjustment." [2]

None of the examples noted above indicate an uncorrected lunar pattern based on computation. Either they are the result of poor observation, or, if computed, show either an attempt to adjust the computation to observations by the addition or subtraction of two, three or four days or the substitution of one type of computation for another which had proved defective.

[1] Teeple, 1930, p. 57.
[2] Letter dated August 19, 1933.

If the computations were corrected or changed, one can not accept the Ludendorff-Spinden thesis of an uncorrected lunar count based on computation which had accumulated a four-day error after some centuries of usage. If the lunar recordings were based on observation, the Ludendorff-Spinden thesis is equally unacceptable, as Teeple has shown. If only uncorrected lunar counts were used, the accumulated error at some cities would have amounted to weeks.

Had the Maya possessed this philosophy of an inviolate lunar count allowed to get out of step with lunar observations rather than be permitted to undergo correction, it is difficult to believe that this same concept would not equally well apply to Glyph C of the lunar series, with which C and D are so intimately linked. If the count of Glyphs D and E was inviolate, one would similarly expect the groupings of lunations as expressed by Glyph C to be equally inviolate. Such, as is well known, is not the case.

The evidence, then, indicates that different cities changed their methods of computations, and that, as Guthe has pointed out, the different cities used different bases for the starting points of their respective counts. On the other hand, so far as I know, no evidence has ever been produced in favor of the inviolate lunar count, but the theory was adopted as the only means of saving the 12.9.0.0.0 correlation from being totally wrecked.

Another point of controversy is as to whether the Maya counted their moons from full moon or new moon. So far as I know, no evidence in favor of the former thesis has even been offered, and its support again is due to the fact that without this assumption the 12.9.0.0.0 correlation inevitably collapses. Evidence in favor of the count having been made from new moon has been given elsewhere,[1] but is summarized below for the sake of completeness. The lines of evidence are:

1. Bishop Landa's explicit statement that the Maya counted from when the new moon rises.

2. So far as is known, no Indian tribe, not even the Haida, count moons from full moon, which is more difficult than counting from new moon.

3. The Aztecs described the waning moon as dying, and when the old moon disappeared they said it was dead. Were the count from full moon to full moon it is hardly likely that the Aztecs would have said that it died in the middle of the count. In calendric and astronomic matters there was such a close bond between the Maya and the inhabitants of Central Mexico that this evidence is utile.

4. Similarly the Yucatecan Maya said that the waning moon was going away, and this scarcely suggests a count from full moon. The modern lowland Maya count from new moon, and if they originally counted from full moon it is strange that there is no reference in Spanish writings, nor any reference in native writings to the change over, such as occurs in connection with the new ideas about eclipses introduced by the Spaniards and carefully noted in the Chilam Balam of Chumayel.

5. Among the modern Kekchi new moon is called "The moon is born" and during the last quarter the moon is said to sleep. The same terms are used by the Pokomchi. Among the Kekchi Indians around Carcha the first few days of the new moon are called, according to Dieseldorff, *ac li po*, which means new moon.

[1] Teeple, 1930, p. 49; Thompson, 1932, pp. 409–413.

6. Glyph D of the lunar series is in its normal form a crescent, while glyph E, indicating twenty days later, has, normally, the form of a full moon. Were the count from full moon, it is unlikely that the full moon would be indicated by a moon crescent, and it is equally unlikely that Glyph E, which would fall about five days after new moon, would be shown as a full moon.

7. The period of the planet Venus was counted from its first appearance after conjunction or its disappearance before conjunction, and not from its period of greatest brilliance. One would expect the Maya to apply the same rules to the moon.

8. According to the commentator of the Codex Telleriano-Remensis the Aztecs paid no attention to lunar eclipses. This would hardly be expected of a people that counted from full moon.

Should there be any evidence that the Maya count was not from new moon or within a day or so of it, apart from the fact that such an arrangement does not fit the 12.9.0.0.0 correlation, it should be produced to be weighed against the eight lines of evidence in favor of a new moon count that are cited above.

THE CERAMIC EVIDENCE

Vaillant has recently set forth his ideas on how his conclusions on Maya and Mexican pottery sequences and their dating affect the various correlations.[1] The ceramic evidence leads him to favor a correlation in the neighborhood of 11.3.0.0.0 or possibly even as far forward as 10.10.0.0.0. However, this conclusion rests largely on his dating of the ceramic periods at Holmul. He suggests that the end of the uniform lunar count, which he places at 9.18.0.0.0, may have coincided with the close of figure painting, typical of the Holmul V period.

From an epigraphic viewpoint the date of 9.18.0.0.0, suggested by him as the close of the figure painting phase, might be challenged. While it is possible that the unification of the lunar count might have coincided with a wide diffusion of figure painting, yet there is no apparent reason to suppose that the abandonment of the uniform lunar count caused the disappearance of figure painting. The end of the period of lunar uniformity would, one may presume, have affected only a small group in each city—the small priest astronomer group—while the ordinary people, including the potters and artists, probably never knew of the change. In any case, it is difficult to believe that a change in the method of numbering moons would have had any effect on any other phase of Maya activity, even had the whole community been cognizant of the change.

If, however, the end of the uniform lunar count could be employed to mark the close of the figure painting phase, Vaillant's own argument is stultified. He shows that figure painting was in vogue at Copan at 9.16.5.0.0, but at that time had not reached its highest level,[2] but by that date Copan had abandoned the uniform lunar count, and Quirigua had followed suit before another five years had passed. Indeed, Teeple himself suggested that the later lunar dates, such as those on stelæ erected at Naranjo around 9.18.0.0.0, which actually agree with the uniform lunar system, probably only did so by accident.[3]

[1] Vaillant, 1935. Doctor Vaillant was so kind as to place the manuscript of this paper at my disposition prior to its publication.
[2] Merwin and Vaillant, 1933, p. 81.
[3] Teeple, 1930, p. 61.

On the other hand, glyphs painted on at least two Holmul V type vessels[1] are stylistically later than the classical types which lasted at least until 10.3.0.0.0.[2] This evidence leads to the conclusion that figure painting continued to flourish after 10.3.0.0.0. The unfilled gap produced by Vaillant's application of ceramic periods to the various correlations from 11.3.0.0.0 to 12.9.0.0.0 is thereby shortened.

Vaillant has tentatively placed Holmul I at the end of Cycle 8 and the beginning of Cycle 9, with the implication that Holmul III was flourishing about 9.10.10.0.0.[3] However, Holmul I burials are found beneath rooms with vaulted roofs at Holmul under circumstances that almost unquestionably point to the interments having been made subsequently to the erection of the vaulted rooms. That is to say the finds indicate that the Maya knew how to erect rooms with their typical false vaults before Holmul I pottery was fully developed.

Linton Satterthwaite, on the other hand, has collected evidence indicating that the Maya vault was not in use at Piedras Negras much before 9.10.0.0.0.[4] Since the Usumacintla Valley was in many ways culturally more advanced than the Petén, it is unlikely that the Maya vault was in use in the latter area before its introduction at Piedras Negras. If, therefore, the Maya vaulted arch was unknown at Piedras Negras much before 9.10.0.0.0, it is extremely unlikely that it was known at Holmul two hundred or more years earlier. In that case Holmul I presumably did not flourish at the close of Cycle 8 or very early in Cycle 9.

Should the vault evidence hold good for the Petén area, Holmul I must be dated around 9.10.0.0.0, and Holmul III presumably would be dated toward the last quarter of Cycle 9, and Holmul V would have flourished during part of Cycle 10, thereby confirming the epigraphic evidence.

At San José, in British Honduras, sherds from vessels closely resembling the tripod cylindrical vase shape typical of classical Teotihuacan occur at the end of the second ceramic period, which can be correlated with Holmul III and Uaxactun II. This would suggest that the classical Teotihuacan style or something from which it was directly derived was contemporaneous with Cycle 9.

If Vaillant is correct in placing Holmul III at about the middle of Cycle 9, and in dating the later classical Teotihuacan at not earlier than the end of the Tenth Century, then the correct correlation can not be earlier than about 11.0.0.0.0. If on the other hand, Holmul III should be placed toward the end of Cycle 9, the ceramic evidence would indicate an earlier correlation in the neighborhood of 11.7.0.0.0, but the Teotihuacanoid sherds at San José are probably not the result of direct influences from Teotihuacan, but from some other culture, perhaps of earlier date, which emanated influences in both directions.

The complete absence, so far as present evidence goes, of plumbate ware in the Petén or British Honduras is a strong argument against the acceptance of a correlation in the neighborhood of 10.10.0.0.0. According to that correlation the

[1] Merwin and Vaillant, 1933, Plate 30, c. The Kin sign.; Plate 29, c. The inverted Ahau glyph. Another probable example is to be found on a plate illustrated by Gann (1928, p. 74).

[2] Beyer, 1933.

[3] Merwin and Vaillant, 1933, pp. 82.

[4] Mr. Satterthwaite has kindly permitted me to refer to this conclusion he has tentatively reached, but the evidence on which it rests has not yet been published.

Mexican period in Yucatan should have started about 9.16.0.0.0. The interval of nearly 140 years between that date and the latest Petén date (10.3.0.0.0) should have been ample for plumbate to have found its way into the Petén, since the cities of that region are closer to the supposed center of distribution of plumbate pottery in El Salvador. Indeed, this interval of 140 years should in all fairness be extended, since it is difficult to believe that the Petén cities were abandoned immediately after 10.3.0.0.0. On the other hand, one must admit the possibility that the Petén cities may have had unimportant trade relations with outside cities, although this possibility does not appear very strong.[1]

Plumbate is also absent from San José, a site certainly occupied for some time after the close of Holmul V,[2] and a center of trade with distant areas. The absence of plumbate at San José would, therefore, perhaps be an argument against a correlation as late as 11.3.0.0.0, which would make the last date in the Petén as late as A. D. 1150, and the post-Holmul V periods at San José considerably later, even if the close of Holmul V is placed as early as 9.18.0.0.0 (A. D. 1050 in this correlation), but we have seen that Holmul V appears to have been flourishing at a later date.

The tentative dating of Holmul III as contemporaneous with the latter part of Cycle 9 implies, of course, that the Maya cities of the south were not abandoned at the time dated monuments ceased to be erected. Indeed, the theory of a great exodus from the "old empire" region early in Cycle 10 has been challenged in recent years.[3] Actually, Pusilha supplies an instance where pottery continued to be produced after the erection of monuments ceased.

It would seem, then, that ceramics can not be used as positive evidence in the correlation problem until the various phases can be more securely linked to dates in the Maya calendar. Pottery is, however, of immense value in warning us to walk as Agag did. Nevertheless, contradictory as the conclusions on pottery now appear to be, all lines of ceramic evidence point to the Katun 13 Ahau of the Spanish Conquest having fallen prior to 12.0.0.0.0, but probably after 11.0.0.0.0.

On the assumption, well substantiated by documentary evidence, that the start of a Katun 11 Ahau coincided within a few years with the foundation of Merida and that 12 Kan 1 Pop fell on or about July 26 in 1553, and on the second assumption, which is probable, but not susceptible to actual proof, that the Maya count was maintained unbroken, the possible correlations will be briefly discussed one by one with a view to seeing which fit best the conclusions already reached.

10.10.0.0.0, 13 AHAU 13 MOL CORRELATION

This correlation fits better the astronomical deductions from the Dresden Codex and the monuments than any other correlation in agreement with our two assumptions, provided that a small adjustment is made.

[1] The excavations at San José, British Honduras indicate that the Petén region was not cut off from the outside world, since trade pieces from widely scattered regions are found there in the late deposits. The presence of engraved red ware, but the absence of plumbate at San José, would argue that plumbate is later than engraved red ware.

[2] There is no ceramic period at San José that can be directly correlated with Holmul V, but the third ceramic period at San José seems to find its closest parallels with Holmul V, and certainly with the late period at Mountain Cow, which in turn connects with Holmul V. The fourth and fifth ceramic periods at San José certainly covered a considerable span of time.

