APPENDIX II

The Correlation Question

Who can direct, when all pretend to know?
—GOLDSMITH, *The Traveller*

IN A PREVIOUS publication (Thompson, 1935) I have reviewed the pros and cons of the various correlations which have been propounded. I have no desire to cross that swamp again, for I still adhere to the conclusion which I then reached, namely, that the 11.16.0.0.0 correlation is the most acceptable; but I still feel that the evidence in its favor is not irrefutable. Accordingly the discussion will be confined to examination of such new evidence as has accumulated in the past 14 years and of the new correlations proposed during that time.

All dates not otherwise specified are in the Gregorian calendar or, in the case of those prior to 1582, have been converted to that system from the Julian or old style reckoning by the addition of the requisite 10 days.

THE 260-DAY COUNT

Lincoln (1942) has investigated the calendar of the present-day Ixil Maya of the northwestern part of the highlands of Guatemala. The year bearers are I'k, Tche E, and Noh, corresponding to the Ik-Manik series. The year bearer 6 Noh (6 Caban) ran from sunset on March 10, 1940, or from midnight, six hours later, until sunset or midnight of March 11, 1940. This is not directly stated in the text, but the date can be recovered from the information that 6 Noh returned at sunset or at midnight of November 25, 1940, and continued through the day of November 26, 1940. The interval from March 10 to November 25 is 260. This is in exact agreement with La Farge's data from Jacaltenango, where a year bearer 7 Ah (Ben) entered at sunset of March 15, 1927, and ran through the day of March 16. The interval from that date to 6 Noh, sunset of March 10, 1940, is 4744, which is the distance from 7 Ah to 6 Noh (64 days and 18 × 260). It is also in agreement with La Farge's data from Santa Eulalia, where a year bearer 12 Chinax (Etz'nab) entered on the evening of March 13, 1932, and continued till sunset of March 14.

As this double dating is confusing, and as, moreover, it seems not impossible that the highland days actually are counted from midnight to midnight or perhaps from dawn to dawn, and may appear to start at sunset because of the guest system (p. 102), I shall not refer in future to the positions in our calendar on which a day may have entered, but that on which it was current. That is to say 6 Noh *entered* at sunset on November 25, 1940, but was *current* on November 26.

There has been some confusion in the discussion of the correlation of Aztec and Maya days of the 260-day period. The best Aztec-European double date is that of 1 Couatl for the surrender of Cuauhtemoc on August 23, 1521 (August 13 O.S.), because this is given by several sources. The distance from July 26, 1553, is 11,660 days. If those are subtracted from 13 Chicchan, the equivalent of July 26, 1553, according to the present-day calendars of Guatemala, the position 1 Chicchan is reached, and that is the Maya equivalent of 1 Couatl.

Sources disagree on the Aztec date for the entrance of the Spaniards in Tenochtitlan on November 18, 1519. Sahagun says it fell on 1 Eecatl on the eve of 10 Quecholli; Chimalpahin gives the position 8 Eecatl 9 Quecholli. Sahagun is surely wrong in giving the Aztec day as 1 Eecatl because the position 8 Eecatl for November 18, 1519, leads forward to 1 Miquiztli (Cimi) on July 26, 1553. To conform to the 1 Couatl and 13 Chicchan placements, 7 Cipactli must fall on November 18, 1519, and 8 Eecatl on November 19. These two dates have been fully discussed by Seler (1902–23, 1:177–83) and Caso (1939), both of whom conclude that November 18 should correspond to 7 Cipactli.

Spinden (1924, p. 100) calls attention to two other cases of double dating in the Annals of Tecamachalco. The equivalent of September 14, 1575, was a day Itzcuintli three days before 2 Acatl, and which must be 12 Itzcuintli; the second double dating gives February 11, 1576, as corresponding to 4 Tecpatl.

The first of these dates, projected backward, leads to 13 Couatl (Chicchan) on July 26, 1553; the second is not in agreement with the first. The interval in Aztec positions is 148 days; in the European calendar it is 150. Therefore, according to the first entry 4 Tecpatl would have to correspond to February 9. Should the second entry be the correct one, the equivalent of July 26, 1553, would be 2 Mazatl (Manik).

