AN APPRAISAL OF A NEW MAYA-CHRISTIAN CALENDAR CORRELATION

Por LINTON SATTERTHWAITE, JR.
University Museum.
University of Pennsylvania.

The new correlation dealt with is by an astronomer and mathematician, Prof. Chales H. Smiley of Ladd Observatory, Brown University. It appears just as new radiocarbon evidence seemed to narrow the field in such a way as to exclude it, along with other "early" correlations associated with the names Spinden, Makemson and Dinsmoor, and to admit later "intermediate" variants of a family of correlations associated with the names Goodman, Thompson, and Martinez. Archaeologists who think the latter are too late have one more choice among "early" proposals. Presumably they would prefer elimination, not addition to the list.

The Smiley correlation is founded on astronomy alone, and it should be stated at once that the writer is no astronomer. If I undertake to criticize in this field it is the conviction that Maya astronomy must have been rather simple astronomy, such as a layman ought to be able to understand. If nevertheless I make naive mistakes, they can be corrected and we may all be the wiser because the mistake provoked the correction. Further, founding a correlation on astronomy alone does not free the proponent from an ultimate obligation to measure it against other categories of evidence, and against other and incompatible astronomical assumptions or inferences which have been proposed. So I shall first make some rather superficial comparisons with some other correlations which have been proposed, and then specifically examine the astronomical bases for the new proposal, especially Smiley's radically new interpretations of the Venus Table of the Dresden Codex. In the process it has seemed pertinent to affirmatively deal with this table in the context of what I have called the "Modified Thompson B2" correlation, which Thompson has accepted. The astronomical effects of the 2-day modification involved have not heretofore been given much attention, and certain new ideas are introduced. Readers should allow for such personal bias as may not have been suppressed as favoring that correlation.

The new correlation has been proposed and is being justified in a series of five scattered and very short papers listed under *References*. The same proposition may appear in two or more places, sometimes in such a way that all must be cited to get the full implication. To save space we shall cite using "Sm1", "Sm2", etc., the numeration of papers reflecting the order in which they seem to have been written. The first appeared late in 1960, the last early in 1962.

General Comparisons with Other Correlations

Labels for various correlations are provided in Table 1, and will be frequently used in this paper. They are needed to save space and —more importantly— to avoid confusion. The reference letter "Z" for the Smiley correlation is now added to an earlier list (Satterthwaite and Ralph, 1960, p. 180). The lunar bases are also new, giving *average* moon ages at the Maya Long Count base according to the differing correlations, not approximately true ages as calculated with Schram's *Mondphassen Tafeln* in Smiley's similar list (Sm4, pp. 240-241). New also is "(.00)" after each constant, for which the named correlationists are not responsible. This means that the average ages are as of zero hours in Greenwich Mean Time, corresponding to decimally-expressed times of day in Schram's tables. This addition to the constant (or to the letter for it) means the Maya days are taken to begin at schematic "6 PM" sunsets, in local time. One may substitute "(.50)" when postulating "6AM" sunrise days. Thompson has noted that this question is not well settled (1950, p. 102). The "Schram" ages in Smiley's list correspond to the more orthodox time of day connoted by the label "Z(.00)". I believe this refinement is not foolish since if the Maya recorded a visible solar eclipse, they saw it in the daylight portion of their day. A half-day error in a correlation could throw the eclipse into the wrong Maya day.

Putting this question aside, Cor. A3 of our list is the "Spinden" one, while B4 is the original Thompson modification of the Goodman correlation, with Correlation Constant or "Ahau Equation" 584,285. In the literature this acquired the label "Goodman-Thompson-Martinez" or "Goodman-Thompson" although, if precision is implied by the context, only Thompson's proposal can be referred to. Such loose nomenclature is unsuited to astronomical analysis. Our labels reflect two bacward modifications, the second, "Modified Thompson 2" or "B2", having been accepted by Thompson himself more than a decade ago (1950, p. 305). Smiley first uses "Goodman-Thompson" for this (Sm 1, p. 215), but with the same label shifts to 584,284 (B3) in his list of correlations and comparative moon ages (Sm4, pp. 240-241). For a fair comparison with the "Good-

TABLE 1

LONG COUNT CORRELATIONS AND THE AVERAGE LUNATION

			At Long Count Base 13.0.0.0.0 4 Ahau 8 Cumku		
Correlation Labels			Correlation Constants	Average Ages	Long Count Dates at JDN 1,874,463 (Av.Age - 0.8133)
	Z	Smiley	482,699(.00)	11.893	9.13. 6. 0. 4
12- 9	A1	Makemson	489,138(.00)	13.225	9.12. 8. 2. 5
12- 9	A2	Mod.Spinden	489,383(.00)	21.980	9.12. 7. 8. 0
12- 9	A3	Spinden	489,384(.00)	22.980	9.12. 7. 7.19
	Y	Dinsmoor	497,878(.00)	13.170	9.11. 3.15. 4
11-16	BG	Goodman	584,280(.00)	7.664	8.19. 3.15. 3
11-16	B1	Martinez H.	584,281(.00)	8.664	8.19. 3.15. 2
11-16	B2	Mod.Thompson 2	584,283(.00)	10.664	8.19. 3.15. 0
11-16	B3	Mod.Thompson 1	584,284(.00)	11.664	8.19. 3. 4.19
11-16	B4	Thompson	584,285(.00)	12.664	8.19. 3. 4.18
	X	Kreichgauer	626,927(.00)	12.492	8.13. 5. 6.16
11- 3	C	Escalona Ramos	679,108(.00)	12.939	8. 6. 0. 7.15
10-10	D	"Weitzel"	774,078(.00)	12.562	7.12.16.11. 5

NOTE: For various correlations, first colum gives Baktun and Katun at 13 Ahau near A. D. 1540. Last colum gives Long Count date at Jan. 1, A. D. 420, Julian Day Number 1,874,463, Mean Age being —0.8133 (see R. B. Weitzel, Mean New Moons, *Popular Astronomy*, LVII, 6, pp. 283-285. Northfield). Average ages at Long Count base calculated with 29.53059 per moon from these bases.

(.00) added to constants indicates assumption that given Julian and Maya day began at midnight, Greenwich Mean time, and at prior 6 PM in local time, taken as approximate sunset time. Adjust to (.50) for schematic 6 AM sunsets as Maya day-beginning points.

man-Thompson" variant still being defended one should substitute the constant 584,283 and moon age 11.29. The issue is not between correlations which consistently place new and full moons at the same Maya dates, as implied. For the same time of day, whether (.00) or (.50), the B2 correlation places all Maya days 1.23 days behind Smiley Correlation positions in the average lunation; by Schram calculations for the Initial Series base it places that Maya date 1.50 days behind.

