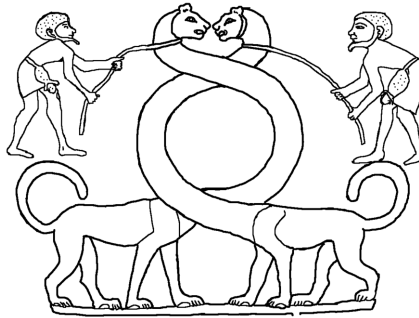


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# THE LEVANTINE WAR-RECORDS OF RAMESSES III: CHANGING ATTITUDES, PAST, PRESENT AND FUTURE\*

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## **Summary: The Levantine War-Records of Ramesses III: Changing Attitudes, Past, Present and Future**

This paper begins with a historiographic survey of the treatment of Ramesses III's claimed war campaigns in the Levant. Inevitably this involves questions regarding the so-called "Sea Peoples."<sup>1</sup> There have been extraordinary fluctuations in attitudes towards Ramesses III's war records over the last century or more—briefly reviewed and assessed here. His lists of Levantine toponyms also pose considerable problems of interpretation. A more systematic approach to their analysis is offered, concentrating on the "Great Asiatic List" from the Medinet Habu temple and its parallels with a list from Ramesses II. A middle way between "minimalist" and "maximalist" views of the extent of Ramesses III's campaigns is explored. This results in some new identifications which throw light not only on the geography of Ramesses III's campaigns but also his date.

**Keywords:** Ancient Egypt – Canaan – Late Bronze Age – War Records – Toponymy

**Resumen: Los registros de la guerra levantina de Ramsés III: Actitudes cambiantes, pasado, presente y futuro**

Este artículo comienza con un recorrido historiográfico del tratamiento de las supuestas campañas bélicas de Ramsés III en el Levante. Inevitablemente esto implica pre-

\* I would like to dedicate this paper to the memory of the late David Lorton for all his help on Egyptological matters. I know he would have enjoyed sharing his knowledge towards the present paper.

<sup>1</sup> No attempt has been made here to provide anything near a comprehensive bibliography of the burgeoning number of papers on the so-called "Sea Peoples," which would require a volume itself.

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guntarse sobre los llamados “Pueblos del mar”. Ha habido cambios extraordinarios en las actitudes en torno a los registros de guerra de Ramsés III a lo largo del último siglo y antes—aquí brevemente reseñadas y reevaluadas. Sus listas de topónimos levantinos también presentan problemas de interpretación. Ofrecemos un acercamiento a su análisis, más sistemático, concentrándonos en la “Gran lista asiática” del templo de Medinet Habu y sus paralelos con la lista de Ramsés II. Exploramos una interpretación intermedia entre las visiones “minimalista” y “maximalista” de la extensión de la campaña de Ramsés III. Esto nos lleva a algunas nuevas identificaciones que iluminan no solamente la geografía de la campaña de Ramsés III sino también su datación.

**Palabras Clave:** Antiguo Egipto – Canaán – Edad del Bronce Tardío – Registros de Guerra – Toponimia

## INTRODUCTION

Just over a century ago the great orientalist Archibald Sayce described how the attitude of Egyptologists towards pharaonic claims of conquest could shift dramatically:

*At one time it was the fashion to throw doubt on the alleged conquests of Ramses II. in Western Asia. This was the natural reaction from the older belief, inherited from the Greek writers of antiquity, that Ramses II was a universal conqueror who had carried his arms into Europe, and even to the confines of the Caucasus. With the overthrow of this belief came a disbelief in his having been a conqueror at all. The disbelief was encouraged by the boastful vanity of his inscriptions, as well as by the absence in them of any details as to his later Syrian wars. But now we know that such scepticism was over-hasty. It was like the scepticism which refused to admit that Canaan has been made into an Egyptian province by Thotmes III. and which needed the testimony of the Tel el-Amarna tablets before it could be removed.<sup>2</sup>*

<sup>2</sup> Sayce 1912: 201–202.

As Sayce noted, it took the momentous discovery of the El Amarna letters to dispel doubts about an 18<sup>th</sup> dynasty Levantine empire. He had long had faith in the veracity of the New Kingdom accounts of political and military affairs in Syro-Palestine. Beginning as early as 1876, he used them—in the face of considerable scepticism—to develop the hypothesis that the Hittites once ruled an empire reaching from the Black Sea and the Aegean to northern Syria and that it was based at Boghazköy in central Anatolia. He must have been doubly gratified when the excavations began at Boghazköy in 1906, with the decipherment of cuneiform Hittite following in 1915.<sup>3</sup> Not only was the reality of the Hittite Empire confirmed, but the archives from Boghazköy added flesh to the bones of Ramesses II's claims to have struggled with the Hittites for control of northern Syria.

Rameses III also claimed to have campaigned in the Levant (as we know mainly from his Medinet Habu inscriptions). Yet there were evidently sceptics in Sayce's day, to whom he replied that we should bear in mind the cases of Thutmose III and Ramesses II: "We have no reason to doubt that the campaigns of Ramesses III. in Asia were equally historical."<sup>4</sup> Admittedly the evidence for Ramesses III's claimed Asiatic campaigns is of a different character to that of Thutmose III or Ramesses II. None of the written sources from the reign of Ramesses III is anywhere near as detailed as we might wish. There are, for example, no "annals" such as those we have from the time of Thutmose III;<sup>5</sup> nor anything resembling the plentiful records (from reliefs to cuneiform records) of Ramesses II from which we can reconstruct his wars and subsequent diplomacy with the Hittites. As Breasted put it:

*Had these [Ramesses III's] wars been reported in the sober and intelligible style of Thutmose III's Annals, we should have known much of them which it is now safe to say we shall never know. It is difficult to describe the character of these Medinet Habu inscriptions. Perhaps, under*

<sup>3</sup> For a brief account (with references) of Sayce's role in the discovery of the Hittites, see James *et al.* 1991a: 113–119, 362.

<sup>4</sup> Sayce 1912: 202.

<sup>5</sup> See Redford 1986 for pharaonic "annals" and other forms of ancient Egyptian "history writing."

*the influence of the Kadesh poem [of Ramesses II], it has now become impossible to narrate a war or victory of the Pharaoh in any other than poetic style. The record must be a poem.*<sup>6</sup>

Breasted's observation still stands—towards explaining the difference in character between Ramesses III's war records and those of his New Kingdom predecessors. The evidence for Ramesses III's campaigns is more equivocal and hence susceptible to more interpretations than those allowed, for instance, by the "annalistic" records of Thutmose III.

### SOURCES FOR RAMESSES III'S ASIATIC CAMPAIGNS

The evidence for Ramesses III's Asiatic campaigns can be divided into three groups:

(A) The elaborate series of reliefs from his mortuary temple at Medinet Habu which include battles with the so-called "Sea Peoples," *i.e.* the Philistines (*Peleset*) and their allies. The most relevant scenes for the present matter are on the northern wall and their arrangement is generally accepted to progress from right to left in terms of date order, with additional information on reliefs in the interior of the temple.

Hence, starting from the right, Plates 17–18 and 22 deal with his first Libyan war.<sup>7</sup> More detail is carried on scenes from inside the second courtyard in the temple, beginning with Plates 27–28, which date this campaign to Year 5. They also include two elements which are not necessarily connected to the first Libyan campaign. First something which Edgerton and Wilson described as a "generalised reference to a defeat of Amor."<sup>8</sup> Kitchen translates this as "The Chief ('he of') of

<sup>6</sup> *ARE* IV: 12–13. On the "shift" in style here see the brilliant essay by Spalinger (2017).

<sup>7</sup> Unfortunately, the numbering of Kitchen (2008) is different as a system for the texts, while Drews (2000) introduces his own system of Panel numbers. For the sake of clarity Plate numbers given here are those of the appropriate volumes of the Epigraphic Survey and Edgerton and Wilson 1936.

<sup>8</sup> Edgerton and Wilson 1936: 20.

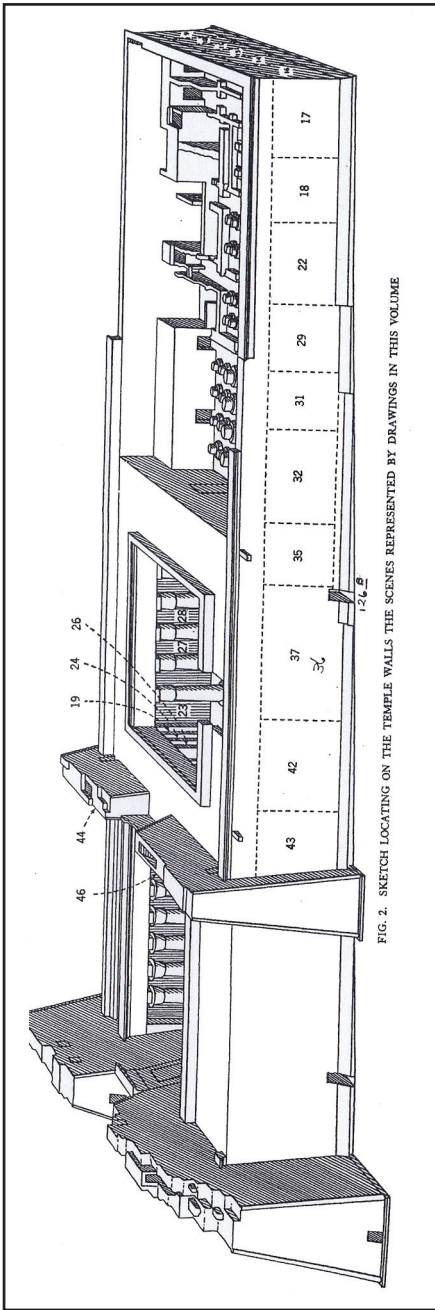


FIG. 2. SKETCH LOCATING ON THE TEMPLE WALLS THE SCENES REPRESENTED BY DRAWINGS IN THIS VOLUME

**Fig. 1.** Sketch showing the position of Ramesses III's battle reliefs at Medinet Habu. From *MH* I, 12, Figure 2. © University of Chicago Press 1930. The caption is from the original with some handwritten annotations, perhaps by an archivist at the Chicago Oriental Institute. 126 B refers to a Plate of that number in *MH* II, a photograph of an entrance in the northern wall.



Amurru is (but) ashes,<sup>9</sup> his seed is no (more), all his people are taken captive, scattered, and brought low. Every survivor from his land come with praise, to behold the great Sun of Egypt over them..." There are no details of any campaign or actual fighting, though later in the same text we are told that the "northern countries shivered in their bodies," namely the *Peleset* and *Tjekker*, who attacked Egyptian territory—some came on land while others attempted to enter the Nile river mouths.<sup>10</sup> The wording here can easily be read as a digest of the more famous "Sea Peoples" war in Year 8,<sup>11</sup> but of course it may well have been a precursor to the major conflict.<sup>12</sup>

Returning to the series of reliefs on the exterior north wall, Plate 29 shows the Pharaoh issuing arms to his troops,<sup>13</sup> evidently against the *Peleset* who are described as "in suspense, hidden in their towns."<sup>14</sup> On Plate 31 he is shown marching to Djahi (the traditional Egyptian term for Palestine-Phoenicia) "to trample down every foreign country that has infringed his frontier..." The enemies are not named but clearly the *Peleset* and their allies seem clear from context. The next Plates (32–34) show him in the famous land battle with the so-called "Sea Peoples." The caption is fragmentary and names no particular group, but the *Peleset* type known from other reliefs are depicted as the victims of Pharaoh's onslaught: "Ramses III in his chariot charges into the thoroughly disorganised Sea Peoples. He is supported by Egyptian infantry and foreign auxiliaries. The Sea Peoples flee on foot and in their chariots, while their women, children and baggage move away in heavy ox carts."<sup>15</sup> The next Plate (35) depicts the Pharaoh, showing off

<sup>9</sup> Kitchen 2008: 19. See Goedicke 2001 for the expression "made into ashes" with particular respect to "Asiatic" foes.

<sup>10</sup> Edgerton and Wilson 1936: 30; Kitchen 2008: 22.

<sup>11</sup> Edgerton and Wilson 1936: 19: "This great inscription of 75 lines, written retrograde, bears the date 'year 5'. Yet an analysis of the contents makes it apparent that it contains also a record of events we usually date 'year 8.'"

<sup>12</sup> There is no reason why the Libyans could not have attacked Egypt together as a co-ordinated plan with the *Peleset* and *Tjekker*.

<sup>13</sup> Edgerton and Wilson 1936: 35–36.

<sup>14</sup> Kitchen 2008: 24.

<sup>15</sup> Edgerton and Wilson 1936: 38.

his muscles by shooting lions. Next come the scenes (Plates 37–39) showing the graphic sea battle between Ramesses III and “Sea Peoples” in ships, who are depicted as both *Peleset* and *Sherden* types. Plate 42 shows officials bringing captives from this battle to Pharaoh. Plate 43 follows on, with captives (*Tjekker*) from the wars being presented to the Theban Triad. In Plate 44 captives, identified as *Peleset*, *Denyen* and *Shekelesh* are being dedicated to the goddess Mut.<sup>16</sup>

The detailed account of the events of Year 8 comes from the front of the Second Pylon, inside the Temple (Plate 46). Translations of this controversial record are too commonplace to repeat here: but the familiar points are the “foreign countries” making a “conspiracy in their isles,” their defeat of the Hittites and related states, a devastation somewhere in Amor (Syria), and how a coalition of *Peleset*, *Tjekker*, *Shekelesh*, *Denyen* and *Weshesh* were moving towards Egypt by land and sea and were defeated by the Pharaoh.

Just how far north Ramesses III marched into the Levant in this campaign has always been a moot point. The Year 8 inscription states that he organised his frontier defences against the invading “Sea Peoples” in Djahi.<sup>17</sup> As the territory of Djahi seems to have started at the very border of Egypt, opinions have always varied as to where the famous land and sea-battles took place.<sup>18</sup> The general consensus seems to remain that these battles took place not far from Egypt: in the case of the land-battle somewhere in Palestine and the sea-battle in the very mouths of the Nile. For convenience I have made a separate category (below) of the war scenes which are universally agreed to concern locations much further to the north.

<sup>16</sup> Edgerton and Wilson 1936: 47–48.

<sup>17</sup> Plate 46; Edgerton and Wilson 1936: 54; Kitchen 2008: 24.

<sup>18</sup> Breasted (*ARE* IV: 40), along with Sayce, assumed that Ramesses III had marched into “Syria.” See also Cook (1931: 320–321) who understood that Ramesses III had taken his forces to Phoenicia (Djahi) to repel the so-called “Sea Peoples,” then (as his control of Syria weakened) to make “the last effort of Egypt against the Hittites.” For recent views see Kahn and Ben-Dor Evian references below.

(B) Five towns in all are depicted. Two (1–2) shown with Hittite-looking defenders (Plate 87)<sup>19</sup> the name of one is lost, the other is labelled “the town of *irt*,” often read as “Arzawa” the name of a Hittite vassal state in western Asia Minor (see further below). (3) Storming of a fortress “Tunip of Hatti” with defenders in Syrian costume (Plate 88)<sup>20</sup>—Tunip being a well-known Syrian city from New Kingdom records; (4) Another city (nameless) defended by Syrians (Plate 90);<sup>21</sup> (5) the “town of Amor” (Plate 94)<sup>22</sup> *i.e.* Amurru, once the main polity of Late Bronze Age Syria. Though these were bundled together by Breasted<sup>23</sup> there are clearly two campaigns here, depicted on two different walls, the Syrian one before the Hittite (to continue the left to right progression).

(C) Ramesses III also left a number of toponym lists which include Asiatic place-names. There are two long lists from the Great Temple at Medinet Habu, XXVII and XXVIII.<sup>24</sup> The first, with some 125 names, is often referred to as “the Great Asiatic List;” the second is of equal length but has fewer certain Asiatic names and is in much poorer condition—making it extremely hard to interpret. Five shorter lists with some Asiatic names also survive.<sup>25</sup> All these lists still lack rigorous modern analysis—with the partial exception of Kahn and Redford’s recent work (see below). Most earlier studies are coloured by

<sup>19</sup> Edgerton and Wilson 1936: 94–95; Kitchen 2008: 61.

<sup>20</sup> Edgerton and Wilson 1936: 95–96.

<sup>21</sup> Edgerton and Wilson 1936: 96–97.

<sup>22</sup> Edgerton and Wilson 1936: 100–101.

<sup>23</sup> Breasted, *ARE* IV: 68–77.

<sup>24</sup> Simons 1937: 164–173.

<sup>25</sup> Simons 1937: 84, 174, XXIX, 13 names accompanying the “Blessing of Ptah,” Great Temple at Medinet Habu (*MH* II, Pls. 104–105; Edgerton and Wilson 1936: 120). Simons 1937: 85–86, 175, XXX, 14 names, Great Temple at Medinet Habu (*MH* I, Pl. 43; Edgerton and Wilson 1936: 46). Simons 1937: 176, XXXI, 7 names, Medinet Habu Pavilion; Simons 1937: 86–88, 176, XXXII, Karnak, small Amun temple, fragment, only two Asiatic names readable; Simons 1937, 88, 176, XXXIII: Karnak, small Amun temple of Ramesses III, 12 names, very badly damaged. Of these only the first three are well preserved enough to be susceptible of interpretation. They all fall into the Kitchen’s category 3 of Egyptian toponym lists, which he defines as follows (Kitchen 2009: 130): “*Lesser Lists*. (a) Abregés of longer listings; (b) ‘heraldic,’ often limited to traditional names (*e.g.* Nine Bows) or to major entities beyond Egypt.”

the assumption that Ramesses III slavishly copied *all* his lists from those of earlier pharaohs. The same cliché used to be trotted out for the toponym list of Shoshenq I of the 22<sup>nd</sup> Dynasty, rightly and forcefully discredited by Kitchen:<sup>26</sup> it has proved to be one of the most *original* of all such lists.

### “MINIMALIST” VIEWS OF RAMESSES III’S CLAIMS

Returning to trends in attitudes towards Ramesses III’s campaigns, in 1906 Breasted was prepared to see both land and sea battles with the “Sea Peoples” as having taken place near the coast south of Arvad (northern Phoenicia).<sup>27</sup> British Egyptologist Henry Hall was far more cautious, placing the land and sea battles with the Sea Peoples close to the frontier of Egypt itself; he did allow, however, that Ramesses III later marched to Amurru to restore Egyptian authority there, although not as far as the Euphrates.<sup>28</sup> In Hall’s understanding the place names from the Euphrates region in Ramesses III’s toponym lists (such as Carchemish) were “due probably to a very bad habit begun in his reign, that of copying the names of cities captured in the wars of Thothmes III...”

Attitudes against the reality of Ramesses III’s claimed campaigns continued to harden in the mid-to-late 20<sup>th</sup> century. By then it was becoming the received wisdom that Ramesses III did not campaign as far as northern Phoenicia. This view was symptomatic of a more general one regarding the originality of his war records, which casually dismissed them often *in toto* as copies from the records of the “great” Ramesses. Of the Medinet Habu war records Faulkner wrote that “the inscriptions contain but a halfpenny-worth of historical fact to an intolerable deal of adulation of the pharaoh ...”<sup>29</sup>

<sup>26</sup> Kitchen 1986: 432, n. 49.

<sup>27</sup> Breasted *ARE* IV: 34.

<sup>28</sup> Hall 1927: 382–383.

<sup>29</sup> Faulkner 1975: 241.

Regarding the Nubian battle scenes, the magisterial Alan Gardiner felt that they “seem likely to be mere convention borrowed from earlier representations.”<sup>30</sup> Likewise Faulkner: “...the scenes of a Nubian war at Medinet Habu are surely only conventional with no historical reality behind them.”<sup>31</sup>

Gardiner dismissed a Syrian campaign entirely.<sup>32</sup> Faulkner was only slightly more generous: “...the scenes in question are anachronisms copied from a building of Ramesses II. Yet there may be a substratum of historical fact beneath them...”<sup>33</sup> Surprisingly, after his generally scathing remarks, Faulkner allowed that Ramesses III “may have attempted to follow up his success [defeating the “Peoples of the Sea”] by “pushing on into Syria to drive the enemy farther away from Egypt...”

