Comparative Perspectives on the Study of Script Transfer, and the Origin of the Runic Script

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Abstract. The paper discusses a series of cases of script transfer with regard to the role played by script inventors in an effort to determine whether a premise held by certain scholars in runology, viz. that scripts are always created by individuals, is warrantable.

1. Preliminary Remarks

When setting out to research the derivation of the Runic script, the scholar soon finds that—even considering the appeal that is particular to questions about first beginnings and origins—the amount of literature dedicated to this problem exceeds expectations. Making this observation is in fact a commonplace of runology, serving as introduction to numerous studies concerned with the issue.

> Die frage nach dem alter und dem ursprung der runen ist so oft aufgeworfen und auf so viele verschiedene weisen beantwortet worden, daſs man fast versucht sein könnte zu sagen, daſs alle möglichen, denkbaren und undenkbaren ansichten zu worte gekommen sind. […] Es ist eine sehr groſse literatur, die hier vorliegt; aber die qualität steht leider im umgekehrten verhältnis zur quantität.[1]

This paper represents a slightly reworked section of my doctoral thesis *Raetic and runes. On the North Italic theory of the origin of the Runic script* (University of Vienna 2018).

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1. "The question of the age and the origin of the runes has been asked so often and answered in so many different ways that one might almost be tempted to say that all possible, plausible and absurd views have been heard. […] It is a very large literature that exists here; but unfortunately the quality stands in inverse relationship to the quantity."

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The above quote is a representative example—the fact that it dates from 1887, being a passage in Ludvig Wimmer's *Die Runenschrift* (p. 11), which is widely considered to mark the beginning of modern Runic studies, should serve to convey an idea of the present state of affairs.

Curiously, the topic's popularity is due not so much to the pull of the challenge to find a plausible explanation at all, but to the abundance of possible solutions which suggest themselves. The difficulty lies not in constructing a (more or less) convincing argument, but, faced with a host of such, in comparing, assessing, and ultimately choosing that path through the thicket which one considers least fraught with obstacles—for, after all, few models yet have been conclusively disproved, and none was so compelling that somebody else did not prefer another (Williams, 1996, p. 121; Williams, 1997, p. 190). Arguments are adduced from all relevant fields—linguistics, archaeology, ancient history, grammatology, and their various subfields. Much hinges on the weighting of the different aspects, as the starting point often determines the result (Heizmann, 2010, p. 18); comparing and weighing the models against each other becomes an almost hopeless endeavour (Barnes, 1994, p. 12f)

To a certain extent, the possibility of a piece of data being assessed differently, its being considered relevant to the issue or not, is rooted in the methods proper to the humanities. Still, there are some recurring points in the discussion of the Runic origin-question which may be either cleared up or at least shown to involve matters which are not sufficiently well understood currently to be used to build theories on. For example, alphabet history or, generally, script history is regularly, yet usually somewhat vaguely referred to in the literature. There are a number of claims and premises which relate to historical and comparative grammatology—concerning for example the likelihood of source eclecticism in the development of new scripts, the validity of the argumentum ex silentio with regard to evidence gaps, or the role of orthographic features such as writing direction in script transfer—which have been employed as arguments in the discussion of Runic derivation, and I believe that something can be gained—if not in terms of concrete results, then at least methodologically—from a comparative investigation of these issues, to determine whether such claims are justified, whether they must be refuted, or whether their argumentative value is in fact nil. Systematic comparative studies of script transfer would benefit not only runology and other epigraphic/palaeographic fields which could profit from substantiated comparative and typological data, but also the study of historical grammatology per se.

This paper, like the presentation on which it is based, represents a small and selective contribution to one of these very large and general issues of script history: how do new scripts come into being? Specifically: do new scripts "develop" or are they "created"? Do they emerge through gradual diffusion, or are they the work of purposeful inven-

tors? The question is at the same time fundamental and elusive, and the answer (either of the two, or a more differentiated one) may seem obvious to many scholars. Still, unargued statements about how script is thought to be transferred to a new writing community is found in the—not only runological—literature, whereas I have not come across an explicit discussion of the matter, which I think the topic warrants. This paper cannot, of course, serve as a comprehensive study, but is intended as a stimulus for future research.

2. Runic Derivation

2.1. The Search for the Model

For general orientation, a short summary of the issues involved in the quest for the model of the Runic script is in order. The older fuþark, the oldest rune row as shown standardised in tab. 1, was used by speakers of Germanic languages between the 2nd and 8th century AD. It is an alphabetic script, and similarities to the Mediterranean alphabets are immediately evident, e.g., ᛃ—iota, ᛊ—sigma, ᛒ—beta, ᛚ—lambda. Upon closer inspection, however, many of the individual letter shapes and grapheme-phoneme correspondences are quite surprising insofar as they find no clear models in the south, e.g., ᛖ /e/, ᚹ /w/, ᛜ /ŋ/. Runic equivalents of alphabetaria show that the order of the row is entirely different—hence the term *fuþark* instead of *alphabet*. The letter names given in tab. 1, though fully transmitted only in later mediaeval sources, can be quite reliably shown to go back to at least the 4th century—unlike the Mediterranean letter names, let alone the simple syllabic letter designations of Latin, the names of the runes are lexically meaningful in the language which the letters denote.

Table 1. The normalised letter forms of the older futhark together with their transliteration, (supposed) phonetic values, and the (sometimes only tentatively) reconstructed Proto-Germanic rune names (following Düwel, 2008, p. 198f)

ᚠ	**f**	*f*	**fehu*	ᚺ	**h**	*h, x*	**haglaz*	ᛏ	**t**	*t*	**tīwaz*
ᚢ	**u**	*u, ū*	**ūruz*	ᚾ	**n**	*n*	**naudiz*	ᛒ	**b**	*b, ƀ*	**berkanan*
ᚦ	**þ**	*þ, ð*	**þurisaz*	ᛁ	**i**	*i, ī*	**īsaz*	ᛖ	**e**	*e, ē*	**ehwaz*
ᚨ	**a**	*a, ā*	**ansuz*	ᛃ	**j**	*i̯*	**jēran*	ᛗ	**m**	*m*	**mannaz*
ᚱ	**r**	*r*	**raidō*	ᛇ	**ï**	*ī*	**īwaz*	ᛚ	**l**	*l*	**laguz*
ᚲ	**k**	*k*	**kaunan*[1]	ᛈ	**p**	*p*	**perþō*[2]	ᛜ	**ŋ**	*ŋ*	**ingwaz*
ᚷ	**g**	*g, ɡ*	**gebō*	ᛉ	**ʀ**	*z/r*	**algiz*	ᛞ	**d**	*d, đ*	**dagaz*
ᚹ	**w**	*u̯*	**wunjō*[3]	ᛊ	**s**	*s*	**sōwilō*	ᛟ	**o**	*o, ō*	**ōþalan*

Runic inscriptions appear somewhat abruptly on portable items in the second half of the 2nd century AD in southern Scandinavia (southern Norway, Denmark and northernmost Germany), seemingly well beyond the limits of literate Europe at the time. The earliest inscriptions are very short; where they are understandable, they encode the personal names of owners, writers and manufacturers (and sometimes, fancifully, also weapons); there is no evidence for public literacy in the earliest phase. There is some debate on which exact Germanic language (stage) is encoded in the first documents; it is accordingly hard to argue how well the older fuþark represents the phonemic system(s) of the language(s) it denotes. Graphically, the script is very uniform from the beginning; beyond a few minor differences in letter forms, there are no recognisable regional or chronological variants. The writing direction, on the other hand, is not fixed, and word separation is optional; apparently random retrograde runes, mirrored runes and various types of ligatures are common.

In some ways, the older fuþark is quite an ordinary specimen of Palaeo-European scripts—a group which, after all, boasts members like the Iberian script and Ogam—but the fact of its existence remains baffling in many respects. The plethora of contributions to the question of how the runes came to be is usually collected in three camps according to whether the (primary) model is the Latin, the Greek, or a North Italic alphabet, respectively. Each of these camps includes a large number of widely different theories which involve different geographical, diachronic or stylistic alphabet variants and emphasise different aspects—formal, grammatological, linguistic, archaeological, historical, cultural—of the borrowing, and have correspondingly different virtues and shortcomings. As of today, no single attested Mediterranean alphabet has been identified which provides everything a model for the runes ought to provide, namely:

- models for all Runic graphemes and motivation for their sound values,
- explanations for the deviant order and the letter names,
- paradigms for the epigraphic culture (writing conventions and text types), and
- a plausible historical context for a borrowing.

While the recent decades have seen, to some extent, a shift away from formal to historical-archaeological considerations, it is the letter forms and values which were and are the focus of theories of Runic origin. Since the work of Jacob Bredsdorff (1822), the scientific community has been widely agreed that the runes are not derived directly from the Phoenician alphabet; the claim that they represent a Germanic or even Indo-European proto-script has also rather lost in appeal. The many suggestions offered to this day work with a handful of potential model

alphabets which are derived from each other and are consequently so similar in many respects that the distinction between genetical and typological developments is as difficult as the identification of discrete geographical and diachronic variants (Wimmer, 1887, p. 20; Mees, 1999, p. 149). The debate moves within such a narrow field that the numerous possibilities for formal derivations are hard to prefer to each other—any rune can be argued to correspond to a daunting number of letters from various northern Mediterranean alphabets and alphabet variants. Many derivations proposed by scholars have been criticised and/or rejected for what was considered an inadequate or even principally flawed handling of the establishment of graphic or systematic relationships between model letters and runes. The demand for a consistent approach which respects both character shapes and grapheme-phoneme relationships, and for the avoidance of ad-hoc explanations, is found regularly in the runological literature—again, it can be traced back to Wimmer (1887):

> da ich als h a u p t g r u n d s a t z für die ableitung zweier alphabete von einander die forderung aufstelle, daſs die zeichen einander s o w o h l i n f o r m w i e b e d e u t u n g entsprechen müssen, wofern man nicht, wo dies in der einen oder andern richtung nicht der fall ist, ganz evident die gründe der abweichungen nachweisen kann. Sonst wird man leicht zu den willkürlichsten und unbegreiflichsten zusammenstellungen verleitet (1887, S. 120).[2]

How hard it is to meet this requirement was demonstrated by Wimmer himself. The initial impact of his seminal work was probably to no little extent owed to the favourable impression that his tidy presentation of well and elaborately argued derivations of all the runes from the letters of the Classical Latin alphabet made in comparison to earlier efforts. In hindsight, Wimmer heads a long and illustrious line of scholars whose theories involve a few plausible or even seemingly obvious correspondences and explanations beside a considerable number of motivations for discrepancies that range from the disputable to the highly improbable. It is often attempted to support individual derivations by referring to similar, but unrelated developments in other alphabets, by positing principles of rune formation which are then used to circularly motivate the forms they were inferred from, and/or by making unsubstantiated assumptions about the circumstances of the derivations. Ad-hoc explanations of sound values which were switched, adapted or misinterpreted, and letters which were inverted, mirrored, doubled and confused

2. 'since I posit as *main principle* for the derivation of two alphabets from each other the requirement that the characters must correspond to each other *in form as well as in meaning*, unless one can, where this is not the case in one or the other direction, demonstrate evidently the reasons for the deviation. Otherwise one is tempted to the most arbitrary and incomprehensible combinations'.

with each other, are equally numerous.[3] Morris (1988, p. 48) calls this the "mental gymnastics" of Runic derivation.

2.2. The Rune Master

In light of what was said in the previous section, it becomes understandable that some scholars have asked the question whether the search for individual derivations is really, as claimed by Wimmer, useful and necessary. Indeed, there is a school of thought in runology which licenses sidestepping the problem of character correspondence on the basis of the claim that the fuþark is not so much an adjusted derivation of its model alphabet, but more of an independent creation. That is, the fuþark is not regarded as the result of an adoption whose deviating features must be reasonably accounted for, but as an adaptation undertaken by a purposeful creator who made ultimately arbitrary decisions about the treatment of letter shapes and values (including the use of superfluous characters and the introduction of new ones), the inner logic of the system (or lack thereof), the overall style—in short, about every aspect of the new script he created. As a consequence, the modern scholar's attempt to derive each rune from a letter in a Mediterranean alphabet must be "a fruitless endeavor" (ibid., p. 150). This view was, I believe, first advanced by Askeberg (1944), who wrote that the fuþark was not "en slavisk kopia", but "en tämligen fri omarbetning av förebilden"[4] (78). Askeberg's statement was echoed by Moltke (1976, p. 53) and features in a near-translation in Moltke's (1981) paper: the fuþark is "not a slavish imitation, but a free moulding" (7), the focus on the letters and their sequence an infatuation (6).

The notion of a rune master who created a script for the Germanic language is present from the 18th century, with Göransson (1747) observing that the fuþark was the work of a "sehr weisen meister" (§ 3)—"Die runen sind nicht von einem heiden, sondern von einem frommen und von gottes heiligem offenbartem worte hocherleuchteten und weisen gottes-manne erfunden" (§ 7)[5]—and is found regularly in the runological literature.[6] Some scholars think of a small group of peo-

3. Examples and discussion, e.g., in Odenstedt (1990, pp. 145–167) and Morris (1988, pp. 9–54).

4. 'a slavish copy'—'a rather free reworking of the model'.

5. 'very wise master'—'The runes were not invented by a heathen, but by a pious man of God, wise and highly enlightened by God's holy revealed word'. Cited from Wimmer (1887, p. 12) (there already in German translation).