We have therefore four entries from Aztec records. The 1 Couatl date, given by three independent native sources, leads to 13 Couatl (13 Chicchan) on July 26, 1553; the

first entry in the Annals of Tecamachalco does likewise. The second entry in the Annals of Tecamachalco leads to 2 Mazatl (Manik) on July 26, 1553; the better of two contradictory statements on the entry of the Spaniards into Tenochtitlan leads to 1 Miquiztli as the equivalent of the same Gregorian date.

This is pretty good evidence that the Aztec almanac was in line with those still functioning in the highlands of Guatemala, and that therefore Landa was probably wrong in placing 12 Kan 1 Pop on July 26, 1553. He should, I think, as La Farge has previously pointed out, have equated 12 Kan with July 25 and 13 Chicchan with July 26. Indirect support of this is perhaps deducible from evidence on the Chol calendar presented below.

There is a possible explanation of this one-day difference: Landa reached Yucatan in 1549; his calendar is securely dated as 1553, but it is extremely doubtful that Landa's native informant had enough knowledge of the European calendar to make the correlation of the two systems. It is possible, or even probable, that Landa had acquired sometime before 1553 the information that the year bearers fell on July 16 (O.S.), and when he came to set his Maya data against the European calendar, he utilized the information that he had gathered two or three years earlier, unaware of the fact that, because of the leap day in 1552, the position 1 Pop had moved from July 16 (O.S.) to July 15. It had stood at July 16 (O.S.) from 1548 to 1551. He might well have picked up the July 16 equation in 1550 or 1551, for at that time Christianity had made little impression in Yucatan (human sacrifice was still rampant 10 years later), and there is no reason why Landa should not have witnessed the year-bearer celebrations in some heathen community in 1550 or 1551, or casually heard of it from one of his colleagues, and made a note of the date in the European calendar. Probably at that time the complete integration of the Maya calendar with paganism was hardly apprehended, and therefore it had not yet been deemed a work of the devil meriting extirpation.

As we shall see in discussing the LC, there is indirect evidence which might support the above explanation of the apparent error of one day in Landa's correlation.

THE 365-DAY COUNT

In the annual report of the Division of Historical Research, Carnegie Institution of Washington, for 1945–46, F. V. Scholes gives advance notice of a hitherto unknown manuscript on the Manche Chol. This is Martin A. de Tovilla's *Relación histórica dyscreptiva de las provincias de la de Verapaz y de la del Manché de el Reyno de Guatemala.* Scholes presents an extract dealing with the Chol calendar from the manuscript, which is dated May 17, 1635. Tovilla states that the 360-day period ended June 28 and was followed by "the five days of great fast," which ended July 3. "Thus on July 4th begins the first day of the year according to their account."

It appears probable that this account was obtained from the Indians in the period 1631–33. If the latter date, 20 leap days had caused a backward slip in the Chol calendar of that amount, and new year in 1553 would have corresponded to July 24. This is one day before our corrected equation of 12 Kan 1 Pop = July 25, 1553. It will be remembered that Tovilla's dates are in Gregorian and that a leap day was intercalated in A.D. 1600. We do not know what set of year bearers the Manche Chol favored. If they used the Akbal-Lamat set that would account for the one-day discrepancy from the Yucatan position. That they so did is probable since the Kan-Muluc set stems from a Campeche-Yucatan innovation, and no other part of the Maya area, so far as we know, adopted the new system. In that case Chol 12 Kan 2 Pop would have fallen on July 25, 1553, and 11 Akbal 1 Pop would have been the year bearer on July 24, in agreement with our deduction.

It is probably more than a coincidence that the Chol calendar from the Alta Verapaz (Thompson, 1932) starts the year on July 14. If this is assumed to be Old Style dating, it would mean that the calendar had been frozen into the European between 1548 and 1555, depending on whether the Yucatec year started on July 15 or 16 (O.S.) in 1553 and on which set of year bearers the Chol used. The reductions in the Alta Verapaz culminated in the year 1549.