George Hughes stressed “the fact that Ramses III patterned his mortuary temple after that of Ramses II, but on a smaller scale.”<sup>34</sup> Nims listed the many comparisons he observed between the two buildings, from the general arrangement to specific details of iconography and text:

*The evidence of the copying of the Ramesseum reliefs by the scribes who planned the reliefs in Medinet Habu shows that a large number of the ritual scenes in the latter temple had their origin in the scenes in the former and occupied the same relative positions in both temples.*<sup>35</sup>

Most of the similarities concern cult and religious scenes *per se*, though with some differences with respect to the placement of military scenes:

<sup>30</sup> Gardiner 1961: 282.

<sup>31</sup> Faulkner 1975: 244.

<sup>32</sup> Gardiner 1961: 288.

<sup>33</sup> Faulkner 1975: 243–244.

<sup>34</sup> Epigraphic Survey 1963: x.

<sup>35</sup> Nims 1976: 175.

*Ramses III used the rear face of the first pylon of Medinet Habu for accounts of his military exploits, just as Ramses II used the equivalent space at the Ramesseum for his. The long account of Year 8 of Ramses III was carved on the front face of the north tower of the second pylon at Medinet Habu; the parallel wall at the Ramesseum seems to have been occupied by the famous battle poem of Ramses II. The rear face of this pylon at the Ramesseum, on the other hand, shows battle reliefs below scenes of the Min Feast, as does the lower register of the east wall of the first hypostyle hall south of the axial doorway, while in Medinet Habu the corresponding walls have religious scenes.<sup>36</sup>*

Building on the observations of Nims, Lesko took the extreme position that *all* of Ramesses III's war records at his mortuary temple of Medinet Habu and elsewhere, were copied from the work of predecessors—with the exception of his second Libyan campaign, dated to Year 11.<sup>37</sup> In Lesko's view, even the famous records of the "Sea Peoples" battles were borrowed from the nearby (and now-destroyed) mortuary temple of Merenptah.

A major factor in the dismissal of Ramesses III's northern campaigns has been the assumption that the Medinet Habu reliefs show his troops storming two Hittite towns (see above). Indeed, the inhabitants of the two towns look Hittite in appearance. One is labelled "Tunip," while the name of the second has been frequently read as "Arzawa." As the location of the Hittite vassal kingdom of Arzawa in western Anatolia (on the Aegean seaboard) is certain,<sup>38</sup> the idea that Ramesses III would have been able to campaign this far, Sesostri-like, strikes as absurd.

<sup>36</sup> Nims 1976: 171.

<sup>37</sup> Lesko 1980: 1992.

<sup>38</sup> See e.g. Hawkins 1998.

Gardiner flatly stated that:

*All these pictures are clearly anachronisms and must have been copied from originals of the reign of Ramessēs II: there is ample evidence that the designers of Medinet Habu borrowed greatly from the neighbouring Ramesseum. Confirmation is given in the papyrus [Harris I] cited above; this has no mention of a Syrian campaign, still less of one against the Hittites. All that is said is that Ramessēs III “destroyed the Seirites in the tribes of the Shōsu”; the Shōsu have already been mentioned as the Beduins of the desert bordering the south of Palestine, and ‘the mountain of Se’ir’ named on an obelisk of Ramessēs II is the Edomite mountain referred to in several passages of the Old Testament. It looks as though the defeat of these relatively unimportant tent-dwellers was the utmost which Ramessēs III could achieve after his struggle with the Mediterranean hordes...<sup>39</sup>*

With these words, a nadir was reached in the assessment of Ramesses III’s military activities. It still prevailed forty years later when Kenneth Kitchen wrote:

*There is no evidence that he invaded Palestine in Year 12 (a rhetorical text of that date itself proves nothing). The Medinet Habu Syrian war-reliefs are most likely merely copies from those of Ramesses II, as they include entities no longer extant for Ramesses III to battle against. Ramesses III attacked not Israel, but Edom in south Transjordan, as the factual descriptions in Papyrus Harris I make clear.<sup>40</sup>*

<sup>39</sup> Gardiner 1961: 288.

<sup>40</sup> Kitchen 1991: 238.

By the “entities” which Kitchen described as “no longer extant for Ramesses III to battle against,” he meant various Anatolian states such as Hatti and Arzawa which were allegedly swept away by the “Sea Peoples” invasion of Ramesses III’s Year 8.<sup>41</sup> Otherwise it is clear that in 1991 Kitchen, like Gardiner, was arguing that Ramesses III did not campaign any further than the Sinai/Negev area—as no campaigns further north are mentioned in the “factual descriptions” from Papyrus Harris. More recently Strobel went even beyond Gardiner, Kitchen, Lesko and others, writing what can only be described as a tirade against Ramesses III. For reasons of space only a few quotes follow:

*Ramses III started his triumphal report on the walls of the temple in Medinet Habu, which was finished in his year 12, with his “Nubian War.” However, this war never happened. The same is true for the “Asiatic or Syrian War”, the last of the reported military deeds. Ramses’ ideological invention of these wars should bring his deeds on the same level as the triumphs of Ramses II and Merenptah, especially Merenptah’s Asiatic war. The texts and reliefs of Ramses III are no “war journal” or realistic picture of his military campaigns, but a triumphal self-representation on a highly ideological degree. The texts are first of all rhetorical and formulaic; the events are presented and described in a fixed ideological scheme and language...Ramses III was a “plagiarizer and self-*

<sup>41</sup> Cf. Cifola (1991: 20) on the geographical terminology of Ramesses III’s war reliefs: “...for the Asiatic campaign, there is the mention of the besieged fortresses; but these geographical references cannot be trusted since they would presume an improbable Egyptian advance towards northern Syria, as far as the border of Ḫatti; but, by this time, Ḫatti politically has come to an end.” The last statement cannot be true as we have no idea when such an event happened relative to Egyptian chronology. Kahn (2010: 17): “...it is impossible to precisely date the fall of the Hittite Empire or to assess its role, if any, in the Levant during the reign of Ramesses III.” There has also been far too much concentration on the collapse of the central polity at Boghazköy. There continued to be “Great Kings of Ḫatti” in the cadet kingdoms of Carchemish and Tarhuntassa. The literature on this is voluminous and patchy in quality. Some of the best studies remain those of Itamar Singer (2000), with other papers, particularly on Tarhuntassa in his collected essays (2011) as well as numerous papers by David Hawkins (for references see conveniently Hawkins 2009).



*aggrandizer of the first order.” He ordered direct copies from the records and illustrations of the Ramesseum and without doubt, from the today destroyed funerary temple of Merenptah in his direct neighbourhood. He even took a quite important amount of blocks, recuts and not recuts, by quarrying other temples, especially those of his predecessors.<sup>42</sup>*

Were we to take all the negative opinions together, Ramesses III’s military efforts would have been confined to repelling Libyan invaders in his year 11 and a minor raid against “bedouin” in the Sinai area.

Such a picture seems unrealistic, to say the least. Ramesses III’s records talk of tribute from northern lands, the supply of his temples by goods and tribute from foreign lands (notably Djahi and Kharu), and the revenues drawn from temples maintained in the empire, including the construction of a new one in “Canaan.”<sup>43</sup> Ramesses III ruled Egypt for 31 years in relative security and prosperity, with tribute drawn from Levantine domains. One wonders how this feat was achieved, in economic terms, if the Egyptian army was so idle, only fighting defensive wars and never active beyond the frontiers—with the exception of an allegedly trivial foray against the Shasu of Edom. Such a picture goes totally against the grain of what we know of New Kingdom dominion and economics. It has also long run counter to the archaeological evidence from the Levant.

## THE ARCHAEOLOGICAL EVIDENCE

In the days of Sayce (see above) the Amarna and Boghazköy archives were the “smoking guns” proving the reality of the campaigns of Thutmose III and Ramesses II. Was there an equivalent for Ramesses

<sup>42</sup> Strobel 2011: 187–188. Strobel (2011: 245, n. 136) accepts all of Lesko’s negative stances with one exception, disagreeing with his assumption “that there was no first Libyan campaigns at all.”

<sup>43</sup> See Grandet 1983.

III? No, but what Ramesses III lacked in terms of new literary documents was amply recompensed in terms of archaeological finds—from small finds such as numerous scarabs,<sup>44</sup> a statue fragment from Byblos,<sup>45</sup> the “pen-case” of an officer at Megiddo<sup>46</sup> to the plethora of discoveries at Beth-Shean, beginning in 1923 with a seated life-size statue outside the “northern temple” to inscriptions from its doorways and jambs, and the “pen-case” of another local official.<sup>47</sup> Most of these finds had been made by the mid-20<sup>th</sup> century, such as the Megiddo pen-case in 1937. Taken together they should have had an impact on views about the reality of his Levantine expeditions further north than the Sinai region (where inscriptions are known from the mining centre of Timna, etc.).

So how did Egyptology react to such finds?

An interesting dichotomy arose.<sup>48</sup> While many Egyptologists have been reluctant to allow Ramesses III any military action in western Asia north of Sinai, archaeologists were identifying a phase at the transition from the Bronze to Iron Age in Palestine as a period of “Egyptian empire”—largely under the early 20<sup>th</sup> Dynasty. Evidence for this comes most clearly from southern sites like Tell esh Shari‘a, Tell el Far‘ah (south), Gaza and Deir el-Balah and, to the north, Megiddo and Beth-Shean in the Jezreel Valley.<sup>49</sup> At the latter, pottery and other evidence suggest an *increased* Egyptian presence during the early 20<sup>th</sup> Dynasty.<sup>50</sup> It is clear that his successor Ramesses IV maintained a presence at Beth-Shean,<sup>51</sup> though it seems that he was the last pharaoh to hold sway so far north.<sup>52</sup>

<sup>44</sup> See conveniently Higginbotham 2000: 251–252.

<sup>45</sup> Weinstein 1992: 142; Kitchen 2008: 215.

<sup>46</sup> Loud 1939: 1; Kitchen 2008: 214–215; Feldman 2009.

<sup>47</sup> See conveniently Mazar 1993: 217–222.

<sup>48</sup> As stressed in James 2010: 70.

<sup>49</sup> Weinstein 1981: 20–22; 2012: 164–167, 168–169.

<sup>50</sup> Martin in Mazar 2006: 127, 152; cf. remarks in James 2010: 69–70.

<sup>51</sup> Porter 1998; 2008; Weinstein 2012: 169.

<sup>52</sup> Zwickel 2012; van der Veen and James 2015.

With respect to the reality of Ramesses III's campaigns, the arch-minimalist Lesko noted: "Archaeological evidence should help to resolve these problems."<sup>53</sup> But he restricted his comments here to an alleged destruction of Beth Shean by Ramesses III (for which there is not a shred of evidence), mentioning but failing to appreciate the significance of an inscription of his Chief Steward from that site, *i.e.* the power of Ramesses III reached as far as the Jezreel Valley. The idea that Ramesses III's campaigning in Palestine was limited to Edom overlooks the archaeological evidence. Other mid-to-late 20<sup>th</sup> century Egyptologists, such as Wilson, appreciated more fully the importance of the archaeological finds:

*Ramses III still held his Asiatic empire in Palestine. His statue has been found at Beth Shan and there is record of him at Megiddo. He built a temple for Amon in Palestine, and the gods owned nine towns in that country, as his due-paying properties. The Egyptian frontier was in Djahi, somewhere along the coast of southern Phoenicia or northern Palestine.*<sup>54</sup>

Wilson allowed Ramesses III's empire a fairly generous reach, but the implication is that he merely "held" it as an inheritance from his 19<sup>th</sup> dynasty predecessors (see below) without any active campaigning. Likewise, Kitchen states that "the Egyptians under Ramesses III maintained their overlordship over both the Canaanites and the newcomers..."<sup>55</sup>

Weinstein was more positive. Stressing the scarcity of late 19<sup>th</sup> dynasty remains from Palestine,<sup>56</sup> coincident with "Egypt's domestic problems," he clearly attributed a more active policy to Ramesses III than one of inheritance: "Ramesses III seems to have done his best to restore a measure of control in Palestine."<sup>57</sup> Likewise Redford: "...

<sup>53</sup> Lesko 1980: 86.

<sup>54</sup> Wilson 1951: 259.

<sup>55</sup> Kitchen 2003: 143.

<sup>56</sup> For a more recent review of the evidence see James 2015: 246–247.

<sup>57</sup> Weinstein 1981: 22.

Rameses III had been able, by dint of military activity, to reassert his authority over much of Palestine and perhaps parts of Syria as well...”<sup>58</sup>  
So also Morkot:

*Rameses III certainly did emulate Rameses II—but in no superficial way. Archaeology is now showing that Rameses III did, in fact, manage to renew Egyptian control over parts of western Asia...*<sup>59</sup>

These writers appreciated the obvious: the “smoking” gun for Rameses III is provided by the archaeological and inscriptional remains from both southern Palestine (e.g. Lachish) and the Jezreel Valley (notably Beth Shean and Megiddo).

#### FURTHER PROBLEMS WITH THE “MINIMALIST” ARGUMENTS

The oft-produced argument (see Gardiner and Kitchen above) that—all Rameses III’s claimed campaigns—only those mentioned in Papyrus Harris are “factual” is specious. As well as contradicting the archaeological evidence, it overlooks the character and historical context of the document. Papyrus Harris I was composed in the reign of Rameses III’s successor Rameses IV, its last four pages giving an invaluable historical retrospect (put in the mouth of Rameses III) beginning with the rise of Setnakht, founder of the 20<sup>th</sup> Dynasty.<sup>60</sup> It continues with a summary of Rameses III’s achievements, the military section being conspicuously short. There are many possible reasons for this, the most obvious being that Rameses IV may well have wanted to minimise their importance. While Rameses IV maintained control over Egypt’s Palestinian domains for a short while, the “empire” (north of the Gaza region) almost certainly collapsed during his reign.<sup>61</sup>

<sup>58</sup> Redford 1992: 290.

<sup>59</sup> Morkot 2000: 95.

<sup>60</sup> For a recent translation see Peden 1994: 215–223.

<sup>61</sup> Zwickel 2012; van der Veen and James 2015.

The Ramesses III “minimalists” also seem to have overlooked the fact that all the military achievements of Ramesses III mentioned in Papyrus Harris I concern cases where large numbers of foreign captives had been introduced into Egypt: Sherden and Weshesh (allegedly by “hundreds of thousands”) were settled in fortresses; Shasu from Seir, who along with their goods and livestock were brought to Egypt and presented them “to the Ennead as slaves for their estates;” Meshwesh, Libu and others were also settled in fortresses bearing the pharaoh’s name (again “hundreds of thousands”). The legacy of Ramesses III’s foreign campaigns, these captives would have been a conspicuous and growingly familiar sight long after his death—as recognised in Ramesses IV’s composition of the Papyrus.

The idea that Ramesses III’s Shasu campaign was against “relatively unimportant tent-dwellers” as Gardiner called them has taken firm root in the literature. Faulkner called it a “minor campaign...probably no more than a punitive expedition against raiding nomads.”<sup>62</sup> In his monumental study of the expeditions of Seti I, Murnane vacillated about the importance of that pharaoh’s campaign against the Shasu in his first year.<sup>63</sup> Seti claimed that the enemy were in disarray, but that does not mean they were easy meat.<sup>64</sup> His battles with them were recorded in a series of detailed reliefs which show them being defeated from the border city of Tjaru to cities generally identified as Raphia and Gaza—along the key route the “Ways of Horus” into Palestine. Despite all this, Murnane stated that the evidence “all pointed to a little war.”<sup>65</sup> So also Weinstein: “This military operation was clearly a minor affair.”<sup>66</sup> Why so?

<sup>62</sup> Faulkner 1975: 244.

<sup>63</sup> Murnane 1990.

<sup>64</sup> Spalinger (2005: 188) remarked that on Seti’s march towards Gaza: “The first enemy encountered was the ever-present Shasu, the seminomadic marauders and troublesome tribal units that operated on the fringes of civilization. They have no horses and chariots, and most assuredly posed no major threat to the Pharaoh and his army.” One wonders what good chariots would have been in sandy-desert areas, where wheeled vehicles would have been useless.

<sup>65</sup> Murnane 1990: 40.

<sup>66</sup> Weinstein 2012: 171. Most extreme, Strobel (2011:189) even puts an exclamation mark after their mention in one of Ramesses III’s documents, viz “Bedouins (!).”

Such evaluations are value-judgments from a “modern,” western perspective—that somehow tent-dwellers must be inferior, politically, culturally or militarily. When the Portuguese arrived in “Ethiopia” in the 16<sup>th</sup> century it took them some time to find the grand court of the Abyssinian monarch—it was itinerant, effectively a roving city composed of tents.<sup>67</sup> Kitchen has made some very telling remarks here, for example with reference to the dynasty of tented kings from Old Babylonian Manana: his point being that ancient kingships should not be defined in terms of whether they had a stable or central capital city.<sup>68</sup> It is not the place to investigate the matter here, but one has to note a vagueness in the literature as to where exactly the core of Late Bronze Age Edom (attested in Egyptian texts as early as Merenptah)<sup>69</sup> may have lain—to the west or east of the Arabah—and whether it was merely a geographical term. This vagueness is paralleled by a scholarly tendency to ignore any value in the intriguing list of the Edomite “kings” (Genesis 36 and 1 Chronicles 1:43–54)—who were said to have ruled before there was a king in Israel. Notably they all seem to have reigned from different centres, “none of which can be identified with any confidence, regardless of the date assigned to the lists.”<sup>70</sup> Were we to take the medieval Abyssinian model, such centres need have been no more than major caravan sites near sources of water and other essential resources. Without mentioning this analogy, Kitchen makes a salient point: “The consequent scarcity of tangible physical remains in the archaeological record is, therefore, not surprising...”<sup>71</sup> Current

<sup>67</sup> John Bimson has drawn my attention to two relevant passages in the Old Testament. In Numbers 13:19 Moses sends out his spies to Canaan to find out “whether the cities they dwell in are camps or strongholds.” This might be indirectly relevant in that it shows that, for the biblical writers at least, a “camp” (the same word is used for Jacob’s nomadic camp in Gen 32:21 and later for the Israelites on the move in the wilderness) could be considered a “city.”

<sup>68</sup> Kitchen 2003: 473–474. One of Kitchen’s most scathing comments (p. 474) is too irresistible to avoid: “...let us have no more daft theory that kingdoms need local equivalents of Buckingham Palace or the White House before snooty (and irrelevant) anthropologists will deign to recognise them!”

<sup>69</sup> See Kitchen 1992: 27.

<sup>70</sup> Edelman 1995: 5.

<sup>71</sup> Kitchen 1992: 27.

research on the origins of an Edomite monarchy tends to concentrate on Late Iron Age “state formation” east of the Arabah, notably in the region of Bozrah,<sup>72</sup> possibly to the detriment of wider possibilities both geographic and chronological.

As a further point, Ramesses’ campaign against the Shasu would surely have been a key factor in a wider ranging and comprehensive policy—to expand Egypt’s influence and commercial interests to the east and southeast. His achievements, as proclaimed in Papyrus Harris I included: an expedition to the land of Punt (on the African or Arabian coastline of the Red Sea) to procure incense (myrrh); a mining expedition to “Atika,” location uncertain though it is generally identified with the copper rich area of Timna in the southern Arabah, east of the Sinai peninsula;<sup>73</sup> and an expedition to the “malachite country” of Hathor, almost certainly Serabit el-Khadim in eastern Sinai, where nearby deposits of malachite<sup>74</sup> and an Egyptian temple of Hathor are known. Serabit el-Khadim has produced a dedicatory inscription of Ramesses III from his year 23.<sup>75</sup> The precise location of these sites aside, the archaeological evidence, including scarabs and a rock inscription of Ramesses III at Timna, shows that he had reclaimed the whole Sinai region for Egypt.

<sup>72</sup> See van der Steen and Bienkowski 2006 and Tebes 2016.

