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Understanding Relations Between Scripts II
Early Alphabets


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Chapter 5

Much ado about an implement! – the Phoenicianising of Early Alphabetic

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‘I must profess I know enough to hold my tongue, but not enough to speak.’
Elias Ashmole, 1652

Introduction

Since the days of William Foxwell Albright (1891–1971) a great many scholars have published about the early alphabetic writing of the second millennium BC and the labyrinthine paths by which it developed into the alphabet now called the ‘Phoenician’. Much has been asserted about matters concerning which we do not have nearly enough material to say anything for sure, and even now there is a running debate about how to reconceptualise the earlier phase(s) of the Levantine alphabetic scribal culture from ‘Early Alphabetic’ up to a presumed ‘Phoenician’ writing, and from the Late Bronze to the Early Iron Ages and beyond (Sass 2004–2005; Goldwasser 2011, 2015, 2018; Finkelstein and Sass 2013; Hamilton 2014; Sass and Finkelstein 2016). Yet sometimes one gets the impression that new findings call previous evidence to be updated faster than new theories can be established.  

1 Elias Ashmole, Theatrum Chemicum Britannicum, 1652, Prolegomena, 5 ff.
2 Cf. also Hamilton 2014, 30: ‘The scholarly consensus about the periods of early alphabetic scripts has virtually collapsed during the last decade.’ For example, Sass and Finkelstein 2016 themselves chased after a new idea on the basis of another new, tiny two-letter-fragment, revoking what they wrote only few years before (Finkelstein and Sass 2013): the later article is filled with ‘we now realise’s, ‘we now believe’s and similar corrections (2016, 26, 27, 28, 30, 39, note 22). This, to be sure, is not at all to blame the authors! Rather it is to illustrate the current state of research and scholarly discourse.
'Early Alphabetic'

The term ‘Early Alphabetic’ was coined by Albright in 1948, and has since then become established as a concept of its own. In fact, besides a conventionally set time frame from c. 1900–1000 BC there seem not to be any useful definitions of what ‘Early Alphabetic’ is at all.

The small corpus of known early alphabetic writings [...] consists of the so-called ‘Proto-Sinaitic’ inscriptions (early alphabetic writings recovered from the turquoise mines and temple complex of Serabit el-Khadim and the nearby wadis) and the handful of other early alphabetic inscriptions from Palestine. (Darnell et al. 2005, note 5)

The starting point of ‘Early Alphabetic’ is simply given by the oldest alphabetic inscription(s) known at any given time, which for Albright still was the Sinai/Serabīt el-Khadim inscriptions alone but is currently also set by the Wadi el-Hūl inscription (Darnell et al. 2005). At any rate this was some time in the early second millennium BC and somewhere in Egypt, the Sinai or even in the southern Levant (see Haring, this volume).

The lower end of an ‘Early Alphabetic’ phase, however, is a different and fuzzier question, the answer to which sometimes gives the feeling of being rather arbitrary. The simple but crucial question however is as follows: what point is the Levantine segmental system which we call ‘alphabet’ no longer early-looking enough to be ticketed as ‘Early Alphabetic’? In other words: what made alphabetic writing ‘early’, or by changing what features does it lose its ‘earliness’?

The most prominent distinction was developed in the footsteps of Albright in a number of publications by Frank Moore Cross (Cross 1954, 1979, 1980, and more; Hamilton 2014, 30). Cross distinguished a Proto-Canaanite or Early Alphabetic from an Early Linear Phoenician phase, the latter starting around 1050 BC – which was simply a date set by and shortly after the most commonly accepted date for the Ahīrōm (Ahiram) sarcophagus inscription(s) (Lehmann 2005, 2008b, 2015) as a demarcating border line. Yet this is no longer as easy as it seemed to be less than half a century ago. In more recent research, the date of the Ahīrōm inscriptions has been much more controversial (most recently Sass 2017). Although I myself made the most recent critical re-edition of the Ahīrōm inscriptions, I nevertheless refused to join in the often all-too-optimistic date stamping of Ahīrōm, which ranges from the thirteenth century up to 850 BC. Albright first set it at c. 1000, Edith Porada (1973) and Ellen Rehm (2004) tried to raise the date up to the thirteenth century on the basis of its decoration and other art-historical factors, while Ronald Wallenfels (1983) and Benjamin Sass (2005) from a more archaeological and palaeographic point of view made efforts to lower its date down to the mid-nineteenth century. Anyway, in most of these approaches it appears that the majority of arguments for dating the Ahīrōm sarcophagus inscription use more features of the coffin itself and its art and decoration than of scribal or content-related aspects. However, even if one still adheres to a conventional middle date of c. 1000, there remain obstacles to taking the date of Abīrōm as the the general watershed time for Early Linear Phoenician.

First, alongside the Ahīrōm sarcophagus inscription (KAI 1) as a supposed chronological pivot, there is the often disregarded inscription on the tomb’s shaft (KAI 2) – which, from a technical point of view, is clearly not a graffito but a chiselled incision (Lehmann 2005, 39–54). According to the long-standing and most conventionally accepted (but nevertheless questionable – see Sass 2017, 116 ff) ‘Albright–Cross–Harvard’ scale, however, the shaft inscription appears to be typologically older in stance than the sarcophagus inscription(s). Nevertheless, it has been much overlooked for the last decades in any debate about Early Alphabetic. Taking the Ahīrōm shaft inscription into serious consideration may, however, cause some turbulence within the time frame of later ‘Early Alphabetic’ – depending on the assessment of an inner chronology of both the Ahīrōm sarcophagus and the shaft inscriptions ensemble altogether (Lehmann 2005, 53–54).

