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Dear Brothers:

Your letter of February 28, 1980, arrived safely about two weeks ago. First, I would like to tell you that I appreciated it, and especially the way in which it was written: discussing - in a spirit of brotherly love and respect - the question of chronology as such, not the questioner, his motives (if he is illoyal, apostate, presumptuous, etc.), his right to examine the subject and question previous conclusions, his right to share his findings with his friends and ask for their reactions, and so on - as if this was the problem. However, when a fire-alarm starts to hoot, it would not be relevant to conclude that something has gone wrong with the alarm and that a close examination of it in order to bring it to silence would solve the problem. I find it encouraging that you, in your most recent letter, turn your whole attention to the "fire", so to speak.

I appreciated also very much the initiative taken by Brother Bengt Hansson in visiting me last December for a private talk, after which he wrote to you about my situation.

Of course, I understand quite well that it is impossible for you to enter into a lengthy correspondence with me on the subject of chronology and debate over every detail in my treatise ("The Gentile Times Reconsidered"). On the other hand, I do not hesitate to answer your letter, partly because the points you brought up against my conclusions are clearly invalid, which is easy to demonstrate, and partly because I will present two additional lines of evidence to the ones earlier presented in the treatise against the 607 BCE date. I am not doing this because I find pleasure in "questionings" and "debates", or because I want to prove that I am right and you are wrong. Who is right is not important. What is right is important. Truth and facts belong to all who love them. That's why I feel I should answer your letter.

A true scientist (and any truth-lover, for that matter) invites reactions and friendly criticism, as that will give him opportunity to correct errors and bring his thoughts closer to the truth. I take it for granted that this is your attitude, too, and as you write in your letter that "whatever you say will be read and carefully considered", I feel confident that the criticism and additional information offered on the following pages will be carefully examined by you without preconceived opinions. Openness to truth, courage to look into and face the truth, and readiness to accept the truth when it is clearly seen, no matter what it turns out to be - these are the marks of a truth-lover, of a true Christian. It would be foolish to spend a lot of time and energy on trying to defend, to the very last, a certain idea or interpretation, only because there is nothing to put in its place, if all the evidences we have indicate it has

nothing to do with reality. If, nevertheless, such an idea or interpretation is published, preached and spread out to millions of people and is presented, not only as a theory, but as a basic Christian truth, the rejection of which is said to arouse the wrath of God, then this would be more than foolish. It would be a grave sin, for which Jehovah would hold us responsible.

The points you brought up will be discussed one by one under different headlines. If you choose to write some lines and tell me what you think and how you feel about the information below, this would be much appreciated. If you believe I could be of any help in your further investigation of this subject, perhaps by giving further information on some detail or sending some material you need, feel free to ask me.

I regret that this letter with the discussion on the following pages has become so long. Yet, I have tried to explain things as briefly as possible. I had intended to add some additional information on the subject, too, but perhaps that could be done in another letter in the future, if necessary.

Brothers, thank you once again for your kind letter! With Christian love.

Your brother,



Carl Olof Jonsson

Enclosures:

1. Correspondence Jonsson-Sachs.
2. Some pages from J. Oates' article in Iraq XXVII, 1965.

# THE CANON OF PTOLEMY

Your first comments concern the Canon of Ptolemy, and your statement that his Canon cannot be regarded as an "unerring guide" for the chronology of the Babylonian era is, of course, correct. This was basically what I said in my treatise, too. Ptolemy's figures have been accepted by modern historians, not because his Canon in itself is regarded as an unerring guide for the chronology of the Babylonian and later periods, but because (1) his figures have been confirmed by a number of much older sources for the chronology of that time, some of which are contemporary with the Neo-Babylonian era, and because (2) in his great work Almagest he establishes the chronology presented in his Canon as an absolute chronology with the aid of a large amount of astronomical material gathered from ancient sources. His calculations have been checked and verified by modern astronomers, and more than that: Modern discoveries of ancient astronomical tablets, especially so-called astronomical "diaries", have enabled modern astronomers and historians to establish the absolute chronology of almost the whole period covered by the Canon of Ptolemy, independently of Ptolemy and his eclipses, thus giving an independent confirmation of his chronology. So what matters today is not the Canon of Ptolemy, but the older documents and sources that have confirmed it: astronomical diaries, contract tablets, chronicles, king lists, contemporary Babylonian and Egyptian stelae, etc. - and the Bible.

