

Considerations on the sign ʃ and the problem of its interpretation in Tifinagh inscriptions

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The history of the «hook» as a character for the phoneme /g/ is a story of success and continuity. The depiction of a throwing-stick kept its basic form from the very beginning in the scripts of Byblos and Sinai in some regions of the Near East and the Mediterranean for about 2000 years (e.g. Clarke, 1884: pl. xvii-xx; Gardiner, 1916: comparative table; Ullman, 1927: 315 & tabl. p. 314; Diringier, 1943; Albright, 1948: 14; Moore Cross, 1980: tabl. p. 16; Healey, 1990: 26 and tabl. 11; Hamilton, 2003: tabl. p. 36). It was not only used in the Phoenician, Aramaic, Canaanite and Samaritanic scripts but also in the Greek, Phrygian, Lycian, Etruscan, Iberian and Latin alphabets.¹ The Greeks called it gamma (Γ). The Etruscans – who had no g sound – used it for the k sound, and passed it on to the Romans as C. Later on, when the Romans wanted to distinguish the voiced guttural stop /G/ from the

unvoiced /C/, they added a short bar to C, producing G, and inserted it in the alphabet at the place formerly occupied by the Greek zeta (Bruce, 1948: 1; Healey, 1990: 40).

In nearly all of these alphabets the basic form was varied in almost every possible way:

ʃ ʃ ʃ ʃ >

and twisted/reflected in several directions, e.g.:

ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ
ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ
ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ ʃ

The same goes for the continuity of this sign in the Libyco-Berber script from the first occurrence (probably about 700 BC, in some inscriptions of the High Atlas, e.g. Azib n'Ikkis) throughout the time of ancient and classic inscriptions to the transitional ones. There is not the slightest doubt about its phonetic value (Fig. 1).

The real problem starts with the greatest change in the whole

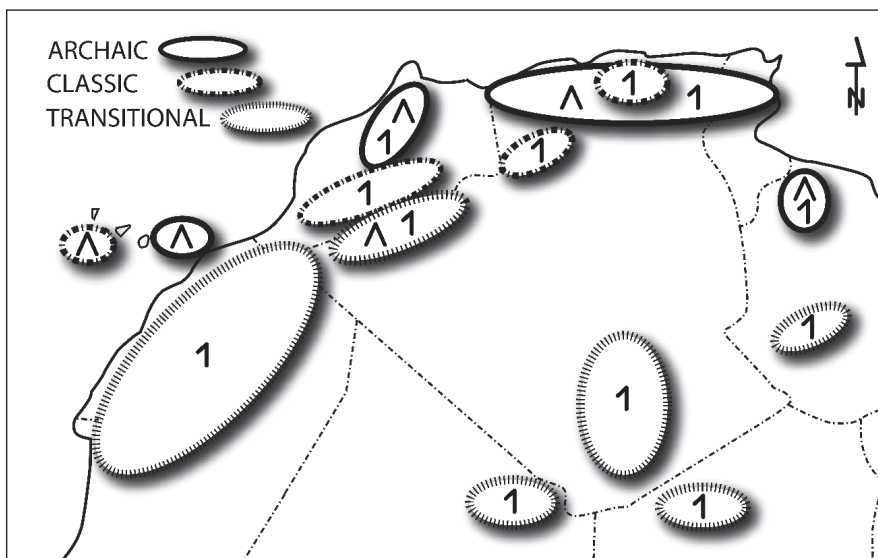
history of the scripts of the Libyco-Berber type: the change to the so-called Tifinagh script. We do not know when this important step took place, but this new type of alphabet is used till recent times in some regions of the Sahara.

Theoretical reflections

A lot of Tifinagh alphabets for the whole Tuareg territory have been published so far, but only two of them consider the essential regional differences within this territory. According to Prasse, 1972, the sign ʃ exists only at the southernmost border of the Tuareg territory: among the Azawagh (ⵛⵉⵎ) and among the Kel-Ensar/Tinbuktu (ⵉⵎⵓⵙ). Nearly the same goes for Aghali-Zakara: his table – first published in 1993 (Aghali-Zakara, 1993: 144) – takes into account only the Azawagh, but the last edition (2007: 28) adds the possible phonetic value of /z/.

These assertions are definitely wrong. In fact the sign ʃ in all its graphic variants is well documented in nearly all regions of the huge Tuareg territory: Ahaggar, Tassili, Akakus, Messak, Ayer, Adrar (Fig. 2). However, so far there has been no detailed examination of the phonetic values in these different regions.

Fig. 1. Regional distribution of alphabets and their variants of the hook sign.



¹ However, one should be aware that external likeness of two signs does not necessarily indicate a genealogical relation, nor an identical value. Other considerations have to be taken into account (Diringier, 1943: 79).

