The Shapira Strips in Light of Paleography: Six Impossible Things Before Breakfast

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Résumé. *Cet article revient sur les aspects paléographiques des « Shapira scrolls », ou plus précisément « Shapira strips », qui avaient été déclarés faux en 1883 par Charles Clermont-Ganneau, Christian Ginsburg et d'autres chercheurs. Dans des publications récentes, Idan Dershowitz minimise la pertinence de la paléographie pour discuter de l'authenticité de ces documents. On montre ici que l'ensemble des témoignages disponibles, notamment les tableaux paléographiques faits à l'époque, se recoupe suffisamment pour établir que l'écriture des « Shapira strips » est problématique, qu'elle ne correspond à aucune époque de l'histoire du paléo-hébreu, et que ses anomalies étaient déjà présentes sur certains des Moabitica, ces faux grossiers vendus par Shapira.*

Incredible things happen in life. Forgotten records of the great John Coltrane resurfaced, decades after his death.[1] An earlier version of Marcel Proust's *À la recherche du temps perdu* was found in a safe a couple of years ago and published this year.[2] In biblical studies, the Dead Sea Scrolls constitute the paramount example of an unexpected, astounding discovery. Now, in an article[3] and a book,[4]

[1] *Both Directions at Once: The Lost Album*, recorded in 1963 and released by Impulse! in 2018.

[2] M. Proust, *Les soixante-quinze feuillets* (Paris: Gallimard, 2021). For a review in English, see https://www.the-tls.co.uk/articles/les-soixante-quinze-feuillets-marcel-proust-review-marion-schmid/.

[3] I. Dershowitz, "The Valediction of Moses: New Evidence on the Shapira Deuteronomy Fragments," *ZAW* 133 (2021): 1-22.

[4] I. Dershowitz, *The Valediction of Moses: A Proto-Biblical Book* (FAT 145; Tübingen: Mohr Siebeck, 2021). Another book published two weeks later, and written independently, calls for a re-examination of the case: R. K. Nichols, *The Moses Scroll* (Saint Francisville: Horeb Press, 2021). It contains a fascinating account of the dramatic story that unfolded in 1883.

Idan Dershowitz defends the authenticity of the famous, or infamous, Shapira strips, unprovenanced documents which surfaced in the late 19th century but were then regarded as forgeries, and are now lost. If he is right, then we are talking of nothing less than two copies of a proto-Deuteronomy which Dershowitz calls "the Valediction of Moses," or "V." The scholarly community should be grateful to Dershowitz for rediscovering papers belonging to Shapira, for producing a critical edition of V and for highlighting a number of textual and linguistic features of this text (with the help of another scholar, Na'ama Pat-El, for this latter aspect). That said, epigraphers are used to examining sensational documents that prove to be more or less ingenious fakes. Reading about the long history of the innumerable forged texts that have duped scholars throughout centuries,[5] including in recent years,[6] is a sobering exercise. Shapira himself sold hundreds of forged inscriptions, notably the Moabitica.[7] The least that can be said is that caution should be exercised.

At Dershowitz's instigation, a conference about the Shapira strips was organized at Harvard Law School in 2019. Then, in the wake of his publications, the text of Shapira strips was again subjected to scientific scrutiny, in online[8] and printed publications, as well as during a webinar organized by Daniel Stoekl Ben Ezra on June 10, 2021.[9] Important, in fact damning, linguistic problems in the text V were pointed out by B. Suchard,[10] R. Hendel[11] and

[5] A. Grafton, *Forgers and Critics: Creativity and Duplicity in Western Scholarship* (Princeton: Princeton University Press, 2019[2]).

[6] See e.g. A. Sabar, *Veritas: A Harvard Professor, A Con-Man, and the Gospel of Jesus's Wife* (New York: Doubleday, 2020).

[7] M. Heide, "The Moabitica and their Aftermath," in *New Inscriptions and Seals Relating to the Biblical World*, edited by Meir Lubetski (Atlanta: SBL, 2012), 193-241.

[8] Including a previous version of this article, which I uploaded to academia.edu on May 13, 2021.

[9] This webinar was recorded and can be watched at https://www.youtube.com/watch?v=brrlKidjfEI.

[10] B. D. Suchard, "A valediction to Moses W. Shapira's Deuteronomy document," forthcoming in *BiOr.*

[11] R. Hendel, "Notes on the Orthography of the Shapira Manuscripts: The Forger's Marks," *ZAW* 133 (2021): 225-30.

R. Holmstedt.[12] (Dershowitz himself had noted in his book that its language does not correspond to a previously attested dialect.[13]) Stoekl Ben Ezra demonstrated that there is no parallel in the Dead Sea Scrolls and more generally in Hebrew manuscripts to the creases that appear on the strips.[14] The exegetical arguments put forward by Dershowitz to the effect that V was a proto-Deuteronomy were convincingly refuted by Benjamin Suchard,[15] Jeffrey Stackert,[16] and Konrad Schmid.[17]

What about the paleography? Although the strips are lost and the available photographs are useless because the leather was too dark, we do have various sources of evidence about the script, since drawings and paleographical tables were made by several people in 1883. André Lemaire[18] and Christopher Rollston[19] reached the same conclusion: the script is indicative of forgeries. The reliability and significance of this evidence was, however, minimized by Dershowitz in his publications, and by Benjamin Sass during the webinar,[20] with the exception of respectively two and one letters drawn by an artist. The present article examines afresh the various pieces of evidence about the paleography of the strips, shows that these elements converge in a way that establishes their own reliability, so that we have a reasonably good idea of the script of the documents. It also demonstrates that this script is problematic in three respects (a set of anomalous letterforms, the wrong stance for a

[12] R. Holmstedt, "Linguistics", paper given during the webinar on June 10, 2021.

[13] In an excursus written together with N. Pat-El, in *The Valediction of Moses*, 96-130, esp. 96, 130.

[14] The only possible parallel he found was on an Etruscan document.

[15] Suchard, "A valediction."

[16] "Literary Observations on Shapira Deuteronomy and Idan Dershowitz's The Valediction of Moses," paper given during the webinar on June 10, 2021.

[17] "Is V a Literary Precursor to Deuteronomy?," paper given during the webinar on June 10, 2021.

[18] A. Lemaire, "Paleography's Verdict: They're Fakes!", *BAR* 23.3 (1997): 36–39.

[19] C. A. Rollston, "Deja Vu all over Again: The Antiquities Market, the Shapira Strips, Menahem Mansoor, and Idan Dershowitz," http://www.rollstonepigraphy.com/?p=896.