Second (the shaft inscription aside), taking the Ahīrōm sarcophagus inscription(s) as a typological or palaeographic border line only makes sense under the premise that its script was the landmark of an outstanding scribal innovation within a narrow time slot of some fifty years. This, however, cannot be proven and is not even probable. Rather, as I demonstrated some years ago (Lehmann 2008b, recently accepted by Sass 2017), an in-depth calligraphic analysis of the sarcophagus inscription shows that it already must have had a presumably multigenerational tradition of calligraphic know-how and experienced flat writing craftsmanship in its background. This either means

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3 Albright 1948, and see Hamilton 2002.

4 See, for instance, Healey 1990.

5 Or 1900–900 as, for instance, Hamilton 2010, 1, similarly also Hamilton 2015, 127.

6 For instance, Nieszolewski-Špano 2007, 175: the inscriptions of Ahīrōm and Yehimilk mark the border line after which a relatively uniform Phoenician script in Canaan quickly becomes clear and their date around 1000 BC suggests that the creation of Phoenician script must be dated at ca. 1050 BCE. It means the terminological division of earlier forms of script, called proto-Canaanite or Canaanite (to which group the Sinaitic inscriptions belongs) and later simply Phoenician’ (emphasis mine).
that the ‘set date’ 1050 by Cross must be lowered some 100 years (much welcomed by Wallenfels or Sass) or that we are to raise the end of ‘Early Alphabetic’ and the beginning of linear Phoenician by some generations, i.e. by some hundred years or so, into the end of the eleventh century at least. For the time being my impression is that a consensus is not possible, at least not that way.

Millard summarised the crucial objection to granting the Aḥīrōm and other Byblian inscriptions a central position in early alphabetic history (2012b; 411):

Byblos is best known for inscriptions in the early Phoenician alphabet […]. That alphabet already existed before the 12th century BCE and presumably arose in Phoenicia. The fact that it is best attested at Byblos may be an accident; it could have been current simultaneously in other Phoenician centers, so allowing for local variations, as the later Phoenician alphabets show. Therefore, when an inscription is discovered, the Byblos inscriptions deserve to be used for comparison, but they should not necessarily become the paradigm. Furthermore, the fact that they were engraved on hard surfaces may mean they do not reflect precisely the current hands of scribes using cursive script with ink on papyrus or leather.

If we dispense with a fixed date, it seems most feasible to define ‘Early Alphabetic’ as every alphabetic writing which is not yet discernible as Phoenician.

‘Phoenician’

‘Phoenicia is an anachronistic term’ (Schniedewind 2013, 54).10 ‘Phoenicia’ accounts for a conventional concept that derives and reproduces itself as an heirloom of the western reception of antiquity11 – and so is ‘Phoenician script’. There was no Phoenicia. There is not, and never has been, any ethnic, political or ‘national’ entity that understood or labelled itself as ‘Phoenicia’, nor has there ever been any Phoenician people. Rather, to say it more pointedly with the late Brian Peckham (2014, 559):

Phoenicia was a Mediterranean state of mind, it created a world it could fill, mapped it, and outlined it loosely.

The idea of Phoenicians, the blue-and-red dye-maker merchants of the Levant, is a modern construct, rather a modern identification than ancient identity. And like all constructed identities in history, it is methodologically flawed:

• exclusively sinistrograde writing direction (abandonment of dextrograde and boustrophedon)
• reduction to eventually 22 letters (abandonment of long-alphabet devices)
• ‘ʾabgad order (abandonment of halaḥam or other sequences)
• fully linearised letter forms (abandonment of pictographs)

Since the current hands of scribes using cursive script with ink on perishable material like papyrus or leather.

Religious affiliations, mythical origins and ethnic identities are human constructs, and we simply falsify history by fathering on peoples in the past identities which they did not construct, or had not yet constructed, for themselves. (Millard 1993, xix)

Nevertheless, like ‘Early Alphabetic’, ‘(Early) (Linear) Phoenician’ too has long since established itself as a concept of its own. There is still a tenacious adherence as to the term ‘Phoenician’ as well as to the term ‘Phoenician script’. This derives in large measure from the work of Frank Moore Cross, who for almost half a century advocated the grammatical nomenclature ‘(Early) (Linear) Phoenician’. The term gained a certain notoriety because the available data according to Cross (1980 [= 2003, 213–230]) allows for the most likely assumption that the ‘Hebrew script’ was derived from an ‘Early Linear Phoenician alphabet’.

Thus, in palaeography and historical graphematics we are long since much too accustomed to tracking down a seemingly uniform, not-yet-diversified-but-already-linear, non-pictographic alphabet as ‘Phoenician’. Its commonly accepted parameters, following Cross and the late Joseph Naveh, are defined as:

10 Or: ‘Phoenicia, like all history, is a construct, a product of historiography, an answer to questions, a companion of interest’ Peckham 2014, xix.
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But in more recent research, and triggered by findings of the last decades, all this seems rather less simple. Anyway there is the question left over: why ‘Phoenician’ at all? Is it not true that this script series was also used by carriers who were not seen as ‘Phoenicians’ by anyone, as for instance in Azatiwata’s Luwian stronghold Karatepe-Aslantaş in Cilicia, which he himself called Azatiwadya?12 Calling this and other examples ‘Phoenician script’ – though their language is undisputably Phoenician-Canaanite – might falsely suggest that some until then underdeveloped and formerly illiterate population somewhere had simply adopted the ḥeqh ɣrammatw, the writing practice of people who were allegedly literarily superior, as for instance the Byblian, Tyrian, Sidonian and other Levantine folk that might have called themselves ‘Phoenicians’ – which is not true at all. There is no unambiguous evidence for the export of ‘Phoenician’ script into underdeveloped or maybe illiterate regions and populations. In any case it was not simply ‘Phoenician’. And there is no basis for the romantic-and-monocausal notion that Hebrew-Israelite, Moabite and other scribes had adopted just ‘the script’ of just ‘the Phoenicians’ (whosoever these might have been) before eventually something like their own regional or ‘national’ (if ever one might use this term) script ductus evolved.