SHIFTS IN YEAR BEARERS

It has now been established fairly satisfactorily that the shift which caused, for example, the second, seventh, twelfth, and seventeenth positions in a month to coincide with Ahau, rather than the third, eighth, thirteenth, and eighteenth positions, took place at least as early as the middle of Cycle 9, in Campeche, and that it had no effect on the LC, for Etzna 18 and 19 give IS 9.12.0.0.0 10 Ahau 7 Yaxkin and 9.13.0.0.0 8 Ahau 7 Uo. Furthermore, a date 9 Ahau 17 Mol, which is followed by a head and then the katun sign with bracket, occurs on Stela 9 at that same site (fig. 35,9–*11*), and this almost surely corresponds to 9.19.0.0.0 9 Ahau 7 (8) Mol (Proskouriakoff and Thompson, 1947). None of these readings is absolutely certain, but they are all highly probable and, taken together, make a very strong case for this shift having occurred about the middle of the Classical or Initial Series Period.

The IS at Xcalumkin reads 9.15.12.6.9 7 Muluc (1 Kankin). Following Glyph A of the lunar series there is an unknown glyph with coefficient of 1. It is logical to

assume that this is a head variant of Kankin, and that the coefficient of 1, accordingly, is evidence that by that date the shift had been adopted at that site (fig. 35,*8*). Miss Proskouriakoff has also noted a date 5 Ahau 2 Kayab, surely corresponding to 10.1.0.0.0 on Oxkintok 3 (fig. 35,*15*).

There are also two stelae (nos. 18 and 20) at Yaxchilan which record respectively 3 Eb 14 Mol and 6 Ix 16 Kankin, both of which are in the new system. Miss Proskouriakoff points out that these two stelae belong to a group of monuments at Yaxchilan which has stylistic affinities with the Puuc (fig. 35,*12,13*).

Lunar data from Etzna confirm that there was no break in the LC. Etzna 19 has a lunar series perhaps recording a moon age of 25 days, whereas one would expect about 27–28 days. The reading of 25 days is a little open to question. Both D and E are present. The former, of course, is without coefficient; the latter has an irregular bar in front of it, to the left of which there are two or three dots, probably weathering in the frame. The Xcalumkin IS conforms to the calculated moon age of that date. The date of Etzna 18 was chosen partly because the recorded moon age agreed with 9.13.0.0.0, but stylistic considerations (Stelae 19 and 18 are very similar in design) confirm the choice. The lunar series on this stela, accordingly, can also be used as evidence against a break in the LC.

The data from Etzna show clearly that the shift in month positions had no effect on the LC. It is necessary, therefore, to discard the possibility that this shift may have been accompanied by a break in the katun count, as Weitzel (1947) has supposed. Similarly, the one-day break which I had postulated at the time of the shift (Thompson, 1935) cannot have taken place. I think, therefore, that in view of the probability that Landa was one day off in placing 12 Kan on July 16 (O.S.), and the virtual certainty that there never was a break of one day in the LC, my original Ahau equation of 584,285 must be reduced to 584,283, making it two days greater than the Martinez-Hernandez equation.

LONG COUNT

No new information of a direct nature bearing on the engagement of tuns or katuns in the LC is forthcoming. It does seem almost certain that the author of the series of prophecies for the years in a Katun 5 Ahau which is given in Tizimin and Mani had access to the list of tun endings which was utilized by Don Juan Xiu when he made the entries on the famous page 66 of the Chronicle of Oxkutzcab. It would appear that the unknown author took the entry of a tun ending on 5 Ahau 17 Zec in a year 13 Kan beginning in 1541 and made it the start of a katun in 1593 by adding one CR. There seems to have been some doubt about the month position. Don Juan Xiu writes 5 Ahau 16 Zec; the Tizimin and Mani versions have 5 Ahau 15 Zec; it should be 5 Ahau 17 Zec. Later, the author inserts material which indicates that the katun ended on 5 Ahau in 1613, and it is obvious that he was not at all well acquainted with the workings of the katun count. The important point, however, is that he almost certainly had before him the sequence of tuns utilized by Juan Xiu, and therefore that series was an important source of material, and may have been in existence a long while before Juan Xiu copied it in A.D. 1685. We can be certain, however, that the statement that the series of prophecies was written in European script in Chetumal in 1544 is a falsification. Obviously no Maya could have written in European characters as early as 1544, a year before the arrival of the first Franciscan friars in Yucatan.

A puzzling feature in Landa's typical year is that the prophecies for the year were read on what would appear to have been 5 Zip, and this was followed in succession by the feasts of the doctors, hunters, and fishers, respectively, on 6, 7, and 8 Zip. One wonders why the prophecies for the year were read in Zip, rather than at the beginning of Pop, and why these other important festivals should have followed in quick succession. It struck me as possible that these entries refer to celebrations for the new tun rather than for the new year. Indeed, one gets the impression from Landa's account that the question of the prophecies for the year was settled during the ceremonies which began in Uayeb.