[3] Thompson, 1931, pp. 230–231; Thompson, 1932, b, pp. 14–15.

If the equation 11 Chuen 18 Zac equals February 25, 1544, is accepted without making the three-day amendment proposed by Martinez, it leads to the equation 10.10.6.12.4, 12 Kan 1 Pop equals July 20, 1553. The Ahau equation to be added to Julian is 774078. This gives a new moon at 9.16.4.10.9, which was also the date of an eclipse (J. D. 2186927), not, however, visible in the Maya area. It leads to a heliacal rising of Venus fifteen days before the Dresden Codex date 9.9.9.16.0, 1 Ahau 18 Kayab.[1]

Despite this close agreement with the lunar data, there are a number of serious objections to the correlation. It makes the Katun 13 Ahau end in 1546, which is against all documentary evidence, unless one accepts Landa's statement that the reign of a Katun ceased ten years before the close of a Katun and assumes that the ends of the reigns and not the actual Katun endings were used in counting the Katuns in the Chilam Balam. It is also five or six days out of agreement with the Aztec and all other Maya 260-day counts.

If 10.10.0.0.0, 13 Ahau 13 Mol is the Katun 13 Ahau of the Conquest, the last date in the south, 10.3.0.0.0, corresponded to A. D. 1408, which is, in all probability, too near the date of the arrival of the Spaniards. So far as is at present known, the introduction of metal into the Maya area took place after 10.0.0.0.0, since no authenticated find of metal has ever been recorded from a horizon of indubitably earlier date.[2] If the above correlation were correct, the evidence would strongly suggest that the Maya had been acquainted with metal for less than one hundred and fifty years before the Spanish Conquest. On the other hand the discovery of copper bells in the Southwest United States on a fairly early horizon would indicate their manufacture in Mexico long before this date.

This argument, of course, is not decisive. The Maya of 10.0.0.0.0, or even earlier, may have had a limited quantity of metal objects. Their absence from votive caches and burials might be due to the fact that they lacked the prestige of ancient usage,[3] or the peninsula of Yucatan, being a peripheral area, may have failed to receive metal objects in trade until long after their introduction in Central Mexico.

This 10.10.0.0.0 correlation allows only one hundred and fifty years for the Nahua period at Chichen Itza if one assumes that the Initial Series lintel at that city (10.2.10.0.0) represents the close of the purely Maya period. This short period is reduced to a scant seventy years if one accepts the implication of the Chilam Balam that activity ceased at Chichen Itza after the fall of Mayapan in Katun 8 Ahau (1468 in this correlation).

[1] It is assumed that the reader has a grasp of the sources of the matters discussed in these pages, such, for instance, as the Venus counts, the Chronicles of Oxkutzcab, year bearers, etc., etc., and for this reason sources are not cited in the following pages.

[2] The only metal, found under archæological conditions in the Maya southern area, was discovered at San José, British Honduras, in 1934, in apparent association with the fifth ceramic period. This, in turn, is believed to date from after 10.0.0.0.0. Since the above was written a fragment of a gold figurine, probably of southern origin, has been found in a vault beneath Stela H at Copan (9.17.12.0.0). Since no other gold has been found in the Maya "Old Empire" region, this may represent a stray trade piece that found its way to Copan at this early date, since there has never been any reason to doubt that gold was worked in South America and the Panama-Costa Rica region before this knowledge spread to the Maya. Copan, being the nearest large Maya city to this region, might well have been the first Maya city to receive objects of gold in trade.

[3] *Cf.* On monuments in European churches, the recumbent knight is shown with his sword, hardly ever with firearms. One might also note the practise of the modern Maya of Quintana Roo of kindling the sacred fire by the old wood twirling method.

It is barely possible, however, that the Initial Series lintel at Chichen Itza was carved during the apparent period of Maya re-occupation following the downfall of Nahua influences at this city. Tozzer has already recorded his reasons for believing that the Northeast Colonnade at Chichen Itza belongs to this period.[1] The possibility that the Initial Series Lintel dates from the same period should not be entirely overlooked.[2]

Admittedly, it is no more than a remote possibility. In this connection it would seem not impossible that the Itza, who have always been looked on as Simon-pure Maya, were actually Nahua. Roys has pointed out that they were never considered real Maya.[3] Here again I am not expressing a belief, but calling attention to a possibility. After all, the Chronicles are contradictory and obscure. If one chronicle could contain an insertion of thirteen Katuns between the first arrival of the Spaniards and their Conquest of Merida, is it not very possible that the other events are equally confused?

Although flaws can be picked in the assignment of events to the different Katuns, yet it is hardly probable that events can be compressed in such a manner as to fit into a correlation that makes 10.10.0.0.0 the Katun of the Conquest. This correlation, then, can be rejected on the following grounds:

1. As it stands it is out of agreement with the apparent data regarding the moon and the planet Venus, or, if emended to fit these, it does not agree with 12 Kan on July 26, 1553.
2. It makes the Katun end in 1546, and for this there is no evidence from historical sources with the exception of the very dubious application of the statements about the reign of a Katun ending ten years after the actual start of the Katun. It also disagrees with the Chronicle of Oxcutzcab.
3. It is to a certain extent in conflict with the ceramic evidence, and leaves, in all probability, too short a period for Nahua influence at Chichen Itza. The alternative reconstruction, in which the Initial Series Lintel would fall in the post-Nahua period, is extremely dubious.
4. Its acceptance would indicate that metal was in all probability unknown in the Petén as late as A. D. 1400.

Individually, none of these objections can be considered decisive, but taken as a whole they argue very strongly against the correlation.

11.3.0.0.0, 13 AHAU 13 PAX CORRELATION

In the opinion of Vaillant a correlation in the neighborhood of 11.3.0.0.0 would appear to fit best the ceramic evidence. As an opinion entirely independent of partizanship, and one, moreover, based on entirely different grounds, it is worthy of great weight as a factor in reaching a decision.

[1] Tozzer, 1928, p. 162.

[2] It has been stated that the Initial Series lintel had been re-used as a wall stone in the late vaultless temple in which it was found. However, the photograph in Field Museum files, taken by E. H. Thompson at the time of the discovery of the lintel, shows it lying immediately behind the Atlantean figures, and the impression is very strong that it was originally in position resting on their tops. The fact that part of the inscription is hidden by the Atlantean figures should not be taken as irrefutable evidence for re-usage during the Nahua period. The above paragraph is not written as an argument in favor of the Initial Series lintel having been originally designed for the doorway of the Atlantean figures, but in protest against the way in which theories, which fitted into a preconceived outline of Maya history, have been accepted in the past without an attempt to examine alternative possibilities which conflict with these preconceptions.

[3] R. L. Roys, 1933, p. 84.

However, if 11.3.0.0.0 was the 13 Ahau of the Conquest, and 12 Kan 1 Pop is placed at July 25, 1553, a serious disagreement is immediately apparent so far as the apparent lunar and Venus data are concerned. This correlation makes 9.16.4.10.8 fall some sixteen days after a new moon, although both the Dresden Codex and the monuments indicate that it should fall on a new moon or within a day or so of it. Similarly it makes 9.9.9.16.0, 1 Ahau 18 Kayab about as far away from a heliacal rising of Venus, four days after Inferior conjunction, as is possible. However, if one is prepared to ignore the evidence given on page 67 against the Maya lunar count having been made from full moon, this correlation would lead to J. D. 2092031 as the equivalent of 9.16.4.10.7, a possible date of the start of the Lunar count in the Dresden Codex, and this date coincides with a full moon eighty-nine days after a lunar eclipse on J. D. 2091942. Nevertheless one is scarcely justified in overthrowing the evidence that the Maya counted from or very near new moon and that 9.9.9.16.0 was at a heliacal setting or a heliacal rising of Venus unless the evidence for such a correlation is very strong on other grounds.

Such evidence does not exist for the 11.3.0.0.0 correlation. The Katun in question would end in 1543, and for this there is no direct evidence in any historical source. Landa's statement indicating that the Katun might have ended in 1541 is the nearest approach, unless one accepts the statements of the death of the water bringer. These could be made to lead to a Katun ending in 1542, but they are so contradictory as to deserve little credence as they stand (p. 57). In addition to being in disagreement with the evidence on the years in which Katuns may have ended, this correlation fails to agree with the Chronicle of Oxkutzcab. Arguments against this correlation are:

1. It makes indicated new moon dates fall some sixteen days after new moons. The date 9.16.4.10.8 reduces to J. D. 2092032, ninety days after a lunar eclipse.
2. It makes 9.9.9.16.0 about as far from a heliacal rising of Venus as is possible.
3. It fails to agree with the Katun ending possibilities as indicated in the historical sources.
4. It runs counter to the list of Tun endings given on page 66 of the Chronicle of Oxkutzcab.

While it is more than possible that a correlation in the neighborhood of 11.3.0.0.0 is the answer to our problem, it can not be this one. Until ceramic evidence unmistakably indicates this date as the approximate position of the arrival of the Spaniards, it would be a waste of labor to discuss all the possible correlations that might spring up once one admitted a break in the Long Count some time between 10.3.0.0.0 and the arrival of the Spaniards. As Long (page 97) points out, such a break is unlikely in view of the way the Maya count stood the strains and stresses of the post-Conquest period without losing a day.

11.16.0.0.0 (GOODMAN-MARTINEZ) CORRELATION

The correlation that makes 11.16.0.0.0, 13 Ahau 8 Xul the 13 Ahau of the Conquest was first proposed by J. T. Goodman as long ago as 1905. It was neglected for over twenty years until restated in a slightly different form by Martinez

Hernandez.[1] Martinez, however, rejects the equation based on Landa's year that 12 Kan 1 Pop equals July 26, 1553. His correlation leads to July 23, 1553, instead of July 26, 1553. He believes that the Maya calendar used by Landa was collected in 1553, but that Landa knew that the start of Pop was on July 26 in 1541, but failed to intercalate three days to take care of the leap year days that intervened between these two dates.

Martinez was the first to discover the passage in the Chilam Balam of Tizimin which correlates 11 Chuen 18 Zac with February 25, 1544. This, however, he corrects to February 28, 1544, on the perfectly plausible grounds that the Indians, although well aware of their own year, were not sufficiently well acquainted with the Christian year at such an early date as 1544, and for that reason made an error of three days. With this correction the 11 Chuen 18 Zac date is brought into accord with his equation 12 Kan 1 Pop equals July 23, 1553. Thereby the Ahau equation 584281 is obtained.

However, this equation, as Long has pointed out,[2] makes 9.16.4.10.8 four days before a new moon, but this date according to the interpretation of the data followed here, and with which interpretation Martinez appears to agree, should fall on a new moon. The Maya date 9.16.4.10.8 reduces to M. D. 1412848, and adding to this the Martinez Ahau equation 584281, J. D. 1997129 is reached. Oppolzer gives an eclipse falling on J. D. 1997163, and the new moon prior to this would fall on J. D. 1997133 or 1997134. It is clear, then, that the Martinez equation is four or five days short at this point, and will be from three to five days short of all new moons recorded by the Maya.

It has been claimed that Stela 1 at Poco Uinic records an eclipse in the Goodman-Martinez correlation. The date in question reduces to M. D. 1425516, which with the Martinez equation 584281 becomes J. D. 2009797. According to the Oppolzer tables the eclipse, which was total in Central America, occurred on J. D. 2009802. It is clear that the Martinez equation shows an error of five days if this date is meant to record the eclipse in question.

Similarly 9.17.0.0.0, which Quirigua and Piedras Negras recorded as the date of a new moon, should reduce to new moon in the Martinez correlation, if the correlation is correct. This date with the addition of the Martinez equation becomes J. D. 2002681. Oppolzer gives 2002685 as the date of an eclipse.