[20] B. Sass, "A Note on Paleography," paper given during the webinar on June 10, 2021.

number of letters, and the presence of ligatures), that the same problems are already found in the Moabitica, and that the strips are fakes, probably made by the same circle of people who forged the Moabitica.[21]

1. Methodology

First of all, a word about methodology seems in order. Dershowitz's main conclusion about the use of paleography is as follows:

> While paleographic analysis can be essential for the purposes of authentication and dating, this is not the case for the Shapira manuscripts, considering that the original objects are lost. We do not have a single photograph of the manuscripts in which text is discernible, and we have seen that Ginsburg's drawings are unreliable and mutually contradictory.[22]

He is careful enough to write that paleographical analysis is not "*essential* for the purposes of authentication and dating", which is not the same as to say that it has no value. Yet in practice, he does not make anything of the paleographical data except for a few remarks on two letters (more on this later); he works as if he had *carte blanche* to proceed unimpeded by paleographical objections. But of

[21] I am grateful to Ronald Hendel, Christopher Rollston, Andrew Burlingame, André Lemaire, and Matthew Hamilton for reading an earlier version of this paper and making helpful comments. I also benefited from discussions with Idan Dershowitz, Benjamin Suchard, Harald Samuel, Michael Press, Benjamin Sass, Michael Langlois and Ross Nichols. While the work of "Shapiramaniacs" is sometimes ignored by scholars, they often have a better knowledge of the historical circumstances and sources from the 19th c.; I truly appreciate the generosity of Hamilton and Nichols, who shared with me some of their knowledge and sources, as they do for scholars, whether the latter believe the strips are genuine or not. I warmly thank them. Finally, I am grateful to my wife, Sarah, for checking the English of this article.

[22] Dershowitz, *The Valediction of Moses*, 31.

course, this is not how most epigraphers, used to dealing with forgeries, see things.[23] In their eyes, when dealing with a document that (a) is unprovenanced, (b) is now lost and therefore escapes any possible physical dating and chemical tests, (c) was offered for one million pounds by a man who had already sold hundreds of forgeries,[24] the least that can be said is that the burden of the proof lies on the shoulders of anyone who would argue in favor of its authenticity. And the threshold of proof is very high.

But if a scholar wants to argue in favor of the authenticity of the documents while adding to all that precedes that (d) we cannot really assess the paleography of the document, i.e. we do not have much idea of the script (except for two letters), then it means that we are completely in the dark. This is not a better situation, it is worse; it is a non-starter. If, more cautiously, as Dershowitz does, a scholar (d') does not deny all value to paleographical analysis but dispenses himself from taking it into account except for two letters, then it is very difficult to see how his demonstration could be regarded as satisfactory by epigraphers. Proceeding in such a fashion is *de facto* proceeding outside the boundaries of what is scientifically admissible in the eyes of most specialists of the Old Hebrew script.

It should also be noted that the discussion is excessively simplified in VM,[25] in several respects. Regarding the alleged provenance,

[23] For methodological caveats, see various articles written by C. A. Rollston, notably "Non-Provenanced Epigraphs I: Pillaged Antiquities, Northwest Semitic Forgeries, and Protocols for Laboratory Tests," *Maarav* 10 (2003): 135-93.

[24] According to some accounts, Shapira also sold a forged leather scroll of the Book of Numbers, which had been artificially colored in dark brown (see C. Adler, "Remarks on the Shapira Hebrew Roll, deposited in the Rush Library at Philadelphia," *Transactions of the American Philological Association (1869-1896)* 15 [1884]: xli; Isaac H. Hall, "On a Shapira Roll in Philadelphia," *JAOS* 11 [1885]: cxc-cxi; O. K. Rabinowicz, "The Shapira Scroll: A Nineteenth-Century Forgery," *JQR* 56 [1956]: 9-10; C. Tigay, *The Lost Book of Moses* [New York: HarperCollins, 2016], 306-308). That said, Matthew Hamilton has found possible explanations for the anomalies of this scroll noted by scholars in the 19th c. (personal communication), and this case probably needs reconsideration.

[25] Most of the contents of Dershowitz's article are included in his book, but the article still contains some other remarks on paleography. For the sake of commodity, I will refer to the combined works as VM.

Dershowitz builds an argument on the fact that the strips supposedly came from caves, like the Dead Sea Scrolls discovered much later[26] – but so did some Moabitica.[27] With regard to Shapira's sincerity in the affair, the fact that he made a correction to his own transcription of V (in papers that Dershowitz had the merit of discovering) is not an absolute proof that he himself believed they were authentic.[28] Pretending not to be able to decipher and understand a text that one has forged is a well-known trick used to lure scholars.[29] A trick that Shapira seems to have used himself in 1872 when he sent the reproduction of an inscription to Albert Socin. Above the "original" text, he had written a Hebrew transcription with a question mark after a word and a superlinear *ṭet* written above a *taw*, as if he had hesitations. In fact, the transcription was entirely bogus: the text was a copy of an inscription already sold to a certain Henry Lumley, the previous year, as a document from the time of Moses, whereas it was actually a copy of a funerary Nabatean inscription.[30] (At that time, forgers sometimes made several copies of the same inscription to sell to different people).[31] Even

[26] Dershowitz, *The Valediction of Moses*, 94.

[27] E. F. Kautzsch and A. Socin, *Die Aechtheit der Moabitischen Alterthümer Geprüft* (Strassburg: Karl J. Trübner, 1876), 42.

[28] As already noted by Suchard, "A valediction."

[29] Even recently, according to Sabar, *Veritas*.

[30] Enthousiastic, H. Lumley announced his "finding" in a letter to the *Times*, then sent another letter to retract himself ("New Moabite Stone," *The Times*, January 26, 1872, p. 6; "The Last New Moabite Stone," *The Times*, January 27, 1872, p. 8; I owe these references to R. Hendel). The alleged translation was: "We drove them away – the people of Ar Moab at the marsh ground, there they made a thankoffering to God their King, and Jeshurun rejoiced, as also Moses their leader." Socin reproduces Shapira's annotation as follows:

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רשנו זער מאב בספת שמה עשו חג אדן מלכם וישרן אז(?) שמחו ומשה רבם

See A. Socin, "Über Inschriftenfälschungen," *ZDMG* 27 (1873), 133-34 and fig. I. The original Nabatean inscription had been studied in M. A. Levy, "Eine neue nabatäische Inschrift aus Ammonitis," *ZDMG* 25 (1871): 429-34 (and the figure preceding the article). This is inscription no. 195 in *Corpus Inscriptionum Semiticarum II*, vol. 1 (Paris: Klincksieck, 1889), 217.

