Identity in the Making: Greeks in the Eastern Mediterranean during the Iron Age

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Abstract
Although Greek contacts with the Southern Levant during the Iron Age have been studied at length, the matter remains controversial in many aspects. The present study provides an overview of East-West contacts during the first half of the 1st millennium BC, suggesting to divide it into five major periods of contact. These periods, involving a different chronological setting, are characterized by different ‘total contexts’, heavily shaped by geopolitical dynamics. It is suggested that every period of contacts (or their absence) requires a different explanation.

Introduction
For scholars interested in Greek contacts with the Southern Levant during the Iron Age two developments in the late 7th century BC are truly remarkable: the establishment of Naukratis in Egypt and the massive appearance of East Greek pottery on the eastern shores of the Mediterranean. It is not surprising therefore that these themes were chosen, inter alia, for the 28th British Museum Classical Colloquium. However, any attempt at discerning and decoding patterns in the dispersion of East Greek pottery in the Levant, as well as explaining the Naukratis phenomenon, requires an understanding of East–West contacts during the first half of the 1st millennium BC. Such an overview is undertaken here.

However, since I could not hope in the present format to do justice to the whole range of issues that preoccupy scholars dealing with Greeks in the East, I offer instead an extremely brief synopsis of Greeks in the East during the Iron Age, with special emphasis on a few thorny issues.

Since I shall concentrate on a number of broad historical/archaeological issues, it is perhaps prudent to acknowledge that every generation writes its own history and that every scholar has a view of the past coloured by his/her education, experience and environment. I have no pretensions therefore that my interpretations of East–West contacts will be taken as the only possible scenario. On the other hand, I hope that among the pool of potential explanations for the changing nature of East-West contacts, the model I offer best accounts for the available evidence.

From an epistemological point of view, I am on the side of many who argue that among the three main poles – realism, positivism and idealism – it is usually realism that offers the most useful point of departure for any archaeological reconstruction, especially when this realism is combined with a healthy dose of scepticism and a pinch of imagination. And although I can accept, at least to a certain extent, that in too many cases ‘there are no facts, only interpretations’, archaeology does often supply facts. Some facts, such as the presence or absence of Greek pottery on the eastern shores of the Mediterranean, matter a great deal. The question remains: what we are going to do with these facts? But before I embark on the 'pots and people' question, I would like to emphasize the significance of the historical/chronological context – the backbone of any historical interpretation.

The accumulation of data, an essential beginning, should lead to contextualization involving the understanding that different chronological settings may represent different geopolitical dynamics. Ian Morris rightly observes that one of the major shortcomings of the post-modern trend of emphasising connectivity and mobility is its timeless. He points out that many of what he calls first wave studies 'showing links between Greek and Near Eastern cultures, often threw together evidence scattered across centuries, disregarding traditional chronologies'. The recent contribution of Horden and Purcell takes this approach even further, arguing against interpretations that emphasize radical change and violent discontinuity in the Mediterranean past. What is offered instead is a vision of a permanently integrated Mediterranean, wherein change is constant and ubiquitous, but generally local in its effects. Such a reconstruction, with its emphasis on microregions, leaves little room for pivotal turning points in Mediterranean history, since the assumed connectivity stretches across extremes of time, by-passing geo-political boundaries and empires, together with symbolically expressed ideologies of economic exchange and political domination.

With mobility as the norm and a permanent feature of human activity around the Mediterranean shores, we are forced to ask questions differently. Or, as Emma Blake recently put it, ‘rather than ask, why did people move, one may ask, why did people stay put in some cases?’ Heavily affected by current globalization, Horden and Purcell’s vision of the Mediterranean is already considered by some, and not without reason, as ‘one of those manifest watersheds in the study of antiquity’, which will take a generation of historians to digest. Indeed, taking into consideration a number of earlier studies in favour of a permanently connected Mediterranean, one is tempted to suppose that we are witnessing a paradigm shift.

What is missing in the portrait of a permanently connected Mediterranean, however, is the notion of historical/chronological context. In this regard, Bakhtin’s concept of the total context of an utterance provides an applicable insight. The total context relates to the ways in which voices circulate in both spoken and written dialogues and, according to Bakhtin, is unrepeatable. Even if one repeats the words employed in the same order, the total context would be always different, if for no other reason than because the words have already been uttered once.

And when Horden and Purcell insert the distribution of Late Bronze Age ox-hide ingots into the model of a permanently connected Mediterranean, for instance, comparing it simplistically with the whole spectrum of later metallurgical distributive systems, the ‘total unrepeatable context’ of...
particular periods is lost. The problem is not one of comparing some chronologically distant metallurgical distributive systems. After all, the merits of the comparative approach are undeniable. Likewise, analogies are appropriate tools and salient features of any historical/archaeological investigation. The problem is a deliberate unwillingness to recognize that the distribution of Late Bronze Age ox-hide ingots should be understood on its own terms and against the background of Late Bronze Age geo-political dynamics, which are a world apart from the distributive systems of the Greeks and Romans, let alone those of medieval Genoa. Or, as Mario Liverani observes, 'the “Bronze Age”, invented as a classificatory device for tools and weapons, can still be used as a large historical label, encompassing similarly structured socioeconomic systems and quite sharply opposed to the (differently labelled) preceding and succeeding periods'; (emphasis added – A.F.).

Although it might be relevant, I am not concerned here with the long-running debate involving polarising tendencies ‘to see the past as Same (a primitive version of our present, which teleologically evolves into it) or as Other (as a remote, alien, fundamentally different world)’. My main concerns are socially embedded cultural contexts and their chronological settings. Therefore, with regard to metallurgical distributive systems, the only reliable conclusion that may be deduced from the analogies scattered across the centuries is, in my view, an acknowledgment that different distributive systems have existed in the Mediterranean at different times. However, in order to understand the forces driving these and other exchange activities, they must be viewed in their proper chronological/historical contexts. It is not helpful to gather all the cases of connectedness and mobility under the same rubric of a permanently interconnected Mediterranean without distinguishing between different historical periods.