According to the 11.16.0.0.0 correlation (equation 584,283), tun endings between 1551 and 1553 were:

(11.16.12.0.0)	4 Ahau	7 Zip	August 31, 1551 (O. S.)
(11.16.13.0.0)	13 Ahau	2 Zip	August 25, 1552 (O. S.)
(11.16.14.0.0)	9 Ahau	17 Uo	August 20, 1553 (O. S.)

The festivals described by Landa were:

Prophecies for year	5 Zip	August 29 (O. S.)
Doctors' feast	6 Zip	August 30 (O. S.)
Hunters' feast	7 Zip	August 31 (O. S.)
Fishers' feast	8 Zip	September 1 (O. S.)

Plausibly, although one cannot be more positive than that, Landa collected data on these festivals in 1551, just as he may have obtained the 1 Pop = July 16 equation at that time, or perhaps in 1552. In the one case the festivals would have clustered about the new tun; in the other, they would have fallen in the first days of the new tun.

CERAMIC EVIDENCE

The evidence of pottery, I think, still continues to disbar the Spinden correlation and also any correlation that makes any position in the Maya LC prior to 11.0.0.0.0

coincide with the Spanish conquest. To discuss the matter at great length would not be advisable at this time, but one or two outstanding points can be reviewed.

In the Maya area plumbate pottery of the effigy type appears after the Initial Series Period, for it has been found in tombs at Copan built in refuse of buildings of the Great Period, and is nowhere found with deposits of that period in the Peten. It is absent from the Puuc except for one sherd at Uxmal (Brainerd, 1941), but it is associated with buildings of the Mexican Period at Chichen Itza and, on the evidence of one vessel purposely deposited in the debris of the Caracol, it was still in use when that city was abandoned. Apparently, it had disappeared by the time Mayapan had established itself firmly as the chief city of United Yucatan.

In the Valley of Mexico and surrounding areas plumbate occurs in the Mazapan Period, but it is wanting from Aztec II–IV, and from ceramic deposits earlier than Mazapan. Vaillant (1941) places the start of Aztec II around A.D. 1300; Caso (1941) has the same horizon commencing at approximately A.D. 1225. I am inclined to favor the Caso dating, but let us split the difference and consider A.D. 1260 as the inception of Aztec II.

The best reconstructions of Yucatecan history (Roys, 1933; Thompson, 1941b) place the abandonment of Chichen Itza in the last decade of the twelfth century. This, it will be seen, agrees well with information from central Mexico, for plumbate continued for a short while after the conquest of Chichen Itza, say until A.D. 1225, whereas it had disappeared in central Mexico before A.D. 1260.

The Mexican Period is said to have lasted 220 years and certainly started after 10.3.0.0.0, because hieroglyphic stone lintels at Chichen Itza belong to the pre-Mexican Period. We have therefore:

End of Initial Series Period	10. 3.0.0.0
220 years of Mexican Period	11.0.0.0
	10.14.0.0.0
Mayapan dominant 260 years	13.0.0.0
	11. 7.0.0.0
Post-Mayapan Period 80 years	4.0.0.0
Spanish conquest of Yucatan	11.11.0.0.0

This scheme, of course, is a rough one. If a hundred years on either side is allowed, the range of dates in the LC corresponding to the katun of the Spanish conquest would be from 11.6.0.0.0 to 11.16.0.0.0. Actually, there is some evidence that the Mexican conquest of Chichen Itza was not in 10.3.0.0.0, but in 10.8.0.0.0, and in that case the final date becomes 11.16.0.0.0. Adjustments might shorten these periods sufficiently to lead to an 11.3.0.0.0 correlation, but the periods can hardly be expanded to agree with the 12.9.0.0.0 correlation, for there is then a displacement of plumbate or a gap of some 350 years between the end of the Initial Series Period and the appearance of effigy plumbate. Such a gap can hardly have existed in view of the certainty that the appearance of simple plumbate can not be set long after the close of the Initial Series Period; it may, in fact, be coeval with the final stages of that period (Thompson, 1948), and effigy plumbate probably followed close on simple plumbate.