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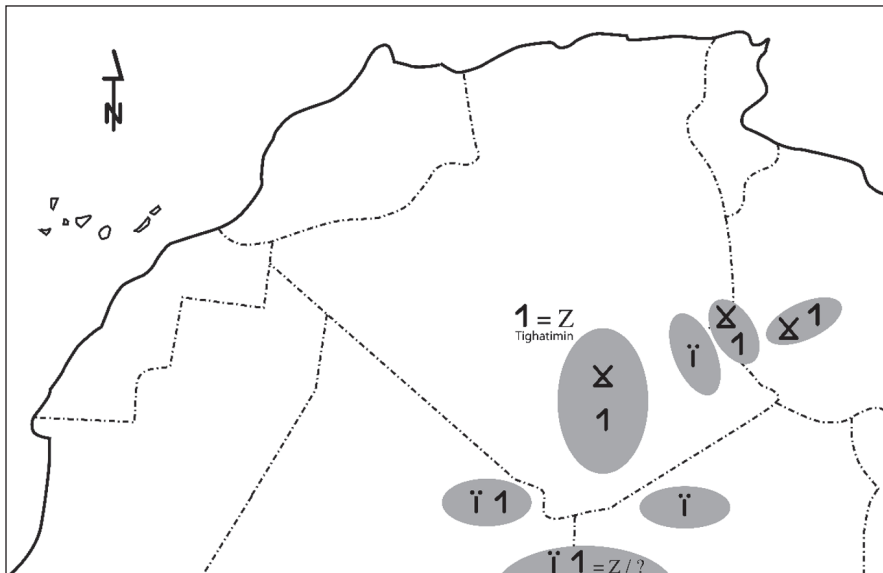


Fig. 2. Repartitions of signs 1, X and i in the Tuareg territory.

Additional statements about the phonetic value

Aghali-Zakara and Drouin (1988: 90) quote among the «variable signs» in Tifinagh script four variants (↖↗↘↙) with the phonetic value /z/. In 2001 Aghali-Zakara emphasizes that emphatic consonants normally are not represented in writing certain Tuareg dialects of the South. Nevertheless the following signs #, X, and ↖ can mean emphatic /z/ in some cases, but /z/ or /j/ as well (2001: 5). In 2002 he again confirms the sign ↖ as a multivalent one, representing /z/ or /j/ (2002: 3). The same can be found in 2007: 14, when Aghali-Zakara and Drouin as a result of the examination of 109 inscriptions of the «Sahel nigéro-malien» state that ↖ can represent /z/ or /z/.

It should be made perfectly clear: In all these publications not a single claim can be found stating that ↖ should be a representation of /g/ – this phoneme being written either as X or as i in all Tuareg regions (Fig. 3 and 4). The only exception is when Aghali-Zakara and Drouin (2007: 14) quote Beguinot (1938), who gave this sign the value of /g/ or /gʷ/ for the Fezzān, but they immediately specify that this interpretation is not confirmed by Nehlil (1909) for Ghāt in this area. And at the end of the same publication, they explain that several uncertainties in Beguinot’s work urged them not to accept the value he gave to this sign. Again they insist: according to their view, ↖ can only represent /z/ or /z/ (Aghali-Zakara & Drouin, 2007: 112).

Practical transliterations

Taking into consideration this unambiguous determining of the phonetic value of ↖ it is the more interesting to have a look at how this sign was transliterated in various publications of concrete inscriptions.

In 1937 Marcy interpreted twice a «Tifinagh inscription» (denomination by Marcy!) at the site Tighatimin in Algeria, documented and published by Reygasse (1932):

- «↖ is /g/ in ancient inscriptions» (1937a: 92)
- «the inscription is ancient, using ↖ for /g/ instead of X» (1937b: 6).

Of course, he did not explain why a Tifinagh inscription with the incipit !: «nk» (= nāk) and the sequence O: «rgh» (= areγ) should be ancient.

A long time later Aron (2004: 5) interpreted Tifinagh inscriptions (denomination by Aron!) from the Fezzān, published by Beguinot in 1938, and transliterated seven graphic variants of ↖ as /ǧ/ or /gʷ/. Then Drouin interpreted ↖↖ as «Gogga» or «Gogo», ↖||: as «ag Elu» (2006: 5) and !O: as «Agis(a) h» or «ag Isa» (2007: 6).

In 2006 Hachid commented an inscription from the famous monument of Abalessa in Ahaggar (Algeria). This short vertical inscription (below called Abalessa 1)

Fig. 3. Repartitions of signs in the Tuareg territory for values g, gh, gʷ, kh and q.