Indeed, the presence or absence of Greeks in the Eastern Mediterranean during the Iron Age suggests that there is no single model that would explain these contacts (or their absence) through different time periods. Quite the opposite: judging from the facts on the ground (and there are some), every subsequent historical period requires a different explanation, a different narrative.

Greek contact with the eastern Mediterranean during the Iron Age: stressing the context

The area under discussion runs from the coast east of Cilicia down to the Sinai Peninsula. The contacts in question may be divided roughly into five major periods, each involving a different chronological setting. These settings are characterized by different ‘total contexts’ heavily shaped by geo-political dynamics.

First period: a renewal of contact

The first period is characterized by the presence of mainly Euboean pottery (but also Attic and Atticizing) found in northern Syria, Phoenicia and northern Israel in the late 10th, the 9th and the better part of the 8th centuries BC. The assumed Phoenician superiority in virtually everything leaves, according to many modern scholars, no room for independent Euboean ventures at such an early date, especially to the East. When even pure Cypriot ventures are labelled Cypro-Phoenician, it is quite obvious that Euboeans could not compete with the advanced Phoenicians, let alone establish a trading post at Al Mina toward the end of the 9th century BC.

However, the trend during the last decades of pinpointing the beginning of Phoenician expansion to as early as the 11th/10th centuries BC, if not earlier, is based almost entirely on a handful of presumably historical sources: to a lesser extent on the so-called ‘Report of Wenamun’ and to a larger extent on the biblical accounts regarding the cooperation between Kings Solomon and Hiram I. These sources can no longer be treated as reliable. Furthermore, the low Iron Age chronology, advanced in Israel nearly a decade ago, has enormous implications for the Aegean world.

First, it leaves no room for Phoenician colonial expansion before the late 9th–early 8th centuries BC. The presence of imported Phoenician vases in the assemblages at Palaeaphos Skales should not imply the beginning of Phoenician colonisation of Cyprus before their establishment in Kiton at the late 9th century BC. Indeed, judging from available archaeological evidence, the initial Phoenician expansion overseas, accompanied by settlements abroad, took place only in the second half of the 9th century BC; and I refer to the well-known Phoenician establishment at Kiton, but also to evidence from new radiocarbon dating from Carthage and Southern Spain.

In my view, this expansion may be explained as a result of pressure from Hazael, the king of Aram Damascus. A plethora of archaeological data accumulated in Israel, such as Hazael’s inscriptions and possible destruction layers, mostly in northern Israel, but also to the south in biblical Gath, suggests that Hazael’s kingdom was one of the most serious players in the Southern Levant during the second half of the 9th century BC.

I believe that Susan Frankenstein’s theory, that the Phoenician specialization in trade, accompanied by their settlements abroad, should not be seen entirely as free-trade activity, but rather in the context of their functioning as commercial agents for the Neo-Assyrian Empire, is basically correct. However, judging from the archaeological data regarding the beginning of Phoenician expansion overseas, this delicate arrangement, which eventually transformed the Phoenicians into pan-Mediterranean traders, started in the days of Hazael, with Phoenicians serving the trade ambitions of Aram Damascus.

Second, and even more important, the low Syro-Palestinian chronology provides, finally, an anchor for Aegean Proto-Geometric and Geometric chronologies.

A minimalist approach to the beginning of Phoenician affairs in the Mediterranean leads, in conjunction with a low chronology, to an emphasis on the principal role played by the Euboeans in the renewal of contact between East and West, culminating in the establishment of Al Mina sometime around 800 BC. This, of course, occurred on behalf of local rulers. The same pattern will be observed almost 200 years later, with the establishment of Naukratis in Egypt. In this regard, Boardman’s notion that we should consider a trading port at Al Mina as a modest precursor of Naukratis is rather attractive. The Greek presence in the Eastern Mediterranean at this early period seems always to be restricted and controlled by local
Therefore, I strongly disagree with the idea that accepting a prominent Euboean role in Early Iron Age journeys to the East makes one Helleno-centrist. The Euboeans were conducting these journeys because they were interested in re-establishing lost contacts with the East. It would give to the ruler of Lefkandi, for example, an enormous advantage compared to other contemporary Greek rulers. For the Greek side it meant a great deal. For the East, it does not seem to mean much at all. But for the Greeks it meant the beginning of the Orientalizing movement, with a minor Phoenician contribution, but mainly, through the Syrians, as was already suggested long ago and on many occasions by John Boardman. To this, one should add the adoption of the Greek alphabet, sometime around the middle of the 8th century BC. All in all, although the renewal of contact may be attested during the 10th/9th centuries BC, it certainly intensified during the better part of the 8th century BC at least until the beginning of the Neo-Assyrian domination over the Southern Levant.

Second period: the Neo-Assyrian domination

Greek contacts with the East were halted by Assyrian expansion; here we arrive at a second period, the period of Assyrian domination. The recent understanding of the processes that took place in the Southern Levant near the end of the 8th and during the main part of the 7th centuries BC shows unprecedented involvement of the Assyrian administration in local affairs. This involvement may be seen in a variety of fields, such as the annexation of many Levantine kingdoms accompanied by the transformation of some of them into Assyrian provinces; population exchanges; re-arrangement of the borders and intensive construction activity. The latter is particularly visible in the coastal area, which is dotted with Assyrian emporia and fortresses. One of the most important Assyrian goals was the supervision of Phoenician trading activity. In this regard, as I have already stated, Susan Frankenstein’s theory viewing the Phoenicians as commercial agents for the Neo-Assyrian Empire seems to be basically correct. Concerning the Eastern Mediterranean, it is quite clear that every aspect of Phoenician commerce was closely overseen and taxed by Assyrian officials. What we are witnessing here is a delicate balancing act. On the one hand, the Phoenicians enjoyed the stability produced by the pax Assyriaca and the exclusive access to the network of trade-routes and trade-centres across the Eastern Mediterranean. On the other hand, their commerce was strictly regulated and taxed. The Phoenicians involved in commercial and colonial activities in the Western Mediterranean, far from their Assyrian masters, doubtless enjoyed a higher degree of flexibility than their counterparts in the Eastern Mediterranean. From the point of view of the present colloquium, however, the most important conclusion is that, with regard to the southern Levant, this new world-order left most of the mainland Greeks quite effectively out of the game.