12 Çambel 1999. For the very best photographs, see Çambel and Özyar 2003. For the phenomenon of multilingualism (and multigraphism) in Cilicia see Payne 2007.
Labelling a certain manifestation of ‘the’ early alphabet as ‘Phoenician’ is a mere aftermath of modern antiquities reception that has viewed and still often views these oriental Levantine folk, the ‘blue-and-red-dye-dealers’ from eastern shores, through the glasses of the Greeks, ōi παραλόγον ἄνδρας ἐκεῖ τινα τῶν Φοίνικων τὰ γράμματα – ‘who took up the letters after being taught by the Phoenicians’ – and declared these letters to be called ‘Phoenician’ (Φοινίκης κεκληθεῖται):13 and whom Pliny the Elder later hailed as a gens in ‘magnā gloria litterarum inventóris’ – of great glory (because) of their invention of letters.14 The overpowering force of this tradition and classicists’ reception of antiquity still has impact on both the Western view of Levantine people and the classicists’ appreciation of the alphabet. It also explains why Frank Moore Cross (1921–2012), the grand master of West Semitic epigraphy in the last century, succumbed to the term ‘Early Linear Phoenician’.15

Depending on how we assess finds in any given region, one might argue that what some would call a ‘Phoenician’ ductus or script type per se did not evolve earlier than the eleventh century. But if the adjective ‘Phoenician’ is not apt as an ethnonym at all, we should also replace it when referring to paleography and the history of writing. Even if ‘Early Linear Phoenician’ was only meant as a technical term, ‘Phoenician Script’ and even the German term ‘Phoenician Mutterschrift’ (though used in English, which is even worse)16 have become historically unsound and semantically misleading.17 Incidentally, it is not easy to define a Phoenician language (Röllig 1983, Knauf 1990)nor what the diagnostic feature of Phoenician script is at all. In many cases indeed, namely where documents of what some would call ‘Southern Phoenicians’ (Xella 2017, 153) are concerned, it seems that the main diagnostic feature is more ‘non-standard Hebrew’ linguistics and a ‘non-Yahwistic’ (rather ‘Aštaric’ or ‘Ēšmunic’ etc.) onomasticon than a positive definition.18

And so what about the suitability of a term (‘linear’) Phoenician script?...
(‘Early Linear) Phoenician’ script and similar expressions are henceforth only suitable as an *umbrella term* for a transnational (koine-) alphabetic script standard in the Levant in the late second and early first millennia BC. To escape false subliminal ethnic, ‘national’ or linguistic prepossessions it might be better avoiding labels such as ‘Canaanite’ or ‘Phoenician’, and using instead another, neutral term. For this, Hamilton recently offered the helpful concept of a three-stage model of early alphabetic scripts in the Levant (Hamilton 2014, 30). He proposed three periods of *one continuous tradition* of alphabetic writing to be designated with the most neutral *identifiers* available: Early Alphabetic A, ca. 1900–1400 BCE; Early Alphabetic B, ca. 1400–1000/950 BCE; and Early Alphabetic C, ca. 1050–after 900 BCE (or to the rise of distinctly national scripts). The most interesting period here is his third phase, Early Alphabetic C, which ‘witnesses a direct continuation of the earlier forms of letters but shows considerably less variety in terms of their shapes or stances’ (ibid., 31). The transition from B to C may have been a long, possibly centenial ‘overlapping period’ as a ‘time of transition to a newer way of writing exclusively from right to left in horizontal lines’:

The change from multidirectional to single-directional ways of arranging alphabetic writing probably occurred over the course of several generations; for people living in Canaan during this period of transition, no change may have been perceptible. Nothing abrupt need be envisioned—no supplanting of an indigenous script tradition by that of a close relative; no change in writing styles to imitate scribes in a distant royal court [...]. (ibid., 49 f)

This third phase of Hamilton’s answers largely to Cross’ ‘Early Linear Phoenician’ but evades its factual aporias:

I would change the name ‘Early Linear Phoenician’ to ‘Early Alphabetic C’ to express the continuity of this third phase with preceding periods of early alphabetical handwriting and to avoid any suggestion that there were national scripts late in Iron Age I or early Iron Age II. (ibid., 43, emphasis mine)\(^{19}\)

However, to think about continuity and ‘periods of one continuous tradition’ (above) seems to me a bit too optimistic. As a matter of fact, such continuity cannot be proven, and with new findings this becomes even more improbable.

Rather, in my judgement, the late second- and early first-millennium scribes in Tyre, Sidon, Byblos etc. and in the territories of the later kingdoms of Israel, Judah, Ammon, Aram, Moab and so on acted on the same common *graphemic* platform of something that we may call a ‘Levantine linear koine’, which in principle was the same as of their respective neighbours, whatever the vehicle of its diffusion might have been. This breeding ground of a shared scribal culture was primarily not the outcome of shared educational traditions (which appeared only later as an additional trigger), but lies first and foremost in the rooting of a still very small intellectual elite of linear alphabet scribes in the Late Bronze Age who, as supra-national and trans-lingual freelancing sustainers of a still quasi-arcane knowledge of segmental (i.e. phonemic-alphabetic) writing (which even might have been also a kind of ‘disruptive innovation’, Goldwasser 2018), acted on their own accounts.

This *graphemic* platform could stand (and in fact it did stand) in *individually* different stances and ductūs, i.e. *graphetic* differences that did not interfere with readability within a very small literate elite. Rather, it might have discerned them as mere allographs.\(^{20}\) In the Late Bronze/Early Iron Ages we have a loose and spacious trans-regional, non-hegemonic and even supra-lingual standard. It is not a typological standard set by a local administration or school, rather by means of writing technique. This *technically induced typographic standard* is not yet discernible in Hamilton’s ‘Early Alphabetic B’, but then in nearly all ‘Early Alphabetic C’ inscriptions (Hamilton 2014, 43 f, figs 10–11), in the *punched* inscribed arrowheads,\(^{21}\) Byblos Azorbaʿal-spatula and Tekke bowl,\(^{22}\) and also in some more metal-*incised* (Kefar Veradim\(^{23}\)) or stone-*chiselled* (Ahirōm, Ṣipṭaʿal etc.) inscriptions of what then is called Early Linear Phoenician. Many of them give the impression of being typographically reduced to an image that is put together of homogeneous, uniform-length strokes.