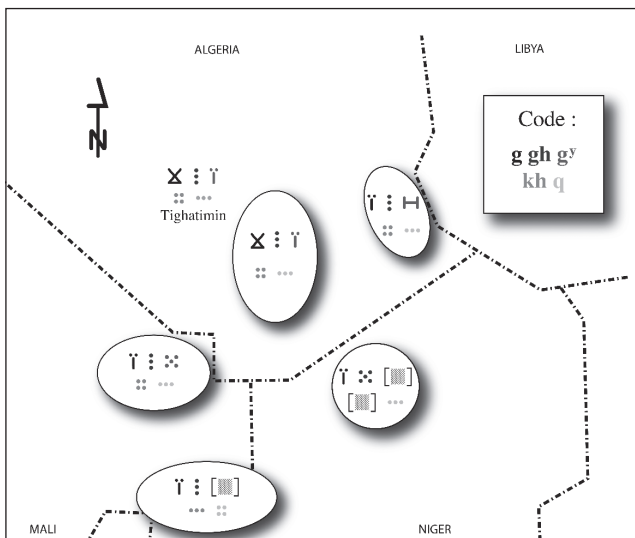
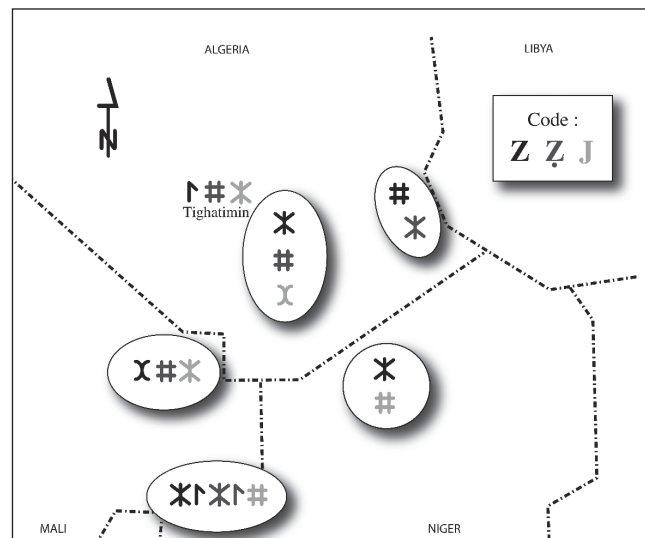


Fig. 4. Repartitions of signs in the Tuareg territory for values z, z and j.



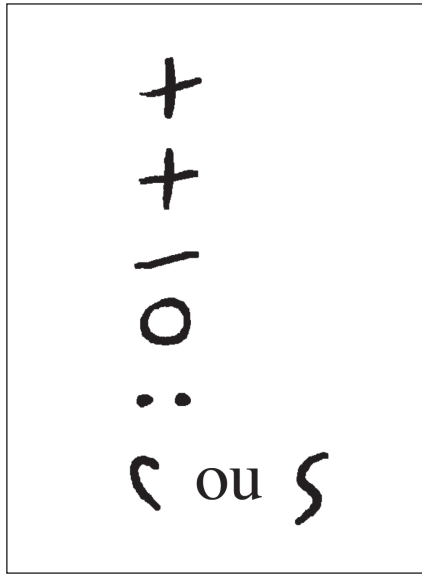


Fig. 5. Engravings and inscriptions at the bottom of a wall in the Abalessa mausoleum (after Hachid, 2006: fig 2).

Fig. 6. Drawing of the inscription of Fig. 5, according to Hachid (CAD JLLQ after Hachid, 2006: fig. 4).

Fig. 7. Engraving of a camel and typical Tifnagh inscriptions on slabs included in the upper part of a wall of the Abalessa mausoleum (CAD JLLQ after Lhote, 1949: pl. III, fig. 1).

Fig. 8. Typical Tifnagh inscriptions on a slab included in the upper part of a wall of the Abalessa mausoleum (CAD JLLQ after Lhote, 1949: pl. III, fig. 2).

near the depiction of a horse, with a second line hardly visible (Fig. 5) had been published by Camps thirty years before (Camps, 1976: fig. 8). Hachid's drawing of the inscription (Fig. 6) hesitates between two interpretations of the first sign from below, but the most probable is that this sign must be a \uparrow . Hachid emphasizes the fact that the stone bearing these engravings is included in the bottom layer of the wall and that this position practically precludes any possibility of engraving it after the construction of this wall (Hachid, 2006: 96, and see her fig. 2). The funerary use of the Abalessa monument is well dated from the 4th-5th AD by the funeral furniture (Camps, 1974; Trost, 1986; Grébenard, 1994; Hachid, 2006). A piece of wood from the funerary

bed was radiocarbon dated from 1480 ± 130 years BP, that is to say between 425 AD and 662 AD at one sigma, or between 254 AD and 782 AD at two sigmas (Stuiver & Reimer, 1986-2005). Archaeologists now agree to say that the monument itself must have been erected some time before its transformation as a mausoleum, most probably during the 3rd or 4th century AD.