According to the Martinez correlation there was a heliacal rising of Venus at about 9.9.9.16.15, whereas the Dresden Codex indicates that the heliacal rising took place at 9.9.9.16.0. However, as Teeple has pointed out,[3] there was an accumulated error of about four days at this date. That being so, the heliacal rising should have taken place at about 9.9.9.16.4. The error of about eleven days in the Venus count reached by the Martinez correlation is not particularly serious. If the second date, 10.15.4.2.0, 1 Ahau 18 Uo is considered to be a Venus base, the Martinez correlation still shows an error, as this date would then fall about three days before heliacal rising. With 9.9.9.16.0, 1 Ahau 18 Kayab placed

[1] Martinez Hernandez, 1926.
[2] Long, 1931.
[3] Teeple, 1925, pp. 402–406.

at heliacal setting, there would still be an error of about four days after allowing for the cumulative error.

Although the Martinez correlation shows many indications of being correct, it must be rejected if our postulate that 9.16.4.10.8 is a new moon date is correct.

11.16.0.0.0 (GOODMAN-THOMPSON CORRELATION)

This correlation, published early in 1927,[1] differs from that previously proposed by Martinez by only four days. It makes 12 Kan 1 Pop equal July 26, 1553. This gives an Ahau equation of 584284, but it was presumed that a break of a day occurred when the days lost one day in month position (*i. e.* 13 Ahau 2 Pop was used instead of 13 Ahau 3 Pop). In this way an equation of 584285 was obtained for the early dates. The correlation was reached by trying to reconcile the historical and apparent astronomical data. The result was the same as that subsequently reached by Teeple in his exhaustive analysis along the same lines.[2]

With the equation 584285, new moon dates are obtained for the new moons apparently recorded on the monuments and in the Dresden Codex. Although 9.16.4.10.8 (J. D. 1997133) is the date of a new moon, it is not the date of an eclipse such as one might expect at the start of the eclipse tables. This, however, is not necessarily an argument against the correlation. The Maya must have been preparing to use an eclipse table for some time prior to this date and, not possessing any other equipment than the naked eye, they could scarcely have been expected to predict every visible eclipse.

This correlation leads to a heliacal rising of Venus at about 9.9.9.16.11, whereas the table in the Dresden Codex appears to indicate that the heliacal rising took place at 9.9.9.16.0, but there was a three-day error at this time, and this would mean that the heliacal rising took place at about 9.9.9.16.8.

On the other hand certain deductions from these tables, discussed on page 64, suggest that the Venus year may have started and ended at heliacal settings. Should that have been the case, the date 9.9.9.16.0, 1 Ahau 18 Kayab plus three days (the cumulative error of between three and four days indicated by the eight day correction at the close of the cycle) plus 584285 reaches J. D. 1948648, which was the date of a heliacal setting of Venus.

It might appear that a difference of only four days between the two correlations is being unduly magnified, but for astronomical work a correlation that is in error only to the extent of a few days is as useless as one that is many years in error. The Martinez version does not agree with the lunar data and, therefore, must be rejected. I do not wish thereby to indicate that the correlation that I have sponsored is necessarily correct. I am very far from feeling that it is infallible, and have said so on many occasions.[3]

In the Martinez correlation no Katuns are excised from the lists in the various Chilam Balams, the count being thereby carried back to 8.7.0.0.0. In the Goodman-Thompson correlation thirteen Katuns are excised, following the arrangement

[1] Thompson, 1927.
[2] Teeple, 1930.
[3] Thompson, 1927, p. 3; 1930, p. 42; 1932, p. 373; Thompson, Pollock and Charlot, p. 148.

proposed by Solis Alcala. Neither of these possibilities, of course, makes any difference to the correlations, but they affect history. Long believes that the Goodman-Thompson equation should be accepted in preference to that of Martinez, but considers that Martinez' arrangement of the history is better than that followed in the other version.[1]

Ceramic evidence may, perhaps, fit the 11.3.0.0.0 correlation better than it does the 11.16.0.0.0 correlation, but, as indicated on page 69, the evidence is not so unfavorable to the 11.16.0.0.0 correlation as at first appears. Unpublished ceramic evidence from San José would appear to favor the 11.16.0.0.0 correlation, but at the time of writing the material from this site has not been fully analyzed, and it is not impossible that a more intensive study may lead to a reversal of this first impression.

The correlation is in agreement with Landa's typical year for 1553, and is in agreement with the important list of Tun endings on page 66 of the Chronicle of Oxkutzcab. It is within the limits set for the fall of the Katun 13 Ahau of the Conquest, and is in agreement with indirect references in the Chilam Balam of Chumayel.[2]

Although there is much to be said for the correlation, there are the few points discussed above, which are to a certain extent unfavorable. They are:

1. The correlation makes 9.9.9.16.0 about eleven days before a heliacal rising, although the Dresden Codex appears to indicate it should not be more than three days before this event, but if the Venus year was counted from heliacal setting, this objection falls to the ground.
2. It reaches new moon at 9.16.4.10.8, but this is not the date of an eclipse.
3. The ceramic evidence may indicate that this correlation makes the "Old Empire" too early.

11.16.0.0.0 (BEYER) CORRELATION

This, the latest attempt to correlate the two calendars, was published by Beyer in 1934.[3] It is based on a statement of Martinez[4] which was probably not meant in the way that Beyer appears to have taken it. The statement can be englished as follows:

"New moon occurred on January 15 [O. S.] and February 14 [1542], and on the latter date an eclipse of the sun, visible in Yucatan, took place. Discounting six hours for the longitude of Merida, it occurred and was visible at 3 o'clock in the afternoon. In the Maya calendar of the Books of Chilam Balam of Mani, Tizimin, and Kaua and on pages 50–58 of the lunar table of the Dresden Codex it occurred February 24, 11.16.2.6.7, 2 Manik 5 Ceh, year of 13 Kan."

From this statement Beyer obtains his equation 2 Manik 5 Ceh equals February 14, 1542 (O. S.) and from this one obtains the equation 12 Kan 1 Pop equalled July 13, 1553 (Gregorian).

[1] Long, 1931.
[2] Roys, R. L., 1934, p. 206.
[3] Beyer, 1934.
[4] Martinez Hernandez, 1928, p. 6.

There is a considerable amount of confusion with regard to this matter, but a little clear thinking will solve the problem. First the eclipse took place on February 14, 1542 (O. S.), and not on February 24, 1542 (O. S.), but the Chilam Balam calendars, if one follows the Martinez method of reconciliation equate 2 Manik 5 Ceh (Old Maya style) with February 24 (O. S.). Since Martinez claims that this calendar was written in or about the year 1542, the count must have been Julian. In that case 2 Manik 5 Ceh could not have been the date of an eclipse, which occurred ten days earlier. Martinez states that this difference shows that the Julian calendar was ten days in advance, whereas, of course, it was ten days behind Gregorian. Martinez was confused in a manner that is very understandable, converting a Julian date into Gregorian by subtraction instead of addition. It is easy when one is working with such material to make this slip. That it was no more than a slip is shown by the fact that Martinez does not base his correlation on 2 Manik 5 Ceh equals February 14 (O. S.), but uses the February 24 (O. S.) equivalent.

Since there is no statement in the Chilam Balams that an eclipse fell on 2 Manik 5 Ceh, it is clear what Martinez wished to say. He wished to say that 2 Manik 5 Ceh was the equivalent of February 24, 1542, and on that day an eclipse was visible in Yucatan according to Oppolzer (overlooking that the difference of ten days could not be accounted for as a change from Julian to Gregorian), and also calling attention to the fact that 2 Manik is one of the dates in the lunar tables in the Dresden Codex. However, he forgets that if the Dresden Codex eclipse table started at 9.16.4.10.8, as he has stated elsewhere, then by the time eight centuries had lapsed, and the count was around 11.16.0.0.0, 2 Manik would no longer be an eclipse date, as the cumulative error in the tables would have amounted to about four days. Furthermore, it would appear doubtful that the calendar recorded in these Chilam Balams actually represented A. D. 1542. The dubious association of days with month positions would suggest a considerably later date.

Beyer, overlooking these contradictions, advanced the new equation 2 Manik 5 Ceh equals February 14, 1542 (O. S.), as the basis of his new correlation.

Apart from the fact that the correlation is based on a false interpretation of data, the following objections can be advanced:

1. The Beyer correlation individually is not supported by a single statement in historical times, and is contradicted by the great mass of evidence indicating that 12 Kan 1 Pop fell on, or at the most six days before, July 26, 1553.

2. It makes the Maya lunar count start at full moon, but, as we have seen, this is extremely unlikely.

3. It makes the disagreement with the Venus data ten days greater than in the case of the Goodman-Martinez correlation already discussed.

4. The evidence of the moon glyphs at Piedras Negras, brought forward by Beyer in support of his thesis, works equally well for any correlation that is based on the lunar count having started from new moon. Indeed, the evidence is probably stronger for a count from new moon, since the prefix appears to be a variant of the Yax superfix, indicating new or fresh moon, and not the end of a moon as Beyer suggests. Indeed, Teeple

had described this very hieroglyph as indicating new moon in his unpublished notes, written several years ago.

Since, however, this correlation is based on a hasty misinterpretation of an unverified statement, the explanation of the facts are sufficient to lead us to discard it.

Since the above was written Beyer has withdrawn his correlation. However, as this new paper is to be published in a journal, the circulation of which among Maya students is much less than that of the Middle American Research Series of Tulane University, it has seemed advisable to retain this discussion in case any other student should continue to adhere to this correlation.

12.9.0.0.0, 13 AHAU 8 KANKIN CORRELATION

This correlation was originally proposed by Morley, who advanced arguments for placing A. D. 1536 in the Katun which ended on 12.9.0.0.0, 13 Ahau 8 Kankin.[1] As subsequently refined by Spinden,[2] it is in agreement with most of the historical data, although it disagrees with page 66 of the Chronicle of Oxkutzcab. It is in absolute agreement with the equation 12 Kan 1 Pop equals July 26, 1553, and makes the Katun 13 Ahau end in 1536. However, it makes 9.16.4.10.8 fall some ten days after a new moon instead of at new moon, as the dates on the monuments and in the Dresden Codex appear to require.

Spinden and Ludendorff argue that the Maya reckoned their lunations from full moon to full moon, and that the four-day interval from full moon, remaining even with this assumption, is due to the use of a formal computed lunar count which had accumulated an error of four days between the date of its inception in Cycle 7 and the period of the monuments.

Actually, as we have seen, there is evidence against a formal and uncorrected lunar calendar (p. 66), and there is no reason to assume its usage save the exigencies of this correlation. We have also seen (p. 67) that there is strong evidence against the lunar count having been made from full moon to full moon. Should one ignore the evidence against these two assumptions, it would be possible to fit any correlation to the lunar evidence, for there is no apparent reason for limiting the error to four days once one is prepared to ignore the evidence against the formal uncorrected lunar count. A maximum accumulated error of seven days in either direction would permit any conceivable correlation to be fitted to the lunar data, using either new or full moons as bases.

The Spinden correlation also disagrees with the heliacal rising or setting of Venus supposed to have taken place at 9.9.9.16.0, 1 Ahau 18 Kayab, according to the Dresden Codex. At that date the 12.9.0.0.0 correlation shows Venus to have been about as far from inferior conjunction as is possible. However, owing to the peculiar structure of the Maya calendar by which two Calendar Rounds equal 65 uncorrected Venus years, dates that show Venus to have been at inferior conjunction in one correlation will roughly coincide with a superior conjunction

[1] Morley, 1910.
[2] Spinden, 1924; 1928; 1930.

of Venus in another correlation which is 13 Katuns (actually 13.3.11.0) earlier or later, since this interval is 162½ uncorrected Venus years. As it stands, the Venus data from the Dresden Codex are definitely against the 12.9.0.0.0 correlation.