The 12.9.0.0.0 correlation is clearly too long; is the 11.3.0.0.0 correlation too short? That depends on the estimated length of the Mexican Period at Chichen Itza. The end of plumbate (c. A.D. 1260), which we have seen was shortly after the fall of Chichen Itza, becomes 10.8.15.0.0 in that correlation; the start of the Mexican Period can not be earlier than 10.3.0.0.0 because at that date pre-Mexican buildings and hieroglyphic lintels were still being erected. At the outside, then, there are about 110 years into which to fit the Mexican Period, probably nearer 80 years, for one must allow some 20 years for the partial collapse of the Caracol after the abandonment of the site and before the deposit of the plumbate jar in its collapsed masonry, and the introducers of Mexican influences can hardly have walked into Chichen Itza the day after 10.3.0.0.0 was completed. The architects must tell us whether it is reasonable to suppose that the great mass of Mexican buildings could have been erected in that time, and, furthermore, whether or not at least three distinct styles or phases (Early Castillo, Warriors, and that group of late degenerate structures of which the Temple of the Initial Series, House of the Grinding Stones, Temple of the Interior Atlantean Columns, and the Casa Redonda are examples) could be compressed into a period of 80–110 years. Perhaps this ceramic and architectural frame can be garbed in the 11.3.0.0.0 correlation, but I fear the fit will be a bit tight around the hips and quite a lot of ankle may show.

THE MAKEMSON CORRELATION

The ingenious thesis of Dr. Makemson (1946) is that Landa never attempted a day-for-day correlation, but that the July 16 = 1 Pop equation was the work of a later copyist. Why Landa should have gone to the considerable trouble of placing the calendar for 1553 against the Maya year without attempting to correlate the two is hard to explain, but Dr. Makemson supposes that he did this. She further supposes that a copyist noted Landa's entry against 1 Imix 18 Yax, "Here begins the count of the calendar of the Indians, saying in their tongues 1 Imix," and that the copyist noted that this entry occurred opposite July 17; further, that the copyist supposed that the day began on sunset on July 16 and ran through July 17. She next

assumes that the copyist, in view of the above entry, rearranged the whole calendar to make 12 Kan 1 Pop coincide with July 16. To those suppositions yet others must be added. We must assume that only this copy or others made from it was available in Yucatan, that the Maya were so ignorant of their calendar that they accepted its start on July 16, in midsummer at a time when the maize was about a foot high, whereas they had known, according to the Makemson thesis, that new year fell late in the fall, long after the maize had been harvested. We must also suppose that the scribe—a member of a class of notorious hack writers—knew what no one else has ever reported from Yucatan, namely, that the Maya day ran from sunset to sunset, and that he corrected Landa in that respect. On the other hand, we must assume that he was so ignorant of the Maya calendar that he did not know that 1 Imix was the start of the 260-day almanac, although he had read it in Landa's manuscript, in the sentence before the place where he is supposed to have inserted the statement that 1 Pop was always on July 16.

Mrs. Makemson's idea that the Maya were so ignorant of their calendar that they accepted Landa's scribe's version of it is contradicted by Sanchez de Aguilar's statement that the attempts of the early friars to extirpate the Maya calendar were without success because most of the Maya knew it because it had been handed down to them from their forebears. Sanchez de Aguilar, who places first of Pop on July 17, surely did not take his material from Landa. His information on native customs does not repeat Landa; it supplements him. Moreover, in the use of the expression *cuch haab* and in the three names he gives for Uayeb he cannot be following Landa who used no such Maya terms. His spelling of month names differs from that of Landa. Accordingly, Aguilar is an independent source for the Maya year's having started with first of Pop in mid-July. If that is the case, Mrs. Makemson's elaborate explanation of the actions of this rascally scribe falls to the ground, but that is the cornerstone of the elaborate structure she has erected to house an astronomical correlation. Without it this towering mass of assumptions piled one upon another can ill withstand "from the lips of truth one mighty breath." Having dismissed Landa's correlation, Mrs. Makemson opts for an Ahau equation 489,138, which is just 246 days before the Spinden correlation. The result is that 12 Kan 1 Pop coincides with November 22, 1552 (Gregorian), and holds the LC position 12.9.17.9.4.