Differences of this order must be significant whenever Maya recorded or inferred moon ages are dealt with, as for example, when trying to identify the lunar phase (or phases?) from which Maya moon ages were counted. Possibilities to consider are "Old Moon" and "Visible New Moon" days, and one or two "Dark" or "Dead Moon" days between, and possible shifts from one base to another. For attacks on this unsettled problem see Thompson, 1944; Satterthwaite, 1947, pp. 78-79, 1948, 1949; Thompson, 1950, pp. 236-237; Satterthwaite, 1951. So far as I have made out, Smiley thinks of Maya moon ages as being counted from "Astronomer's" New Moon Day, the day of conjunction. This is, perhaps, in line with his conclusion from his own work, "that we must cease to think of the Mayans as ignorant savages; we must regard them instead as an enlightened people who, in the field of astronomy, were centuries ahead of the rest of the world". Behind this conclusion are speculations that Maya knowledge of astronomy reached back to about 3391 B. C., but in regions south of the equator (Sm2, p. 226).

Turning again to our Table 1, Correlations A2 and the already noticed and still viable B2 are modifications of A3 and B4 respectively, made to gain precise agreement with the historical and present-day 260-day "Sacred Round" cycle. They are precisely 260 vague years apart, and may be distinguished as "early" vs. Later but "intermediate" reactions to identical interpretations of the same ethno-historical evidente. The new correlation Z, together with A1 and Y, belon with the Spinden variants in early group, so far as archaelogical controls are concerned, with or without radiocarbon checks.

If a correlation is correctly founded on non-astronomical evidence it ought to pass correctly formulated astronomical tests. Despite great diversity of opinion as to what such tests should be, there seems to be general consensus that elaborate efforts to supply them for the Spinden correlation must be rejected, and modifying from A3 to A2 would make no difference. Makemson, an astronomer, undertook to vindicate the original Thompson correlation (B4) so far as the astronomical texts of the Dresden Codex are concerned (1943). As noted above, with the two-day shift to B2 a re-examination of these astronomical controls is called for, and a beginning at this has been made by the writer, though this is not yet published (Satterthwaite, n. d.). It is a re-working of Makemson's 1943 approach which will have to be compared with other astronomical interpretations, such as Smiley's. Later I try to carry the re-working process forward a bit.

In devising his tests Smiley considers the recorded Long Count dates 9.9.9.16.0 and 9.16.4.10.8 as the respective epochs for the Venus and for what he calls the "Lunar Table" of the codex. He fails to consider evidence noted by Teeple (1930) and followed in principle by Makemson (1943) that these two tables actually functioned from later, derived epochs. The Teeple interpretation, presented by him as an alternative, will be summarized below. One cannot properly judge the B2 correlation astronomically without considering late Maya epochs, which it certainly requires.

The "Lunar Table" (Dr., pp. 51-58) has a structure which permits it to be set to reach eclipse syzygies, including all locally visible eclipses in a period of 405 moons, and it has therefore generally been held to be an eclipse-predicting table. Smiley refuses this test, calling it a "Lunar Table", the function of which was probably merely "to permit the prediction of new moons over short intervals" (Sm3, p. 224). The seeming eclipse-predicting structure recognized by other astronomers is not mentioned. By eliminating the eclipse test for this table he is able to postulate an apparently other unknown one, which must be referred to in the basic assumption that "the second line on page 24 of the Dresden Codex indicates dates common to a Venus ephemeris and a solar eclipse table" (Sm1, p. 215). I think very few who have worked on the astronomy of the codex will accept these related propositions without full justifying explanation.

Subsequent to her work on the original Thompson correlation (B4), Makemson reverted to the orthodox 9.9.9.16.0 and 9.16.4.10.8 epochs for the two codex tables, making a different assumption from Teeple's for the lunar situation at 9.16.4.10.8, but not denying that it should be a proper epoch for solar eclipse prediction. Smiley notes her assumption that this should be the first of a sequence of solar-lunar-solar eclipses at half-moon intervals (Sm5, p. 7); he does not mention Teeple's trial later epoch on the assumption that it should be near the conjunction of the Sun with the Moon's node, when, I believe, the sequence might be lunar-solar-lunar. Teeple makes no point of the latter possibility (if I am correct); and Makemson is careful to specify that her assumed "run" of three eclipses may be observation records or predictions. It might have been added, I believe, that only one of two solar eclipses bracketing a lunar one could ever have been observed in Maya country. Teeple derives his near-the-node hypothesis from analysis of the Maya table itself, and does not mean to attribute to the Maya knowledge of lunar-solar celestial mechanics as now understood (See Teeple, 1930, p. 86-91; Satterthwaite, 1947, pp. 142-147).

Makemson's correlation (A1) involved choice among many possibilities in order to obtain agreement (within a year) with the "Short Count" evidence, thus placing a Katun 13 Ahau in AD 1535. She went to extreme lengths to explain away some portions only of the ethno-historical evidence for Sacred Round and Vague Year correlation. I suggest that her correlation is as unacceptable from this point of view as was Spinden's from the astronomical point of view (Thompson, 1950, pp. 306-307). But I also suggest that those who want an early correlaiton for archaeological reasons and who believe astronomy alone can establish it, should carefully examine Makemson's A1 solution before accepting the new one.

Correlation Y, by Dinsmoor, offers yet another "astronomical" choice. So far as I know, only the correlation constant has been released for published notice. We may learn a little from this. It agrees almost exactly with Makemson in respect to the average lunation, being (for same time of day) 1.28 days ahead of the Smiley correlation, and 2.51 days ahead of the Modified Thompson 2 (B2) correlation. Experiments with it suggest that Teeple's hypothesis of late Maya epochs for the Venus and the "Lunar Table" as an eclipse table has been accepted. For this reason I suspect that, of the three early correlations Z, A1 and Y, this one (Y) may be best founded on the Maya material itself. One wishes that Professor Dinsmoor, a great scholar in other fields, would publish his foray into Maya astronomy.

Radio Carbon Controls

The three early correlations disagree emphatically with the ethno-historical evidence, except for the Makemson contact with the least specific such evidence, the katun count. But they all fall within 1-sigma limits provided by independent radiocarbon datings of Maya-dated wooden beams from Tikal, first by Kulp (one beam) then by Libby (two beams). Smiley used these results when finding his correlation, but later says they are no longer needed for his thesis. He merely notes that subsequent radiocarbon results by Ralph, for seventeen beams from temples at Tikal "appear to favor the Goodman-Thompson correlation" (Sm1, p. 215; Sm5, pp. 7-8).

The new radiocarbon results appear in Satterthwaite and Ralph, 1960. If valid, they completely reverse the Kulp-Libby picture. The measure of disagreement for any of the early correlations is of the order of 7 or 8 sigma, with Correlation Z at the outer extreme. Thus acceptance of any of the early correlations seems to call for taking one of four positions, or some combination of them:

a) The latest Maya dates in Tikal Temples 1 and 4 are about 260 years earlier than cuttings of the wood on which they were carved.

b) The radiocarbon method as now developed is still unrealiable at intervals of this order, even when applied with special precautions and yielding consistent results for numerous samples in two groups.

c) Ralph's results reflect multiple and serious error in sample-collectiing.

d) They reflect multiple and serious error in the radiocarbon counts and/or the calculations based on them.