In 2008 Le Quellec published an old photograph taken by the French explorer Félix Dubois in September 1907, and showing another inscription (below called Abalessa 2: two lines near a horse led by a possible anthropomorphic figure). This inscription is pecked on a stone included in a wall of the same monument, also at its bottom layer (Fig. 11). Most significant is the fact

that the horse is presently upside down, which means that the stone was engraved before being used by the builders of the primitive *tiyremt*, for whom the engravings were obviously of no importance. This gives us a *terminus ante quem* for the inscription, which must predate the erection of the wall. If these observations are correct, then it follows that these two lines date from the 3rd or 4th century AD at the latest, without the slightest doubt (Le Quellec, 2008: 3).

It must be stressed that the position of the inscriptions reported by Camps/Hachid and Le Quellec differs from the ones previously mentioned by other authors for the same site. Lhote mentions 36 inscriptions from Abalessa. He published two photographs of *in situ*

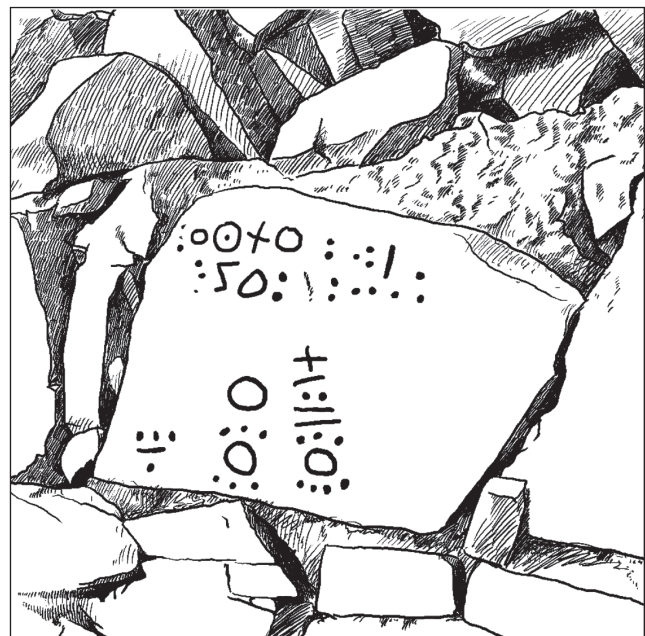
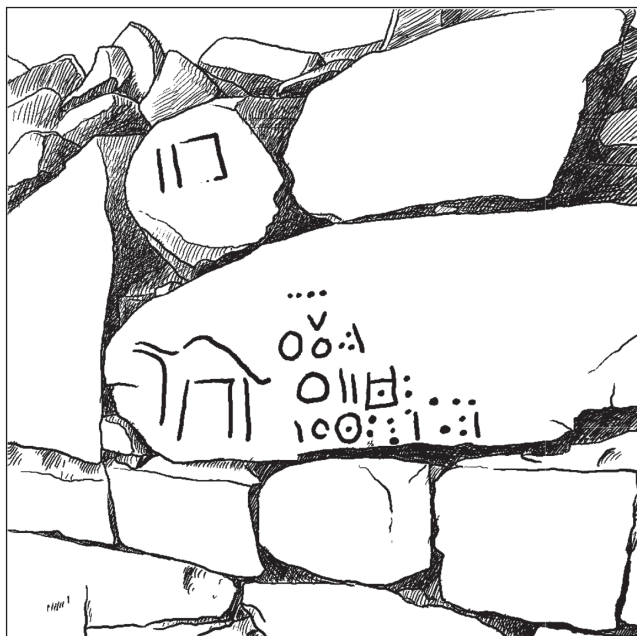




Fig. 9. The engraved stone noticed by Reygasse in the Abalessa monument. (Photo courtesy Malika Hachid).