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19 Another term to avoid all ‘national’ and other aporia is what Sass and Finkelstein 2016 recently called ‘post Proto-Canaanite’. This, however, goes along with new aporias that cannot be discussed here.

20 In later times with much more source material available to research, there is sufficient evidence for different but simultaneous states of development, shape, stance, ductūs, and typological ‘normalisation’ or typological realisation even at the same place, e.g. the Samaritan mixed scripts from Mt. Garizim, the Tobiah-inscription from Iraq al-Amir, and more. And there is no reasonable argument for the often tacitly made assumption that the same should not also have been possible in the fledgling phases(s) of a still ‘normalising’ alphabet’s history, i.e. in the late second millennium. Quite the contrary, I suppose. The then still very small elite of freelance scribal guilds did not need to care for tiny differences in stance or length of downstrokes and the like to provide readability for a broader public.

21 Today there are more than 60 inscribed arrowheads known. Because all except one (Ruweisēt 1926) stem from the antiquities market they are, however, to be handled with the utmost care in respect of their authenticity – which I do not assess too highly. However, because, from a technical point of view, some of these are very similar to objects from controlled excavations like the Tekke bowl (Lehmann 2018) or the Azorbaʿal spatula, it is possible to include them here with all due caution as additional (but not as basic!) evidence. Especially the arrowhead-script (if ever there had been something like that, which I consider more mere fancy) is because of its technical conditionality (hailingmarking) no archaic norm but a mere field of allographs; also seemingly early diversifications of letter shape are mostly just technically induced and therefore typologically inoperative and chronologically feeble, as is the case for instance with alev as crossed (k) versus non-crossed (quasi-k) shapes. As a ‘skeleton standard’ however, these are close to the notion of a ‘Phoenician typewriter’.


23 Alexandre 2006, recently also Lehmann 2018.
5. Much ado about an implement! – the Phoenicianising of Early Alphabetic

Most of our knowledge of early Phoenician script is still taken from such inscriptions. The shapes of scores of these are in part determined by technical constraints such as the gauge of a chisel’s blade, the Kefar Veradim inscription, small fragments like the Reḥov sherds and newly discovered but hitherto unpublished Gezer fragments aside. Single or double-length strokes are the normal consequence (exceptions more often than not only made with typologically rounded letters like lamed or ‘ayin) and determine the basic shape of the linear-alphabetically standardised letter-forms, as they are nowadays accessible for us – ‘Phoenician’ only because most of them are found in a territory that the Greeks deemed to be the origin of the Phoenicians.

Accordingly, for example, the identical length of the five and three strokes of mem and nun respectively on the Byblian Azorbaʿal spatula (Fig. 5.1) should not be regarded easily as ‘early features’ (Rollston 2014, 76, the same misjudgement already in Rollston 2010, 20, and passim). Many authors miss, inter alia, that almost all letters of this punched text have almost identical stroke length according to the width of the respective tool’s blade. This chisel or punch was used, as was normal in bronze (and also in certain kinds of stone), for vertical thrust only. Domes are made only where typologically required, as for instance with ‘ayin, waw and, as a surprise, also with yod, strokes are extended by double-thrust as an exception only where it was typologically inescapable (i.e. with pe and lamed). Most interestingly, it was technically not possible to fall below a given stroke length, as can be easily seen by the left horizontal protrusions in het (lines 3.4) and by the bottom crossings of šin (line 2)! This technically induced typological concentration to almost straight-cut strokes in single or double length, which is indeed found in the majority of the early linear Phoenician, i.e. Hamilton-C or ‘post Proto-Canaanite’ (Sass and Finkelstein 2016) inscriptions, was a kind of ‘typewriter-like’ structural simplification. This also became or was the precondition to decode every local or individual figurative idiosyncrasies in letter stance and shape as mere allography of the one letter matrix. This again only made it possible later, in the course of speeding up the writing process, to fan out the ‘Phoenician’ standard into several regional (but not ‘national’) script types.

An intermediate script or a transitional phase?

To me, it seems inadvisable to suppose a ‘mixed script’ (e.g. Finkelstein and Sass 2013 passim) to escape the straits of having too little Iron I material for a differentiation between professional scribes (who may have used different material, as for instance papyrus) and the occasional scrawlers, the output of which we only have preserved in clay.

24 The unpublished Gezer sherds will be edited by the present author in the near future. On the Kefar Veradim inscription, see Lehmann 2018.
25 For different chiselling techniques depending on the type of stone, see Keimer (2015).

There is a most recent tendency, prominently advanced by Finkelstein and Sass to assume from certain sparse data a considerably long transitional phase where some kind of type mixing was en vogue, or was the predominant ‘scribal’ attitude. At the end of the day this involves the presupposition, or existence, of a certain ‘intermediate’ or ‘transitional script’, that could be described by essential, distinct and distinctive, unique features. However, as I understand it, this is not the case. Rather, different unexpected features are found here and there, and do not fit all together in one framed picture within a given time slot. Because nowadays we are accustomed to the stabilised, normative-looking appearance of mostly printed script (but similar even so with mediaeval and older manuscripts), we find it hard to tolerate a less standardised script appearance. Nevertheless such mixed-looking or really ‘mixed-script’ phenomena must not rashly be ascribed to a non-stabilised or barely-literate stage of script typology, since these appear also in realms and periods of obviously higher literacy – though we do not know the exact reasons for this: see, for instance, the Mt Garizim inscriptions from the third century BC, in some of which, palaeo-Hebrew and Judaeo-Aramaic letters are mixed together (Dušek 2007, Stadel and Lehmann forthcoming).

