

Appendix A

One of the few reasonable objections that can be raised to our proposed displacement of the Saite dynasty has already been dealt with. We refer to the supposed occurrence of the name Shabataka in an inscription of Sargon II discovered at Tang-I-Var and dated by the Assyriologist Grant Frame to 706 B.C.?. Critics of the current revision will be quick to point out that this date is reasonably close to the traditional dates for the 25th dynasty king who bears this name. That fact alone is sufficient to convince those who are already predisposed to maintain the status quo, of the reasonableness of their position. For the record, however, we present in greater detail our opinion of this inscription and the frailty of the conclusions based upon it.

The Tang-i Var Discovery

"One of the most important results of a series of surveys carried out by the Archaeological Service of Iran in 1968 was the discovery in Iranian Kurdistan of a neo-Assyrian relief with a cuneiform inscription. The relief is carved into a niche on the flanks of the Kuh-i Zinaneh in the Tang-i Var pass near the village of Tang-i Var". "Cut into the face of a vertical cliff at a point 40 m. above ground level" the relief is both difficult to access and badly preserved. Photographs and casts of the relief were made by the Iranian authorities and the find was announced and discussed by 'Ali Akbar Sarfaraz in a 1968-69 article in the journal *Iran*. Several photographs and some sketches were included in the journal article. "Regrettably, the inscription is badly worn and is basically illegible from the published photographs and copy." The Iranian authorities did not recognize its significance.

The inscription attracted very little attention in the western world. The journal article, written in Farsi, was largely ignored, though a few Assyrian specialists made note of its existence, and at least in one instance additional photographs were sought (see below). The fact that "it might be attributed to Tiglath-pileser III or Sargon II was first suggested by Julian Reade in an article published in 1977", based on the legible portions of the text in photographs accessible to him.

The situation changed dramatically only a few years ago. We quote Grant Frame, the Assyriologist responsible for the recent developments:

While carrying on work on the inscriptions of Sargon II for a volume of the Royal Inscriptions of Mesopotamia project, I noted the possibility that the inscription at Tang-i Var would need to go in that volume. Having been informed by Dr. I. D. Levine that Dr. Francois Vallat had taken photographs of the relief in the early 1970s, I wrote to Dr. Vallat about the matter. With great

generosity, Dr. Vallat has allowed me to make use of all the photographic material that was in his possession (22 slides) and the edition presented below is based solely on this material. Dr. Vallat has informed me that he went to Tang-i Var in 1971 on behalf of the Susa mission and at the request of R. Labat. Dr. Vallat described the current state of the inscription and the difficulty he had in photographing it while dangling from a rope.⁴⁶¹

The photographs obtained from Dr. Vallat were of sufficient quality to permit Dr. Frame to transcribe, then transliterate and translate the Tang-i Var inscription. The results of his efforts, and the revelation that the inscription contained a reference to the 25th dynasty king Shabataka, were announced to the scholarly world in 1999 at a March 20th session of the 209th meeting of the American Oriental Society (with the innocuous title "The Rock Relief at Tang-i Var") , and later that year in the journal *Orientalia* in an article entitled "The Inscription of Sargon II at Tang-i Var". The latter article is the source of all the information quoted above.

It is important to note that the photographs obtained by Dr. Vallat, those on which Frame depends for his transcription, are of poor quality. The reflection of the sunlight, the shadows created by the impressions of the cuneiform signs and by the sides of the niche into which the inscription was placed, and the deterioration of the rock itself, combine to obscure much of the text. Caution suggested to Dr. Frame that he should perhaps wait to obtain confirmation of his tentative transcription. Scholarly considerations encouraged an immediate publication. He describes his dilemma in the article:

Because of the damaged nature of the inscription and the fact that some sections are inevitably better represented by the photographic materials than others, some parts of the text cannot be read and the reading of others is tentative. Nevertheless, it is possible to read a large portion of the inscription and to be certain that the ruler responsible for it was the Assyrian king Sargon II (721-705 B.C.). In view of the importance of the inscription and the length of time which it has lain unpublished it seems best to present this preliminary edition as it is, rather than wait until further photographs and/or casts should become available or until someone is able to collate the inscription in situ. Since any copy of the text made by me would be based solely upon the available photographic materials, I have decided to provide here the most useful photographs of the inscription (Figs. 3-18) rather than a copy of the text. (p. 34-5)

The Tang-i Var inscription was clearly the production of the Assyrian king Sargon II. It is, in fact, a summary of the major military accomplishments of his reign, the latest of which occurred in the last years of his life, perhaps as late as the year 707 B.C. Duplicate versions of many of the same events can be found on inscriptions in this king's palace at

⁴⁶¹ *Orientalia* 68 (1999) 34

Khorsabad (Dûr Sharrukîn). The relevant section of Frame's translation, that which refers to the Iamani incident which provides the context for the inclusion of Shabataka's name, is contained in lines 19 to 21 of the inscription.

19 I plundered the city of Ashdod. Iamani, its king, feared [my weapons] and ... He fled to the region of the land of Meluhha and lived (there) stealthfully (literally: like a thief). 20 Šapataku' (Shebitku), king of the land of Meluhha, heard of the mig[ht] of the gods Aššur, Nabu, (and) Marduk which I had [demonstrated] over all lands ... 21 He put (Iamani) in manacles and handcuffs ... he had him brought captive into my presence.

Before proceeding we note one anomaly in Frame's translation, one to which we will return briefly in the final section of our discussion. Line 20 concludes with an indication (...) that words at the end of the line may be obscured in the cuneiform text. Line 19 does not. In fact, the sense of the translation assumes that no text intervenes between the flight of Iamani described in line 19 and the reaction of Shabataka in line 20. But in fact the end of every line in the Tang-i Var inscription is obscured by shadow, a fact indicated by the repetition of the symbol [(...)] at the conclusion of every line in the transliterated text that precedes the translation in the *Orientalia* article. As Frame notes at the beginning of his commentary, "Due to shadow on the photographs, it is not always clear how much, if anything at all, is missing at the right end of the lines. While [(...)] has regularly been put at the end of lines in the transliteration, the translation has generally ignored this." (p. 41). Clearly in line 19 Frame has decided that nothing is missing.