The ceramic evidence also strongly suggests that this correlation is incorrect. The Vaillant chart shows a long period, unrepresented by any pottery types, in this correlation, and, at the same time, no contemporaneity between the early Maya and the Highlands of Mexico.[1]

The architectural and artistic evidence, too, would not appear to favor this correlation. For instance, the St. Andrew's cross lattice adornment of façades was in use as early as 9.16.13.0.0 (A. D. 514 in the Spinden correlation) at Holactun, but it survived, apparently, at Chichen Itza until the start of the Nahua period (A. D. 1191 in the Spinden correlation), and probably at other cities of Northwest Yucatan.[2] However, it hardly seems likely that this motif, apparently of no particular religious significance, would have remained unchanged over a period of almost seven hundred years. Since it is improbable that the temple at Holactun was the first to employ this type of decoration, the period in the Spinden correlation would almost certainly exceed seven hundred years.

Other characteristics of the early period which, according to the 12.9.0.0.0 correlation, must have endured unchanged for an inordinately long time include flint tridents and manikin scepters. The first of these is found on Stela 30 at Naranjo and other cities of the southern area, and must date from before A. D. 455 in the Spinden correlation, while it also occurs on the frieze of the Temple of the Jaguars at Chichen Itza.[3] This late building must date from the Thirteenth Century at the earliest, so this apparently unimportant implement or ornament remained unchanged for some 800 years.

The manikin scepter is too well known to need any description, occurring as it does, in so many of the southern cities on monuments dated in Cycle 9. It occurs also on the frescoes of the Temple of the Chac Mol at Chichen Itza, a building dating from not earlier than the Thirteenth Century, and probably later. Strangely, the manikin scepter, after having varied considerably in the cities of the south, is found in the Chac Mol fresco in full classical style. Although one expects a religious emblem to vary little, it does not seem likely that there would be so little change in six hundred years, after the previous two hundred years had witnessed a marked development, and shown that the symbol was not preserved unchanged for religious reasons.

While none of these arguments based on art development is particularly strong, yet taken together they serve to add a little weight to a very strong case against the correlation in question. This case can be summarised under the following headings:

1. The correlation requires that the Maya count be made from full moon to full moon, but the evidence against such an assumption is very strong.

[1] Vaillant, 1935.
[2] Thompson, 1931, pp. 354–356.
[3] Blom and La Farge, p. 310.

2. It calls for a computed and uncorrected lunar count, but evidence has been given indicating strongly that corrections were made, and that a cumulative error could not, therefore, have existed.

3. It is in disagreement with the ceramic evidence presented by Vaillant.

4. It disagrees with the Venus data in the Dresden Codex.

5. It appears to disagree with the presumed development of art and architecture.

6. It places the end of Katun 13 Ahau in April 1536, but the best evidence indicates the Katun ended three to four years later.

7. It disagrees with the Chronicle of Oxkutzcab.

In view of these arguments and the refutation in Appendix I of astronomical claims made on its behalf, the correlation can not be accepted in the light of present knowledge.

13.2.0.0.0, 13 AHAU 3 ZOTZ CORRELATION

This correlation formerly received the support of Bowditch, Long and Joyce. Although perfectly feasible when first advocated, research in recent years has shown it to be no longer tenable. New evidence demands its rejection for the following reasons:

1. The date 9.16.4.10.8 falls in this correlation about twenty-one days after new moon.

2. The date 9.9.9.16.0, 1 Ahau 18 Kayab, indicated by the Dresden Codex as the day of a heliacal rising of Venus, proves to be about forty days after Heliacal rising in this correlation.

3. The correlation makes an impossibly long gap between the end of the so-called Old Empire and the Mexican period. The Vaillant charts show no pottery that could fill this gap.

4. The other arguments used against the gap made by the 12.9.0.0.0 correlation apply with much greater force to this correlation.

CONCLUSIONS

We have briefly reviewed the apparent pros and cons of the various correlations that agree with Sixteenth Century evidence as to the positions of the day and month signs in the European calendar, and the approximate time in our calendar when the Katun 13 Ahau of the Conquest fell.

It must be confessed that the results are not decisive. The Goodman-Thompson correlation appears to fare best, but even this has its drawbacks. So far as dependence on Sixteenth Century evidence is concerned, there we must let the matter rest for the present. Perhaps architectural or ceramic evidence will be sufficiently strong as a result of a few more years' research to set definite limits within which the correlation must lie. Vaillant's ceramic evidence, indeed, points to a correlation in the neighborhood of 11.3.0.0.0, but until such external evidence is more definite, it does not seem advisable to ignore the strong evidence already presented against this correlation involving, as it does, the rejection of the 12 Kan-July 26 correlation and 1 Pop-July 26 correlation, not to mention the Katun evidence and that of the Venus tables in the Dresden Codex.

Certain correlations have been suggested apart from those discussed above. These are based only on astronomical evidence in the inscriptions, and entirely ignore the post-Spanish data. However, we have seen that this is in many ways extraordinarily strong. Particularly is this the case with regard to the correlation of our calendar with the various 260-day counts of Central America. We have seen that the 260-day counts of the Aztecs, Yucatecan Maya, Quiché, Cakchiquel, and Jacaltecas were in conformity with a maximum error of two days. We also know that in the case of the Quiché and Jacalteca counts, there is only a single day's error between them and the Yucatecan Maya count recorded by Landa in the middle of the Sixteenth Century.

The ancient mechanism has withstood close on four centuries of Spanish domination and clerical opprobrium. Driven underground, it has obstinately continued to function to the present time without the loss of a single day, and, like the giant lizards of Komodo, has proved to be a hardy survivor of a past age. Is it credible that the sacred count should have broken down at some time prior to the arrival of the Spaniards and yet have been capable of withstanding unchanged nearly four centuries of alien contact?

History tells us of the disturbances that followed the introduction of the Gregorian calendar in England, yet the calendar played a relatively unimportant part in the life of Eighteenth Century England. In Middle America, where life from birth to death was regulated by the 260-day count, it is almost inconceivable that such a change could have been made voluntarily. Furthermore, one can be sure that such an upheaval of the whole mechanism of divine guidance would have encountered the strongest opposition from the extremely powerful priest-astronomer class.[1]

The only logical explanation of a break in the Maya day count would be based on the assumption that the Maya and Mexican 260-day counts were at some time out of conformity with each other, and that the Maya counts were changed so as to bring them into conformity with that functioning in the Highlands of Mexico. Since, however, the Mexican and Maya counts, must, presumably, have had a common origin, why should one expect either group to have slipped cogs? The unchanging record of the modern counts argues against this. Furthermore, granting that the Yucatecan, Quiché and Cakchiquel 260-day counts were altered to bring them into conformity with the Mexican, why was the Jacalteca also changed? So far as is known, Mexican influences were almost negligible in that remote region. Furthermore, had the Yucatecan 260-day count been altered to make it conform with the Aztecan Tonalamatl, is it not very probable that the Yucatecan 365-day count would have been similarly and simultaneously shifted so as to make 1 Pop conform with the start of the first Aztecan month. Such was not the case, the starts of the two years at the time of the Conquest did not coincide. That the 260-day count and the 365-day year were fundamental is apparently shown by the fact that they occur in all known Middle American systems, but when we come to a long count, we find the Aztecs had none, the Cakchiquels possessed

[1] Afghanistan supplies an interesting example of the results of the opposition of clerical conservatism to a modernization program.

one of four hundred days, while the "Old Empire" and the rest of Yucatan had the 360-day count. This would suggest that the long count was introduced at a later date, and never spread so widely as the earlier 260-day and 365-day counts.[1]

It is possible that there was a break between the end of the epoch of the monuments and the arrival of the Spaniards, but overwhelming evidence from other sources, such as astronomy, is necessary before the proponents of any correlation that does not make 12 Kan fall on or very close to July 26, 1553, can off-hand reject this and similar lines of evidence. The *onus refellendi* clearly lies with them.

The possibilities of obtaining a correct correlation by relying only on material in the inscriptions do not seem so bright as they did a few years ago. The examination of Ludendorff's findings shows how easily coincidences can be mistaken for proofs. For example, the Venus glyphs appear to be so self-contradictory, that, by taking certain examples and ignoring others, one can produce the results one wants, and, if required, it is possible to make 10.0.0.0.0 coincide with the birth of Caractacus or the death of the Duke of Clarence in the butt of Malmsey wine. Similarly one can get certain agreements out of the Mars tables by careful selection.

It is not improbable that the Maya made no record of eclipses, planets, equinoxes, solstices or suns overhead except when these phenomena happened to coincide with dates which they wished to record for some other purpose.

In the light of present evidence an open verdict must be returned. Certain correlations appear to have been ruled out, but with so many possibilities an indisputably correct solution can not be reached by the process of elimination. In discussions of Maya archæology, it would be best to avoid as far as possible the use of dates in our calendar, and where this is not possible, let each writer use the correlation, for which he has a predilection, but with a clear statement that such dating is provisional.

If an indisputably correct correlation is ever reached, it will be largely due to the careful drawings of the glyphs of the Lunar Series collected by Morley in the course of many seasons of bush wanderings under the most trying conditions from one end of the Maya area to the other. Without this arduous preliminary work Teeple could not have established the meanings of Glyphs C, D and E of the Lunar Series, other advances could not have been made, and one of the most important checks on the correlation question would not be known.

[1] Readers are referred to the excellent paper by Oliver La Farge, *Post-Columbian Dates and the Maya Correlation Problem.* Unfortunately this was published too late to be discussed in this paper.

Appendix I

THE ASTRONOMICAL APPROACH

By J. Eric Thompson

In recent years Professor Ludendorff has enumerated many Maya dates which develop astronomical importance in the Spinden correlation, which he supports. At first glance these astronomical findings appear very convincing, but an analysis of some of them has such unfavorable results as to cause one to cast doubt on the mass of his interpretations.

In paper 6 of his series[1] he examines a number of stelæ which, he believes, deal with astronomical matters. These inscriptions were not picked haphazard, but were chosen as likely to yield the best astronomical results after a preliminary survey of the Julian equivalents in the Spinden correlation of all Maya dates. The Falernian fields that yielded the best harvest were situated at Copan and Naranjo.

Below are listed the dates at Copan chosen by Ludendorff together with the most important resultant findings of an astronomical nature when these dates are converted into the Julian calendar by means of the Spinden equation. To simplify the matter, convergences or conjunctions of planets with individual stars or with constellations as well as cases where planets return to the same or approximately the same longitudes as have been reached at earlier dates are not listed, although noted by Ludendorff. An examination of this material will show that the deductions to be made from Ludendorff's conclusions would apply equally well to these less important heavenly occurrences.

Monument and Date No.	Reading	Anticipatory Comment	Astronomical Data
UI	9.14.19. 5. 0.	Wrongly Read	Convergence of Venus and Mars. Both planets near vernal point.
UII	9.15. 0. 0. 0.	Tun-Ending	Jupiter almost at summer solstice. Jupiter at trine to sun.
UIII	9.15. 8.10.12	Wrongly Read	Mercury-Mars conjunction 3 days later. Saturn close to autumnal point. Saturn opp. to Mars 2 days later. Saturn opp. to Mercury 3 days later.
UIV	9.15. 9. 0. 2	" "	Lunar eclipse 1 day earlier. Jupiter opp. to sun 1 day earlier. Convergence of moon and Jupiter. Mars almost trine to sun.
UV	9.15. 9.10.17	" "	Close convergence of Mercury and Jupiter 1 day earlier. Mars trine to Mercury. Saturn opp. to sun 5 days earlier.
UVIa b c	9.15.12. 5. 0 9.15.12. 5. 7 9.15.12. 5.17	" "	{Mercury at eastern elongation from sun. {Convergence of Venus and Mars. Venus at greatest brightness. Jupiter at point of summer solstice. Jupiter opp. to sun.
UVII	9.16.12. 5.17	Correctly Read	Nothing of astronomical importance.