[31] They even made copies of genuine inscriptions and tried to sell them as originals, see for instance the case of the stele prohibiting access to the Temple

the concerns that Shapira raised regarding the authenticity of the strips in a letter to Hermann Strack[32] might have been a way to sound honest and cautious, especially after the Moabitica fiasco. It is still possible that in the case of the strips, Shapira was sincere, as he claimed to be in the case of the Moabitica (he said he had been duped by Salim al-Kari), but this should not be taken as given.[33]

Finally, because Dershowitz thinks that Shapira sincerely believed in the authenticity of the strips, and because Shapira said he had acquired them from Bedouins for a small sum, Dershowitz argues that no clear motive can be found for their creation if they were forgeries. Neither money nor fame could have motivated the supposed anonymous "mastermind" who fabricated them.[34] However, leaving aside the fact that we are not obliged to believe Shapira's account, a number of other motives can lead forgers to create a text, as Rollston notes.[35] Some of the first scholars to reflect on the strips were struck by the fact that the text was free of problems highlighted by biblical criticism in the traditional version of Deuteronomy (especially some confusions in the itinerary of the Hebrews), a fact pointed out by Shapira himself.[36] The strips, written in a seemingly archaic script, with some resemblance to that of the 9th century Moabite stone, gave the impression that a version of Deuteronomy already existed before the 7th century. This seemed to contradict the documentary hypothesis in the form put

to Gentiles, which was reproduced by M. Boulos (C. Clermont-Ganneau, *Les fraudes archéologiques en Palestine* [Paris: Ernest Leroux, 1885], 39-48.)

[32] This letter is dated May 9, 1883. A transcription is available on Ross Nichol's academia.edu page and on his blog: https://themosesscroll.com/moses-shapiras-9-may-1883-letter-to-hermann-strack/.

[33] I would add that whether innocent or guilty, his fate and the consequences for his family are heartbreaking. Even though his daughter's novel is not designed to be purely autobiographical, it witnesses the sufferings of Shapira's family. The French edition has recently been republished: M. Harry, *La petite fille de Jérusalem* (Paris: Éditions des Malassis, 2016). If we believe her account, her father was sincere in this affair, but the testimony of a relative on this point cannot be relied upon, as it is, naturally, biased, and she might have wanted to vindicate him.

[34] Dershowitz, *The Valediction of Moses*, 39-40.

[35] Rollston, "Non-Provenanced Epigraphs I," 191-92.

[36] See the account by Nichols, *The Moses Scroll*, 63-68.

forward by Julius Wellhausen, which had just provoked strong reactions in many religious circles.[37] So a (possibly religious) motivation linked to the contemporary discussion on biblical criticism should be considered.

2. The evidence

Now let us take stock of what we have in terms of sources for the paleography of the Shapira strips:

(1) drawings of the strips made under the supervision of Christian Ginsburg;[38]
(2) a paleographical chart made by Ginsburg (or an assistant of his); it was part of the dossier that the British Library (BL) intended to, but for unknown reasons did not, publish[39] (Figure 2);
(3) a paleographical chart made by Hermann Guthe[40] (Figure 1);
(4) a drawing of a column and a drawing of a sequence of characters in *The Graphic*, made by an unknown illustrator;[41]
(5) a drawing of a column in the *Ilustrated London News*, with unfortunately only a few legible characters.

[37] See P. Kyle McCarter Jr, "Why All the Fuss?," *BAR* 23.3 (1997): 40.

[38] Some of them are published in Dershowitz's publications and are also available at https://en.wikipedia.org/wiki/Shapira_Scroll.

[39] It is part of a file entitled "Reports by Dr Rieu", which also contains a note saying that a committee on October 13th, 1883, decided to publish, as soon as possible, "the report of Dr Ginsburg, with text, translation and notes;" this report was to include a "comparative table of letters" in addition to the "facsimiles." It is also written that "proof-sheets of the publication were before the Board" and "the Trustees approved." However, the publication of the chart did not happen. The version of the paleographical chart that is included in the file was prepared by Dangerfield lithographs.

[40] Hermann Guthe, *Fragmente einer Lederhandschrift enthaltend Mose's letzte Worte an die Kinder Israel, mitgeteilt und geprüft von Hermann Guthe* (Leipzig: Breitkopf & Härtel, 1883), chart at the end of the book. An English translation of this book is available at R. Nichols's academia.edu page.

[41] He or she signed with initials at the bottom left of the page, although it does not seem possible to identify him or her. The drawing served to illustrate an article written by Boscawen in the same issue of the journal (I thank M. Hamilton for pointing this out to me).

Here is what Dershowitz makes of this evidence:

(1) He quotes André Lemaire noting serious paleographical problems in a drawing made under the supervision of Ginsburg. Dershowitz agrees that these are problematic, but he discredits the reliability of Ginsburg's drawings.

(2) Dershowitz did not include nor mention the BL paleographical chart in his publications.[42] It is published here for the first time (see Figure 2, columns under the name "manuscript").[43]

(3) Surprisingly, or perhaps not, Dershowitz's publications do not contain any real argument against Guthe's chart of letterforms. The only possible criticism is contained in the following sentences:

> Ginsburg and Guthe were well versed in the paleographic curriculum of their generation, and that knowledge appears to have influenced how they saw and represented the script of the Shapira manuscripts. Illiterate artists, on the other hand, are largely immune to such pattern-recognition hazards.[44]

At the same time, Dershowitz writes that "Guthe's descriptions and discussion of the letterforms are considerably more useful than Ginsburg's drawings, for example, although they too cannot be taken at face value."[45] Nevertheless, how useful Guthe's paleographical descriptions and discussions exactly are, what precise limitations they bear, we are not told. His letterforms chart is, in practice, ignored.

[42] He orally mentioned it during the webinar organized by Daniel Stoekl Ben Ezra on June 10th, 2021. The next day, at my request, he kindly sent it to me, for which I am grateful.

[43] With permission from the British Library Corporate Archive. It is almost simultaneously published in a popular article co-written with R. Hendel, to appear in *BAR* (2021). I am most grateful to Katie Espley, from the British Library, for her help in getting the permission to publish this chart. Unfortunately, the page was not completely flat when the image provided to me was taken. When editing this article, M. Langlois was able to correct this aspect of the chart for the relevant columns in Figure 2 (I thank him for his help).

[44] Dershowitz, "The Valediction of Moses," 15.

[45] Dershowitz, *The Valediction of Moses*, 31.

(4) and (5) We read in Dershowitz's book: "Most il[l]uminating of all are the naive drawings prepared by artists who were not literate in Paleo-Hebrew, which have been largely overlooked."[46] In my view, such drawings have their own shortcomings, but I agree that they should be examined too.[47] Yet the only use he makes of them concerns two letters, *he* and *yod*.

In a word, as already announced, Dershowitz leaves aside virtually all of the evidence. Sass, for his part, disregards all the material left by the scholars who examined the strips.[48] We are left with the vertiginous idea that all the paleographical data assembled by these scholars – even the paleographical charts – are useless, whereas a few letters copied by an artist are valuable. He does not use all of the latter, however (some are obviously problematic): just the one, a special *yod*, that he believes supports the case for authenticity.