Also, support and confirmation from archaeological data seems to be less useful for the epigrapher and palaeographer than it is for the archaeologist. It is maybe not advisable too, to parallelise ceramic typology and letter typology, which both...
might demand a different approach because of the very dissimilarity of the available material alone. The outcome of the problem is obvious, as stated by Sass and Finkelstein (2016, 28): ‘If ceramic typology points to the end of Iron Age IIA while letter typology favours early Iron IIA, something is clearly amiss in one of these typologies. [...] It would moreover imply a Proto-Canaanite-to-cursive transitional phase of matchless length, roughly four generations, for which we have no parallels and no ready explanation.’

Examples for such inscriptions where the data do not fit together are, from several different points of view, the so-called ‘Rapa’ palimpsest arrowhead (Fig. 5.2) (Martin 1962),26 the Fakhariya bilingual (Abou-Assaf et al. 1982) because of its curled lamed and dotted ayin letters (which for Sass [2005] are reason to label its script as ‘eccentric’ and ‘archaising’), and more besides. The scarcity in particular of earlier epigraphic material, as it is preserved on second-rate (i.e. ceramic instead of papyrus) flat-writing material alone and is used by predominantly second-class non-professional and occasional-only ‘scribes’, would provide enough explanation.

A crucial point here is also how to deal with cursivisation and with the emergence of cursive traits in particular. It is not beyond any reason to conclude that cursivisation of a script is a marker of standardisation or at least of a commonly accepted and applied writing standard – maybe but not necessarily centralised – and maybe also of increasing literacy. At the same time, however, cursivisation technically originates in flat writing. Its spread into incised writing therefore indicates the increase of flat soft surface material (i.e. papyrus). I would not so persistently adhere to a ‘lost papyri theory’. But as a matter of fact it is neither easy to prove that the ‘lost papyri’ once really existed nor to explain why regional graphemic differences first appeared after the rise of cursive standards. At any rate, this is not so easy an issue as it seems at first glance simply because there is no proper definition of what really a cursive trait is, and how many cursive traits in a single inscription are sufficient or necessary to flag an inscription as cursive written.

This is particularly difficult if only two broken letters are preserved, as is the case for instance in the Megiddo sherd featured by Sass and Finkelstein (2016), who nevertheless claimed it as cursive: ‘the two Megiddo letters belong to the earliest cursive or “post Proto-Canaanite” phase of the West Semitic alphabet, at the same time still flipped left to right, a last relic of the waning Proto-Canaanite tradition’ (ibid., 24). What really is cursive with these two fragmented letters must remain an open question.27 Only a meagre definition is given by Sass and Finkelstein, though cursivisation seems to be an important issue (2016, 30): ‘Cursive or “post Proto-Canaanite” characteristics include for instance the acute angles and/or inclination towards the next letter of he and het, and the lengthened downstrokes of mem, nun and samek.’

So one is forced to admit that there is a certain vagueness even in the argument. At least one can say that cursivisation is an effect of speed-up in writing that causes – depending on the writing angle – either a flattening of letter-heads or a lengthening and curvature of downstrokes, or both. However, it is not possible to treat every flattening or lengthening of a letter as a cursive trait. The problem arises if it is not an ink-written but chiselled (in stone) or punched (in metal) inscription, which by definition cannot be cursive in itself but only display a reverberation of cursivisation effects which had taken place in flat-and-ink writing. So it does not help to flag one or two letters on the Azarba’al arrowhead as being cursive to deduce that the inscribed arrowheads straddle all three script-phases from the Proto-Canaanite (El-Khadr 1–3) through some mixed ‘Proto-Canaanite and cursive’ (Rapa, Ruweiseh, Gerba’al, Pères blancs, Zakarba’al) to fully cursive (Azarba’al - Fig. 5.3) arrowheads, the latter exhibiting ‘marked cursive inspiration in the elongated lamed and nun, low zayin and...

26 A new and improved reading will be given by Lehmann (in prep.). The so-called ‘Rapa’ palimpsest arrowhead, in possession of the Beirut National Museum, was published in 1962 by Martin. Its lower text looks typologically much older than the upper text – at least according to the ‘Albright–Cross–Harvard scale’. A new examination shows clearly that the established reading must be abandoned. Instead, the upper and later, but according to the established scale seemingly older text reads Ḥḥ̣ WŁ ǁ BN YḤŠ, which means that the arrowhead should be renamed from ‘Rapa’ to ‘Wala’.

27 See already the recent demurs of Vanderhooft 2017. Vanderhooft also reconstructs the left broken letter differently as samekh (not he), which I hold to be dubious because both letters do not stay on track in the line-of-writing. Anyway the angle of either the bet or the allegedly reconstructed he (or samekh) is not fitting, i.e. there is no correct tracking of both letters anyway, which again rules out a ‘cursive’ writing. Further there is indeed to doubt also the reconstruction of the presumptive bet. In the published photographs this looks as if there is another faint upward stroke to the head, thus ‘closing’ the figure which then cannot be any longer a bet. Because the inscription is written top-down (or bottom-up) as demonstrated by Sass and Finkelstein 2016, fig. 1c and d, it is but the clumsy head of a vertically standing qof with a short or broken stem (if not a decorative element at all).
no trace of the Proto-Canaanite style or mirror-image letters’ (Sass and Finkelstein 2016, 33, 36 and table 2). As long as there are no fully (i.e. not-mixed) cursive and ink-written inscriptions that display at least most of the letters of the alphabet, a theory about an ‘intermediate’ or mixed script in the transitional phase from Proto-Canaanite to ‘Phoenician’ and beyond is too speculative.