Smiley closes his latest paper with the remark that "It seems likely the new correlation may be the true one" after referring to seven Maya dates, claimed to be recorded in the Dresden Codex, which show a Jupiter relationship in his correlation. He says "Of course, these dates might just happen by chance to lie near to Jupiter conjunctions with the sun, but the odds against it are of the order of a billion to one" (Sm5, p. 8; see also Sm4, p. 241, where the odds seem to be given as ten million to one). The existence in the codex of five of Smiley's "Jupiter Dates" will be questioned in another place. To balance such probability figures, which appear to be very impressive to a non-mathematician, is it not proper to ask that the new radiocarbon results be explained away along the lines suggested above? And then is it unfair to ask that the odds be calculated against chance 1-sigma coverage of the B2 correlation, with its trio of ethno-historical agreements?

To facilitate judgment on the merits, Ralph's radiocarbon work has been very fully published, the collecting of samples is described, and the question of contemporaneity of carved dates with the wood itself is fully discussed in the cited paper.

Astronomical Inferences and Assumptions

The writer agrees with Thompson that the correct correlation should follow the weight of the evidence in all available categories, and that this approach now points very strongly toward Correlation B2. However there are those who, with Smiley, believe they can find the true correlation by analysis of supposed Maya astronomical evidence alone. So an examination of hte new and highly unorthodox interpretations of the two codex tables seems in order. After all, a fresh approach and lack of orthodoxy are the conditions for real break-throughs. We shall keep our eye on the B2 correlation as the best alternative, and on what seemed to be break-throughs of thirty years ago by John E. Teeple (1930).

A basic assumption for the new correlation is the reasonable one that the Maya were interested in the relationships of the Venus cycle to Solar Eclipses, plus the more dubious one that this is reflected in the Venus passage on pp. 24:46-50 of the Dresden Codex. The assumption of what may be called "eclipse overtones" in a Venus-structured table imply, I think, a first interest in Venus-Moon relationships. These can be investigated with the "lunar remainders" of Table 2, which is a special-purpose skeleton model of the chronological structure of the actual table. For a full exposition and discussion see Thompson, 1950, pp. 221-29.

The basic unit is a 584-day cycle of four stations which repeat at intervals of 236-90-250-8 days. These intervals appear five times across the bottoms of pp. 46-50; our model gives only the accumulating totals or "position numbers" from a higher row. By consensus the four stations of the cycle define schematic periods of the planet as Morning and Evening Star. The "Venus Zero" ideally should be set four days after Inferior Conjunction, ending the schematic 8-day period of disappearance and beginning the Morning Star period. In this paper Venus ages or "deviations" are from the Maya "zero", not from the midpoint of the 8-day period which should ideally be at inferior conjunction. Deviations are according to an expansion of Spinden's table which in turn is based on a ms. table by the astronomer Willson. Results are claimed only to be "sufficiently accurate for our purposes" (Spinden, 1930, p. 81).

The 584-day Maya period will here be called that of the "Vague Venus Cycle" to distinguish it from the slightly shorter average period of 583.92 days. As the vague cycle accumulates average error this must change one's position in the period of actual variation from average, which I take to be of the order of ± 3.5 days. The variation is surely much greater than that of the moon, here taken as ± 0.59 days. The accumulation of average Venus error may be quickly found with the Venus remainders of Table 2.

The reference scheme at the left in the table saves much space in discussion, makes for precision, and brings out patterns. Station D ends the vague cycle at "Venus Zero", beginning the Morning Star period which ends at Station A; similarly Stations B and C define the Evening Star period. Numbers (1A, 2A, etc.) are added to station letters in order to show positions in the five sequent 584-day cycles through which the lettered stations repeat with different augural descriptions. One infers such a function for non-calendrical glyphs associated with each of the twenty stations by being placed in the same columns as the station-dates themselves. Among them are two sets of world direction glyphs. All such glyphs are represented by "GG" in the table. Station D, the "Zero" station of the 584-day cycle, seems more important than the others by mere inspection, being followed each time by prominent panels of three groups of glyphs and three pictures of gods. ("GG. PP" in the table).

The cycle thus described covers 8.2.0 (2920) days, at which distance Stations A-D, considered as dates, have completed one round with the cycle of 20 Day Names and also with the 365-day Vague Year cycle. Hence each station then repeats at the same Day Name and Vague Year position, but with a different Day Number. A thirteen-row table of Sacred Round dates, and multiples of 8.2.0 on p. 24, provide for following the complete round with the Day Numbers. If the changing Day Numbers affect the augury, there is no written indication. Obviously the recorded auguries apply repeatedly; and if so, no "eclipse overtones" can be indicated by undeciphered glyphs, except, conceivably, in the 3-column "Introduction" on p. 24. Here the first two columns fix an epoch for the table at 1 Ahau 18 Kayab in "Ring Series" style, counting from the last 1 Ahau 18 Kayab before the Initial Series base. There is reason to believe Smiley would not read thus, but this is not important here. The third column gives the same epoch in Initial Series form, 9.9.9.16.0 1 Ahau 18 Uo, the 18 Uo being an obvious mistake for 18 Kayab. However fixed, Smiley agrees that an epoch for the 584-day cycle was recorded here, this being 9.9.9.16.0 1 Ahau 18 Kayab.

Our reference scheme numbers the multiples of 8.2.0 up to the 12th, in descending order as in the codex. Proceeding in reverse (upward order) we pass over four "aberrant" numbers (Ab1-Ab4), which do not fit the pattern. In the top row we find the first to fourth multiples of the period of the complete round of the 8.2.0 cycle, the first being 5.5.8.0 (37,960) days. These also are in descending order for normal reading (IV-I in the table).

It is difficult to avoid the conclusion that, aside from the aberrant numbers, we have a "ready reckoner" for following the Vague Venus cycle through five complete rounds, reaching any desired position, counting from the epoch, out of a total of 1300 positions for the 20 described stations. The first example of Table 3 illustrates how one may find the precise distance at nearly the maximum coverage-maximum if we confine ourselves to using only one of the given multiples of the complete round. The amount of arithmetic operations with counters is no more than three additions (or subtractions if the problem is Venus position at a given Long Count Date). The example also illustrates our reference scheme. We could have written

TABLE 2

CALCULATION TABLE FOR 584-DAY "VAGUE" VENUS CYCLE
(Based on Dresden Codex, pp. 24; 46-50)