This somewhat selective use of evidence is based on arguments that are, in my view, flawed, although Dershowitz and Sass helpfully point out some limits of the drawings. That said, since the document ostensibly made by Ginsburg for the sake of paleographical analysis is his chart, the drawings are of secondary importance; moreover, the two letterform charts already constitute a sufficient basis for the paleographical analysis. As a matter of fact, none of my conclusions will be based on a letterform that is attested solely on the drawings. So, to keep this article from being excessively long, I will discuss the reliability of the drawings in an Appendix, and confine myself to discussing the reliability of the charts in the next section. Afterwards I will discuss the script in more details, beginning with the few letters discussed by Dershowitz and Sass (sections 4 and 5), then enlarging the scope to other letters (section 6).

[46] Dershowitz, "The Valediction of Moses," 15.

[47] An interesting aspect of the sequence of letters at the top of the illustration in *The Graphic* is that the artist seems to have reproduced the thin and thick strokes of some characters. However, in a number of cases, the variations in thickness do not reflect the normal ductus of Old Hebrew scribes. In addition, the same illustrator seems to have drawn thin and thick strokes when reproducing some letters from the Moabite Stone, which is not an ink inscription (same illustration, bottom left).

[48] Sass, "A Note on Paleography."

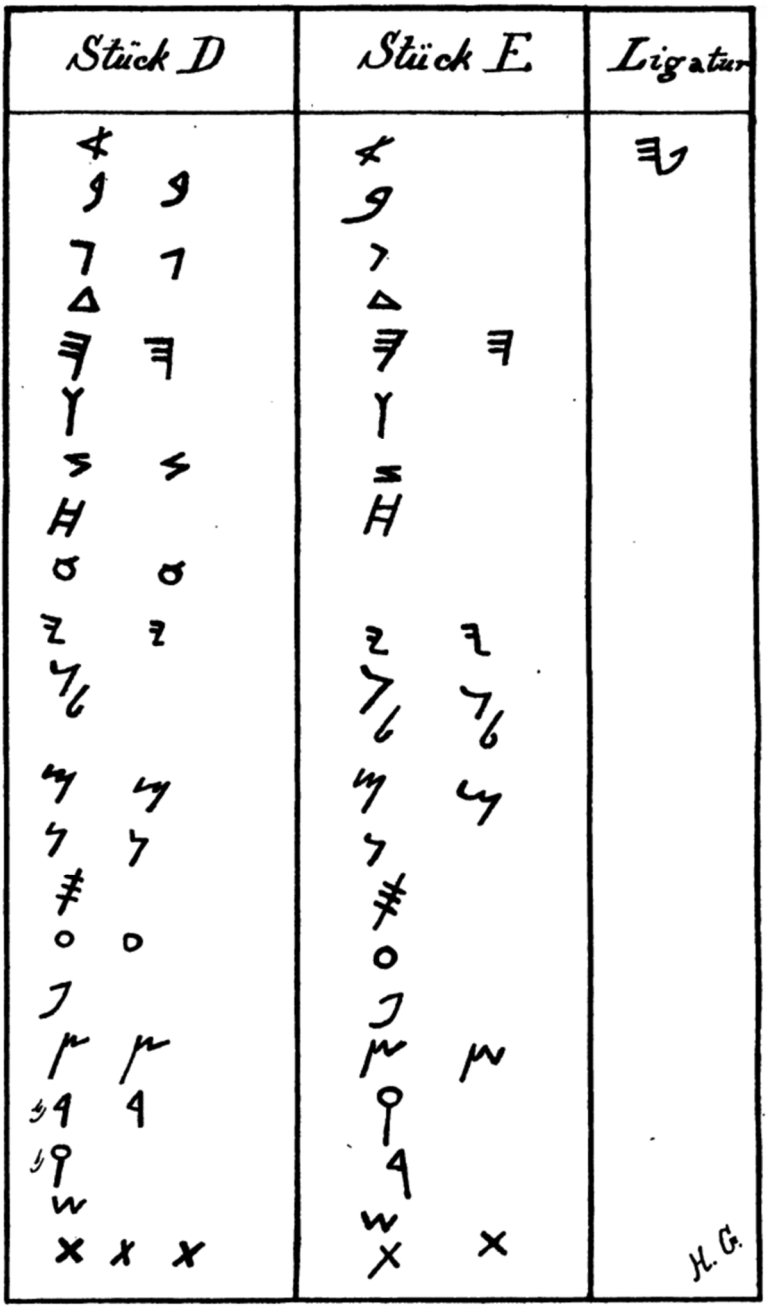


Figure 1: Guthe's letterforms chart

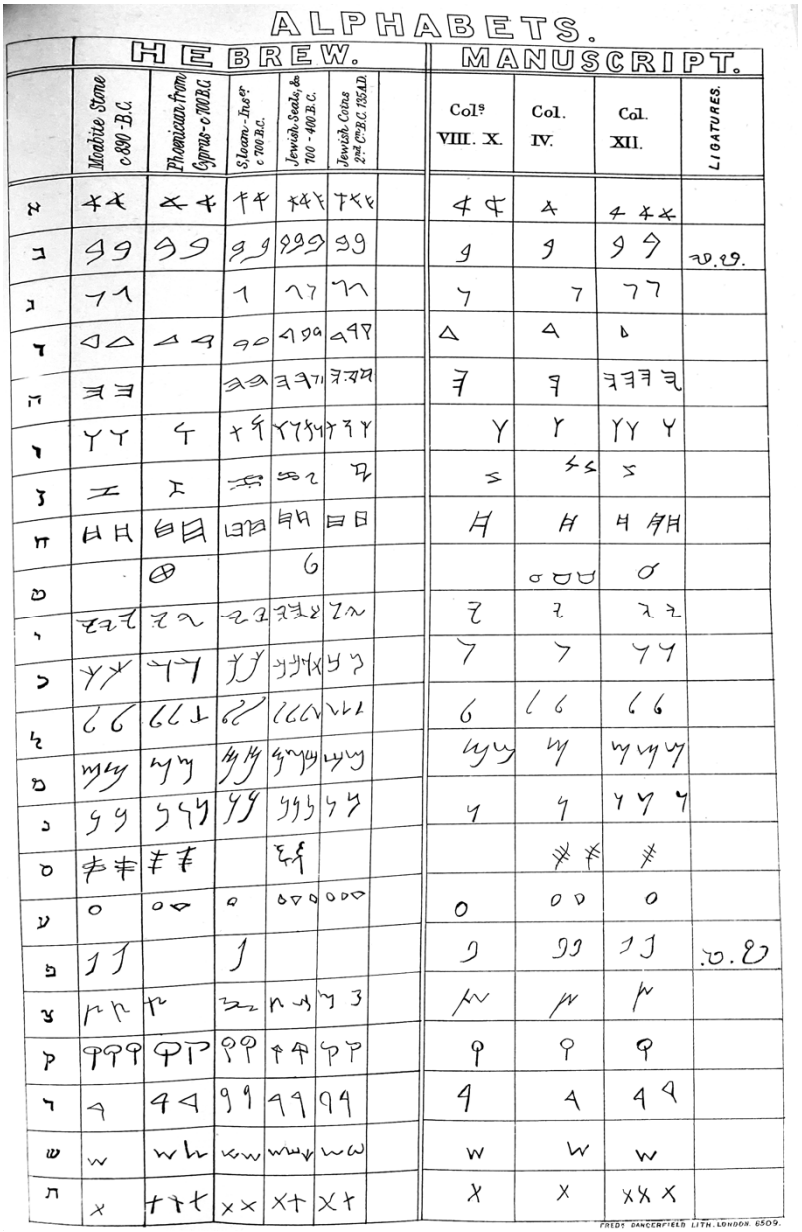


Figure 2: Ginsburg's letterforms chart

3. On the reliability of the paleographical charts

The remark by Dershowitz and Sass that may concern the paleographical charts is that scholars are liable to pattern-recognition hazards. However, to note that scholars are sometimes unconsciously influenced by their previous knowledge of paleography when they visually analyze a letterform in a new document, and/or when they draw it, is one thing. To argue that this marginal problem nullifies their work is quite another – an extravagant claim indeed, especially when it concerns paleographical charts, the aim of which is, by definition, to record the letterforms as they appear on a document. With the same argument, one could argue that the more experienced a paleographer is, the less reliable his drawings and paleographical analyzes are! In the case of Shapira strips, the risk of pattern-recognition hazards was minimized by the fact that every letter in the alphabet occurred several times on the strips and most of them, many times, so that a false impression given by one occurrence could easily be corrected by many others. Moreover, as Table 2 shows, and as we shall see in more detail later, some of the problematic features in the script that are attested in Ginsburg's and Guthe's works are confirmed by the artist of the *Graphic*, who, according to Dershowitz and Sass, was not subject to the same pattern-recognition hazards.

In addition, both Guthe's and Ginsburg's charts record variants of letterforms. That is, both scholars noticed that the strips themselves contained variations in the shapes of the letters and their stances, and they decided to record these variations in their charts. The pains thus taken to represent slight differences in morphology and stance between these variants is a testimony to the care they gave to this work.[49] They were well aware of how important it was

[49] In Ginsburg's chart, the columns to the left (under the title "Hebrew") were included for comparison and were probably not all made afresh from the documents themselves – certainly not in the case of the Siloam Tunnel inscription, which was excised from the tunnel years later (in 1890). The first published drawings of the Siloam Tunnel inscription, made in extremely difficult conditions, were sometimes of low quality. As a result, the material used for this column was not good. Ginsburg probably did what many epigraphers do when they publish

to decide whether the letterforms were similar or not to already known inscriptions; in particular, Guthe, who discusses the paleography of the strips in his book, had an obvious interest in knowing the shape of each letter of the alphabet. Ginsburg's chart is more detailed, it records more variant letterforms, either because he aimed at more exhaustivity, or because he had much more time to study the strips, or because the columns of the strips he used contained more variants (it is difficult to tell whether they based their charts on the same columns, since the numbering used by Ginsburg is not explicated).