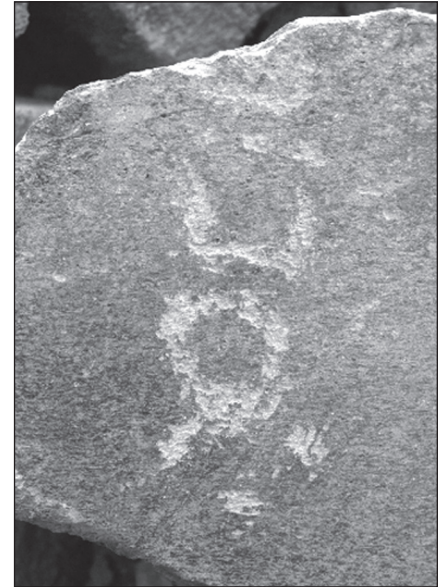


Fig. 10. Detail of the same stone, showing that the inscription is broken at its upper part. (Photo courtesy Malika Hachid).

stone slabs from the mausoleum with several engraved inscriptions and a small camel (Lhote, 1949, and our Fig. 7 and 8). He mentions at least 28 inscriptions from the monument itself (Lhote, 1949: 35):

— five inside the mausoleum: No. 538-540, 562 (⋯⊞⊞) and 563 (⋯⊚⊞)²;

— fifteen in the external wall of the ENE side (No. 541-555);

— five in the external wall of the S side (No. 556-560);

— three or four on an isolated stone in the rubble: No. 568 (⋯⊞⊞⊞), 569 (⊞⋯⊞⊞⊞), 571-a (⊞⋯⊞⊞⊞), 571-b (⋯⊞⊞⊞)³;

No stroke-sign appears in Lhote's list, and two of these inscriptions begin with the usual Tifnagh incipit ⊞⋯ *nāk*. Lhote himself summarizes the situation in these terms (our translation): «The inscriptions placed in the interior [of the mausoleum] have almost no patina, but they are undoubtedly ancient, that is to say, older than the excavation of the kasba. The patina of the external

engravings is much more intense» (Lhote, 1949: 35). Five years later, he added this commentary: «All the engravings I saw on the stones of the fort [of Abalessa] had a relatively clear patina and, according to my mind, they cannot have been delineated after the construction of the walls. They look like the result of the amusement of aimless caravaneers looking for a shelter in this place to rest a few hours; they are at some distance from the ground, that is to say within arm's reach, a fact which tends to confirm this point of view» (Lhote, 1954: 77, n. 26, our translation). Indeed, the photographs published by this author show two engraved slabs included in the *upper part* of the walls (Fig. 7, 8).

On the contrary, the inscriptions published by Camps/Hachid and Le Quellec are *at the very bottom of the walls*. It would have been extremely difficult and strange to peck them in that situation. Such an assumption is not only highly improbable, it is almost impossible. Moreover, the one discovered by Dubois must have been of a rather dark patina, as he had to chalk it in order to take a photograph (Fig. 11).

All these observations confirm Reygasse's statement: «I made copies of mutilated Tifnagh inscriptions in the walls of Tin Hinan's fort, used for building the fort itself, and therefore predat-

ing this monument without any doubt» (Reygasse, 1940). Unfortunately, Reygasse never published these copies, but one of his photographs clearly shows a stone bearing a broken inscription and reused to build one of the walls of room No. 7. He concludes that two inscriptions are «older than the 4th century» and that this is «the only example of a discovery of archaic tifnagh in the Hoggar which can be dated with a relative certainty» (Reygasse, 1950: 103, and fig. 155). Such a statement should not be neglected.

Thanks to Malika Hachid, we can present here a recent photograph of the inscription mentioned by Reygasse (Fig. 9). It appears clearly that this inscription is broken in its upper part (Fig. 10), and the fact that it was pecked before the stone being used in the construction of the wall is confirmed by our colleague, who wrote to us: «One cannot question the fact that the inscribed stone is not in its original place because, as all the neighbouring stones, it is strongly maintained by the same clayish concrete. Nobody has paid attention to this detail, except Mohand Hamoudi from the Office of the National Tassili Park: this concrete has a highly specific nature and a very particular colour, and it cannot be found on the spot. According to local Tuaregs, it can be found in only two places in the

² NB. Not all transliterations are given here because twelve of them are mentioned by Lhote in his text, but the corresponding figures (Lhote, 1949: fig. 26, 30) are lacking in the original publication.

³ NB. Lhote mentions one inscription «No. 571» in his text, but in fact he gives two figures with this number.



Fig. 11. Engraved stone with a horse and an inscription as discovered by Félix Dubois in 1907 (after Le Quellec, 2008: fig. 4).

Fig. 12. Drawing of the engraved stone of Fig. 11. Black: certain; dark grey: questionable; light grey: dubious (CAD JLLQ).

surroundings of Abalessa. When observed with a magnifying glass, this concrete shows small plant debris which may have helped consolidating the clay » (Hachid, *in litteris*, January 28th, 2009). Now there is still a difficulty: this inscription must predate the erection of the monument, and yet the characters are «recent» tifinaghs with dot-signs at the beginning. We can only hope that the whole story will be elucidated one day by ¹⁴C dates, as carbon is probably still present in the plant debris.