	Ref. Scheme	Epoch (Epochs?) in "Introduction" (first 3 columns)					
P.24,	RS(RN1)	1 Ahau	18 Kayab	GG	6. 2. 0	4 Ahau	8 Cumku
	RS(DN)	("	")	GG	9. 9.16. 0. 0	1 Ahau	18 Kayab
	IS	4 Ahau	8 Cumku	GG	9. 9. 9.16. 0	1 Ahau	18 Kayab(18 Uo)
						Remainders	
		Completed "5.5.8.0" Rounds with Day Numbers			GG,PP	Av.Venus	Av.Moon
P.24,	IV/ 0/5D	1. 1. 1.14. 0	1 Ahau	(V.year	-	+20.80	+23.237
1st	III/ 0/5D	15.16. 6. 0	1 Ahau	positions	-	+15.60	+10.045
Row	II/ 0/5D	10.10.16. 0	1 Ahau	of start)	-	+10.40	+26.384
	I/ 0/5D	5. 5. 8. 0	1 Ahau		-	+ 5.20	+13.192
		Completed "8.2.0" (2920-day) Rounds					
	0/12/5D	4.17. 6. 0	6 Ahau		-	+ 4.80	+16.720
P.24,	0/11/5D	4. 9. 4. 0	11 Ahau		-	+ 4.40	+20.249
	0/10/5D	4. 1. 2. 0	3 Ahau	(Vague	-	+ 4.00	+23.777
3rd	0/ 9/5D	3.13. 0. 0	8 Ahau	Year	-	+ 3.60	- 2.226
	0/ 8/5D	3. 4.16. 0	13 Ahau	positions	-	+ 3.20	+ 1.303
to	0/ 7/5D	2.16.14. 0	5 Ahau	of	-	+ 2.80	+ 4.832
	0/ 6/5D	2. 8.12. 0	10 Ahau	start)	-	+ 2.40	+ 8.360
5th	0/ 5/5D	2. 0.10. 0	2 Ahau		-	+ 2.00	+11.888
	0/ 4/5D	1.12. 8. 0	7 Ahau		-	+ 1.60	+15.417
Rows	0/ 3/5D	1. 4. 6. 0	12 Ahau		-	+ 1.20	+18.948
	0/ 2/5D	16. 4. 0	4 Ahau		-	+ 0.80	+22.474
	0/ 1/5D	8. 2. 0	9 Ahau		-	+ 0.40	+26.002
		Described 8.2.0 Round with Day Names and Vague Year					
P.46	0/ 0/1A	11. 6	(+ 2) Cib	8 Zac	GG		- 0.245
	0/ 0/1B	16. 6	(+ 1) Cimi	18 Muan	GG		+ 1.164
	0/ 0/1C	1.10.16	(+ 4) Cib	4 Yax	GG		+14.919
	0/ 0/1D	1.11. 4	(+12) Kan	12 Yax	GG	+ 0.08	+22.919
					GG,PP		
P.47	0/ 0/2A	2. 5. 0	(+ 1) Ahau	3 Zotz	GG		+22.674
	0/ 0/2B	2. 9.10	(+ 0) Oc	13 Mol	GG		+24.082
	0/ 0/2C	3. 4. 0	(+ 3) Ahau	18 Uo	GG		+ 8.307
	0/ 0/2D	3. 4. 8	(+11) Lamat	6 Zip	GG	+ 0.16	+16.307
					GG,PP		
P.48	0/ 0/3A	3.16. 4	(+ 0) Kan	2 Muan	GG		+16.062
	0/ 0/3B	4. 2.14	(+12) Ix	7 Pop	GG		+17.471
	0/ 0/3C	4.15. 4	(+ 2) Kan	17 Mac	GG		+ 1.695
	0/ 0/3D	4.15.12	(+10) Eb	5 Kankin	GG,PP	+ 0.24	+24.695
P.49	0/ 0/4A	5. 9. 8	(+12) Lamat	16 Yaxkin	GG		+ 9.450
	0/ 0/4B	5.13.16	(+11) Etznab	6 Ceh	GG		+10.859
	0/ 0/4C	6. 8. 8	(+ 1) Lamat	11 Xul	GG		+24.614
	0/ 0/4D	6. 8.16	(+ 9) Cib	19 Xul	GG	+ 0.32	+ 3.083
					GG,PP		
P.50	0/ 0/5A	7. 2.12	(+11) Eb	18 Cumku	GG		+ 2.839
	0/ 0/5B	7. 7. 2	(+10) Ik	0 Zec	GG		+ 4.247
	0/ 0/5C	8. 1.12	(+ 0) Eb	10 Kayab	GG		+18.000
	0/ 1/5D	8. 2. 0	(+ 8) Ahau	18 Kayab	GG	+ 0.40	+26.000
					GG,PP		

KEY TO ABBREVIATIONS: Ring Series (RS); Ring Number, first position (RN1); Distance Number (DN); Glyphs, non-calendrical (GG); Pictures (PP).

NOTES: Upper and Lower vague year dates omitted: numbers in parentheses are increases in Day Numbers.

the total distance as "IV/12/5C 1.6.7.3.12" without losing awareness of the specific composite nature of this distance number, as found. We are at Station 5C after the 12th described round and the fourth complete round were completed. Reversing the problem, if asked for the Long Count position of this station-position we would proceed as in the example of Table 3. Had we wished the Long Count position for 12/5C *before* completion of the *first* complete round we would refer to it as position 0/12/5D, counting from the epoch, as before. In this scheme D marks the end of a 584-day cycle, and 5D also marks the end of an 8.2.0 "round" cycle. They are "zeros of completion" but it seems better to use the letter here as elsewhere. It reminds us of our position in the last column of p. 50. Thus our "absolute zero" is 0/0/5D, not 0/0/0. Smiley's Venus manipulations do not extend beyond 0/11/2C, counting from 0/0/5D.

TABLE 3

EXEMPLES OF USE OF VAGUE AND CORRECTED VENUS EPOCHS

IS	9. 9. 9.16. 0	1 Ahau	18 Kayab	0.00
IV/	1. 1. 1.14. 0	1 Ahau		+20.80
12/	4.17. 6. 0	6 Ahau		+ 4.80
5C	8. 1.12	6 Eb	10 Kayab	+ 0.32
IV/12/5C	10.15.17. 1.12	6 Eb	10 Kayab	+29.92
IS	9. 9. 9.16. 0	1 Ahau		0.00
CEV1/	19. 9. 5. 0	1 Ahau		- 0.80
12/	4.17. 6. 0	6 Ahau		+ 4.80
5C	8. 1.12	6 Eb		+ 0.32
CEV1/12/5C	10.14. 4.10.12	6 Eb	(10 Pax)	+ 4.32

In the first example of Table 3 we also used the average Venus-Cycle remainders of Table 2 to find the *accumulated* average Venus error at the distance 1.6.7.3.12, finding it to be the grossly inaccurate 29.92 days. Yet if we do not infer that the Maya made calculations of position in the Vague Venus cycle at distances of this order we have no explanation of the four multiples of 5.5.8.0 (I-IV). On the principle that the best explanation of a table is that which explains the most of it we may infer that such calculations were made, but the size of the error suggests that the aberrant numbers Ab1-Ab4 (Table 2), which do not fit the vague cycle pattern, may give the clue to a scheme for correcting such accumulated error. So far as I know Teeple was the first to see that this might be so. The result was what I shall call a scheme of "Corrected Venus Epochs". This scheme must be understood before one is equipped to judge whether Smiley should dismiss it without discussing it. This he does by implication, without even mentioning the name of a giant in the long history of this problem, except in a list of published sources. To make good this lack I shall try to explain the Teeple correction thesis, though in my own way and with my own terminology. Involved also are minor elaborations of it, one by Thompson, the other first presented here.