Furthermore, these paleographical charts, albeit independently made,[50] overlap with and corroborate each other, down to the most

new inscriptions: preparing second-hand reproductions of letterforms for the columns concerning epigraphs already published elsewhere and used for comparison, and fresh letterform columns for the new document. Indeed, by contrast, the columns to the right (under the title "manuscript") were clearly made from direct examination of the Shapira scrolls, which were available to Ginsburg for weeks; this is why, like Guthe, he was able to have distinct columns in the table for distinct columns in the manuscript(s).

[50] Guthe examined the strips during the first week of July 1883, he wrote the foreword to his book on August 14, and the book appeared on August 31 (see the account by Nichols, *The Moses Scroll*, 87). So he completed his work long before Ginsburg published the few drawings he ever published (Ginsburg, "The Shapira Ms. Of Deuteronomy," *The Athenæum* 2915 [1883]: 305, in an article published September 8); as for Ginsburg's chart, it was never published. Therefore, Ginsburg's work could obviously not have influenced Guthe. Conversely, Guthe's book came out too late, on August 31 in Germany, to reach Ginsburg in England when he prepared the drawings already published on September 8. And a close comparison shows that the typical letterforms of Ginsburg's charts, even the most idiosyncratic forms of the strips, are already attested on these drawings (the only exception is the form of *zayin*, which is a bit different). This shows that Ginsburg had already identified these letterforms before Guthe's book could possibly have influenced him. In fact, as noted by Nichols (*The Moses Scroll*, 87), the first notice of Guthe's book in England appeared the very day the first drawings were published (September 8), in the form of a brief unsigned book notice ("The Shapira MSS. of Deuteronomy," *The Academy* 592 [Sept. 8, 1883]: 161-62). Such notices of books appearing in other countries were possible thanks to foreign correspondents (often communicating by telegram), whereas the shipping and delivery times of parcels from Germany make it highly unlikely that the book itself could have reached Ginsburg when he was preparing the first drawings (the making of which must have been a long task, not just a job done the day before they were sent to the

surprising eccentricities of the script of the Shapira strips.[51] Table 1 illustrates this overlap by showing some similar forms from both charts. While Ginsburg's contains still more variant letterforms, it confirms all of Guthe's letterforms (with the possible exception of *yod*) with only minute differences that are to be expected in charts from the 19th c., and that may also be due to the fact that they did not draw the same occurrences of a given letter.[52] Thus, some of Ginsburg's letterforms include some strokes that are drawn in a less rectilinear way than in Guthe's chart (although this may also be partly due to variations between and within the columns they used).[53] This is, however, a superficial difference that does not af-

printer). Similarly, Ginsburg's own chart was likely already prepared before September 11, when Dangerfield's firm was asked to estimate the cost of making a lithograph of it (the estimation and the order are kept in the file "Reports of Dr Rieu" at the British Library). In fact, there is not a single hint in the 19th c. sources that Ginsburg ever noticed Guthe's work, let alone used it (I thank Nichols for discussing this with me).

The independence of the two charts from each other is also manifest in their "calligraphic" details (the strokes tend to be more rectilinear in Guthe's chart, and a bit curvier in Ginsburg's). In addition, Ginsburg recorded more paleographical variants, which indicates that he did his own homework. None of Ginsburg's columns is a reproduction of a column by Guthe, as missing, additional or different letterforms appear when comparing any two of their columns. It is clear – if one were to imagine such an extreme scenario – that he did not copy the letterforms from Guthe. Ginsburg did copy Clermont-Ganneau's facsimile of the Moabite stone, but it was not a letterforms chart, and he did so because he did not have a choice: the necessary documents were simply not available to him (see the Appendix). Not so in the case of the strips, which he examined for weeks: he did not need to learn the script from Guthe but knew it intimately since he had spent weeks in August deciphering it (he published his transcriptions in several installments the same month).

