Discussing the Chronology of the New Kingdom

WOLFGANG HELCK

Fundamental to any progress in considering the chronology of ancient Egypt, and hence also of the entire eastern Mediterranean, is the determination of how much time elapsed during the epoch of the so-called New Kingdom – that is, the 18th to 20th Dynasties. Feasible theories may be arrived at by approaching the problem from either of two directions. In the first we try to establish astronomical data; in the second we start with recorded dates from the reigns of individual kings. That the one method does not work properly without the other is well recognised. The two routes differ only in this: to which evidence they give priority. While data about regnal years can be accepted as evidence directly in order to produce a useful result, astronomical references must be interpreted initially under certain assumptions, and in the process allowances must be made which can themselves be called into question. Thus it seems safer to start with the recorded regnal dates than with our interpretations of real or putative Sirius or new-moon dates. We should also dispense with attempts to establish, say, (non-Egyptian) solar eclipses and conjunctions of Jupiter and Saturn; such attempts are important for us only because they show that they result in an apparently settled and self-corroborating chronology which, in the light of everything we know about ancient history, contradicts all probability! On the other hand, the reign-length method does not work without the astronomical, as is clear from its having to begin from the assumption that a barge captain’s observation of the new moon in the 52nd year of Ramesses II fixes the reign of that king to the years 1279-1213.

The understandably fragmentary nature of the ancient Egyptian record does, however, leave the initially safer method of reign lengths open to the possibilities of diverse interpretations. Since fresh, relevant material comes to light only very rarely, the discussions concentrate more and more on basic methodological questions. It is to Kitchen’s credit that in his contribution to the First International Colloquium on Absolute Chronology in Gothenburg in August 1987 he brought the proposals advanced there face to face with the opposite positions and thereby opened up the discussion to the points which he regarded as controversial. Within an exposition the questionable nature of such points is readily overlooked by listeners or readers.

In the following we shall take up Kitchen’s contribution and as broadly as possible set out particularly the fundamentals of the various views, in order to put recent conclusions on a more solid footing.

Bekenkhons and Seti I

It seems appropriate to cite as our first example the question of the reign length of Seti I. Kitchen emphatically supports the ‘perfectly clear observations of M. L. Bierbrier’ that Seti I must have reigned for at least 15 years. He therefore rejects my view that Seti I died not long after the completion of his Year 11. Bierbrier utilised the biographical details on the Munich statue of the Theban high priest, Bekenkhons, who explains by way of summary that at the age of 5 he was brought up in the stable of the king Mn-[m3t-R-ne] for 11 years and then served under Ramesses II in various Theban priesthoods for 70 years. We know that Ramesses then installed the son of Bekenkhons, Rm, as his successor. These 70 years cannot be accommodated in the 66-year reign of Ramesses II, even if one does not count the individual years of Bekenkhons as complete years and assumes that his son’s promotion occurred more or less in the last days of Ramesses II. Whatever the case, Bekenkhons must have left the stable and been provided with a priestly office several years before Seti died, i.e., he cannot have been reared for 11 years in...
the stable of Seti I, so Seti must have reigned longer—‘at least 12 years’ (Bierbrier).

I myself put forward a different explanation. We have to start from the fact that in this epoch of the New Kingdom so many monuments of various kinds have been preserved that almost every year must be attested in one way or another. That is particularly true when ostraca, vessel inscriptions or papyri are preserved in addition to the monuments. The following details concerning Seti I together with his predecessors and the early reign of his successor, Ramesses II, make it clear that here and there the evidence for a single year is missing, but never for several years consecutively. That would, however, be the case if we had to add 15 or indeed more years for Seti I:

Ay

1. 
2. Winejar inscription KRI VII 65, 9
3. Urk. IV 2109, 8
4. Urk. IV 2110, 13
(5.) very short