[1] Ludendorff, 1933.

Monument and Date No.	Reading	Anticipatory Comment	Astronomical Data
1I	9.11.19. 5. 0	Wrongly Read	Mercury at superior conjunction 2 days later.
1II	9.11.19.15. 8	" "	Venus and Saturn in very close convergence.
1III	9.12. 3.14. 0	Correctly Read	Saturn in quad. to Mars 1 day earlier. Jupiter at trine to sun 1 day earlier. Jupiter at autumnal point.
1IV	9.12. 5. 0. 0.	Tun-Ending	Saturn in conj. with sun 3 days later. Jupiter in quad. to Venus.
1V	9. 7.19.17.11?	Dubious	No astronomical importance. Date doubtful.
1VI	9.13. 0. 0. 0.	Tun-Ending	Saturn in opp. to Mars 2 days later. Saturn in quad. to Mercury.

There are, therefore, a total of fourteen dates discussed after discarding the fifth date of Stela 1, which is queried by Morley and produces nothing of astronomical importance. It must be admitted that these dates yield an amazing amount of astronomical occurrences, such as one might, at first glance, consider to be very strong evidence indeed in favor of the Spinden correlation, and their interpreter appears a veritable Cincinnatus turning defeat into victory.

However, Ludendorff unwittingly used published data which had been wrongly interpreted. Elsewhere I have shown that all the Maya dates from Altar U, as used by Ludendorff, are wrong with the exception of Date UVII and, possibly date UII.[1] Morley, to whom I submitted that paper before its publication, agrees that the dates must be moved up one Calendar Round as in that paper. In view of this change of the original datings, practically all of Ludendorff's astronomical findings for Altar U must be purely the result of chance. Indeed, the only non-Tun ending date that is correct—the date 9.16.12.5.17, 6 Caban 10 Mol—is the only date to which Ludendorff fails to ascribe astronomical importance.

There can be little doubt that the first and second dates of Stela 1 have also been wrongly read. Each of them is followed by Glyph G of the Lunar Series.

The first date reads: 10 Ahau 13 ?, Glyph G ninth form. Now the date 9.11.19.5.0, 10 Ahau 13 Ceh, as used by Ludendorff, calls for the first form of Glyph G, not the ninth form as recorded. The date recorded must fall at the end of a Tun or at the end of 9 Uinals to agree with the recorded form of Glyph G. Furthermore, the date is followed by a statement which declares the end of a Katun, the coefficient of which is 6, 7 or 8.

Before proceeding further, let us examine the eroded month glyph. This is clearly Chen, Yax, Zac or Ceh. The prefix under a magnifying glass shows a rounded semi-circular outline, although part of this is gone. However, it is sufficient to show that it could not be that of Ceh, for the nicks, typical of Ceh, are absent. Neither could it be the prefix (superfix) of Zac. Its outline indicates Chen, or a less usual form of Yax. A close examination of the interior of the glyph reveals cross-hatching. This cross-hatching, of course, is indicative of Chen, when associated with the given main body of the glyph.

The only Katun with a coefficient of 6, 7 or 8 to end on 10 Ahau, and one of these four months with a coefficient of 13 between 4 Ahau 8 Cumhu and the close

[1] Thompson, 1935.

of the following Cycle 13 is 8.6.0.0.0, 10 Ahau 13 Chen. This date answers all the requirements. It is the best reading of the month glyph; it conforms with the required form of Glyph G, and, as required, it terminates a Katun 6, 7 or 8.

Although the position 8.6.0.0.0, 10 Ahau 13 Chen is open to a little doubt because it falls at such an early date, yet it is in all probability correct, while it is very clear that the reading followed by Ludendorff is wrong, since, as already pointed out, it fails to agree with the form of Glyph G, does not mark the end of a Katun, and, in all probability, the month sign is not Ceh. Date 1, II, read by Ludendorff as 9.11.19.15.8, 10 Lamat 16 Zotz, must also be wrong, as it is connected with the previous date with a Secondary Series of 10.8. Furthermore, it is followed by the first form of Glyph G. It must be remembered that Morley, whose readings Ludendorff follows here, deciphered these dates before the meaning of Glyph G was known. With our present knowledge of Glyph G as a check, we know that 9.11.19.15.8 can not be correct, as this position calls for the second form of Glyph G. However, the following reading for this passage fulfills all the requirements:

8.6.0. 0.0, 10 Ahau 13 Chen. Glyph G, first form. End of Katun 6
10.8

8.6.0.10.8, 10 Lamat 16 Pop. Glyph G, second form.

Let us now list in three groups the astronomical findings of Ludendorff for these two monuments. The groups record those astronomical occurrences that fell respectively on correctly interpreted dates, incorrectly interpreted dates, and Tun-ending dates.

Subject	Right	Wrong	Tun-endings
Ludendorff's readings	2	9	3
Percentages of above	14.29 *p. ct.*	64.29 *p. ct.*	21.42 *p. ct.*
Conjunctions with Sun	0	1	1
Oppositions to Sun	0	3	0
Lunar eclipses	0	1	0
Inter-planetary oppositions	0	2	1
Inter-planetary conjunctions or close convergences	0	6	0
Venus at greatest brilliance	0	1	0
Quadrature of planets	1	0	2
Vernal, autumnal points, etc.	1	4	1
Trine	1	2	1
Eastern elongation	0	1	0
Total	3	21	6
Percentages of Total	10 *p. ct.*	70 *p. ct.*	20 *p. ct.*

An examination of the percentages is revealing. It shows that the dates that are wrong have a higher percentage of the astronomical phenomena than the dates that are correctly read. Furthermore astronomical occurrences are higher on Tun-ending dates than on correctly read non-Tun-ending dates. Since the former were, presumably, chosen for non-astronomical reasons, one would hardly expect to find more than a few astronomical coincidences. The researches of

Morley and others have shown without the slightest doubt that monuments were erected to mark even dates, and these dates would have been commemorated whether the sky was a traffic jam of planets or perfectly devoid of astronomical interest.

Let us now turn to the three stelæ at Naranjo, of which Ludendorff treats in the same paper. These monuments produce the following astronomical phenomena in the Spinden correlation according to Ludendorff's findings:

Monument and Date No.	Reading	Anticipatory Comment	Astronomical data
12, I	9.17. 0. 0.12	Wrongly Read	Mercury inf. conj. 2 d. earlier. Saturn opp. 4 d. later. Venus near vernal point. Mars and Venus in quad.
12, II a	9.18. 8. 8.12	Correctly Read	Venus becomes visible in west.
b	9.18. 8. 8.16	" "	Mars in quad. to Saturn.
c	9.18. 8. 8.18	" "	Mars at summer solstice point. Saturn near autumnal point.
12, III	9.18. 8.11.11	" "	Mercury-Venus conj. 2 d. later. Mercury greatest elong. 3 d. earlier. Saturn near autumnal point.
12, IV	9.18. 8.16. 2	" "	Mercury-Saturn conj. 2 d. earlier.
12, V	9.18. 9. 0.13	" "	Nothing of importance.
12, VI	9.18. 9. 9. 8	" "	Mercury in west after conj. Venus-Jupiter conjunction 4 d. later.
12, VII	9.18. 9.13.15	" "	Venus-Jupiter in quad. 2 d. later. Venus-Saturn in quad. 2 d. later. Jupiter and Saturn in opp.
12, VIII	9.18.10. 0. 0	Tun-Ending	Venus conj. 1 d. later. Mars at vernal point Saturn in conj. 6 d. earlier
13, I	9.17.10. 0. 0	" "	Mercury conj. Jupiter opp. Saturn conj. 5 d. earlier.
14, I	9.17. 0. 0. 0	Wrongly Read	Mercury-Saturn opp. Venus inf. conj. 1 d. earlier. Jupiter opp. 5 d. later.
14, II	9.17.13. 4. 3	Correctly Read	Mercury inf. conj. 2 d. earlier. Mars at vernal point.
14, III	9.18. 0. 0. 0	Tun-Ending	Mercury inf. conj. 4 d. earlier. Jupiter opp. 6 days earlier. Saturn opp. 2 d. earlier.

Actually Date 12, I has certainly been wrongly read. Although Maler's photograph is not very clear, there is no doubt that the month sign at B1 is not Pop. One can distinguish the crossed bands indicative of the month signs Uo or Zip, while none of the features of Pop are distinguishable. Hermann Beyer, whose opinion I asked on this matter, shares these views. In a letter to me dated September 17, 1934, he announces his certainty that the month glyph can not be Pop, but must be Uo or Zip. At the same time he suggests a very plausible correction of the first Secondary Series to connect it with an opening date 13 Eb 5 Zip, which, as he points out, occurs on Stela 10 at this same site. In my opinion there is no doubt that he is right.

The date 14, I does not occur on that monument according to verbal information supplied by Doctor Morley. There are, therefore, two dates wrongly deciphered in this series. The whole series can again be classified in three groups according to whether they have been incorrectly deciphered, or, if correctly read, whether they fall on Tun- or non-Tun-endings. Such a classification is given below:

Subject	Right	Wrong	Tun-Endings
Ludendorff's Readings	9	2	3
Percentage of above	64.29 *p. ct.*	14.28 *p. ct.*	21.43 *p. ct.*
Conjunctions with sun	1	2	5
Oppositions to sun	0	2	3
Interplanetary oppositions	1	1	0
Interplanetary conjunctions or close convergences	3	0	0
Quadratures of planets	4	0	0
Vernal, autumnal points, etc.	4	1	1
Eastern elongation	1	0	0
Planets visible after conjunction	2	0	0
Total	16	6	9
Percentages of total	51.61	19.36	29.03

An examination of the percentage figures produces some interesting results. The Tun-ending dates and the column for wrong dates produce higher percentages of the total of astronomical occurrences than their totals of dates call for. On the other hand the correctly deciphered non-Tun-ending dates form 64.29 per cent of the total number of dates involved here, but the astronomical occurrences that occur on these dates are only 51.61 per cent of the total of astronomical occurrences. Thus the Tun-ending dates and the wrongly deciphered dates produce proportionally a greater number of astronomical occurrences when the Spinden equation is applied than the dates correctly deciphered. Furthermore, these results are obtained by giving every astronomical occurrence the same value, whereas in actual fact the Tun-ending and wrongly deciphered dates produce astronomical phenomena of greater importance, such as solar conjunctions and oppositions (twelve out of a total of thirteen). On the other hand, the total of sixteen astronomical occurrences on the rightly deciphered dates is largely formed by interplanetary conjunctions, quadratures and autumnal, solsticial and vernal points. These are of less importance, and it is doubtful if the Maya were interested in such phenomena. For instance, the opposition of one planet to another would not be a visible phenomenon.

In a similar manner it can be shown that many of the supposed recurrences of planets at the same positions as were recorded on previous dates, both at Copan and Naranjo, must be ignored, for in many cases one or both of the dates involved in the equation has been wrongly read.

The same can be said of some of the supposed records of the sidereal revolutions of the planets. In this connection it should be noted that Ludendorff has never offered an explanation as to how the Maya could have learned the sidereal revolutions of the planets. It would be extremely difficult to acquire such knowledge without a realization of the fact that the earth and planets revolved around the sun. It is quite clear, however, that the Maya never attained this knowledge.

Redfield and Villa have recently published their study of the modern village of Chankom in Yucatan. They record that the old Maya belief that eclipses were caused by the devouring of the sun or moon by a monster still prevails.[1] Such beliefs are not consonant with a grasp of the Copernician theory.