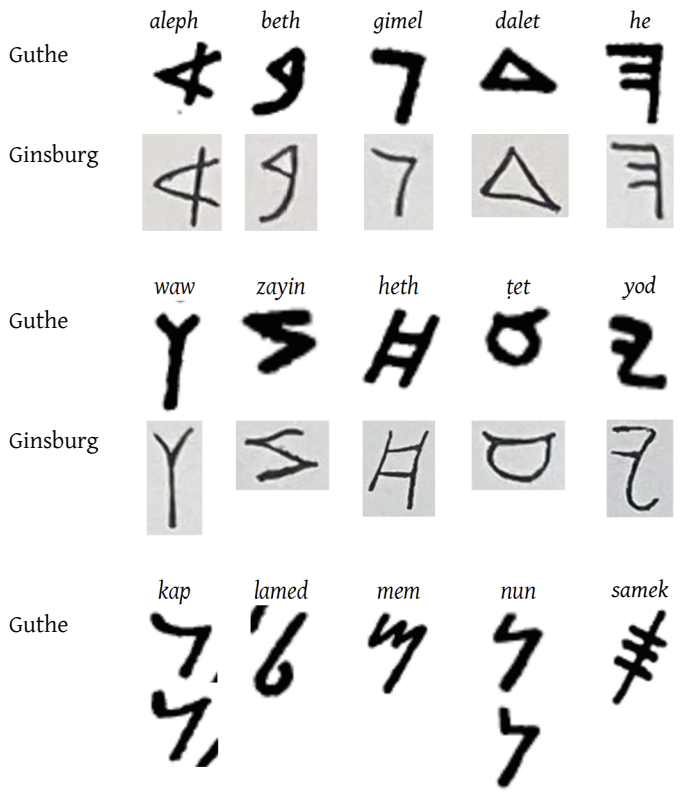
[51] As already noted by S. Guil, "most of the paleo-Hebrew letters which were drawn by Guthe are similar to those drawn by Ginsburg" ("The Shapira Scroll was an Authentic Dead Sea Scroll," *PEQ* 149 [2017]: 21).

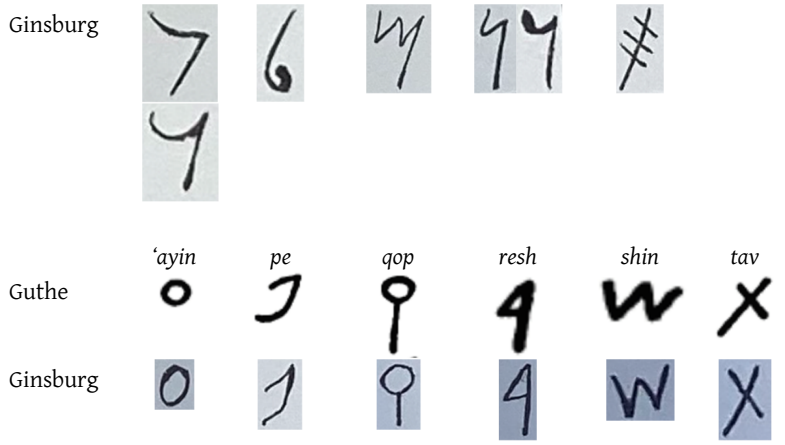
[52] It should also be noted that on the image of Ginsburg's chart provided to me, the letters are seen from an angle (because the page was not flat when the photo was taken). So, in reality, *pe* and *tav* are even more similar to the corresponding forms in Guthe's chart than appears in Table 1.

[53] Even this slight difference is far from systematic: for instance, both scholars draw the top horizontal stroke of *kap* as a bit curved, and the downstroke of *nun* as rectilinear.

fect the paleographical analysis, because both manners of reproducing the letters point to the same fundamental letterforms; they encode the same relevant information about morphology (e.g. the number of strokes and their directions) and stance, which is sufficient for the present purpose. Paleography is neither calligraphy nor photography. Similarly, epigraphers do not always reflect, in their paleographical charts, the thin and thick strokes of ink inscriptions: this is a shame with regard to the study of the ductus, but this has no impact on the identification of the morphological features and the stances of the letters.

Table 1





Such a remarkable overlap for so many letters would not be obtained if the charts did not reflect the script of the documents. Under such conditions, the theory according to which both scholars failed to correctly record the letterforms does not only impute a considerable number of implausible blunders to two experienced scholars: it imputes the *same* series of implausible blunders to them. In fact, the risk for these scholars would have been to erroneously "project" some letterforms they knew from the Moabite stone onto the strips. Indeed, at that time, Mesha's stele constituted the best candidate for an archaic Hebrew text allegedly found in Moab.[54] However, both charts agree about the letters that are, at least in some instances, similar to the script of the Moabite stone; they also agree about the letters that just sharply diverge from it. In the latter case, they drew the same, unexpected letterforms. To anticipate a bit the detailed discussion of section 6, they record (the same) five totally unexpected letterforms that do *not* appear on the Moabite stone (*zayin*, *ṭet*, *kap*, *ṣade*, and *qop*). And the only parallels to these five surprising letterforms appear in other scripts (Aramaic, Phoenician or Nabatean), on some Moabitica, and

[54] Both scholars also knew the Siloam Tunnel inscription, but a comparison with the column representing its script on Ginsburg's chart immediately shows that it had no influence here, and the same can be said for Guthe: see the image of the Siloam Tunnel inscription that he had published the previous year in H. Guthe, "Die Siloahinschrift," *ZDMG* 36 (1882): 725-50 (the image immediately precedes the article; it is a photograph of a gypsum cast).

perhaps for one letter (*kap*) on some Paleo-Hebrew coins from the 2nd c. BCE. To imagine that Guthe eclectically "picked" letterforms in such a variety of scripts to make the paleographical chart of a Hebrew manuscript which he had before his eyes already strains credulity. To imagine that Ginsburg independently did exactly the same, for the same letters, defies imagination: the probability of such a set of extraordinary coincidences is zero. The only reasonable explanation, again, is that both saw these letterforms on the strips.

As if that were not enough, we shall see later that a number of Guthe's and Ginsburg's letterforms are confirmed by other sources. The artist of the *Graphic* not only corroborates a number of Mesha-like letters, but also two of the anomalous letterforms (*zayin* and *kap*). Shapira himself, in his letter to Hermann Strack,[55] notes that all but three letters are similar to the script of the Moabite stone, and he draws forms of *ṭet* and *kap* (from the strips) that corroborate what we see in Guthe's and Ginsburg's charts. Should we believe, for instance, that these four witnesses (Guthe, Ginsburg, the anonymous illustrator and Shapira),[56] including an artist supposedly not liable to pattern-recognition hazards, all independently had the same surprising idea of borrowing an extremely rare form of *kap* from coins from the 2nd century BCE? This is beyond imagination. Should we rather imagine that these witnesses were correct when they drew the surprising letterforms, and that it is the other, Mesha-like letterforms, that are due to pattern-recognition hazards? That would settle the debate, because these five letterforms are already enough to establish that the documents are forgeries (more on this later). However, this scenario is as equally unlikely, because here again, several independent witnesses corroborate each other for too many letters.[57]

On top of that, Guthe and Ginsburg converge in noting the same other anomalous features that will be discussed in section 7: the

[55] I thank Nichols for sending me an image of the page of the letter where Shapira drew a couple of characters.