I noticed with interest your statement about Ptolemy, that "When he gives information about eclipses, that information is correct" (p. 1). One of the ancient eclipses he records is dated to the 5th year of Nabopolassar (Almagest, V 14, p. 340), identified by modern astronomers with the lunar eclipse of April 22, 621 BCE. This, of course, fixes Nebuchadnezzar's 1st year to 604 BCE (as his father Nabopolassar ruled for 21 years), and his 18th year (when he desolated Jerusalem) to 587 BCE. I noticed some years ago that the Aid book makes an attempt to identify this eclipse with one that took place 20 years earlier, on June 1, 641 BCE, to correspond with the chronology of the Aid book. In support of this identification it is added that "This earlier eclipse was total (i.e., 12 digits or more) as compared to the very minor one of only 1.6 digits in 621 BCE" (p. 331:3). This statement proved to me that the author of the Aid article had never read the description recorded by Ptolemy and checked how this description squares with the alternative identification he presents. Ptolemy's source gave a fairly detailed description of this eclipse, including its magnitude. It is clearly stated that it was a minor one, and that only a fourth of the diameter of the moon was eclipsed. Thus the fact that the eclipse of June 1, 641 BCE, was total does not support the identification, it actually makes it impossible! I find this error all the more remarkable, as the author earlier in his article stresses the importance of knowing if an eclipse was total or not. Other details, too (the month, the day, the time of the night, etc.) make the eclipse of June 1, 641 BCE impossible and establish the eclipse of April 22, 621 BCE as the only possible eclipse that fits the description. (R.R. Newton's recent attempt to "prove" that Ptolemy fabricated some of the eclipses he records seems to have been a mistake: his "evidences" have been rejected by all competent scholars, as far as I know). This is only one of several other errors I have come across on examining the "Chronology" article in the Aid book. Excuse me for pointing this out, but I hope such errors will be corrected in future editions of this otherwise excellent and extremely valuable Bible dictionary.

ASTRONOMICAL "DIARIES": VAT 4956 - AND BM 32312!

Commenting on VAT 4956, you repeat your earlier argument (Aid, p. 331:8, discussed in my treatise, p. 39), that "while the astronomical information on this tablet points to the year -567/66 (568-567 B.C.E.), attributing the tablet to the 37th year of Nebuchadnezzar may be simply the opinion of a scribe assembling and dating 'a collection of astronomical observation texts,' working at a 'much later' period" etc. What you suggest is, that the astronomical diaries were not originally dated, but that dates (such as the "37th year of Nebuchadnezzar") were inserted in the texts by later copyists. Is this a plausible explanation, or is it a desperate attempt to explain away a text, which would otherwise once and for all puncture the 607 BCE date and establish 587 BCE as the absolute date for Nebuchadnezzar's 18th regnal year?

The astronomical diaries usually contained very detailed information about the positions of the moon and the five (then known) planets, as well as other information (meteorological events, earthquakes, market prices, and sometimes also historical events). As you point out, VAT 4956 is dated from the beginning of the 37th year to the beginning of the 38th year of Nebuchadnezzar, but this is not enough: throughout, almost all events mentioned in the text are dated, giving the month, the day, the time of the day (or night), etc. About forty dates of this kind are given during Nebuchadnezzar's 37th year, and of course it was not necessary to repeat the regnal year at all these places, as this was given at the beginning of each year (as is done also in other documents, such as annals and chronicles). Do you really think it is likely that the original texts contained all these dates during a year, but not the regnal years at the beginning of each year? Surely it must be regarded as extremely unlikely that the Babylonian astronomers would leave out such an important detail! All texts of this kind that have been discovered (about 1200 fragments of astronomical diaries have been discovered, a third of which are so well-preserved that they may be dated) are dated to the year of the reigning king. Are all these dates inserted by later copyists? Most of the diaries cover the period 385 - 60 BCE, but a number of them are copies from earlier periods. And now I will present another diary, older than VAT 4956, that once again confirms the chronology given by the Canon of Ptolemy and refutes the 607 BCE date.