<sup>73</sup> Papyrus Harris I (78, 1–4) boasts that Ramesses III’s agents retrieved an enormous amount of copper from Atika, carried by ship and on land by donkeys (see conveniently *ARE IV*, 204 and for a more recent translation Peden (1994: 32 and comments n. 4). Though much of her work is now neglected due to her more extreme ideas (e.g. locating Alashiya in Egypt rather than Cyprus!), the late Alessandra Nibbi produced a number of interesting challenges which should not be forgotten. With fair logic she identified Atika with (Gebel) Attāqah/Attaka, a mountain in Egypt which lies to west of the Gulf of Suez (see e.g. Nibbi 1981: 49, 146). Schulman (1976: 130, n. 77) raised a major objection—that no copper has been found at Gebel Attāqah despite heavy mining of the area. Nibbi (1997: 13–14, nn. 9–10; 305–312) attempted to tackle this problem but failed to find conclusive evidence from geologists that Attāqah was once copper-rich. Here we are left with a classic dichotomy: between Beno Rothenberg’s suggestion (1972: 201) that Atika was Timna (rich in copper), with no philological evidence at all; and Nibbi’s location at (Gebel) Attāqah, which is perfect philologically but, so far, uncertain geologically. Given that the copper veins may have been mined out, Nibbi’s case deserves further consideration.

<sup>74</sup> Lucas and Harris 1962: 204.

<sup>75</sup> Weinstein 2012: 171.

Control over the Shasu would have facilitated Ramesses III's further ventures. Like the campaign of Seti I, one of Ramesses III's first tasks would have been to subdue them and secure the coastal route to Palestine, the "Ways of Horus." Further, his ambitions clearly went further eastwards than the Sinai area. In 2011 a spectacular discovery was made at Tayma in the desert of north-eastern Arabia: an inscription of Ramesses III.<sup>76</sup> The style of the cartouches is closely matched by three from the Sinai and Timna; Somaglino and Tallet use these to offer a map giving a route from these sites to Tayma much further eastwards (see **Fig. 2** below).

This all suggests that establishing Egyptian control over Arabian trade-routes may have been a key part of Ramesses' eastern policy; subduing the Shasu of the Seir region (and likely further east) would have been an important step in securing such access to incense trade.<sup>77</sup>

Finally, a close reading of Papyrus Harris I certainly does *not* rule out campaigns further north than Edom. The opening statement from the military section is: "I advanced the boundaries of Egypt on all sides," a claim certainly supported by the archaeological finds. Then follows the passage summarising Ramesses III's defeat of the so-called Sea Peoples:

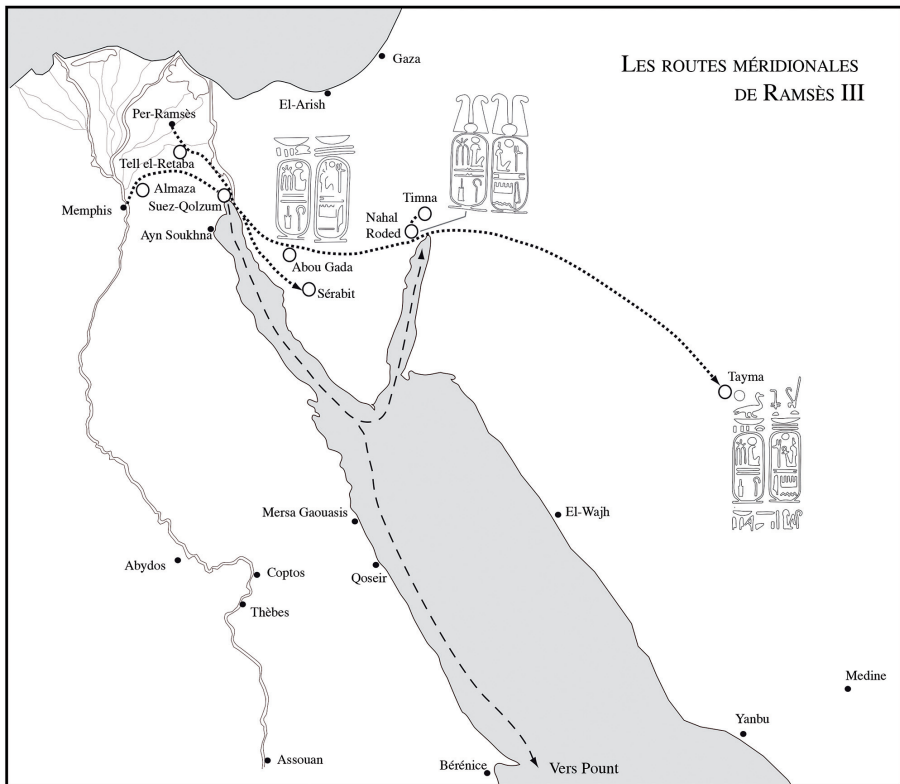
*I slew the Denyen in their islands, while the Tjeker and the Philistines were made ashes. The Sherden and the Weshesh of the Sea were made non-existent, captured all together and brought in captivity to Egypt like the sands of the shore.*<sup>78</sup>

<sup>76</sup> Somaglino and Tallet 2011: 2013.

<sup>77</sup> For a later period (Iron Age), see Tebes (2006: 45): "The archaeological evidence argues that the traffic in Arabian incense between southern Jordan and the Mediterranean was controlled by the nomads living in the Negev and Edom."

<sup>78</sup> Trans. Wilson 1969: 262.





**Fig. 2.** Map showing the distribution of identical cartouches of Ramesses III on rock inscriptions from the Sinai area to Tayma in the Arabian desert.  
 © C. Somaglino and P. Tallet 2011; reproduced by kind permission of the authors.

Whatever and wherever the “islands”<sup>79</sup> of the Denyen were (presumably off the Levantine coast, including Cyprus), they would certainly have been north of Edom! If we assume this referred to a coastal

<sup>79</sup> Redford (2000: 12) notes “The term “islands” (*iw.w*) has, by some been forced to yield a nuance of ‘coastal lands’, but this is unjustified as Egyptian has a number of words already for “maritime littoral...” This may well refer to Leahy (1988: 195), who had argued that the *iw* mentioned in an early inscription of Amasis is unlikely to have been Cyprus and that the term could apply to “high-lying areas of whatever kind.” But see here Jansen-Winkel (2014) who, working from a new collation of the Elephantine Stela, does not discern the term *iw* at all.

area of the Levant it accords with the statements from Ramesses III's other inscriptions that he reached the lands of his enemies. Thus he claimed to have attacked the Peleset in their land, "in suspense, hidden in their towns."<sup>80</sup> And in his famous Year 8 inscription, after recording the defeat of the enemy coalition, that he had brought "ruin to their cities, devastated at one time, their trees and their people having become ashes."<sup>81</sup> It has long been argued that these are not mere rhetorical claims,<sup>82</sup> a view now increasingly accepted.<sup>83</sup> The Peleset, Denyen and Tjekker are depicted identically on Egyptian reliefs, with the familiar "feathered" headgear, armour, kilts, etc. Granted that the Peleset were already settled (or settling) in Philistia, and that under the late 20<sup>th</sup> Dynasty Dor is called a city of the Tjekker (in *Wenamun*) a Levantine location or origin for the Denyen seems likely.<sup>84</sup>

Wherever they were situated, if we assume (in agreement with Kitchen) that the Papyrus Harris account is "factual," in order to attack the Denyen in their land then Ramesses III must have brought his troops much further north than Edom. The texts recording the land battle are surely the series on the Medinet Habu reliefs, culminating in the famous land battle in which Ramesses III defeats an army wearing the costume typical to the Denyen, Tjekker and Peleset. Thus, contra Gardiner, Kitchen, *et al.* a Levantine campaign *is* mentioned in the Papyrus Harris, but in the same abbreviated form used to summarise Ramesses III's other military achievements.

Lesko's theory that Ramesses III copied his war-reliefs from the *destroyed* mortuary temple of Merenptah, ranks as a classically untestable hypothesis. Where it *can* be tested, with respect to the specific names of the so-called "Sea Peoples" enemies faced by the two pharaohs in question, there are huge differences. As stressed many years ago (see **Fig. 2**), there are only three peoples in common in the lists of allegedly maritime enemies from Merenptah and Ramesses III:

<sup>80</sup> Kitchen 2008: 24.

<sup>81</sup> Kitchen 2008: 35; cf. Edgerton and Wilson 1936: Plate 46, ll. 34–35.

<sup>82</sup> Nibbi 1972: esp. 48–49, 59; James 1995; 2000; Bikai 1992; Drews 1998: esp. 58; 2000.

<sup>83</sup> See *e.g.* Kahn 2011; 2015; 2016; forthcoming.

<sup>84</sup> See Simon (2015) for a detailed discussion of the northern Syrian and Cilician possibilities.

the Sherden, Teresh and Weshesh.<sup>85</sup> There is no hint of Ramesses III's major foes, the Peleset, Tjekker and Denyen, in the records of Merenptah.<sup>86</sup> As Kitchen put it:

*The style and content of the year 5 texts of Merenptah and Ramesses III are different, and the latter's year 8 text is also different from the year 5 text of Merenptah (there is—and was—no year 8 war and text of Merenptah...The events and protagonists are different...The Sea Peoples of Merenptah were not wholly identical with those of Ramesses III. Merenptah fought no Philistines, while Ramesses III faced no Aqaywasha, and so on.*<sup>87</sup>

As noted above, the other source generally assumed for Ramesses III's copying is of course the works of Ramesses II. The plan of Medinet Habu is indeed based on the Ramesseum, though on a smaller scale. Nims listed the many similarities he observed between the two buildings, from the general arrangement to the specific details of iconography and the very texts themselves; but it should be noted that most of these similarities concern cult and religious scenes *per se*. With respect to military scenes there are differences, beginning with their placement.<sup>88</sup> With regard to composition, there is nothing remotely resembling the “Sea Peoples” reliefs of Medinet Habu on the Ramesseum or other building of Ramesses II. Both the reliefs and the accompanying texts are unique, with highlights like the stunning depic-

<sup>85</sup> The latter two are known *only* (briefly and fleetingly) from the records of these two pharaohs. See the excellent compilation of references to the “Sea Peoples” in Egyptian, Hittite and Ugaritic texts by Adams and Cohen 2013. From the records of Ramesses III, we have three references to the Weshesh and two to the Teresh. Re the Sherden, I have argued that the name simply indicates “mercenaries” (James unpublished 1995). They were ubiquitous in the Eastern Mediterranean, and long before the time of Merenptah. They are known as mercenaries at Byblos in the El Amarna period and as captive troops under Ramesses II. Their occurrence in enemy-lists of both Merenptah and Ramesses III has no significance, and certainly cannot be used to argue any “copying.”

<sup>86</sup> See Yurco 1997: 503–504.

<sup>87</sup> Kitchen 2012: 16.

<sup>88</sup> Nims 1976: 175, 171.

	Peleset	Tjekker	Denyen	Shekelesh	Weshesh	Teresh	Sherden	Ekwesh	Luka
"feathered" helmet	•	•	•						
lands, towns near Egypt	•								
later settled in Palestine	•	•							
Rameses III, Year 5	•	•							
Rameses III, Year 8	•	•	•	•	•				
enemy in land battle reliefs		Peleset-type							
enemy in sea battle reliefs		Peleset-type					Sherden-type		
Rameses III, other	•	•	•			•	•		
horned helmet							•		
turban						•			
Merenptah				•		•	•	•	•
pre-Mer. raiders or mercenaries							•		•
"-esh" name				•	•	•		•	
"of (the countries of) the sea"					•	•	•	•	

**Fig. 3.** Chart showing an analysis of the three groupings of the so-called "Sea Peoples." They should not be lumped together and, while they may have acted in concert at times, there is little in common between the enemies of Rameses III and those of Merenptah, who were allies of the Libyans. © P. James, handout from James 1995.

tion of a naval battle with the Philistines and their allies. And although the scenes showing wars with the Libyans and Nubians are held to be stereotypical, they have many details which show them to be original compositions.<sup>89</sup>

### “ALL CHANGE!”

There has never, of course, been any consensus on the reality and extent of Ramesses III's claims to northern conquests. As documented above, in the 1970s the majority view of leading Egyptologists was that most of them were fictional. The last few decades have seen a sea-change in opinion regarding the reality of Ramesses III's military exploits. Two factors must have played a part here: the archaeological evidence—including the find of a statue of the pharaoh as far north as Byblos—and the complete change of mind by Kenneth Kitchen. This presumably came about as he studied further and published his translations and commentaries on the Asiatic toponym lists of Ramesses III. Now there has been a complete turnaround, where the old “southern” school has now changed largely into a far “northern” one.

As we have seen, the idea that Ramesses III's campaign to Djahi (Palestine and/or Phoenicia) was a historical reality has become increasingly appreciated. An array of reliefs depicts his preparations and efforts to fight the Philistines and their allies and it has become increasingly obvious that this was in *their own lands*. The land-battle reliefs show women and children in ox-carts amongst the enemies. The traditional interpretations have always been that these display the tail-end of a massive migration of itinerant “Sea Peoples,” who for some unknown reason marched all the way from the Aegean to the borders of Egypt, destroying the Hittite Empire and picking up some Anatolian oxen on the way. Drews offered the brilliant insight that the famous battle scene between Ramesses and the *Peleset* and their allies does not

<sup>89</sup> Kahn (2010: 20, n. 45) lists in some detail the differences between the Nubian war reliefs of Ramesses II and III. Most significant of these, surely, is the appearance of mercenaries on the Egyptian side of the *Peleset/Tjekker* type, wearing the characteristic “feathered” headdress. Warriors dressed in this style do not appear on the extant reliefs of Ramesses II.

show the Pharaoh repelling an invasion by land from these enemies—but far more likely *his* invasion of Palestine, to retaliate against them and their sea-borne allies for incursions on Egyptian territory and attempts to penetrate the Nile river mouths.<sup>90</sup>

Even more radical are the ideas published in various papers by Kahn, which offer arguments in support of Ramesses III's claimed campaigns in Syria. The reliefs depicting these wars show rather conventionalised scenes of Syrian and Hittite cities being besieged, usually assumed to have to been copied from those of Ramesses II. But Kahn has recently defended the originality of the northern siege scenes, pointing out major differences between those of Ramesses III and Ramesses II:

*While comparing the war reliefs of Ramesses III with those remaining from his Ramesside predecessors (mainly Ramesses II and Merenptah), it becomes clear that there are no exact original reliefs to copy from—none are even similar in composition, division of registers, or even scene details. The scenes in the different temples differ in the details of the heavily fortified town and the fierce resistance of the defenders upon the walls.*<sup>91</sup>

As to the notorious problem of Ramesses III attacking Hittite towns, the evidence here has been clearly misread. One scene shows an attack on a town, with Syrian inhabitants, labelled “Tunip of Hatti”—which is reasonable as the city would have long been within the Hittite orbit. Another scene shows an attack on two towns where the inhabitants have a “Hittite” appearance; the name of the second is missing, while the reading of the first name as “Arzawa” has always been problematic (see later). The orthography of this name (*i-r-t*) is slightly different from the spelling *i-r'-t-w* (generally read as Arzawa) in the Year

<sup>90</sup> Drews 1998; 2000.

<sup>91</sup> Kahn 2010: 16. More recently Redford (2018: 143) has joined the defence of these scenes against the charge of being “generic and unhistorical...slavishly copied from the military art of Ramesses II.”

8 inscription.<sup>92</sup> That the inhabitants of these two cities are depicted as Hittite in appearance does not mean that they were located in Anatolia—again, Hittites had been ruling over and settling in northern Syria for centuries. The problem was recognised by Kitchen, who proposed a reading of the name as “Arzi,” with the note: “This name is not Arzawa, could be for Ullaza, but is most likely an error for As/zir.”<sup>93</sup> Ullaza or Ulassa lies on the Lebanese coast about 20 km north of Byblos. In an extraordinary *volte face* from his 1991 position (above), Kitchen can now write that:

*Here opinions vary all the way from taking these scenes in some measure seriously to treating them as pure fiction, just recopies of scenes of Ramesses II at that Ramesseum—the more so as the Chicago translators.... wrongly identified one “Hittite” fort as Arzawa (deep in Asia Minor!) instead of Ullaza (Phoenician coast). Not even Ramesses II could ever have reached Arzawa; and this spurious identification lent Ramesses III’s reliefs an equally spurious air of unreality that has stimulated widespread skepticism among scholars (including myself).<sup>94</sup>*

Further evidence for Ramesses III’s Syrian campaign(s) may come from the toponym lists he inscribed at Medinet Habu. These have always attracted far less serious study than those, for example, of Thutmose III, perhaps because of the assumption that they are simply copies of the lists of Ramesses II. So, Weinstein:

*The Asiatic references in the only “topographical lists” of the 20<sup>th</sup> Dynasty—those of Ramesses III at Medinet Habu—were largely copied from earlier sources...so their mentions of Syrian place-names are evidently without military significance.<sup>95</sup>*

<sup>92</sup> Transliterations from Edgerton and Wilson 1936: 95, 37. See Kahn 2010: 17.

<sup>93</sup> Kitchen 2008: 61 and n. 1; cf. Kahn 2010: 17.

<sup>94</sup> Kitchen 2012: 12.

<sup>95</sup> Weinstein 1992: 142–143.

Many sections of the Great Asiatic List of Ramesses III<sup>96</sup> are indeed paralleled by that of Ramesses II. But as Simons remarked long ago: "... the lack of originality of even the lists of Ramses III at Medinet Habu should not be exaggerated."<sup>97</sup> There are major differences between the two lists, not only in the arrangement of those sections.<sup>98</sup> As Kitchen notes: "The first 69 Asiatic names (S. Tower) do not seem to appear in any other known list..."<sup>99</sup> This actually constitutes *over half the list*. While the absence of a similar composition from earlier lists does not prove beyond any possible doubt that it is original to the time of Ramesses III, it should certainly give us serious pause for thought about the usual assumption that all the lists of Ramesses III are mere slavish copies.

Kahn and Redford have also stressed the uniqueness of this portion of Ramesses III's "Great Asiatic list."<sup>100</sup> Kahn rightly questioned Astour's attempts to locate many of them at randomly scattered and often obscure sites throughout the whole of Mesopotamia!<sup>101</sup> Those toponyms that are clearly identifiable (such as Carchemish and Emar) strongly suggest a focus on northern Syria.<sup>102</sup> Kahn has combined this evidence with that from the reliefs that depict the siege of Syrian or Hittite towns to reconstruct a campaign by Ramesses III against

<sup>96</sup> By "Great Asiatic List" I mean Simons' *XXVII* (Simons 1937: 164–169), which comprises some 125 entries. Edgerton and Wilson (1936: 109): "The list is chiefly Semitic, but it has African padding at its southern end (Nos. 95–97 and 123–125)." The prisoners' heads on these are African (Simons 1937: 78). List *XXVIII* (Simons 1937: 170–173) is about the same length (124 entries) but contains many more (apparently) African names (Nos. 1–72), thought to be taken from the African lists of Thutmose III and Seti I (Edgerton and Wilson 1936: 114).

<sup>97</sup> Simons 1937: 80, n. 4.

<sup>98</sup> I hope to deal with the entire Great Asiatic List of Ramesses III in some detail elsewhere.

<sup>99</sup> Kitchen 2009: 134. Strictly speaking, many of the names *do* occur on earlier toponym lists, e.g. No. 28 *mtn* is clearly Mitanni and No. 29 *krkmš* Carchemish. This does not spoil the point that this part of the list as a whole seems to be a composition unique to the time of Ramesses III.

<sup>100</sup> Kahn 2011: 4 and Redford 2018: 142.

<sup>101</sup> Astour 1963; 1968.

<sup>102</sup> Kahn 2011: 11. While I largely accept Kahn's identifications, there are instances of disagreement which I hope to deal with elsewhere (James in prep.) See now Redford's analysis (2018: 143–147) which has many agreements with Kahn's work but also significant differences.