It might, of course, be argued that the Chankom beliefs represent the survival of the superstitions of the common folk as opposed to the knowledge of the priesthood. However, a passage translated from the Chilam Balam of Chumayel by Roys clearly indicates the same ancient Maya belief.[2] There, in a section headed by a cross, which Roys notes is indicative of a European text, the writer gives the European explanation of the cause of eclipses together with illustrative diagrams, and calls attention to the fact that the old belief that the phenomenon was caused by a devouring animal is not correct. This passage, as Roys points out, can be dated as having been written before the introduction of the Gregorian count into Yucatan. The author, therefore, must have been one of the early group of educated Maya who were taught to read and write. It is, accordingly, almost certain that he was a member of the old aristocracy, and as such was conversant with the beliefs of the Maya intelligentsia as to the cause of eclipses. His statement shows that these were the same as those of the common people, and that they did not include any belief that could be reconciled with a grasp of the fact that the planets revolved about the sun.

Even if the Maya had grasped this fact, they were still a very long way from being able to measure the sidereal revolutions of the planets. Long, on page 98, expresses a similar scepticism in this level having been reached by the Maya, and L. Roys in letters to me is of the same opinion (Appendix II).

Actually, as has been noted above, a number of the supposed records in the Maya inscriptions of sidereal revolutions of the planets can be rejected owing to dates, on which they were based, having been erroneously read.

No Maya astronomer could be expected to remember the long lists of astronomical phenomena deduced by Ludendorff without glyphs to jog his memory, but, so far as one can tell, there are no glyphs for the planets associated with the cited dates. According to Ludendorff, Venus figures in many of these calculations, but the glyph is not recorded.

In the compass of this paper it is not possible to review every astronomical finding of Ludendorff, but enough has been cited to cast a considerable cloud of doubt on Ludendorff's findings being more than the result of coincidence. After all, there are few days in the calendar entirely devoid of astronomical interest, particularly when a margin of four or five days on each side of an occurrence is allowed. Within these limits about one day in four will mark the conjunction or opposition of some planet with the sun, while if quadratures and other phenomena recorded by Ludendorff are counted, permitting a similar margin of error, there would be on the average more than one occurrence of astronomical interest for every night in the year.

[1] Redfield and Villa, p. 206.
[2] R. L. Roys, 1933, pp. 86–88.

It would seem, then, that Ludendorff has not established the correctness of the Spinden correlation, but, unwittingly, has shown that astronomical phenomena are worthless as proofs of correlations unless accompanied by glyphs indicating the nature of the phenomenon. Even with this last safeguard, the evidence is not certain, for, as has been pointed out on page 78, the Venus glyphs attached to dates can be used as supporting evidence for more than one correlation.

Spinden, however, brings forward one glyph as evidence for his correlation. This glyph, he states, represents the equinoctial dates.[1] Let us briefly examine the dates with which this glyph, together with the various variants cited by Spinden, has been used. In the following table are listed the dates with which the glyphs are found, so far as I have been able to discover them. The dates in our calendar represent the equivalents of these dates in the Gregorian calendar according to the Spinden correlation.

City	Monument	Date	Gregorian
Palenque	T. of Fol. Cross	9.12.18. 5.16, 2 Cib 14 Mol	September 23
Palenque	T. of the Sun	9.12.18. 5.16, 2 Cib 14 Mol	September 23
Palenque	T. of the Sun	9.10.10. 0. 0, 13 Ahau 18 Kankin	July 29
		or	
		9.10. 8. 9. 3, 9 Akbal 6 Xul	August 18
Palenque	T. of the Sun	1.18. 6. 4.17, 9 Manik 10 Tzec	July 29
Palenque	Death Head	9.13, 0. 0. 0, 8 Ahau 8 Uo	May 19
		or	
		9.12.19.14.12, 5 Eb 5 Kayab	March 12
Piedras Negras	Lintel 3	9.15.18. 3.15, 7 Men 18 Chen	October 2
Piedras Negras	Throne	9.17.10. 6. 1, 3 Imix 4 Zotz	June 3
Yaxchilan	Lintel 15	4 ? 12 Zip	May 25—June 10
		or	
		4 ? 12 Uo	May 5—May 21

Only one date of the seven with which this glyph occurs has an equinoctial significance in the Spinden correlation. There is, therefore, little justification for the belief that this glyph represents the equinox, even should the correlation be correct. For the same reason the glyph is of no value as an argument in favor of the correlation in question.

An examination of the occurrences of the bound-up moon glyph, which Spinden declares to be an eclipse glyph, produces similar results. An occasional hit, but more frequent misses, can not be cited as evidence for the correct interpretation of a glyph or as evidence for the correctness of a correlation.

A passage that might well prove of significance in the correlation problem occurs on the so-called re-used lintel at Naranjo. The inscription reads:

9. 7.14.10. 8, 3 Lamat 16 Uo
2. 5. 7.12

9.10. 0. 0. 0, 1 Ahau 8 Kayab

The interval of 16352 days between the two dates is exactly twenty-eight synodical revolutions of Venus of 584 days (actually 28x583.92 = 16350 days), while forty-one synodical revolutions of Jupiter amount to 16354 days. There is

[1] H. J. Spinden, 1924, pp. 151–152.

a Venus glyph associated with the calculation as well as a glyph, like a dog's head, which might be the glyph for Jupiter. Furthermore the first day is Lamat, the sacred day of the planet Venus, and the second day is 1 Ahau of great ritualistic importance in the Venus count.

The calculation suggests that the two planets might have been in conjunction on both dates. Doctor Philip Fox of the Adler Planetarium was so kind as to calculate for me the positions of these planets on these dates in the 11.16.0.0.0 and 12.9.0.0.0 correlations. Without taking into account the eccentricities of the orbits, he finds that on the Julian positions corresponding to the 11.16.0.0.0 correlation there was an interval of roughly twenty degrees between the planets on both occasions. He suggests that the fact that at sunrise the Sun would have been on the horizon, Jupiter 20° above the horizon and Venus 40° above the horizon might be significant. However, so far as we know, angles of 20° and their multiples had no significance for the Maya unless, possibly, the time intervals between the appearances of the planets could have had some significance. If the Maya divided their nights into nine "hours," one "hour," calculating the night as twelve of our hours (*i.e.* from 6 p. m. to 6 a. m.), would have been eighty of our minutes, which would be 20°. In that case Venus rose at the seventh "hour," Jupiter at the eighth "hour" and the sun at the ninth "hour."

Seler believed that among the Mexicans the night was divided into nine "hours," but his views on the matter appear to have been influenced by his belief that the Lords of the Nights ruled over the consecutive "hours" of the night. It is not at all impossible that the night was divided into nine "hours" corresponding to the nine underworlds. The Maya and Mexicans must have had some means of dividing the night, and a division of nine is the most logical. This explanation of the phenomena is merely offered as a new line of approach, which might prove fruitful.

On the two dates cited the planets do not occupy significant positions in the 12.9.0.0.0 correlation. Jupiter is about 42° west of the sun, and is, therefore, a morning star, while Venus is about 30° east of the sun, and is, therefore, an evening star. Rough calculations made by me show that the positions for the planets on these days in the 10.10.0.0.0 and 11.3.0.0.0 correlations appear to have no significance.

Another calculation on the Hieroglyphic Stairway at Naranjo involving 9.10.0.0.0 is perhaps of significance. On Inscription 6 occurs:

9. 9.18.16. 3, 7 Akbal 16 Muan
1. 1.17
9.10. 0. 0. 0, 1 Ahau 8 Kayab

The Venus glyph is associated with the first date, and the interval between the two dates is 397 days, which is less than two days less than a synodical revolution of Jupiter.

At 7 Akbal 16 Muan, Venus according to the 11.16.0.0.0 correlation is less than 5° east of the Sun, just a few days after superior conjunction, while Jupiter is at approximately the same position as at the last date. That is to say the planet is just about 20° west of the Sun, rising some eighty minutes (1 Maya "hour"?) before the Sun.

In the 12.9.0.0.0 correlation, as Spinden has pointed out, Venus is at inferior conjunction on 7 Akbal 16 Muan. Jupiter would have been about 42° west of the Sun, as at 9.10.0.0.0.

Any correlation, deduced on other grounds, that makes 9.10.0.0.0 coincide with a conjunction of Venus and Jupiter would have much to recommend it.

There are few Venus glyphs in the inscriptions after discarding the occurrences of the glyph in the Introductory Glyph of the month Yax, for these have no astronomical importance.[1] A few of the Venus glyphs associated with dates, such as 1 Ahau, and possibly days with coefficients of seven, may also have merely a ritualistic significance. In that case it should be a fairly simple task to plot the positions of the various planets in all proposed correlations for these dates.

[1] Beyer, 1931.

Appendix II

MAYA PLANETARY OBSERVATIONS

By Lawrence Roys

In his *Astronomical Notes on the Maya Codices*, Robert W. Willson, the Harvard astronomer, opens with a delightful essay on primitive astronomy. The movements of the planets seemed indeed complicated until the simple picture drawn by Copernicus (Sixteenth Century) was thoroughly appreciated. Before that time it is certain that the more conspicuous phenomena received the most attention, and it is quite safe to say that eclipses, conjunctions and heliacal risings and settings ranked first (after the commonplace movements of the Sun and Moon). After these were recorded, it would seem that positions of the planets in the zodiac, meridian passages, oppositions and a number of minor relationships such as quadratures began to be observed with interest. It was only with records of these observations as a background that early astronomers and philosophers began to theorize coherently regarding the scheme of the universe.

The Dresden Codex records remarkably accurate knowledge of the synodic periods of the Moon and Venus. Visual observation would give this knowledge to any people with an accurate written calendar and enough interest to preserve the records. The Maya possessed these things and were well versed in other parts of the field of observational astronomy, and consequently we are inclined to ascribe to them similar knowledge of the synodic periods of all the visible planets and any other knowledge of heavenly phenomena that is obtainable with the same equipment. They even kept track of eclipses in a way that enabled them to predict possible eclipse dates, but this ability also is easily explained by intelligent use of the equipment just mentioned. In short, it is well proved that the Maya were far advanced in the field of observational astronomy, but still there is little or no evidence indicating that they had entered within the borders of any such theoretical science or philosophical thought analogous to that of Thales and his successors in Greek astronomy.

Mr. Thompson has suggested to me that I discuss the question as to whether the Maya could have known the sidereal periods of the planets as well as their synodic periods. It seems very improbable that they knew these sidereal periods, but a general denial is hardly in order where there is no direct Maya evidence on the subject. However, the difficulties of obtaining the astronomical records necessary for these determinations are so great, and the mathematical logic so advanced, that they appear too difficult for a people in the Maya stage of civilization, and the burden of showing a reasonably simple way of finding sidereal periods lies on anyone who suggests that they were known to the Maya.

Young's *Astronomy* defines *sidereal period* as "the time of its revolution around the Sun, from a fixed star to the same fixed star again, as seen from the Sun." The same phenomenon viewed from Earth moving around a large orbit appears in considerably distorted form and is not easy to measure accurately even with modern instruments. Young's *Astronomy* says "the sidereal period can not be directly determined from our observations, but the synodic period can, for it is the interval between successive conjunctions or oppositions," and then gives the algebraic formula for computing the sidereal period from the synodic period. Presumably the Maya knew no algebra.

Our modern grasp of the mechanics of the solar system starts with a picture of the concentric orbits of the planets drawn to scale with the Sun at or very near the center. With this simple conception as a mental background, it is not easy to realize how peculiar the movements of the planets would appear to a pre-Copernican astronomer. Rather than generalize, I will review as briefly as possible the patterns of apparent movement of the planets (there are only five of them visible to us) and the reader can judge whether or not their sidereal periods were determinable by the Maya.

JUPITER

As the brightest planet that makes the entire circuit of the heavens consistently from west to east, Jupiter must have commanded especial attention very early in the history of all astronomies. As soon as the zodiac was recognized as the highway of the planets, it would have been natural to try to determine the period of Jupiter in this circuit. A student can hardly help asking: Why can not a fairly close approximation of the period be found by taking some bright star lying close to the ecliptic, such as Aldebaran, as a zero point and recording a number of successive passages of Jupiter past the star, eliminating from the record all retrograde passages and the re-direct passages after retrograde motion?