In an article, entitled "Babylonian observational astronomy" and published in Philos. Trans. Royal Soc. London, ser. A. 276 (1974), pp. 43-50, professor Abraham J. Sachs (the scholar who coined the term "diaries" for these texts and without doubt the foremost authority on astronomical diaries living today) gives a short presentation of the diaries. On p. 48 he mentions that the oldest datable diary is from the year 652 BCE and says: "I found the astronomical contents to be just barely adequate to make this date virtually certain. It was a great relief when I was able to confirm the date by matching up a historical remark in the diary with the corresponding statement for -651 in a well-dated historical chronicle." (My stress). This seemed to be a text of great importance for the question of the Babylonian chronology, and as the text and a translation of it had not yet been published, I wrote to prof. Sachs and asked a couple of questions:

- "1. What information in the diary makes the date -651 'virtually certain'?
- "2. What kind of historical remark in the diary corresponds with what statement in which well-dated chronicle?

In his answer Prof. Sachs gave me all information I had asked for, and even sent me an IBM copy (almost as good as his photograph) of the diary in question, BM 32312. Of special interest is, that your argument that the diaries may have been dated by copyists from a much later period cannot be applied to this diary, as "year as well as month names are broken away", yet, thanks to the historical remark in the diary, this information can be supplied from another source.

Firstly, the year - 652 BCE - is fixed by the astronomical information. Sachs writes: "The preserved astronomical events (Mercury's last visibility in the east behind the Pisces, Saturn's last visibility behind Pisces, both around the 14th of month I; Mars' stationary point in Scorpio on the 17th of month I; Mercury's first visibility in Pisces on the 6th of month XII) uniquely determine the date." According to the Canon of Ptolemy, 652 BCE corresponds to the 16th year of Shamashshumukin (the predecessor as king of Babylon of Kandalanu, who was followed by Nabopolassar, the father of Nebuchadnezzar).

Secondly, the diary states that the king of Babylon was involved in a battle at a place called Hirit on the 27th day of the 12th month. If Ptolemy's chronology is correct, this battle was fought in the 16th year of Shamashshumukin. Can this be confirmed? Yes, it can. As Sachs points out, we find the same information in a well-known chronicle.

The chronicle is the so-called "Akitu Chronicle", BM 86379, latest translated by A. K. Grayson in Assyrian and Babylonian Chronicles (1975), pp. 131, 132. The chronicle covers a part of the reign of Shamashshumukin, including his 16th year. Not only is the battle at Hirit mentioned, but also the day and the month when it was fought, precisely as in the diary. Obv. 13-16 of the chronicle reads:

"On the twenty-seventh day of Adar (= 12th month) the armies of Assyria and Akkad did battle in Hirit. The army of Akkad retreated from the battlefield and a major defeat was inflicted upon them. (However), there were still hostilities (and) warfare continued."

I am sure you realize what this new evidence means. The astronomical events described in the diary fix the battle at Hirit on Adar 27 to the year 652 BCE. The "Akitu Chronicle" clearly shows that this battle at this place on this day was fought in the 16th year of Shamashshumukin. Thus, Shamashshumukin's 16th year was 652 BCE. But this is also the year given by the Canon of Ptolemy for Shamashshumukin's 16th year! Once again, as so many times before, Ptolemy's Canon is confirmed, and this at a point that also, once again, refutes our 607 BCE date. Dating the destruction of Jerusalem in the 18th year of Nebuchadnezzar to 607 BCE dislocates all dates before Nebuchadnezzar 20 years. It would, for example, put Shamashshumukin's 16th year in 672 BCE, not in 652 BCE. But this is refuted by this new diary, which is another independent witness supporting the 587 BCE date for the desolation of Jerusalem. Prof. Sachs adds in his letter: "I should perhaps add that the absolute chronology of the regnal years of Shamashshumukin was never in doubt, and it is only confirmed again by the astronomical diary."

For your convenience, I have enclosed the correspondence with Prof. Sachs and the material he sent me.

THE NABONIDUS HARRAN STELE - AND THE NABONIDUS BABYLON STELE!

Your comments on the Neo-Babylonian chronicles agree with the more detailed information I presented in my treatise, pp. 28, 29, so I will immediately proceed to your objections to the Harran inscription.