In the Teeple Venus correction scheme error in the vague cycle is not taken out a day at time; it is merely held within limits by making corrections when they will return one to can be set as an approximate eclipse interval. I think Smiley's reasoning is that the aberrant number is a separate statement of the composite number for Station 11/2D, and that it implied to the Maya that calculation by some eclipse table shared this approximate average reality at what we here call a "Corrected Venus Epoch" ("CVE") — this being at 1 Ahau, like the recorded and implied later epochs for the vague cycle. This presumed esoteric "1 Ahau" requirement leads of necessity to making corrections of a vague cycle Station D position by 4 days or its multiple. Alternatively, it may be said that one makes corrections of the complete round period (5.5.8.0) by 6.9.0 or its multiple. The 4-day and 8-day corrections needed for long-distance returns to accuracy are indicated in both styles below.

4-day or	12/1D	4.18.17.4 (35,624) 5 Kan	5. 5. 8.0
6.9.0	(0/5D)	— 4	— 6. 9.0
correction		4.18.17.0 (35,620) 1 Ahau	4.18.17.0
8-day	11/2D	4.12. 8.8 (33,288) 9 Lamat	5. 5.8.0
or 13.0.0	(0/5D)	— 8	— 13.0.0
correction		4.12. 8.0 (33,280) 1 Ahau	4.12.8.0

Table 2 shows that the error at 12/1D is 4.88, that at 11/2D is 4.56. Ideally (under the 1 Ahau requirement) an 8-day correction (having a net effect of — 3.44 days) should be used when four 4-day corrections have piled up a corrected-epoch error of + 3.52 days.

In favor of this far-from accurate scheme it should be noted that the complete Sacred Round table and associated augural statements are unaffected, except that when one makes a correction he "leaps forward" by 6.9.0 or 13.0.0 across uncounted "lost" stations after 12.1 D (61st cycle) or after 11/2D (57th cycle). This is perhaps roughly analogous to the "lost" 10 Julian Calendar dates caused by the Gregorian adjustment of the Christian year, in 1582. That did not affect the ceremonial values of the days of the Christian week, or of the same dates in the new style year.

We have seen that no "special dates" can have been marked on pp. 46-50. The Teeple correcting interval 4.12.8.0. appears on page 24 (Ab 3 of Table 2), and is an important bit of evidence for the scheme. From Teeple's point of view it is mere coincidence that this equals the position of 11/2D (I Ahau 18 Uo) in the table proper, to be obtained by adding two given numbers. Granted an 8-day backward correction from a Station 11/2D, the corrected number must be equivalent to the uncorrected position number for 11/2C, because of the 8-day interval between Stations C and D. Also coincidence is the fact the aberrant number 4.12.8.0., one of four, is within about 1.00 day of being an average lunar interval which interval, so set that the Venus epoch and this "special" Venus station were reached by it. Even if such an interpretation is granted, it is not clear that the Teeple correction scheme must be rejected, as it has been. Smiley offers no evidence for the eclipse nature of this number in Maya thinking, other than the number itself, and his use of it to find correlations which permit the idea. And he does not give notice that another meaning has been proposed for it.

Teeple saw specific evidence in the codex, still to be noted, for Maya use of his 4-day and 8-day corrections in the proportion of 1:2, leaving it unsetted whether a fuller record would reflect the ideal 1:4 proportion. Thompson corrects Aberrant Number 4, adding 13.0 to 1.5.5.0 to obtain 1.6.0.0., and uses the latter as evidence for the ideal proportion. This is, perhaps, a good place to note that if the vague and corrected Venus epoch systems coexisted, the two systems could have only one common epoch at 1 Ahau 18 Kayab, and by "late Teeple epoch" we really mean "*late common epoch*", at IV/0/5D of Table 2, rather than at 0/0/5D. Thompson, adopting what we call the late common epoch, projected the 1:4 pattern back, finding a CVE 1.6.0.0 after the recorded vague cycle epoch 9.9.9.16.0 (1950, p. 266). Smiley needs this same number as an approximate eclipse interval, for an elaboration of his "eclipse overtone", but merely hints at the possibility that it should have been found in the codex. Granting the correction, his thesis rests on only two given numbers, and both have been given other meanings under the corrected epoch hypothesis.

Curiously, Thompson endows the Maya of codex times with knowledge of the ideal 1:4 Venus correction period, but denies them knowledge of the corresponding ideal Lunar Correction period for the same esoteric control. (Thompson, 1950, pp. 235-236). The ideal lunar proportion is 1:5, i. e. five periods of 1.13.4.0 (11,960) days to one shorter period of 1.9.11.0 (10,660) days — the cycle of corrected epochs covering 9.15.13.0 (70,460) days. Considering the comparative shortness of the lunar cycle and its lesser variation from average I submit that the same basic approach to the correction problem ought to produce the ideal equation for the lunar cycle, long before it could be averaged out for the Venus cycle. Further, I think there is very suggestive evidence for the 9.15.13.0 lunar cycle in the codex (Satterthwaite, n. d.). For this reason Table 4, with the common epoch at 1 Ahau 18 Kayab, expands the Teeple Venus correction scheme one point beyond the ideal, because then the Venus correction cycle, with an average Venus

TABLE 4

HYPOTHETIC "1 AHAU" CORRECTED VENUS EPOCH CYCLE AND "ABERRANT" NUMBERS OF DRESDEN CODEX

						Remainders	
	Intervals	Ref.	Positions in Cycle of Corrected Epochs			Av. Venus	Av. Moon
		CVE6	0. 0. 0. 0	1 Ahau	18 Kayab	0.00	0.000
	4.12. 8. 0	CVE1	4.12. 8. 0	1 Ahau	18 Uo	-3.44	- 0.975
	4.18.17. 0	CVE2	9.11. 7. 0	1 Ahau	13 Mac	-2.56	+ 5.134
1:2	4.18.17. 0	CVE3	14.10. 6. 0	1 Ahau	3 Xul	-1.68	+11.242
	4.18.17. 0	CVE4	19. 9. 5. 0	1 Ahau	18 Pax	-0.80	+17.350
1:4	4.18.17. 0	CVE5	1. 4. 8. 4. 0	1 Ahau	8 Çhen	+0.08	+23.459
1:5	4.18.17. 0	CVE6	1. 9. 7. 3. 0	1 Ahau	18 Pop	+0.96	+ 0.037
			"Aberrant" Numbers (Dr. p. 24)				
		Ab1	1. 5.14. 4. 0	1 Ahau		+ 17.36	+22.262
		Ab2	9.11. 7. 0	1 Ahau		- 2.56	+ 5.134
		Ab3	4.12. 8. 0	1 Ahau		- 3.44	- 0.975
		Ab4	1. 5. 5. 0	1 Ahau		+242.72	- 4.578

NOTE: Zero Venus and Moon Ages at start of cycle are arbitrary; values given are changes from actual average ages at the start.

error of only about one day, meshes with the Lunar Correction cycle at its third multiple, 1.9.7.3.0. The second example of Table 3 is valid for either the 1:4 or the 1:5 scheme, and makes comparison with the corresponding calculation with the vague cycle.

Below the tabulation of the 1:5 Venus scheme in Table 4 we give the four aberrant numbers of p. 24, two only of which have been noticed thus far. Correction of the 1.5.5.0 as a Maya mistake seems hazardous and I prefer to leave it unexplained; but note that Ab3 as well as Ab2 has a meaning in the correction scheme, whether the proportion is 1:2 or better. Here, juxtaposed in the descending order used for the multiples of the complete round and those of the described cycle, and also without descriptive glyphs, we find the first two CVE positions of the postulated scheme, 9.11.7.0 and 4.12.8.0.