By referring to the file of the *American Ephemeris and Nautical Almanac*, this may be done for our present lifetime. When Jupiter passed Aldebaran in 1882, the Sun probably blinded our view of the conjunction, but commencing with the following visible conjunction of May 15, 1894, we find conjunctions at the following intervals: 4367 days, 4134 days, 4356 days. *The true sidereal period is 4332.6 days.* The errors in the three observed periods are 35, 198 and 24 days, respectively. Even if the three observed periods are averaged, the result, 4286 days, is 46 days from the truth. To puzzle the observer further, the periods of conjunction with Aldebaran are quite different from the corresponding periods when other stars are taken as the zero point. By using the star A Tauri, only 9 degrees westward, we find the following periods: 4128 days, 4360 days and 4365 days, respectively. By selecting stars in other parts of the zodiac we get still other variations.

A period of about 4360 days (11.93 years) might have been recognizable, but sometimes the observed period was nearly eight months shorter than that, and sometimes about double when the Sun's position made observation of a conjunction impossible. For the Maya to have discovered the true sidereal period seems very far from likely. The problem is further clouded by the additional conjunctions that occur when the zero point happens to be included in the retrograde part of the planet's cycle.

MARS

January 23, 1896, closed a series of visible conjunctions of Mars with a certain star in Sagittarius lying practically on the eighteenth hour circle. The next two conjunctions with this zero point were unobservable because of the nearness of the Sun, but the third was probably observable in the dusk. Commencing January 23, 1896, the intervals between conjunctions were 2131, 709, 705, 540, 695 and 706 closing the series. Following this series was an interval of nearly eight years during which the three conjunctions occurring therein were unobservable. No retrograde motion occurred at hour circle eighteen to complicate this series.

To find a series complicated by retrograde motion, we can select as zero point the star *o* Virginis lying at hour circle twelve. No conjunction was observable for nearly six years. Then commencing August 25, 1897, with a visible conjunction, appear the intervals, 710, 707, 523, 162, 540, 709 followed by eight years with no observations possible. In the preceding series the retrograde conjunction is omitted, but the re-direct conjunction is included. If the latter also is omitted, the series become, 710, 707, 523, 702, 709, not unlike the series in the preceding paragraph.

Intervals of about 705 days predominate, with a central interval of only three-quarters of that length. Such a series does not suggest averaging, but even the average for it is little closer to the *true sidereal period of 687 days* than the predominating figures ranging over 700. I can see no invitation here for the Maya astronomer to average, and I believe that a considerable burden of proof rests upon anyone who suggests that this was done.

It should be stated that we can get a figure of 685 plus by adding the three non-observable intervals to the five usually observed, and dividing by eight. That the Maya had any reason for exploiting this possibility seems very questionable.

SATURN

Saturn's sidereal period is 10,759.2 days, which is 29.46 years. At the close of a complete sidereal revolution of the planet, Earth has made 29 complete revolutions plus a half revolution which brings it to the opposite side of its orbit. This last feature makes it difficult even to approximate by observational methods the figure 10,759 within a lifetime. If the zero observation is made when the planet is near opposition, the completion of the period occurs when the Sun is between Earth and Saturn; and only the double period of 59 years may be observed. It would take several successive observations of this double period to get it at all accurately, and this might run into centuries of time.

The single apparent period of Saturn may be observed if the zero observation is near quadrature. However, the same feature of the extra half revolution complicates matters. When Saturn has exactly completed its revolution, Earth is 180 million miles from its original position, at an angle that badly distorts the observation. This distortion introduces an error of 6 to 12 degrees, and consequently the apparent period is affected. For instance Saturn was in visible conjunction with the star in Sagittarius lying on the eighteenth-hour angle on January 21, 1900, and again in visible conjunction with that star 10,646 days later. Here we have a variation of 113 days from the true period. Using the star on the fourteenth-hour angle, I found a pair of visible conjunctions 10,642 days apart.

My file of the *American Ephemeris* does not cover time enough to study successive periods for Saturn, but drawings made to scale indicate that it would take centuries of crude averaging to obtain its sidereal period or, as an alternative, several lifetimes of very intelligent averaging. This may have been within the grasp of the Maya, but I seriously doubt it.

VENUS

Unlike the three preceding planets, the orbit of Venus is smaller and lies entirely within that of Earth. It is mechanically impossible for Venus to make the circuit of the zodiac except as an appendage of the Sun. Its movement through that circuit is really that of the Sun, badly distorted it is true, but primarily that of the Sun. It can only appear at celestial positions within 48 degrees of the Sun's position at the same time.

With such a mechanical layout, it seems impossible that the Maya knew anything of the sidereal period of Venus, or even had any reason for wanting to know of it. It is true that we can easily compute it due to the coincidence that the short period of 2920 days happens to be (within a day or two) the common multiple of our year and of the synodic period of Venus. However, this computation depends upon a rather advanced understanding of the geometry and motion of the solar system, and its availability to advanced geometers has no reasonable bearing on the Maya situation.

MERCURY

Like that of Venus, the orbit of Mercury lies within that of Earth and the same conclusion applies to both planets. It is so close to the Sun that it can only appear in the heavens within 28 degrees of that body. The difficulties of observing it are well known. Its orbit is so elliptical that its synodic periods vary considerably. I think that I need say no more.

CONCLUSION

Summarizing, I think that Venus and Mercury are eliminated as reasonable possibilities. For the superior planets, it is arithmetically possible to obtain the sidereal period if given the following:

(a) A clear idea of the averaging process.

(b) An understanding that these planets continue their circuit of the zodiac regardless of the apparent interruption due to the Sun's frequent interference with observations.

(c) A written language capable of recording phenomena that are not repeated within a lifetime so definitely that the record may not be misunderstood.

I do not think that it is within the realm of probability that the Maya culture fulfilled these requirements. Even if they understood the averaging process (a very doubtful assumption), the numerical pattern of astronomical intervals found did not suggest its use. As to the next point, it is conceivable but far from certain that the Maya had advanced to this degree. The last point is speculative, but my own investigation of Glyphs C, D and E leads me to believe that only as late as Baktun 9 did they perfect a method of recording the lunar half-year. Consequently, I doubt if they had progressed anywhere nearly as far in definitely recording the obscure and extremely puzzling movements of the planets.

To grant the Maya all three of these abstract conceptions is an assumption that appears to me far beyond the realm of probability.

Appendix III

REMARKS ON THE CORRELATION QUESTION

By R. C. E. Long

I am in general agreement with Mr. Thompson.

As to the Aztec dates I would not say that some agree with Spinden's thesis and some with Martinez's. For the Aztec dates Spinden follows Seler in holding that the day names fell on the same month days as in the Old Empire Maya calendar, which would result in 8 November 1519 being equivalent to 7 Cipactli 9 Quecholli. This makes all dates in the Spinden and also in the Goodman-Thompson correlation fall one day later than the Aztec days equivalent to them in the tonalamatl. But Martinez not only differs in the correlation of the Maya and Julian calendars, but also holds that the Aztec was similar to the New Empire Maya calendar in the month days on which the day names fall, which would make 8 November 1519 equal 8 Ehecatl 9 Quecholli. By these two assumptions it results that in the Goodman-Martinez correlation, the Aztec dates fall one day after the Maya ones.

A matter which is still uncertain is whether Landa's statement really means that 1 Pop equals 16 July 1553. Landa knew nothing of the Maya method of counting by elapsed time, and when he says "el primero de su mes Popp" he may have been referring to 0 Pop, not 1 Pop, especially as he says that this was the first day of the year. I think that there is no doubt that the year of Landa's calendar was 1553, having regard to the sequence of year bearers given in the Books of Chilam Balam and the Chronicle of Oxkutzcab, but Spinden's supposed demonstration that it must be 1553, because the first of January is marked with the letter A, proves nothing. In the Church Calendar every first January is marked A, the first of the series of seven "ferial" letters. If Sunday falls on A, the first of January, then the "dominical letter" for that year is said to be A, but this has nothing to do with the invariable series of the seven ferial letters, which is all that Landa gives.

I am decidedly of opinion that there was no break in the 260-day almanac since its inception. In this I differ from Teeple (*Maya Astronomy*, p. 47), who suggests that a day might have been missed or added by error. Surely if the Jacalteca and Quiché have kept the count unbroken for about four hundred years since the Spanish Conquest, although the knowledge of the calendar had to be kept secret by a small number of people who kept the count merely by memory without any system of writing, then in the pre-Conquest period, when the calendar was that of the only religion of the country under an all-powerful priesthood, and was checked by the constant erection of monuments and celebration of ceremonies, there could be no more possibility of a day being skipped or added than there is with our own calendar. In this connection I should say that I think many writers take an exaggerated view of the secrecy of the calendar in pre-Conquest times. The calendar seems still fairly generally known among the Quiché, and Cogolludo, writing of Yucatan in the Seventeenth Century, says that "most of the Indians know it." Landa, our best authority, says that they "regulated by it their festivals and trade and business and affairs as we do with ours." So among the Aztec, the calendar fixed the payment of tribute, the holding of courts and markets, and the celebration of their dramatic sacrifices which were witnessed by all. I think that the Spanish references to the knowledge of the calendar being confined to the priests and chiefs mean that the knowledge of the auguries derived from the calendar, and of course also the understanding of the long calculations and of astronomy was confined to these privileged classes, but I think that the people in general knew that it was, for example, the day 1 Kan, but that they depended on the priests to give them notice of what ceremonies should be celebrated on that day.

The Jacalteca calendar, so well elucidated by La Farge, is very instructive as to the usage in ancient times. The year bearer ceremony, and that of the close of the 260-day period after it, of course depend strictly on the calendar and must therefore gradually shift round the tropical year, but the other ceremonies (excluding those influenced by the Christian calendar) seem really to be determined by the seasons and the 260-day almanac. The principle evidently is that the time of holding such a festival, for instance as that of new beans, depends on the growth of the beans, but at the same time a lucky day is sought for the festival, in this instance Ahau. A similar system was used at the beginning of the Christian era by the Jews in fixing the date of the Passover. It must always fall on a full moon, just as the Jacalteca ceremonies must fall on a lucky day in the 260-day almanac, but the choice of the particular full moon was governed by the state of the crops.

I think that the Maya were not really disturbed by their shifting calendar. A purely calendrical festival, like that of the year bearer, might shift right round the year without any practical inconvenience, but such feasts as depended on the crops were, I believe, fixed in the same way as among the Jacalteca. This explains the enormous number of tonalamatls in the codices, which show favourable and unfavourable days for most of the important activities of life. When there were so very many of them in the books to chose from, it was easy for the priests to fix a lucky day at the proper time in the natural year.

It was quite otherwise among the Aztec. Their festivals from which the months were named were of a character dependent on the seasons. In consequence they had from time to time to displace them as shown by Seler.

I agree with Thompson's views as to the lunar calendar, in opposition to those of Spinden and Ludendorff, especially on the question of the lunations beginning on new moon. I very much doubt that these stone-age Maya of the Old Empire, remarkable as their achievements certainly were, could have been such skilled astronomers as Ludendorff's views would require them to be. Ludendorff himself feels this difficulty, for on page 15 of his paper (1933) on Plates 51 and 52 of the Dresden Codex, he says that it may be thought to be very bold to assume that the Maya had the astronomical concepts, quadrature and trigonal position evolved by the astronomers of the Old World, and he rightly says on page 47 that hardly anything is left to assume (that is under his thesis) save that the Maya had simple instruments for angular measurement. He reproduces Spinden's figure, taken from an Aztec codex, in support of this. Now the figure shows an eye looking through crossed sticks (Ludendorff's "quadrant" is merely a speech sign). These sticks were probably only a guide to the eye of the observer and served to fix his own position in taking a sight at some heavenly body when it was in line with a distant landmark. It is more than doubtful that it was an instrument for observing angles as such. Even granting that it was used to observe the height of a star above the horizon, it is a long way from such a simple observation of angular height to a true measurement of angles by dividing a circle into degrees or similar units, and the latter was necessary before the observers could arrive at the concept of trigonal position, which implies first that the Maya measured angles and secondly that they then thought of dividing the circle by three. Incidentally it may be remarked that the number three was not of much interest to them.