You hold that the Harran inscription is open to doubt, because it seems to present a 3-year reign for the Assyrian king Assur-etillu-ili before Nabopolassar, while contract tablets are dated up to the 4th year of this king. But since Gadd published the translation of the text in 1958, others have examined the difficulties he pointed out and offered solutions to them. The most important studies are the ones by Wolfgang von Soden (ZA, Vol. 58, 1967, pp. 241-255), Joan Oates (Iraq, Vol. 28, 1965, pp. 135-159), and Julian Reade (JCS, Vol. 23, 1970, pp.1-9).

After the death of Assurbanipal of Assyria and Kandalanu of Babylon (these kings might have been identical, Kandalanu being Assurbanipal's Babylonian throne-name) in 627 BCE, Assur-etillu-ili's legitimate right to the throne was opposed by a general, Sin-Shum-lishir, and then by a brother of Assur-etillu-ili, Sin-shar-ishkun. This caused a civil war, and Nabopolassar, a military commander in the south, took advantage of the situation and seized the throne of Babylon. Then, for some years, at least three armies operated at the same time in Babylonia, and it is understandable that a number of the cities changed sides more than once, as it was no longer evident which authority was to be viewed as the legitimate one. The confusing situation is reflected in all documents dated in this period, Assur-etillu-ili being regarded as king in one city while his brother Sin-shar-ishkun was accepted as king in another.

Oates in his article demonstrates that Adda-Guppi, the mother of Nabonidus and a priestess at the Sin-temple in Harran, abandoned Harran in the 3rd year of Assur-etillu-ili and moved to Babylon, but actually, the Harran inscription does not state that this was his last year, as Gadd concluded. From this time onward Adda-Guppi served under Babylonian kings. Reade, who accepts this basic conclusion, demonstrates in his article how all the confusing documents from the period may be harmonized. The solutions offered are in agreement with the Harran inscription, which even has proved to be very helpful in the attempt to reconstruct the events during this chaotic period. There seems to be no reason, therefore, for maintaining an overcritical attitude toward the Harran inscription.

If you still feel there is, then consider the fact that there exists another stele from the reign of Nabonidus, THE NABONIDUS STELE FROM BABYLON (discovered in 1896), that confirms the length of reigns given on the Harran stele for the Babylonian kings! This stele (cf. Gadd, p. 73; I did not deal with this additional evidence in my treatise) was evidently made in the 2nd year of Nabonidus (see the discussion by P.-R. Berger in Die Neubabylonischen Königsinschriften, Vol. I, 1973 p. 110ff.). According to the translation published in Pritchard's ANET, pp. 308-311, this stele states that "the temple el-hul-hul in Harran was in ruins for 54 years", until Nabonidus in the beginning of his reign took measures for restoring it. Now the Harran inscription as well as the chronicle BM 21901 date the destruction of the temple in Harran to the 16th year of Nabopolassar (i.e. 610/609 BCE). In adding up the remaining regnal years up to the beginning of Nabonidus' reign given in the Harran inscription - 5+43+2+4 - we get exactly 54 years! So if you feel the information of the Harran stele should be rejected, what are we to do with the same confirming information given by the Nabonidus Babylon stele? Reject that, too? Why? Should we reject every new evidence that contradicts our theories? Is that a good method? Will it bring us closer to the truth?



THE EGIBI HOUSE TABLETS - AND THOUSANDS OF OTHER CONTRACT TABLETS!

Again, you just repeat the argument from the Aid book (discussed in my treatise, pp. 32, 33) that, though we have many contract tablets from all years of Neo-Babylonian kings represented in the Canon of Ptolemy, there could still be a period of 20 years from which tablets are missing.

You refer to the fact that we have no contract tablets in the Egibi House collection that cover the years 7, 32, 33, 34 and 36, of the Persian king Darius. Well, as Boscawen and other scholars after him have stressed, the firm Egibi and Sons prospered from the time of Nebuchadnezzar up to the time of Darius. The collection of tablets of this firm discovered in 1875-6 ends with Darius. No tablets from the reigns of later kings were discovered in the collection. Tablets in the collection from the end of Darius' reign are sparse. But the reign of Darius began almost two decades after the end of the Neo-Babylonian era. The important thing to consider is, that the tablets cover the reigns of all the Neo-Babylonian kings we know to have existed, not only from the Canon of Ptolemy, but from a number of earlier documents as well, some of which were written contemporary with the Neo-Babylonian period.