6. E.g., Wimmer (ibid., p. 176); Bugge (1913, p. 185); Kluge (1919, p. 48); Baesecke (1940, p. 101); Rosenfeld (1956, p. 236); Kabell (1967); Jensen (1969, p. 129); Höfler (1971, p. 135); Jungandreas (1974, p. 366); Elliott (1989, p. 9); Rausing (1992, p. 202); Williams (1996, p. 213); Birkhan (2006, p. 89).

ple within whose sphere the fuþark originated rather than of a single person, but tend also towards purposeful creation.[7]

Though theories involving an unsophisticated creator (i.e., a person with little to no literacy in the model script according to the terminology of Daniels (1996a)) do exist (e.g., Fairfax, 2014, pp. 215–217 and Friesen, 1918–1919, p. 12, whose Germanic script inventors received only very basic or inadequate tutoring), the inventor of the runes is more often considered to be a speaker of a Germanic language not merely with competence in writing the source language, but often with some level of classical education. The sophisticated creator is necessary particularly for theories which consider certain aspects of the Runic script to be so tidy that they cannot be explained but by a purposefully regulating hand—this concerns mainly the "perfect fit", i.e., the much-debated bi-unique correspondence between the runes of the older fuþark and the phoneme system of the language it initially denoted,[8] and the phonetically ordered rune row.

Theories which involve the reconstruction of a phonemic fit require a sophisticated inventor who performed a (graphemic and) phonemic analysis of model and target language (e.g., Derolez, 1998, p. 109).[9] Grønvik (2001, p. 58f) says that the runes were created "durch einen einmaligen, genau geplanten und in einem Zug durchgeführten Vorgang". The creator was

> ein Mann mit eingehendem Verständnis des eigenen Sprachsystems, aber auch mit sicherer Kenntnis lateinischer Schrift und Kultur. Wir können ihn uns als einen bereisten und hoch kultivierten dänischen Häuptling vorstellen, der imstande war, das Prinzip der Buchstabenschrift zu übernehmen und es seiner eigenen Sprache anzupassen, der aber zugleich eine bedeutende sozia-

7. E.g., Moltke (1981, p. 4); Braunmüller (1998, p. 18f); Spurkland (2005, p. 6).

8. This is not the place for an exhaustive discussion of the question of the perfect fit. There are some problematic cases in the rune row which require particular attention, also in terms of motivating their existence despite the ruling hand of a creator. Basically, there are four options to accomodate these elements: (1) the script is older than the oldest preserved texts and consequently fitted to a different phoneme system (e.g., Antonsen's explanation of ᛇ); (2) the script is tied to the model in more ways than one (usually theories involving script magic or gematria, e.g., Wimmer's explanation [Wimmer, 1887, p. 135f.] of ᛇ as a filler to make twenty-four letters); (3) the creator failed to completely emancipate himself from the normative force of the model (e.g., Antonsen's explanation of ᛜ); (4) the creator did not have a perfect grasp of the model (e.g., Williams, 1997, p. 186).

9. See also Agrell (1938, p. 89); Alexander (1975, p. 7); Odenstedt (1990, p. 169); Beck (2001, p. 6f); Stoklund (2003, p. 172); Düwel (2003, p. 582); Braunmüller (2004, p. 25); Düwel (2008, p. 181); Heizmann (2010, pp. 18–20); Spurkland (2010, p. 65); Barnes (2012, p. 10), and Dillmann's *Runenmeister*-entry in *Reallexikon der germanischen Altertumskunde* (2003, 540f).

> le Position in seinem Heimatland hatte, so daß sein Alphabet sich bei seinen Standesgenossen schnell durchsetzen konnte.[10]

The sophistication of the rune master(s) is also discussed explicitly by Braunmüller (1998, p. 18f), who ascribes the creation of the runes to a small, presumably co-ordinated group of inventors with Latin education, either soldiers or traders: one must

> wohl davon ausgehen, dass [the rune masters] über ein nicht geringes linguistisches Fachwissen verfügt haben, das sie wohl nur im Umkreis einer Sprache mit einer längeren Schrift- und Bildungstradition erworben haben können [...] Den 'Erfindern' der Runenschrift muss beispielweise bekannt gewesen sein, welches Abbildungsverhältnis zwischen Allophonen und Phonemen in der/den Entlehnungssprache/n bestand, wie dort die Zuordnungen von Phonemen und Graphemen aussahen sowie schließlich auch, ob es mehrere Grapheme für 1 Phonem [...] und ob es z. B. 1 Graphem für 2 Phoneme [...] gab. Darüber hinaus mußten die ersten Runenmeister [...] die eigene Sprache dahingehend untersucht haben, ob es hier nicht Phoneme gab, für die im Ausgangs- oder Entlehnungsalphabet keine entsprechenden Grapheme zu finden waren. [...] M. a. W., es ist, zumal nach der Analyse des sehr guten Phonem-Graphem-Abbildungsverhältnisses im älteren Fuþark, davon auszugehen, daß hier Leute mit einem fundierten Fachwissen am Werk waren und daß sie zweifellos die Absicht hatten, eine einheimische Gebrauchsschrift zu schaffen.[11]

The same goes for theories which explain the order of the rune row as phonetically motivated, e.g., Jensen (1969, p. 134), who postulates patterns in the distribution of types of articulation, adding: "The hypothesis that so much abstract theory lies behind the alphabet of our

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10. 'by a one-time, precisely planned operation executed in one go'—'a man with in-depth understanding of his own language system, but also with reliable knowledge of Latin writing and culture. We may picture him as a travelled and highly cultivated Danish chieftain who was capable of adopting the principle of alphabetic writing and adapting it to his own language, but who at the same time had an important social position in his homeland, so that his alphabet could establish itself quickly among his peers'.

11. 'assume that [the rune masters] possessed considerable linguistic expertise, which they can only have acquired in contact with a language with a long tradition of writing and education. [...] It must, for example, have been known to the 'inventors' of the Runic script which relationship existed between allophones and phonemes in the source language(s), how the allocation of phonemes and graphemes worked there, and finally also whether there was more than one grapheme for one phoneme [...] and whether there was, e.g., one grapheme for two phonemes. Furthermore, the first rune masters must have [...] studied their own language with regard to whether there were phonemes for which no corresponding graphemes could be found in the source or model alphabet. [...] In other words, one must, particularly after the analysis of the excellent phoneme-grapheme relationships in the older fuþark, assume that this was the work of people with sound expertise, and that they had without doubt the intention to create an indigenous functional script'.

shaggy forefathers may be hard to swallow for whosoever believes that new scripts arise only through corruption of other alphabets" (p. 134). (Cf. also Miller, 1994, p. 68.)

The assumption that the creator knew what he was doing calls for a motivation to explain the many non-obvious deviations from the model. The most popular stance is to suspect an ethno-nationalistic motive behind the reworking, viz. that the runes were created as a "Geheimschrift" ('secret script', Grønvik, 2001, p. 58) which was designed specifically to be undecipherable to a person literate in the source language.[12]

If the involvement of a deliberate creator, who maybe even purposefully distorted the model, is assumed, certain aspects of the relationship between model and, in the present case, rune row become irrelevant to the argument of derivation—most importantly, the problems pertaining to graphic forms and grapheme-phoneme relationships. Deviations from the model can be summarily explained as idiosyncrasies which are due to an individual's fancy and do not require or indeed do not allow for detailed argumentation. The potential for randomness in this bottleneck-approach is acknowledged by Miller (1994, p. 67): "There is no reason to accord the fuþark inventor(s) any less creativity or prerogative than known script designers."

Of course, as was shown above, the notion that the older fuþark is a deliberate creation is not merely an excuse to save one's self the task of explaining the details of the script's weirdness—features like the phonemic fit and the deviating order of the row are indeed best explained through the intervention of a creator. The uniformity of the earliest Runic documents is also frequently taken to speak for a one-off creation as opposed to a gradual development (e.g., Mees, 1999, p. 145; 2000, p. 57). All features, however, which have been claimed in favour of a rune master are ultimately theory-dependent, i.e., they are not accepted by all scholars and/or have also been explained differently, and thus cannot be used as conclusive arguments for the existence of an inventor. Also, there are other characteristics of early Runic writing which have been cited as arguments for a gradual borrowing process, such as the preponderance of owner's inscriptions, which Markey (2001, p. 88) considers to reflect the first stage of the borrowing process: reproduction of the model without a specific purpose. Pedersen (1923, p. 51f) assumes a pre-attestation phase in which the Runic script was gradually developed out of an imitation of the Latin alphabet. Following Pedersen, Odenstedt (1990, pp. 163–167) expresses the opinion that all the peculiarities of the fuþark can be explained organically and that the fuþark does not de-

12. Such and similar positions in, e.g., Musset (1965, pp. 47–49); Prosdocimi (1985, pp. 392–395; 2003, p. 438); Scardigli (1993); Barnes (1997, pp. 9–11); Griffiths (1999, p. 193); Stoklund (2003, p. 178); Williams (2004, p. 272); Spurkland (2010, p. 76); Heizmann (2010, p. 20).

viate from its (in his case also Latin) model so far that a "single inventor (often described as "ingenious")" (1989, p. 48) needs to be brought in. It is admissible to argue that certain differences between the Runic script and the Mediterranean alphabets point to the existence of a sophisticated creator of the runes, but this assessment is tentative, and must not in turn be used to explain those very same characteristics.

A way to avoid the circularity of an argument which motivates a Runic feature with a rune master and the rune master with that same feature is to propose that script transfer exclusively happens through the intervention of a script creator. For the runes, this was claimed by, e.g., Elmer Antonsen, who is of the opinion that, generally, the adaptation of a script for another language requires a person who is not only bilingual, but endowed with an intuitive understanding of linguistics, who must learn the model script in all its aspects and then systematically rework it (1987, p. 26). Antonsen states quite decidedly that writing is never borrowed via diffusion, but always systematically adapted by an individual (1996, p. 7).

That the assumption of an individual creator was considered communis opinio in runology from early on is demonstrated by the emphasis with which this view is sporadically repudiated.[13] Taylor (1879), who thinks that the developments undergone by scripts are subject to laws akin to those governing language, rejects the derivations from Latin letters proposed by Wimmer (1887) on the basis that they neglect the "fundamental principles of alphabetic change":

> His method assumes that the inventors of the runes arbitrarily discarded a certain number of the Latin letters, and then without any Sufficient Reason invented other letters to supply the vacant places. If his explanations are correct, several of the runes, instead of having been evolved, like the letters of all other alphabets, by the action of slow and natural processes, must have been invented off hand by some alphabetic lawgiver, [...] whose arbitrary behests were promptly obeyed over a vast region extending from the Rhone to the Baltic, and from the Baltic to the Danube. (p. 27f)

Schrader (1901, p. 736) dismissively writes:

> Die Vorstellung von einem "genialen praeceptor Germaniae", wie man jenen Mann ernsthaft genannt hat, der seinen Deutschen ein Alphabet zusammengesetzt haben soll, dürfte jeder kulturgeschichtlichen Analogie entbehren."[14]

13. See also Luft (1898, p. 1f); Hempl (1896, p. 17).

14. 'The concept of an "ingenious praeceptor Germaniae", as that man has in all seriousness been called [namely by Meyer (1896, S. 162)], who assembled an alphabet for his Germans, probably lacks any analogy in cultural history.'

More recently, Markey (2001, pp. 84–86) comments critically on the notion that writing could be invented on the spot—in his opinion, script transfer exclusively happens by incremental diffusion.

Is either of these positions correct, and if so—which? The runes are not a primary script, but a secondary one. Hence, our question does not concern the processes leading to the primary creation of script, but the mechanisms of script transfer. Despite the fact that the data situation is rather better here, there being a great deal more cases and the documentation extending into recent times, these mechanisms are not clear at all. Do scripts diffuse from one script culture into another, or are they adapted by individuals? If the borrowing happens between two specific groups of people, such as traders or priests, should this be considered a subtype of the first or the second case? How to assess cases in which a conscious creation undergoes secondary changes in use within the writing community, or, conversely, a script which has already been in use to some extent and is only afterwards systematically adapted? Can we distinguish such processes in ancient times without the help of secondary sources, i.e., actual accounts of the borrowing? Can we posit rules for how writing is borrowed and associate them with different cases—assuming that different things happen to the original script in the different scenarios—and can we use these to identify the processes in those cases where no historical information is available (or trustworthy)?

3. Script Transfer

3.1. What Is a New Script?

An issue that needs to be addressed in this context is what exactly we call a "different" and therefore, in a transfer situation, a "new" script, in opposition to the same script for a different language. I suspect that, for many scholars, this distinction is immediately connected to the question of how scripts come about, in that only the intervention of a creator results in what can be considered a new script, whereas the gradual transfer of a script to a new writing community does not. With the prevalent definition of "script" as an inventory of graphemes which can serve for the denotation of different languages, resulting in language-specific writing systems with their various orthographies,[15] the above distinction is intuitively plausible—gradual diffusion involves mainly orthographic and minor graphic changes, while a script inventor may completely reform the model script's characters or simply come up with new

15. E.g., Sproat (2000, p. 25); Coulmas (2003, p. 35); Daniels (2018, p. 155).

ones to create a sufficiently different character set: the Latin alphabet spread gradually through Western Europe with only minor adaptations, hence is has remained one script with language-specific orthographies, but the Cyrillic alphabet was a one-time creation and is therefore considered a different script.