What are the chances against this being mere coincidence?

Teeple's late epoch hypothesis does not involve preliminary adoption of a correlation. It is founded on the first aberrant number 1.5.14.0 which, like the second, 9.11.7.0, has no apparent function in the Smiley correlation. The larger number gives the distance from the early vague-cycle epoch to the epoch of an 8-day correction when, it may be supposed, known accumulated error in the 4-day corrections themselves, was canceled out. If we adopt this interpretation —and no other is available so far as I know— we should look for a correlation which puts 9.9.9.16.0 (1 Ahau 18 Kayab) unrealistically earlier than actual Venus Zero, but puts 10.15.4.2.0 (1 Ahau 18 Uo) close to it.

Table 5 first shows the relationships between the early and "late common" 18 Kayab epochs and the corrected 18 Uo epoch, without benefit of assumed correlation. Below we give their approximate Venus and Schram lunar deviations according to the B2 (.50) correlation. Note that the 4.12.8.0 is just as good a lunar interval between Last Quarter Days as between Eclipse days, and that in this correlation, as we move from the earliest vague-cycle epoch to the common epoch, both at 18 Kayab, we move from a "Dark Moon" day (actually Conjunction Day) to a day which could have been correctly observed as Last Quarter day.

The lunar relationships may of course be coincidence, but choice of a base at Last Quarter might well have a special meaning. "Old Moon" Day, which precedes Dark Moon days, some of them potencial eclipse days, must have been much more difficult to observe correctly than was Last Quarter Day. That phase may well have been regularly observed for as a check on whether, in a particular lunation, Old Moon day was going to agree with prediction, or fall on the day before or after it. There is, of course, no suggestion that these lunar rather than eclipse "overtones" are indicated in the codex. But they follow from a correlation which does not depend on them, except that I have chosen Correlation B2 (.50) rather than B2 (.00) so as to throw the moment of Last Quarter within the 1 Ahau 18 Uo date, rather than within the day before.

TABLE 5

RELATIONSHIPS OF VENUS EPOCHS

(Early Vague, Late Teeple Common and first Teeple Corrected Venus epochs at 1 Ahau with their lunar and Venus cycle positions in the B2 (.50) correlation.)

Early Vg. Ep.		9. 9. 9.16. 0		9. 9. 9.16. 0	18 Kayab
	Ab1	1. 5.14. 4. 0	IV/	1. 1. 1.14. 0	
Common Epoch				10.10.11.12. 0	18 Kayab
			Ab3	4.12. 8. 0	
Cor. Ep. 1		10.15. 4. 2. 0		10.15. 4. 2. 0	18 Uo

			Venus	Moon	
9. 9. 9.16. 0	1 Ahau	18 Kayab	-15.1	- 0.89	Lun. Conj.
10.10.11.12. 0	1 Ahau	18 Kayab	+ 4.7	+22.15	Lun. LQ
10.15. 4. 2. 0	1 Ahau	18 Uo	- 0.5	+20.84	

NOTE: Ref. symbols as in Table 2; Lunar deviations per Schram table calculation; Venus deviations for "Venus Zero" 4 days after conjunction, per table based on Spinden 1924.

The late Maya epoch setting is not a two-day shift from that adopted by Makemson when investigating the B4 correlation. It does very nicely for the B2 correlation, and is the only one possible for any correlation, if three instead of two of the four recorded aberrant numbers are to be explained. There is no claim that Smiley could not accept the Teeple correction scheme and the late common epoch, and have a differing but in the long run equally accurate set of corrected epochs. Note, however, that he cannot accept this scheme and then justify the assumptions on which his correlation rests. The impressive evidence for the correction scheme must be rejected, like the evidence for the eclipse-predicting nature of the "Lunar Table".

We have paid scant attention to the fact that a 4-day or an 8-day correction changes the repeating set of vague year positions at the twenty stations, moving it backward by four or by eight days. This is the effect on the disembodied pattern, but it is not quite the whole story. Reference is made to the tabulation on p. 263 and the 8-day correction as an example. We do not merely reach a corrected Station 11/2D at 1 Ahau and, in this case, at 18 Uo. We decree that we have cut short the round with the 13 Day Numbers, and are at a corrected Station 0/5D. So we should slide the new series of year dates to the right by three 584-day cycles, so that our 1 Ahau 18 Uo is in the last column, that of the uncorrected 0/5D position at 1 Ahau 18 Kayab. Similarly, with a 4-day correction made at position 12/1D, we should move the pattern by four cycles, to give it the desired position 0/5D.

The same effect is obtained by reducing the old vague year positions for each of the stations by constant amounts. These are 300 (15.0) days for an 8-day or 13.0.0 correction, and 150 (17.10) days for a 4-day or 6.9.0 correction. This rule, starting from 18 Kayab as the common epoch, gives the year positions for the successive corrected epochs of Table 4. For a complete "ready reckoner" covering the distance there indicated one should apply the rule six times, and to each of the twenty stations, recording the 18 Kayab row and also six others ending at 18 Uo, 13 Mac, 3 Xul, and so on.

It is clear that if, for space-saving or some other reason, one row only was omitted, the rule could be quickly applied mentally at the station required by a particular problem — *provided* one knew whether to retreat 300 or 150 days from the station of the next earlier and recorded row. For this one must know the position of the corrected epoch being used with reference to the last 8-day correcting epoch. In fact the codex omits the 18 Uo row, giving the 13 Mac and 3 Xul rows as upper and lower rows, with the 18 Kayab row as a middle one. Nevertheless a mistaken 18 Uo as part of the terminal date of the epoch in Initial Series style suggests that 18 Uo was in mind when page 24 was painted. This is fair confirmatory evidence that the first and third aberrant numbers, on the same page, were supposed not only to reach 1 Ahau, but also 18 Uo, as required by Teeple's 8-day and 4-day correction scheme. Note that space was lacking for recording 18 Uo in the proper places.

Absence of the 18 Uo line of twenty vague year positions for the 8.2.0 cycle led Makemson to reject this part of Teeple's hypothesis (1943 and 1947), thus leaving the mistake and two of the aberrant numbers unexplained. It seems preferable to see meaning in the mistake and in the two numbers, and to try to explain omission of the 18 Uo row. I suggest that the 18 Kayab row was needed for Vague Venus Cycle calculation at very considerable distances into the user's past, in the form of forward counting from the 9.9.9.16.0 epoch. On the other hand, space was at a premium, and calculations in the corrected system were seldom made behind the 13 Mac corrected epoch. If this was occasionally necessary the simple mental calculation of the year date was made. The 3 Xul corrected epoch was in the recent past, with coverage extending into the future, as far as would ordinarily be needed. Hence the 3 Xul row is the latest of the series given.