Another thing to remember is, that the Egibi tablets are only a part of the many thousands of contract tablets from this period. These other contract tablets, too, give the same length of reigns for the Neo-Babylonian kings! This I also stressed in my treatise, p. 31ff. Every year of the reign of Darius, too, are covered by tablets of this kind, of course.

My argument from this evidence is still left unanswered: If there was an additional period of 20 years during the Neo-Babylonian era, why do we have so many tablets from every year of Neo-Babylonian kings known to have existed, not only from the Canon of Ptolemy, but from all the earlier (and sometimes contemporary) documents, the Harran inscription, the building inscriptions, chronicles, king lists, Berossus, etc., covering the length of reigns known for these kings - but not one single tablet from the 20-year period you would like to add to the Neo-Babylonian era? And further, Why are the thousands of tablets that must have been written during this supposed 20-year period missing from the same 20-year period that is also missing in all the other documents - and not from another period of 20 years? And further, Why are the tablets missing from a period of exactly 20 years? Why not from 17, 13, 7 - or different years distributed throughout the Neo-Babylonian period? Who travelled around in the Neo-Babylonian kingdom and picked off all the many thousands of business letters dated in this 20-year period and hid or destroyed them, before modern archaeologists started to unearth the temple-archives, public archives, etc. in the ruins of the Babylonian cities? From time to time, new quantities of contract tablets are discovered, translated and - sometimes - published - but the 20-year period we are looking for never turns up. As I formulated the argument in the treatise: "If you are casting a dice many thousands of times without ever getting up number six, you will at last have to conclude: 'There is no number six on this dice'. The same is true, too, of the missing 20 years we are looking for: They never turn up, because they never existed."

Now, just as you repeated your argument I had answered. I have repeated my argument that was left unanswered.

THE 26TH DYNASTY OF EGYPT: AMASIS AND PSAMMETICHUS III

In "The Gentile Times Reconsidered", pp. 39-44, I mentioned a number of synchronisms between Egyptian, Babylonian, and Judean rulers, three of which are given by the Bible (2 Kings 23:29; Jer. 46:2, and 44:30). I demonstrated that the Egyptian chronology for this period has been independently established from contemporary Egyptian sources. Yet all synchronisms agree completely with a dating of the destruction of Jerusalem to 587 BCE, while the deviation from the 607 BCE date for this event is consistently about 20 years. The contemporary Egyptian chronology, therefore, presents another independent witness against our 607 BCE date.

In your letter you try to undermine this strong evidence by referring to the conflicting figures given by later historians, Diodorus and Syncellus. But in establishing a chronology the contemporary documents cannot, of course, be refuted, or even questioned, by figures given by historians from a much later period. Their figures have often proved to be distorted, and this distortion usually increases, the farther away from the period these historians lived. As to Manetho, his figures are best preserved by Julius Africanus (221/222 CE; cf Waddell in Manetho, London 1948, pp. xvi, xvii), and his figures for Amasis and Psammetichus III agree with the contemporary sources, as do also the figures given by Herodotos, the historian who lived closest to the time of Amasis and Psammetichus III (Herodotos was born c. 484 BCE, that is, about 40 years after the reign of Psammetichus III; thus, during his visit to Egypt, Herodotos could even have met individuals, who were born before the reign of Psammetichus III). Diodorus of Sicily, on the other hand, lived during the reigns of Julius Caesar and Augustus, more than 500 years after Psammetichus, and Syncellus wrote his work toward the end of the 8th century CE, or more than 13 centuries after Psammetichus! Do you really think that the figures presented by these two late historians could be used to question figures given by contemporary sources?

Of the historians of the classical (Greek and Roman) period the Aid book states: "All of these lived after the Assyrian and Neo-Babylonian period . . . . For the Assyrian and Neo-Babylonian periods, then, none of these writers present information based on personal knowledge but record, rather, the traditional views they heard, or, in some cases, may have read and copied. . . . Not only this, but what we know of their writings is today dependent upon copies of copies, the oldest copy often dating no farther back than the medieval period of the Common Era." (Aid, 332:4,5) The figures you quote from Diodorus and Syncellus, therefore, do not mean much and weigh nothing against contemporary evidence. I quoted Herodotos and Manetho/Africanus only because their figures have been confirmed by modern discoveries of documents, contemporary with the reigns of Amasis and Psammetichus III.