Amurru. Though naturally with some major differences, it is effectively a return to the position of Sayce, almost exactly a century earlier.<sup>103</sup>

Kahn's examination of the northern war reliefs is encouraging, though caution is needed. In his earlier publications he overlooked some further war scenes, from the exterior west wall of Ramesses III's south temple at Karnak in the precinct of Mut. The best preserved shows the king in triumph after the end of a Libyan war.<sup>104</sup> Less well preserved scenes depict the pharaoh battling Syrians, with this inscription accompanying a scene showing captives:

*Receiving captures, being what the (mighty) sword of His Majesty brought off, when (he) overthrew the fallen ones of Qadesh, along with all the chiefs of foreign lands who has come with them, (when) His Majesty was alone, without his army with him—when he gave charge to his army-leaders, saying: “Bring you the captures from my captives, (from) when I was alone, there being no infantry with me, and no chariotry.”*<sup>105</sup>

Here we see exactly the familiar claim made by Ramesses II, that he charged alone against the vast coalition of armies brought by the Hittites to defend Qadesh.<sup>106</sup> So Mosjov, who stated confidently that the scenes from the precinct of Mut were:

*...copied from the southern enclosure wall of the Karnak Temple. They represent Ramesses II and the battle of Kadesh. The sculptors did not notice that the inscription they were copying referred to the famous single-handed battle of Ramesses II against the Hittites and could have no possible application to any achievement of Ramesses III. These monuments of Ramesses II were copied even when inappropriate.*<sup>107</sup>

<sup>103</sup> Sayce 1912: 141–142, 202–207.

<sup>104</sup> Mosjov 2012: 290.

<sup>105</sup> Trans. Kitchen 2008: 67.

<sup>106</sup> Gardiner 1960: 98.

<sup>107</sup> Mosjov 2012: 290.

It seems unfair to claim that the sculptors were blindly copying inscriptions of Ramesses II. (For one thing any blame should be laid on scribes.) On the other hand, the “victory” claim, with the Pharaoh depicted as charging alone against the enemies is surely borrowed from Ramesses II’s Qadesh inscriptions. In these the main foe faced by Ramesses II is not the ruler of Qadesh, but the King of Hatti and all his allies and subject states. Kahn (forthcoming), rather half-heartedly tries to defend some originality in the Ramesses III version, but admits that: “It is hard to tell.”

All the same, Kahn has taken a further step, arguing that it was the Philistines who were attacked in Ramesses III’s north Syrian campaign. He locates them (at some point in their alleged migrations) far to the north of traditional Philistia in the Amuq valley region. While forcing us to re-examine old ideas, Kahn’s model raises more questions than it answers. Much of Kahn’s case relies on an interpretation of Ramesses III’s Great List of Asiatic towns, where it has long been realised that the first 69 entries, including clear references to Carchemish and Mitanni, show that we are in the north Syrian area at least as far as the Euphrates. Kahn rightly rejects the scatter-gun approach of Astour but his own, concentrating on the region around Alalakh, has tended to “hoover” northwards toponyms which might well be better located much further south.<sup>108</sup>

Oddly enough Kitchen is now one of the main advocates in locating Ramesses III’s toponyms in the north, many of which have perfectly good southern correlates in the south. A good case in point concerns the toponym *y-r-d-n* which occurs in parallel lists (see below) of Ramesses II (no. 15) and Ramesses III (no. 91). Against the grain of common sense, Kitchen follows Helck in reading this as an unknown

<sup>108</sup> For example, Kahn (2016: 162) argues that the “cluster” of names 70–78 belongs to northern Canaan. These include the usual examples of Harnaim and Shebtuna, though Kahn himself notes that: “Several of these toponyms are known from the Qadesh inscriptions of Ramesses II but do not occur in any of his surviving topographical lists.” Similar names do, however, occur in the toponym lists of Ramesses III but their context suggests that there were southern rather than northern Canaanite towns (see below).

toponym Ardana or Arduna, identified with modern Arde near Tripoli.<sup>109</sup> There are no grounds for rejecting the obvious reading Jordan, because *ì-r-d-n* is not accompanied by a water determinative. In the well-known Satirical Letter (Papyrus Anastasi I), the writer teases another scribe on his knowledge, including the question: “The stream of Jordan [*Yrdn*] how is it crossed?”<sup>110</sup> The context, placing it near Beth-Shean makes it clear that this is the familiar Jordan.<sup>111</sup> Gardiner was in no doubt on this despite the lack of the determinative for water.<sup>112</sup> The arbitrary relegation of this toponym to the Tripoli area merely deprives us of an obvious clue as to where we should locate this section of the lists of Ramesses II and III.

### THE BLESSING OF PTAH FROM MEDINET HABU AND KRAHMALKOV’S “ROAD” THEORY

A century ago Sayce and his contemporaries seem to have appreciated that there was no need for a “maximalist” or “minimalist” approach: rather we should be considering the clues given us by the toponym lists, analysing their meaning, and not bundling them together because of preconceptions in either direction.

Something of a “middle way” was suggested by Krahmalkov (1994), a senior expert on West Semitic philology who argued that various itineraries or “roads” could be detected in Egyptian New Kingdom records—and that these confirm the routes taken by the Israelites in

<sup>109</sup> Kitchen 1999: 69; 2008: 74. He seems to have an aversion to reading “Jordan” in the toponym lists. The name (*yrđn*) occurs in the list of Shoshenq I, but he claims (1986: 441) that due its context (# 150 at the end of his Row X) near Negebite names it could be a “South-West Palestinian name like Jorda.” No such place is known. Furthermore, its position at the end of a line (in terms of the sculpture) means that it may well be grouped with the entries in the following Row. As Kitchen himself notes: “In the VI<sup>th</sup> to X<sup>th</sup> rows, no clear segments of march-routes can be mapped; too many names are unknown, and even of those that are known but few can firmly be located. Row XI is so incomplete that its geographical contribution is nearly negligible.”

<sup>110</sup> Trans Gardiner 1911: 24\*; Wilson 1969: 477.

<sup>111</sup> Aḥituv 1984: 123.

<sup>112</sup> Gardiner 1911: 24\*, n. 16; cf. Aḥituv 1984: 123.

their march into and conquest of Canaan. Naturally these were in Transjordan and Palestine. With respect to Ramesses III, Krahmalkov cited the short toponym list accompanying the “Blessing of Ptah”<sup>113</sup> The text merits close examination as, for other reasons, it has bearing on the originality of Ramesses III’s inscriptions.

The “Blessing of Ptah” consists of a long address by the god to the Pharaoh, to which Ramesses III replies outlining the good works he has done for the deity. It is dated to his Year 12. Interestingly this matches the date “in or shortly after the year 11,” deduced by Breasted for a major Asiatic campaign of Ramesses III—that depicted in the reliefs showing the storming of cities in Amor and elsewhere to the north.<sup>114</sup>

Overall the text mirrors that from the Abu Simbel stela of Ramesses II, year 35.<sup>115</sup> It is not a straightforward copy however: Breasted carefully noted all the differences between the two versions.<sup>116</sup> One is particularly significant. The Ramesses II version commemorates his marriage to the daughter of Hattusili III, stating that Ptah had “made for thee the land of Hatti into subjects of thy palace...His eldest daughter is in front thereof, to satisfy the heart of the Lord of the Two Lands...”<sup>117</sup> The Ramesses III version renders this as “every land” instead of “Hatti” and “their sons and daughters as slaves to thy palace” instead of mention of a Hittite bride.<sup>118</sup> It omits the final part altogether, which referred to friendly Egypto-Hittite relations.<sup>119</sup> The text clearly reflects a changed political situation, to which the Egyptian scribes had responded sensitively. It can only mean that the central polity of Hatti based at Hattuša had weakened to such an extent that specific reference to an old marriage alliance (now redundant) was no longer worth boasting about; or that a new Hittite power centre or centres had replaced it.

<sup>113</sup> Simons 1937: 174, List XXIX.

<sup>114</sup> Breasted, *ARE* IV: 79

<sup>115</sup> See Kitchen 1996: 99–110.

<sup>116</sup> *ARE* III: §§394–414; cf. Kitchen 1996, 99–110.

<sup>117</sup> Trans. Breasted, *ARE* III: §410.

<sup>118</sup> Breasted, *ARE* IV: §134; cf. Kitchen 1996, 107.

<sup>119</sup> Kitchen 1996: 107; 1999: 161.

If the “imperial” centre at Hattuša had collapsed, at least two new polities where “Great Kingship” was claimed were in existence: Tarhuntassa in southeastern Anatolia and Carchemish on the Euphrates, initiating what Kitchen has nicely described as an age of “mini empires.”<sup>120</sup> The indications from military scenes and lists of prisoners of Ramesses III suggest that Egyptian relations with these mini-Hittite kingdoms were not friendly. Either the ruler of Tarhuntassa but most likely Carchemish could represent iconically “the [or more exactly “a”] despicable chief of Hatti as prisoner”<sup>121</sup> in a row of bound captives which appears to depict the seven main northern enemies of Egypt.

While Egyptologists have often decried Ramesses III’s “copies” for their errors, it is interesting to note that to Breasted’s eye the Ramesses III version of the Ptah Blessing was less corrupt and the grammar better than the Ramesses II version at Abu Simbel.<sup>122</sup> From their own analysis, Edgerton and Wilson (emphasis added) concluded:

*The Medinet Habu text was not copied from the Abu Simbel Text. In the first place, it was liberally re-edited, both as to the political situation and perhaps for better phraseology. In the second place the Abu Simbel text is itself obviously secondary, for it contains such corruptions as could have originated only in copying through the hieratic from a hieroglyphic text. The connecting link must be sought elsewhere. One might hazard a guess that the text was first prepared for a temple at Memphis (that of Ramses II?), perhaps on the occasion of a royal jubilee there. The Abu Simbel text was then a copy of this, without deliberate alteration. The Medinet Habu text was adapted and edited for a different king and different location.*<sup>123</sup>

<sup>120</sup> Kitchen 2003: 98–100, 616, Fig. 15.

<sup>121</sup> Kitchen 2008: 80.

<sup>122</sup> Breasted, *ARE* III: 175, n. b.

<sup>123</sup> Edgerton and Wilson 1936: 119–120.

As the subtle changes in Ramesses III's "Blessing of Ptah" demonstrate, his version was no slavish copy of that prepared for Ramesses II.

Both versions of the Blessing are accompanied by a list of towns that Ptah had placed in the power of the Pharaoh. Regarding the list that accompanied Ramesses II's Blessing, Kitchen notes: "The topographical list at Karnak is badly damaged and appears to differ in arrangement and content from that of Ramesses III at Medinet Habu."<sup>124</sup> In short, nothing like the toponym list from Ramesses III's Blessing of Ptah is known from earlier Egyptian records. It is reasonable to assume, then, that its composition is original to Ramesses III. Besides, the removal of any reference to the Hittite marriage alliance from the text of the Blessing itself shows that the scribes who prepared it were well aware of Egypt's foreign relations at this time.

These facts make an analysis of the accompanying place-names worthwhile. The first observation regarding the "Blessing of Ptah" toponyms is that they are also known from the Great Asiatic List of Ramesses III—as can be seen from the comparisons made by Simons tabulated below.<sup>125</sup> The transliterations are Simons' with the odd correction from Edgerton and Wilson and Kitchen.

*Blessing of Ptah (XXIX)*

1. *t-ś-t*
2. *ì-r-<y>*
3. *ì-n-t-k*

*Great Asiatic List (XXVII)*

25. *t-ś-ḥ*
26. *ì-r-<y>*
44. *ì-n-t-k*

BELOW SCENE: FACING LEFT

4. *k-r-n*
5. *ì-t-?*
6. *t-r-b-ś*
7. *t-r-n*

40. *k-r-n*
41. *ì-q-?*
42. *t-r-b-ś*
43. *t-r-n*

<sup>124</sup> Kitchen 1999: 160.

<sup>125</sup> Simons 1937: 164–169.

## BELOW SCENE: FACING RIGHT

- |                                  |                    |
|----------------------------------|--------------------|
| 8. <i>h-r-n-m</i> <sup>126</sup> | 70. <i>h-r-n-m</i> |
| 9. <i>r-b-n-t</i>                | 71. <i>r-b-n-t</i> |
| 10. <i>h-b-r</i>                 | 77. <i>h-b-r</i>   |
| 11. <i>itr</i>                   | 101. <i>itr</i>    |
| 12. <i>r-h-b</i> <sup>127</sup>  | 105. <i>r-h-b</i>  |
| 13. <i>y-h</i>                   | 115. <i>y-h</i>    |

Despite small variations in spelling (and one difference in order) there can be no doubt about the close relationship between the two lists. See Simons who described the Ptah list as “an extract from the longer Asiatic list (*XXVII*) on the same tower.”<sup>128</sup> As it is composed of three extracts, Kitchen’s choice of the word “abregé” might be more accurate: by definition the Blessing of Ptah list would fall into Kitchen’s category 2 (*a*). *i.e.* “Abregés of longer listings.” But is the Ptah list merely this, a random summary of names from the longer list “of no importance”?—as Simons said in an uncharacteristically dismissive remark. At the very least, analysis of the list might reveal something of the mechanics of the selection used by its compilers.<sup>129</sup>

There are barely any recent discussions of this list, with the notable exception of Krahmalkov (1994, 56, 61) who offered some interesting suggestions regarding the identifications of the last four names in the list (Nos. **10–13**). These he argued represent an “Egyptian road” running from north to south, from southern Judah to places in the Negev. Thus:

*h-b-r* = Hebron.

*itr* = Athar, which gave its name to the “way of Atharim” (Numbers 21.1) which seems to have run from Arad to Kadesh-barnea.

<sup>126</sup> For some unexplained reason Kitchen (1996: 101) reads this as “Naharin,” *i.e.* Mitanni. See remarks below, n. 157.

<sup>127</sup> As Krahmalkov (1994: 62, n.16) noted, the reading of *r-h-d* given in Simons has been corrected by Kitchen to *r-h-b*—see Kitchen 1996: 101 and 1999, 160, where he locates this Rehob as the well-known township some 4 miles from Beth-Shean.

<sup>128</sup> Simons 1937: 12, n. 1.

*r-h-b* = a Rehob, which Krahmalkov identified with the oasis of Ruhayba, south of Beersheba.

*yh* = Yahu. His map and text indicate a location in north central Sinai. Krahmalkov's suggestions are attractive, in particular the suggestion that *h-b-r* was Hebron (see below). Though he does not mention it, his placement of *yh* is well supported by other New Kingdom texts. The "Shasu land of Yahu" (*Šsw yhw*) is mentioned by Amenhotep III in a list of three "Shasu lands" and in a list of six "Shasu lands" by Ramesses II.<sup>130</sup> From other evidence these were likely placed in the Edom/Seir region.<sup>131</sup> The biblical association of Yahweh with Mt Seir (Deut. 33:2; Judges 5:4) makes this a likely location for the "land of Yahu."

At first glance Krahmalkov's "road" theory might seem plausible. Redford had already used the great toponym list of Thutmose III to identify a campaign itinerary of 15 names in Transjordan.<sup>132</sup> Some of the identifications remain uncertain and Redford's hypothesis, as Kitchen put it "remains such, but has much to recommend it."<sup>133</sup> Krahmalkov offered his own version of the list, running from south to north—the reverse direction to that proposed by Redford! Given the different interpretations possible, it would be safer to call the Transjordanian toponyms in question a group or cluster. Further, Redford's case has a distinct advantage here in that he checked the

<sup>129</sup> The first three entries on this list are obscure. Breasted (*ARE* IV: §134–135) assumed they are "African," an idea which Kitchen (1999: 160) took further, stating that all the names before no. 8 "appear to be African and obscure at present." Here we encounter an unfortunate tendency among Egyptologists when interpreting difficult to read place-names, to describe anything that appears unintelligible as "African," without offering any philological evidence for the supposition. Breasted himself noted that the thirteen name rings are accompanied by "thirteen Asiatic (sic!) prisoners" (*ARE* IV: §134), adding "sic!" as if the Egyptian artist had made some mistake! The heads of the prisoners surmounting the name rings are clearly Asiatic and not African (see *MH* II, Pl. 105). Simons (1936: 84) did not repeat Breasted's "African" suggestion, quite rightly describing it as an "Asiatic list."

<sup>130</sup> Giveon 1971: 25–28; 74–77.

<sup>131</sup> Redford 1992: 272–273; cf. Kitchen 1992: 25–27. Oddly, Kitchen (1999: 160) is lukewarm about the identification of *yh* with the Shasu land of *yhw*.

<sup>132</sup> Redford 1982.

<sup>133</sup> Kitchen 1992: 25.



topography on the ground. He also took in 15 names from the Thutmose III list, whereas Krahmalkov was extremely selective, only using a small selection of four (numbers 95, 98, 99, 100).

The same problem affects his “itinerary” interpretation of the list from the Ptah Blessing. Krahmalkov took only a selected portion, nos. 10–13, ignoring, without any explanation, entries 8 and 9 on the same panel, let alone 1–7 many of which are (to us) presently unlocatable. As can be seen from the table above, the toponym list from the Blessing of Ptah is without a shadow of a doubt a much compressed version of the Great Asiatic List of Ramesses III. It is not a “road.”

### TOWARDS A NEW METHODOLOGY

Krahmalkov’s “cherry-picking” from one list or another to suit some biblically-based ideas (specifically an Israelite Conquest *c.* 1200 BC) was unsound, though his much ignored paper offered some promising identifications to be examined shortly.

What should one expect from Ramesses III’s lists and how can we discern “truth” from pure “fiction,” or more accurately propaganda? This can be approached by using the control of his other “historical” records and more important archaeology. From the buildings and inscriptions at Beth Shean and Megiddo we know that he had a military presence in the Jezreel valley (see above). A statue fragment of Ramesses III found at Byblos<sup>134</sup> might suggest a measure of influence this far north. But with the possible exception of Tyre no Phoenician towns appear to be mentioned in his toponym lists, a point which Sayce drew attention to well over a century ago.<sup>135</sup> On the other hand, as I hope to show below, the toponym lists are fairly clear in showing

<sup>134</sup> Kitchen 2008: 215; 2012: 13.

<sup>135</sup> Sayce 1912: 206: “One point about this list is very noticeable. None of the great Phoenician cities of the coast are mentioned in it. Acre, Ekdippa, Tyre, Sidon, and Beyrout are all conspicuous by their absence. Even Joppa is unnamed.” As noted below Tyre *may* be mentioned in Ramesses III’s Great Asiatic List (XXVII, 121). Otherwise, we need to note the point made by Bikai (1992: esp. 133) that there is no evidence of the destruction of any Phoenician town at the LBA/IA transition.

groups of towns claimed to be under his control, which would have been needed to reach the Jezreel valley sites—and further north.

First and foremost, it must be stated that they *do* contain repetition of place-names from the great lists of Thutmose III and Ramesses II—as appreciated long ago by the Sayce. Yet, regarding Ramesses III’s “Great List” he noted that:

*...the names, however are practically those already enumerated by Ramses II., and they occur in the same order. But the list given by Ramses III, could not have been copied from the older list of Ramses II. for a very sufficient reason. In some instances, the names given by the earlier monarch are misspelt, letters having been omitted in them or wrong letters having been written place of the right ones, while in the list of Ramses III. the same names are correctly written.*<sup>136</sup>

To what degree Sayce was correct here remains to be seen, but a century later Kitchen would agree at least in some instances.<sup>137</sup> This reinforces the point that the lists of Ramesses III were not mere copies of those of Ramesses II, but that they both derived from a common template. Such templates would have kept by scribes on papyrus—there is no question of sculptors running from one monument to another as some see to infer.

Some compositions, such as the long toponym list of Thutmose III show much cohesion in terms of the geographical arrangement of their entries (see above Redford re Transjordan)—even though one of the name clusters has been largely relocated by modern scholars from south to north.<sup>138</sup> With respect to Ramesses III, most of his lists have received little or no recent analysis. From our perspective some appear

<sup>136</sup> Sayce 1912: 202.

<sup>137</sup> *E.g.* with respect to Ramesses II 10 (Kerak), Kitchen (1999: 68) notes: “The second *k* lacks its ‘handle,’ an error remedied in the copy in Ramesses III’s Medinet Habu list...” Kitchen (1999: 70) also prefers the Ramesses III reading (76) of *m-š* to the *m-š* at the beginning of Ramesses II (25).

<sup>138</sup> Compare the map of Petrie 1896: 324 with that of Aharoni 1979: 155.

unintelligible and randomly organised. One is the long List XXVIII, the first half of which nos. 1–72) appears to largely cover African and Red Sea names (including Punt).<sup>139</sup> The second half seems to be largely Asiatic, but with barely any readable names. Yet it includes (no. 77) a possible Ain-Gihon (“spring of Gihon”), as restored tentatively by Edgerton and Wilson—a reading “not excluded” by Simons.<sup>140</sup> Gihon was the mainspring of water at Jerusalem and this appears to be the only possible instance of such a place name in Egyptian records. At present our understanding of the arrangement of difficult and damaged lists such as XXVIII is far too rudimentary to make informed guesses.