Haremheb

1. Urk. IV 2132; KRI VII 57, 13 (?)  
2. KRI VII 58, 15; 57, 11 (?)  
3. Urk. IV 2177, 6  
4. KRI VII 59, 2  
5. KRI VII 58, 11  
6. KRI 58, 12; Haeny, mortuary temple of Amenhotep III, 66  
7. Urk. IV 2162, 10; KÂT (2. ZwZt), 143, Nr. 152; KRI III 42  
8. Urk. IV 2170, 15  
9. JEA 75, 1989, IX (tomb of Maia)  
10. KRI VII 86, 10  
11. 
12. Quirke, JEA 72, 86, Fig. 4  
13. KRI VII 58, 2; G. Martin, JEA 65, Table 3, Nr. 2

Ramesses I

1. KRI I 4, 2.11
2. KRI I 2, 9

Seti I

1. KRI I 8, 8; 9, 3; 11, 15; 38, 2; 39, 2; 40, 11; 41, 10; 100, 7; 231, 16; 363, 10; VII 69, 11  
2. KRI I 45, 5; 244, 11; 250, 12; 260, 5; 282, 5; 270, 6; 363, 14; pap. Tur. 57450  
3. KRI I 241, 12; 243, 5; 273, 2; 364-7; VII 8, 12; 30, 3; 60, 9  
4. KRI I 46, 2; 59, 5; 237, 7; VII 65, 2 (?); 84, 6  
5. KRI I 59, 7  
6. KRI I 60, 4  
7. KRI I 368, 9 (after Gutgesell, Ostraka I 276, note 2, but Lane-  
Merekah)  
8. KRI I 62, 7-63; 2, 102, 6 = VII 9, 2; VII 31, 3; 55, 7  
9. KRI I 65, 14; 73, 11; 74, 7; 368, 13; VII 31, 3  
10. 
11. KRI I 75, 8  
(12. only a few days)

Ramesses II

1. KRI I 84, 14; II 324, 12; 325, 5; 337, 4; 338, 10; III 283; 844, 2  
2. KRI II 359, 13; 344, 10; III 348, 3  
3. KRI II 347, 2; 353, 14  
4. KRI II 1, 9; 224, 6  
5. KRI II 12, 5; 102, 2; 795, 5; 796, 9; III 636, 9.10; 636, 13  
6. KRI VII 172, 1  
7. KRI II 746, 15; VII 172, 1  
8. KRI II 148, 9.11.12.13; 149, 2-5; 361, 2  
9. KRI II 361, 5; 363, 4  
10. KRI II 149, 12; (VII 68, 11; 86, 10 ?)

NB It is noteworthy that the dated vessel inscriptions from the Ramessese listed by Kitchén are to be ascribed not to Ramesses II but to Merenptah, since they break off with Year 9 and with a few scattered examples first increase in number at the end of Ramesses II’s reign.

This schedule demonstrates that the supposition of further regnal years for Seti I after Year 11 would produce such a striking gap in the record as to make the supposition improbable.

On the other hand, how is the discrepancy of Bierbrier’s inherently logical deduction to be explained? Here I can only fall back on my earlier suggestion, that whereas Bekenkhons left the ‘school’ (as the ‘stable’ should be regarded) when it had the name of the new king, he did not enter a stable of Seti I; rather at that time the stable must have borne the name of Haremheb. I am convinced that even now anyone in such a situation would refer to his school (or his hometown) by its new name and not by the name it bore on his admission (or birth) – it could be that he wished to make thereby a conscious ‘political’ statement. Not that such a statement was to be expected, since the transition from Haremheb to the Ramessides apparently passed off without particular disturbances; nonetheless, it may have been expedient at least to bind the name of the school with the new dynasty and not with that of the transitional ruler, Haremheb. In my view this explains why Bekenkhons referred in the name of the ‘school’ to Seti I. We ought not to understand this detail as if it had been the name of the stable when Bekenkhons entered it, from which it would then follow that Seti I reigned at least 15 years and we must assume a gap of at least 4 years in the record. Such a gap cannot be established after the first half of the reign of Amenhotep III – not until the end of the 20th Dynasty!