The single limited point of contact that was left was again Al Mina, which became a port of trade toward the end of the 8th and during the 7th centuries BC. But after c. 700 BC, Euboean imports to the Southern Levant almost disappear. Starting from Al Mina’s Level 6, it is mainly East Greek pottery that shows up during the period of Assyrian domination, not Euboean. Besides it is not yet entirely clear who was responsible for carrying this pottery to Al Mina. Did it arrive directly from Eastern Greece or was the Cypriot connection involved? What appears to be quite clear, however, is that mainland Greece seems to be without direct connections with the East, starting from the period of the Neo-Assyrian domination. In fact, excluding Al Mina, while even at this site there is a clear structural break between Levels 7 and 6, Greek pottery (except for a few insignificant cases) is almost non-existent in the Neo-Assyrian contexts. This contrasts with a much broader distribution prior to the Neo-Assyrian domination and, especially, immediately after its collapse.

Lanfranchi’s recent speculations regarding Greek contact with the Neo-Assyrian Empire, which are based, archaeologically, almost exclusively on Haider’s earlier study, will find no echo in the archaeological realities of the Southern Levant. Dependent as they are on mistaken representations and understandings of the archaeological data involved, Lanfranchi’s historical implications, according to which Assyrians favoured Greeks over Phoenicians in commercial and settlement activities in the southern Levant, can confidently be rejected. Similar confusion regarding the Greek pottery in the Southern Levant appears in Rollinger’s recent attempt to draw a picture of Greek contacts with the East during Neo-Assyrian period. Likewise, his suggestion that we consider the individuals mentioned in the Near-Eastern texts as laman + suffixes other than āya as possible Greeks acting in the midst of the Neo-Assyrian Empire, seems to reside on rather shaky ground.

Both archaeological and historical data suggest that during the Neo-Assyrian regime the Greeks occupied a marginal space in the Mesopotamian understanding of the universe. Bearing in mind the Neo-Assyrian imperial ideology, with its pretensions of ruling a universal domain, such a role for Greeks is understandable. Located in the ‘midst of the sea’, where the Neo-Assyrian regime was not able to insert them physically into the ‘correct relationship’ with the imperial new-world order, Greeks were reduced to the status of ‘disparate, remote people living on the edge of the world’ in the Neo-Assyrian map of the world.

The Phoenicians apparently were chosen to serve as commercial agents for the Neo-Assyrian empire not because they were natural-born traders, although their expertise should not be underestimated, but because the Neo-Assyrian regime was able to control their trade, which was not without benefits for both sides. Given this state of affairs, I tend to agree with Helm’s suggestion that, for the Greek side, ‘the imperial obligations imposed on permanent residents in Assyrian provinces made life in the Levant unattractive’. Indeed, as Helm pointed out more than 25 years ago:

Even in the few nominally independent port cities such as Arvad, Tyre, Ashkelon and Gaza it is likely that Greek traders would have encountered Assyrian administrators, commercial regulations and economic institutions. It was doubtful these contacts, and the contacts with other representatives of Assyrian provincial government, which gave visiting Greeks the not inaccurate impression that the entire east Mediterranean coast comprised ‘Aṣṣur’s administration.

The unprecedented involvement of the Neo-Assyrian administration in the local affairs of the Southern Levant (see above), attested both historically and archaeologically, is certainly in accord with Helm’s suggestions. In this regard, Amélie Kuhrt’s rather sceptical look at the evidence for direct
contact between Greece and the Mesopotamian empires is particularly revealing. Although, as in the earlier periods, the Greeks definitely continued to meet Easterners, this time these were mostly Phoenician competitors. And these are indeed the Homeric Phoenicians.

The nature of direct contact between the Greeks and the Near East during the second period in my provisional scheme suggests therefore the beginning of a ‘Great Divide’ rather than Burkett’s Orientalizing revolution.

It should be explicitly stated, however, that the concept of a Great Divide does not imply an immediate break in contacts. It is better described as a gradual process, starting with Tiglath-pileser III’s annexation of the kingdom of Unqi/Patina in 738/737 BC. If Zadok’s identification of Al Mina as Aṣḥāt in Tiglath-pileser’s inscription on the Iran stele is correct, this might indicate that right after the annexation of Unqi, an Assyrian emporium was installed at Al Mina, in order to regulate and incorporate the existing Greek enclave into the sphere of the Neo-Assyrian realm. Already at that time, a letter from Calah (Nimrud) (ND 2370), sent most probably to Tiglath-pileser III by Qurdi-Aṣšur-lāmūr, points to a possible Ionian raid on the Phoenician coast. To this one may add a reference to the town of Yauna, mentioned in a Neo-Assyrian letter (ND 2737) published a few years ago by Sagg. The letter contains no firmly dateable details. However, the themes discussed and the arenas of operation seem to be echoed in the letters of Qurdi-Aṣšur-lāmūr, who was probably the governor of Šimarra in the time of Tiglath-pileser III. In this regard, Na’aman’s suggestion that we identify the town of Yauna with Ras el-Bassit, would, if accepted, point to a possible Greek presence at this site at that time. Hereafter, however, the handful of Neo-Assyrian sources that mention Ionians, mostly in hostile contexts, when combined with an almost total lack of Greek pottery in the Neo-Assyrian assemblages (see above), leave little doubt about an intensification of the Great Divide.