We *do* have a better handle on matters when we are able to compare lengthy parts of one pharaoh’s toponym lists to those of others. Simons (1937: 75) did the basic groundwork here, providing a handy comparison of the lists of Thutmose III, Seti I and Ramesses II.<sup>141</sup> It is clear that the latter two have little reliance on those of Thutmose: the 19<sup>th</sup> dynasty lists are clearly original compositions which must reflect the campaigns of Seti I and Ramesses II. Aharoni drew up a map using Seti’s toponyms,<sup>142</sup> which is extremely plausible—and may well, as he understood it, reflect a military itinerary. Otherwise Simons’ listings show that one of Ramesses II’s lists (XXIV) matched that of his father Seti (XIII) so closely that there is little doubt that it was derivative.

Other lists of Ramesses II are quite different and certainly reflect his more extensive campaigns, largely aimed at the growing power of the Hittites in northern Syria. Still this should not mean that all the place-names concerned were far to the north. After his effectual defeat at Qadesh, Ramesses II had to reassert his authority over much of Canaan, as did his successor Merenptah who famously claimed to have conquered Gezer, faced rebellions in the south.<sup>143</sup> It should be no coincidence that the survey work of Peter van der Veen and his team

<sup>139</sup> See O’Connor 1982: 933, n. 1.

<sup>140</sup> Edgerton and Wilson 1936: 115; Simons 1937: 172.

<sup>141</sup> Simons 1937: 75.

<sup>142</sup> Aharoni 1979: 178–180; Simons 1937, XIII.

<sup>143</sup> See conveniently Kitchen 1982: 67–68, 215; 2004.

has uncovered a surprising number of fragmentary inscriptions and statuary (including a bust of an Egyptian princess!) which point strongly to an interest in the Jerusalem region in the late Ramesside period.<sup>144</sup>

There are names in common between lists of Ramesses II and Ramesses III, especially with regard to two: Ramesses II XXIII and Ramesses III XXVII often known as his “Great Asiatic List.”<sup>145</sup> The similarities are inescapable (see tables below). However, they do not necessarily mean that Ramesses III’s list was a slavish copy of his predecessor’s, any more than his version of the “Blessing of Ptah” was copied from that of Ramesses II. As shown above this text was a standard one which was scrupulously edited by the scribes of Ramesses III to reflect new conditions (notably the collapse of a Hittite alliance). The most cautious understanding of the similarities between the two is that the scribes were working from a standard copy-book of places presumed to be under the Pharaoh’s control.

As a rudimentary methodology, a simple technique is used for analysing these lists. In the tables below I have compared them, beginning with the Ramesses II version first and in the second the Ramesses III version—using the transcriptions compiled by Simons with the odd correction. At the outset it needs to be stated that there is nothing like these compositions from the records of earlier pharaohs such as Thutmose III or Seti I. Krahmalkov’s “road” theory has been shown to be faulty. Rather we should think in terms of “clusters,” which may well contain mini-itineraries in the sense of mental maps. Avoiding the question for the moment of the location of any of the toponyms, I offer a simple analysis, comparing the two lists and breaking them into groups.

The rule followed here is simple: where one list or the other jumps to a different position numerically, a break or change of locale is assumed: so, I have drawn a line. Some of these changes may be “false” but are included for the sake of caution—the exercise being to

<sup>144</sup> Theis and van der Veen 2012; van der Veen 2013; van der Veen and Ellis 2015; Burger Robin 2015.

<sup>145</sup> Simons 1937: 157–159.

determine which toponyms are agreed by both versions to belong together and afford us with some ideas of geographical “clusters.”

Ramesses II, List XXIII

1.  $r-\langle i \rangle-\check{s} q-d-\check{s}$
2.  $y-[n]-\underline{d}-t$
3.  $[\text{c}^n-n]-g-r$

Ramesses III, List XXVII

108.  $r-\check{s} q-d-\check{s}$
109.  $y-n-\underline{d}-t$
110.  $\text{c}^n-n-g-r$

- 
111.  $r-w-i-r$
  112.  $b-r$
  113.  $q-m-q$
  114.  $q-b-r-\text{c}$
  115.  $y-h$
  116.  $t-\langle w \rangle-r$
  117.  $\acute{s}-n-n-r$
  118.  $m-n-d-r$
  119.  $\underline{d}-b-b$
  120.  $i-m-t$
  121.  $\underline{d}-\langle w \rangle-r$
  122.  $k-r-n$
  - 123-25. “African”

- 
4.  $r-\underline{h}-\underline{d}$
  5.  $\acute{s}-i-b-t$
  6.  $k-\underline{t}(i)?$
  7.  $q-\acute{s}-r-\text{c}$
  8.  $q-\underline{t}-i-\acute{s}-r$
  9.  $y-\text{c}-q-b-r$
  10.  $k-r-k$
  11.  $q-(m)-\acute{s}-p-t$
  12.  $i-\underline{t}-r$

- 
105.  $r-\underline{h}-\underline{d}$
  106.  $\acute{s}-i-b$
  107.  $k-\underline{t}(i)?$
  102.  $q-\acute{s}-r-\text{c}$
  103.  $q-s-\underline{t}-i-\acute{s}-r$
  104.  $y-\text{c}-\{q\}-b-r$
  98.  $i-k-\acute{s}$
  99.  $k-r-k$
  100.  $q-\acute{s}-[b?]-p-t$
  101.  $i-\underline{t}-r$

13. <i>q-ś-n-r-m</i>	89. <i>q-ś-n-r-m</i>
14. <i>q-r-p-n</i>	90. <i>q-r-t-p-n</i>
15. <i>ì-r-d-n</i>	91. <i>ì-r-d-n</i>
16. <i>ḥ-r-t</i>	92. <i>ḥ-r-t</i>
17. <i>q-r-ḥ</i>	93. <i>q-r-ḥ</i>
18. <i>ì-k-t</i>	94. <i>w-r-w</i>
19. <i>ì-b-r</i>	95-97. "African"
20. <i>q-r-m-n</i>	84. <i>q-t-m-n</i>
21. <i>q-ś-r y-b-n</i>	85. <i>q-ś-t-b-r-n</i>
22. <i>š-m-š-n</i>	86. <i>š-m-š-n</i>
23. <i>ḥ-d-ś-t</i>	87. <i>ḥ-d-ś-t</i>
24. <i>ì-t-r</i>	88. <i>ì-r-t</i>
25. <i>m-ś....</i>	76. <i>m-š-k-t-(ś?)-n-r</i>
26. lost	77. <i>ḥ-b-r</i>
27. <i>y-[n—m]</i>	78. <i>y-n-m</i>
28. <i>d-r-b-n</i>	79. <i>d-r-b-n</i>
29. <i>ì-p-q</i>	80. <i>ì-p-q</i>
30. <i>ì-b-h-y</i>	81. <i>ì-b-h-y</i>
31. <i>m-[k-t-r]</i>	82. <i>m-k-t-r</i>
32. <i>[q-r]-t</i>	83. <i>q-r-t-k</i>
33. <i>q-.....</i>	
34. ??	
35. <i>y-.....</i>	
36. <i>c-.....</i>	
37. <i>ì-.....</i>	
38. lost	
39. lost	
40. <i>b-r-[?]</i>	
41. lost	
42. <i>k-r-?</i>	

- 43. *g-b-?*
- 44. *lost*
- 45. *lost*
- 46. *lost*
- 47. *lost*
- 48. *?-r-m-?*
- 49. *lost*

Here is the same comparison but with the Ramesses III version used as the template:

Ramesses III, List XXVII

Ramesses II, List XXIII

- 70. *h-r-n-m*
- 71. *r-b-n-t*
- 72. *b-y-t d-q-n*
- 73. *q-r-b-q*
- 74. *k-r-m-y-m*
- 75. *š-b-d-n*

- 
- 76. *m-š-k-t-(š?)-n-r*
  - 77. *h-b-r*
  - 78. *y-n-m*
  - 79. *d-r-b-n*
  - 80. *ì-p-q*
  - 81. *ì-b-h-y*
  - 82. *m-k-t-r*
  - 83. *q-r-t-k*

- 
- 25. *m-š.....*
  - 26. *lost*
  - 27. *y-[n—m]*
  - 28. *d-r-b-n*
  - 29. *ì-p-q*
  - 30. *ì-b-h-y*
  - 31. *m-[k-t-r]*
  - 32. *[q-r]-t*

- 
- 33. *q-.....*
  - 34. *??*
  - 35. *y-.....*
  - 36. *c-.....*
  - 37. *ì-.....*

38. lost  
 39. lost  
 40. *b-r-[?]*  
 41. lost  
 42. *k-r-?*  
 43. *g-b-?*  
 44. lost  
 45. lost  
 46. lost  
 47. lost  
 48. *?-r-m-?*  
 49. lost

- 
84. *q-t-m-n*  
 85. *[q]-ś-t-b-r-n*  
 86. *š-m-š-n*  
 87. *ḥ-d-ś-t*  
 88. *ì-r-t̄*

- 
20. *q-r-m-n*  
 21. *q-ś-r y-b-n*  
 22. *š-m-š-n*  
 23. *ḥ-d-ś-t*  
 24. *ì-t̄-r*

- 
89. *q-ś-n-r-m*  
 90. *q-r-t̄-p-n*  
 91. *ì r-d-n*  
 92. *ḥ -r-t̄*  
 93. *q-r-ḥ*  
 94. *w-r-w*

- 
13. *q-ś-n-r-m*  
 14. *q-r-p-n*  
 15. *ì-r-d-n*  
 16. *ḥ -r-t̄*  
 17. *q-r-ḥ*

95-97. “African”

- 
98. *ì-k-ś*  
 99. *k-r-k*  
 100. *q-ś-[b?]-p-t*  
 101. *ì-t̄-r*  
 102. *q-ś-r<sup>c</sup>*  
 103. *q-s-t̄-ì-ś-r*

- 
18. *ì-k-t*  
 19. *ì-b-r*  
 10. *k-r-k*  
 11. *q-(m?)-ś-p-t*  
 12. *ì-t̄-r*  
 7. *q-ś-r<sup>c</sup>*  
 8. *q-t̄-ì-ś-r*



- |  |   |
|--|---|
| 104. $y^c\text{-}\{q\} b\text{-}r$         | 9. $y^c\text{-}q\text{-}b\text{-}r$               |
| 105. $r\text{-}h\text{-}\underline{d}$     | 4. $r\text{-}h\text{-}\underline{d}$              |
| 106. $\acute{s}\text{-}\grave{i}\text{-}b$ | 5. $\acute{s}\text{-}\grave{i}\text{-}b\text{-}t$ |
| 107. $k\text{-}\underline{t}\text{-}(i)?$  | 6. $k\text{-}\underline{t}\text{-}(i)?$           |

- 
- |   |  |
|---|--|
| 108. $r\text{-}\acute{s} q\text{-}d\text{-}\acute{s}$ | 1. $r\text{-}\langle i \rangle\text{-}\acute{s} q\text{-}d\text{-}\acute{s}$ |
| 109. $y\text{-}n\text{-}\underline{d}\text{-}t$       | 2. $y\text{-}[n]\text{-}\underline{d}\text{-}t$                              |
| 110. $^c n\text{-}n\text{-}g\text{-}r$                | 3. $[^c n\text{-}n]\text{-}g\text{-}r$                                       |

- 
111.  $r\text{-}w\text{-}\grave{i}\text{-}r$   
 112.  $b\text{-}r$   
 113.  $q\text{-}m\text{-}q$   
 114.  $q\text{-}b\text{-}r\text{-}^c$   
 115.  $y\text{-}h$   
 116.  $t\text{-}\langle w \rangle\text{-}r$   
 117.  $\acute{s}\text{-}n\text{-}n\text{-}r$   
 118.  $m\text{-}n\text{-}d\text{-}r$   
 119.  $\underline{d}\text{-}b\text{-}b$   
 120.  $\grave{i}\text{-}m\text{-}t$   
 121.  $\underline{d}\text{-}\langle w \rangle\text{-}r$   
 122.  $k\text{-}r\text{-}n$   
 123–125. “African”

## INITIAL PREMISES

The lists contain two long series Ramesses III’s entries (111–122) and Ramesses II (34–50) of roughly the same number of names—15 and 17 respectively allowing for damaged text. Despite the very damaged character of the Ramesses II version<sup>146</sup> it is clear that it bears no resemblance to the Ramesses III cluster or any other toponym list, as far as we can see. One must assume that the two sections were composed for each pharaoh. The Ramesses III version contains a number of interest-

<sup>146</sup> Simons 1937: 157–159.

ing names which require much further investigation. To identify *ì-m-t* (120) with Syrian Hamath and *ḏ-<w>-r* (121) with Phoenician Tyre is tempting. But there is always the problem of homonyms, particularly in the case of Hamath. Seti's Beth-Shean stela shows that the ruler of a much closer Hamath was powerful enough to have taken Beth-Shean: it evidently lay south of the latter in the Jordan valley.<sup>147</sup> Discussion of this section (111–125), not paralleled in the Ramesses II version, is beyond the aims of the present paper.

Both Kitchen and Kahn have stressed that the first 69 names in Ramesses III's Great Asiatic list are a unique composition, not known from the extant lists of any earlier pharaoh. Excluding the remote possibility that it was copied from a missing list of Thutmose I or III, it would seem that the list comprises places that Ramesses III claimed to have conquered or controlled. The inclusion in the list of Carchemish, centre of a cadet branch ("Great Kings") of the Hittites is consistent with the change in attitude towards the latter clear from the "Blessing of Ptah" and the depiction of a "chief of Hatti" as a prisoner (see above). Yet any idea that Ramesses III *actually* seized or conquered the 69 cities in hostile territory so far from Egypt is vanishingly unlikely. No one would countenance the idea that Thutmose III attacked the 350 cities given in his Great List.<sup>148</sup> The Pharaoh himself clearly explained. The king of Qadesh and numerous allies were holed up in Megiddo and before its surrender Thutmose proclaimed to his officers during the siege that: "...the capture of Megiddo is the capture of a thousand towns!"<sup>149</sup> Significantly Qadesh appears first in the Great List. It seems very likely that many of the minor place-names in the early part of Ramesses III's Great List were from records of a local regional power such as Alalakh or even Carchemish. Still, that there were some struggles in northern Syria is strongly suggested by a combination of the evidence.

<sup>147</sup> *ANET*, 253 and n. 5; Murnane 1990.

<sup>148</sup> Simons 1937: 109–122.

<sup>149</sup> Trans. Lichtheim 1976: 33.

Agreement regarding the originality of the composition of 1–69 certainly does not lead to acceptance of Kahn’s speculation that these toponyms reflect Ramesses III’s wars against the Philistines and that they first settled in the Amuq region in northern Syria before moving to southern Palestine. The case depends largely on a *single tentative* reading of the spelling “Palistin” rather than “Walastin” on an inscription of king Taita from Aleppo.<sup>150</sup> Hawkins himself concentrates on the locally made Mycenaean-style pottery as evidence for a new dynasty under king Taita, centred at Tell Tayinat. With his usual candour he added this caveat, when turning to the possible connection between the ethonym Philistines “and the kingdom of Palistin/Walastin... concluding that it is not implausible. In a sense, however, the plausibility or otherwise of the suggestion is of no great significance.”<sup>151</sup> Singer was directly critical: “Although there is almost nothing intrinsically impossible in history, Kahn’s recent historical reconstruction of an Egyptian intervention in Taita’s land of Palastin is implausible.”<sup>152</sup> For a further appeal for caution in this matter see Adams and Cohen.<sup>153</sup> Kahn supports his case by reference to the large amounts of apparent Mycenaean IIC-style pottery in the Amuq region.<sup>154</sup> Yet as Adams and Cohen stress: “...this ceramic tradition with local variation is a feature of most coastal regions of the Levant and Cyprus in the early Iron Age where it is variously identified with whatever “Sea People” group is geographically preferred by any given author.” But for the moment Kahn has yet to offer an explanation for the million-dollar question: why, how and *when* could the Philistines have moved from Amuq to southern Palestine to create the small kingdoms of their traditional pentapolis?

<sup>150</sup> Hawkins 2009: 171; 2011.

<sup>151</sup> Hawkins 2011: 52.

<sup>152</sup> Singer (2017: 628) adds: “...the equations between Egyptian and Alalahian suggested by Kahn (2011) are much too precarious and problematic...” Redford (2018: 120) is lukewarm, noting that Taita’s “Land of Palastin” need be nothing more than a geographical location wherein some remnants of the Peleset sought refuge *en route* to the fray, or after the abortive attempt on the Delta...”

<sup>153</sup> Adams and Cohen 2013: 662–663, n. 19.

<sup>154</sup> Kahn 2011; 2015; 2016; followed by Ben-Tor 2017: 164–165.

Further discussion of the opening section of Ramesses III XXVII (1–69) will have to be saved for elsewhere.<sup>155</sup>

### ANALYSIS OF TOPONYMS 70–110 FROM RAMESES III'S "GREAT ASIATIC LIST"

While the tendency of 19<sup>th</sup> century scholars such as Sayce was to locate group 70–75 in Palestine, current trends now attempt to place them in Syria and Lebanon.<sup>156</sup> Likewise Kitchen who, after his “conversion on the road to Amurru” (as it were), completely changed his mind about Ramesses III not having campaigned further north than Edom. While this is an encouraging development one feels that a new bandwagon has been jumped on, moving clusters of places to the far north. As we shall see many of them have not only a southern “Canaanite” feel to them but an Israelite or Judahite one.

#### 70. *h-r-n-m*

Kitchen (2008, 74) reads as “Harnam.” As well as beginning a cluster here it also appears at the head of a new section in the Blessing of Ptah list (XXIX:8, see above).<sup>157</sup> Though not known from other toponym lists it would seem to be of some importance. Still, it has long been noted<sup>158</sup> that *Hrnm* occurs in the satirical Papyrus Anastasi I, 22, 4,<sup>159</sup> where the scribe under criticism is told: “Thou has not gone to the land of Takhshi, Kur-mereren, Timnat, Qadesh, Deper, Azai or Harnaim.”<sup>160</sup> Some of these locations are definitely Syrian (Takhshi and Deper), and probably Qadesh (though there was more than one place of that name).

<sup>155</sup> James in prep.

<sup>156</sup> E.g. Kahn (2011; 2015; 2016; forthcoming) and Ben-Dor Evian (2015; 2016).

<sup>157</sup> Note needs to be made of the highly eccentric reading of Kitchen (1999: 160) which takes no. 8 in the Ptah list as “Naharin,” *i.e.* Mitanni in the far north! To the best of my knowledge no other scholar has accepted this reading which, from the Chicago copies (*MH* II, Plate 101; see also Edgerton and Wilson 1936: 109) is clearly *hrnm*.

<sup>158</sup> Sayce 1892: 31, 38; Edgerton and Wilson 1936: 109; Jirku 1937: 45.

<sup>159</sup> Gardiner 1911: 24\*.

<sup>160</sup> Trans. Wilson 1969: 477.

Hence the majority of scholars have assumed that this is the Harnem or Harmel on the route to Qadesh,<sup>161</sup> known notably from the war records of Ramesses II (*Irnem/Aronama* in the reading of Gardiner).<sup>162</sup> However, the location of other named sites is unknown and there is, of course a southern Timnat (the Timna valley in the southwestern Arabah). The rhetorical and tricky nature of the text, which seems to jump deliberately from region to region in challenging the knowledge of its addressee, makes it difficult to locate this *hrnm*.<sup>163</sup> Immediately after the mention of *hrnm* the scribe is accused of being unaware of Kirjath-anab and Beth-Sepher which, despite some complications, Gardiner had “little doubt” about their location in the hill-country of Judah.<sup>164</sup> There is then a fair case for considering a southern location for the *hrnm* in Papyrus Anastasi I.