This correlation shares with that of Spinden the overwhelming disadvantage of leaving a great gap in the ceramic sequences of the Maya area and central Mexico, which has already led every person who has studied the pottery of either area to abandon it, but at the same time it does not partake of the one strong point in favor of the Spinden correlation, namely, agreement (with a one-day lag) with the almanacs of the Aztec and the present-day Maya of the highlands of Guatemala. Furthermore, all sources place the beginning of Pop during the sixteenth century in July, and, since Mrs. Makemson's paper was written, data on the Manche Chol calendar have confirmed this general equation. I can see no reason for supposing that all the sources which make 1 Pop coincide with July 16 (O.S.), or thereabouts, stem from Landa. Mrs. Makemson to the contrary, we have no certain information that Landa's manuscript was widely copied in Yucatan, nor is its influence apparent in all later historical documents written by Spaniards, although some of the authors of the Relaciones de Yucatan had access to it. Lopez de Cogolludo and Herrera probably studied a copy in Spain.

I do not think that much stock can be put in the Makemson thesis that the festivals detailed by Landa fit better a year starting in November than one starting in July. The descriptions of the rain-making ceremony in Mac and Pax have been shown by Long (1923a) to refer to the burner period, a celebration related not to the year, but to the 260-day period. Mrs. Makemson supposes that the reason for the absence of festivals in Ceh was that the men were busy in the fields at that time (June 29–July 18 in her correlation), but it is in February, March, April, and early May that the agriculturalists are most busy. Her correlation, of course, agrees very well with the lunar data and that concerning the planet Venus, but that was the basis on which it rested. The paper serves to illustrate the dangers of trying to establish a correlation on two or three lines of evidence instead of considering all evidence, that of the positions in the 260-day almanac, the years, the katuns, the moon, Venus, pottery sequences and architecture.

ESCALONA RAMOS CORRELATION

A correlation which makes the katun of the Spanish conquest occupy a position in the vicinity of 11.3.0.0.0 has had supporters for many years. Lehmann (1910) was probably the first to propose such a correlation, and Leslie Mitchell had a somewhat similar scheme. Escalona Ramos (1940) has published a day-for-day correlation using the Ahau equation 679,108 which makes 11.3.0.0.0 13 Ahau 13 (12) Pax correspond to March 11, 1543 (O.S.). This causes Landa's 12 Kan 1 Pop to fall on May 1 (O.S.), which means that it is 75 days out of conformity with the almanacs of all other groups, and in disagreement with what Landa himself writes. Escalona Ramos solves this problem to his own satisfaction by supposing that there were two different calendars, one the LC, the other the popular calendar. He supposes that in the popular

calendar leap days were inserted, and cites a hodgepodge of long-since discredited references to the use of leap days by Aztec and Maya. I can see no evidence whatever for a second calendar which comprises a separate cycle of 52 years, with insertion of leap days every fourth year. If this was the popular calendar, why is it that the popular calendars of the present-day Maya of the Guatemalan highlands do not also insert leap days, and why should those almanacs without leap days check (with a one-day gap) with 12 Kan on July 26, 1553, which was in a cycle allegedly corrected by the insertion of leap days? Furthermore, we have the definite statement of Tovilla that the Manche Chol did not use leap days, and their new-year's day agrees within a day or so with Landa's date for 12 Kan 1 Pop by counting back the requisite multiple of 365 days, not a multiple of 365¼ days. The argument for this 52-year cycle with its leap days is against all the evidence, and represents an unsuccessful attempt by the author of the correlation to keep one foot in the camp of astronomy and the other in the Landa year. If Escalona Ramos accepts Landa, his lunar data are about nine days out. He must, therefore, ignore Landa's year or reconcile it by this strange method with his other data.

This correlation does not place Venus at heliacal rising after inferior conjunction on any of the possible dates in Dresden. Escalona Ramos chooses as his base 10.10.11.12.0 1 Ahau 18 Kayab, at which time, according to his calculations, Venus was 235 days past heliacal rising and about to be lost to sight, about 53 days before superior conjunction. However, the Maya and other peoples of Middle America were interested in heliacal rising of Venus four days after inferior conjunction at the end of the eight days of invisibility. The Venus table in Dresden ends at that point; there is no emphasis on heliacal setting nearly two months before superior conjunction.

Although I have not been averse to an 11.3.0.0.0 correlation, I can see very little to recommend this particular version of it.