The 18 Uo corrected epoch was nevertheless located in the Long Count by the first aberrant number 1.5.14.4.0, thus relating it to the earliest vague cycle epoch; while the third aberrant number, 4.12.8.0, told the reader more emphatically that this was a major 8-day correction point, the first after the common epoch. It was necessary to know this in order to lock the whole corrected epoch cycle into the long count, and permit projecting it forward or backward as special and probably rare problems might require. As noted before Thompson projects the 1:5 pattern backward to a point 1.6.0.0 after 9.9.9.16.0, at 9.10.15.16.0. 1 Ahau 8 Zac. I doubt if this was much of a chore for him, or that it would be such for the Maya. But in either case it would require knowing where some one 8-day correction was made, the natural one being that following the common epoch.

If this line of thought seems acceptable, it does not explain why the aberrant series gives the 9.11.7.0 cycle-position of the 13 Mac epoch, but not the 14.10.8.0 for the 3 Xul row (see Table 4). No satisfactory explanation occurs to me. But it could very easily have been calculated, and it may have been squeezed out of the codex by the fourth aberrant number, 1.5.5.0 — whether this is left unexplained or is corrected to

1.6.0.0 in accordance with Thompson's theory for it. The layout of the page allowed room for only four of these special distance numbers.

In terms of the correction scheme the three recorded rows of vague year dates might be called "separate ephemerides", to borrow Smiley's phrase. Each is valid in counting from a particular corrected epoch only. Rejecting this thesis without discussion, Smiley suggests "that they were designed to give dates of solar eclipses preceding and following solar eclipses associated with some special dates in the Venus ephemeris". (Sml, p. 215) Interpreting, I think the middle or 18 Kayab row on pp. 46-50 becomes the one and only "ephemeris", and here, as we have seen, no "special dates" can have been actually indicated by the Maya. Use of the word "associated" permits one to assume an eclipse "overtone" though the eclipse may have fallen near but not precisely on the "special date". As stated, there is no requirement that the eclipse was visible to the Maya, except at the epoch 9.9.9.16.0 itself.

To implement this surprising thesis, only the epoch itself and Station 0/11/2D at 18 Uo are utilized, presumably because it can be argued that they are specially singled out on p. 24. If we are restricted to these two "special dates" in the 18 Kayab "ephemeris", we have two "trios" of eclipse or near —eclipse dates, each trio at same day in the Sacred Round— in this particular case the day being I Ahau for both trios. The 18 Uo trio is far advanced in the complete 5.5.8.0 round, where the accumulated average Venus error is over 4.40 days. Smiley calls this position "Somewhat later" than the epoch (Sml, p. 215).

The two trios are given in Table 6, in what seems to me to be the logical order, with Smiley's Roman numbers for the six dates at the left, and Julian Day Numbers per Correlation Z (.00) next to the right. Here also we show the intervals which Smiley thinks existed between the three rows of vague year dates, in decimal notation. To obtain Dates IV-VI, for example, one must first find the LC date V (a Venus ephemeris station); then we go back to Date IV; returning to Date V one then goes forward to Date VI. One needs to know that the respective intervals to be used are 1.6.0.0 (9360) days and 1.13.4.0 (11,960) days. In a restatement of this theory Smiley hints that the aberrant 1.5.5.0 might be a mistake for the needed 1.6.0.0, and says that the 1.13.4.0 may be found in the introduction to the "Lunar Table" — i. e. not here, where it is required for the theory. (Sm4, pp. 238-239). In fact two out of four digits must be changed to obtain the first multiple of 11,960 in the "Lunar Table" introduction. Such a correction has been postulated before, but it seems dubious.

Now, assuming this "eclipse trio" idea is not rendered mechanically possible by mere coincidence, it cannot possibly explain more than a few of the forty entries comprising the 13 Mac and the 3 Xul rows. Using the average lunar remainders of Table 2, and ± 2.00 days of leeway, will show that in one complete 5.5.8.0 round only 33 stations can be loosely classed as at "same moon age" as the epoch; finding their Long Count and Julian Day Numbers in the Z (.00) correlation, and then checking against Oppolzer's *Canon* shows that in this correlation only five of them are eclipse or near-eclipse dates. This, I suggest, is fatal to this "eclipse trio" theory for the upper, middle and lower rows of vague year dates.

TABLE 6

SMILEY'S DATES I-VI, AT 1 AHAU

					Venus	Moon (S.Ecl.)
	III	9.11. 3. 2. 0	13 Mac	1,859,019	+271.3	-0.4
				- 11,960		
Epoch	II	9. 9. 9.16. 0	18 Kayab	1,847,059	- 0.5	-0.7
				- 9,360		
	I	9. 8. 3.16. 0	3 Xul	1,837,699	+588.4	+0.4
	VI	9.15.15.10. 0	18 Pax	1,892,299	+276.9	-1.0
				- 11,960		
0/11/2C	V	9.14. 2. 6. 0	18 Uo	1,880,339	- 4.8	-1.3
				- 9,360		
	IV	9.12.16. 6. 0	8 Chen	1,870,979	+563.3	-0.5

In a re-statement of it Smiley adds some details which I probably do not fully understand. The 9360-day interval is not only said to be a close approximation of a solar eclipse interval 9361.20 days, but a "clear" approximation of 16 Venus cycles at 584 days each, i. e. 9,344 days. The 11,960 day interval, very close to a solar eclipse interval of 11,959.89 days, becomes also "an evident approximation of 20.5 × 584 = = 11,972" days. If the complete rows are explained at all, this must be in the following paragraph (Sm4, pp. 238-239) where Smiley sees inferior and superior Venus-conjunction relationships between the rows. In Table 6 I have given the actual Venus ages from "Venus Zero", 4 days after conjunction, for Dates I-VI, according to the table based on Spinden's. These might help in checking this "Venus conjunctions" approach, which is not worked out in any detail. To the writer it does not look promising as a substitute for Teeple's correction scheme. I do not claim to understand it fully.

In Table 6 I have also added the data on eclipse visibility in the Maya area according to Willson's list. In obtaining his correlation Smiley required that Date II, the 9.9.9.16.0 epoch, should be at a locally visible eclipse (Sm1, p. 215; Sm2, p. 223). The date selected was a winter solstice one, December 21, A. D. 344 (Sm5, p. 8). In Sm4, p. 242, the caption for a drawing confirms Sm1, p. 215, stating that this was an *annular* eclipse, a spectacular phenomenon which might help explain its selection by the Maya; but the drawing also seems to show that it was visible only well out to sea except where the trajectory just touches Northwestern Mexico. Willson's list makes it visible to the Maya, with magnitude p. 5. A clarification would be helpful for laymen like myself. I suppose that, *weather permitting*, this eclipse was seen by the Maya, or at least the more southerly Maya, but not as a particularly spectacular one. However this may be, I think the question arises whether the new correlation depends in part on good weather during a particular December morning, well before the end of the rainy season.

It should be noted that, as I understand it, putting the 9.9.9.16.0 Venus epoch at (or very close to?) a winter solstice permited choosing between two alternative correlations allowed by the other assumed controls, including local visibility. The number 9.9.9.16.0 itself is analysed into 3735.5 solar year approximations as evidence for this solar year element in the Maya record (Sm2, pp. 222-223). This proposition will not be discussed here. A proper judgment requires a preliminary discussion of two incompatible interpretations of "Ring Numbers" and their accompanying Distance numbers, in this case 6.2.0 and 9.9.16.0.0, in the first two columns of p. 24. Pertinent also are the supposed "Determinant" dates of the inscriptions on stone, as recognized by Teeple and Thompson, and one's view of Maya accuracy, or lack of it, in solar year calculation.