We are therefore faced with a fundamental decision, whether to follow an interpretation of an apparent inconsistency in our consideration of the facts (namely the interpretation of Bekenkhons’s name for the ‘stable’), which we can also explain in another way, or to follow a collection of facts (namely the recorded regnal dates), the rejection of which results in an improbability (namely the ‘gap’ in the chain of evidence).

Abbreviations:

JEA = Journal of Egyptian Archaeology

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The Mes Inscription

Exactly the same problem arises in relation to the other point which Kitchen brings against my exposition, namely the length of Haremheb's reign. He lays great weight upon the so-called Mes date, i.e. the mention of a '59th Year of Haremheb' in a presumed copy of a document which was written down as proof of a claim to ownership in the tomb of Mes, who lived under Ramesses II. Here he holds to the usual understanding of the year number, namely that it comprises the reigns of the outlawed kings Akhenaten, Smenkhhkare, Tutankhamun and Ay. Accordingly Akhenaten died in his 17th year, Smenkhhkare probably in his 3rd, Tutankhamun in his 10th, Ay probably in his 5th year, i.e. 34 or 35 years would have been imputed to Haremheb, and the Mes date would then be Haremheb's 24th year. Since, however, it is hardly likely that this was also Haremheb's last year, a few more years must be ascribed to that king. But here one has always ignored the fact that there is no certain evidence for a regnal year of Haremheb beyond the 13th, as the above schedule shows.7 The alleged 16th year on a stone vessel, published by Redford,8 can be discounted as a modern falsification, just like the Year 27 on a limestone ostraca, since the latter certainly stems from the time of Ramesses II and mentions the entry of King Haremheb's statue during a procession. The vessel inscriptions listed by Koenig,9 with mention of a garden-plot of Haremheb's, reveal that the regnal dates should be limited to Years 2-13.10 For apparently only the living king ever provided food for the settlement at Deir el-Medineh.

According to the old interpretation of the Mes date we would have a normal attestation for Haremheb through dated monuments from Years 1 to 13 and then a complete gap from Year 14 to 25 or even further. Because of what is otherwise the density of evidences for dated records before and after Haremheb this is wholly improbable. Kitchen attributes this gap to 'mere accident and our ignorance'; the pattern of evidence from the epoch does not render this feasible. We must explain the Mes date, which even Kitchen calls 'curious', in a different manner – it is not 'too obvious to dispute' that the date assigns the Amarna kings to Haremheb. How the number could be explained remains for the time being open: it may be an error; it could be a deliberate falsehood, for in these proceedings some documents were already admitted to be false. It must be emphasised, however, (to return to fundamentals) that the Mes document is not a genuine, primary record, but a presumed copy in the

THE MES INSCRIPTION

Year 59 under the Majesty of the King of Upper and Lower Egypt, Djeser Khepru Re, Setepenre, [Son of Re] Haremheb-Meriamun. Copy of the examination [made by] the priest of the [litter], Aniy, who was an officer of the court, of the estate of the shipmaster Neshi [which was in the] village of Neshi, as follows: "I arrived at the village of Neshi, the place where the lands are and of which the citizeness Ur[ero] and the citizeness Takharu spoke. They assembled the heirs of Neshi together with the notables of the town who make... of the estate [of] Neshi in order to hear their statement(s). List of the names of the witnesses of Neshi:

The citizeness Kakay
The citizeness Hunetudjebu

.............
The soldier (?) ... Baka
Making four persons.

List of the names of the witnesses who came from the town in order to swear on oath: The field-worker Horihemeferher...

... what they said with one voice: "As Amun endures, and as the ruler endures, we shall speak truthfully... as to me, I am from the town... (to)day. I beheld the estate of the shipmaster Neshi, it being in the control of the heirs... in the time of the enemy from Akhenaten... Akhetaten [where] one was. ..."