10.10.0.0.0 (WEITZEL) CORRELATION

The latest correlation to be proposed makes 10.10.0.0.0 13 Ahau 13 Mol the katun of the conquest (Weitzel, 1947). Weitzel's thesis is that month positions were shifted forward one position, i.e. 4 Ahau 8 Mol shifted to 4 Ahau 7 Mol, in order to distinguish dates in a new calendar which was based on a concept of counting time by the day on which the period started rather than that on which it was completed, and this was achieved by subtracting 80 days, viz.:

(10.5. 0. 0.0) 10 Ahau 8 Muan, katun ends May 18, 1448 (O.S.)
4.0

(10.4.19.14.0) 8 Ahau 7 (8) Zac, katun begins February 28, 1448 (O.S.)

Presumably Weitzel does not suppose that the katuns were given any LC position in his new calendar. He assumes that this new concept of counting by the beginning days of periods was due to Mexican influences, and cites the Mexican year of 365 days as being named for its beginning date, but that is not certain, for Caso (1939) brings forward good, although not irrefutable, evidence that the Aztec years were named not by the first day of the year then current but by the last day of the year (Caso, 1939). Even should he not be convinced by Caso's arguments, there is not a tittle of evidence for placing the Aztec year bearers at the start of the year. The alternative thesis places them at the start of Toxcatl (the sixth or seventh month).

Furthermore, texts from Etzna already cited (p. 305) make it clear that the shift of one day in month positions made no difference to the LC. The date 9.12.0.0.0 is recorded as 10 Ahau 7 Yaxkin at Etzna, whereas other cities wrote it 9.12.0.0.0 10 Ahau 8 Yaxkin. Nowhere is there any evidence for a backward shift of 80 days.

In an attempt to reconcile his correlation with the archaeological picture, Weitzel is forced to consider such purely Maya buildings as the Monjas and the Three Lintels as of the Mexican Period. No Maya hieroglyphic inscription at Chichen Itza can be safely referred to the Mexican Period. Weitzel cites an inscription on the tail of a serpent column in the definitely Mexican Temple of the Wall Panels in support of his ideas, but he omits to repeat what had been written on this subject by the excavator: "The portion belonging to the north column is plainly part of a hieroglyphic lintel which has been cut down to the proper size and the feathers and rattle treatment added, so that there is almost complete obliteration of the glyphs" (Ruppert, 1931, p. 124). The plate amply demonstrates the correctness of Ruppert's description, and in the caption repeats the information that the stone is reused. The only other writer to treat of this inscription likewise notes that it is "a re-used inscription fashioned into a serpent's tail" (Beyer, 1937, p. 169). It is hard to see how the re-use of a piece of a lintel as part of the tail of a serpent can be taken as evidence for the contemporaneity of hieroglyphic stone lintels with buildings of that period. Nor is the presence of a snake with a human head in its jaws evidence that the lintel on which it occurs belongs in the Mexican Period, as Weitzel claims, since this motif is found throughout the Classical Period in the Central Area (e.g. Yaxchilan L 13, 14, 15, 25).

If the Initial Series Lintel was not re-used—and it almost certainly was because part of the inscription was

covered when it rested on the Atlantean columns—an extraordinary archaeological situation arises. It is known that Atlantean columns belong to the close of the Mexican Period and are later than buildings in the styles of the Temple of the Warriors, the Temple of the Jaguars, and the Temple of the Wall Panels. The Initial Series Lintel was dedicated in 10.3.0.0.0, but the lintels of the Monjas are admitted by Weitzel to have been dedicated in the same katun. In fact, the CR date of the Temple of the Initial Series is a year earlier than that on the various lintels in the Monjas, both readings again being accepted by Weitzel. There is some reason to believe the Monjas lintels may have been re-used because in some cases the width of the inscribed part of the lintel is too great for the doorway, and parts of glyphs are hidden under the masonry. Even ignoring that possibility and accepting the lintels as contemporaneous with that typically Maya construction the second story of the Monjas, Weitzel's scheme allows no time at all for the architectural and stylistic sequences which include such buildings as the sub-Castillo, the Castillo, and the Temples of the Chac Mool, the Warriors, the Jaguars, the Wall Panels, and the High Priest's Grave. This is so because this whole sequence and more besides lie between the second story of the Monjas and the Atlantean columns of the Initial Series Temple. Too much time may be allowed for the Mexican Period at Chichen Itza by other correlations, but one can not reduce that span to nothing.