[56] Moreover, we shall see that this form is further confirmed by a fifth piece of evidence (its similarity to *gimel* on the strips).

[57] Not only Guthe and Ginsburg, but also the *Graphic* illustrator for a number of letterforms, and Shapira himself, who wrote in his letter to Strack that all but three letters were already attested on the Moabite stone.

wrong stance of several letters, and the presence of ligatures. Here again, the scenario of coincidences is not a viable option. In the end, we have to face the facts: while certainly imperfect (as are all such charts), the paleographical charts prepared by the German scholar and his British colleague corroborate each other, and are corroborated by other witnesses, which establishes their reliability.

4. About the stance of *he*

I now turn to the examination of the script of the Shapira's strips, beginning with the very few features that are discussed by Dershowitz and Sass.

As already mentioned, VM contain remarks about the shape of two letters, *he* and *yod*. Regarding the former, several scholars have noticed that on Ginsburg's drawings, *he* leans to the right, which is abnormal in Paleo-Hebrew. This also happens on the two charts of letterforms. VM counters that on the illustration of Fragment A, column 1, published by *The Graphic*, a "naïve artist" drew *he* as leaning to the left. And it is argued in VM that non-epigraphers draw what they see, whereas scholars are liable to pattern-recognition hazards. The trouble with this argument is that if Ginsburg and Guthe had been influenced by their previous paleographical knowledge, they would have drawn *he* as leaning to the *left*.[58]

[58] Indeed, this is how *he* appears in most instances on the Mesha stele, and this is how it appears on Clermont-Ganneau's facsimile of the stele, correctly reproduced in Ginsburg's own facsimile of the stele. In passing, one may wonder if the artist working for *The Graphic* was really "naïve," because at the bottom-left of the same page, we can see paleographical charts of the Mesha stele, of the Siloam inscription and of "Oldest Heb. M.S" (?), clearly made by a non-specialist. The letter *he* appears three times on the chart taken from the Siloam inscription, and each time it leans to the left. This may have influenced the illustrator who drew the line at the top of the illustration on the same page, presumably the same person referred to by the initials at the bottom-left. The entire page was published in *The Graphic* (Sept. 1, 1883): 224. The archives of *The Graphic* are to be found at https://www.britishnewspaperarchive.co.uk/titles/the-graphic. An image of these charts is also available on Wikimedia Commons, although it is presented

Furthermore, the same page of *The Graphic* contains a drawing of Fragment E in Va, column 3,[59] with at least four instances of *he* (see Fig. 3): one is vertical (in line 1), the three others lean to the right (lines 3 and 4).[60] So if we really take these drawings made by illustrators seriously, as is suggested in VM, then we now have a confirmation that *he* was leaning to the right on Fragment E in at least a number of instances. More precisely, this illustration suggests that the stance of *he* was variable on the strips. This is corroborated by the two paleographical charts. But if the stance of *he* was variable on the strips, it is a paleographical anomaly.

Figure 3[61]

5. About the special features of *yod*

The only other piece of evidence that is regarded as reliable in VM, in the paleographical data, are two aspects of *yod* visible on an il-

there as published in the Scientific American: https://upload.wikimedia.org/wikipedia/commons/8/8c/Shapira_Strips%2C_1883%2C_Scientific_American._01.jpg

[59] The identification is made in Dershowitz, *The Valediction of Moses*, 18.

[60] See the readings in Dershowitz, *The Valediction of Moses*, 143.

[61] This image is from Wikimedia Commons, which indicates that the source is Dershowitz's book: https://en.wikipedia.org/wiki/Shapira_Scroll#/media/File:Dersh7.png.

lustration in *The Graphic* (see Fig. 4): the presence of a cursive flourish (or reflex), and the fact that in one instance, the letter is penned without raising the pen.

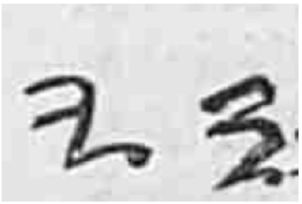


Figure 4[62]

VM argues that these features are now attested in inscriptions that were not yet unearthed in Shapira's lifetime, notably some Samaria ostraca, found in the early 20th c. So we read that "in light of our expanded comparanda following the many new epigraphic finds since 1883, various features once regarded as proof that the Shapira manuscripts were forgeries now appear to validate their antiquity,"[63] and this is presumably what is referred to later in the book, when we read that "the little reliable paleographic data we have points to the manuscripts' exceptional antiquity."[64] The idea is that a forger living at the end of the 19th c. could not have known that *yod* sometimes has a cursive reflex. At first glance, this is a fine argument, the kind of "uncanny fact" that makes the discussion in VM exciting. On second thoughts, it does not seem that strong, because ticks were spontaneously added to letters on the Moabitica. But the argument is certainly worth investigating.

If truth be told, I am not entirely convinced that what we see on *The Graphic* illustration are cursive reflexes, because they look more like dots or extremely small ticks, the kind of things you make at the end of a stroke when you write with an ink pen. We do have instances of small cursive reflexes on some inscriptions from Kuntillet 'Ajrud, but as far as I can see, they are still ticks, longer than

[62] Detail from the image available on Wikimedia Commons, where the source indicated is Dershowitz's book: https://en.wikipedia.org/wiki/Shapira_Scroll#/media/File:Dersh7.png.

[63] Dershowitz, *The Valediction of Moses*, 33.

[64] Dershowitz, *The Valediction of Moses*, 94.

what we see on *The Graphic*. Nevertheless, for the sake of the argument, I will admit that what we see on *The Graphic* illustration are cursive reflexes.

As a matter of fact, a Paleo-Hebrew seal with two *yods* bearing a cursive reflex had already been published by Moritz Abraham Levy in 1869 (see Fig. 5).[65] He published it as an *althebraïsch Inschrift* and explicitly noted this special feature in his discussion.[66] This happened nine years before Shapira first mentioned his strips, in what was one of the very few books containing inscriptions in a script explicitly identified as Hebrew. At that time, a person desiring to forge a very old-looking book in Hebrew did not have many places to search to find examples of the old Hebrew script in order to imitate it.

Figure 5

Now since Shapira was already advertising the strips to Schlottman in 1878, and since according to him the strips were first presented to him the same year by Bedouins who told him that they were found about 12 years before,[67] I imagine that one could perhaps argue that the strips were discovered approximately in 1866, before the publication of Levy's book. Of course, this entails believing Shapira's account.