**Third period: stressing the significance of the late 7th-century BC contact, during a brief period of Egyptian domination**

The next period, although chronologically brief, is the most important for the purposes of the present colloquium. I refer to some 20–25 years of Egyptian rule in the Southern Levant, following the Assyrian withdrawal. When the Assyrians pulled out from the Levant sometime in the twenties of the 7th century BC, the Egyptians took over their territories and ruled until the Babylonian invasion. This period, the third in my provisional schema of the Greek presence in the Levant, lasted until the Babylonian destructions at the end of the 7th and in the early 6th centuries BC.

The sudden and massive appearance of East Greek pottery on the coastal plain of Israel toward the end of the 7th century BC and its subsequent disappearance after only a few years fits the time-span during which the area fell under Egyptian rule. Following Nadav Na’aman’s insightful observations, I have elsewhere discussed at length the East Greek pottery assemblages found in places such as Ashkelon, and the fortresses of Mezad Hashavvahu and Kabri, arguing that these represent Greek mercenaries in the employ of the Egyptians. In this reconstruction, the placement of these garrisons along the coast together with the employment of Kittim along the southern fringe of the kingdom of Judah, conforms to two Egyptian goals: first, to protect the coastal plain – the main route to the North; and second, to protect the Arabian trade networks, which the Egyptians inherited from the Assyrians. The modest finds of East Greek pottery in the vicinity of major military bases probably reflect Greek mercenary activities in these areas rather than pottery trade.

Many scholars, however, have claimed that the abundance of East Greek pottery should be taken as evidence of East Greek trade. In these reconstructions even the coarse East Greek cooking pots are considered a tradable commodity to the East. In my view, most of these reconstructions are untenable. The attested distribution and the nature of East Greek finds in the region of Palestine are insufficient to prove either the existence of a developed pottery trade or the existence of a directional exchange of other goods that may be less visible in the archaeological record.

An additional point that argues in favour of East Greek mercenary garrisons rather than trading emporia is the restriction of East Greek trade to Naukratis in Egypt. It must be remembered that the establishment of Naukratis toward the end of the 7th century BC overlaps with the appearance of East Greek pottery on the Israeli coast. There is hardly any doubt that the entire coastal plain up to Phoenicia should be considered Egyptian domain. In these circumstances it is reasonable to assume that Egyptians would not have allowed the uncontrolled establishment of East Greek emporia on the Southern Levantine coast, just as they did not allow it in Egypt itself. While Phoenicia proper and the areas to the north might have enjoyed East Greek trade during the Egyptian interlude, the evidence collected so far from the southern part of the Eastern Mediterranean points mainly to East Greek mercenary activity.

The sudden appearance of Greek mercenaries in the East and their employment by the different Near Eastern Powers continues to be a subject of debate. In my opinion, both historical and archaeological evidence suggests that the presence of Greek mercenaries in the region should be explained as an organized movement orchestrated by a central Egyptian authority. These Greeks were not individual mercenary adventurers but were formally garrisoned. I cannot accept the ideas expressed by several scholars that East Greek assemblages point to individual adventurers or small groups of Greek mercenaries pursuing Homeric honour and glory. I dealt with this issue in detail a few years ago, and I intend to expand the discussion elsewhere. Likewise, today I am even more convinced that attempts to attribute the employment of Greek mercenaries to Egyptian vassals, be it the kingdom of Judah or the kingdom of Tyre, should be abandoned.

Most recently, however, Wenning defended his date for the establishment of Mezad Hashavvahu between 600 and 598 BC, under the reign of King Jehoiakim. This is in contrast to Na’aman’s suggestion that the fortress of Mezad Hashavvahu was abandoned in 604 BC, the year in which Nebuchadnezzar II launched a campaign to the Philistine Coast and destroyed Ashkelon. In my opinion, however, Na’aman’s scenario remains the most plausible option. Moreover, I hope I was able to demonstrate that since the abandonment pattern attested at Mezad Hashavvahu points to a ‘planned abandonment without anticipated return’, it fits nicely with the assumption that this Egyptian fortress was intentionally abandoned in face of the approaching Babylonian army.
The historical improbability of Wenning’s scenario, on the other hand, which attributes the employment of Greek mercenaries to Jehoiakim, who was an Egyptian vassal, has already been demonstrated" and there is no need to revisit it here. Likewise, from a strictly archaeological point of view, Wenning’s entire case rests on the presence of a single pottery sherd he attributes to the North Ionian Late Wild Goat style. Even if we assume that the sherd has been identified correctly, Wenning’s belief that it cannot be earlier than 600 BC is untenable. The East Greek pottery chronology for this period, with its approximate dates, rests on synchronisms with Palestinian destruction levels and on synchronisms with Corinthian and Attic pottery. It is simply impossible to assume such precision (+/− 4 years, which is the difference between Wenning and myself) in dating this North Ionian East Greek sherd. In terms of absolute chronology, both the East Greek pottery and the local pottery from Mezad HaShavyahu may be placed either in the late 7th or in the early 6th centuries BC. Therefore one must consider the broader historical situation.

In support of his thesis, Wenning cites Niemeier’s response to my treatment of the finds from Mezad HaShavyahu. Niemeier’s critique, however, is confused. First he concurs with Wenning that ‘Mezad HaShavyahu was erected by King Jehoiakim during the brief period of possible Judahite autonomy after 600 BC and was abandoned when Nebuchadnezzar II attacked Judah in 598/97 BC.’ On the next page, however, he contradicts himself, claiming that the pottery assemblage at Mezad HaShavyahu may be interpreted ‘as evidence that Greek mercenaries were in the service of Egypt at the site, since the Egyptian army was the only army in which large units of Greeks served.’