The Iamani incident referred to in lines 19-21 is known from three other inscriptions on the walls of Sargon's palace, and from a stela found elsewhere. It is important to review the details of the event insofar as they are known. We quote from an article by the Egyptologist D.B. Redford in an 1985 article in the *Journal of the American Research Center in Egypt*.

Thanks to a variety of studies over the last 25 years, the year 712 B.C. has emerged as an anchor date in the history of the Late Period in Egypt. The general course of events leading up to and culminating in the Assyrian campaign against Ashdod in that year is now fairly sure, and may be sketched as follows. Sometime early in 713 B.C. the Assyrians deposed Aziri, king of Ashdod on suspicion of *lese-majeste*, and appointed one Ahimetti to replace him. Very shortly thereafter, however, and probably still in 713, a spontaneous uprising of the Ashdod populace removed this Assyrian puppet in favor of a usurper Yamani. Throughout the fall and winter of 713 Yamani contacted the other Philistine cities and the inland states of Judah, Moab, and Edom in an effort to organize an anti-Assyrian coalition, and sent to "Pharaoh (Pir'u) king of Egypt" for aid. In the spring of 712, however, Sargon dispatched the tartan with a detachment of troops against Ashdod, and Yamani fled in haste to Egypt. Unable to find a safe haven in Egypt, Yamani passed clean through the land

ana ite Musri sa pat Meluhha, "to the frontier of Egypt which is (contiguous) to the territory of Kush." At this point he fell into the hands of the king of Kush who, at an unspecified later date, extradited him to Assyria. (pp.6-7)

The point being made by Redford in this article is that the king of Egypt (Pir'u, king of Musri) mentioned in the Assyrian annals must be the last king of Manetho's 24th dynasty (Bocchoris). He is clearly distinguished from the king of Melukkha., who must therefore be Shabaka, the king in waiting. [For the contrary opinion held by K.A. Kitchen and others, see below] It follows from these considerations that Shabaka has not yet conquered Egypt. Largely on this basis Redford, and many others, argue that Shabaka's conquest of Egypt must have taken place in 712 B.C. at the earliest. The chronology is very tight.

If the Assyrians attacked at the earliest possible time of the year (Nisan) in 712, and if Yamani took to flight immediately, he would scarcely have reached Kush in less than 5 or 6 weeks, which would put his arrival there no earlier than the middle of June. We would be safe in assuming, however, that by the end of the summer he had attained the frontier of Kush. By using an unusual locution, the Assyrian scribe explicitly conveys an important fact which he is aware of, viz. that Yamani's flight took him all the way to the border between Egypt and Kush at Elephantine, and that only then did he enter territory belonging to the Kushite king. In other words, the campaign of Shabaka which finally won Egypt for the 25th Dynasty and terminated the regime and life of Bocchoris, had not yet taken place by the late summer of 712 B.C. (p. 8)

Redford goes on to conclude that the invasion of Egypt by Shabaka most likely occurred late in 712 B.C., and, since dated monuments of this king attest a reign of at minimum 15 years, he derives the dates 712-698 B.C. for this king. Based on other consideration, not discussed in his 1985 article, Redford dated the reign of Shabataka, Shabaka's successor, to the years 699-690 B.C., assuming a brief "coregency" between Shabaka and Shabataka. For Redford, as for all Egyptologists, the 690-664 B.C. reign of Taharka, the ultimate king of the dynasty, is accepted as axiomatic.

Frame recognized from the outset that the Tang-i Var inscription provides "an important piece of new information with regard to the episode of Iamani of Ashdod, a piece of information which will require Egyptologists to revise their current absolute chronology for Egypt's twenty-fifth dynasty." By "new information" he refers to the newly revealed identity of the king of Melukkha who captured Iamani and surrendered the fugitive king to Sargon. Without exception, all Egyptologists prior to the Tang-i Var discovery had identified this king as Shabaka. For Redford, the identification was pivotal. Before we read Frame's opinion on the import of the new discovery we should understand clearly the two schools of thought which preceded the Tang-i Var discovery.

One group of scholars, of which Redford is a major proponent, understand from the Assyrian texts that in 712 B.C. Egypt was ruled by a single pharaoh, referred to as "Pir'u

king of Musri", who must be distinguished from the king of Melukkha referred to in the same texts. As we have already stated, only one possible scenario presents itself once this political bifurcation is assumed. Bocchoris must be the king of Egypt; Shabaka must be the king of Melukkha, soon to invade and conquer the country and begin the 25th dynasty.

The other group of scholars, represented most notably by K.A. Kitchen, believe that "Pir'u, king of Musri" and the "king of Melukkha" are one and the same king. They date the reign of Shabaka on other bases, and interpret the Assyrian references in ways that will support their independently established chronology. Kitchen, for example, places Shabaka in the time frame 716-702 B.C., overlapping the Iamani incident. This compels him to equate the kings of Musri and Melukha. (TIP 341) That equation does not derive naturally from a reading of the Assyrian texts.

The revelation that the king of Melukkha in the Iamani incident is Shabatata, not Shabaka, requires a dramatic reconstruction for both of these groups. In order to better understand the implications of the discovery we reproduce below the four possible chronologies which might explain the new data. The four groups represent the only three possible identifications of "Pir'u, king of Musri" who ruled Egypt at the time of the Ashdod rebellion in 713 B.C.