Cursivisation, moreover, is not a pre-determined supra-regionally decreed innovation of West Semitic expert scribes.28 It is rather the technically induced calligraphic result of a new writing implement, which at first hesitantly only established itself in the Early Iron Age, before it eventually became the overall powerful implement of the fast-and-fluent tachygrapher-scribe in the early first millennium. As a consequence, as far as (even late) Early Alphabetic is concerned, it is unlikely that an inscription bears cursive and non-cursive forms together – at least not in a way that can be interpreted by means of typology and palaeography.

Or, to say it the other way round: cursivisation in itself is not apt as a distinguishing feature for typological stratification – all the less, if cursive and non-cursive traits are found together in one inscription or in one single word.29

Rather, the degree of cursivisation and its assessment by modern scholars depends on where and how three millennia ago a professional scribe and his (new) implement prevailed. As long as the linear alphabet was not a domain of any royal court or another centralising administration, this alphabet could have looked very different from region to region at the same time (this, inter alia, is what the so-called Rapa arrowhead proves). And vice versa later.30

Thus, in contrast, I hold that there was neither an intermediate ‘post-Proto-Canaanite’ script as such (Sass and Finkelstein 2016, 26 et passim), nor any transitional script phase as a quasi-overlapping period (see Hamilton, above), which both seem too unidimensional/unidirectional a model. Rather I prefer to imagine a ‘transitional phase of scribes’. By this, I mean a period of vagrant freelancer scribes in the very late Bronze and early Iron Ages working on their own accounts. These scribes seem to have been even specialists in different script types (cuneiform, hieroglyph, alphabetic), but were not bound to any court, kingdom, local warlord or administration; rather, they worked on demand, at their own risk and own expense in the Eastern Mediterranean ‘bazaar of writing systems’ (Singer 2000, 27).

Already in the Amarna period there were royal courts with scribes. But there is increasing evidence (Millard 1999, 318, recently Vita 2015) that some of these scribes were not courtly officers of one single city or state, but rather migrant specialists (‘specialized retainers’, Byrne 2007, 16, 22) and freelancers who had a certain radius

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28 Ironically Stephen A. Kaufman (1986, 3): ‘But is not such wide ranging uniformity a trifle strange? It almost requires us to imagine the following scenario: Every twenty years or so the council of Phoenician scribes meeting together for their national academic conference would decide on a few changes in letter stances and shapes. Whereupon not only would all of that group begin to use such forms in their own inscriptions, but the illustrated proceedings of that meeting would be disseminated instantly all over the Ancient Near East, so that all those scribes who could not afford the fare to get to the assembly would be able to incorporate the proper forms into their own inscriptions.’

29 For example, if the extreme left-slanting downstroke prolongation of both the 'M' and the 'N' letters of the ‘extremely similar’ (Aḥituv and Mazar 2014, 43) Tel Amal and Reḥov 5 jar inscriptions, which both read LNMS (Levi and Edelstein 1972, 336 and pl. 254; Aḥituv and Mazar 2014, 194 fig. 6, 43 ff. and193, fig. 5), really were a true cursive trait, why then is the 'M' in both the same small inscriptions so apparently stiff and obviously non-cursive? And why, furthermore, do both the two more examples of the same name from the same region and of the same time frame, which are written below the shoulder in Reḥov 6 (Aḥituv and Mazar 2014, 44 ff. and 195, fig. 7) and on the broken body sherd Reḥov 4 (ibid., 193, fig. 4), not display this peculiarity? Aḥituv and Mazar note that this discrepancy in appearance might be chronological or ‘due to the style of writing’ (2014, 45). An additional close observation helps to clear this point for an easier and more plausible explanation: in both first mentioned cases (Tel Amal and Reḥov 5) the word is written above the shoulder of the respective jars. This obviously influenced the extreme left-slanting completion of the letters which normally require a bit more space underneath for their prolonged but still non-slanting downstroke. But it is most noteworthy here also that the complete letters 'M' and 'N' – only their downstroke prolongations – are slanted due to the missing bottom space on the shoulder of the jar: the ‘M’ and ‘N’ of Reḥov 5 for instance both are slanted by ‘47°’ and ‘33°’ respectively in comparison with the same writing in Reḥov 6, but as opposed to the adjacent letters which do not require additional downstroke space. One should be careful, for instance, also with the commonly hold opinion (for example Sass and Finkelstein 2016, 30) that the Hebrew script-variant was born concurrently with the cursive itself. Rather, if the low angle appeared first, extreme cursivisation would be the inevitable effect, resulting in an appearance many would flag as ‘Hebrew’, even though it is merely a low-angle cursive. See also Sass and Finkelstein 2016, 30 note 23: ‘The Hebrew identification is by the closeness of the lettershapes in inscriptions from Reḥov and Ṣafi […] to those of the Mesha Stele and Samaria ostraca, decades younger’. Q.e.d. — the effect was areal, not ‘national’ (however, many scholars would not refrain from judging the Mesha script for ‘Hebrew’ for political reasons).
within which they worked (Vita 2015, 137–150). But this, to be sure, was cuneiform writing.  

A segmental alphabetic flat writing, on the other hand, was something entirely new. Apart from Ugarit, where alphabetic cuneiform was the predominant writing system, linear alphabetic first was the domain of migrant experts who worked supra-regionally. The technical difference, alphabetic cuneiform versus linear alphabetic, underscores also the socio-cultural difference: it was the ‘subversive innovation of the cultural and geographical periphery’, which ‘remained on the fringes of the canonical script-repertoires [...]. It was not promoted by any institution, state or group of power holders’ (Goldwasser 2011, 284).  

During the collapse of the Late Bronze Age Levantine networks the administrative and prestige version of the alphabet, that is, alphabetic cuneiform, also came to an end, and it was those vagrant freelance scribes who filled the vacuum. They were not only accustomed to multiple, changing, writing implements, but they also developed a new and powerful implement of their own: the ‘chisel shaped broad nib reed pen’.  