The main issue in Niemeier’s reply, however, is to reject my suggestion to attribute the presence of the Greek garrison at Tel Kabri to the Egyptian administration, since, according to Niemeier, these Greek mercenaries were in the pay of Tyre. Niemeier’s conclusions are based on two assumptions: first, that after Assyrian withdrawal Tel Kabri belonged to Tyre; and second that the small proportion of Greek pottery found at the site points to individual soldiers of fortune pursuing Homeric values. Even if the first assumption is true, it would simply imply that the kingdom of Tyre, like the kingdom of Judah, was required to provide supplies to Egypt’s East Greek mercenaries. Likewise, Niemeier’s second assumption is hardly defensible. The proportions may be misleading, since only a small portion of the Late Iron Age fortress at Tell Kabri was excavated. Besides, it is not necessary to deduce that a small proportion of Greek pottery should represent individual adventurers on behalf of Tyre rather than a small contingent stationed by the Egyptians.

All in all, it appears from the archaeological record that dependent local powers were obliged to provide supplies to Greek mercenary units, and to cooperate with these Egyptian representatives in every possible way. The rationale behind the establishment of the fortresses at Mezad HaShavyahu and Tell Kabri is logistical. These and, most probably additional hitherto undetected fortresses, served as focal points for collecting supplies for Egyptian troops on their way to the Lebanese coast and northern Syria and, no less important, on their way back to Egypt. More important, places like Mezad HaShavyahu, where East Greek mercenaries co-existed with Judahites, definitely offered points of direct contact, and provided channels of cultural exchange through which certain Greek ideas penetrated into Judahite texts and vice versa. But the employment of East Greek mercenaries was an Egyptian prerogative, not Judahite or Tyrian. And this is where we find the Lydian connection.

The crucial role played by the Lydians with regard to the thousands of Ionian and Carian mercenaries hired by Psammetichos I emerges from the Rassam Cylinder, in which Gyges, King of Lydia, is accused by Ashurbanipal of having sent his army to the aid of Psammetichos I. It appears that the first Mermnad ruler might have imprudently challenged the Assyrians during the reign of one of the most powerful Assyrian kings. In my view, Lydian imperial policy triggered a sudden explosion of East Greek activity in different directions.

Space constraints prevent me from addressing this issue at proper length but I intend to do so elsewhere. I think, however, that there are good reasons to suspect that, contrary to scholarly consensus, which connects the dispersion of Ionians abroad with an aggressive Lydian and later Persian policy toward the Ionian cities, it is cooperation rather than confrontation that we are witnessing here. In the East, via Egyptian connections, Lydian imperial ambitions opened the way to Greek mercenary penetration, followed by the establishment of Naukratis. In the North, it opened the way to the Ionian colonization of the Black Sea, which, I believe, is better explained in the context of rising Lydian imperialism. The role that East Greeks played on behalf of Lydian domination is much the same as that played by the Phoenicians on behalf of the Assyrians.

The negative view suggested by Herodotus’ remarks regarding Ionian enslavement, first by the Lydians and later by the Persians (Hdt.1.6; 1.169), is somewhat misleading, since, archaeologically, these are the most prosperous periods in East Greece, at least until the Ionian revolt. This is quite contrary to the situation observed during the period of Athenian domination. Besides, there is little doubt that Herodotus’ biased account on this issue, addressed mainly to a mid-/late 5th-century-BC Athenian audience, reflects the realities and perceptions of the time of his writing, rather than genuine states of affairs in earlier periods.

Summarizing the third period in my provisional schema, I wish to emphasize that from the second half of the 7th century BC, East Greece, via Lydian mediation, rediscovered Egypt and then, during a brief period of Egyptian expansion toward the end of that century, the rest of the Eastern Mediterranean. But it is East Greece that was involved in both mercenary and trade activity in the Eastern Mediterranean. For mainland Greece the Great Divide was still there. Even in the later period, during the reign of Amasis, when we hear of an Aiginetan presence in Naukratis, the Aiginetans, being the sole representatives of a broadly taken mainland Greece, ‘did set up separately a temenos of Zeus on their own initiative’. What can we learn from the fact that the Aiginetans were excluded from the Hellenion, which was established by Ionians, Dorians and Aeolians in a very unusual act of early Greekness? Is it possible that the common denominator behind the mixture of the poleis that participated in the establishment of the Hellenion has more to do with the fact that all of them were located in East Greece? Whereas for the Samians and Milesians, who also kept their temene separately, a good case can be made that their presence in Naukratis goes back to the late 7th century
ac, it would be hard to postulate the same for the Aiginetans. Perhaps what we are witnessing here is not an all-embracing pan-Hellenism but rather the crystallization of an East Greek identity, dictated by geography?

**Fourth period: the Neo-Babylonian Empire**

The Neo-Babylonian period is characterized by a total lack of Greek material in the southern part of the Eastern Mediterranean. During the major part of the 6th century BC, the period of greatest prosperity at Naukratis, this part of the Levant, except for a few inland areas, is in ruins, chiefly serving as a buffer zone with Egypt. In the northern part of the Eastern Mediterranean, there is a settlement gap at the site of Al Minā. However, a good quantity of 6th century East Greek pottery found at Tell Sukas suggests that it may have served as a point of contact. This notion, however, should be accepted only with hesitation, since it is possible that the majority of East Greek material can be dated to the last two decades of the 7th century BC/very early 6th century BC, implying that the main phase of the Greek presence at Tell Sukas may have started during the period of Egyptian political domination, slightly overlapping with the beginning of the Neo-Babylonian rule. After a certain gap in the settlement’s history during the better part of the Neo-Babylonian period, the next phase of the Greek presence at Tell Sukas may be pushed into the last third of the 6th century BC, implying that it should be viewed mainly as the result of Persian rule and not necessarily Neo-Babylonian. This issue, however, deserves additional study.