This extract comes at the end of the Mes text. Translation by G. A. Gaballa, The Memphite Tomb Chapel of Mose (Warminster 1977), p 25. The word 'estate' above renders hunpet, which Gaballa leaves untranslated. Dots represent places where the text is damaged.
tomb of someone who has profited from it. It therefore ought not to be valued in the way it would have to be if the date stood in a genuine papyrus. By contrast, the evidence cited above for the Years 1-13 are primary sources, and their sudden cessation must, in my opinion, be accorded a higher value than the dubious ('curious') copy in the tomb.

As regards the usual view that the years of the Amarna kings were ascribed to Haremheb already during his reign, perhaps one should point out that, on the contrary, we have evidence for the years presumably of Akhenaten being then characterised as those of 'the Lawbreaker', as in the letter which refers to the '9th year of the Lawbreaker' (ṣb) — the year when a man died who was the subject of an inquiry. It does not say, '9th year of Haremheb', as it really ought to have stated according to the received theory. Likewise the Mes inscription speaks of 'the time of the enemy of Akhenaten'. Thus, an 'amalgamation' of the reigns of the Amarna kings through (or under) Haremheb or under Ramesses II is not genuinely attested at all — on the contrary.

The Suppilluliumas Affair

With regard to the reign of Seti I, as well as Haremheb's, two interpretations may be distinguished: one which may be termed the 'mathematical' and one which goes further by also taking into account the solution's probability. This antithesis comes to the fore in the treatment which Kitchen gives to the question of Dahamunzu in his paper. With emphasis he characterises the identification of Niburuurja of the Suppilluliumas annals with Tutankhamun (Nb-hprw-R') as alone philologically and historically secure, and all other reflections as 'forced'; whereby he himself tries to foist his traditional idea on us 'in highly uncritical fashion'. Here the complete dependence on so-called philological criteria leads him to formulate these criteria in mathematical form, i.e., he presupposes that the transcription of foreign words — in this case, of the ancient Egyptian $t/bnt-nswt$ into the cuneiform Dahamunzu — is carried out according to fixed, inalterable laws which are universally valid.

But this is completely to overlook the human component, i.e., that these transcriptions are performed by human beings who are subject to outside influences, in our case for example linguistic customs, associations, mishearings. According to Kitchen only a Tahimini would be 'correct', and Dahamunzu is 'wrong'. But in speech, or when words are appropriated, there is no 'right' and 'wrong' — precisely because of the ever-present outside influences at the moment of appropriation. Certainly it is dangerous to carry through this 'mathematic' in the way Krauss does, following Fecht, and to conclude that the transcription Dahamunzu 'demonstrates' that already at that time an 'i' as in $hmt = him$ turned into 'a' as in Da-ham- ... , and to take that as proof that Niburuurja could also be transcribed as Nfr-hprw-R' = Akhenaten. Kitchen is absolutely right to reject this. But it is not a question of these 'philological' proofs, for we must come to terms with the fact that these are not absolute but depend, much more than we would like, upon external influences which we cannot turn into laws.

There remains for the question of who Dahamunzu was only the historical method. And in this case the assertions which Kitchen makes are not tenable. He believes, since Dahamunzu speaks about not having a son, that this can only be Ankhesnamun, the spouse of Tutankhamun (Nfr-hprw-R'), since the latter had no son. But neither Nefertiti nor Kyia, the wives of Akhenaten, had sons! Kitchen refers to the princes Smenkhare and Tutankhamun; they would have been at hand to ascend the throne — nonetheless, whose sons were they? Certainly not Akhenaten's, otherwise they would at some point have had to come forward, especially if the continuation of the dynasty depended on it. Surely they stem from a collateral line of the dynasty, for otherwise Tiy's lock of hair would not have been buried with Tutankhamun; but they had no right to the throne, and they were chosen as a last resort.