Table 19: Chronology assuming Pir'u is not Shabaka (Redford Group)

.Date	Egyptian Pharaoh	King of Melukkha
713 B.C.	King of 24th or prior Dynasty	Shabaka
712 or soon thereafter	Shabaka invasion	Shabaka
706 or x years earlier	Shabaka	Shabatata replaces Shabaka (Iamani incident soon after)

Table 20: Chronology assuming Pir'u is Shabaka (Kitchen Group)

Date	Egyptian Pharaoh	King of Melukkha
713 B.C.	Shabaka (y years after conquest of Egypt)	Shabaka
706 or x years earlier	Shabaka	Shabataka replaces Shabaka (Iamani incident soon after)

Table 21: Alternate Chronology assuming Pir'u is Shabaka

Date	Egyptian Pharaoh	King of Melukkha
713 B.C.	Shabaka (y years after conquest of Egypt)	Shabaka
intermediate year	Shabaka dies. Shabataka replaces Shabaka	Shabataka
706 or x years earlier	Shabataka	Shabataka (Iamani incident)

Table 22: Chronology assuming Pir'u is Shabataka

Date	Egyptian Pharaoh	King of Melukkha
713 B.C.	Shabataka	Shabataka
706 or x years earlier	Shabataka	Shabataka (Iamani incident)

We will discuss aspects of these four chronologies later. It is time to hear Frame's impression of the newly discovered text:

Neither the Display Inscription nor the Display Inscription of 'Room XIV mentions the name of the ruler of Melukkha who sent the fugitive Iamani to Sargon for punishment and it has generally been assumed that the ruler in question was Shabaka (or Shabako), a ruler of Egypt's twenty-fifth dynasty". This dynasty, also known as the Kushite or Napatan dynasty, came from Nubia, which was referred to by the ancient place name "Melukkha" in Neo-Assyrian texts. Shabaka's reign has been dated to 716-702 B.C. by most recent scholars, although the possibility that he reigned 714-700, with the last two years of his reign being a coregency with his successor has also been raised". Shabaka was succeeded by his nephew Shebitku (or Shabataka), the son of Shabaka's predecessor, Piankhi. Most scholars have assumed that Shebitku's reign should be dated to 702-690 B.C., thus assuming that he ascended the throne during the reign of Sennacherib". Line 20 of the Tang-i Var inscription, however, states that the ruler who extradited Iamani during the reign of Sargon was Shebitku (^mša pa ta ku ^lu). This would thus raise difficulties for the current Egyptian chronology. The Tang-i Var inscription almost certainly dates to the year 706 (see above) and must have been composed before Sargon's death in 705. The other two inscriptions of Sargon referring to the forced return of Iamani also date from late in Sargon's reign. Both the Display Inscription and the Display Inscription of Room XIV record matter involved with the completion of the new capital of Dur-Sarrukin and, as already mentioned, that city was inaugurated on the sixth day of the second month of 706. Thus, the Tang-i Var inscription would indicate that Shebitku was already ruler by 706, at least four years earlier than has generally been thought. We still do not know exactly when Iamani was delivered up to Sargon. Sargon's capture of Ashdod is recorded in the king's annals for his eleventh regnal year, but no mention is made of either Iamani's flight or extradition. It is not impossible that although Shebitku was ruler at the time the Tang-i Var inscription was composed, Iamani had actually been returned in the time of the previous ruler, Shabaka. The Assyrian scribe of the inscription could conceivably have credited the action to the current ruler of Egypt rather than the preceding one. Nevertheless, it seems likely that sometime between about 712 and 706 Iamani was delivered up to Sargon and that the Kushite ruler of Egypt at that time was Shebitku: certainly Shebitku seems to have been ruler by 706. (pp. 53-4)

We immediately notice two things in Frame's discussion of the Shabataka reference. In the first place he doesn't mention the Redford chronology, though perhaps deliberately, since, as we shall see, Redford provides an accompanying article to explain his point of view. We are more surprised to see Frame describing the Iamani incident as if it might have taken place in 706 B.C.? What is happening? The answer is transparent. He is reinterpreting the history of the Iamani incident in such a way as to marginalize the

problem. All interpreters do the same. The response was predictable. But we should read his comments carefully on that account. We would miss the point of Frame's discussion if we think that the only problem that results from the Tang-i Var text is the necessity of dating the beginning of Shabataka's reign four years earlier than is typically done. What is the gist of Frame's argument?

There exist only two possible interpretations of the Iamani incident in light of the revelations of the Tang-i Var inscription, read in conjunction with the parallel texts. Frame discusses them both. Either Shabataka was the Melukkhan king who captured and extradited Iamani, or he was not.⁴⁶² If Shabataka was the Melukkhan king, then the most pressing question is the date of the Iamani incident. There is really only one possibility - 712 B.C. But the earlier the date, the greater the problem for Egyptologists and traditional historians. Thus the balance of Frame's discussion argues the possibility that the Iamani incident took place later than 712 B.C. Frame spends most of his time demonstrating that the Tang-I Var relief was inscribed around 706 B.C., thus establishing the latest possible date for the capture and return of Iamani. This is all fine and good, as long as we do not confuse "lowest possible date" and "lowest probable date". There are good reasons for maintaining the 712 B.C. date. We cite the two most obvious.

1. A straightforward reading of the Assyrian texts tells us that the Iamani incident took place in 712 B.C. All the discussion in the world does not change that fact. All interpreters of the Assyrian texts for well over a century, scholars from all persuasions, have concluded that Iamani was captured soon after his flight to freedom. Even Redford assumed that as a fact when, in his article quoted above, he stated that "at this point he (Iamani) fell into the hands of the king of Kush who, at an unspecified *later date*, extradited him to Assyria." But it matters not at all if Iamani was captured in 712 B.C. and returned to Sargon as much as six years later. The parallel texts state that the king of Melukkha who captured Iamani also acted to return him to the Assyrian king. And the Tang-i Var inscription attributes both actions to Shabataka. If we assume a period of years between the capture and return of Iamani, that merely forces us to move the beginning of Shabataka's reign back an identical number of years. We will assume, in the discussion that follows (and in the tables above), that extradition followed capture immediately.