**Fifth period: the beginning of Persian domination**

The fifth and final period in my short overview begins with the end of Babylonian and the beginning of Persian rule during the last third of the 6th century BC. A significant difference (that finds expression in the pottery repertoire) must be noted between East Greek assemblages from the end of the 7th century BC and the renewal of East Greek imports observed toward the end of the 6th and during the 5th centuries BC, which may point to commercial activity. This time, unlike in the earlier period, there is an abundance of amphorae made in Chios and Samos (but other localities are also represented) as well as banded bowls. The distribution is considerably wider than during the third period. During the 5th century BC, East Greek pottery is gradually replaced by Attic imports. Properly appreciating the nuances of the Persian period, however, would require a separate study well beyond the scope of the present endeavour.

**Greeks and the Eastern Mediterranean during the Iron Age: some final observations**

Nowadays, no scholar would even imagine reconstructing the history of Greece without considering oriental influences. And, to my mind, the only way to understand the genesis of Greek civilization is by putting it into a broad geo-political context: it is the western periphery of the East. However, I also think that making everything that has emerged on Greek soil ‘a gift from the East’ simply misses the point. If, as many modern scholars want us to believe, the impact of Eastern civilizations and influences was so total and tremendous, how and why did the ancient Greeks manage to produce the idea of the polis, a community of equal, local-born men, which stands in total opposition to everything which the East symbolizes?
Mediterranean, although even there it too often serves modern political agendas rather than unbiased historical interpretations.

Our case is Janus-faced: on the one hand, at least until the beginning of the Persian Empire, the great powers of the Near East show little interest in Greek affairs; on the other hand, even in the periods of Greek exclusion from the Near Eastern milieu, the challenges posed by the older civilizations, and a variety of Greek responses to these challenges, continue to be among the central factors in shaping Greek identities. In many ways these influences were turned inward, negotiated among the Greeks themselves as they attempted to make sense of the East. In this regard, the concept of ‘negotiated peripherality’, developed by Nick Kardulias and adopted by Ian Morris for Iron Age Greece, is especially helpful. Morris argues for a nuanced and chronologically sensitive approach that takes into consideration a plethora of Greek responses to Near Eastern challenges. In his reconstruction the ‘totality of context’ is prominent, since chronologically different geo-political configurations yielded distinct Greek responses. Morris also convincingly shows that these responses, triggered by the renewal of contact with the East, varied significantly among different Greek communities: some struggled to preserve the model of isolation, while others embraced the East. The basic premises of Morris’ approach are reasonable. Nevertheless, in view of the low chronology in Israel, they need to be modified in a way that emphasises Eubeoan agency in the initial establishment of contact, rather than Phoenician (see above). And Morris also fails to recognize, like so many others, the significance for Greeks of the Great Divide.

The Mediterranean was indeed, as Morris suggests, ‘a smaller place in 700 than it had been in 800’. However, despite the assumed ‘collapse of distance’ (due to the technical advances in shipbuilding), the Great Divide resulted in the gradual exclusion of mainland Greece from the Near Eastern koine and paved the way for a re-negotiation of Greek peripherality. I cannot discuss here all the possible consequences of the geo-political disengagement between mainland Greece and the Near East after the Neo-Assyrian expansion. As a telling example, however, one may consider the widespread appearance of domestic ‘Hero and tomb cults’ in late 8th century bc mainland Greece. Indeed, even if the initial occurrences of ‘tomb cults’ may be projected into the Proto-geometric period, it doubtless remains a salient feature of the Late Geometric period. One is tempted to ask therefore, what are the reasons for such a sudden obsession with ancestors and local heroes? How does it happen that only toward the end of the 8th century bc, Greeks everywhere begin to rediscover and admire their local past, attaching themselves to mythical ancestors and heroes? Many of the wide variety of explanations already offered have merit, but the concept of a Great Divide, as suggested here, may provide an additional, explanatory background for the sudden emergence of an active quest for local roots. Once again, it is a diversity of inwardly focused Greek responses – this time to the exclusion from the Near Eastern koine – that we are witnessing. It is worth mentioning that unlike what will emerge as a poleis zone, with its Eastern influences and abundant orientalia, the ethne, which were never truly involved in dialogue with the East, showed no interest in hero and tomb cults in the periods discussed.

In my opinion, it is plausible to suggest that establishing ties with a remote heroic past rather than with the East should be viewed as one of the main outcomes of the Great Divide. Furthermore, it is not at all improbable that the rise of what Morris calls the ‘middling ideology’ in Archaic Greece, culminating eventually in Athenian democracy, should be seen and explained against the background of this Great Divide. To a certain extent, this might be a real ‘Near Eastern gift’ contributing in the most important way to the rise of the Greek polis and its institutions. If things had turned out differently and, as in previous periods, the elites of mainland Greece had maintained their links with the East, the ‘middling ideology’ would not necessarily have won. However, given that the Assyrians seem not to have had any interest in establishing direct control over remote Greece, a Great Divide was very nearly inevitable.

I want to conclude by pointing out that from the end of the 8th century bc until the Persian period the ‘mainland Greeks’ are barely if at all attested in the Near East. East Greece, the main mediator between East and West, is another story. But to my mind, at least during the Archaic period, it should be considered more a part of the East than a part of the West. East Greeks fully experienced this dual status. Physically they lived in the East, and were part of the Eastern milieu. But, in part because of proximity they had constant contact with their mother country and this and only this prevented East Greeks from losing their ethnic and cultural identity altogether. This was otherwise a very real possibility: we need only recall the complete assimilation of the Philistines, who, in a much earlier period, penetrated too deeply into the Levant.