Kitchen does not go further into the improbability that arises when one accepts Ankhesnamun as the writer of the letter to Suppilluliumas, viz., her request for a Hittite prince is a coup d'état, and the Egyptian messenger calls these proceedings a 'disgrace' (tepnumar); underlying forces of the state prevent this 'disgrace' at the last minute through the murder of Zanaza — and Ankhesnamun as author of this coup d'état lived on peacefully and with honour, as evidenced by the ring that bore both her name and Ay's? It is precisely the emphasis of the so-called 'philological' method that compels him to believe what
is also the least likely, so long as the philology (Nib can ‘only’ be Nbi, never Nfr) is right. Again it must be stressed ‘for the umpteenth time’ that these laws as developed by philology are fallacious. He also does not discuss the contradiction that in Tutankhamun’s tomb king Ay, at the mummy of his predecessor Tutankhamun, performs the ceremony of the opening of the mouth. A royal burial took place ritually after 70 days, which allow no time for the several months of negotiations between Dahamunzu and Suppiluliumas, as described in the latter’s annals. But if Ay was already king at Tutankhamun’s burial — how can then the Hittite envoy hold back this fact from his master and not report that everything is over, since the Egyptians had already got a new king? Hence, with the identification of Dahamunzu with Ankhosnamun there pile up absurdities and improbabilities, which all immediately disappear if a widow of Akhenaten’s wrote the letter. This is also the only supposition which fits into the arrangement of the Suppiluliumas annals, since the report lies roughly in the middle of that long work (comprising at least 12 tablets).

Further, the text states that the ‘queen (SAL.LUGAL) who was Dahamunzu’ wrote the letter; why this circumlocution and why not, as expected, ‘Then the queen wrote’? It seems probable to me that this is an indication that it was not actually the ‘Great Royal Consort’ who wrote, but a wife who bore ‘only’ the title of a ‘King’s Consort’ (i3-hmt-nswt). Can it then be coincidence that this was just the case with Akhenaten, whose second wife went by the full title ‘only’ of ‘i3-hmt-nswt and Great Beloved of Akhenaten'? It is Kyia who writes the letter and then suffers the fate which usually meets the leader of a failed coup d’état. Incidentally, Nefertiti is not in question: not just because she vanishes at the end of Akhenaten’s reign; her daughters continue to hold her memory in esteeem in official inscriptions, as for example where they set up her name, ‘born of the Great Royal Consort Nefertiti,’ instead of the name of Kyia.

All in all, it should be clear that, contrary to Kitchen’s emphasis, only one historical sense emerges when we connect the Dahamunzu episode with the death of Akhenaten, whereas no sense emerges when we see Ankhosnamun in this queen — despite all ‘philology!’ And the same is true for the Hittite Nfr-h-prw-R² = Nibhunrija.

**Thutmose II**

That the length of Thutmose II’s reign is ‘a very open question’ may be readily admitted. Here the series of regnal years which have been cited in considering the reign lengths of Seti I and Haremheb are of no help, for until the beginning of Hatshepsut’s reign so few regnal dates have survived that we must reckon with fairly large gaps. If, as Kitchen does, one rejects every attempt to draw a measure of truth from Manetho’s data, few clues are in fact left over. That the Year 18 instanced by Daressy ought not to be used coming from a statue that has since gone missing — may be accepted following Beckerath’s explanation: insofar as the reading is correct at all, the statue belongs to the time of Hatshepsut, whose name Thutmose III replaced secondarily with that of his father, as is elsewhere attested. All we have left is the fact that at his death the two children Nofrure and Thutmose were still babies. Thus the addition of 3 regnal years does command a certain probability.

From these examples the following conclusion should be drawn. Even where the issues are chronological we ought not to put too much trust in strict mathematical logic; rather, we must take into account the human components, whereby the main criterion must be historical probability alongside an emphasis on definite presuppositions which are connected with this probability, for example —

1. In epochs in which such a density of dating evidence exists that almost every regnal year of a king is attested, fairly large gaps in the data are unlikely. That applies to our discussion of Seti I and Haremheb. Thetreto apparently contrary indications (the Bekenkhons details, the Mes date) should be subordinated and for them an explanation found which first of all must be sought in the human realm.

2. Philological facts are not binding, because they are not invulnerable and not to be regarded as mathematical conclusions excluding everything else.

