REVIEWS AND DISCUSSION

Trevor R. Bryce, "Ahhiyawans and Mycenaeans—An Anatolian Viewpoint", *Oxford Journal of Archeology* 8(3), 1989, 297-310.

The identification of Hittite-mentioned *Ahhiyawa* with the mainland of Greece is now generally agreed upon by Hittitologists.[1] This identification is ultimately based upon Forrer's modest statement of 1930.[2]

Bryce's survey of Ahhiyawa-texts is useful, since it presents most historical texts on the subject in a (partial) translation in English. A Hittitologist's approach would perhaps have shown more in detail what new readings have been found since the translations in German were presented in the 1930's, but Bryce has chosen for a multi-disciplinary approach and concentrates on what can be said on the activities of Mycenaean Greeks in Anatolia.

This view goes back to Forrer's original views of 1924, in which he announced to have found Greeks in Hittite texts, which was rejected unconditionally by Hittitologists at the time.[3]

Now that Ahhiyawa is situated in Greece rather than anywhere else, can we rely on Forrer's earlier views as well? In my opinion, equations of geographical and personal names based on Oral History, on texts in Mycenaean Greek or, as has been defended, in Luwian (e.g. GN Achaiis, (W)ilon, Troia, Karia, Lukia; PN Alexandros, Motylos, Asios), have a profound influence on the knowledge of geography of western Anatolia.[4] The alternative reconstructions offered by Hittitologists who denied the Ahhiyawa-identification have caused more problems to the interpretation of the texts then they have solved, so a return to the geography as proposed for the west by Garstang and Gurney[5] is acceptable.[6]

[1] The leading article is H.G. Güterbock, "The Ahhiyawa Problem Reconsidered", *AJA* 87 (1983), 133-8.

[2] In *Kleinasiatische Forschungen* I (1930), 253 Forrer argued as follows: 1. Ahhiyawa is a Great power; 2. Because of this, it cannot be situated within Anatolia; 3. For a location outside of Anatolia, the Greek mainland is the likeliest candidate.

[3] Cf. Güterbock (1983), 133 n. 1-2.

[4] Cf. D.W. Smit, *Talanta* XVIII-XIX (1986-1987), 49-50.

[5] J. Garstang & O.R. Gurney, *Geography of the Hittite Empire*, London 1959.

[6] Cf. Güterbock (1983), 133, 138.

It can be defended, therefore, that the Mycenaeans had some knowledge of Hittite-controlled Anatolia. But it still has to be shown that they occupied a considerable part of it at any period. The circumstantial evidence provided by archeology tends to abandon this view, as too few Mycenaean artefacts have been shown to argue for Greek control of any site.[7] We may not need abundant pottery, as Milawanda *was* once part of Ahhiyawa[8] and Hittite control over Alasija/Cyprus cannot be confirmed by archeology either.

The reconstruction of Hittite history is by no means easy. Even if a view seems to be based on a substantial number of documents it always has to be kept in mind that the present state of the evidence allows only a degree of certainty of ca. 80-90%. New documents are found and interpreted continuously and our views on the history of Ahhiyawa may be influenced profoundly by texts that do not mention it. According to the Bronze-tablet[9] two of the three vassal-kingdoms that were created by Mursilis II in 1318 still existed in ca.1235.[10] This does not prove that Hittite control over the west was undisturbed, but, in my view, makes a Mycenaean challenge of the *pax Hethitica* at least less radical.

It is a "Greek", rather than an Anatolian viewpoint to argue for a strong Ahhiyawa. It should be kept in mind that only the earliest Hittite Ahhiyawa-text records actual fighting[11] and that even this did not cause the Hittite king to be *kurur* (in a state of war) with Ahhiyawa.

Ahhiyawa does not even appear to have controlled the seas. In the Sausgamuwa-treaty[12] Tuthalijas IV sets an embargo on Ahhiyawan ships sailing to his vassal, but he does not consider Ahhiyawa's comment in any paragraph. In other texts the Anatolians sail to and fro unhindered; Bryce is inaccurate when he holds that the islands between the two countries were controlled by Ahhiyawa as early as 1319/18 (300).[13]

Ahhiyawan domination of Milawanda has only been attested in KUB XIV 3 and this document is unique in more than one respect. A

[7] Cf. C.B. Mee, "Aegean Trade and Settlement in Anatolia in the Second Millennium B.C.", *Anatolian Studies* 28 (1978), 121-56.

[8] It may be no great surprise that the concentration of Mycenaean finds is greatest in and around Miletos.