Notes

1 I am grateful to Udo Schlotzhauer and Alexandra Villing for their kind invitation to attend the 28th British Museum Classical Colloquium ‘The Naukratis Phenomenon: Greek Diversity in Egypt’. Likewise, I wish to express my gratitude to numerous scholars who have offered valuable comments on an earlier draft of this paper, including John Boardman, Margalit Finkelstein, Israel Finkelstein, Baruch Halpern, Peter James, Amélie Kuhrt, Ira Malkin, James Muhly, Benjamin Sass, Oren Tal, Alexandra Villing, Ran Zadok and especially Ephraim Lytle. Obviously, the responsibility for the views expressed henceforth rests with me alone.

2 In Lipton’s (2004) famous treatment of the ‘Inference to the Best Explanation’, this kind of explanation may be considered as the ‘likeliest’ and the ‘loveliest’.

3 Trigger 1998.

4 Joffe 2003, 82.

5 Morris 2003, 42.


7 Horden and Purcell 2000; see also Purcell 2003; Horden 2005; Horden and Purcell 2005.

8 Horden and Purcell 2000, 5.


10 Blake 2004, 240.


12 Shaw 2004, 453.

13 See, e.g., Shaw 2001; Morris 2003; Malkin 2003a, 2004; and see papers in Blake and Knapp 2005.


15 Morson and Emerson 1999, 125-7; Joyce 2002, 29-34.


17 Kocka 2003.


19 Liverani 2005a, 48.

20 Moreland 2000, 2, emphasis in original.


22 A number of studies offer useful summaries regarding the earliest Iron Age finds of Greek pottery in the Eastern Mediterranean: e.g.,
For the demolition of a long-standing scholarly consensus that the dispersion of Cypriot Black-on-Red pottery in the Aegean should be connected with a Phoenician monopoly of commercial networks, see Schreiber 2003, passim, esp. 312.


For 'Report of Wenamun' as a piece of literature rather than historical account, see Helck 1986; Baines 1999; Schipper 2005; for the date of composition, see Suss 2002, with further references.

For the numerous supporters of Phoenician domination in the Mediterranean already at the beginning of the Iron Age it may perhaps come as some surprise to discover that the biblical testimony regarding the cooperation between Kings Solomon and Hiram I does not reflect the realities of the 10th century BC, a fact that has been recognized for some time. The literature on the subject is enormous; see e.g. Druckers; Finkelstein and Silberman 2001, 2006, with further references.

Needless to say that the same holds true regarding the Classical literary tradition, which suggests that the foundation of Cadyz, Utica and Luxus took place at the turn of the 12th/11th centuries BC.

After Finkelstein 1995a, 1996, 1999. Whether or not to accept Finkelstein's low chronology is still a subject of ongoing discussion, mainly based on a re-evaluation of the Palestinian archaeological evidence. The literature is extensive and I do not intend to summarize the history of the question here. But judging from the most recent publications, the so-called conventional Palestinian chronology, with a huge United Monarchy of Kings David and Solomon as well as early Phoenician expansion in the days of Hiram Iis, at least to my mind, doomed.

Fanthakin (forthcoming a). That is not to deny the existence of some meagre pre-colonial contacts with places like Cyprus, and see Gilboa 2005.

Bikai 1983.


Docter et al. 2005; Nijboer 2005, with further references.

Aubet 2005, 372-81; Torres Ortiz 1998, 2005. The recent suggestion by Nijboer and Van der Plicht (2006), that the beginning of Phoenician settlement activity abroad may be pinpointed to the first half of the 9th century BC, if not before, is barely defensible, as it is based on a few ^14C dates obtained from a secondary mixed deposit at Huelva (south-west Spain).

For detailed accounts of Hazael's realm, see Na'aman 1995a; Dion 1997, 191-204; Yamada 2000, 310-20; Hafthorsson 2006.


See Na'aman 2000; Coldstream and Mazar 2003; Finkelstein 2004.

Maeir 2004.

Fanthakin and Finkelstein 2006, 30-2.

Frankenstein 1979.


Fanthakin 2001a; Coldstream 2003. The most recent suggestion that the Proto-Geometric period should start c. 1100 BC, if not earlier (Newton et al. 2005a, 2005b), is impossible to sustain. Such a drastic upward chronological revision for the Proto-Geometric period, based on the data from Assiros, is unacceptable as it stands against all other data collected in the southern Levant. Besides, the Proto-Geometric amphora in question is not necessarily correctly identified and may belong typologically to Submycenaean or even Late Helladic IIIIC (cf. Muhly 2003, 28). Likewise, the old wood effect may be responsible for the high dendrochronological dates from Assiros (Finkelstein and Piasetzky [forthcoming]).

Following Muhly's original suggestion from 1985 (unlike Muhly 1999).


Cf. Coldstream 1998a. Although I tend to agree with Boardman (1999c, 42) that the question of 'who was first?'... seems quite meaningless, indeed almost childish', it has never disappeared from view and remains the subject of continuous controversy.

Saggs 2001, 166-7, pl. 33.

The appearance of East Greek pottery in Levantine assemblages to be crucial. In fact, the destruction of Ashkelon by Nebuchadnezzar II in the month of Kislev 604 bc, as reported in the Babylonian Chronicle (Wiseman 1961, 68-9, 85; Stager 1996, 61, n. 1) and the East Greek pottery assemblage exposed in Ashkelon's destruction layer (Waldbaum and Magness 1997; Kerschner and Schlörhauer 2005. Fantalkin 2001b, 143-4).

The location of Mezad H. ashavyahu in the vicinity of the natural anchorage of Yavneh-Yam (cf. Galili and Sharvit 2005), supports Na'amans 1991a, 51) suggestion that Necho II and his army may have sailed as far as the Lebanese coast and launched campaigns from there. In this regard the increasing importance of the naval forces under the Saïte Dynasty should definitely be emphasized (cf. Lloyd 1972).