The implement – the calligraphic turn

It is perplexing to see how, once standardised, a ‘late Early Alphabetic’ (as I prefer to call it) fanned out into so many different Northwest Semitic alphabet traditions within only a few hundred years in the first half of the first millennium. But this clearly came along with the invention and introduction of a new writing implement that caused an acceleration in writing and calligraphic proficiency: it was the so-called chisel-shaped broad-nib pen instead of the brush-like implement which remained in use in Egypt and also in Eastern Asia. With the decline of the Egyptian hegemony over the Levant, the rush brush-tip pen was subsequently given up, and a triumphant rise of the rush broad nib pen in the Levant began.  

This implement, as for instance seen in the palette-box of the scribe’s orthostat from Zincirli (KAI 218), was a reed pen cut from the ubiquitous, cosmopolitan rush juncus maritimus. Writing with this implement gained a lasting effect, which was based upon the inherent mechanics of writing itself. Moreover, here the mechanics of path dependence triggered an irreversible script differentiation from allography to typology. The new implement caused another important feature, which in interplay with writing surface and scribe’s hand posture triggered far-reaching distortions of letter shapes and, consequently, the birth of ‘new’ scripts. Earlier, in the late second millennium, the crucial decision had been the divergence in writing direction, i.e. dextrograde or sinistrograde – and no-one knows exactly why. However, now in the first millennium BC, the parting of the ways was considerably more manifold and ended up in an almost total divergence of scripts at the end of the first millennium, based on a clear and surprisingly simple rationale: speed and angle of writing.  

In his unpublished 1986 doctoral dissertation, the Dutch scholar Gerrit van der Kooij identified the parameters for what became crucial when speeding up writing by professional scribes. A close investigation of the Northwest Semitic script traditions of the first half of the first millennium BC has shown that script changes leading to seemingly different ‘national scripts’ depend almost exclusively on changes in the scribe’s hand and the different dynamics of regional development, and influence of dominant scribal schools.  

I contend that van der Kooij’s approach, though only poorly taken notice of, explains much of the surprisingly fast development of letter stances and ductus and the eventual divergence in the realm of the West Semitic alphabet in as surprisingly short period of less than half a millennium. With only a little exaggeration we might say that van der Kooij (maybe unwittingly) got the theory of everything here, while others were still collecting data.  

The main factor, as van der Kooij proved through painstaking analyses, was the position of the hand when forming a letter, which results in the angle of writing, i.e. the angle of inception that starts the main vertical stroke of a letter. What van der Kooij found was an obvious regionalisation and that these regions can generally be

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31 It is, as a side note, the disadvantage of both the important books of Carr (2005) and van der Toorn (2007) to overly rely on putative comparative evidence from Egypt and Mesopotamia, or from the considerably later Graeco-Roman world. The alphabet as a segmental system, and to a greater extent the linear alphabet, was something entirely new and different.  
32 ‘It is very unlikely that the invention [of the early alphabet] itself took place under the auspices of a political or cultural Canaanite center, such as the one in Byblos. In this case we may expect that such an advantageous communication tool born and sustained by Canaanite officialdom would gain priority and would show examples of state and administrative discourse, and a quick standardization. This process is nicely exemplified by the Ugaritic alphabet.’ (Goldwasser 2015, 128, similarly 2018)  
33 ‘There is good reason to assume that it was indeed a chisel-shaped broad-nib pen that he used. This is proven indirectly by the inscriptions accompanying this and contemporary artefacts from the same site, which already incontrovertibly display traces of this very calligraphic tradition – as for instance dovetailing and pair-kerning or the thickened, ‘quasi-italicizing’ bases of the Zincirli letter bet (Lehmann 2008a, 149–151, 151–154 and table 19).  
34 ‘A path-dependent sequence of economic changes is one of which important influences upon the eventual outcome can be exerted by temporally remote events, including happenings dominated by chance elements rather than systematic forces.’ (David 1985, 332).  
35 Van der Kooij 1986, 90–93, 244–251, 253. Compare also his study on palaeography in van der Kooij 1976, 37 ff.  
36 Van der Kooij 1986, 90–93, 244–251, 253. As long as there is no better explanation, we must assume that also the earlier change and fixing of writing direction(s) had the same cause. However marginal the reason might have been: once a dominant writing direction had been set, the canon of ‘path dependence’ became the rationale for all further development and did not allow any reconversion, either of direction or of shape.
distinguished as politically and often culturally defined population groups – all of which now points to underlying spatial and well organised school traditions:

The main differentiation is based on the difference in size of the angle of inception, and it soon isolated the script traditions for primarily the Hebrew and probably also southern Trans-Jordan from those used for the Phoenician and the Aramaic Language. (Kooij 1986, 244)

In short: a small angle of writing (45° to c. 10°) was used for Hebrew texts. This was the reason why no very considerable changes within the composition of the letters came about, i.e. why there were no considerable deviations from the ‘Phoenician typewriter’. On the other hand this script exhibited powerful developments in cursive writing.

A larger angle of writing of c. 45–50° was used in the ductus of texts in the Trans-Jordanian region, ‘Ammonite’, and in Phoenician areas, where the angle later reached 60°. But instead of inherent changes of ductus and form as happened in the Aramaic context, here the enlargement of the angle results in a left-inclination or ‘slope’ of up to 45° to the left.

The most considerable enlargement of the angle of writing takes place particularly in the Aramaic script tradition and also in some trans-Jordanian sites like the ink-written scripts used on inscriptions from Deir ‘Alla, probably starting about the end of the eighth century BC or somewhat earlier. This angle of writing was 50–60° and caused more serious changes of graphs.

To sum up, with words by van der Kooij (1986, 250):

[...] all writing has been spread by way of ink writing ‘schools’. This, then, implies that almost all writing, with all kinds of material-implement combinations including those that presuppose a very specific technical skill, had been accomplished by people trained in ink writing, or put into practise by craftsmen copying ink-written texts.