2. The fact that the palace inscriptions and the Tang-i Var text were inscribed in the year 706 B.C. all but demands that the incidents referred to, including the activities of Shabataka, took place at a much earlier date. Those annals look back as far as the beginning of Sargon's reign. The *latest* datable incident took place in 707 B.C., but most are much earlier. And why, we ask, would the annalist not indicate a separation in time

⁴⁶² There is no point our discussing error in this essay, though I suspect the possibility will be raised more and more as Egyptologists scramble to escape the dilemma caused by the presence of Shabataka's name. If an error is to be conjectured why not simply assume that the annalist spelled the name of Shabaka wrong? If so we can end our discussion here.

if one took place. All versions of the Iamani incident which document the flight of Iamani in 712 B.C. go on immediately to recount his capture and extradition and then proceed to relate other accomplishments of Sargon. There is no hint that the return of Iamani was the last event in Sargon's illustrious career, an addendum added to his annals at the last moment. Unless we assume that the Shabataka news was "hot off the press" and that the return of Iamani took place as the inscriptions were in progress, only to be inserted into the text of an incomplete story, then we must date the return of Iamani much earlier than 706 B.C. How much earlier is the only question?

But determining the date of the Iamani capture is not the only problem that manifests itself when we examine the chronological possibilities. In two of the four suggested chronologies (tables 19 and 20) we have the untenable circumstance of Shabaka ruling Egypt while Shabataka is ruling Melukkha. This scenario makes absolutely no sense. There are a host of objections. While the possibility of a coregency between the two pharaohs has often been proposed, no scholar has ever suggested that Shabaka yielded sovereignty of Melukkha to his nephew shortly after he conquered Egypt. And why would Shabataka, ruling over a country far removed from the influence of the Assyrians, feel any need to kowtow to Sargon, when Egypt, ruled by his uncle, served a protective buffer shielding him from any possible harm. Why also would Shabaka allow his nephew to take this action, something which he himself had refused to do for a number of years? A final problem centers around the status of Melukkha as a national entity. Sargon speaks of Melukkha as if it were a separate country, not a province of the Egyptian Empire. Shabataka's action in capturing and extraditing Iamani is that of a sovereign king of a sovereign nation. This cannot be a "coregency" under any circumstance.

Any explanation proposed for the Tang-i Var inscription which assumes that Shabaka is ruling in Egypt with Shabataka as co-ruler in the extreme south, bearing the title king of Melukkha, must, in our estimation, be set aside. The text of the palace Display Inscription clearly portrays the king of Melukkha as a sovereign in his own right ruling in the remote regions bordering on the south of Egypt, not the nephew of the king of Egypt, sharing his throne. Listen to the annalist!

The king of Melukkha who [lives] in [a distant country], in an inapproachable region, the road [to which is ...], whose fathers never - from remote days until now" - had sent messengers to inquire after the health of my royal forefathers, he did hear, even (that) far away, of the might of Ashur, Nebo (and) Marduk. The awe-inspiring glamour of my kingship blinded him and terror overcame him. He threw him (i.e. Iamani) in fetters, shackles and iron hands, and they brought him to Assyria, a long journey. ANET 286

Under no circumstances can the king of Melukkha and the king of Egypt be considered as close relatives. The suggested chronologies in tables 19 and 20 must be rejected outright. The two remaining viable alternatives (tables 21 & 22) assume that either Shabaka or Shabataka ruled the whole of Egypt and Melukkha in 713 B.C. And since we

argue that the Iamani incident took place in or near 712 B.C., there is little to choose between the two possibilities.

Both these remaining options are fraught with difficulties. Both require assigning Shabataka a reign length of upwards of 22 years. And both depend on the identity of the Pir'u of Musri and the king of Melukkha in the Assyrian annals, a contentious issue discussed earlier in this essay. Redford justifiably argues for a distinction between the two kings. That is what the Assyrian texts seem to describe. We agree with him. But if so then these two remaining chronologies must also be rejected. We are faced with a classic case of "damned if you do" but "damned if you don't". We cannot tolerate a distinction between the two kings; neither can we justify equating them.

It should follow from the argument thus far that none of the proposed chronologies adequately explains the data in the Assyrian annals. The only legitimate response is to question the viability of the traditional history which creates the problem.

Frame is clearly uncomfortable with the implications of the inscription (why else would he mention the possibility of error) and in the *Orientalia* article he turns the discussion over to his colleague, none other than the same D.B. Redford quoted many times already, whose history is most severely challenged by the newly discovered text.

My colleague at the University of Toronto, Prof. Donald B. Redford has kindly prepared a brief study of the implications this passage in the Tang-i Var inscription has for Egyptian chronology of the Twenty-Fifth Dynasty and it is presented immediately following this article. (p. 54)

Redford's *Orientalia* article is brief, as its title indicates.⁴⁶³ As he had done in 1985, Redford continues to maintain the centrality of the year 712, though he does make some concessions. Shabaka's invasion of Egypt, he argues, need not have taken place in 712 B.C., but it must postdate the year 713 B.C.. His revised chronology is essentially that represented by our table 19. He begins by espousing the 706 B.C. minimum date for the beginning of Shabataka's reign, as if that were established as the probable date by Frame. To make matters worse he actually reduces the date to 706-705 B.C., with a hint that even the year 705 B.C. is possible, as if Iamani could be captured, transported two thousand miles to the Khorsabad palace, while news of the event was transmitted to Tang-i Var, there to be added to the rock relief, all this in the last days of Sargon's life.

"The death of Sargon II is now the *terminus ante quem* for the appearance of Shebitku with a title indicating the status of a head of state. Since the stela most probably was inscribed in Sargon's final year, the extradition of Yamani could have taken place as late as 706-705 B.C.". (p. 58)

⁴⁶³ D.B. Redford, "A Note on the Chronology of Dynasty 25 and the Inscription of Sargon II at Tang-i Var", *Orientalia* 68 (1999) 58-60

To be fair Redford does express a reservation. He acknowledges that the extradition of Yamani "*could have taken place* as late as 706-705 B.C.". But the initial caution is soon set aside and Redford proceeds on the assumption that this latest possible date for the beginning of Shabataka's reign is in fact the correct date. He seems preoccupied with determining the *chronological* consequences of Shabataka ruling alongside Shabaka in 705 B.C., rather than explaining the problematic character of this assumed coregency. For the record we quote from his *Orientalia* article:

If Shabaka maintained the same sequence of regnal years from his accession, presumably on the death of Pi(ankh)y, and did not associate Shebitku with him as coregent, then the new evidence would oblige us to place his accession no later than 720 B.C., and possibly higher according as his reign exceeded 15 years. His conquest of Egypt, clearly accomplished by his year 2, and the consequent termination of the 24th Dynasty, could have fallen no later than 719 B.C. But this presents a major difficulty. For the decade preceding 713/12 B.C. the Assyrian and Biblical records contain several allusions to Egyptian rulers, but none of them can be interpreted as an allusion to a Kushite king ruling over Egypt. The individuals referred to must, in fact, be the ephemeral or otherwise unidentified rulers belonging to pre-25th Dynasty regimes. The implication, then, is that as late as 713 B.C. when Yamani sought aid from Pir'u, Shabaka's invasion had not yet taken place. It follows that Shabaka year 1 must fall in or after 713 B.C., and his 15th in or after 699 B.C. (p. 59)

The proposed dates for Shabaka have not changed appreciably since Redford's 1986 article. There is no point in objecting. It is not the date when Shabaka began to rule which causes the problem with the chronologies of tables 19 and 20. It is the fact that the dual kingships of Shabaka and Shabataka make no sense. Redford does discuss the joint reign but is clearly uncomfortable with the concept. Shabataka, he claims, had the "trappings of kingship" as he "administered" the Kushite homeland. (Let the reader decide if this description suits the king of Melukkha in his actions vis-à-vis Iamani and the Assyrian ruler.) He fails to discuss the problems associated with the extended length of Shabataka's reign.

One solution might be to resuscitate the old theory of a "coregency" between Shabaka and Shebitku, although in the past such a notion has won only half-hearted acceptance. One is struck by the fact that, while Shabaka's dated texts (in the main private documents from the business community or the chancery, and therefore liable to date by the years of the regime in power) are relatively plentiful from the thebaid to the Delta, Shebitku's are conspicuous by their absence. Only the year 3 graffito from the Karnak quay is a clearly attested text germane to the problem of a coregency. It is conceivable that, with Shabaka's conquest of the lower Nile valley and the subsequent removal of the royal administration to Memphis, the conqueror had created the necessity of a bifurcation in the government of his vast dominion. While his own reign

provided the dating scheme, Shabaka had, by his 8th year, realized the need to separate off the administration of the Kushite heartland; and this he assigned to his nephew Shebitku, complete with the trappings of kingship. One of the latter's initial acts would have been the extradition of Yamani who, as the text says, had fled ana pat Meluhha, and had been living there ever since." (p. 60)

For the most part Redford has avoided dealing with the issues we raised earlier. He had little choice. The problems have no ready answer in the framework of the traditional history.

We conclude this section of our discussion with a few remarks on the length of Shabataka's reign, since this is the second difficulty with the remaining viable chronologies (tables 21 and 22), those which assume that Shabataka ruled both Egypt and Melukkha for upwards of 22 years.. Lest the reader fail to appreciate the extent of this aspect of the chronological problem we should review a few details of the traditional history of the 25th dynasty. In that history it is an accepted fact that the terminal king Taharka ruled Egypt for 26 years, from 690-664 B.C.. Those dates are incontrovertible and are considered "certain", not only by Egyptologists, but by scholars from all related persuasions, and even by the growing field of "revisionists" who question the reliability of Egyptian chronology in earlier ages (the present author excepted). The dates for Taharka cannot move by more than a year without causing serious disruption to the entire history of the late period. Dates earlier than Taharka are in flux. The only certain date for earlier kings is the year 690 B.C. for the death of Shabataka. The only remaining question regarding this king is the date when his reign began. At most he is assigned 13 years, based largely on Manetho, as interpreted by Africanus (who records 14 years) and Eusebius (who records 12 years). At minimum he is credited with 3 years, based on the fact that no monument exists bearing his name with a year date higher than that number. His dates vary accordingly from 703-690 B.C. to 693-690 B.C.

The higher number for Shabataka (13 years) is adopted for one reason only, to assist in solving a problem of long standing in biblical interpretation. The text of 2 Kings 19:9, which describes the 701 B.C. siege of Jerusalem by Sennacherib, Sargon's successor on the Assyrian throne, relates how the Assyrian king responded "when he heard concerning Tirhakah, king of Cush, that he was coming out to fight against him". Tirhakah in this text is identified with Taharka by all but a few scholars (interestingly, Redford is one of the exceptions), this in spite of the fact that Taharka's kingship began eleven years later. The identification makes no sense, but is maintained nevertheless for expediency. Who else could Tirhakah be if not the 25th dynasty king in waiting, since the 25th dynasty is firmly entrenched in that time frame in the traditional history.? But since a hieroglyphic text, originating from a temple in Kawa, states specifically that Taharka was summoned to Egypt for the first time by his brother Shabataka, at the time ruling within Egypt, it must be the case that Shabataka's reign began some time prior to 701 B.C.. It follows that Shabataka's reign should be dated at least as early as 702 B.C., *in spite of the lack of monumental evidence.*

The lengthening of Shabataka's reign to accommodate the reference to Taharka as king of Egypt in 701 B.C. is a classic case of one mistake begetting another - bad history creating further historical error. There is no monumental evidence suggesting that Shabataka reigned more than a few years. In all of Egypt there are but a handful of allusions to his presence. Raising the length of his reign from three to twelve or thirteen years, as was done before the Tang-i Var discovery, was problematic enough, some would say completely unjustified, a conclusion reached by many scholars who refuse to credit this king with more than the 3 years demanded by the monuments, this in spite of the supposed biblical reference to Taharka. To now raise the length of his reign to 22 years and counting goes beyond the pale. And what will be the outcome when historians attempt to fit the 727-712 B.C. minimum dates for Shabaka into the already crowded decades of the late 8th century. It will be interesting to see how the debate continues.