Summary and Conclusions

The new correlation has been compared rather superficially with certain others, making use of the labeling system of Table 1. In this the letter "Z" is assigned to the Smiley correlation. A new refinement permits one to specify the time of day, in Greenwich Mean Time, at which the Maya days are assumed to begin. Most correlationists leave this unsettled, thus allowing for alternative assumptions when judging their evidence.

The conclusion of the paper is that the great weight of present evidence in all categories except the purely archaeological one points very strongly to the "Modified Thompson 2" or "B2" variant of the 11-16 "intermediate" correlation, hence against a new "early one". Purely archaeological data apparently cannot resolve the issue — at least not in the present state of divided opinion.

Radiocarbon controls can be most precisely brought to bear only when applied to Maya-dated beams from Tikal, and we have noted early results favoring an early correlation, and a more ambitious later program favoring an intermediate correlation. Smiley first used the earlier findings as a control, but his present position appears to be that it is proper to operate in an astronomical vacuum, so to speak, without considering radiocarbon or any other category of evidence, including the ethno-historical evidence. His correlation disagrees emphatically with all of the latter, as well as with the latest radiocarbon datings of Tikal beams.

If we accept this restriction to astronomical assumptions and interpretations of the actual Maya record, the suggestion here is that the new correlation is unacceptable on these grounds alone. Without discussion, we are asked to reject what most students, including other astronomers, have considered as well established. For example, the eclipse-prediction function of the "Lunar Table" is denied without any substitute explanation of its apparent eclipse-syzygy structure. In its place an undefined "eclipse table" seems to be assumed on behalf of the Maya, so set in time that it shared the interval 4.12.8.0 with the Venus table.

Teeple's correction scheme for the Venus table is rejected, again without discussion, and also without any hint of a substitute, though we are also asked to think of the Maya astronomers as ahead of their contemporaries in the rest of the world. It seems to me that the Teeple correction scheme is very well founded in the Maya record, as has been shown, and that it is of paramount importance for the correlation problem. I think an understanding of how it works is essential, and it is hoped that Table 2, and our use of it, will make this reasonably clear for those who understand the Long Count, but have only a hazy idea of the astronomical tables of the Dresden Codex.

It has been noted that Teeple explains three "aberrant" numbers of the Venus table, while Smiley interprets only one of them. Teeple explains all forty vague year dates comprising an upper and lower row of such entries. Smiley explains only four of them with an "eclipse trio" theory which calculation shows could not possibly explain half of them. Such meagre results *vis-a-vis* the actual record do not indicate to a non-mathematician that it was Teeple who became bemused by entries which fitted his hypothesis only by mere chance.

One feels safer in urging rejection of a purely astronomical correlation if he has a substitute which makes sense in the astronomical category also. Hence I have engaged the corrected Venus epoch cycle in real time according to the B2 (.50) correlation. It functions there very well, with interesting but possibly coincidental lunar results. We do not here undertake to find a late epoch for the eclipse table, though a believable one should be found, if the correlation is correct. The Teeple scheme has been expanded beyond the ideal point adopted by Thompson because we are then returned to almost exactly the same average moon age. For the present this may be regarded as a mere experimental hypothesis which might lead to something recognizable in the record as a whole. There is no hint of it in the Venus table itself.

REFERENCES

MAKEMSON, M. W.: The Astronomical Tables of the Maya. *Carnegie Institution of Washington, Contributions to American Anthropology and History*, No. 42, pp. 183-221. Washington. 1943.

— The Maya Correlation Problem. *Publications of the Vassar College Observatory*, No. 5. Poughkeepsie. 1946.

SATTERTHWAITE, L.: Concepts and Structures of Maya Calendrical

Arithmetics. *Museum of the University of Pennsylvania, Philadelphia Anthropological Society*, Joint Pub. No. 3. Philadelphia. 1947.

— Further Implications of Thompson's Readings of Maya Inscriptions at Copan. *Actes du XXVII^e Congres International des Americanistes*, pp. 467-493. París. 1948.

— Moon Ages of the Maya Inscriptions: The Problem of their Seven-day Range of Deviation from Calculated Mean Ages. *The Civilizations of Ancient America, Proceeding of 29th International Congress of Americanists,* Vol. 1, pp. 142-154. Chicago. 1951.

— Calendrics of the Lowland Maya (ms. prepared for Middle American Handbook). N. D.

SATTERTHWAITE, L. and E. K. RALPH: New Radiocarbon Dates and the Maya Correlation Problem. *American Antiquity*, Vol. 26, No. 2, pp. 165-184. Salt Lake City. 1960.

SCHRAM, ROBERT: *Kalendariographische und Chronologische Tafeln.* Leipsig. 1908.

SMILEY, CHARLES H.: A New Correlation of the Mayan and Christian Calendars. *Nature*, Vol. 188, No. 4746, pp. 215-216. London and New York. 1960. (Sm1)

— The Antiquity and Precision of Maya Astronomy. *Journal of the Royal Astronomical Society of Canada*, Vol. 54, No. 5, pp. 222-226. Toronto. 1960. (Sm2)

— The Paths of Solar Eclipses. *Journal, Royal Astronomical Society of Canada*, Vol. 55, No. 5, pp. 211-217. Toronto, 1961. (Sm3)

— Bases Astronómicas para Una Nueva Correlación entre los Calendarios Maya y Cristiano. *Estudios de Cultura Maya*, Vol. 1, pp. 237-240. México. 1961 (Sm4)

— The Mayan Calendar. *Astronomical Society of the Pacific*, Leaflet No. 392, pp. 1-8. San Francisco. 1962. (Sm5)

SPINDEN, HERBERT J.: Maya Dates and What They Reveal. *Museum of the Brooklyn Institute of Arts and Sciences, Science Bulletin*, Vol. 4, No. 1. 1930.

TEEPLE, JOHN E.: Maya Astronomy. *Carnegie Institution of Washington, Contributions to American Archaeology*, No. 2, Pub. 403, pp. 29-115. Washington. 1930.

THOMPSON, J. E. S.: Jottings on Inscriptions of Copan. *Carnegie Institution of Washington, Division of Historical Research, Notes on Middle American Archaeology and Ethnology*, No. 38, Vol. 2, pp. 48-64. Cambridge. 1944.

— Maya Hieroglyphic Writing: Introduction. *Carnegie Institution of Washington*, Pub. 589. Washington. 1950.

WILLSON, R. W.: Astronomical Notes on the Maya Codices. *Papers, Peabody Museum, Harvard University*, Vol. 6. No. 3. Cambridge. 1924.

— Table of eclipses visible at Copan, from Christian Era to Spanish Conquest. Unsigned ms., 16 sheets covering facing pages with printed Numbers 8-39; pp. 22-23 copied as p. 22 in Willson, 1924, Table II, there referred to as "Obsercable in Yucatan". Photostat obtained through Dr. A M. Tozzer Ca. 1932. N. D.