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### The Genealogy of Nespahehenhat

<table>
<thead>
<tr>
<th>Year</th>
<th>Name</th>
</tr>
</thead>
<tbody>
<tr>
<td>790</td>
<td>Nespahehenhat – temp. Osorkon I</td>
</tr>
<tr>
<td>810</td>
<td>Nespatytwy (b)</td>
</tr>
<tr>
<td>830</td>
<td>Khonskhu</td>
</tr>
<tr>
<td>850</td>
<td>Nespatytwy (a)</td>
</tr>
<tr>
<td>870</td>
<td>Ankhef</td>
</tr>
<tr>
<td>890</td>
<td>Nesamun</td>
</tr>
<tr>
<td>910</td>
<td>Ipuy (b)</td>
</tr>
<tr>
<td>930</td>
<td>Iufenamun – Ramesses II/Merenpah (?)</td>
</tr>
<tr>
<td>950</td>
<td>Ipuy (a) – temp. Ramesses II</td>
</tr>
<tr>
<td>970</td>
<td>Roma – temp. early Ramesses II</td>
</tr>
</tbody>
</table>

The genealogy recorded on Statue 42189 in the Cairo Museum. Bekenkhons was almost certainly the brother of Ipuy, the ninth person in the above genealogy. He succeeded his father Roma as Second Prophet of Amun and is known to have attained the office of High Priest of Amun by Year 46 of Ramesses II. Chronology based on an average 20 years per generation (from D. M. Rohl, A Test of Time, p 381).
(Nibhururija). What is decisive even in this case is historical probability (Ankheshnamun). As a parade example one can add here the coregency which is winkled again and again out of certain clues between Amenhotep III and Akhenaten. The improbability that in one and the same kingdom two mutually antagonistic ideologies should hold sway without any visible trace of their antagonism (Amenhotep III builds peacefully for Amun in Karnak, Akhenaten meanwhile has the name of Amun scratched out, etc.) can really only not occur to people who regard even the history of the Egyptians as exercises in arithmetic and not as the politics of human beings.

It might prove helpful for a better understanding of the events of those times if such a person now and then cast a look at his own time. □

Notes and References

1. Originally published as ‘Zur Chronologiediskussion über das Neue Reich‘ in Ägypten und Levante III (1992); here translated by Steven Robinson and re-published with the kind permission of the original editor, Manfred Bietak, and the author’s widow, Erika Helek.
2. Papyrus Leiden 1 350 verso.
5. KRI II, pp 67ff.
7. The possibility also exists that a 14th year of Haremheb is attested: the fragmentary docket in KRI VII 57, 12.14 from Deir el. Medina seem to be similar in form to those cited in KRI VII 58, 2, where the 13th year and the House of Haremheb are mentioned. Since apparently Deir el Medina was supplied only by the reigning king, this Year 13 can be assigned to Haremheb; therewith perhaps also the 14th. And if we read the year which in KRI VII 58, 10 is restored to Year 34 as 14, we would have evidence for it, since the ‘House’ of Haremheb is mentioned. Here there do exist various kinds of uncertainty: on the other hand, nothing of a chronological nature need change, because it was also previously possible to accept that Haremheb reigned for a full 13 years and died in his 14th. – Since the form of the inscription is similar, the docket in KRI VII 57, 13 and 11 should perhaps also be assigned to a 1st or 2nd year of Haremheb.
12. KRI III 158, 15.
14. This objection falls away if R. Denkhaus (MDAIK 39, 1983, pp 29f) is right in his opinion that Ay had transferred Tutankhamun’s mummy and grave goods from his original tomb in the western valley of the Valley of the Kings to his present one (KV 62), in order to take over Tutankhamun’s tomb (KV 23) himself. For this rebuttal can have happened only at a time that was somewhat removed from the first enthronement.
16. The proposal would be to amend the 13 years which Manetho ascribes to Chebron to 3 years. See W. Helek: MDAIK 17 (1961), p 109, and E. Hornung: ‘Untersuchungen zur Geschichte und Chronologie des Neuen Reiches‘ in Archäologische Anzeige 11 (1964), p 32. [SJF]

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