Though Psammetichus III ruled for only six months, you mention that "the highest date available from monuments for this king is year 2". Actually, this information does not contradict a rule of six months for this king, which anyone acquainted with the Egyptian method of reckoning regnal years will easily realize. The Egyptians applied the nonaccession-year system (see my treatise, p. 42, and Appendix A, pp. 88,89), that is, the year in which a king came to power was counted as his first regnal year. Psammetichus III came to power at the end of the year 526 BCE,



probably only a few weeks before the end of the year. At this time the Egyptian calendar year almost coincided with the Julian calendar year, so Psammetichus ascended to the throne at the end of the Egyptian calendar year, too. Though he ruled for only a fraction of this year, it was (according to the Egyptian nonaccession-year system) reckoned as his first regnal year. His second regnal year, therefore, began only a few weeks after his accession to the throne! If he ruled for only six months, documents dated up to the fifth month of his second regnal year are only what we should expect to find. Actually, we have three documents (papyri) from his second year, dated to the 3rd, 4th, and 5th months of his 2nd year, corresponding to the first half of the year 525 BCE. Then, in May or June, 525 BCE, Psammetichus was dethroned by Cambyses. - See P. K. Kienitz, Die politische Geschichte Ägyptens vom 7. bis zum 4. Jahrhundert vor der Zeitwende, Berlin 1953, p. 156, footnote 7.

Further, you refer to "a document mentioned in the publication Notice des papyrus démotiques archaïques (by Revillout)" that "gives four years of rule to a King Psammetichus that the author claims is Psammetichus III" (my stress). E. Revillout was one of the founders of the publication Revue Egyptologique in the 1870s, and the article you refer to was evidently the one published in Vol. VII, No. II, 1892, pp. 41-44, though I could not find the statement you refer to in that article. But in an article published in Vol. III, No. IV, 1885, pp. 187-191 (and in Vol. VII, No. III, 1896, p. 139) Revillout mentions and quotes a document dated in the 4th year of a King Psammetichus, which he holds to be Psammetichus III. Such a view could still be held at that time, but since then many important documents have been discovered, translated and published that have made Revillout's hypothesis antiquated and obsolete. For example, some of the Apis stelae were translated and published by Breasted in 1906 (Ancient Records of Egypt), the "Petition of Petiese" was published in 1909 (F. Ll. Griffith, Cat. of Demotic papyri, III), the "Demotic Chronicle" in 1914 (W. Spiegelberg, Demotische Studien 7: Die sogenannte Demotische Chronik), and the inscription in Wadi Hammamat was published, too, during this time (H. Gauthier, Le livre de rois d'Égypte, Vol. 4). The three last mentioned documents all give Amasis 44 years of reign. Because of these and other evidences, Revillout's hypothesis can no longer be upheld, and it is not shared by any recent scholar. The document quoted by Revillout, therefore, referred to one of the earlier kings known by the name of Psammetichus.

As to Krall's and Spiegelberg's date for Psammetichus III (528/527 instead of 526/525 BCE), these scholars, too, were active before the chronology of the 26th dynasty was definitely established (they belong to the later part of the last century and the early part of the twentieth century). The only disunity among scholars was for a long time if Amasis ruled for 43 or 44 years. This problem was definitely solved in 1957, when Richard A. Parker published his important article "The Length of Reign of Amasis and the Beginning of the Twenty-Sixth Dynasty" in Mitteilungen des Deutschen Archäologischen Instituts, Kairo Abteilung, XV, 1957, pp. 208-212. The evidence he presented in support of the year 570 BCE as the 1st year of Amasis was so strong that his conclusion in a very short time obtained general acceptance. I will try to briefly recapitulate it below:

A papyrus in the Louvre collection of papyri, No. 7848, dated to Year 12, I Šmw 21 of Amasis (= the 21st day of the 9th month of the Egyptian civil calendar, Pachons) refers to an oath taken "before Khonsu ... in Year 12, II Šmw 13, being the 15th lunar day of (lunar) I Šmw."

As is seen, this papyrus is double-dated, giving a date in the 12th year of Amasis both according to the Egyptian civil calendar and according to the lunar calendar. While the civil calendar of 365 days "travelled" through the solar year (with approximately one day every fourth year), the lunar calendar, of course, was fixed to the moon, with the 1st day of every month always beginning on the morning of crescent invisibility.