Both Cook and Dussaud offered an interesting reading (close to that of Wilson’s)<sup>165</sup> for the Papyrus Anastasi reference: “Horonaim,” the ending of which suggests a dual case. There was a Horonaim in Moab as known from Jeremiah (48:3, 48:5, 48:34) and Isaiah (15:5): they both refer to “the road to Horonaim,” showing that it was on a strategic route, while it is also known from the famous inscription of king Mesha.<sup>166</sup>

Regarding the Horonaim in the letter of the satirical scribe, Dussaud suggested an identification with the two Beth Horons, Upper and Lower.<sup>167</sup> This use of Horonaim seems to be attested in an important Septuagint variation to the Massoretic text in the account of the flight

<sup>161</sup> See e.g. Kahn 2010: 16.

<sup>162</sup> Gardiner 1960: 8 and 17, P61.

<sup>163</sup> Gardiner (1911: 24\*, n. 4) felt that the *t-m-n-t* here was surely not the Timnath of Judah, “for while of the seven names given here four are unknown, the other three are N. of Damascus.” Still, Gardiner noted that the places “appear to range from the North of Syria to the extreme South of Palestine.”

<sup>164</sup> Gardiner 1911, 24\*, n. 7.

<sup>165</sup> Cook 1931: 326 and Dussaud 1928: 74.

<sup>166</sup> See Dearman 1989: 188–199 and Lipiński 2006: 348–351 for the possible locations of Moabite Horonaim. For a guesstimate see Aharoni 1979, 338, Map 26.

<sup>167</sup> “H-r-n-m est, selon nous, à lire Horonaim (voir Syria, 1927, p. 258); il ne s’agit pas de la ville de ce nom en Moab, mais plutôt de Bet-Ḥoron divisée en deux sites dont l’ensemble a pu aussi être dénommé Ḥoronaim.”

of Absalom (2 Samuel 13:34): “And the watchman came and told the king, and said, I have seen men coming from the way of Oronen, by the side of the hill.” Because of the geographical setting (involving a report from Jerusalem) this *Oronen* is often understood as a dual form, \*Horonaim—for the two Beth Horons and accordingly restored in the text.<sup>168</sup> The dual form may be reflected again in the Ὠρωνίβ of Septuagint Joshua 10.10–11.

### 71. *r-b-n-t*

Not known from earlier lists but obviously a \*Lebonath/Libnath. See Kitchen: “Rabnat is phonetically comparable with (*e.g.*) Hebrew/W. Semitic Libnah, and cf. the ‘Shasu-land’ R/Laban, the Amarah West lists...”<sup>169</sup> Possibly Libnah (Shephelah) or Lebonah (northern kingdom). Aharoni noted the Libnath mentioned in Joshua 19:26–27 in the boundary description of Asher: “which reacheth to Carmel westward, and to Shihor-libnath; and it turneth toward the sunrising to Beth-dagon...” He added that the *r-b-n-t* of Ramesses III could well be this Libnath, there mentioned alongside a Beth-Dagon.<sup>170</sup> The Septuagint (followed by Eusebius and Jerome) treats Shihor and Libnath (Labanath) as separate places. Aharoni suggested that the biblical Libnath may have been the important trading centre of Tell Abu Hawam.<sup>171</sup> However, there were other Beth-Dagons. One is mentioned by Sennacherib as a city belonging to the kingdom of Ashkelon and thought to lie s.e. of Joppa.<sup>172</sup> Significantly, Joshua 15:41 places another Beth Dagon in the south, only three entries before Libnah. Joshua 15:37–44 groups it with Lachish and Migdal-gad: “Zenan, Hadashah, Migdal-gad, Dilean, Mizpeh, Joktheel, Lachish, Bozkath, Eglon, Cabbon, Lahmam, Chitlish, Gederoth, Beth-dagon, Naamah, and Makkedah: sixteen cities

<sup>168</sup> See *e.g.* Gordon 1986: 265. It is fair to note that Simons (1959: §776) was sceptical of these emendations.

<sup>169</sup> Kitchen 1999: 160.

<sup>170</sup> Aharoni 1979: 258, 283, n. 194.

<sup>171</sup> Aharoni 1979: 258.

<sup>172</sup> See conveniently Aharoni 1979: 389–390, *Map* 32.

with their villages. Libnah, Ether, Ashan, Iphtah, Ashnah, Nezib, Keilah, Achzib, and Mareshah: nine cities with their villages.” Since a Beth-dagon and a Libnah are included together here in city lists of the Shephelah,<sup>173</sup> these would be logical locations for these toponyms, especially if we have correctly identified the surrounding entries, with the preceding as the two Beth-horons. NB, Joshua 10:28 places the Israelite capture of Libnah just after the battle at Beth-horon and the capture of Makkedah.

### 72. *b-y-t d-q-n*

Not known from any other Egyptian toponym lists, including that of Ramesses II, but obviously a Beth Dagon (“house”/“temple” of Dagon): “...as all commentators have recognised.”<sup>174</sup> From the southern context preferred here, this could either be the one mentioned by Sennacherib south of Ashkelon or the one mentioned in Joshua as near Libnah (see entry 71 above).

### 73. *q-r-b-q*

Presently unintelligible.

### 74. *k-r-m-y-m*

Simons compares to Thutmose III, List I:96, *krmn*. Jirku read as *krmm*.<sup>175</sup> The name is generally read as “Carmel” (with Egyptian *n* for *l*) but this was a common toponym. Thutmose I:49 *krymn* is likely to be near the Mount Carmel headland as the previous entries are Acco (I:47) and Rosh-Qadesh, “the Holy Head” (I:48).<sup>176</sup> As for Thutmose I:96, this must have been in a different location from I:49. Petrie argued from his

<sup>173</sup> See Simons 1959: §318.

<sup>174</sup> Edgerton and Wilson 1936: 109; cf. Kitchen 2008: 74.

<sup>175</sup> Simons 1937: 118; Jirku 1937: 45.

<sup>176</sup> Simons 1937: 117. See Aharoni 1979: 66, 149; Aḥituv 1984: 124.

understanding of the context that it should be identified with the Arab village of Kurmul 7 m south of Hebron.<sup>177</sup> Carmel is listed shortly after Hebron in Joshua 15:55 as an attendant village.<sup>178</sup>

### 75. *š-b-d-n*

Simons: “Not improbably = *š-b-t-n* [Thutmose III] I:73, a, c).”<sup>179</sup> Otherwise no similar names are known from toponym lists. Breasted assumed that this is the same as the Shabtuna on the Orontes,<sup>180</sup> just south of Qadesh in Syria.<sup>181</sup> However, it should be noted that the orthography is different (*d* instead of *t*) and that the general context here does not favour a Syrian location. (*i.e.* no confirmed Syrian toponyms in preceding or succeeding entries). Even if this is the same as Thutmose III List I:73 it is not clear that the latter is Shabtuna in Syria as assumed by Aharoni and others). On his map of Thutmose III’s campaigns, Aharoni cautiously includes a question mark next to his location of a Shabtuna south of Qadesh.<sup>182</sup> His caution may have derived from the fact that it does not appear in a clearly Syrian context in the list. Indeed, the last identifiable preceding entry is 70, which Aharoni located at Jett in Sharon.<sup>183</sup> Petrie offered a southern alternative for the *š-b-t-n* of Thutmose III, *i.e.* Shebtin, 9 miles east of Ludd.<sup>184</sup> We thus arrive at a plausible set of identifications, all in southern Palestine, with a rough north to south order.

<sup>177</sup> Petrie 1896: 329.

<sup>178</sup> Also, Samuel 25: 2, ff. The importance of this Carmel in the 10<sup>th</sup> century BC is shown by the story of David and his future wife Abigail; she was the widow of Nabal, clearly a rich landowner based at Carmel, where he was said to have kept a thousand goats and three thousand sheep for shearing. For its location see Simons 1959: 149 and Aharoni 1979: 288, Map 20.

<sup>179</sup> Simons 1937: 168.

<sup>180</sup> *ARE* IV: 131.

<sup>181</sup> Gardiner 1960: 59.

<sup>182</sup> Aharoni 1979: 155, Map 9.

<sup>183</sup> Aharoni 1979: 161.

<sup>184</sup> Petrie 1896: 327 and 324, Fig.163.



To continue with the next sections of Ramesses III's list, which are paralleled in that of Ramesses II:

**76.** *m-š-k-t-(š?)-n-r*

Very hard to interpret—as Simons noted “All signs extremely crowded in this name-ring and some slightly damaged.”<sup>185</sup> Undeterred, Kitchen offers a completely manufactured place-name “Mashkat-Senir” which he locates near Kefr Meshke at the west foot of the Hermus range.<sup>186</sup>

**77.** *ḥ-b-r*

While *ḥ-b-r* is missing in the Ramesses II version, it is reasonably assumed that it should be restored in space No. 26. True to style, for this Kitchen suggests an unlocated and totally unattested place-name “Khibur.”<sup>187</sup> From the context (see above and below) it should seem obvious that Hebron is meant. Surprisingly such an identification has been ignored in recent years—though it was obvious to an earlier generation of scholars such as Sayce, who described it as “long since been recognised.”<sup>188</sup> Krahmalkov remarked that “...the name Hebron is spelled without final -n in the Egyptian according to a common convention in the transliteration of Semitic place-names in Egyptian.”<sup>189</sup> The possibility that such a historic centre should be mentioned somewhere in Egyptian documents is hardly surprising.

**78.** *ynmw*

For this toponym there is some controlling evidence. A similarly spelt place-name heads a short toponym list of Seti I.<sup>190</sup> The name recalls the

<sup>185</sup> Simons 1937: 168.

<sup>186</sup> Kitchen 1999: 70; cf. Aḥituv 1984: 139.

<sup>187</sup> Kitchen 1999: 70, 160.

<sup>188</sup> Sayce 1892: 39.

<sup>189</sup> Krahmalkov 1994: 62, n. 15.

<sup>190</sup> Simons 1937: 146, XVIa/1.

well-known Yenoam, but the orthography is different. Jirku restored it as *yn(')m*,<sup>191</sup> though Edgerton and Wilson were less sure.<sup>192</sup> Kitchen rejects the identification as he believes this part of the list should be located in the Lebanon region: hence “Yeno‘am of Galilee would seem out of place here.”<sup>193</sup> A stronger objection remains the spelling. Noth preferred not to restore the ayin, reading it as “Yanum” and identifying it with modern Yunim, 10 km northeast of Baalbek; while Kuschke took it as modern Yammuneh, 20 km northwest of Baalbek.<sup>194</sup> A better match for *ynmw* which requires no amendment was suggested by Krahmalkov—Janim or Janum (Josh. 15: 53), a city in the hill-country of Judah, near Tappuah (west of Hebron).<sup>195</sup>

### 79. *drbn*

Simons notes that Daressy and Müller read the last group as *tw*,<sup>196</sup> likewise Jirku gives *Drbnt*,<sup>197</sup> which might suggest a possible corruption of Daberath (a place-name in Issachar). However, Edgerton and Wilson note “Final sign 3, not *tw*,” leaving *drbn* as the most likely reading.<sup>198</sup> Kitchen reads “Durbana,” citing Helck’s suggestion of Tarbul in the Beqa‘, 35 km north of Kefr Meshke.<sup>199</sup> Krahmalkov argued that the name “could be a simple misspelling of Dibon, a city in Judah mentioned with Hebron in Nehemiah 11:25. Spelling errors of this kind (a false r) are common in the Medinet Habu topographical list of Ramesses III.”<sup>200</sup> Alternatively, might this not be a defective spelling of Debir, a major city of southern Judah? In Joshua 15:48 Debir occurs in the group immediately to the west of Hebron.

<sup>191</sup> Jirku 1937: 45.

<sup>192</sup> Simons 1937: 146, XVIa/1; Edgerton and Wilson 1936: 109–110.

<sup>193</sup> Kitchen 1999: 70

<sup>194</sup> References in Kitchen 1999: 70.

<sup>195</sup> Krahmalkov 1994: 61

<sup>196</sup> Simons 1937: 168.

<sup>197</sup> Jirku 1937: 45.

<sup>198</sup> Edgerton and Wilson 1936: 110.

<sup>199</sup> Kitchen 1999: 70.

<sup>200</sup> Krahmalkov 1994: 62, n. 15

**80. *ì-p-q***

Obviously an Apheq as all agree. Kitchen cites Noth in identifying it with “the ‘obvious’ Apheq, at Afqa close to the sources of the Nahr Ibrahim, at the springs of Adonis...”<sup>201</sup> There is nothing “obvious” in the matter when there are good candidates much further south. In the time of Samuel (1 Sam. 4:1; 1 Sam. 29:1) Apheq NW of Jerusalem in the northern Sharon was evidently a Philistine stronghold. However, as suggested long ago by Sayce this could be the Judahite Apheq(ah) (Joshua 15:53), described as being near Hebron.<sup>202</sup> Given that this site belongs to an apparent “Hebronite” cluster (see above), this Apheq(ah) is preferred.

**81. *ì-b-h-y***

Kitchen reads as “Abkhiya,” with no comment on location.<sup>203</sup> At present this name seems impenetrable.

**82. *m-k-t-r***

Reading uncertain as the spelling is actually *mktr* rather than *mktr* as often transcribed. Hence Kitchen reads as an unidentifiable “Maks/zir.” Strictly speaking he is correct, though of course the possibility remains that the Egyptian artists made a slip. The name has been read by all other scholars as a variant spelling of *mktr* and hence simply “Migdol” or “Migdāl,” the common West Semitic term for a fortress.<sup>204</sup> In this context “Migdol” could have been Migdal-gad of Judah (Joshua 15:37), as suggested by Sayce,<sup>205</sup> or (less likely) the Migdal-Eder near Bethlehem (Genesis 35:21).

<sup>201</sup> Kitchen 1999: 70; cf. Ahituv 1984: 62.

<sup>202</sup> Sayce 1892: 39; 1912, 206.

<sup>203</sup> Kitchen 1999: 70

<sup>204</sup> Simons 1937: 168; Jirku 1937: 45; Edgerton and Wilson 1936: 110; Seguin 2007: 42; Burke 2007: 33, 35.

<sup>205</sup> Sayce 1892: 39.

### 83. *q-r-t-k*

Here the Ramesses III version *q-r-t-k* is a puzzle, not solved by Kitchen's random (and unknown) "Qurzik(k)a."<sup>206</sup> If we follow the Ramesses II spelling [*q-r*]-*t*, then a much simpler reading, "Kerioth/Keriath" is more intelligible. There was a Keriath-Hezron in the far south of the territory of Judah (Joshua 15:25) but a more likely candidate may be Keriath-jearim to the west of Jerusalem in the Shephelah. Joshua 15:25 states that it was also known as Keriath-Baal, 1 Chron. 13:6 as Baalah. It was evidently an important site, presumably once the centre of a cult of Baal, and the place where the Ark of the Covenant resided for a while until King David moved it to Jerusalem (2 Sam. 6: 2–4).<sup>207</sup>

So far, the results of the northern school of thought (as exemplified in Kitchen's analysis) with regard to the above place-names are far from impressive. Not a *single* one relies on an ancient place-name. For four (77, 81, 82, 83) there are no reasonable identifications suggested at all, while such associations of *m-š-k-t-(š?)-n-r* (76) with Syrian Kefr Meshke are a long shot to say the least. In most cases Kitchen's readings seem to have been made hastily—though hardly surprising given the more than herculean task of transcribing, translating, editing and adding commentary to the vast corpus of Ramesside texts. It is not meant as a criticism of Kitchen's extraordinary feat to challenge some of his identifications. Does a "southernist" approach here make better sense of the above two clusters? Names like "Apheq," "Migdol" and "Kerioth," especially when they occur together, and in the company of a probable "Hebron" (*h-b-r*) are redolent of southern Palestine, indeed the area of the tribe of Judah. As Krahmalkov remarked, the grouping of *h-b-r* (77), *ynmw* (78) and *ipq* (80) forms a striking parallel to the grouping of a Janim and Apheqah with Hebron in Joshua 15:52–54, describing one the regions allotted the tribe of Judah: "Arab, Dumah, Eshan, Janim, Beth-tappuah, Apheqah, Humtah, Kiriath-arba (that is,

<sup>206</sup> Kitchen 1999:70.

<sup>207</sup> Aharoni 1979, 249, Map 18. Miller and Hayes 1986: 127 and 131, Map 12.

Hebron), and Zior: nine cities with their villages.” The grouping, based on an ancient source, provides a context completely missing from the northernist school which is largely based on modern Arab place-names.<sup>208</sup>

To summarise the identifications suggested above:

<u>Ramesses III, XXVII</u>	<u>Suggested Location</u>
70. <i>h-r-n-m</i>	*Horonaim, the two Beth Horons
71. <i>r-b-n-t</i>	Libnah
72. <i>b-y-t d-q-n</i>	Beth Dagon
73. <i>q-r-b-q</i>	unknown
74. <i>k-r-m-y-m</i>	Carmel in Judah
75. <i>š-b-d-n</i>	Shebtin in Judah
76. <i>m-š-k-t-(š?)-n-r</i>	unknown
77. <i>h-b-r</i>	Hebron
78. <i>ynmw</i>	Janim
79. <i>drbn</i>	Debir
80. <i>ìpq</i>	Apheqah
81. <i>ì-b-h-y</i>	unknown
82. <i>mktr</i>	Migdol-gad

The many plausible matches strongly support a “southern” school of interpretation for this part of the list. To continue with this line of investigation, we continue with the next two clusters in Ramesses III’s Great List.

#### 84. *q-t-m-n*

In the Ramesses II version (20) this is given as *q-r-m-n*, but the Ramesses III *q-t-m-n* may be more accurate. The “Song of Deborah”

<sup>208</sup> Krahmalkov 1994: 61. It might be objected that the case is weighted against in favour of the southern locations as there is nothing like the rich biblical onomasticon of place-names in the north. However, the Old Testament does, after all, mention many places in the Lebanon and Syria, while the rich onomastica from Ugarit, Alalakh and Emar surely balance the picture.

(Judges 5:21) refers to: “That ancient river, the river Qishon.” Following the Hebrew text more literally (as does Krahmalkov), one can read “The River Qishon is the River Qedumim (קְדוּמִים)” *i.e.* “the ancient river” with Qedumim as a second name for the river. Krahmalkov argues that the biblical text here needs correction: “...the Hebrew Qedumin has d for the original r, a very common spelling error...”<sup>209</sup> It sits uncomfortably to correct the biblical text by use of an uncertain Egyptian toponym. If we use the Ramesses III version, then *q-t-m-n* could = biblical Qedumin without problem (*d* and *t* are common equivalents in Egyptian and Asiatic texts). Alternatively, and to follow the Ramesses II spelling *q-r-m-n*, this could be a place in the vicinity of Mt Carmel<sup>210</sup> If the following entry does concern the river Qishon this would fit well as the river debouches into the sea near Mt Carmel.<sup>211</sup>

### 85. [q]-ś-t-b-r-n

Kitchen transcribed the Ramesses III version as “<Q>aw<s>{h}i<r>-Y’a {ti} bur,”<sup>212</sup> which seems unintelligible. It is agreed that the Ramesses II version no. **21** is less corrupt. Edgerton and Wilson read this as *q-ś-r-Y-b-n*, with Kitchen in agreement rationalising this as “Qasir-Yubana.”<sup>213</sup> As the Egyptian use of *r* for *n* in Asiatic place-names is well known, the first element could well be read as Qishon, a town to the northwest of the nearby homonymous (**84** above) river. It was reckoned as a “Levitical” city with “its suburbs” in the tribal area of Issachar (Joshua 19:21).<sup>214</sup> Regarding the second element,

<sup>209</sup> Krahmalkov 1994: 79.

<sup>210</sup> Ahituv 1984: 124.

<sup>211</sup> In his study of Deborah’s wars, focussing on the differences between the prose and poetical versions, Gass (2017: esp. 328–331) discusses the biblical references to the Wadi Qishon, usually identified with the *Nahr el-Muqatta’* which drains the Jezreel valley westwards into the Mediterranean. Gass postulates a second Qishon in the eastern Jezreel.

<sup>212</sup> Kitchen 2008: 74.

<sup>213</sup> Edgerton and Wilson 1936: 110; Kitchen 1999: 70.

<sup>214</sup> Simons 1959: 185; Aharoni 1979: 303, Maps 9, 22, 23.

Krahmalkov suggested “Qishon of Jabin.”<sup>215</sup> Judges 4–5 relates in some detail the nearby defeat by Deborah and the Israelites of Sisera the general of Jabin, “king of Canaan.” Krahmalkov argued that Qishon was where the latter assembled his troops, explaining how the town was named after him. But it seems fairly clear that the Qishon involved was the river, while Jabin’s capital is specified as Hazor.