[9] H. Otten, *Die Bronzetafel aus Bogazköy*, Wiesbaden 1988.

[10] Only the kings of these two (Mira and the Seha-riverland) were akin to the Hittite royal family.

[11] Only one death on each side (with the name of the Hittite victim) is recorded.

[12] Cf. Güterbock (1983), 136.

[13] On KBo III 4 and the islands see elsewhere in this volume. The idea of an intermediary zone is worked out by F. Schachermeyr, *Mykene und das Hethiterreich*, Vienna 1986.

study of this and all related documents which accepts the identification of Ahhiyawa is needed, but it may appear that Bryce's dating, which follows only Güterbock and Singer[14] needs to be adjusted from Hattusilis III to Muwatallis.[15] Any historical reconstruction of the events of KUB XIV 3 should consider the information provided by fragments that mention Pijamaradus, e.g. KBo XIV 35, XIX 78 and XXVII 4.[16]

Atpas and Pijamaradus are mentioned together in two documents only. The dating of the Manapa-dU-letter (KUB XIX 5 + XIX 79)[17] is decisive for the dating of KUB XIV 3, not vice-versa, as Bryce holds (303). If its author is the same Manapa-dU as known from other texts, a Hattusilis-dating for KUB XIV 3 is problematical. He may be someone unknown,[18] but Atpas' *recorded* activities do not include a "takeover" of the Seha-riverland (Bryce 307).

Mycenaean scholars had better wait for this and other problems to be solved as convincingly as the identification of Ahhiyawa before surrendering to the desire to reconstruct history based upon too little evidence.

D.W. Smit

[14] I. Singer, "Western Anatolia in the 13th Century B.C. According to Hittite Sources", *AS* 33 (1983), 205-17.

[15] So recently D.F. Easton, "Has the Trojan War been found?", *Antiquity* LIX (1985), 194; D.W. Smit, *The Chronology of the Tawagalawas-letter* (forthcoming).

[16] See S. Heinhold-Krahmer, "Untersuchungen zu Piyamaradu (1)", *Orientalia* 52 (1983), 81-97; *Ibid.* (2), *Or.* 55 (1966), 47-62.

[17] Cf. Ph.H.J. Houwink ten Cate, "Sidelights on the Ahhiyawa Question from Hittite Vassal and Legal Correspondence, JEOL 28 (1983-84) 33-61.

[18] Defended by M. Popko, "Zur Datierung des Tawagalawas-Briefes", *Altorientalische Forschungen* 11 (1984), 201.

Greeks in Thrace: Some comments on the Studies of the Dutch Archaeological and Historical Society, vol. 10: Isaac, B.H., *The Greek Settlements in Thrace until the Macedonian Conquest*, ISBN 90 04 06921 6, Brill, Leiden 1986.

The sources on which we must rely to get our picture of Thracian history are rather scarce. For the historic period we have some written sources[1]. Most of these sources are written by authors who did not have autoptic knowledge of Thrace. The information we may gather from all these sources is often limited to the events or situations in the southern part of Thrace, i.e. the northern coasts of the Aegean and the area of the Straits and the Propontis. The hinterland and the Greek colonies on the Pontus and their hinterland are mentioned obliquely, if at all. This means that our knowledge by means of historical sources shows many lacunae.

These lacunae force us to use other sources: archaeological, numismatical and epigraphical. Since the mid-sixties of this century much new information has been gathered by research in these fields. But here also white spots occur, e.g. because of the fact that in the present Turkish part of Thrace too little research has been carried through, or because of the fact that summaries of articles in Russian or Bulgarian, written in a western European language are sometimes very summary, or because still too much material is waiting for publication[2].

The lack of available information is also visible in modern literature. The most coherent and complete review of the colonization of Thrace and the Pontus-region up to now are to be found in a limited number of books and articles. In *The Greeks Overseas*[3] a review is given of the colonization by Greeks and contacts with indigenous peoples in Thrace and the Pontus-region as a whole. For the northern coast of the Pontus a useful bibliography on Greek colonies exists, though it contains no books or articles younger than 1962 or written by non-Russians[4]. The articles *Thrake* and *Pontos Euxeinos* in the *RE* contain much infor-

[1] Casson, St., *Macedonia, Thrace and Illyria*, Oxford 1926 (repr. Groningen 1968), p. 260-268 gives a review of classical authors on Thrace, as does Danov, Chr. M., *Altthrakien*, Berlin/New York, 1976, p. 21-52.