The papyrus quoted above gives the information that in the 12th year of Amasis, the 13th day of II Šmw (= Payni, the 10th month) fell on the 15th day of the lunar month, which means that the 1st day of the lunar month (the morning of crescent invisibility!) fell on I Šmw 29 (= the 29th day of the 9th month) of the civil year. Such a situation did not occur every year, of course. With the aid of an astronomical table, such as Herman H. Goldstine's New and Full Moons 1001 BC to AD 1651 (Philadelphia 1973) it is easy to check which years during the 6th century BCE such a situation occurred. Though astronomical tables usually give dates according to the Julian calendar, this is no problem, as the Egyptian calendar dates are easy to translate into Julian dates. Which year fits the situation described in the papyrus from the 12th year of Amasis?

Parker was able to demonstrate that the year, in which the morning of crescent invisibility fell on I Šmw 29 of the Egyptian civil calendar must have been the year 559 BCE (Oct. 5, Julian calendar). Amasis' 1st year, therefore, must have been 570 BCE. This leaves 45 years only for the combined reigns of Amasis and Psammetichus III, prior to Cambyses' conquest of Egypt in May or June, 525 BCE (570-525 = 45). As Amasis ruled for 44 years (chronologically), Psammetichus III must have ruled for 1 year (chronologically). And no matter how you reckon the reigns of Amasis and Psammetichus III, you have to give them a combined reign of 45 years, and the total length of the 26th dynasty is in any case clearly and firmly established.

This is a remarkable confirmation of the reigns of Amasis and Psammetichus III as given by other documents, isn't it? Of course, we will find one or two other years earlier in the 6th century, too, when the lunar month started on I Šmw 29, or at least close to that date, but these earlier years do not agree with any other document, or with any of the figures given by later historians either.

Thus, the chronology of the 26th dynasty is firmly and independently established, and the synchronisms given in the Bible clearly point to 587 BCE as the date for the destruction of Jerusalem. Once again, we find an independent witness against our 607 BCE date for that event.

THE 70 YEARS - CAPTIVITY OR SERVITUDE?

On p. 4 of your letter you state that I regard the 70 years as "seventy years of captivity to Babylon", beginning "in the accession year of Nebuchadnezzar", after which you bring forth some arguments against such a view.

Unfortunately, you seem to have misunderstood the view I presented and defended in my treatise. I never made such an application, as a careful reading of the treatise will prove. Though I argued that some captives - including Daniel and his friends, according to Dan. 1:1ff. - were brought to Babylon soon after the battle of Carchemish, I strictly applied the 70 years to a period of servitude, not only for Judah, but also for the surrounding nations, in close agreement with the wording of Jeremiah's prediction ("these nations will have to serve the king of Babylon seventy years", Jer. 25:11). Already from my sub-heading on p. 49 you should have noticed that I stated the issue as "THE SEVENTY YEARS: DESOLATION OR SERVITUDE - WHICH?" - not as "Desolation or captivity". I also noticed that you, in the large-print edition of NW, at the head of p. 826 described the seventy years as the "70 years' servitude", just as I had done.

"Servitude", of course, is not exactly the same thing as "captivity"; and it did not mean captivity (or desolation!) for the nations that surrounded Judah, did it? For most nations "servitude" meant vassalage only, though for Judah a part of the servitude came to include a period of both captivity and desolation. I quoted a number of scriptures in support of the fact that the servitude began long before the destruction of Jerusalem (treatise, pp. 49-55), such as Dan. 1:1, Jer. 27, 28, and 35. Others could be added, for instance 2 Kings 24:1, where it is stated of Jehoiakim: "In his days Nebuchadnezzar the king of Babylon came up, and so Jehoiakim became his servant for three years" etc. When he rebelled, this immediately caused acts of reprisal in order to keep him in subjection. So, even for Judah, the servitude began many years before the destruction of Jerusalem, and - as this text proves - several years before the end of Jehoiakim's reign.