Perhaps a simpler reading might be arrived at. The alleged massacre of Jabin’s troops at the river is mentioned so many times in the Old Testament (Judges 4:7; 4:13; 5:21; 1 Sam. 12:9; Psalms 83:9) that it became iconic. The last reference calls for damnation on Israel’s enemies (“Sisera, as to Jabin, at the brook of Qishon”) includes “Assur” illustrates a late date for the composition. (See also 1 Kings 18:40 where the site of Elijah’s slaughter of the prophets of Baal is emphasised as being the Qishon.) The idea that Qishon should be named by tradition after a powerful king who was defeated there is not out of court. Though similar examples are rare, the name of the British general, Lord Cornwallis, whose career in the American War of Independence went from victories to eventual defeat (in 1781), was adopted by a surprising number of places in the present USA.<sup>216</sup>

Some names in Egyptian toponyms can be read jointly. If we read toponym **84** as “the ancient” together with **85** we would have the “ancient [river] of Jabin.” In other words, “our old river where Jabin’s army was drowned.” This is highly speculative so I naturally welcome better readings of these difficult entries.

<sup>215</sup> Krahmalkov 1994: 61. Without reference to Krahmalkov, Tobola (2013) agrees with the reading “Jabin” here. Tobola’s poorly referenced article goes far beyond this with some completely fanciful readings, arguing that the difference between the Ramesses II and Ramesses III versions reflects a change in the toponym from a meaning of “war-band” of Jabin to “war-band of Deborah”! As well as requiring much restoration to the Ramesses III version of this name, Tobola’s model requires too much reliance on a chronological framework based on the notoriously difficult Judges period. Admittedly I have experimented here with the name Jabin as a component of this entry, with the caveat that this remains highly speculative.

<sup>216</sup> Martin 2007.

**86. š-m-š-n**

It is fascinating to see the name of the great Hebrew hero (actually Danite) Samson in an Egyptian text, albeit within a toponym. His legendary escapades make him comparable to the English Robin Hood, while his name is derived from that of the West Semitic Sun-god Shemesh. Hence presumably a place named after the god or the hero. Kitchen reads “Shamshana,” but in terms of location offered little, except reference to Helck’s suggestion “Bshemshin, 13 km/8 miles ENE of el-Batrun.”<sup>217</sup> Again, the new “northernist” approach here relies on random choices of place-name in Lebanon/northern Syria. Sayce suggested Ir-Shemesh (Joshua 19:41) which he understood as “the city of the Sun-god.”<sup>218</sup> This lay within the lands allotted to the tribe of Dan.<sup>219</sup> Otherwise, there were also at least two towns named Beth-Shemesh within Israel. Within Solomon’s listing of twelve regional officers, a Beth-Shemesh is listed as under the control of one Ben-Deker (1 Kings 4:9). It was apparently in Danite territory, close to both the borders of Judah and Philistia. It may well be the same as the above mentioned Ir-Shemesh. There were sites with same name further north but still within the bounds of Israel. A Beth-Shemesh is listed together with Beth-Anath as a town claimed by the tribe of Naphtali (Judges 1:33; Joshua 19:38). The biblical evidence is confusing, as the tribe of Issachar also laid claim to a Beth-Shemesh (Josh. 19:22) which was not too far away—in the area of the Jezreel valley. Aharoni locates it near Qishon.<sup>220</sup> For the location in Issachar, Khirbet Shemsin, south of Lake Tiberias has been suggested.<sup>221</sup> Despite the temptation of a location in Danite territory, a site in the Jezreel valley may be more plausible, given the context of the guesstimates made for the previous entries (**84** and **85**). Khirbet Shemsin would be a good candidate as it would preserve the *n* of the original.

<sup>217</sup> Kitchen 1999: 70; 2008: 74.

<sup>218</sup> Sayce 1912: 205.

<sup>219</sup> For references see Simons 1959: 200, 203.

<sup>220</sup> Aharoni 1979: 308, Map 23.

<sup>221</sup> See Aḥituv 1984: 175–176; Tobola 2013: 9 and n. 21.



**87.** *ḥ-d-ś-t*

Krahmalkov suggested that the name is preserved in the Arabic Ayn al-Hadatha, located near Qishon,<sup>222</sup> though this fails to have any biblical or ancient confirmation. More accurately Kitchen reads “Hadasat/|Hadisat,” though following Helck he locates it in the northern Lebanon near Tripoli.<sup>223</sup> Easily read as “new (town)” as in Qarthadast (Carthage). Could be also be Hadashah, a town in the lowland of Judah named between Zenan and Migdal-gad mentioned in Josh. 15:37—as suggested by Sayce.<sup>224</sup> The best alternative is that it should be read as a “compound” entry together with **88**.

**88.** *ì-r-t*

The Ramesses II equivalent gives this as *i-t-r*. Unlikely to be Ether, a Judahite town near Libnah known from Joshua 15:42, as Egyptian *t* normally corresponds to West Semitic *s* or *z*. Hence Kitchen reads “Asir,” tentatively placing in the Tripoli region.<sup>225</sup> Elsewhere he prefers the Ramesses II version “Aris (*for* Asir).”<sup>226</sup> If the Ramesses III orthography is more correct, as Sayce argued it could reflect Hebrew *eretz* (land).<sup>227</sup> Together with the previous entry (**87**) one could read “new land,” which could be anywhere.

<sup>222</sup> Krahmalkov 1994: 62.

<sup>223</sup> Kitchen 2008: 74.

<sup>224</sup> Sayce 1892: 39.

<sup>225</sup> Kitchen 1999: 70.

<sup>226</sup> Kitchen 2008: 74.

<sup>227</sup> Sayce 1892: 39.

### 89. *q-ś-n-r-m*

More fully *q-w-ś-n-r-m*.<sup>228</sup> One of the most controversial entries. Sayce<sup>229</sup> rightly dismissed the readings of earlier scholars, which were made before sufficient clearing of the Medinet Habu had been made. With the help of Wilbour he read **89** and **90** in the Ramesses II version as Qa-n-Salem, meaning “district of Salem,” and “Qal-p(a)a(na), with similar readings for the Ramesses III versions (see **Fig. 4** below).<sup>230</sup> It seems equally possible to read the *n* before the *ś*,<sup>231</sup> which raises the intriguing possibility that this is a reference to Jerusalem. The readings apart, that **89** and **90** were in southern Canaan is suggested by the fact that in both the Ramesside versions the names appear in the same cluster as *i-r-d-n*, which despite Kitchen and others must surely be the Jordan. Kitchen reads as Qausan-rom,<sup>232</sup> which he describes as

<sup>228</sup> Some scholars (e.g. Oded 1971: 47; Lipiński 2006: 364) have read the name of the Edomite god Qos in this and other Ramesside toponyms. The worship of Qos is well attested from Edomite royal names and ostraca of the 8<sup>th</sup> century BC onwards, but otherwise unknown earlier. Kelley (2009, 257, n. 8) provides handy lists of the relevant names from the lists of Ramesses II “*q-ś-r-a*, *q-(m?)-ś-p-t*, *q-ś-n-r-m*, *q-ś-r y-b-n*, and perhaps *q-t-i-ś-r* (lines 7, 11, 13, 21, 8)” and Ramesses III “[*q*]-*ś-t-b-r-n*, *q-ś-n-r-m*, *q-ś-[b?]-p-t*, *q-ś-r-a*, *q-ś-t-i-ś-r* (lines 85, 89, 100, 102, 103).” As references to Qos, Dearman (1995: 123) notes these as plausible but problematic. Indeed, they are. There are different readings in the two parallel lists for all the alleged “Qos” names (except for Ramesses II 7/Ramesses III 102). RII 11/RIII 100 is particularly difficult to read and the *q-ś* element occurs only in the Ramesses III version. For RII 8/RIII 103, while the reading of the first element as “Gath” has been ruled out, the second part may still be Asher (see below), which if it is the northern Israelite tribe (see 103 below) would make a Qos theophoric element unlikely. RII 21/RIII 85 can be reasonably read as beginning “Qishon” (see above) and appears to belong to a Jezreel Valley cluster, again making a Qos element unlikely. This leaves the *q-w-ś-n-r-m* in question. While it cannot be ruled out entirely that it begins with a Qos element, I prefer the reading offered here: it seems to belong to a pairing with the following entry 90, which also begins with “Qau” but lacks the *ś*. NB Bartlett, Kitchen and Aḥituv (nor for that matter, Simons and Edgerton and Wilson) have accepted any of these as names as reflecting the worship of Qos in the Late Bronze Age.

<sup>229</sup> Sayce 1892: 19–20, 25.

<sup>230</sup> Müller examined these names in 1904. With respect to the Ramesses II version, and despite his best efforts he remained uncertain about the reading of “strange names” such as *Qau-sa-na-ru-ma* (Müller 1906: 47).

<sup>231</sup> Pers. comm. Robert Morkot.

<sup>232</sup> Kitchen 2008: 74.

“obscure,” though he thinks the second element may be Semitic “high.”<sup>233</sup> It is odd that Egyptologists have missed reading the first element as “hill” or “height”<sup>234</sup> with *n* meaning “to” or “belonging to.”<sup>235</sup> This would give us “the height of *s-r-m*” or “the height of Salem” if one follows the Wilbour/Sayce reading.

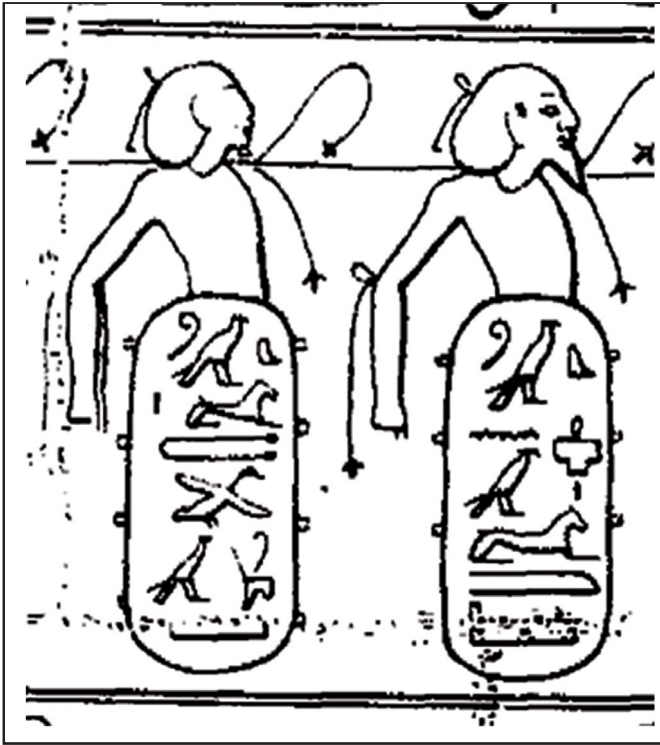


Fig. 4. Captives 89–90 (r to l) from Ramesses III’s “Great Asiatic List.” From *MH II*, Pl. 101. © University of Chicago Press 1930.

<sup>233</sup> Kitchen 1999: 68.

<sup>234</sup> See Gardiner 1957: 27, 489, 596, 615). The dictionaries compiled by other eminent philologists, ranging from early to modern, give many instances of *qau* (sometimes with acceptable, small variations in orthography) with the meaning “height” or “hill.” See Budge 1920: 760–762; Faulkner 1962: 275; Erman and Grapow 1971: 4; Hannig 2001: 847.

<sup>235</sup> Gardiner 1957: 80, 88, 127.

### 90. *q-r-t-p-n*

More fully *q-w-r-t-p-n*. Kitchen reads as Qarutpana.<sup>236</sup> The Ramesses II version lacks the third character *t* and is read by Kitchen as Qarupana, with the unlikely suggestion that it may reflect the Qalpanu in northern Syria known from the records of Assurnasirpal II.<sup>237</sup> If the Ramesses III version is more correct orthographically we can read it as “the height of *t-p-n*,” with the *r* standing for *n* (common in Egyptian versions of Semitic names). The second element *t-p-n* could well be Şaphon, an excellent suggestion made by Peter van der Veen<sup>238</sup> Şaphon was a common West Semitic name for mountains. One was located in Transjordan (Judges 12:1), also the name of a clan within the tribe of Gad (Numbers 26:15).<sup>239</sup> Albright and Aharoni located it at Tell es-Sa‘ideyeh,<sup>240</sup> though Zarethan has also been offered as a name for this site.<sup>241</sup> Whatever the case, Şaphon must have lain near Tell es-Sa‘ideyeh where there was a major bronze-smelting industry at the cusp of the LBA/IA, associated with Egyptianising objects. The excavators date these to the 20<sup>th</sup> Dynasty<sup>242</sup> though a late Ramesside date would be more cautious.

### 91. *i-r-d-n*

Obviously, the Jordan, as noted above. See above (esp. n. 109) for Kitchen’s denial that such references concern the Jordan.

<sup>236</sup> Kitchen 2008: 74.

<sup>237</sup> Kitchen 1999: 68.

<sup>238</sup> Pers. comm. Peter van der Veen.

<sup>239</sup> One cannot compare the alleged reference to Zaphon in the list of Shoshenq I, which has been discredited by Kitchen (1986: 299, n. 300): “As II:20 is totally lost, any identification with (as with Zaphon by Mazar and Aharoni) can only be purest guesswork.”

<sup>240</sup> Albright 1924–1925: 45–47; Aharoni 1979: 443, 308, Map 23.

<sup>241</sup> See Tubb 1993: 1295. For a discussion of the complexities of site identification in this area of Jordan see Schaaf (2012: 144–147).

<sup>242</sup> Tubb 1993: 1299.

## 92. *h-r-t*

Kitchen reads as “Khilsa” in the Lebanon as per the other entries in this cluster<sup>243</sup>—not worth further scrutiny as they are all second-hand suggestions which fit Kitchen’s stubborn reluctance to accept that *i-r-d-n* (91) was the Jordan. The name may well reflect the common NW Semitic term *hrt*, meaning “camp” or “cultivated land.”<sup>244</sup> King David was said to have gone to a forest called Hareth in Judah (apparently near Adullam) when he left Mizpah of Moab (1 Sam. 22:5). However, we appear from the next few entries to have crossed the river. Hence perhaps the important Moabite fortress of Kir-heres or Kir-Haraseth<sup>245</sup> (Jeremiah 48:31; 2 Kings 3:25) is a good candidate. Once generally thought to be an alternative name for Kerak (see 99 below),<sup>246</sup> Miller expressed some doubt,<sup>247</sup> and the detailed of Jones study has demonstrated that this common identification is mistaken.<sup>248</sup>

## 93. *q-r-h*

As suggested by Sayce and Krahmalkov, easily Qarho<sup>249</sup>—either an alternative name for Dibon, a Moabite capital, or its citadel, as known from the Mesha inscription.<sup>250</sup> This reassures us that this cluster has taken us across the Jordan.

<sup>243</sup> Kitchen 1999: 69; 2008: 74.

<sup>244</sup> For examples and discussion see Tebes 2017: esp. 74–75.

<sup>245</sup> A suggestion also made by Krahmalkov (1994: 58). In one biblical reading this Heres was the site of a battle between Gideon and two Midianite kings (Judges 8:13).

<sup>246</sup> See e.g. Aharoni 1979: 340, 348; Krahmalkov 1994: 58.

<sup>247</sup> Miller 1989: 35–36.

<sup>248</sup> Jones 1991; with Miller (1992: 85–86) in agreement.

<sup>249</sup> Sayce 1892: 26; Krahmalkov 1994: 58.

<sup>250</sup> Lipiński 2006: 333–337.

**94. *w-r-w***

Sayce suggested the possibility of Ar, a Moabite city known from Numbers (21:15; 28).<sup>251</sup> Presumably realising that Egyptian *w* makes an unlikely match with Hebrew ayin, he suggested that Ar (meaning “city” in Hebrew) was also known by its Babylonian equivalent *uru*. It is difficult, however, to conceive such a route of transmission to the record of Ramesses III.

**95-97. “African”**

The next cluster is particularly difficult to interpret and seems to be rather eclectic—though the first two entries comfortably continue the Transjordanian entries above.

**98. *i-k-ś***

The Ramesses II version (18) *i-k-t* seems more intelligible.<sup>253</sup> Krahmalkov reasonably suggested (Tel) Iktanu in Transjordan.<sup>253</sup>

**99. *k-r-k***

Easily Kerak in southern Moab.<sup>254</sup>

**100. *q-ś-[b?]-p-t***

Ramesses II version (11) *q-(m)-ś-p-t*, which Kitchen reads as Qamsapuya and as “unknown.”<sup>255</sup> Agreed.

<sup>251</sup> Sayce 1892: 33; 1912: 204.

<sup>252</sup> Edgerton and Wilson 1936: 110.

<sup>253</sup> Krahmalkov 1994: 56, 58.

<sup>254</sup> For its location and history see Bromily 1986: 41–42.

<sup>255</sup> Kitchen 1999: 68.

**101. *ì-t-r***

Krahmalkov (see above) posited an Athar from the “way of Atharim” (Numbers 21.1) in the Negev (see above). Joshua 15:42 refers to an Ether near Libnah, possibly the same as that referred to in Joshua 19:7, assigned to the southern tribe of Simeon.<sup>256</sup>

**102. *q-ś-r-c***

Kitchen: “Qausara.”<sup>257</sup> With regard to the identically spelt Ramesses II version (7) Kitchen remarked “no good suggestion has been made.”<sup>258</sup> Agreed.

**103. *q-s-t-ì-ś-r***

Kitchen: “Qaus- $\{sa>hi\}$ asir.”<sup>259</sup> The Ramesses II version (8) *q-t-ì-ś-r* has raised problems as it has been read as “Gath-Asher” and identified with Jett in western Galilee.<sup>260</sup> The alleged “Gath” element lacks the *n* usual in Egyptian spellings.<sup>261</sup> Accordingly, Kitchen reads it as “Qaws/z-Asir, noting that it “unlocated at present.”<sup>262</sup> Agreed. The *ì-ś-r* element remains interesting as it also occurs in a toponym list of Seti I<sup>263</sup> and the “Letter of the Satirical Scribe.” It is commonly, though not universally, identified with the biblical tribe of Asher,<sup>264</sup> who were situated close to Phoenicia I. I hope to address this question elsewhere.

<sup>256</sup> See Aharoni 1979: 261, 353, 434.

<sup>257</sup> Kitchen 2008: 74.

<sup>258</sup> Kitchen 1999: 67.

<sup>259</sup> Kitchen 1999: 74.

<sup>260</sup> Aharoni 1979: 181, 193.

<sup>261</sup> See Ahituv 1984: 157.

<sup>262</sup> Kitchen 1999: 67.

<sup>263</sup> Simons 1937: 147.

<sup>264</sup> Gardiner 1911: 25\*, n. 12; e.g. Aharoni 1979, 183.

**104.** *y<sup>c</sup>-{q} b-r*

Restorable from Ramesses II **9**, *y<sup>c</sup>-q-b-r*. With characteristic awkwardness, the best Kitchen can offer is “Ya‘qubr/lu,” identifying it with an obscure place “Meqa Bera” (!) near Krak de Chevaliers in Syria.<sup>265</sup> It has traditionally been read as Jacob-el.<sup>266</sup> Beth-el (assigned to the tribe of Benjamin), where Jacob is said to have erected a pillar or altar (Genesis 28:19; 35:7) is a very likely candidate.

**105.** *r-h-d*

Kitchen reads as Ruhizzi, which at first glance would appear to be the Ruhizzi of the El Amarna letters which from context was certainly in southern Syria.<sup>267</sup> However, Kitchen has pointed out orthographic problems in the identification with cuneiform Ruhizzi, concluding that there were two similar names: one “the real Ruhizzi” in the region of Syrian Qadesh and *r-h-d* “which remains unlocated geographically.”<sup>268</sup> Luhith in southern Moab (Isaiah 15:5; Jeremiah 48:5) near the southern end of the Dead Sea is a tempting candidate but there may be a philological difficulty with the last radical, in that Egyptian *d* does not seem to have equivalents in Hebrew *tau*.