Of course, the question of what makes a script one script rather than another one is more complex than this, and its discussion would exceed the scope of this paper. I limit myself to a reference to Wang (2019), who shows that the definition of the Latin or Roman alphabet as one cohesive script based on the modern writing systems which are considered to employ it is hard to justify on purely graphematic terms, and involves both historical and social factors. Historical connections inform our definitions in some cases, but not always—one would be hard pressed to pinpoint the intervention that made different scripts out of the Greek and Latin alphabets, but as different scripts they are unanimously regarded. It must also be said that many runologists do not appear to ascribe to the above distinction, considering a creator necessary for a script's systematic adaptation to a new language ("reworking"), without explicitly referring to changes in its outer form. I will leave this aspect of the matter aside in the following sections, and use the terms *script* and *writing system* interchangeably (as done in Cubberley (1996, pp. xliii–xlv)).

3.2. Adaptation vs. Adoption

Isaac Taylor as cited above provides an example for an alphabet historian who expressly declares himself for gradual diffusion as the primary means of script transfer. Otherwise, I have not been able to find a lot in the way of categorical statements, but the ones I did come across point towards a general preference for the purposeful inventor. Prominently, Gelb (1963, p. 199) observes that

> we must always reckon in the case of all great cultural achievements with the decisive intervention of men of genius who were able either to break away from sacred tradition or to transfer into practical form something on which others could only speculate.

However, he also admits that

> [u]nfortunately, we do not know any of the geniuses who were responsible for the most important reforms in the history of writing. Their names [...] are lost to us forever in the dimness of antiquity.

In an article concerned with the typology of the spread of script, Voogt (2012), who adheres to the traditional view that primary scripts evolve gradually from precursors of some description, contrasts these cases with borrowings: secondary scripts cannot be expected to pass

through the same stages of development as primary ones; their emergence happens "relatively sudden" (p. 2) and they "need to be largely completed before the script can be put to use" (p. 6). Daniels, in a short introduction concerning the invention of writing, expresses himself somewhat vaguely: in the context of *Scripts Invented in Modern Times*, he exclusively refers to "grammatogenists" Daniels (1996a, p. 578), then writes: "The normal way for a society to acquire its own script is by evolving, adapting, or adopting an existing writing system" (Daniels, 1996b, p. 579), and contrasts this scenario with cases in which one person creates an original script which does not have much in common with the model. Curiously, there appears to be disagreement about what the communis opinio on the matter is (whether one subscribes to it or not). O'Connor (1996, p. 90), writing about the development of the Semitic script from the Egyptian one, observes that "there is a long-standing and plausible tradition of regarding writing as an invention, i.e., as something that reflects the work of one person at one time". McManus (1991) in his treatment of Ogam paints a different picture—he repeatedly makes a point of how older theories about the origin of that script are faulty because they are based on the principle that developments must be natural, while he himself advocates, as a new approach, to "ascribe at least some of the peculiarities to the *creative* rather than the *natural* input" (p. 13). He ascribes the creation of Ogam to a "creative individual or school" and opines that the details of the derivation "can be safely left to the ingenuity of the creator".

Jeffery (1990) in her study of the archaic Greek alphabets devotes some space to the discussion of different scenarios for script transfer, in which contexts they happen, and how to tell them apart:

> How does an illiterate people A normally achieve literacy? It may be in sufficiently close contact with a literate civilisation B to acquire the knowledge inevitably from mutual intercourse, particularly if there are intermarriages which produce bilingual speakers; this may be either because literate members of B are scattered throughout A or because in one particular area people of both A and B are in contact, whence the knowledge is spread to the rest of A. The diffusion of the Roman alphabet country by country throughout the Roman Empire illustrates the former method on a large scale; the spread of the alphabet through archaic Etruria from the original contact of the Greeks of Kyme with the Etruscans illustrates the latter. Alternatively, a script may be deliberately introduced into the illiterate country A by an individual or small group of persons, as happened in the cases of the Gothic, Armenian, and Cyrillic (or Glagolitic) scripts. A member of A or B, outstanding in position and personality, and with a thorough knowledge of the B script, creates a script for A by synthesis, basing it upon the existing B script and adding any extra signs felt to be necessary for the A language, either by borrowing from other scripts or by newly invented signs. The underlying motives for this may be either political or religious, or a mixture of both, but in either case they imply a more deliberate connexion between the two coun-

> tries than is indicated by the more haphazard method of commercial contact, such as the contact between the Etruscans and the Greeks of Kyme (p. 1f).

Jeffery distinguishes between two basic types of script transfer:

Type 1 The knowledge of writing diffuses "inevitably" into a previously illiterate community.
Type 2 The model system is purposefully changed and strategically adapted before being put to use.

Type 1 implies the involvement of a large number of people, a longer time needed before changes are established, and a less uniform result (either only in the early phases, or ultimately leading to local variants). The system is adopted and then gradually adapted to circumstances in use in the same way that any new technology is; it is subjected to a—mainly phonetically conditioned—process of gradual change which eventually results in a more or less different system. The emergence of the new script happens gradually, in step with actual practice; changes accrue due to problems which arise in use. The eventual result of a different script is not intentional: the model script is used to write a different language—the users would conceivably consider themselves to be using the model script even at a time when new conventions have created a system which differs notably and systematically from the model. Type 2, on the other hand, presupposes one person, or a small group of co-ordinated persons, who devise(s), in relatively short time, a new system, more or less closely modelled on an existing one, on the drawing board. This new script is immediately uniform, the formalisms and rules are binding, and any variation is the consequence of secondary developments.

Jeffery associates type 1 with a lack of sophistication: users who are interested in the practical aspects of the technology do not demand a great deal from the system in terms of phonological precision and consistency; they initially adopt graphemes and their values without reflection. Any changes and adaptations, such as the loss of superfluous characters or the substitution of foreign (sound) values with similar ones in the new language happen automatically. Jeffery names the creation and distinction of duplicates and the borrowing of individual characters from other sources as innovations which are typical of scenarios of this type. On the other hand, the recycling of unnecessary characters for phonetically dissimilar sounds, the creation of individual characters without a graphic model, as well as changes in script type, she assumes to be particular to sophisticated creations (p. 4).

Certainly, and this is the point made by some runologists, it is the slow, unstrategic diffusion borne by many which is generally considered to lead to results that can be registered statistically, compared and

used to determine what is called "principles of alphabet history"[16]—thus also Daniels (1996a, p. 583), who observes that insights into the process of script invention can only be got from the study of unsophisticated grammatogenies. A single creator, on the other hand, forms a black box: while he may be equally inclined to make phonologically or graphically obvious and comprehensible choices in his work, he must be expected to sometimes solve a problem in a completely arbitrary manner or even introduce purposefully unnecessary changes—if the creator makes an effort to set his creation apart from the model, extensive redesigning may take place. Unstrategic diffusion does not provide a context for abrupt changes by which a system loses its tradition of transmission; even small-scale "creative" innovations would have a hard time getting established, and the reasons for why it developed as it did should be reconstructable.

TABLE 2. The differences between script transfer types 1 and 2 based on Jeffery (1990, pp. 1–4)

Type 1—Diffusion	Type 2—Invention
gradual	abrupt
automatic	deliberate
practice-based	theory-based
unsophisticated	sophisticated
unco-ordinated	co-ordinated
many people	one person or small group
unintentional changes	strategic changes
unregulated	binding rules
variation	uniform
duplication of letters	reallocation of letters
source eclecticism	new characters
natural	arbitrary
principles of script history	not reproducible

It is not evident, however, that the differences between the effects of these two types of script transfer are quite as clear-cut. Jeffery's allocation of certain kinds of changes in letter shape and value to different types is interesting, but would need to be supported with a considerable number of convincing examples to be diagnostically useful. Also, the distinction between "unsophisticated diffusion" and "sophisticated cre-

16. There is of course no reason why it should not be possible to identify tendencies unspecific to script type which can be applied to different kinds of script; the usual reference to alphabet history is due to this script type being the best studied one.

ation" as implied by Jeffery is intuitive, but not universal. On the one hand, we know of unsophisticated script inventors; on the other hand, it is debatable to what extent diffusion can happen without a certain level of sophistication: people who use the model script, however inexpertly, must either have at least an idea of how to write the source language (if there was extended contact between the groups) or must have been taught the basics without having literacy in the source language; in the latter case, even the most basic instruction (the teaching of the letter inventory and values, or how to write one's name) must involve an explanation of how the characters relate to elements of spoken language. The problem here is the definition of what exactly one calls "sophistication": there is a difference between a person having no understanding of how a writing system works beyond the notion of visual signs encoding meaning, a person having rudimentary skills in writing the source language, a person being bilingual or well trained in writing the source language, and a person having enjoyed an education which includes theoretical linguistic/grammatological knowledge of some sort.

Another one of the problems involved in Jeffery's distinction between creation and diffusion is the fact that one can imagine a considerable number of scenarios—as indeed demonstrated by runologists—which are hard to assign to either of the two options. Jeffery books as a subtype of type 1 the borrowing of writing within one particular group of people, with the script spreading to the rest of the population after a certain period of time. Yet in such a case, a fairly uniform and functional system may develop before spreading to other groups of users. If this earliest phase happens not to be attested, or to be attested so sparsely that the documents' relevance is dubiuos, the existing inscriptions may appear to reflect a systematically created script, despite having evolved without the help of a purposeful inventor. The question is ultimately not only which scenarios of script transfer are possible, but how and under which circumstances they can be identified and classified by modern scholars.

Historical examples for sophisticated script invention or adaptation as envisioned by Jeffery (type 2), Gelb and Voogt do of course exist in quantities. Indeed, documented cases of the emergence of new scripts in recent times are almost exclusively cases of a purposeful, even if sometimes unsophisticated creation.[17] The question is to what extent these apparently clear-cut statistics reflect reality—it might be argued that these cases are the ones which will be documented (usually by the creator), while examples for the unsupervised diffusion of a script into a previously illiterate society tend to go unnoticed. Even if this caveat should be uncalled-for, it is at least debatable whether the situation in antiquity (and earlier) should be judged on the basis of modern conditions. The abundance of historically documented creations of scripts

17. Examples in Daniels (1996b, pp. 580–585)

is in large part due to the activity of Christian missionaries and their efforts to bring the text of the Gospel into the farthest corners of the earth. It might be asked whether, since the onset of the Age of Discovery, scripts have even had much of a chance to diffuse anywhere—though Voogt himself provides a clear example for gradual, decentralised script transfer from a literary language to a previously unwritten one in Voogt and Döhla (2012): speakers of Nubian on Saï Island (Sudan) have recently taken to using the Arabic script to write their vernacular in public graffiti. There are only few changes from Arabic orthography and sound values, but those appear to have been agreed upon by convention in the small writing community—"in this case there is no clear inventor or teacher of the writing system whom we can immediately identify" (p. 55).

Cases other than modern ones in which the process of development/creation can be retraced with (some) certainty are few and far between. In the following, I will discuss a few examples for different transfer situations with special regard to the more or less arcane figure of the script inventor.

3.3. Creating a Script: Hankul

A special case in all aspects is that of Korean Hankul,[18] whose creation in 1443 and promulgation in 1446 was obligingly accompanied by a contemporary proclamation (*Hwunmin cengum* 'Correct Sounds for the Instruction of the People') and a handbook (*Hwunmin cengum haylyey* 'Explanations and examples of the correct sounds for the instruction of the people', lost until 1940). In an effort to make literacy more widespread than he thought feasible with the complex systems of writing Korean with Chinese characters (*hanca*), King Seycong—or one or more of his scholars—created a purely phonographic script with characters which were designed to be easy to learn.

Hankul ('Han writing', a modern term) was constructed with considerable linguistic insight: five graphically simple consonant characters, whose shape reflects the position of the articulatory organs pronouncing the respective sounds, are used as basis to systematically derive characters for sounds with a different manner of articulation (e.g., doubling for the tense plosives). There is a clear graphic distinction between consonants and vowels; tone is also marked. The great versatility arising from the combination of graphic elements which indicate features, theoretically allowing the denoting of considerably more sounds than necessary for Korean, has led Sampson (1985, pp. 120–144) to introduce a special typological category for Hankul, viz. "featural" scripts. The (original)

18. Korean transcribed according to the Yale romanisation.

system is one of the most logical and symmetrical ever to be created for common use and represents a prime example of sophisticated grammatogeny by one person or a small group of competent persons inventing a script for their language and perfecting it before making it available for use (Taylor and Taylor, 1995, pp. 211–216; King, 1996, p. 219f).