[2] Cf. the remarks of Harding, A. F., "The Regional Context of the Bulgarian Bronze Age" in: *Ancient Bulgaria, Papers presented to the International Symposium on the Ancient History and Archaeology of Bulgaria*, ed. by A.G. Poulter, Nottingham, 1983, Part 1, p. 165, p. 167; though primarily aimed at the situation for prehistorical archaeology, the same rule is valid for that of historical times.

[3] Boardman, J., *The Greeks Overseas*, London, 1980 (3rd ed.), p. 229-266.

[4] Belin de Ballu, E., *L'histoire des colonies grecques du littoral nord de la mer noire*, Leiden 1965 (2nd ed.).

mation and both are extremely useful for their bibliographical data. Regrettably both are not up-to-date. The same conclusion is valid for the books of Danov and Casson, named above in note 1. Danov's book refers to literature published up to 1971. Most of the literature is to be found in the notes: his bibliography is hopelessly limited. Casson's study is still extremely valuable because of its geographical descriptions, but Casson largely omitted the Greek cities in the Pontus-region from his study. He explained this by putting that these cities "belong properly to a grouping that was disconnected from the Mediterranean"[5]. Both for the student of Greek colonization and the Thracologist much—not all—of the problems just described may be compensated by Isaac's book, that takes into account all information available up to 1981.

The work was limited to the Greek settlements in Thrace because, as the author states[6]: "Greek cities have to be studied in conjunction with their hinterland. The peoples controlling the hinterland of the northern settlements were as diverse in antiquity as they are now... . The Thracian group was by far the greatest in number and contained many significant cities. It can, without great difficulty, be isolated geographically and we have better evidence—including literary sources—for many of the Thracian towns than for the remote settlements in Skythia and Asia Minor." The data are presented in geographical order, moving along the coast, from the Strymon to the Danube. To do this as coherently as possible, Isaac has divided the settlements into 5 groups: (1) the lower Strymon valley and the Thasian *Peraia*; (2) Abdera, Dikaia and Maroneia; (3) the Samothrakian *Peraia*; (4) the Thracian Chersonese and the Propontis; (5) Byzantion and the Black Sea.

In 1937 G.A. Short published a study on the location of a number of Greek colonies[7]. He concluded that the main requirements for a settlement were:

- the sites should be defensible against raids by pirates from sea by locating them on islands, on a peninsula or on cliffs;[8]
- presence of food supplies;
- good harbourage;
- defence against attacks by land should be (made) possible.

Of these four "rules" Short considered the first two primary, the third

[5] Casson, p. vii.

[6] P. xii.

[7] Short, G.A., "The Siting of Greek Colonies on the Black Sea Coasts of Bulgaria and Romania", *AAA* 24 (1937), vv. 3-4, p. 141-155.

[8] Cf. Isaac, p. 282 on this point.

and fourth secondary. If we apply these rules to various locations along the Thracian coast of the Pontus, we may note that there are locations, like Kalpes Limen[9] in Bithynia or Salmydessos (Midya, modern Turkish Kiyiköy) that fulfill these requirements. These sites have not been colonised—as it may appear—by the Greeks, though they are situated very strategically. On the other hand, there are sites, like Kallatis (modern Mangalia in Romania), where colonies were situated on spots that would seem hardly adequate.

The description that Isaac gives of the location of the settlements that can be identified with reasonable certainty confirms Short's observations for the major part. Isaac, however, goes at least one step further. He also shows that e.g. economical, strategical and politico-strategical reasons were equally important for the siting of colonies and should, in fact, be ranked among the primary requirements. Of course, especially the importance of economical motives for Greek colonization has been stressed very often in literature. For this area it is, however, the first time that all colonies, their siting and background, have been investigated so thoroughly. This alone does not make Isaac's book a very valuable one. It is also valuable because it is a *Fundgrube* for further reading and, last but not least, because Isaac fulfills the implicit promise of his introduction. He connects, wherever possible, the situation of the Greek colonies with their hinterland. To my regret the relation of Thrace with Persia remains somewhat shadowy, due to the lack of material, but especially the relations of the Greek cities with the Odrysian kingdom become very clear.

Despite all praise that Isaac's study merits, there are a number of points on which I would like to comment or complete it.

1. On p. xi Bulgaria should be included in the number of countries over which the Northern Greeks are spread.

2. On p. 212, 5th line of "13" 340 should be read 430.

3. Isaac mostly uses Chalkedon[10] instead of the more correct Kalchedon[11], which he only uses once[12].

4. Ad p. 144: except by a number of valleys and glens between present-day Kirklareli (Turkey) and Malko Turnovo (Bulgaria) the Istranca Daglari cannot be crossed without great difficulties. The Istranca is generally densely forested with oaks, oak-scrub and brushwood and not very accessible. The most appropriate route, apart

[9] Xenophon, *Anabasis*, VI.iv.1-7.
[10] P. 219-228 *passim*, p. 237-9, p. 250-1, p. 264.
[11] Cf. *RE*, *s.vv.*
[12] P. 236.

from the one just mentioned, is the one Isaac described as an alternative one.