**106.** *s-i-b*

Ramesses II (**5**) *s-i-b-t*, which Kitchen notes appears as a city attacked by the Pharaoh in his war reliefs, in a series for which he notes that “most suggestions are pure guesswork.”<sup>269</sup> It appears in the same scene as a town labelled “Ikata.” If the latter is the same as *i-k-t*, Ramesses II **18**, which appears to have been in Transjordan, then the same may

<sup>265</sup> Kitchen 1999: 68.

<sup>266</sup> Sayce 1892: 27, 40; Edgerton and Wilson 1936: 110 n.\*; Aḥituv 1984: 200; and Knohl 2017 for more recent bibliography.

<sup>267</sup> Kitchen 2008: 74; El Amarna letters 53:36, 56 (Moran: 1992: 125).

<sup>268</sup> Kitchen 1999: 67.

<sup>269</sup> Kitchen 1999: 64.



apply to Sab'ita. The name of the Byzantine town of Shivta (Arabic A-Sbaita) in the Negev<sup>270</sup> might possibly reflect *ś-ì-b-t*.

### 107. *k-t-(i)?*

Kitchen reads as Kazi'i or Kas'i, repeating a suggestion from Helck that this refers to Kezz near the Lake of Homs in Syria.<sup>271</sup> Sayce suggested Gaza.<sup>272</sup> Gath remains another possibility though it lacks the usual *n* known from other Egyptian references. Orthographically *k-t-(i)?* does not provide a good match to either Gaza or Gath<sup>273</sup> It may be a defective spelling for either, or another place entirely.

### 108. *r-š q-d-š*

Kitchen: "Rosh-Qadesh,"<sup>274</sup> *i.e.* "Holy-head(land)." The standard identification is with the promontory of Mount Carmel as it occurs in the major toponym list of Thutmose III (I:48) between Acco (I:47) and Carmel (I:49).<sup>275</sup> (Re the latter and its difference from another Carmel, Thutmose III, I:96, see Ramesses III 74 above). Kitchen rightly raises a question over the usual location of Rosh-Qadesh, pointing out from the order of the Thutmose list that it "could in principle have been any prominent headland along the Mediterranean coast."<sup>276</sup> As alternatives he suggests two sites further north: Ras an-Naqura between Acco and Tyre and Ras esh-Shaqqa some 7 km to the north of Phoenician Batruna.

<sup>270</sup> Segal 1985; my thanks to Juan Manuel Tebes for drawing this possibility to my attention.

<sup>271</sup> Kitchen 2008: 74; 1999: 67.

<sup>272</sup> Sayce 1892: 27, 41.

<sup>273</sup> For the standard Egyptian spellings see Aḥituv 1984: 95–98.

<sup>274</sup> Kitchen 2008: 74.

<sup>275</sup> See *e.g.* Sayce 1892: 26, 41; Aharoni 1979: 155, Map 9, 161; Aḥituv 1984: 162.

<sup>276</sup> Kitchen 1999: 66–67.

**109.** *y-n-d-t*

Otherwise unknown. Kitchen reads as “Yanşata,” with the remark that it “has excited almost no suggestions for its identification,”<sup>277</sup> except for the vague suggestion of Aḥituv “from context” that it may have been in the Beqa‘ valley which makes little sense, given he thinks that Rosh-Qadesh was Mount Carmel.<sup>278</sup>

**110.** *c-n-n-g-r*

Kitchen: “Ayn-Nagar.”<sup>279</sup> He notes the identification with modern ‘Ain ed-Djar/Andjarr in the Beqa‘ valley.<sup>280</sup> Kitchen might have added that the element ‘Ain certainly means “spring” or “well,”<sup>281</sup> while the fact that Andjarr was noted for its abundance of water<sup>282</sup> adds strong support to this identification.

While the locations of this cluster are hard to discern, it is better preserved than the Ramesses II version and is used to restore the latter.

**CONCLUDING REMARKS**

The aim of this study has been twofold. The first is to analyse in as much detail as possible a major section of Ramesses III’s “Great Asiatic List” to see what guidance it can provide regarding his claimed Levantine campaigns; the second to assess its contribution to the chronological understanding of Ramesses III. The conclusions offered for the two issues here can be judged separately.

With respect to attitudes towards Ramesses III’s Levantine campaigns, the extraordinary vicissitudes in the literature have been reviewed in some detail—ranging from early minimalist through (fair-

<sup>277</sup> Kitchen 2008: 74.

<sup>278</sup> Aḥituv 1984: 198.

<sup>279</sup> Kitchen 2008: 74.

<sup>280</sup> Kitchen 2008: 74; cf. Aḥituv 1984: 57.

<sup>281</sup> Hoch 1994: 59, 71–72.

<sup>282</sup> Dussaud 1927: §219.

ly moderate) maximalist approaches, to extreme minimalist and now back again to maximalist approaches. I hope to have shown that the extreme minimalist position (once favoured by Kitchen, Lesko and others) was palpably absurd. It is impossible to imagine how Ramesses III would have had any foreign domains to speak of if there were no military expeditions to the north. The domains ruled by the Egyptians needed periodic shows of force to ensure that tribute was collected. Fortunately, Kitchen and most other scholars have changed their minds and now approach a maximalist position.

Using the methodology pursued above, the “Great Asiatic List” 70–110 falls into six clusters which can be tentatively described as follows: a (largely) Shephelah group; a Hebron group; a Jezreel Valley group; a group on both sides of the River Jordan; further Transjordanian plus Negevite (?) names; and a Lebanese group. As the clusters are in a different order in Ramesses II’s version nothing should be drawn from this, except to say that the spread of toponyms includes southern Palestine as well as routes to the north (via the Beth Horon pass and the Jezreel Valley) which Ramesses III’s troops could have followed up with raids into Amurru and possibly further north.

Of particular interest is the first Ramesses III cluster 70–75. While there are similar names from the documents of earlier pharaohs this section is *particularly worthy of further attention as it appears to be an original composition*. Following the identifications offered here it appears to show an itinerary (in south to north order) of Shebtin (near Ludd), Carmel (southern Judah), Qrbq (?), Beth-Dagon (near Libnah), Libnah and the Beth Horons (Horonaim).

It would appear to reflect a route through the Shephelah region, up to the Beth Horon pass whence Egyptian armies, officials and traders could reach the Jezreel Valley and beyond. Such a route is predicated by the activities of Ramesses IV, evidently the last 20<sup>th</sup> dynasty ruler of Palestine beyond Gaza. As there is no firm evidence that he had direct control of Philistia or the Judaeian highlands, Ramesses IV must have had access to the Jezreel Valley—notably to Beth-Shean where he built or augmented a temple (see above). His records (though minimal) talk

of “rounding up Asiatics in their valleys,” presumably *en route* to Beth-Shean via a corridor through the Shephelah.<sup>283</sup> The same would have surely applied to his more powerful and ambitious father Ramesses III. Cluster 70–75 would seem to list stage-points along the way.

Archaeology supports this identification of a “Shephelah corridor” for the armies of the early 20<sup>th</sup> Dynasty. First, from the evidence of Egyptian inscriptions and despite arguments to the contrary, it seems likely that Stratum VI at Lachish was destroyed by Ramesses III.<sup>284</sup> Second, Faust has shown that settlement in the Shephelah underwent drastic decline at the LB-Iron I transition, with widespread destruction and abandonment<sup>285</sup>—an agreeable congruence between the literary (Ramesses III’s inscriptions) and archaeological records.

A separate, though related issue, are the repercussions of the geographical model proposed here for chronology. Some decades ago my colleagues and I argued, on a broad range of evidence from throughout the Near East, Eastern Mediterranean and North Africa, for a considerable lowering of the shaky chronology of the Egyptian “Third Intermediate Period.”<sup>286</sup> Many other Egyptologists, such as Aidan Dodson<sup>287</sup> have since made small revisions (and lowerings) to TIP chronology. The literature on this revision is extensive.<sup>288</sup>

The shortened TIP we proposed led to two surprising conclusions concerning links between Egyptian and biblical history. One was the lowering of Shoshenq I to the second half of the 9<sup>th</sup> century BC, an idea supported by specialists in fields other than Egyptology.<sup>289</sup> This

<sup>283</sup> van der Veen and James 2015.

<sup>284</sup> James and Kokkinos, in prep; Bimson 2015b: 111–112; cf. Zwickel 2012: 597–598.

<sup>285</sup> Faust 2913. For more detail see Bimson 2015b: 109–111.

<sup>286</sup> James *et al.* 1987; 1991a; 1991b; 1992, 127.

<sup>287</sup> Dodson 2012. See James 2017 for a bibliography of Dodson’s experiments with shortening TIP chronology.

<sup>288</sup> For a brief summary and a new “dead reckoning” backwards from the well established date of 690 BC (the accession of the 25<sup>th</sup> dynasty Taharqo and the *earliest certain date in Egyptian history*) to the start of the 22nd Dynasty see Morkot and James 2015. See also Morkot and James 2009.

<sup>289</sup> As early as 1983 (pp. 88–89) Wallenfels’ study of the Byblite inscriptions led him to suggest on epigraphical grounds that the Phoenician inscriptions on busts of Shoshenq I and Osorkon

would mean that he could no longer be the biblical “king Shishak” said to have cowed Solomon’s successor Rehoboam, seized his “fenced cities” and been paid off from attacking Jerusalem by the treasures from the Temple. The second proposal was that the reign of Ramesses III should date to the late 10<sup>th</sup> century BC and that he, not Shoshenq I, was the biblical Shishak.<sup>290</sup> It was this suggestion that provoked Kitchen’s knee-jerk reaction (see above) that this was impossible as Ramesses III campaigned no further north than Edom. We made it at a time when the very mention of a campaign further north than Sinai seemed taboo with some major Egyptologists (from Gardiner to Kitchen). Here we feel vindicated, at least in this respect, by the complete sea-change in opinion regarding the reality of Ramesses III’s Levantine campaigns—as exemplified in the recent writings of Kitchen himself.

The legitimate question should be raised whether Ramesses III makes a better candidate than Shoshenq I as the “king Shishak” who subdued Judah in year 5 of Rehoboam. The case for the origin of the biblical name cannot be decided by philology alone, and ultimately the choice depends on the outcome of future developments in TIP chronology and whether Shoshenq I belongs to the 10<sup>th</sup> or 9<sup>th</sup> century BC—as remarked by van der Veen.<sup>291</sup> Jerusalem is absent from the toponym list of Shoshenq, and only one of Rehoboam’s fifteen “fenced cities,” Aijalon, appears to be mentioned.<sup>292</sup>

First it should be noted that the character of Ramesses III’s toponym lists is very different to that of Shoshenq I.<sup>293</sup> Almost uniquely

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I might lead to a lowering of their dates from the 10<sup>th</sup> to the late 9<sup>th</sup> century BC. See now more forcefully Wallenfels in press and van der Veen 2015b: 191–192. On stratigraphical grounds Chapman (2009; 2015: 144) has tentatively suggested that the Shoshenq I stela from Megiddo was originally erected in the 9<sup>th</sup> century BC.

<sup>290</sup> We noted (James *et al.* 1991a: 257, 385, n. 135) that Sese or Sessi (*Ssꜣꜣsw*) was a well-known abbreviation of the royal name Ramesses, frequently used for Ramesses II in contexts referring to place-names *en route* to or in the Levant. Sese is also attested for Ramesses III (James *et al.* 1992: 127). For more arguments, thorough documentation and possible explanations for the *q* in the biblical name see van der Veen 2015; for arguments to the contrary see Sagrillo 2015.

<sup>291</sup> van der Veen 2015a: 94.

<sup>292</sup> Bimson 2015a: 5–6.

<sup>293</sup> See Kitchen 2009: 134.

among such documents, the latter's list of Asiatic towns is generally agreed to comprise strings of place-names that largely reflect marching itineraries.<sup>294</sup> A similar argument has been made by Redford for a Transjordanian section of the list of Thutmose III (see above).

Nevertheless, whereas the list of Shoshenq I conspicuously lacks any reference to Jerusalem, that of Ramesses III arguably does (**89**, *Qau-n-salem*, "the height of Salem"). That Ramesses II also mentions this place-name does not weaken the point, but merely reinforces the likelihood that the Ramessides took great interest in the region of Jerusalem—as confirmed by the increasing evidence collected by van der Veen and his survey team (see above). The same applies to the possible reference to Şaphon in Transjordan (**90**) where the nearby (or same) site of Tell es Sa'ideyeh was a major bronze-smelting site in late Ramesside times (see above). Solomon's Phoenician craftsmen were said to have cast the bronze furnishings for his Temple in the clay of Jordan between Succoth and Zarethan (1 Kings 7:46).<sup>295</sup> Jordan is undoubtedly the next toponym in both Ramesses II's and Ramesses III's lists (**15** and **91**). With respect to Jerusalem, the possible reference to the spring of Gihon in a different list of Ramesses III (see above) also should be noted.

Regarding the "fenced cities" of Rehoboam said to have been captured by Shishak<sup>296</sup> half of these lay in the western Shephelah (see map in Bimson 2015b: 110). The Shephelah as a whole suffered a mas-

<sup>294</sup> Kitchen 1986: 294–300; 432–447; James and van der Veen 2015.

<sup>295</sup> See James 2015: 250–251 for discussion, including the biblical identities of Tell es-Sa'ideyeh and Deir 'Alla. For the reliability of the biblical reference to the Jordan Valley in this context, see Zwicker 2015: 149, who makes some important points regarding the physical conditions needed for smelting. The bronze casting site of Deir 'Alla produced LHIIB pottery along with a luxurious faience vase bearing the name of the female pharaoh Twosret which, like that found at Sidon, may have been a prestige gift to a local ruler. Kokkinos (2015: 164–165) speculated that Twosret is a likely historical prototype for the biblical "Queen of Sheba"/Josephine "Queen of Egypt and Ethiopia."

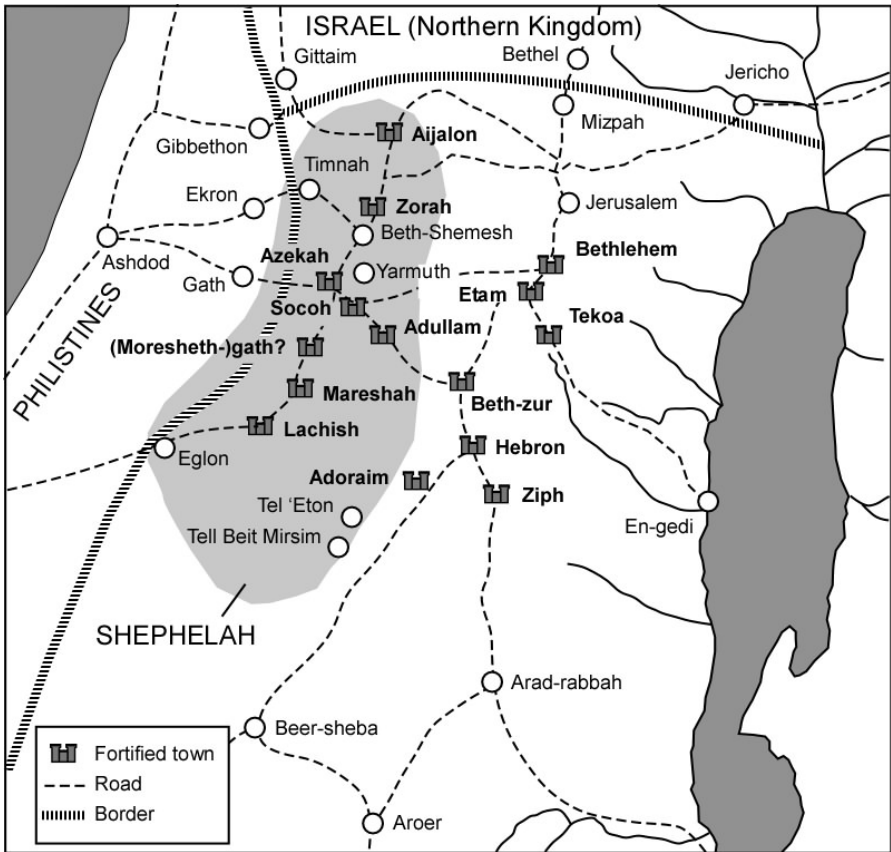
<sup>296</sup> Of course, many have argued that the Chronicler's list of Rehoboam's fifteen fortified cities was compiled much later than his reign, even in Hasmonean times! See Bimson (2015: 6) for references *pro* and *contra* with discussion, on strategic grounds, in favour of the likelihood that Rehoboam "fenced" these cities before and not after the invasion of Shishak.

sive wave of destruction at the LBA/IA transition which, described by Bimson as “the emptying of the Shephelah,” can best be associated with the campaigns of Ramesses III (see above). Regarding individual sites from the biblical list, Hebron is almost certainly identifiable in the list (77). As noted from the evidence of archaeology and Egyptian inscriptions, it seems likely that another of the “fortified cities,” Lachish (Level VI), was destroyed by Ramesses III. If the identification of toponym 70 with dual-form “Horonaim” argued here holds up, it is noteworthy that Solomon was said to have made Lower Beth-Horon (1 Kings 9:17) and Upper Beth-Horon (2 Chron. 8:5) “fenced cities.” While they do not appear in the Chronicles list of Rehoboam’s forts, they would have fitted well into his system of Judahite defences aimed at incursions from both Egypt and its client state, the newly formed kingdom of Israel. For Ramesses III, like “Shishak,” to have seized control of the pass would have been a strategic necessity—to reach the northern highlands and the Jezreel Valley.

There is no room here to rehearse the case regarding the identity of the biblical “Shishak” which has been done elsewhere and depends on much wider considerations than those addressed in the present paper. For the moment I would submit that a combination of the literary and archaeological evidence strongly prefers the Ramesses III over Shoshenq I as the biblical “Shishak.”

Chronological debates aside, I hope the present paper will draw the attention of scholars to an important section of Ramesses III’s “Great Asiatic” list (70–110), its relationship to a (largely) parallel list of Ramesses II and its geographical and strategic significance—matters largely ignored for over a century. On that note, the last words should be left to the almost prophetic scholar Archibald Sayce who made these remarkable comments, without the aid of modern epigraphic surveys or excavations:

*It is in the ruined temple of Medînet Habu that Rameses III. has recorded his victories and inscribed the names of the peoples and cities he had overcome. We gather from*



**Fig. 5.** Map showing the area of the Shephelah and the location of Rehoboam’s “fenced cities,” according to 1 Chronicles 11:5–12. Map by Uwe Zerbst, from Bimson 2015b.

*the latter that his armies had followed the roads already traversed by Ramses II., had marched through the south of Palestine into Moab, and had made their way along the seacoast into Northern Syria...It is plain that the northern campaign of the Pharaoh was little better than a raid. No attempt was made to capture the cities of the coast and re-*



*establish in them the Egyptian power. The Egyptian army passed them by without any effort to reduce them. Possibly the Philistines had already settled on the coast and had shown themselves too strong to be meddled with; possibly the Egyptian fleet was acting in concert with the troops on land, and Ramses cared only to lead his forces to some spot on the north Syrian coast, from whence, if necessary, the ships could convey them home. Whatever may have been the reason, the fact remains that Gaza alone of the cities of the Canaanitish coast fell into the hands of the Pharaoh. It was only in the extreme south, in what was afterwards to become the territory of Judah, that he overran the country and occupied the large towns.<sup>297</sup>*

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## ABBREVIATIONS

*ANET* = PRITCHARD, J.B. (ed.). 1969. *Ancient Near Eastern Texts Relating to the Old Testament*. Princeton, Princeton University Press.

*ARE III* = BREASTED, J.H. 1906. *Ancient Records of Egypt, Vol. 3: The Nineteenth Dynasty*. Chicago, University of Chicago Press.

*ARE IV* = BREASTED, J.H. 1906. *Ancient Records of Egypt, Vol. 4: The Twentieth to the Twenty-Sixth Dynasties*. Chicago, University of Chicago Press.

*MH I* = Epigraphic Survey 1930. *Medinet Habu, Volume 1: Earlier Historical Records of Ramses III*. Chicago, University of Chicago Press.

<sup>297</sup> Sayce 1912: 205–207.

MH II = Epigraphic Survey 1932. *Medinet Habu, Volume 2: Later Historical Records of Ramesses III*. Chicago, University of Chicago Press.

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