Finkelstein 2002.

It is a coincidence that Ionia's cultural renewal, which is sometimes called 'the Ionian Renaissance', started in the 4th century bc, mainly after the 'King's peace' in 387 bc. (cf. Isag 1994; Pedersen 2004; Lawall 2006.

As may be deduced from Hall 1997, 49-50 and Malkin 2003b. It should be noted that earlier scholarship tends to be more sympathetic to 'Barbarian Asia' when describing the relations between the coastal Ionian cities and the Lydian and Persian empires, cf., e.g., Radet 1893; Hogarth 1909, 78; 1929; Lenschau 1913; Dunham 1915, 79-6; and more recently, Balcer 1994, Georges 1994, 2000; Buxton 2002; Burkert 2004.

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Oriental craftsmen working as long-term residents on Aegean soil is well-founded (Muhly 2005).

133 Snodgrass 1980.
134 Raaflaub 2004b.
135 See e.g. Thornton 2000; Boardman 2005.
137 Sherratt and Sherratt 1998, 335; and see also Sherratt 2005, 36.
138 Kardulias 1999.
139 Morris 1999.
140 See also Morris 2000, passim; Whitley 2001, 102-23.
141 Morris 2000, 257.
142 Mazarakis Ainian 1999.
143 See e.g. Coldstream 1976; Antonaccio 1995; Mazarakis Ainian 1999.
145 Antonaccio 1995, 254. Except for a few insignificant cases, see Morgan 2003, 187-95.
147 Cf. Sahlins 2005, who convincingly demonstrates that the intensification of any one opposition is likely to engage and aggravate all the other antagonisms. That is to say the small-scale initial disputes may easily be magnified into large-scale struggles between nations and kingdoms, making macrohistories out of microhistories and vice versa.
148 For a general framework of counterfactual approach, see Tetlock and Belkin 1996; Ferguson 1997.


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Naukratis: Greek Diversity in Egypt

Studies on East Greek Pottery and Exchange in the Eastern Mediterranean

Edited by Alexandra Villing and Udo Schlotzhauer
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This volume has its origin in a workshop on Naukratis and East Greek pottery held at the British Museum in December 2004 as the 28th British Museum Classical Colloquium, the result of a collaboration between the British Museum and members of the Naukratis Project of SFB 295 at the Gutenberg-Universität Mainz. Made possible by the generosity of the Gerda-Henkel-Stiftung and the Caryatid Group of the British Museum’s Greek and Roman Department, to whom we extend our gratitude, the workshop brought together archaeologists, historians and scientists with the aim of generating a fruitful discussion and exchange of ideas and knowledge to further our understanding of the site of Naukratis in its wider, Eastern Mediterranean context. As it emerged, the scientific analysis of pottery samples taken both at the British Museum and elsewhere proved particular vital for many results presented here. To a large extent this was made possible by subsidies from the Deutsche Forschungsgemeinschaft, by the personal interest of Professor Hans Mommsen of the Helmholtz-Institut, Friedrich-Wilhelm-Universität Bonn and the various other individuals, excavations and institutions that allowed material in their care to be analysed, and by the generous help of the staff of the Department of Conservation, Documentation and Science of the British Museum, notably Mike Cowell and Duncan Hook.

As editors, we have greatly enjoyed working with such knowledgeable, reliable and responsive colleagues as have come together for the present volume. The collaborative spirit that pervades the volume has its roots in the stimulating discussion and collaborative ambience of the workshop, which led to further exchanges well beyond the confines of the actual gathering. We are grateful to all participants, who made it such an exceptionally productive experience. The contributions assembled in this volume reflect this ongoing research and discourse, which has helped the volume to be, we hope, not just a gathering of individual papers but more a thematically linked whole.

Many people have contributed to making the workshop, the related research and this volume possible. On the Mainz side, we would like to thank in particular Sabine Weber (Mainz) for her vital input in the workshop and related research, and Ursula Höckmann and Detlev Kreikenbom (Mainz), Naukratis project leaders within SFB 295 – Kreikenbom for his support in organising the financing of the workshop, and Höckmann for much help and constant openness to discussions.

On the British Museum side, we would like to thank in particular Dyfri Williams, Keeper of the Greek and Roman Department, for making the workshop possible and for his unfailing support throughout; all colleagues in Greek and Roman Department and the Educational AV unit for help with organising the workshop; colleagues in the Department of Ancient Egypt and Sudan, especially Jeffrey Spencer and Neal Spencer, as well as in the Middle East Department, for helpful discussions and access to objects; Lesley Fitton, Susan Woodford, Mira Hudson, Bábara Freitas, Sara Cambeta and Sotiria Papastavrou for help with proof-reading; Kate Morton for producing two wonderful new maps and several profile drawings; the British Museum’s Photography and Imaging Department, especially Dudley Hubbard, for producing new photographs of objects at short notice; Lindy Crewe for help with image editing; John Boardman for encouragement and the donation of his invaluable Naukratis archive to the British Museum; and last but not least Josephine Turquet for producing the volume sympathetically and efficiently as ever.

Editorial note
For Greek names a Greek spelling has been retained wherever it was deemed not too unusual for the eye, which invariably means there will be considerable inconsistencies (such as Klaazomenai and Aiolis but Cyrene and Laconia).

A joint bibliography can be found at the end of the volume. Journals have been abbreviated after the guidelines of the American Journal of Archaeology. Some additional abbreviations are used, such as NAA for neutron activation analysis. Stylistic phases in the development of East Greek pottery from various regions have been abbreviated (e.g. as NiA I = North Ionian Archaic I; MileA II = Milesian Archaic II) according to the new system set out in Kerschner and Schlotzhauer 2005.

The order in which the contributions are arranged was in part determined by the practical necessities of printing the colour sections.