Furthermore, in the Weitzel scheme Chichen Itza had not been abandoned by A.D. 1400, but we have seen that Chichen Itza had been deserted before the end of the plumbate horizon, which on evidence from the Mexican highlands almost surely ended about A.D. 1250. Weitzel's correlation also demands that the period of Mayapan's domination coincided with the occupation of Chichen Itza, which again is against the ceramic and architectural evidence. In addition to being at complete variance with the ceramic and architectural evidence, this correlation fails to agree with the data on the Aztec and highland Maya calendars which lead to the equation 12 Kan = July 25, 1553. His Ahau equation can be deduced to be 774,078.

Weitzel assumes that Christian influence in Yucatan was strong enough to cause the calendar to be frozen into the European year between 1528 and 1531; I deem that inconceivable, for at that time Yucatan was unconquered, and there were then no missionaries at work in the province. The attempts of the Montejos to conquer Yucatan from 1531–35 were never consolidated, and we can be sure that the calendar was not frozen at that time, and that no serious attempts had yet been made to evangelize the country. It was not until 1539 that Champoton became permanent Spanish territory, and not until 1541 that the final conquest of Yucatan was commenced and pushed with determination to its conclusion in 1546. It was not until 1545 that the friars arrived, and the first attempt was made to Christianize the country; even then the friars were too few to make much impression on the native way of life. The Maya certainly did not know enough about the European calendar to change their own to conform with it in or before 1532, and one can be reasonably certain that 1 Pop had not been frozen at July 16 even 20 years later.

Weitzel must also assume that the 260-day almanac was tied to the European year, and that is almost unthinkable. The Weitzel thesis requires us to assume that once every four years there was a nameless day (inserted at February 29, 1532, 1536, 1540, etc.?), but no such arrangement was ever followed in the 400 years of Spanish domination in Guatemala. If the last unfrozen year in Yucatan had started in 1530, the year bearer according to Weitzel would have been 2 Muluc on first of Pop equivalent to July 26, 1530 (Gregorian), but the present-day calendars of the Guatemalan highlands projected backwards and the Aztec almanac projected forward from 1521 make 10 Kan, not 2 Muluc, fall on that date.

Weitzel asserts that the Maya year was frozen into the European year so that from 1532 onwards 1 Pop always fell on July 16 (O.S.), but at the same time he uses an equation 2 Ix 1 Pop = July 13, 1543 (O.S.) to bring the 11 Chuen 18 Zac = February 15, 1544 (O.S.), equation into his scheme. Apart from the fact that the entry in question surely dates from much later than 1544, he can scarcely freeze the year between 1528 and 1532, and then get confirmatory evidence for his correlation in an unfrozen entry supposedly a dozen years later.

In brief, this correlation is based on two false premises: (1) the shift in month positions was a late innovation due to a wish to bring the Maya calendar into conformity with supposed Mexican practice, and (2) the shift in month positions was accompanied by a break of 80 days in the katun dates. The correlation is contrary to all archaeological evidence and fails to agree with the almanacs of the Aztec and present-day Maya peoples of the highlands of Guatemala. Moreover, the postulate that the Maya year was frozen into the European year before 1532 is untenable; a second postulate, that the Maya were naming their katuns for their opening days, is contrary to the general evidence (p. 182). One might also add that the evidence for a Katun 11 Ahau beginning late in 1546 is very poor.

If a new correlation is needed, we may have to discard the katun count, but at all costs keep the equation 12 Kan 1 Pop = July 25, 1553.

CONCLUSIONS

There is no prospect, I believe, that any of the three correlations just reviewed can be the right one. The 11.16.0.0.0 correlation with Ahau equation 584,283 still seems best to fit the sundry requirements. The possibility of a pre-Columbian break in the calendar becomes even more remote in the light of the new data on the Manche Chol and Ixil calendars. Moreover, the discovery that the shift in month positions was an early development and was not accompanied by any change in the LC is strong evidence against a break, since had there been one, that was the logical place to have made it.

The Ahau equation 584,283 requires that the Maya reckoned their moons not from the appearance of the new moon or from conjunction, but from disappearance of the old moon or conjunction, perhaps with both bases in use at different times and in different cities. However, we have seen that there is probably glyphic evidence for that thesis (p. 241).