With this brief history in view we ask the question which prompted this entire discussion. How does the revised history explain the reference to Shabataka in a Sargonid text inscribed in the year 706 B.C. apparently attesting the reign of this Melukkhan king as early as 712 B.C.? For it goes without saying that if the "king of Melukkha" named "Shabataku" is the same as the 25th dynasty king Shabataka, the successor of Shabaka, then the entire argument in *Nebuchadnezzar & the Egyptian Exile*, the initial book in this *Displaced Dynasties* series, is null and void. In the revised history detailed in this book the Egyptian reign of Taharka, the successor of Shabataka, began in 569 B.C., not in 690 B.C., and it ended abruptly six years later (564 B.C.) when he was driven from Egypt by the invasion of Nebuchadnezzar. According to this same history Shabataka, whose reign preceded that of Taharka, must have died in 569 B.C. He cannot have acted in 712 B.C. or even in 705 B.C. to return the exiled Iamani to the Assyrian king Sargon.

It appears at first glance that the critic is justified in citing the Tang-i Var inscription as the death knell of the revised history? But the pronouncement of doom is premature. The criticism is without foundation. We object on two grounds. On the one hand we argue that the name of Shabataka is not actually present in the text, i.e. that Grant Frame has mistranscribed the cuneiform inscription in line 20. On the other hand we maintain that even if the name were present the problems it creates belong exclusively to the traditional history, not the revised alternative. We argue the case in the reverse order, first assuming the presence of the name, then arguing for its absence.

Shabataku - Brother or Ancestor of Taharka?

It seems strange to this author that any knowledgeable critic would argue that a king of Melukkha named Shabataku, of uncertain ancestry, named in a neo-Assyrian inscription dated late in the 8th century, *must necessarily* refer to the 25th dynasty Egyptian king of Cushite origin named Shabataka who ruled Egypt immediately prior to Taharka. We admit the attractiveness of this identification for proponents of the traditional history.

Both kings have the same name. Both kings are connected directly or indirectly with a land contiguous with Egypt, possibly, though not necessarily, identical with ancient Cush. And most significantly for proponents of the traditional history, the dates for Shabataka are in reasonable agreement with the c.a. 706 B.C. date assigned to the Iamani incident by Frame and Redford. But "reasonable agreement" is the operative phrase. As we have just seen, the name creates severe problems for the traditional history. Not so for the revised history.

According to Manetho the 25th Egyptian dynasty consisted of "three Ethiopian kings" named Sabacon, Sebichos and Tarcos, identified by Egyptologists as the kings Shabaka, Shabataka, and Taharka of the monuments. Beyond the fact that Shabataka and Taharka are brothers, nothing definite is known about the ancestry of these kings. Even their parentage is not clearly established. It is assumed that Shabaka is of the generation prior to Shabataka and Taharka, and that he is possibly the father of the two brothers, but that fact is not certain. It is further assumed that Shabaka was either a brother or possibly a son of Piankhi who conquered the Egyptian delta earlier, in his 20th year. But again there is no confirmation of the fact. Of the ancestry of this royal family earlier than Piankhi very little is known. Some connection with a king named Kashta is conjectured. Others relate the family to the enigmatic king Rudamon, whose life intertwines with events discussed earlier in this book. Absolutely nothing is known of the generations of this family further back than Kashta.

In light of this lack of knowledge we might well rest our case. We have positioned Shabaka and Shabataka in the time frame 640-570 B.C.. The presence of an ancestor three or four generations removed, identified as a king of Melukkha and bearing a similar name, can hardly be considered a serious objection to the revised history. It would be surprising indeed if several of the ancestors of Shabaka and Shabataka did not bear this name. Several factors combine to argue the case. They have been mentioned in an earlier chapter of our book, but deserve repetition.. The most obvious is the fact that two of the three named kings of the dynasty bear the name in variant forms, attesting, if nothing else, its popularity. It is said that the name *sab* means "wild cat", an appropriate and therefore desirable name for a tribal chieftain who wished to highlight his cunning and physical prowess. According to Flinders Petrie in his discussion of the name of Shabaka:

The present Nubian for the male wild cat is *Sab*, and *ki* is the article post-fixed. Hence in popular talk it is very likely that the king was known as *Sab* or *Shab*...
HE III 284

Petrie argues further that the name passed from father to son, and that in consequence Shabataka was a son of Shabaka.

That Shabatoka was a son of Shabaka is indicated by his name, the syllable *to* or *ato* meaning "son," and being inserted in its grammatical place before the article *ka* or *ki*. HE III 286

It is immaterial whether Petrie is correct in his assessment of the names. The fact that two of the three known kings of the family dynasty preferred the name suggests strongly that it was commonplace within the family. It may even have become a title. If a patronym it might have been passed down for centuries. In fact there does exist a memory, preserved by the Jewish scribes, attesting that very fact. In the table of nations, preserved in the 10th chapter of Genesis (Bereshith) in the Hebrew Bible, the ancestry of the Cushites is traced to Cush, a son of Ham, one of the three sons of Noah.

The sons of Ham: Cush, Mizraim, Put and Canaan. The sons of Cush: Seba, Havilah, Sabtah, Raamah and Sabteca. Genesis 10:6,7

Once again the prevalence of the name *Sab* is apparent. In this case three of the five named sons of Cush - Seba, Sabtah and Sabteca - appear to derive their names from that same linguistic stem. And the name *Sabteca* is arguably the same as that of the king of Melukkha in the days of Sargon of Assyria and that of the Cushite king of Egypt who preceded Taharka in the days of Nebuchadrezzar, son of Nabopolassar, king of Babylon. *It was not an uncommon name.*

At best the critic can argue that temporal considerations favor the identification with Shabataka of the 25th dynasty. Better to equate the Shabataku mentioned in the Tang-i Var inscription with a king who lived at the time the inscription was recorded than to identify him as an ancestor of a king who lived a century later. Or so the reasoning might go, all other things being equal. But as we have already seen, a host of problems surround the identification of the Tang-i Var king of Melukkha with the 25th dynasty king of Egypt. Those problems are sufficiently serious to rule out the identification entirely. Viewed in this light, not only does the Tang-i Var text fail to discredit the revised history, rather, it constitutes an argument in its favor.