In 2008 Drouin added an «A propos» to this topic in arguing against Le Quellec's identification as a «Libyc» or «saharien ancien»*-inscription («formule vague de Marcy reprise par Pichler et d'autres» Drouin, 2008: 5)⁴. Her argumentation is threefold:

1. First she argues that «it is erroneous to affirm that the signs with parallel strokes cannot be Tifinagh» — although such a thing was not affirmed in the paper she is commenting, where all that was asserted is that the presence of

such signs simply increases the «chances» of not being Tifinagh (Le Quellec, 2008: 4). But here we must contradict her: typical Tifinagh rock inscriptions do not include signs with 2, 3 or 4 vertical lines. These are typical indications of the Transitional script, not to be confused with the so-called «Ancient Saharan» (Pichler, 2007a: 74-83). Counter to an affirmation by Aghali-Zakara (2004), the fact that dots are sometimes replaced by strokes in some very recent hand-written texts is not relevant here: the facsimiles presented by this author show that this is imputable to the fact that these texts were written on paper with a modern ball pen. In such conditions, it is easier to draw strokes than dots, and strokes are more visible. When rock inscriptions are concerned, the situation is completely different, as the signs are generally produced by percussion.

2. Secondly, she reads one line of the inscription (Fig. 12) as *ahurey Mata* («I am following Mata»), which appears to be correct if one neglects the fact that the sign 5 /t/ may not be the last one. The last sign of this line, very difficult to make out, seems to be a letter made of three parallel strokes not chalked by Dubois. And between this sign and the fifth one, Dubois' chalking is questionable. Then Drouin reads the other line as *Lita*

ag Duna («Lita, Duna's son») — an acceptable reading.

3. Finally, she considers the inscription published by Camps/Hachid to be *Tit (ta) n araway* («Tit (that of) the mixing/concoction»). This is only possible because she chooses to interpret the last sign of this inscription as a Σ «whose third segment is barely initiated» (Drouin, 2008: 7). But, as no argumentation supports it, this choice is arbitrary. As a matter of fact, the so-called «third segment» of this supposed Σ is so tiny that this sign is closer to a \uparrow than to anything else.

She concludes believing these three inscriptions to «share the same graphic type and messages». But she cannot explain the hiatus between these two conflicting assumptions: 1. inscriptions in the Abalessa mausoleum are Tifinagh (according to Drouin), and 2. the monument dates back to the 3rd or 4th century AD (according to archaeologists).

Our proposal is the following objective reflection of two possibilities of interpretation:

1. Abalessa 2 is a Tifinagh inscription

Argument pro (according to Drouin): it can be read by using the Tuareg language.

Argument contra: contradicting her precedent claims and Aghali-Zakara's, Drouin transliterated \uparrow as /g/ not as /z/ or /z/ as they have

⁴ We would like to stress that we never have re-established this term. We only used it in quotation marks when quoting Foucauld, Marcy, Mauny *et al.* (Pichler, 2007a: 80; Le Quellec, 2008: 4). In contrast, Pichler proposed the exactly defined term «Transitional script».

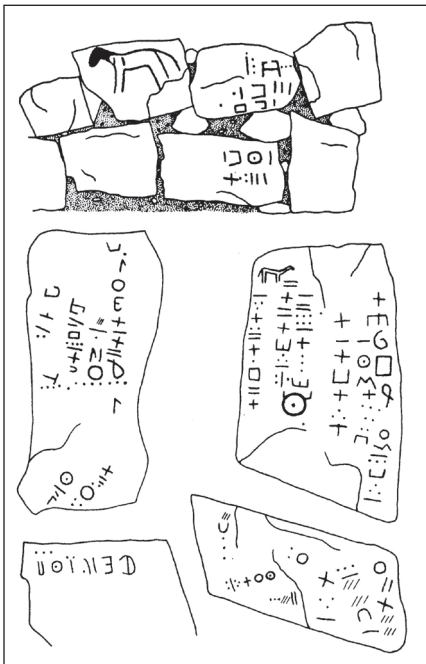


Fig. 13. Typical inscriptions on stone slabs included in tombs 3, 4 and 8 near the Abalessa mausoleum (after Trost, 1986: 89). Photographs of inscriptions 13-1 and 13-2 are given by Camps (1976: fig. 10). Camps' tracing of 13-3 (Camps, 1976: fig. 11) seems to be less accurate than Trost's. The comparison between Hachid's photograph (Fig. 5) and Camps' one (1976: fig. 8) shows that the original place of stone 13-2 was four levels above the engraved stones in Fig. 5, but the stone was removed at an unknown date.

always done before. A transliteration more in accordance with their precedent publications would have resulted in LTZDN – a possible personal name sounding somewhat similar to «Eltuzadan».

This interpretation is in contradiction with the archeological context.