3.4. Claiming to Have Created a Script: Old Persian Cuneiform

A historical document which has been considered to refer explicitly to the creation of a new script is also preserved for Old Persian cuneiform—the text in question is rather less detailed than the Hankul *Explanations*, but it was never lost, being prominently inscribed on a cliff of Mount Behistun (IR), accompanied by a huge relief. The trilingual Behistun inscription, applied some time after 521 BC by mandate of Darius I, consists in Elamite and Old Babylonian versions of the same text, both written in long established varieties of cuneiform, and an Old Persian version written in a script which resembles cuneiform in style, but is of a different type, and features unrelated characters and grapheme-phoneme correspondences. The text is concerned with the legitimisation of rule, and tells of how Darius prevailed over a series of pretenders after the demise of Cambyses II. The section in question, often instructively called "Schrifterfindungsparagraph" ('script invention paragraph', DB/OP § 70 [IV 89–92] and its Elamite counterpart), has been taken to announce that Darius had commissioned the invention of the script then used for the first time in the present inscription. This was already suggested by Weißbach 1911 and elaborated by Hinz (1942, pp. 346–349); Hinz (1952). The Old Persian part is heavily damaged, and a Babylonian counterpart is absent; it is the well preserved Elamite part, a secondary addition to go with the Old Persian text, which contains the crucial reference to something which had not previously existed (which is lost in the Old Persian version). Hinz (ibid., p. 30) argues for a translation of Elam. *tup-pi-me* as 'script' and translates: "[…] machte ich eine andersartige Schrift, auf arisch, was es vordem nicht gab" (p. 32f).[19]

Though Hinz' translation and interpretation of the paragraph were accepted by many scholars (e.g., R. Schmitt, 1998, p. 458f), it is not at all evident. Most importantly, Elam. *tuppime* (*tuppi-* 'inscription' with an abstract suffix[?] *-me*) ∼ OP IV 89 *dipiciça-* may instead signify a type of text (Diakonoff, 1970, p. 99; Tuplin, 2005, p. 224), a version or copy (Huyse, 1999, p. 47; R. Schmitt, 2009, p. 87) or a part of the inscription (Vallat, 2011, p. 266). As pointed out by Hinz (1973, p. 15), this does not necessarily preclude his interpretation: even without an explicit reference

19. '[…] I made a different script, in Aryan, something which had not existed before'.

to script, the claim that the inscription/text/... is the first in Old Persian (Aryan) implies that the Old Persian script (which is not attested to write any language but Old Persian) is used for the first time in the Behistun inscription. Still, of the numerous translations which have been put forward of both the Elamite and the reconstructed Old Persian parts (see Rossi forthc. for an overview of recent attempts), many do not allow for an interpretation in Hinz' sense—for example, Vallat (2011, p. 266) translates the Elamite text as "J'ai traduit autrement en aryen cette inscription. Elle [the OP part] ne se trouvait pas ici [on the rock face] auparavant",[20] doing away with both the reference to the script and the claim of writing Old Persian for the first time.[21] It is not even clear that Old Persian *aryā* (Elam. *har-ri-ya-ma*) refers to the language (Rossi forthc., § 2.2.1). The lines OP IV 97–99 make mention of *tuppime/dipiciça-* being sent among the people—this was taken by Hinz (1952, p. 32) to mean that the new script was disseminated among Darius' new subjects to be learned by them, but it may as well refer to the Old Persian version of the text (R. Schmitt, 2009, p. 87), to Darius' titles and his lineage as mentioned in OP IV 93–94 (Vallat, 2011, p. 268) or to "the political message conveyed by the whole monument" (Rossi forthc., § 2.1.3). As long as there is no agreement on the reading of the paragraph, Hinz' popular interpretation cannot be considered disproved, but it should be borne in mind that the notion "daß *Darius hier tatsächlich für sich in Anspruch nimmt, die altpersische Schrift eingeführt zu haben*"[22] (Hinz, 1952, p. 24) depends on a very specific and uncertain translation.[23]

So, while, in the case of Hankul, the discovery of a document explicating on the origin of the script helped to clear things up, the matter turns out to be more complicated in Old Persian. Apart from the doubtful meaning of the Behistun paragraph, a major stumbling block for Hinz' theory are a number of inscriptions from Pasargadae, the capital of Cyrus II. As in Behistun, the three relevant inscriptions come in triplicate in Elamite, Babylonian and Old Persian. CMa, preserved five times on antae and doorways, reads 'I [am] Cyrus the king, an Achamenid';

20. 'I have also translated this inscription into Aryan. This [the Old Persian part] did not exist here [on the rock face] before.'

21. Cf. Schmitt's translation of the Old Persian text: "[...] (ist) dies die Fassung der Inschrift, die ich hinzugesetzt habe, (und zwar) auf Arisch"—'[...] (is) this the version of the inscription which I have added, in Aryan' (2009, p. 87).

22. 'that *Darius really claims here for himself to have introduced the Old Persian script*'.

23. The interpretation of the section as referring to Old Persian cuneiform is considered to be supported by the fact that the Elamite and Babylonian parts of the inscription were inscribed simultaneously, whereas the Persian third of the trilingua was added belatedly (Mayrhofer, 1978, p. 7). There are issues, however, concerning the layout and the relative chronology not just of the three parts in their entirety, but of subsections (cf. R. Schmitt, 1990), as well as the language in which the text was originally composed (e.g., Bae, 2001, pp. 152–154; Tuplin, 2005, p. 221).

CMc, three times on reliefs depicting Cyrus, reads 'Cyrus the great king, the Achamenid'—no Old Persian versions are preserved of CMc, but a separate Old Persian fragment may belong here (R. Schmitt, 2009, pp. 9, 36). Hinz originally held that these inscriptions, like others from Pasargadae (particularly DMa as reconstructed by him), date from the reign of Darius, who had them inscribed to honour his predecessor (Borger and Hinz, 1959). Nylander (1967, pp. 151–170) adduces arguments to show that they are indeed of Cyrus' time, but suggests that only the Elamite and Babylonian versions were applied under Cyrus, whereas the Old Persian versions were supplied under Darius (p. 175–177)—a proposal followed by Hinz (1973, pp. 19–21) to circumvent the problem of pre-Behistun attestations of Old Persian and Old Persian cuneiform. Others, however, take the Old Persian inscriptions to be original as well (e.g., Diakonoff, 1970, pp. 100–103 with arguments).

Furthermore, a script invention under Darius has been questioned because of the logic (or rather the lack of such) behind the character inventory. Structurally, Old Persian cuneiform is basically an abugida, in which individual characters write a consonant plus one consistent standard vowel and different vowels are denoted by adding elements to the respective <CV>-characters. The graphs of Old Persian cuneiform imitate the general look of cuneiform characters, but are less graphically complex. Old Persian cuneiform has a complete paradigm of twenty-two characters for CV-syllables with inherent *a* (also *ə*); the syllables' vowel can be modified by way of additional vowel characters for *i* and *u*. Beside these, there are also a number of characters for CV-syllables with *i* or *u*. These bonus <Ci/Cu>-syllabograms are unevenly distributed: only two consonants are provided with three characters combining them with all three vowels. Two more get syllabograms with *i*, but none with *u*, with five it is the other way round, and the remaining thirteen consonants come only with the modifiable <Ca>-character. Some syllabogram-gaps are also linguistic gaps (e.g., the syllables *ki* and *gi* do not occur in Old Persian), but others are not (e.g., *ti*, *ni*). According to Mayrhofer (1979, p. 291), the C*i*/*u*-syllables which are represented by extra characters are no more frequent in Old Persian than the ones which are not. Conversely, characters for certain C*i*/*u*-syllables, e.g., *ti* in inflection, might conceivably have been useful (Mayrhofer, 1978, p. 8). A graphic reflection of assimilation processes is not plausible either (Hoffmann, 1976, p. 625f). The selection of <Ci/Cu>-syllabograms appears not to be linguistically motivated.

The orthography is perfectly straight-forward from the writer's perspective: an unmarked <Ca>-character represents C*a*, C*ə* or C, an additional <a> indicates long *ā*. If <i> or <u> follows a <Ca>-character, for whose consonant a <Ci>- or <Cu>-character, respectively, is available, a diphthong must be read. If, in the same case, no <Ci>- or <Cu>-character is available, the spelling is ambiguous: <d[a]-i> is *dai*, be-

cause *di* would be written <di-i>, but <p[a]-i> is *pai* or *pi*. While all <Ca>-characters, as is normal for abugidas, can be modified by the characters <i> and <u>, this is not the case for the <Ci/Cu>-characters, which never represent only the consonant or are modified to represent, e.g., C*a* (†<Ci-a>). However, the <Ci/Cu>-characters do not represent C*i*/C*u* on their own, as would be expected of syllabograms, but must still be accompanied by the respective vowel character (plene writing), so that long and short *i* and *u* cannot be distinguished: <Ci-i> is both C*i* and C*ī*. This redundant vowel marking also occurs sporadically with <Ca>-characters (<C[a]-a> for C*a* rather than C*ā*), conceivably paralleling the rule for <Ci/Cu>-characters (ibid., p. 627).

According to Hoffmann (ibid., p. 622), the redundant vowel marking in <Ci/Cu>-characters is a secondary development, due to an extension of the abugida-principle of modifiable syllable characters—the <Ci/Cu>-characters were originally "traditional" syllabograms. Hoffmann argues that relic spellings can be found in the Behistun inscription: while the text generally follows the standard orthography as outlined in the preceding paragraph, there are instances of <Ci/Cu>-characters being employed without the redundant vowel character, e.g., in the name of Darius' father Hystaspes, which is exclusively (nine times) spelled <vi-š[a]-ta-a-s[a]-pa-> *vištāspa-* (details in R. Schmitt, 1990, p. 26).

These inconsistencies could be explained as scribal errors (Werba, 2006, p. 266) or as the consequences of lack of experience with writing the new-fangled script. R. Schmitt (1990, pp. 25–28) interprets these and other spelling variants as evidence for different hands. Hoffmann, as indicated above, explains them as the remnants of an older orthography, which obviously requires a pre-Behistun existence of the system. According to Hoffmann (1976, pp. 621–623), there is general agreement that the script cannot be much older than the Behistun inscription and that it was not created for a different diachronic stage of Old Persian or even another dialect of Iranian (such as Median, as suggested by Diakonoff, 1970), seeing that the spelling conventions do ultimately fit well with Old Persian as it can be reconstructed from other sources (but see Hoffmann, 1976, pp. 643–645 on a potential historical spelling). An Iranian variety which has phonotactic restrictions fitting the gaps of the character paradigm is not known.

Mayrhofer (1979), following Hoffmann's lead, argues that the <Ci/Cu>- syllabograms are the remains of a defective writing tradition which predates Darius' reign. In reference to Hallock (1970), who connects the graphically simple characters <ku> and <ru> with the name *kuruš*, and Hoffmann's (1976) determination of the principles which (allegedly) govern the creation of the pseudo-cuneiform characters, he attempts to explain the seemingly random selection of *i*- and *u*-syllabograms. Mayrhofer suggests that, during the reign of Cyrus

II, the name of the king *kuruš* was already written with new, graphically simplified cuneiform characters, which represented syllables: <ku-ru-š>, using the syllabary-appropriate spelling conventions which, according to Hoffmann, can still be detected in the Behistun inscription. More characters for spelling frequent words followed—all with vowel *a*, until the names of Cyrus' sons became relevant: <ji> was created for *kamb*(*a*)*ujiya*- (Cambyses II), and <di> for *bardiya*- (Smerdis). The other <Ci/Cu>-characters, according to Mayrhofer, can be accounted for by the text of the Behistun inscription itself: to avoid increasingly complex character shapes, the scribes handled the creation of more <Ci/Cu>-characters economically. Unambiguous words such as Old Persian *puça* 'son' could be written with an ambiguous spelling (<p[a]-u-ca>), but <Ci/Cu>-characters were created for personal names, foreign names and other less commonly used words, e.g., <mi> for *armina*. A systematic character inventory, completely reflecting the phonotactic realities of Old Persian, did not come about due to the pressure of time under which the scribes of the Behistun inscription were working on their addendum (cf. already Hoffmann, 1976, p. 626f). Mayrhofer explains that his theory does not contradict the Schrifterfindungsparagraph (as such), if one reads *tuppime* as 'text' rather than 'script' so as not to exclude the existence of older documents in which certain characters were used to write names. He also believes, like Hallock, to be able to deduce the order in which the characters were created from their graphic complexity, assuming that the simpler a character, the older it is.

Mayrhofer's theory cannot satisfactorily explain all the data—particularly the lack of <Ci/Cu>-characters which would conceivably have come in handy: the lack of a syllabogram for the frequent inflectional ending *ti* can be accounted for, as common vernacular sequences did not have to be spelled unambiguously (1989, p. 180), but there are also syllables in (foreign) names in the Behistun inscription which are spelled ambiguously (Mayrhofer, 1989, p. 182f with explanation attempts). The potentially archaic Pasargadae inscriptions do not support the theory: both CMa and the possible fragment of CMc use standard orthography, also in the spelling of *kuruš* (<ku-u-ru-u-š[a]>) (R. Schmitt, 2009, p. 35f). Still, the theory is accepted by Schmitt 1981, p. 20 and Werba (1983). The latter suggests a more specific model to account for some problems, proposing that the invention of the new script had been commissioned by Smerdis, who would have had as good a motive as Darius for launching a prestige enterprise. Werba reconstructs a hypothetical monumental inscription written with a syllabary, in which the <Ci/Cu>-syllabograms <ku>, <ru>, <ji>, <di> and <nu> occur in Smerdis' name **Bərdiδanu*-. Darius, he suggests, had the monuments of Smerdis' rule destroyed and announced himself as the originator of the script in his own imperial inscription, wrongfully claiming the merit of having created a script for his people. It was only Darius' scribes, schooled in Aramaic,

who introduced the abugida-principle of inherent *a* and re-interpreted some of the old syllabograms accordingly.

Whether the scenarios posited by Mayrhofer and Werba are correct in detail or not, it appears that the inconsistencies in the system are best explained as the consequence of the existence of a (defective?) version of the script prior to its wide dissemination under Darius. As far as this proto-version is concerned, we end up in the same situation as with any undocumented emergence of a new script, not knowing whether the syllabograms were a purposeful creation (as proposed by Werba) or a kind of inconsistently used shorthand which took over gradually before being taken care of by Darius' scribes.