The identification of Shabataku, king of Melukkha as an ancestor of Taharka accords well with the revised history of the late 8th century. We have previously argued that the powerful 22nd dynasty king Osorkon II died in the year 712 B.C. and that toward the end of his reign and in the years immediately following, Egypt became increasingly fragmented. Sheshonk III and Pedubast I, and possibly others, contested for power in the north and a usurper, Takeloth II, emerged in the Theban area, founding the competing 23rd dynasty. We have also argued that this Takeloth/Takeroth II was the opponent of Sennacherib in 701 B.C., and was referenced as Tirhakah, "king of Cush" in 2 Kings 19:9. In the accompanying discussion it was stated, based on arguments supplied by Egyptologists, that Takeloth II was a Theban king and "arguably a king of Cush". The geographical term "Cush" in the late 8th and early 7th centuries, at least as used by Jewish and Assyrian scribes, was all but synonymous with "Upper Egypt". Melukkha must lie further south, "in [a distant country], in an inapproachable region" to use the words of the Assyrian scribe. We assume that Shabataku, king of Melukkha, ruled his kingdom as a contemporary of Takeloth II, Sheshonk III and Pedubast I. This duality of multiple kings ruling Egypt while a "king of Melukkha" ruled the remote regions of the Upper Nile is precisely the situation described elsewhere in the Assyrian inscriptions. In

the annals of Sennacherib which document his 3rd campaign (701 B.C.), in the midst of a description of his assault on the cities of Judah, it is noted that Hezekiah, the king of Jerusalem

... had become afraid and had called (for help) upon the *kings of Egypt* (Mus(u)ri) (and) the bowmen, the chariot(-corps) and the cavalry of the king of Ethiopia (Meluhha), an army beyond counting - and they (actually) had come to their assistance. In the plain of Eltekeh (Al-ta-qu-u), their battle lines were drawn up against me and they sharpened their weapons. Upon a trust (-inspiring) oracle (given) by Ashur, my lord, I fought with them and inflicted a defeat upon them. In the melee of the battle, I personally captured alive the Egyptian charioteers with the(ir) princes and (also) the charioteers of the king of Ethiopia. (ANET 288)

"Kings of Egypt" in this inscription may refer to Sheshonk III, Pedubast I and Takeloth II.⁴⁶⁴ There may be others. The king of Melukkha could be the Shabataku who returned Iamani to Sargon II ten years earlier. Or he may be a successor of this king. The idea of multiple kings of Egypt existing alongside of an independent king of Melukkha is entirely foreign to any historical situation that can be hypothesized for the late 25th dynasty, when either Shabataka or Shabaka is considered to have ruled all of Egypt, including the lands southward as far as the 4th cataract.

We repeat our earlier claim. The presence of the name Shabataku in the Tang-i Var inscription is a problem for the traditional history. It does nothing but affirm the essential reliability of the revised history.

For Frame and Redford there is no convenient solution to the problem of the Tang-i Var text. The two authors have no alternative but to identify Shabataku and Shabataka. It is not the correspondence in name and country which demands the identification. Far from it. If the Tang-i Var rock inscription were ascribed to any Assyrian king earlier than Sargon, or even if were dated at the beginning of Sargon's reign, then the equation Shabataku = Shabataka would not have been given a moment's consideration. The two scholars would have arrived at the same conclusions reached by the present author, that Shabataku was at best an ancestor of the 25th dynasty kings. It was the correspondence in date alone that forced the hand of the interpreters. The reign of Sargon (722-705 B.C.) during which the Iamani incident took place overlaps the chronological placement of the 25th dynasty kings Shabaka and Shabataka in the traditional history, leaving no alternative but to identify Shabataku with Shabataka. There is no room for another king of like name occupying the same time frame. Shabataku cannot be an ancestor of Shabataka if the traditional history is correct. He must be the 25th dynasty king himself.

⁴⁶⁴ We have previously discussed the problems interpreting the geographical terms Cush and Melukkha as used by biblical and Assyrian scribes. Earlier we proposed an alternative explanation of the Assyrian references in this problematic text (see above pages 77-78 and note 83)

But the very same temporal considerations which demand the identification combine to damn it. The only alternative is to question the traditional history which creates the problem.

In view of what has been argued we should conclude the argument here. The Tang-i Var inscription does not support the traditional history and it does not condemn the dates for the 25th dynasty proposed in *Nebuchadnezzar & the Egyptian Exile*.

When the substance of this book was first published in html format on-the internet we omitted any reference to the Tang-i Var inscription from the argument in the book itself, relegating discussion, as here, to a sort of appendix. Its inclusion would have necessitated extensive discussion which would have detracted from the flow of the argument, and the presence of the name was not a problem which needed to be defended, a fact we have just argued. That would have been sufficient reason to ignore it. But in fact that was not the actual justification for its exclusion. At the time there existed in my mind a more fundamental reason. When I first read Frame's article in the fall of 1999 I could not read the name Shabataka in the text. What Frame read as ša-pa-ta-ku-[u] I read as ša pa-at-tu-[u] and have subsequently come to read as ša pa-at tu-[?]. The concluding section of this article is dedicated to a discussion of the proposed emendation.

Shabataku or "ša pa-at tu-?"

There are several reasons for believing that the four cuneiform characters read ša-pa-ta-ka- on line 20 in the Tang-i Var inscription should be read ša pa-at tu- . There are also many reasons for agreeing with Frame. Since I would prefer to see the name than to argue for its absence I will present both sides of the argument.

The presence of the name of Shabataku in the Tang-i Var inscription is unexpected. As we have noted several times already, two other inscriptions describe the same incident and neither one supplies the name of the Melukkhan king. This omission is particularly surprising for two reasons. Both these alternative texts are located in the palace annals of Sargon, lining the walls of palace rooms, where they would be constantly viewed by the Assyrian king and where he would most likely supply the name of the king whose act of contrition is celebrated therein. And one of these texts, the Display Inscription, is much more detailed in its description of events related to the Iamani incident than is the Tang-i Var text. We wonder why the name of Shabataku is supplied only in a rock inscription in a remote mountain pass near Tang-i Var, 40 m. above ground, inaccessible to all but mountain goats and venturesome archaeologists with sturdy ropes.