Further you point out that my quotation on p. 51 from Wiseman's translation of the chronicle BM 21946, that Nebuchadnezzar, after the battle at Carchemish, "conquered the whole area of Hatti-country", more correctly, according to Grayson's more recent translation, should read "Ha[ma]th", a district in Hatti (=Syro-Palestine). I am grateful for your correction of this detail (Grayson's recent work was not available to me in 1977). But does it really change anything? On the next page of my treatise (p. 52) I quoted further from the same chronicle: "In the accession-year Nebuchadnezzar went back to the Hatti-land and until the month of Sebat marched unopposed through the Hatti-land; in the month of Sebat he took heavy tribute of the Hatti-territory to Babylon ...". And for Nebuchadnezzar's 1st regnal year the chronicle states: "All the kings of the Hatti-land came before him and he received their heavy tribute." At all these places the word "Hatti" is well preserved in the original, and Grayson, too, of course translates it as "Hatti" ("Hattu"). Isn't it very evident from the chronicle that Nebuchadnezzar subjugated the whole Hatti-territory (= Syro-Palestine) already in his accession-year, and in his 1st regnal year, at the latest, all the kings of the Hatti-land (which reasonably must have included Jehoiakim, too, which is supported by 2 Kings 24:1) were in

vasallage and paid tribute to Nebuchadnezzar?

It is interesting to note also the wording in the quotation above: "In his accession-year Nebuchadnezzar went back (Grayson: "returned") to the Hatti-land". Evidently, he had been there earlier. Does it not seem very probable that Nebuchadnezzar, after the battle at Carchemish and his conquest of "Hamath", continued southwards and began to subdue the rest of the Hatti-territory, and even took some captives from the Jews, Phoenicians, and Syrians, as Berossus says? This would be in complete agreement with what is actually (without far-fetched re-interpretations) stated in Dan. 1:1ff. But during his offensive southwards, Nebuchadnezzar got message of his father's death, which quickly brought him back to Babylon in order to secure the throne. After that - and still in his accession-year, according to the chronicle, - he went back to the Hatti-territory and continued the operations there.

What we learn, both from the Bible and the chronicle, is that the servitude predicted by Jeremiah began very soon after the battle at Carchemish. If some Jewish captives were brought to Babylon at that time or not, is quite another question. Berossus says so, and his statement is confirmed by a natural and literal reading of Dan. 1:1f. and 2:1. I cannot find how Josephus' statement many centuries later that Nebuchadnezzar did not occupy Judah at that time could disprove this. His statement is not in agreement with the Scripture. His statements have often been proved to be in error, and very often he contradicts himself. He does not give the source for his statement, and it is quite possible that he just gives his own interpretation or guess. If he is right or not does not change the fact that the servitude began in the reign of Jehoiakim, according to the Bible, and in the accession-year of Nebuchadnezzar according to BM 21946.

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As I demonstrated in my treatise, all ancient and most reliable documents we have disprove our 607 BCE. Above I have added two additional lines of evidence to the ones presented in the treatise in support of 587 BCE as the 18th year of Nebuchadnezzar: (1) the diary BM 32312 and (2) the Nabonidus Stele from Babylon. With all these evidences the Bible agree: A natural and simple reading of Jer. 25:10, 11; 29:10; Dan. 1:1; 2:1; Zech. 1:7-12, and 7:1-5 clearly brings us to the same conclusion. Only by a special interpretation of 2 Chron. 36:20,21 and Dan. 9:2 is it possible to arrive at the conclusion that Jerusalem was destroyed in 607 BCE. But in order to uphold this conclusion we have to reject all the secular documents discussed above and in my treatise, and more than that: We must also re-interpret a number of texts in the Bible itself and give them very unnatural, far-fetched applications. But the evidence against the 607 BCE date is overwhelming. Isn't it curious that we cannot accept this overwhelming amount of unanimous evidences in support of the 587 BCE date (we cannot find one document, or even a line in a document, in support of the 607 BCE date), while we at the same time have accepted another secular date - 539 BCE - on much weaker grounds, and even term it an "absolute date", or a "pivotal date"? For about twenty years the "Nabonidus Chronicle" was referred to in support of the 539 BCE date, but since this evidence was rejected (in The Watchtower, May 15, 1971, p. 316), our only "evidence" for the date is the Canon of Ptolemy (!) and an Olympiad date referred to by the late historians Diodorus, Africanus, and Eusebius! - Aid, p. 328:7 and 408:9.