3.5. Being Credited With Creating a Script: Eastern European Alphabets

Despite the fact that the brothers and missionaries Constantine and Michael, later St. Cyril and St. Methodius, undoubtedly played an important part in the history of Eastern Europe, the chronology of the writing of the Slavic languages is still not quite cleared up. The Glagolica, whose character forms are more difficult to derive from a model than those of the Cirilica, which are mostly recognisably Greek, is generally held to be the older adaptation, and the one that is attributed to Constantine, while the Cirilica—despite its modern name—postdates the Moravian mission (Cubberley, 1996, p. 346; Franklin, 2002, p. 93 with n. 38). Constantine's dissatisfaction with the lack of a script for the Slavic language and his creation of the Glagolica, performed spontaneously under divine inspiration before the mission even started, is made much of in the *Vita Constantini*, and indeed the difficulty of finding convincing models for many letters and the apparent mixture of sources has led to a communis opinio which considers the Glagolica a completely independent effort on the part of Constantine (Cubberley, 1982, p. 291; Franklin, 2002, p. 93f). Dissonant voices which argue for pre-Christian writing of Slavic point to two sources: the treatise *On Letters* by (maybe) the Bulgarian monk Khrabr (late 9th or early 10th century), which mentions that the Slavs had "read and divined" by means of "marks and notches" before the establishment of the Glagolica, and a (palaeographically uncertain) reference in the *Vita Constantini* to a Gospel and Psalter written "in Rus letters" which was acquired by Constantine in the Crimea (see ibid., p. 90f for details). Cubberley (1982, p. 292), arguing that Constantine would not have based a script with which to write the Bible on the Greek cursive, from which the Greek-looking Glagolitic letters are best derived, unless he had an already existing Slavic writing tradition to refer to, suggests that there was such an older tradition of writing Slavic with the Greek cursive which had arisen "more or less spontaneously" to fulfil "practical needs of com-

merce and militarism" (p. 291), and was only expanded by Constantine by adding letters for the sounds not present in Greek. Again, we are stuck with the question of whether this putative original version was the result of someone's purposeful adaptation, or whether Slavic was occasionally and unsystematically written with Greek cursive letters before Constantine took the matter in hand (and maybe thereby checked a potential gradual spread and customisation).

From times closer to the emergence of the fuþark, two oft-cited ecclesiastical figures whose work as script inventors is also connected with spreading the Word of God are the Gothic bishop Wulfila and the Armenian vardapet Maštocʿ. Wulfila is credited with the invention of the γράμματα γοτθικά, a Gothic alphabet which is an adaptation of the Greek cursive specifically for his translation of the New Testament around the middle of the 4th century AD, by various ecclesiastical historians already in the 5th century (Krause, 1968, p. 63; Scardigli, 1998, p. 455f). The creation of the Armenian alphabet in the early 5th century AD by the learned cleric Maštocʿ is equally well established, even though he arguably did not work alone, and though the derivation of individual characters is still under discussion. Though the alphabet created by Maštocʿ appears to be original, there was an earlier script. The vardapet's disciple and biographer Koriwn tells of how the king sent an emissary to a Syriac bishop called Daniel to learn letters. The "Danielian" script referred to here may have been an adaptation of the Aramaic alphabet devised by Daniel, but Koriwn's assessment that the characters were a random collection of foreign letters, little suited to represent the sounds of Armenian, may indicate an older tradition of writing Armenian with Semitic scripts. Maštocʿ spent two years teaching this script before he got tired of dealing with its shortcomings and proceeded to create a better system with the help of a Greek scribe (Krikorian, 2011, p. 65f). It cannot be demonstrated that he used the unattested Danielian script as a basis for his alphabet, but only twenty-two of the original thirty-six characters of the Armenian alphabet can be derived from the Greek cursive—unless one wants to assume that Maštocʿ invented the other shapes freely, the best models are found among Semitic scripts. A number of possible sources present themselves, but the best candidates are Pahlavi, used in Armenia before the Christianisation, and the Syriac script, which was like Greek used to write Armenian biblical and liturgical texts (Sanjian, 1996, p. 356f). The possible existence of scripts for Caucasian languages prior to the ones known today is also discussed for Caucasian Albanian (Kananchev, 2011, p. 61f) and Georgian (literature in Imnaishvili, 2011, p. 51; critical Seibt, 2011, p. 85).

The scripts discussed so far have in common that their creation is ascribed to "culture heroes"—self-proclaimed or established through history. While in the case of Hankul, a creation from scratch performed by King Seycong or rather under his aegis is reliably documented by

sources, the circumstances of the emergence of Old Persian cuneiform and of the Eastern European alphabets are at least (to varying degrees) debatable. For Wulfila, the testimony of the sources is not usually called into question (Ebbinghaus, 1996, p. 290), but the men who are credited with the creation of Old Persian cuneiform and the Glagolica have been argued to have reworked pre-existing traditions of writing in the very languages for which they are supposed to have first created their new script. Unless one would claim that all the respective proto-versions were in turn invented by unknown individuals (as with Smerdis' original Old Persian cuneiform according to Werba),[24] we may in fact be concerned with cases of script diffusion, i.e., the employment of a foreign script for one's own language without any prior established adaptations, whose existence was obscured by the secondary intervention of individuals who were in a position to establish extensive changes. The possibility that such earlier versions influenced or even formed the basis of the later reworkings lends an aspect of "naturalness" also to the development of seemingly independent creations. In cases where either no secondary intervention happened, or an intervention happened late enough that we have a lot of older material, we observe script diffusion and gradual development.

3.6. "Ingenious" or "natural"? The first alphabet(s)

The farther back we go in time, the more does the historical figure of the script creator become indistinguishable from the (semi-)mythical script giver whom we know from numerous ancient cultures. In Ancient Greece, it is the name of Kadmos which is associated with the introduction of writing. This connection is so vague that it has even been questioned whether the "Phoenician characters" introduced by Kadmos are the alphabetic ones, which are indeed derived from a North Semitic source, or those of Linear B, which fit better dating-wise (cf. Rocchi, 1991, p. 529 with n. 2; Voutiras, 2007, p. 266f). In any case, the exact circumstances of the emergence of the Greek alphabet remain obscure. A rough time frame is formed by the use of Mycenean Linear B on the Greek mainland until the end of the 12th century and the appearance of the earliest alphabetic documents around the middle of the 8th century. That the Greeks were closely engaged with the Phoenicians through trade in this phase is clear, but a precise dating or location of the transfer is difficult—while classicists, following Carpenter (1933), have tradition-

24. Cubberley (1996, p. 346) ascribes the formation of this proto-alphabet to "some Slavs".

ally favoured a terminus post quem in the late 9th century,[25] Semitists tend towards an earlier transfer date in the 11th century (thus now also Waal, 2020). Furthermore, the central role that has traditionally been accorded the Greek alphabet in the introduction of vowel letters and the spread of the alphabet in the northern Mediterranean is increasingly being called into question.[26]

In his extensive treatment of the matter, Wachter (1989) evokes a somewhat fanciful scenario featuring at least two Greeks and one Phoenician, all of them merchants, gathered together in "einer kleinen Tafelrunde an einem angenehmen Sommerabend"[27] (p. 37). The Greeks, presumably provided with a document containing a Phoenician alphabetarium, memorised what Wachter calls the "Merkspruch", i.e., the recited row of letter names to accompany written alphabetaria. The fact that the Phoenician letter names, meaningless in Greek, were retained is taken by Wachter as indication that the creator of the Greek alphabet was not an individual who knew both spoken and written Phoenician, as such a person would, he argues, have understood the significance of the Phoenician names and would have replaced them with semantically transparent Greek lexemes.

Despite the many local variants attested in the archaic phase and despite the general assumption that the contact between Greeks and Phoenicians was extensive and not locally restricted, a monogenesis of the Greek alphabet as represented by Wachter is communis opinio because of the "auf jeden Fall genialen" ('definitely ingenious'; Wachter, 1987, p. 11) reassignment of a number of Phoenician letters to write the Greek vowels: ʾālep—alpha, wāw—upsilon, hē—epsilon, yōd—iota, ʿayin—omicron. Yet it is evident that the introduction of the vowel characters is connected to the letter names. The Phoenician consonantal anlauts of all corresponding letters except wāw (glottal stop, voiced and unvoiced pharyngeal fricative, palatal glide) were non-phonemic in Greek and may consequently be argued to have simply been lost to speakers of Greek. The resulting, effectively vowel-initial names could then have determined new sound values according to the acrophonic principle. While this works out for ʾālep, hē, yōd and eventually also ḥēt[28], the correlation between ʿayin and *o* is more difficult to argue phonetically. The Semitic voiced pharyngeal fricative did tend to occur in

25. E.g., Heubeck (1979, pp. 75–80); Jeffery (1990, p. 18); Swiggers (1996, p. 267); Woodard (2014, p. 3).

26. E.g., Brixhe (2004); Waal (2020); Elti di Rodeano (2021).

27. 'a small Round Table on a mild summer evening'.

28. Ḥēt was initially used to write *h*, and only came to designate long open *ē* after psilosis eliminated anlauting *h* in Ionian dialects in the 6th century; omega was subsequently introduced for long open *ō* to parallel this distinction between long and short vowel.

the context of *o* (Driver, 1948, p. 179) and has been claimed to have a rounding effect upon *a* (Gardiner, 1916, p. 11; Allen, 1987, p. 171), but the motivation is dubious—the assignment of ʿayin to designate *o* may have been a conscious decision, coupling the last remaining "vowel-initial" letter name with the left-over vowel.

Unlike *i̯*, *u̯* was phonemic in most Greek dialects at the time, so that wāw was initially retained as a consonantal character; a graphic variant to represent *u* was added after tau. Wachter (1989, pp. 37–40), dismissing the evidence of the Würzburg tablet alphabetarium which was presented by Heubeck in 1986 and assuming that upsilon was present as the first additional letter in all known alphabetaria, takes this to indicate that the letter was introduced at the first creation of the alphabet, as an amendment by a person who noticed the asymmetry—viz. the missing letter for one of the five vowels—conditioned by the different phonemic status of the Greek glides. Despite the difficulty in deciphering the alphabetaria on the Würzburg copper plaque, however, Heubeck's original assessment was correct: analysis with modern techniques shows that the Würzburg tablet as well as its two "sisters" (the Fayum tablets, cut from the same copper sheet) feature a Greek alphabet without upsilon (Woodard, 2014, p. 1f). Even if the plaques should be younger than the late 9th century and the inscribed alphabets be ritually archaising (ibid., p. 3f), they testify to an archaic Greek alphabet with the same number of letters as its Semitic model. That yōd was used with a vocalic quality, while wāw retained its consonantal value and the corresponding vowel character was only appended secondarily, points towards a mechanical interpretation of the letter names.

Apart from ʿayin for *o*, there are a number of Phoenician letters whose Greek sound value is not self-evident (Heubeck, 1979, p. 89f). The letter for the Semitic aspirated unvoiced stop (tāw) was not used for that sound's Greek counterpart, but for Greek unaspirated *t*; instead, ṭēt (for the Semitic emphatic unvoiced stop) came to designate Greek t^h. This distribution, in itself surprising, is not even consistently executed: among the velars, kap (Semitic aspirated) designates Greek *k*, but qōp (Semitic emphatic) is not used for the aspirated unvoiced k^h, but displays the typical features of a retained superfluous letter. It appears to have not been used in practice in a number of alphabet variants; where it is employed, it redundantly designates an allophone of *k* in certain contexts (again determined by the letter name). The question of how exactly the four Phoenician letters for sibilants were dealt with is unclear; their treatment (according to the theory of Jeffery, e.g., 1990, pp. 25–28) is adduced as an argument for monogenesis by Marek (1993, p. 29). The use of zayin (*z*) for the Greek dental affricate is general, but different alphabet variants chose šīn or ṣādē for the unvoiced sibilant.

Heubeck (1979, pp. 94–100) prefers to think of a polygenesis, arguing that the above-mentioned distributions of sound values are not so odd

that they could not have come about repeatedly and independently—the vowels possibly with influence from Semitic matres lectionis (cf. Segert, 1963, pp. 48–54; Marek, 1993)—while the early variations are difficult to explain if a one-off authoritative creation is assumed. The latter argument is also adduced by Cook and Woodhead (1959, p. 178), who dismiss the notion of an Uralphabet, but allow for the possibility that the distribution of the vowels was determined only once and spread through the local variants. The naturalness of the mechanics of sound substitution which can explain the changes effected in the transmission is stressed by Brixhe (2007, pp. 282–285), who decidedly rejects the notion of a single creator. The polygenesis theory does rather diminish the relevance of the ingenious creator, assuming instead that any number of Greek (or, for Brixhe, also Phrygian, or, for Waal, unspecified Indo-European) merchants could at some point have had a Phoenician trade partner teach them to write the characters and say the Merkspruch, and come up with a full alphabet by simple sound substitution without a conscious effort to improve upon the system as they had learned it. The monogenesis theory obviously leaves room for the Greek culture hero,[29] but with regard to the possibility that the changes introduced are mechanical, it does not exclude the possibility of unsophisticated adoption (e.g., Marek, 1993). Jeffery, 1990 is also sceptical of the existence of a Greek εὑρετής (p. 4), arguing that the less obvious innovations of the Greek alphabet only indicate that it originated within a limited area (p. 7).