5. Ad p. 145. The fragment of Archilochus that Isaac quotes for Thracian slaves from Salmydessos is misread. The fragment, now generally ascribed to Hipponax[13], is a curse of a person (a Greek) against somebody who was formerly his friend but, since he trampled upon the oath, now his enemy. He wishes:

> ....
> May with pleasure in Salmydessos
> The Thracians with hair on the crown
> clutch him, naked. There he will suffer many hardships
> eating slave's bread
> grown numb with cold....
> ....

The fragment cannot be used as an illustration for the provenance of Thracian slaves from Salmydessos, though it may be used to demonstrate the feelings of Greeks towards life in these surroundings.

6. Ad p. 176, line 8. Isaac speaks of a Persian *satrap* at Sestus, Herodotus only mentions an ὕπαρχος, named Artaÿctes, at Sestus[14] (the reference Isaac gives (n. 99) relates the battle of Marathon). In my opinion the equation ὕπαρχος and satrap is *prima facie* correct, since How and Wells state that ὕπαρχος is the term Herodotus used for the function of satrap[15]. However, ὕπαρχος is also used of the commander of a fortress[16]; Liddell/Scott/Jones/ McKenzie[17] describe ὕπαρχος as 1. the subordinate commander, the lieutenant, 2. the subordinate governor (of satraps).

Up to now the only detailed list of satrapies is given by Herodotus[18]. Since the Thracians are not mentioned there, nor any other European people, I think Isaac has made the equation without proper reserve.

Apart from these points there are some minor inaccuracies and/or printer's errors, that may be corrected, awaiting a next edition, with a list of errata by the publisher.

Isaac's final remarks fit in—and elaborate, due to more research

13 Hipponax, ed. Degani, E., Leipzig, 1984 (Teubner), F. 194.

14 Herodotus, IX.116.

15 How, W.W., and J. Wells, *A Commentary on Herodotus, with introduction and appendixes*, 2 vls., Oxford, 1968, v.I, p. 281 ad III.89.

16 How/Wells, v.II, p. 335 ad IX.116.

17 *GEL, s.v.*

18 Herodotus, III.90-7.

since the 1930's—with Short's conclusions. There are still places where research may be rewarding. I will name two. The first is at Cape Shabla, probably ancient Καρῶν Λιμήν[19]. At the beach, near a pier protruding into the sea, remains of walls and scattered sherds are found. The sherds date mainly from the 1st and 2nd centuries AD, but more ancient remains are supposed to exist. Part of the work to be, however, is underwater and diving conditions—and facilities—are not extremely favourable.

A second, and very promising, site is to the south of Cape Urdovitsa, some 20 kms. south of Sozopol (ancient Apollonia Pontica) on the road to Michurin. There, just to the right of the road over the bridge of a river, of which the water nowadays is dammed by a sandbank during the summer, lies the site of an *emporion*. During a field-survey in 1983 we found there sherds of Rhodian, Samian and Chian ware, as well as an Attic tile, all dating from the 5th or early 4th century BC. Excavation of this site is, for the moment, impossible, since it is claimed to be military area.

There are, however, certainly in Bulgaria, numerous spots where smaller emporia still may be found. Moreover, since archaeological work on sites already known is continuing, it would be a useful thought to publish regularly, perhaps every 10 years, a review of the state of research. The book of Isaac would be an excellent basis for such an undertaking. Isaac's work deserves such a follow-up, to say the least.

J.P. Stronk

[19] Cf. Isaac, p. 261 and 265.

Letter to the Editor of *Antiquity*, Christopher Chippindale:

MISINFORMATION IN *Antiquity*

Reviews are intended to provide readers of a journal with a fair notion of the general contents of new editions in their field of interest. If such a recent publication concerns studies in decipherment, the reader is no doubt anxious to learn how the "results" presented relate to general principles or lines of approach validated in former successful attempts to decipherment.