And a secondary question arises when we reflect on the three decades which passed between the time when the rock inscription was discovered and the time when Grant Frame first read the name Shabataku in the inscription. The discoverers not only viewed

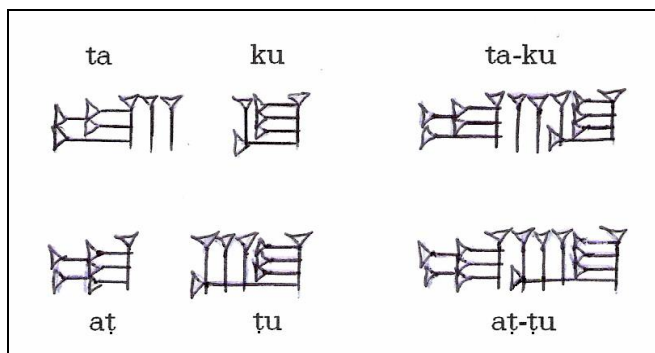
the text from inches away, they made casts of the cuneiform impressions. Dr. Vallat, who took the photographs on which Frame relied, also viewed the inscription from close up. Why did these early observers fail to read the name of Shabataku in the text? Frame argues that the discoverers were not cuneiform specialists. But what of the cast impressions. Were they not examined by specialists? We are not informed as to the qualifications of Dr. Vallat, but we must assume that both he and others who viewed the slides he took of the inscription, failed to notice the name of Shabataka. If R. Labat requested the slides, did he not examine them? Labat was one of the leading Assyriologists of his time. And it does not take a cuneiform specialist to recognize a proper name in a cuneiform text. The name is typically preceded by a single vertical stroke, a name determinant or "personenkeil". Frame sees this mark at the beginning of line 20 and indicates the fact by the transliteration (^m). Why was this not visible to the early observers?

A final argument derives from a comparison of the Display Inscription and the Tang-i Var inscription in their respective accounts of the Yamani incident. We recall from Redford's 1985 article that ...

Unable to find a safe haven in Egypt, Yamani passed clean through the land *ana ite Musri sa pat Meluhha*, "to the frontier of Egypt which is (contiguous) to the territory of Kush." At this point he fell into the hands of the king of Kush who, at an unspecified later date, extradited him to Assyria.

The phrase "*ša pat Meluhha*" can best be translated "which borders (on) Melukkha" [though note that Kitchen and many other translate "which belongs to"] and the phrase "*ana ite*" means literally "to the extremity (of)" Both *pat* and *ite* are construct nouns. It is curious, to say the least, that my initial reading of line 20 of the Tang-i Var inscription saw the identical three cuneiform signs "sa pa-at" followed closely by the place name "KUR Melukkha" ("land of Melukkha") precisely as they appear on the parallel text of the Display Inscription. Frame reads the third sign as "ta" rather than "at". The fourth sign, which Frame reads as "ka" appears to be a "tu". In fact, it is not simply the case that Frame reads "ta-ku" where I read "at-tu" but rather that we see slightly different impressions and that we place the division between the constituent elements of the respective signs differently, i.e. that the combination "ta-ku" taken as a whole is visibly very similar to the combination "at-tu" viewed collectively. (note that the t's in both "at" and "tu" are emphatic) In line 20 of Frame's transliteration we note that the signs immediately following the combination *ša-pa-ta-ku-*, including the word "king" are obscured in the text but supplied in the translation. They may not be present.

Figure 38: Similarity of the Combinations "ta-ku" and "at(emphatic)-t(emphatic)u"



The only change that has taken place since my initial reading is the introduction of a word division between the 3rd and 4th signs. I now read ša pa-at tu-? rather than ša pa-at-tu-? If tu- is the beginning of the construct tu-ur then the emended text reads "which borders on the extremity of the land of Melukkha" This is remarkable close to the *ana ite Musri sa pat Meluhha* of the Display Inscription, and argues for the reasonableness of the emendation.

There are two arguments against the proposed emendation, both of considerable weight, and both highly technical. The first and most obvious is the fact that the emendation of the first half of line 20 requires the emendation of portions of the end of line 19. The problem is difficult to evaluate. The shadows at the end of line 19 constitute one difficulty. The subjectivity involved in reading the many partially obscured signs is another. The problem is compounded by the fact that Dr. Frame, when transcribing the Tang-i Var inscription, was cognizant of the parallel texts and probably used them to at least guide his deliberations.

The second argument is related to the first. I have used the parallel text of the Display Inscriptions to guide my reading of the beginning of line 20. How subjective was my reading of the beginning of line 20? In the spring of the year 2000, I sought a second opinion from a respected Assyriologist. He concurred with Frame. The gist of his response goes as follows (understand that he was responding to my first suggestion, i.e. that the reading should be ša pa-at-tu-) :

I have gotten to look at the ORIENTALIA article. Here are my results. The signs in question are the fourth and fifth. There is a PERSONENKEIL at the beginning. The signs appear to be as Frame read them-ta and ku. They are clearest to me on Plates 7 and 11. I can also make them out on Plates 4, 5, 6,

and 12. You are correct that the signs are very similar, uncannily so in this order, but they still appear to be clearly as Frame read them. The lower horizontal of the Ta extends to the first vertical. There is a space between the third vertical of the TA and the vertical of the KU. The lower horizontal of the KU begins before the first vertical of that sign and not before two verticals back, as it would in T(emph)U. PA:T(emphatic)U is a noun. There is no verb from this root.(personal correspondence)⁴⁶⁵

I have the utmost regard for the opinion of my colleague. I place a question mark over the entire discussion only because it involves questions of eyesight, rather than technical knowledge, and because both he and I (and Grant Frame) are dependent for our conclusions on photographs of extremely poor quality. If my friend is correct then the question is settled. The name Shabataka remains in the text and we revert to the argument presented at the beginning of this discussion. The presence of the name argues for the legitimacy of the revised history. I have no desire to remove it.

Redford concluded his brief note in *Orientalia* appropriately:

The above is suggested with a diffidence born of the knowledge of how spotty is our historical record of the period in question. A number of other solutions are conceivable, and undoubtedly will be trotted out in due time. (p. 60)

We echo these sentiments and await developments.

⁴⁶⁵ Personal correspondence from Dr. Walter Bodine, Near Eastern Languages, Yale University.