Jeffery cites the Etruscan alphabet as an example for her transfer type 1 of contact-induced diffusion. Indeed, the spread of the alphabet to and within Italy is, I believe, widely considered to have happened without the intervention of a script creator—harking back to the remarks on the distinction between different scripts in section 3.1, one might argue that this is because there is no recognisable point at which a new alphabet emerged, even though many of them do end up recognisably different graphically as well as orthographically. According to the traditional account, the Etruscans learned to write from the Greek settlers (or traders) of Pithekoussai and/or Kyme in the 8th century, with whom they must have been in contact since the founding of the colony/trading post. Pithekoussai is the find place of one of the oldest preserved Greek inscriptions, the Cup of Nestor, dated to the last quarter of the 8th century BC (ibid., p. 235). The oldest document of written Etruscan, a kotyle from Tarquinia, is dated to about 700 (Wallace, 2008, p. 17). There are hardly any formal differences between the two inscriptions (different orientation of sigma, asymmetrical vs. symmetrical alpha and slightly different forms of pi)—were it not for the different languages, the two documents would be considered to be writ-

29. See Marek (1993, p. 27) and Heubeck (1979, p. 87f) (n. 520) for collections of epithets.

ten with the same script. The different language is reflected in the script by the non-occurrence of beta, delta and omicron in the kotyle inscription (the corresponding phonemes not existing in Etruscan), and indirectly also in the use of gamma to write not a voiced stop, but the palatal allophone of the unvoiced stop according to the *kacriqu*-rule.[30] The oldest Etruscan alphabetarium (on an ivory writing tablet from Marsiliana d'Albegna; about 650) shows a deviation from the Greek alphabet, in its eastern Greek "red" variety as used in Euboia, in the form of san, which does not co-exist with sigma in any Greek alphabet, and is traditionally considered to be borrowed from a different variety. Still, Wachter (1989) treats the Marsiliana d'Albegna alphabetarium as a testimony for the chronology of the early Greek alphabet. Only by and by do the documented alphabetaria reflect a process of adaptation to writing practice (see also Maras, 2014, p. 77). The archaic Etruscan inscriptions were written with a script that was, for all intents and purposes, Greek, and even the later adaptations came in such a piecemeal manner that it is hard to argue for comprehensive orthographic reforms—and if there were such reforms, they only officially implemented previous developments which had gradually established themselves in use. The existence of local varieties points in the same direction. Yet despite the fact that one could argue that the early Etruscan alphabet is the same script as the early Greek alphabet(s), their subsequent developments result in distinct scripts.[31] Had the documents of archaic Etruscan—a mere eighth of the corpus—not come down to us, the neo-Etruscan alphabet with its evolved letter forms and discarded and additional characters would look like a fairly well thought-out purposeful adaptation.

The same goes for the Latin alphabet, whose emergence is not as well documented—notably, archaic alphabetaria are lacking. The (partial) employment of the Etruscan *kacriqu*-rule in early Latin inscriptions suggests that no systematic adaptation was made prior to the use of Graeco-

30. In archaic Etruscan inscriptions, kappa is used before *a*, gamma before front vowels, qoppa before *u*. This orthographic rule has been explained as phonetically motivated (distinguishing three allophones of the unvoiced velar stop), e.g., Cristofani (1972, p. 471), or as conditioned by the Phoenician/Greek letter names (extending the Greek convention of the use of kappa and qoppa, e.g., Wachter (1987, pp. 16–18); Wachter has to assume that the name of the third letter was *gemma* rather than *gamma*). Of course, the two explanations ultimately amount to the same thing, as the phonetic distinction, even if it was purposeful, must have been suggested by the letter names and the Greek practice.

31. Cf. also Prosdocimi (1990, pp. 195–203), who stresses the difference between the "alfabeto princeps" (the attested alphabetarium) and the "corpus princeps" (the entirety of texts available for reference to the writer) and argues that orthographic rules ("regole d'uso") make the difference between scripts and, consequently, that the Marsiliana d'Albegna alphabetarium, belonging with an Etruscan "corpus princeps", must be considered an Etruscan document.

Etruscan letters to write Latin: while the rule was merely unnecessary in Etruscan, it was detrimental in Latin, where it blocked the use of gamma for *g* (Wachter, 1987, pp. 19–21). One would expect a sophisticated inventor to realise the relevance of gamma next to that of beta and delta, whether the latter were available from the Etruscan model alphabet or taken from the Greek one. Wallace (1989, p. 123f) suggests that ritual exchange of gifts—sometimes inscribed—across ethnographic/linguistic boundaries provided the context for a diffusion of writing into the early Roman culture.[32]

As a final alphabetic example, the North Italic Venetic writing culture is special insofar as there is ample evidence not merely for institutionalised writing, but for a writing cult (see Marinetti, 2002, p. 40f). The oldest Venetic documents demonstrate an early break in the tradition: there is evidence for an archaic Venetic alphabet ("phase 1") which shows similarities with that of the Northern Etruscan city of Chiusi, while the younger variants ("phase 2") are clearly connected with the writing cult of the Portonaccio sanctuary in Southern Etruscan Veii (e.g., Prosdocimi, 1988). The Venetic case appears to provide a solid example for an early unsystematically adopted script being superseded by a sophisticated and institutionalised adaptation as suggested for some of the above-mentioned scripts.[33]

Wachter (1987, p. 8) emphasises the importance of the Merkspruch for the spread of the alphabet in Greece and Italy. Where alphabetaria demonstrate that the order of the row was preserved, they testify to the art of writing being taught and learned—the testimony of the Marsiliana d'Albegna alphabetarium is important not only because it is old, but also because it is inscribed on the rim of a writing tablet, presumably to act as a memory aid for the writer who used the tablet. This does of course not exclude the intervention of an individual adaptor, but the almost seamless adoption of the alphabet in Italy by speakers of various languages in the two centuries following its establishment in Greece points to a "mechanische und ganz auf die Praxis ausgerichtete Methode [...] und eine theoretische Verfeinerung normalerweise erst in zweiter Linie"[34]—thus Wachter (ibid., p. 13) despite his conviction that acts of script creation were performed in Italy (p. 24) as well as Greece. The alphabet in Italy does indeed seem to be a fairly clear case (or collection of cases) of the gradual diffusion of scripts into previously illiterate communities.

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32. Wallace does, however, speak of bilingual "authors" (p. 126); at what point these people are thought to have set to their adaptation work is not made clear.

33. Maggiani (e.g., 2002, p. 56) goes so far as to identify one Pupon Rakos, named on the oldest phase-2 document from Padova, as the Etruscan responsible for establishing Southern Etruscan cult and writing culture in the Veneto.

34. 'mechanical and entirely practice-oriented method [...] and a theoretical refinement usually only secondarily'.

The more or less problematic examples of purposeful and sophisticated script creation and of unsophisticated script transfer and development discussed so far can be juxtaposed with cases where a succession of learned users take a long time to adapt a foreign script to their own language (gradual sophisticated development) and with such where illiterate grammatogenists produce perfectly acceptable systems which have only a passing similarity to the model (unsophisticated script creation). Examples for the two latter types of transfer will be presented in the following sections.

3.7. Unsophisticated Inventors: The Cherokee Syllabary

The Cherokee script was invented by an ingenious tribesman between 1809 and 1821. Unfortunately, information about Sequoyah's life is sparse and partly unreliable; the accounts are collected in Davis (1930). It seems clear that Sequoyah was monolingual (ibid., p. 155) and illiterate. He did, however, understand that shapes which he observed printed on paper (viz. Latin letters) reflected speech. After claiming before the patrons of his public house that he could come up with a tool which would allow the Cherokee to communicate by means of "talking leaves" in the manner of the foreigners, he set to work, initially attempting to invent a character for every word in his language. After realising that such an approach would require more characters than could easily be remembered, and that characters for concepts were not practicable either, he hit upon the notion of writing recurring sounds. According to Davis (ibid., p. 160), he did not rely upon his own language competence, but also listened to others to make sure that all sounds would be represented. He "obtained an old English book" (ibid., p. 30) and used most of the character shapes he found there, modified some and invented the rest. Similarities of Cherokee characters with Latin ones and with Arabic numerals are entirely graphic—since Sequoyah did not read English, there is no correspondence in the sound values. Similarities with letters from the Greek and Cyrillic alphabets (Scancarelli, 1996, p. 587) are probably fortuitious. It must also be pointed out that Sequoyah's original characters were soon assimilated to the letters which were available in printing presses. An early source stresses Sequoyah's lack of "sophistication":

> A form of alphabetical writing invented by a Cherokee named George Guyst,[35] who does not speak English, and was never taught to read English books, is attracting great notice among the people generally. Having

35. Sequoyah's English name, inherited from his allegedly German father (Davis, 1930, p. 153f).

> become acquainted with the principle, that marks can be made the symbol of sound, this uninstructed man conceived the notion that he could express all the syllables by separate characters, but for the specific purpose of writing his native language" (from *The Christian Observer* [London], vol. 26 [May 1826], 317; quoted from Davis (1930, p. 154) [n. 22]).

The result of Sequoyah's efforts, a syllabary of eighty-five characters, is better suited to write Cherokee that the adapted Roman alphabet, though its creator's lack of linguistic training is reflected in the system not being able to write the language quite unambiguously (Scancarelli, 1996, p. 590; Scancarelli, 2005, pp. 359–364)—it would appear that Sequoyah, while paying particular attention to the realities of spoken Cherokee, did not go out of his way to make his system symmetrical.

After a rough start, which almost saw the man executed for sorcery (Davis, 1930, p. 161), Sequoyah's creation took on very well (Walker and Sarbaugh, 1993; Cushman, 2010)—and not only among his own people. According to Unseth (2016), the Cherokee example was emulated by a great number of illiterate societies, inspiring the creation of twenty-one scripts for over sixty-five languages. Sequoyah's case is indeed the first documented case of unsophisticated grammatogeny in Daniels' narrow sense (involving a single creator). More examples can be found in Daniels (1996a), Singler (1996) (West African examples) and Ratliff (1996) (Pahawh Hmong script).[36] A recurring element is inspiration from a dream, which was claimed by the creators of the Vai script in West Africa (Singler, 1996, p. 593f), of the Afaka script created for the Ndjuka creole of Surinam (Daniels, 1996a) and of the Bamum script (A. Schmitt, 1963). The circumstances of the latter's creation are well researched: Njoya, head of the Bamum tribe of Cameroon, became aware of other peoples' ability to communicate via signs made on paper (ibid., p. 14). He first conceived of about four-hundred and fifty iconic ideograms designed for mnemonic purposes (ibid., pp. 110–112). Between 1896 and 1910, a series of six well documented revisions, in whose course Njoya and his scribes re-invented the rebus principle, introduced syllabic writing, which culminated in an eighty-character syllabary called *akauku*.

3.8. Sophisticated Users: Writing Japanese

The converse scenario can occur in a previously illiterate society whose (or some of whose) members have literacy in the source language, coupled with a high level education associated with the prestigious foreign culture—under such circumstances, the employment of the script for the vernacular may happen rather late. Professional scribes who, once the

36. See also Walker and Sarbaugh (1993, p. 88) (n. 1).

notion becomes popular, begin to spell their native language with the well known characters must be assumed to make decisions and come up with solutions which are different from those of people who have only a cursory user's understanding of how the script relates to the source language.

The transition from writing Chinese with Chinese characters to writing Japanese with Japanese kana did not happen suddenly. Chinese writing was first brought to Japan by Korean scholars in the 4th or 5th century AD in the course of a general Sinicisation of Japanese culture. While the Chinese characters, called *kanji* in Japan, were soon used to write Japanese, adaptation processes appear to have started only in the 9th century. All stages of the change from Chinese logo-syllabic to Japanese syllabic writing are not only attested, but still in use today.

The kanji in their original form are used as logograms, i.e., with focus on their semantic content (e.g., the kanji for 'ten' being used to write 'ten' in a Japanese text). For the pronunciation of a kanji, there are two options: on- and kun-reading. On-reading means that the kanji is read according to the Chinese pronunciation (e.g., the kanji for 'ten' being read *shi* 'ten' in Chinese). For kun-reading, the designated word is translated into Japanese (e.g., the kanji for 'ten' being read *tō* 'ten' in Japanese). Whether, for any one kanji in a text, on- or kun-reading is intended must be judged from context. The matter is further complicated by the fact that a kanji can have more than one meaning (e.g., literal vs. metaphorical), and more than one phonetic shape can be associated with a meaning in either language. Furthermore, the phonetic shape based on on-reading is variable due to the phonetic differences between the two languages (i.e., the Chinese phonetic sequence in an on-reading may come out in different ways when pronounced by Japanese speakers), and kun-readings may only approximate the Japanese phonetic shape of the word. There are also conventionalised on-readings, whose pronunciation depends on when they were introduced from which Chinese dialect, resulting in multiple on-readings for one kanji (which can even include conventionalised misreadings). The two readings may be mixed in compound (two-kanji) words (Taylor and Taylor, 1995, pp. 299–303).

These multiple readings become especially relevant when kanji are used to write phonetically. Just like the Koreans, the Japanese saw the necessity to write not only lexical items, but also their grammatical morphemes. To represent a Japanese syllable, a writer could theoretically obtain a sound value via any of the readings described above, always ignoring the respective kanji's semantic content—shakuon/ongana is a phonetic character obtained through on-reading, shakukun/kungana is one which is based on kun-reading. So, the kanji for 'ten' could theoretically be used to write the syllables *shi*, *tō*, *to*, or any of the other sound shapes available through the various reading options mentioned above (examples in Tollini, 2012). A reasonable preference for graphically sim-

ple kanji with convenient one-syllable readings did something to limit the plethora of options, gradually reducing the number of kanji which were habitually used to write phonetic sequences. By the 9th century, the preferred kanji formed a usable system called *man'yōgana*. This was then further simplified to two distinct syllabaries: katakana and hiragana. Katakana developed from the use of man'yōgana in interlinear or marginal glosses, with drastically simplified characters for small and quick writing. Hiragana sports more artful character shapes, being a cursive script used mainly for writing literature. Only a few corresponding hiragana and katakana characters have been derived from the same kanji (Taylor and Taylor, 1995, pp. 306–308).