2. Abalessa 2 is an inscription older than Tifnagh

Arguments pro:

- there are no typical Tifnagh signs (such as H, X, G, etc.);
- there is no incipit such as «(awa) nāk»;
- there are signs with vertical lines (III, III);
- the incipit ≡O≡ is very frequent in the transitional script (Pichler, 2007a: 82, 85-88).

This interpretation perfectly fits into the archaeological context.

In this situation we should remember that there is a third group of inscriptions in the immediate surroundings of the monument. There are 13 tombs of the «Choucha» type and one little tumulus, all of them certainly younger than the monument itself. Tombs 3, 4 and 8 contain stone slabs with inscriptions (Trost, 1986: 89) which can easily be identified as typical Tifnagh, e.g. «nktmtnt» = «nāk Tamu (f) tennet» (Fig. 13-3). Lhote (1949) mentions eight more incipitions:

- four on an isolated stone in an open tumulus S of the mausoleum: No. 564 (I: +J+ :), 565 (IO: Jp: ...), 566 (:I: 3O...), 567 (I: : XE);
- four on an erected stele against a tumulus: 570 (I: C+); 572 (·OllC·), 573 (I: F1), 574 (I: +:).

Four of these inscriptions begin with the usual Tifnagh incipit I: nāk and one with the incipit IO: shurey, written with I and O, not with ≡ and ≡ as in Dubois' document (Fig. 11-12).

Therefore the only way to be consistent with the archaeological context is to conclude that at least some inscriptions on the walls of the Abalessa mausoleum are older

than Tifnagh, and that all the inscriptions on the stones of the surrounding more recent monuments are Tifnagh.

Another inscription with the sign ◀

In 2008 Lachaud published an inscription from Tazega Mellet (Tassili-n-Azjer) «which seems to present old characteristics» (2008: 197) (Fig. 15). To say it in advance: the reading proposed by the local guide Cheikh as «awa nāk Medis» (that's me, Medis) is totally wrong – this is not, by the way, the first experience of this kind. A second statement in advance: sign 2 of his fig. 29 is wrong, but this is only a typographical error (⊖ instead of ∴).

Galand and Aghali-Zakara were invited to comment this inscription in form of an angle: one horizontal and one vertical line. Galand's comment is short, precise and needs not to be discussed, in contrast to Aghali-Zakara's comment, which is full of disputable assertions (Fig. 17).

1. Sign 5: for Galand / g / or / z /, for Aghali-Zakara / d / or / r /

It is legitimate, up to a certain point, to speculate about dubious or hardly visible signs. However, it is not appropriate to say about a perfectly visible sign – such as the sign 5 – that it could be a «mediocre» D or a R of which the lower part would have been less marked. This must be refused, even if the interpretation as D – resulting in the personal name DANDA = «cheval tacheté de blanc» (Prasse, 2003: 107) – perfectly fits into the following reading of signs 13-14 as ays = «horse» (which is wrong too).

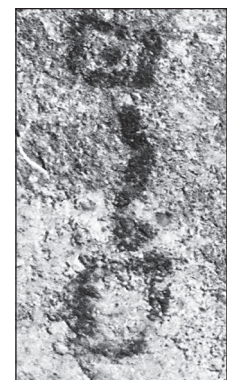
Fig. 14. Warrior and inscription painted at Tazega Mellet (after Lachaud, 2008: fig. 26).



Fig. 15. Detail of the painting in Fig. 14 (photo JLLQ).



Fig. 16. Last signs of the vertical line (photo JLLQ).



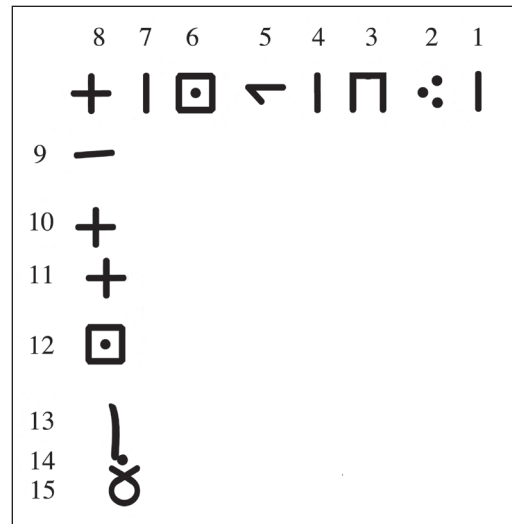
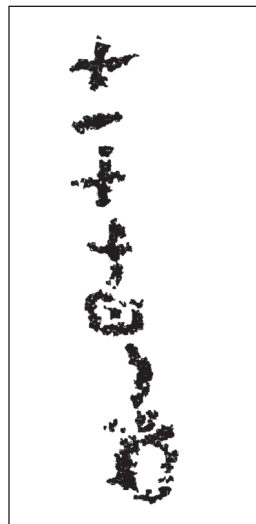
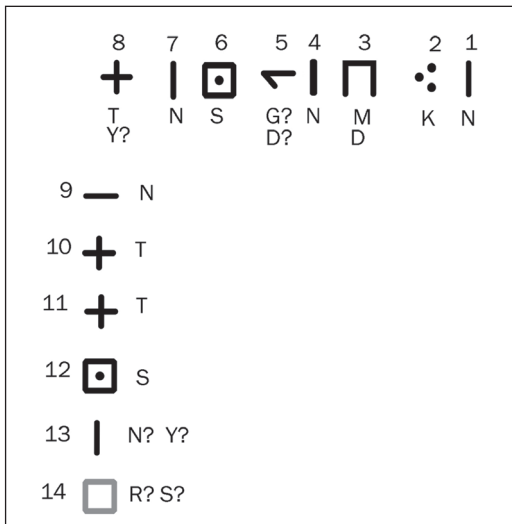