We must remember that the Maya had not, like the ancient Egyptians or the Sumerians, a real writing which could represent every word of the spoken language, either by ideograms or phonetically, but merely an embryo writing which in principle did not differ from the Aztec picture writing (Long, *Maya and Mexican Writing*, to be published in Maya Research, vol. II, No. 1). To my mind Ludendorff does not sufficiently appreciate the difficulties involved in the assumption he makes that a people in the state of culture of the Maya should have had astronomical concepts comparable with those of the great Greek astronomers. The same objection applies to his view that the Maya recognized the sidereal revolution of the planets. This is a very different thing from knowing the synodic periods of some of the planets, which they certainly did, and which required nothing more than careful observation combined with the keeping of records extending over a very long period.

I had myself already drawn attention to the error in Martinez's lunar calculation in my paper read at the International Congress of Americanists at Hamburg in 1930, and also to the improbability of the style of art remaining unchanged for so long a period as the Spinden correlation demands. On the whole I am still inclined to adhere, as Martinez does, to the Brinton arrangement of the katuns in the Books of Chilan Balam, but that is of course a matter independent of the correctness of the Goodman-Thompson correlation.

It certainly seems as if 9- 9- 9-16- 0, 1 Ahau 18 Kayab in the Dresden Codex was intended to be the date of a heliacal rising of Venus, from the prominence given to it and its having a ring number. The difficulty of reconciling it with the Goodman-Thompson correlation may not be insuperable. The Dresden Codex is probably not a very ancient document, though it embodies the learning of the Old Empire, and perhaps the author of it, writing long afterward and relying not on records but on Venus tables, set down this date as a heliacal rising without its being so in reality. We know from Teeple's work that 1 Ahau 18 Kayab was a possible date of helical rising of Venus according to the Venus calendar. Having regard to the many points in favour of the Goodman-Thompson correlation, I think we may eliminate this date as an objection to it. Perhaps the same may apply to 9-16- 4-10- 8 not being an eclipse date.

I entirely agree with Thompson's view that not much more evidence will be available from the inscriptions to support any correlation, and that an archæological check is very much needed. I also agree that the Maya probably did not record astronomical phenomena except when it was necessary to record the date for some other purpose.

As to Landa's statements about the rule of the katuns, Morley (*The Inscriptions at Copan*, p. 576) only quotes the extraordinarily confused account in Landa (p. 317 of the Brasseur de Bourbourg edition) and naturally fails to make sense of it, but makes no reference to Landa's much more intelligible language on page 315 where it is said that they had two idols in their temples dedicated to two of these characters (that is to two Ahaus). To the first, which commenced at the cross placed above the circle (in Landa's diagram), they rendered homage, making offerings and sacrifices to obtain a remedy for the calamities of those twenty years, but after ten years had passed of this first one they did not offer him more than incense and respect. Once the twenty years of the first were passed, they commenced to conduct themselves according to the omens of the second and to offer him sacrifices; having removed the first idol, they put up that of the second in order to venerate him for another ten years. This seems to me to mean that during any one katun there were in the temple the idols of both the reigning katun and the following one, but that the reigning katun only received full honors for the first ten tuns, although it remained in the temple till the katun ending day from which it was named. The Maya, like some other people, did not think it worth while to pay much respect to an administration which would soon be out of office and paid more attention to conciliating the coming power. Here again the Jacalteca usage helps us. The day names in their calendar are called "men," being really not names of days but of "men," or rather gods, who ruled over them, and it was so with the Yucatec katuns.

Since I wrote the foregoing and sent it to Mr. Thompson in 1933, Dr. Beyer has published his correlation. I have only to say that I can not accept it and I entirely agree with Mr. Thompson's criticisms of it.

Appendix IV

THE MAYA YEAR BEARERS

By J. Eric Thompson

The Yucatecan year bearers in the Sixteenth Century, as is well known, coincided not with the so-called 0 Pop, but with 1 Pop, but they were the days Kan, Muluc, Ix and Cauac, whereas by the old system in vogue during Cycle 9, the days Akbal, Lamat, Ben and Cib should have coincided with 1 Pop.

On the other hand it has generally been assumed that during Cycle 9, the Maya year began on the so-called 0 Pop, and as a corollary the year bearers would have been Ik, Manik, Eb and Caban.

In the position before 1 Pop, the same month glyph is usually found in connection with an affix known as the spectacle glyph. This combination has been generally accepted as having the meaning of 0 Pop. In a similar way this affix with any month glyph is generally accepted as having the meaning of zero.

Occasionally, however, a sign resembling the Tun glyph but usually supplied with an apparently lunar superfix is found with month glyphs. This sign has been translated as zero, and has been considered to have the same meaning as the "spectacle" glyph.

There are two occasions where this sign occurs in connection with dates, the positions of which are securely fixed by one or more Secondary Series. The first of these occurs on the Tablet of the Cross at Palenque in connection with the following calculation:

```
13. 0. 0. 0. 0, 4 Ahau 8 Cumhu
        1. 9. 2  Add
────────────────
13. 0. 1. 9. 2, 13 Ik "Tun" sign Mol
 1.18. 3.12. 0  Add
────────────────
 1.18. 5. 3. 2  9 Ik 15 Ceh
```

Here one would normally expect to find instead of 13 Ik "Tun" sign Mol the Calendar Round position 13 Ik 0 Chen. The suggestion has been made that the "Tun" sign be translated as zero and that Mol be read Chen on the assumption that the Maya sculptor carved Mol when he wished to record Chen.

The second occasion where this sign is found with a date definitely fixed by Secondary Series was called to my attention by S. G. Morley. It occurs on a series of shell plaques with inscribed dates found at Piedras Negras by Linton Satterthwaite, by whose courtesy I am able to refer to this unpublished inscription. The calculations open with a Calendar Round date 5 Cib 14 Yaxkin. The position of this in the Long Count is without doubt 9.12.2.0.16, 5 Cib 14 Yaxkin since this date is recorded as an Initial Series on Stelæ 1 and 3 at the same city. After recording another date, also recorded on Stela 1, and a second date, one day after another date recorded on both Stelæ 1 and 3, it reaches by addition the Long Count position 9.14.17.14.17 recorded as 1 Caban "Tun" sign Yaxkin, whereas normally one would expect to find 1 Caban 0 Mol recorded.

On both occasions one finds the previous month recorded with the "Tun" sign. However, this "Tun" sign, as already noted, usually is supplied with a superfix which clearly has a Lunar significance, since it occurs as a lunar affix with Glyphs D, E and F of the Lunar Series, and also as a suffix with the month glyph Kayab, the ruling deity of which may have been the moon goddess.

It is well known that the normal hand form of the completion sign is frequently combined with lunar symbols, such as a shell. There is, therefore, reason for believing that this "Tun" glyph, usually with a lunar superfix, carries the idea of completion. In that case the Calendar Round dates just quoted should be translated as 13.0.1.9.2, 13 Ik Mol completed and 9.14.17.14.17, 1 Caban Yaxkin completed.

This seems a more logical explanation than to suppose that the "Tun" sign has the same significance as the "spectacle" glyph, and to assume that in both the cases, where the positions are definitely fixed, the Maya sculptor made a mistake, recording erroneously the previous month glyphs. As a last resort it is sometimes permissible to attribute an apparent mistake to the Maya sculptor or astronomer-priest, but it is hardly justifiable to advance an interpretation on the assumption that the Maya made mistakes on each occasion where the theory can be checked by calculation.

There is one other certain example of the glyph. This occurs on Lintel 9 at Yaxchilan in association with the day 1 Eb and the month glyph Yaxkin. Here the Long Count position is not known, but on the strength of the two cases cited, one is forced to read the date as 1 Eb Yaxkin completed (*i. e.* 1 Eb 0 Mol), the Long Count position of which is probably 9.14.4.11.12 or 9.11.11.16.12.

Another example of the "Tun" sign with a month probably occurs in the West Court of the Palace at Palenque.[1] Here Maudslay's photograph is not clear, but the drawing that accompanies the photograph shows the Tun sign. The day sign is 13 Manik and the month glyph Yaxkin. The whole probably should be read as (9.8.18.3.7), 13 Manik Yaxkin completed (*i. e.* 0 Mol).

This date is exactly four solar years before the date 9.9.2.4.8, 5 Lamat 1 Mol, which occurs frequently at Palenque. Indeed, it occurs elsewhere in this same palace at Palenque. On the Tablet of the Inscriptions it is used in connection with important calculations into the distant future and into the past. The fact, then, that 13 Manik Yaxkin completed occupies the same position in the solar year as 5 Lamat 1 Mol, and is only separated from it by four solar years would strongly suggest that, apart from the other cases already discussed, the "Tun" sign should be read as a sign for completion.

The interpretation of the "Tun" sign when found with a month glyph as completion indicates that the Maya counted the month from 1 to 20, since the day after a coefficient of 19 has been used is sometimes indicated as the completion of that month, but more often as the next month not yet started.

[1] S. G. Morley, 1920, pp. 98–100, discusses these dates, reaching the conclusion that the sign is the same as the spectacle glyph. He also cites the Leyden Plate as an example. However, researches published subsequent to his report show that in the case of the Leyden Plate the glyph in question is Glyph G of the Lunar Series. The glyph used on Stela 18 at Copan appears to be something different since the bars of the "Tun" are curved and there are other differences.

Since the evidence clearly indicates that a day could be written either as Yaxkin completed, for example, or as "Spectacle" glyph Mol, it is clear that the month Mol was not considered as having already started. Indeed the "spectacle" glyph while undoubtedly conveying the meaning of zero, that is non-existent, should perhaps not be translated as zero, since one tends to think of 0 Mol as an integral part of the month Mol, whereas the Maya appear to have wished to convey that the new month had not yet started at that time. Seler seems to have come nearest the truth in suggesting the translation of "eve of" for the "spectacle" glyph, although the example on page 50 of the Dresden Codex, which he cites, must have been the result of a scribe's error. If the "spectacle" glyph is translated as "eve of," the whole would read "eve of Xul," whereas one would expect either the glyph for completion with Xul or the glyph for "eve of" with Yaxkin.[1]

The evidence indicates that the Old Empire year bearers must have been Akbal, Lamat, Ben and Eznab with 1 Pop, as in the Dresden Codex, since Ik, Manik, Eb and Caban would have coincided with the close of the previous year.

The change from Akbal, Lamat, Ben and Eznab to Kan, Muluc, Ix and Cauac with 1 Pop need not necessarily have involved a break in the Long Count of more than a few hours. If one makes the assumption that the days of the 260-day count were always reckoned from sunset to sunset, and that the days of the 365-day year were always reckoned from sunrise to sunrise,[2] a shift in the moment of recording a Calendar Round date from sunrise to sunset would have changed the year bearers as can be readily seen from the following table.

Sunrise	3 Akbal	1 Pop	Year starts old style
Sunset	4 Kan	1 Pop	Year starts new style
Sunrise	4 Kan	2 Pop	
Sunset	5 Chicchan	2 Pop	

Needless to say, this is merely a suggestion as to what may have happened, but it serves to show that the shift in year bearers does not necessarily involve a break in the Long Count.

The evidence of the "Tun" glyph clearly shows that the Maya year always began on 1 Pop, and that the year bearers during Cycle 9 were Akbal, Lamat, Ben and Eznab.

[1] Seler, 1900.

[2] We know from La Farge's work that among the Jacalteca and, probably, therefore among all Maya, the days of the 260-day count were reckoned from sunset to sunset. The days of the months, however, being based on the Sun, may possibly have been reckoned from sunrise to sunrise.

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