Seen from this angle of incidence, the editorial board of *Antiquity* has made an excellent choice in selecting Mr John Chadwick for reviewing *Ancient Scripts from Crete and Cyprus* (Brill-Leiden, 1988) [see *Antiquity* 63, 238 (March 1989) 181]; for it is well-known that this is a scholar with first-hand knowledge about the deciphering process of Linear B, struggled through by the late Michael Ventris during the 40's and early 50's of our present century. In his earlier days, at least, Mr Chadwick did not grow tired of explaining to scholars and laymen alike the great momentum in the decipherment of Linear B, which resulted from the extremely favourable interaction between internal evidence in the form of Miss Alice Kober's doublets and triplets and external evidence provided by the relationship in form of the script to the Cypriote Syllabary from the Classical Period. The latter line of approach, it must be remembered, served as a starting-point for Ventris' work on the subject (see his contribution to *American Journal of Archaeology* 44 (1940) 494-520), but became a productive source only after verification by Kober's grid, now proving the validity of his readings *ti* and *to* for the signs classed as consonant 1 with alternating vowels 1 and 2 in this grid! (Note that the transliteration of the sign first mentioned as ^{14}i in *Documents*, p. 21 is the result of secondary intervention as becomes evident from a glance at Ventris' first grid, depicted in M. Pope, *The Story of Decipherment* [London 1975] 165.)

Now, in their preface the authors of *Ancient Scripts* have explicitly adhered themselves to the derivational approach and this is taken by Mr Chadwick as a guide in his discussion of their work on Linear A, the hieroglyphs on the Phaistos disc and Cypro-Minoan. But this approach is not applied without moments of verification, intended to minimize the risk of haphazard identifications. So, in connection with Linear A the origin traced for a number of signs in the Egyptian Hieroglyphic and Akkadian Cuneiform only leads up to minor adaptations concerning the value of these signs as compared to their Linear B equivalents, which better suits them for the expression of the Northwest Semitic since the 1930's—with Short's conclusions. There are still places where research may be rewarding. I will name two. The first is at Cape Shabla, probably ancient Καρῶν Λιμήν[19]. At the beach, near a pier protruding into the sea, remains of walls and scattered sherds are found. The sherds date mainly from the 1st and 2nd centuries AD, but more ancient remains are supposed to exist. Part of the work to be, however, is underwater and diving conditions—and facilities—are not extremely favourable.

A second, and very promising, site is to the south of Cape Urdovitsa, some 20 kms. south of Sozopol (ancient Apollonia Pontica) on the road to Michurin. There, just to the right of the road over the bridge of a river, of which the water nowadays is dammed by a sandbank during the summer, lies the site of an *emporion*. During a field-survey in 1983 we found there sherds of Rhodian, Samian and Chian ware, as well as an Attic tile, all dating from the 5th or early 4th century BC. Excavation of this site is, for the moment, impossible, since it is claimed to be military area.

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To determine the significance of these moments of verification for the deciphering process of the ancient scripts under consideration is, of course, up to the competent scholars in the field. But to omit them purposely from a discussion of the book in order to discredit the work as an at random comparison between scripts, lacking underlying system and method (mark the words "vague resemblance", "select from all the various scripts", "No attempt (...) to show any systematic correspondence", "some resemblances between signs, as might be expected to occur between any two scripts", etc.), seems an untolerable act of misinformation. Considering the already observed effort made by the editorial board of *Antiquity* to ensure a qualified review of our book, we are confident that it will grant us the opportunity to amplify the noted omissions by publishing the present Letter to the Editor.

Signed: Jan Best & Fred Woudhuizen.

Answer of Mr Chippindale in his letter from 29 August 1989:

Dear Dr Best [*sic*],

After careful consideration, I have decided not to publish your reply to Mr Chadwick's review. You will appreciate that many, or even most, book reviews could be the starting-point of a debate between author and reviewer, but that is not their function—at least in Antiquity. I think the review was an honest statement of considered opinion as a review should be; by someone with knowledge in the field, as your note fully recognises; and I therefore do not feel there is any error or deficiency in what Mr Chadwick has written, and we have published, that is of a kind to amount to a right of reply.

I have taken the liberty of sending to Mr Chadwick a copy of your note so that he knows privately of your views.

Yours sincerely, Christopher Chippindale.

Answer of the authors to Mr Chippindale, in a letter dated 3 September 1989:

Dear Mr Chippindale,

Thank you very much for your letter from August 29 in reaction to our "Letter to the Editor".

We do believe that free interchange of opinions between scholars is a prerequisite for progress in science, so we are glad that you informed Mr Chadwick privately about our views on his review.

Similarly, we are confident that you will have no objections to our plans to reserve some room for the present correspondence in future publications.

Yours sincerely, Jan Best & Fred Woudhuizen.