Though it cannot, of course, be excluded that, at one or different times, scribes who struggled with the use of kanji to write Japanese made coordinated efforts to reduce and systematise character use, the development of kana happened gradually, only governed by the needs of an uncoordinated writing community and the willingness to follow emerging conventions, however random. Tollini (2012, p. 171) refers to the importance of the early 8th-century chronicle *Kojiki*, the first lengthy text in Japanese, which is prefaced by a passage explaining the difficulties in writing Japanese with Chinese characters and indicating the strategy used in *Kojiki*—such a seminal work may well have served as a reference text for scribes, not unlike the orthographic conventions of Luther's German Bible translation were used as a model by early printers. Still, the general predilection for culture heroes does not exclude Japan: the Buddhist monk Kūkai, founder of the Shingon school of Buddhism, who lived around AD 800, was the right man at the right time and place to be credited with taking the definitive step towards the purely phonetic writing of the Japanese language. Trained in reading the original Indic Buddhist texts, he was acquainted with a purely phonetic writing system. The 11th-century poem *Iroha uta*, famous for containing each of the archaic kana once, is ascribed to Kūkai, but this is not supported by historical sources (Taylor and Taylor, 1995, p. 308). Kūkai's role in the development of phonetic writing in Japan, opposed to that of countless nameless civil servants and scribes taking one little step at a time, is highly questionable. This development may be considered to represent a case of "sophisticated diffusion", with a considerable number of competent users independently introducing changes which are discarded or adopted to gradually accumulate and form a new system.

A similar scenario can be envisioned for the distribution of the Latin alphabet in Europe. The persons who employed the Latin script for writing their native languages were ecclesiastical and lay scholars trained not merely in writing the model language with the associated script, but with a classical education—men who can be assumed to make informed decisions when applying themselves to the task of adapting a script. Yet the adaptations were introduced in a piecemeal manner

to form the various national alphabets with their special characters—often evolved from diacritics—and their different orthographies. The prominent difference between the development in Japan and in Europe is that in the former case, the results were scripts which are typologically different from the model, precipitated by the fundamental difference in language type, whereas the European alphabets keep the structural properties of the model.

3.9. Misunderstood Models? The Indic Scripts

In 1905, the missionary Alfred Snelling and a group of men from Chuuk island got lost at sea and ended up on the more westerly Eauripik island. A few months later they were transferred by the inhabitants to nearby Woleai island, where Snelling died. His fellow passengers returned home, but left their alphabet behind. The Chuukese had been blessed with script, in the form of a minimally adapted variant of the Roman alphabet, in 1878 by an American missionary who introduced regular syllabic letter names for consonants, all following the pattern C*i*. The inhabitants of the Eauripik and Woleai islands must have been taught the letter values through recitation of these names; the difficult circumstances of the transfer and subsequent breaking-off of contact made possible a misinterpretation: left to draw their own conclusions, the islanders took the C*i*-letter names to be the actual sound values, which resulted in a rather lopsided syllabary. The C*i*-characters were used to write all CV-syllables and word-final C; the correct vowel could only be indicated in syllables without an initial consonant (with the four non-*i* vowel characters). A few years later, the system was expanded to designate syllables with vowels other than *i*. The new characters were created mostly according to the rebus principle, i.e., stylised drawings of things whose names correlate with the syllable. Others are modifications of the corresponding C*i*-characters, and four appear to be modelled on Japanese characters. Riesenberg & Kaneshiro 1960, p. 295 assume that four to ten Faraulep islanders were responsible for the creation of this younger version (confusingly called "type 1"), though variants of both the old, defective system ("type 2") and the new, expanded one indicate an "interactive and partly indirect mode of script transmission (and possibly development)" (Justeson and Stephens, 1993, p. 9). Neither type appears to have been widely used; a standard (Roman) orthography for Woleian was created in 1951 (Voogt, 1993, p. 8).

According to Justeson and Stephens (1993), a similar mechanism, viz. a misunderstanding concerning the actual sound values arising from a syllabic strategy of teaching (letter-value recitation, letter naming or syllabic spelling paradigms), caused the formation of a number of other syllabaries, alphasyllabaries and abugidas, including the Old Persian, the

Indian and the Iberian script. Where the receiving group is not literate in the model script, what Wachter calls Merkspruch is "the only shared context of sign use" (Justeson and Stephens, 1993, p. 6). In Old Persian cuneiform, the <Ci/Cu>-syllabograms would then originate from the character names of the Mediterranean area, whereas the <Ca>-characters would go back to the same Aramaic scribal school tradition as the Indic ones (ibid., pp. 33–36).

A connection with the Aramaic abjad is evident for Karoṣṭhī, the older of the two original Indic scripts, which was used to write Gāndhārī in the north-western area of the Indian subcontinent and was ultimately abandoned in favour of Brāhmī. The area in which Karoṣṭhī was used coincides with that which had been under Achaemenid rule; Aśokan edicts are attested in Aramaic versions. Furthermore, the majority of Karoṣṭhī characters correspond to Aramaic counterparts (Salomon, 1998, p. 52). Much like in the Runic script, however, some correspondences concern both character shape and sound value, while others are purely graphic, the Indic sound value being unconnected to that of Aramaic—for example, the Karoṣṭhī character which is graphically based on bēt represents the sound value *ba*, but a character which resembles tāw represents *pa* (Falk, 1993, p. 103). Also reminiscent of Runic character derivations is the necessity to assume inversion, cursivisation and disambiguation. Of the Brāhmī characters, only about half can be associated with Semitic ones, but a derivation from the Aramaic script remains the best option (Salomon, 1996, p. 378; Salomon, 1998, pp. 28–30). Neither of the scripts was originally developed to write Sanskrit (Falk, 1993, p. 134; Salomon, 1998, p. 16).

This sheds doubt on whether the emergence of Indic literacy is connected with Brahmanic scholarship. Going by the extant data, India had a grammarian tradition at the time the Indic scripts (were) developed, whose representatives have been assumed to be responsible for the creation of both systems (see Falk, 1993, p. 133f). The oldest inscriptions, which provide evidence for both Karoṣṭhī (in the north-west) and Brāhmī, are the Edicts of Aśoka, dated to the mid-3rd century BC. Unless one considers the Indic scripts (or one of them) to be at least a century, maybe up to three centuries older than the Aśokan documents (depending on the preferred dating of Pāṇini), the grammarian tradition predates Indic writing.

Regarding the first attestation on the Aśokan stelae, the situation is similar to the Old Persian one in that the first documents are proclamations made by a historical ruler. While for Old Persian cuneiform, a one-off creation of the script has always been the starting point of argumentation because of the Schrifterfindungsparagraph, Aśoka's edicts make no meta-reference to the scripts in which they are written. Some scholars have ascribed the creation of one or both scripts to Aśoka himself or his scribes—see Falk (ibid., pp. 162–165), affirmative and with

arguments for the scripts being no older than the Aśokan inscriptions (e.g., the quick development of Brāhmī during Aśoka's time and the fact that the edicts refer to a proclamation of the texts rather than to reading or copying, which suggests restricted literacy). On the other hand, there is a school of thought which denies the possibility that the large Vedic and grammarian text corpus (especially Pāṇini's grammar) could have been passed down orally (see Bronkhorst, 2002, pp. 798–808 with literature). This position is hard to argue conclusively; putative evidence for a writing tradition prior to the time of Aśoka, including archaeological finds as well as literary references by vernacular and Greek sources, is inconclusive (Salomon, 1998, pp. 11–13). For example, Pāṇini, datable to the mid-4th century BC at the latest, makes reference to scribes (*lipikara*, *Aṣṭādhyāyī* 3.2.21), but this may well refer to foreigners, probably Aramaic scribes (Hinüber, 1990, p. 58; Falk, 1993, p. 258). There is, however, epigraphic material in the form of a group of potsherds, inscribed with proper names, from Anurādhapura (Sri Lanka), which appear to come from strata ^{14}C-dated to the early 4th century BC at the latest (Salomon, 1998, p. 12). Also, the existence of a "fully fledged writing system [...] available for Aśoka to use" (Norman, 1988, p. 14f) as well as alleged graphic variants in the Aśokan inscriptions (Norman, 1993, p. 279) have been used as arguments for a somewhat higher age of the scripts. Norman (1993, p. 279) explains the absence of older documents, much like his runological colleagues, with their being mere administrative records written on perishable supports, assuming that the Aśokan imperial stelae owe their existence to inspiration from Achaemenid monumental inscriptions. Salomon (1998, p. 13f) is inclined to accept an emergence of both Indic scripts in the 5th or 4th century BC, suggesting a scenario with which we are by now well acquainted, viz. that older, rather unsophisticated systems were revised and standardised to make a national script, developed under Aśoka for purposes of governing his vast pan-Indian empire.

Norman (1993, p. 280) attributes the inconsistency concerning the graphic correspondences between characters for similarly articulated sounds in Brāhmī to the script predating grammatical theory—but also Falk (1993, pp. 134–136), despite his preference of a later development, argues against a profound understanding of phonology on the part of the creators, pointing to a number of imperfections and inconsistencies with regard to how the scripts represent the phoneme inventory of the respective underlying Prakrit varieties which he considers to be incompatible with the notion of highly sophisticated inventors. He suggests the involvement of people who had some level of śikṣā training or a vague understanding of phonology as disseminated by such "Studienabbrechern" ('college dropouts') in Brahmanic circles.

An argument against specifically Karoṣṭhī as the work of grammarians is furnished by the character row. The varṇamālā, the standard or-

der of the characters in Indic, which is insightfully arranged by place and type of articulation, was created in the 4th century BC with regard to Sanskrit and does not originally have anything to do with writing. The characters of a script created by persons with Vedic schooling would be expected to be arranged according to the established varṇamālā sequence (as was indeed done later on, when Brāhmī was used to write Sanskrit). Yet, there is no evidence for this, or for an original arrangement which follows that of the abjad. Instead, there is evidence for a different original order of Karoṣṭhī—more in the Semitic style in its apparent randomness—called *arapacana* (after the first five letters). Mainly known in a Sanskritised version from Buddhist texts, the arapacana is epigraphically attested in four documents, none older than the first centuries AD (Salomon, 1990, pp. 258–268). It is not certain that this order is as old as Karoṣṭhī itself—it has been prominently, though tentatively, explained as a mnemonic device for a Buddhist text by Brough 1977, p. 93f. Salomon (1990, p. 271f) suggests the possibility that the arapacana is Karoṣṭhī's conventional character row, indicating that two of the arapacana-inscriptions, applied on writing boards, may be interpreted as the works of pupils and have parallels in inscriptions which have Brāhmī characters arranged according to the varṇamālā. He also points out that, where there is a connection between a character row and a text, it is usually the text which is arranged according to the established order, not the other way round. Salomon hesitates to fully commit to this interpretation because of the presence of twelve seemingly randomly selected characters for conjunct consonants in the arapacana, but Falk (1993, pp. 237–239) does prefer an interpretation of the sequence as an original letter row; the presence of a few obsolete letters may be taken to speak for its being archaic.

A possible point in favour of a sophisticated creation of the Indic scripts is the alleged correlation between language structure and script type: an abugida is an expedient system for languages in which, as in the Indic ones, one vowel occurs considerably more frequently than the others. Yet it would have to be assumed that this consideration was of sufficient appeal to cause the inventor(s) of the Indic scripts to reintroduce the syllabic principle into a script which was modelled on an abjad, rather than to use certain characters as letters for vowels like the Greeks. The latter approach was indeed to some extent followed in Karoṣṭhī, where the graphic correspondent of aleph is the letter for initial *a*, all other letters for initial vowels being graphic variants of it. Salomon (1998, p. 16) (n. 34) suggests that the established "concept of the akṣara or syllable as the essential unit of language" was responsible for the development of a syllable-based writing system in India—a notion which would point to the involvement of grammarians. Falk (1993, p. 336) notes a few (general) advantages of the abugida over the alphabet, but in my view the alternative scenario suggested by Justeson and

Stephens (1993) is altogether more attractive. Rather than being the result of profound linguistic insight, the system originated in "a basic misunderstanding of the principles of the parent system" (ibid., p. 37). An imperfect knowledge of the model could also explain the shape-only correspondences between Aramaic and Karoṣṭhī characters; Falk (1993, p. 238) notes that the first eight characters of the arapacana-sequence are full or at least graphic correspondences with Aramaic characters, suggesting that a semi-literate creator of Karoṣṭhī started with those characters whose values he could remember, and only then began to invent values for familiar forms or entirely new character shapes.

3.10. Complete Chaos? The Carian Alphabet

In the previous section, reference was made to similarities between the issues involved in the origin question of the Indic and the Runic scripts. The Carian alphabet is another excellent case for comparison with the Runic script in terms of the problems with their respective derivation: it has the same structure as its most obvious model, being one of a number of Greek-based alphabets in Asia Minor, and thus can hardly be classified as a result of unsophisticated grammatogeny of the Cherokee type. Yet it deviates from that model in detail in a way which has so far proved inexplicable. It features a few letters which resemble letters of the obvious model and have the appropriate sound value, but also letters which resemble letters of the model but have seemingly random sound values, as well as letters which can be derived from letters of the model at something of a stretch, and some letters which really do not look like anything that might legitimately be compared with the model.[37] Accordingly, the study of the Carian alphabet knows its own version of the runemaster-theory, aptly named the "chaos hypothesis" (also "μεταχαρακτηρισμός"), according to which there is simply no logical relationship between the letters of the Greek and the Carian alphabet—the concept of alphabetic writing and a handful of letters were taken from Greek, but some of the latter were given different sound values at random, and supplemented by newly invented letters. Voogt (2012, p. 5) books the Carian alphabet as representative of his transfer type L4 (borrowed characters, different values) on the assumption that a Carian creator purposefully rearranged

37. The profound difference between the two fields lies in the fact that the Runic script boasts a continuous tradition which connects the last phase of its use with the earliest scholarly treatments, so that it never had to be deciphered. A glance at the history of the decipherment of the Carian inscriptions (Eichner, 1994), with an older tradition of scholarship adhering to the principle that the sound values of graphically similar letters must always correspond to the Greek values, may give an impression of the state the field of runology might be in today if it had started out assuming that ᛖ was *m* and ᛈ was *p*.

the grapheme-phoneme correspondences to make his script unlike the Greek model—though why he allowed alpha, omega, upsilon and san to keep their Greek values, and why it should not have occurred to him to simply change all the letter forms, remains open to question. Adiego (2007, p. 230f) doubts the chaos hypothesis. His own theory (in detail Adiego, 1998) suggests that the Carian letters do in fact go back to those Greek letters which their sound values indicate and that the graphic deviations are due to extensive formal changes introduced between the alphabet's emergence and its attestation. Notably, the Carian alphabet, like the early Greek alphabet, is attested in a number of local variants which may or may not go back to one single proto-alphabet.