The ‘new scribe’ wrote faster, and in fact he wrote differently, i.e. with a different hand posture, which was distinct from that of Egyptian scribes with their almost vertical brush-holding posture, and also remarkably different from the cuneiform tablet scribe. This sparked a calligraphic turn which should not be underestimated, as, for instance, a simple comparison of the Early Alphabetic Lachish Ewer with any Judaean Lachish ostracon shows (despite more than half a millennium of alphabet history in between, which is not within the scope of this paper): both are written with ink on fired clay, but with different implements, and this is what makes the difference in ink-drawn stroke management.

There was, however, also a great disadvantage of this new, fast and universally available implement: it responds extremely sensitively to minimal shifts of the hand’s posture. Thus it necessitates a new and stern discipline of the scribe’s hand which, as it seems, was formerly unknown in Levantine flat writing. This, however, results in a finger-controlled, remarkably constant angle of inception, which became the main precipitating factor for the creative calligraphic turn.

This angle is the main parameter for a quantifiable recording of calligraphic proficiency and its regional distribution. For its calligraphic potential see for instance the famous eighth-century Deir ‘Alla platter inscription from Jordan with its constant angle of 50° and the elegant bottom fading of downstrokes (Hoftijzer and van der Kooij 1976), or the more ancillary ostracon 2 from Lachish in Judaea (seventh century) with a likewise constant but significantly lower angle of only 17°, as was typical for the south.

Yet the effect of thicker and thinner strokes due to the constant hand posture of a skilled scribe is increasingly imitated in chiselled inscriptions in stone, which means that at this point eventually it already has become part of the typological repertoire.

The different angles of inception, their changes and their calligraphic potential were also responsible for the divergence of at least three main calligraphic script ‘channels’ or ductus in Northwest Semitic and eventually also beyond. Theese can be described at first by simple technical terms – an angle of inception of I. ± 50° (mainly early Aramaic and later Phoenician), II. ≥ 60° (Aramaic), and III. < 45°, which was in use in southern Trans-Jordan scripts and generally in Hebrew (van der Kooij 1986, 222). This may be illustrated by a sample of van der Kooij’s painstakingly drawn graph of general script type series (Fig. 5.4a–c).

Again, it was the master’s hand and not the king’s speech, that changed the alphabet. Although we do not know this master’s identity and name, the reverberation of their writing skill and expertise is visible in the change of the alphabetic script. It was the master’s guide and example that turned the ‘typewriter’ into calligraphy and smoothed the way from the proto-Canaanite arrowhead letters into Hebrew or Aramaic, to Syriac and Arabic calligraphy, and beyond.

Invented tradition and scripted origin

There are two main lines to explain how Early Alphabetic became Phoenician. First is a technical point. This is what I have described by the development of a new implement

37 ‘This points to a politically centralised teaching of writing at one or more interrelated royal courts.’ (Van der Kooij 1986, 250)

38 Van der Kooij 1986, 82, 90. This finger-tip-controlled (not wrist-guided) exactness in mark-making may have had its root already in the Ugaritic technique of cuneiform writing, which has been impressively demonstrated by Ellison 2015.

39 The term is already used and illustrated by one of the most famous modern calligraphers, Edward Johnston (1872–1944). Nevertheless, van der Kooij was the first to introduce it into the assessment of ancient Semitic palaeography and calligraphy.

40 The graphs are extracted from a digitised version of van der Kooij 1986, fig. 16 (pp. 349–353) which will be the basis for a print publication in the Kleine Untersuchungen zur Sprache des Alten Testaments und seiner Umwelt series (in prep.). For a detailed caption and explanation see van der Kooij 1986, 220–234.
and its implications for the style of writing and writing technique and the shape of letters and its standardisation. But there is still the question remaining: why should we call this Phoenician at all?

To recall again the late Brian Peckham, Phoenicia is a ‘Mediterranean state of mind’. As for the classification as ‘Phoenician’, there seems to be much more modern identification than ancient identity. Everything which had been said about being ‘Phoenician’ is under general suspicion of being merely an ‘invented tradition’ (Hobsbawn)\(^41\), that is: the factual or putative main actors and parameters of ‘phoeniciandom’ or ‘phoenicianness’ being at least to some extent of ‘scripted origin’. Phoenicia is a ‘western fabrication’, though, to be sure, not a modern one.

\(^41\) Eric Hobsbawn 2010 [1983], 1–2: “‘invented tradition’ is taken to mean a set of practices, normally governed by overtly or tacitly accepted rules and of a ritual or symbolic nature, which seek to inculcate certain values and norms of behaviour by repetition, which automatically implies continuity with the past. In fact, where possible, they normally attempt to establish continuity with a suitable historic past. [...] However, insofar as there is such reference to a historic past, the peculiarity of ‘invented’ traditions is that the continuity with it is largely factitious. In short, they are responses to novel situations which take the form of reference to old situations, or which establish their own past by quasi-obligatory repetition.”
Rather it is an already ancient western fabrication, forged and coined by the Greeks. Nevertheless it became again a modern western fabrication, and all too often our view of the Levantine coast of the late second and early first millennia BC is still in danger to be seen through the glasses of Herodotos and others. However, if Phoenicia is a fabrication of the modern imagination, the Phoenician script has become a fabrication of the modern imagination too.

I argue that it is not the Phoenicians, but rather a Levantine intellectual ‘internationale’ which formed, by the virtuoso use of a new and powerful implement, a new script type and style of writing which eventually overcame every script that had been seen before. I further recommend that in future we use the term ‘Phoenician script’ as an umbrella term only for any kind of ‘typewriter-like’ proto-standardised script in the early first millennium Levant, to call it ‘Early Alphabetic C’ (Hamilton) or, as Benjamin Sass and Israel Finkelstein recently proposed, ‘post Proto-Canaanite’. Anyway, a script that was not yet discernible as a regional or areal script — which formerly and by many scholars was labelled ‘national script(s)’, such as a Hebrew, Moabite, Aramaean etc. ‘national script’. I myself for good reason refuse to do so. But that is another story.