Fig. 17. Reading and transliteration of the inscription of Fig. 14 according to Lachaud, 2008, relying on Galand's and Aghali-Zakara's comments (after Lachaud, 2008, fig. 29).

Fig. 18. Automatic vectorization of Lachaud's photograph with Corel Draw software.

Fig. 19. Our proposal for the reading of the same inscription.

2. The same goes for sign 8: this is indisputably the never changing sign + for /t/ and not for /y/!

3. The most questionable part of the inscription concerns the signs 13-14.

One remark in advance: these two signs never represent YS as claimed by Aghali-Zakara. One could speculate what the creator of the inscription wanted to write, but what he did write is certainly not YS. Sign 13 looks like a slightly bended vertical line. This theoretically could be a twisted N or perhaps an indistinct D. Sign 14 definitely is no S. It is either a ◻ for /r/ or — considering the two little knobs on the upper side of the circle (Fig. 16, 18) — most probably a ⚡ for /f/ in ancient, for /g/ in modern alphabets. Using the possibilities of the DIP-program D-Strech we would prefer to distinguish three signs (Fig. 19): 13 = N or D, 14 = dot, 15 = F or G (←•→, direction of reading →)

To be able to decide the phonetic value of some characters would presuppose that we know about the age of the inscription. Lachaud cites the incipit !: «nk»

and the sign ← for /g/ as possible indications for archaism. In fact «nk» is not archaic: it is typical for recent Tifnagh inscriptions. Ancient Libyco-Berber inscriptions never include the sign ⋮ for /k/, not to speak of the incipit (w)nk.

Besides: were it an old inscription, we should have to transliterate ◻ = b and, of course, ← = g, ⚡ = p^f.

Result: nk dngbn tnt tbdā[p^f]

Anyway, it rather looks like a Tifnagh inscription. In this case we should transliterate ◻ = s, ← = z, ⚡ = g.

Result: nk dnzsn tnt tsdag

However, no way leads to Aghali-Zakara's interpretation as: nk dndsn yn tsys = nāk Danda essāna iyan itātez ayis «Moi Danda, j[en] connais un qui a l'habitude d'endormir le/un cheval.»

Conclusion

It is enormously difficult to transliterate single lines or little groups of inscriptions – not to speak of translating them. In these cases it is not possible to examine complete alphabets or the statistical frequency of signs/phonemes. What we need first is exact terms and definitions. One of us (WP) tried to establish a new basis in the book «Origin and development of the Libyco-Berber script» (Pichler, 2007a)⁵ and we are waiting for corrections and/or additions. All colleagues are invited to have a critical look at it. There can be no meaningful communication if one

part ignores the other. The second step is to collect and publish corpuses of inscriptions covering defined regions as completely as possible.⁶ The third step will have to be the establishment of exact alphabets for defined regions with the help of statistical inquiries based on some thousand signs.⁷

Only such a preliminary work will allow us to answer a lot of open questions concerning the regional distribution and the phonetic value of signs such as ⚡.

In the meantime we should be very cautious: if no unambiguous assignments are possible, a necessary requirement should be to offer two (or more) possibilities of transliteration, specifying if possible which version seems to be more plausible and why it does so.

Aknowledgements

Thanks to Franz Trost and Malika Hachid for allowing us to use their documents (Fig. 5, 9, 10, 13).

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⁵ See also "concordance of terms" in the LBI database (<http://www.lbi-project.org>).

⁶ The only collections published so far are: Trost, 1981: Central Ahaggar; Pichler *et al.*, 2007b: Canary Islands, Morocco (LBI database: <http://www.lbi-project.org>).

⁷ A first attempt was presented by Pichler (1996: 61) for Fuerteventura (Canary Islands).

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