3.11. A Script of One's Own

Ethno-nationalist motives are quite frequently referred to in the context of script creation. The possibility of a conscious effort to set oneself apart from the model was, e.g., suggested for Ogam—the drastic graphic and systematic deviance from traditional alphabets has been explained as "a rebuff to Rome, a deliberate expression of anti-Roman sentiment" (McManus, 1991, p. 14) or as the features of a cipher specifically created to be illegible to people with literacy in Latin (see ibid. with literature). The importance of creating original alphabets for the political and cultural identity of Caucasian speaker communities is stressed by Barkhudaryan (2011), Drost-Abgarjan (2011), Kananchev (2011, p. 63) and Seibt (2011, p. 85). The latter suggests that the Armenian letters were graphically changed so as not to look too Greek to avoid conflict with Persia.[38] But also the adoption of a script (presumably) without intervention of a creator may have motives related to ingroup writing: the first Celtiberian documents, written in the structurally ill-suited Iberian script, date to ca. the middle of the 2nd century BC, i.e., the time of the Numantinian War against Rome—Stifter 2019, p. 109 attributes this delayed adoption of the Iberian script in favour of the Latin alphabet, which must have been known to the Iberian Celts, to "a deliberate political decision fraught with deep cultural symbolism". In the same vein, Justeson and Stephens (1993, p. 38) point to the potential role played by "script as an ethnic attribute" in the context of their theory of misunderstood models—the wish to establish the new script as a mark of ethnic

38. Cf. also Granberg (2010), who argues that, of the alphabets which emerged in the 1st millennium AD in the context of Christianisation, those which wrote languages which had not been written before deviate from to the Greek model in both letter forms and order, while those which replaced and had to compete with previous traditions (Coptic with Demotic, Gothic with Runic, Cyrillic with Glagolitic) emulated the prestigious Greek script.

identity may be a factor in perpetuating the results of misinterpretations in cases where contact with the model writing culture is maintained. As stressed by Coulmas (1989), writing "creates social coherence" (p. 8), it

> indicat[es ...] group loyalties and identities. [...] Language attitudes such as the desire to have an orthography which makes the language in question graphically similar to another or, conversely, makes the language dissimilar to another, may be irrational but they are social facts which often strongly influence the success of a proposed system (p. 226f).

In a Runic context, Rix (1992, p. 141) calls the notion that an attempt to set oneself apart from the culture which provides the model could be the cause for the otherness of the fuþark "modern gedacht" ('a modern thought'), but the possibility can certainly not be excluded for the runes.

This opens the question of whether illiterate communities only adopt scripts if they require the technology to serve a specific purpose. This is claimed, for example, by Spurkland (2005, p. 3), who assumes "a compelling need for a means of written communication due to an expanding economy and growing administrative structure", and Rausing (1992, p. 202), according to whom the fuþark was "devised by practical men to meet a practical need".[39] The opposite position is held, for example, by Williams (1997, p. 181), who observes that "[g]iven the contact with Roman culture, it would be a strange thing indeed if some Germanic individual had *not* been impressed by the Roman art of writing and tried to imitate it". Like Seebold (1986, p. 534) and Odenstedt (1990, pp. 171, 173), Williams argues that, despite the existence of the fuþark, the Germani were "functionally illiterate" (p. 187), relying on oral transmission well into the Middle Ages and using writing for marginal purposes.[40]

The examples of communities which had writing bestowed upon them, nolens volens, are legion. This prominently includes the numerous examples of scripts created by Christian missionaries, whose primary objective was not to raise literacy levels, but to get natives to read the holy texts. Yet cases in which a script, once known, was not used in some way, must be rare, if they exist at all. Like any technology, and probably more than most, writing is a tool which has an immediate appeal, and can and will be used for its own sake, even if it does not serve any particular purpose. The knowledge of writing may also be tied to a cultural asset of high(er) appeal, such as a cult, and enter through the back door. Furthermore, there is the question of who, exactly, "needs" or "is ready for" script—a society as a whole? A specific group of professionals? Any one individual, reacting to a perceived latent demand—or

39. See also, e.g., Düwel (2003, p. 583); Stoklund (2003, p. 173); Heizmann (2010, p. 16).

40. See also Bæksted (1952, pp. 134–138, 328); further Williams (2004, pp. 268–273); Fairfax (2014, p. 187f).

to his own fancies? Hankul was a rather enlightened pet idea of King Seycong, aimed at educating the people—while he considered it useful, it was widely rejected by the members of court, who were classically trained in writing hanca and had no need for the new "proletarian" script (Taylor and Taylor, 1995, p. 212). Förster (2011, p. 35) argues that the Greek-based Coptic alphabet, which superseded the dying Demotic script in the first half of the 1st millennium AD, was vital for keeping the vernacular language alive in the face of Hellenisation, even if it failed to represent that language as well as the obsolete Demotic had, but it may be doubted whether this was the express purpose of its creators/users. The divided Armenians of the 4th century, on the other hand, are said to have been in need of a script for political reasons (Barkhudaryan, 2011, p. 17); the invention of that script, supported by the clerical leaders, immediately triggered the development of national historiography. The Cherokee also took to Sequoyah's syllabary with considerable enthusiasm, with even the shamans putting their wisdom into writing (which they notoriously refused to do in any of the adapted European alphabets), but inhowfar they "needed" a script is open to debate. Are we to assume that the Etruscans would have rejected the Phoenician script, had it arrived at their shores two hundred years before the Greek one, because they could have found no purpose for it?

These considerations are tied to the presumptive creator's provenance and his native language. When assuming script diffusion, it is clear that the people who carry the process are speakers of the target language. When there is talk of a script creator, I believe, scholars also generally think of a member of the previously illiterate culture[41]—runologists, as shown above, definitely do (cf. Rix, 1992, p. 412). This is by no means obvious. Theoretically, the creator (or creators) could have been a member of the new writing culture (Germanic), a member of the model writing culture (Roman/Greek/…) or the member of an intermediary writing culture (Celtic/…)—examples can be found for most scenarios: grammatogeny by speakers of the source language who have attained an understanding of the target language (e.g., the Lisu script[42]), by speakers of the target language who are literate in the source lan-

41. Not so Prosdocimi (e.g., 2002, p. 28), who makes the point that, in regard to script adaptation, the teachers of writing whom he calls "maestri" always belong with the source language's culture in that, even if they should be members of the target language's culture, they can be literate only in the source language. While this is certainly true, Prosdocimi goes on to claim that these bilingual maestri must consequently have the same perspective on the adaptation as their source-language-speaking colleagues, and that therefore a new script is never created to properly fit the target language, but must reflect the necessarily conservative point of view of the source language's maestri.

42. The English missionary James O. Fraser created a highly systematic abugida-like script for the Tibeto-Burman language around 1915 (Daniels, 1996a, p. 581).

guage (e.g., the Armenian alphabet) and by speakers of the target language with no literacy in the source language (e.g., Cherokee). While it is true that the first option draws its many examples from modern grammatogeny performed by missionaries, it must be observed that examples for the second option can be difficult to classify. There is a smooth transition from native speakers of the target language with competence in the source language via functionally bilingual speakers of both languages to thoroughly acculturated persons with only a remote connection to their native culture and language, and in pre-modern times the distinction is often hard to make—e.g., the ethnicity and Slavic competence of Constantine (St. Cyril) has long been a point of contention (see Ševčenko, 1971, p. 341f for an overview).

4. Concluding Remarks

To sum up: very nearly every imaginable process of script transfer is attested or at least being discussed. Scripts are devised for a specific purpose by ingenious and educated men, on a whim by ingenious and uneducated men, by natives and by foreigners, by individuals, co-ordinated collaborators and unco-ordinated groups of people. They evolve gradually out of systems when these are applied to a new language and are adapted secondarily, or not. They define a cultural entity, or are only used playfully until they are abandoned or superseded. They are created by kings, clerics or innkeepers to write literature, lists, or nothing in particular. They are learned, imitated or forced on people; they are purposefully made to emulate or to set apart, they are faithful to the model or accidentally revolutionary.

As said in section 2.2, there is no doubt that in the case of the fuþark, there are features which indicate that its formation did not happen in the same way as that of most other Mediterranean alphabets. The deviating order of the row shows that the alphabet was not learned in the traditional way, through the Merkspruch. Numerous attempts to explain this idiosyncrasy use widely different approaches, from the phonetically motivated re-arrangements mentioned above via graphic considerations (e.g., Kabell, 1967, p. 114) and underlying texts (e.g., Skeat, 1890) to the semantics of the rune names. All these presuppose the regulative hand of a creator; only explanations which work with transmission errors (e.g., Williams, 1996) are reconcilable with diffusion. In the latter case, the question remains how a fairly uniform row emerged. The rearrangement is best explained as the intervention of a creator, even if his motives remain unclear.

Secondly, the graphic uniformity of the earliest inscriptions is supposed to indicate that the Runic script was invented at once, as diffusion should lead to a certain amount of inconsistency and variation in

the early phases and be reflected in documents in which graphic forms and problematic character-sound relationships vary. The homogeneity of the early finds, however, is debatable; the stance taken on the matter depends on how experienced the respective scholar is with epigraphic corpora, and with which one(s), and which standard of uniformity these data suggest to them. Furthermore, it is not even entirely clear whether the documents which are currently considered the earliest Runic inscriptions really represent the initial phase of Runic writing: the argumentum ex silentio is as precarious in runology as it is in other epigraphic fields. The potential precursors of runes such as the Meldorf fibula, if they are to be interpreted as such, do not agree with the notion of a creator, unless one were to assume a scenario similar to that of the Venetic alphabet: initial unsystematic employment of a foreign script for one's own language being cut short by an inventor taking charge.

In any case, the mere fact that all these features have also been explained differently, be it by assuming an unattested intermediary alphabet or some specific context for the transmission, shows that it is far from clear whether a sophisticated creator of the runes ever existed and can be relied on to account for any unexpected feature of the fuþark with his "imaginative approach" (Spurkland, 2005, p. 6).

It is more often than not impossible to be sure which transfer scenario we are faced with, because the impression we get depends on the transmission situation. As noted by Jeffery, uniform invented scripts are not stable and variation will develop; on the other hand, gradual diffusion can be cut short by the intervention of a culture hero or a regulating body at any point. Depending on when our attestation sets in, we may misinterpret the state of the script and the reasons behind its characteristics. We may overlook a systematic creation if the inventor's name is lost and the oldest documents already show some variation, and we may take for an original one-off creation a script that is really just a secondary regulation of a gradually evolved tradition, especially if there is a prominent name associated with it.

Finally, it may be observed that scholars who are concerned with scripts which are known to have been created by highly competent persons with a free hand, such as the Armenian alphabet, still occupy themselves with the search for the models of individual letters, the assumption that the creator invented letters and rules and introduced changes off the top of his head being considered a last resort. There are scholars who seek to account for changes even when assuming a single creator—e.g., Fairfax (2014, p. 217), who points out that assuming what he calls an "impressionistic" element in script transmission does not necessarily mean that letter derivations are unnecessary, as even the alleged creator must be expected to proceed with a certain amount of "procedural rigour". A non-Runic example, referenced by Fairfax himself, is Ebbinghaus (1979), who presents an elaborate attempt at explaining how ex-

actly Wulfila proceded when he derived his Gothic letters from the Greek alphabet. This approach is methodologically sound. Thus Krause (1970, p. 41), who deems Moltke's theory to be a non-explanation:

> Eine solche Erklärung dieses oder jenes Runenzeichens ist freilich im Grunde keine Erklärung. Man wird daher doch bestrebt sein, auch bei zunächst als unableitbar geltenden Runen irgendwelche Vorbilder oder wenigstens Anregungsmuster aufzuspüren.[43]

Similarly, Cubberley (1982, p. 291) observes that theories which explain the Glagolica as a completely original creation are "quite unchallengeable in any formal sense". If we assume that any irregularity or unexpected element in a derived script is due to the arbitrary decisions of an unknown figure lost to history, we move on methodologically dangerous ground. This does not mean that theories which include formal letter derivations according to the "naturalness"-approach could not do with a higher level of methodical rigour—McManus (1991) passes valid methodological criticism on the "juggling and reshuffling" (p. 25) of characters to make them fit with their putative models, writing that "[m]ost attempts to outline the successive stages in the development from the prototype to [in his case] Ogam amount to no more than exercises in anticipating what one knows became the alphabet in its final form" (p. 22)—"[i]t is in effect a hit and miss approach which cannot miss since it has the benefit of hindsight and its arguments tend to become circular in nature" (p. 26).

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43. 'Such an explanation of this or that Runic character is of course ultimately not an explanation. One will therefore still strive to trace some models or at least inspirations also for runes which are regarded as impossible to derive'.

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