At any rate, another *yod* with a cursive reflex is present on a sketch of the Mesha stele made in 1868 or 1869 by none other

[65] M. A. Levy, *Siegel und Gemmen* (Breslau: Schletter'schen Buchhandlung, 1869), Tafeln III, No 7a. This corresponds to *WSS* 316 in N. Avigad and B. Sass, *Corpus of West Semitic Stamp Seals* (Jerusalem: Israel Exploration Society, 1997).

[66] Ibid., 55.

[67] See for instance Tigay, *The Lost Book of Moses*, 175-76.

than... Salim al-Kari. He made a sketch of lines 13-20 and sent a copy containing three lines of it to Clermont-Ganneau in 1869; later, he gave him the complete sketch, or a copy of it.[68] Salim copied the text from left to right, continuously, that is, without going to the next line when the stele did, and his sketch contains a number of mistakes and doublets. But it also contains precious information and Clermont-Ganneau used it when he reconstructed the stele; he published the sketch in 1887, together with an identification of the (numbered) letters.[69] On Salim's sketch, letters No 45 and 46 of line 18 are two successive *yods* belonging to the sequence LYYHHW (see Fig. 6). This corresponds to the actual beginning of line 18, that is, the right-hand end of this line. The correct reading is LY.YHWH. On Salim's sketch, the first *yod* bears a cursive reflex.

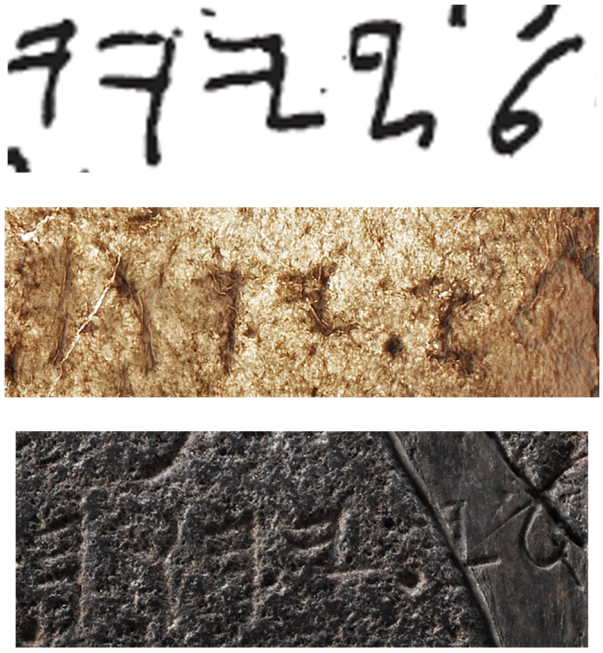


Figure 6

[68] Clermont-Ganneau, *Les fraudes archéologiques*, 156-59.

[69] Clermont-Ganneau, *La stèle de Mésa : Examen critique du texte* (Paris: Imprimerie nationale, 1885), 14-15. A photo of Salim's sketch also appears in I. Bonora Andujar, "Les esquisses de la stèle de Mésha," in *Mésha et la Bible : Quand une pierre raconte l'Histoire*, ed. Th. Römer (Paris, Collège de France, 2018), 24.

The relevant part of the stone itself is lost (what we see is plaster). But on the squeeze, there appears to be a small vertical tick extending below the tail of the first Y. Either this is an actual cursive reflex, but this is not very likely because no other instance is attested on the stele, or the tick is due to some dirt or accident present on the stone before its being broken. Either way, Salim's sketch contained a *yod* with such a tick. Besides, we can see on a number of other letters on the same sketch that Salim had a tendency to "see," and in any case to draw, small ticks at the lower extremity of other letters, like serifs.

The presence of this cursive reflex is uncanny because Salim's sketch of the Mesha stele served as a model for the script of the Moabitica – this is how Clermont-Ganneau discovered the trick in the case of the Moabitica. As scholars readily understood, Salim and his possible co-forger(s) had used a sort of a "paleographical database" inspired by this sketch and various inscriptions, in Nabatean, South-Arabian, and so on.[70] I am not necessarily saying that Salim himself made the strips, although this is not excluded since the break between him and Shapira was relatively recent when the latter first mentioned the strips.[71] (Note, in passing, that the *lamed* with a closed loop on the same line of *The Graphic* resembles the

[70] Kautzsch and Socin, *Die Aechtheit der Moabitischen Alterthümer Geprüft*, 120-38. Note also what A. S. Yahuda writes about Salim al-Kari: "he gave me a notebook with primitive drawings and all kinds of ancient alphabets as well as copies of various inscriptions, some of which he used for his forgeries" ("The Story of a Forgery and the Mēša Inscription," *JQR* 35.2 [1944]: 146.) However, Yahuda's account contains a number of problems; W. F. Albright noted some of them ("Is the Mesha Inscription a Forgery?," *JQR* 35.3 [1945]: 247-50), and Hamilton pointed out others to me (personal communication). That said, we did not need Yahuda's account to conclude that Salim used this kind of "paleographical" database anyway, since it is evident from the Moabitica; in fact, it may well be that it gave Yahuda the idea to create this detail in his story, if it is fictional.

[71] Salim's implication in the making of the Moabitica was fully and definitively exposed in December 1877; see Martin Heide, "The Moabitica and their Aftermath," 215-17. But he was probably dismissed by Shapira earlier in the same year, since in August 1877, Salim sent a letter to Clermont-Ganneau in which he alluded to his (clearly recent) conflict by Shapira ("je veux le perdre comme il m'a perdu"): "The Moabite Pottery," *PEQ* 10 (1878): 100. Thus the collaboration between Salim and Shapira ended not that long before the latter first mentioned the strips, in 1878.

same letter on Salim's sketch). But an instance of *yod* with the cursive reflex, a rarity in the 19th c. epigraphical documentation, was present on a document which entered the "database" and could be used by people from his former circle.

Regarding the single-stroke yod

The other feature noted by Dershowitz (and highlighted by Sass) is the single-stroke *yod*, or zigzag *yod*, that is a *yod* drawn without raising the instrument of writing, in a continuous motion. As noted in VM, this feature was first identified by scholars on some Samaria ostraca. A forger of the 19th c. could not have foreseen this feature, it is argued. Here again, the argument seems fine at first glance, but on closer inspection, it proves weak. To begin with, it would be more interesting if the letterform in question was a complex sign, but here we are merely talking of a zigzag, and it does not take a genius to draw a zigzag. A look at the Moabitica and their myriads of variations on letterforms leaves little doubt as to the ability of forgers to produce all sorts of variants.

More importantly, *all of the alleged parallels are debated*, so the very existence of the single-stroke *yod* in Old Hebrew script is uncertain. Regarding the Samaria ostraca, a couple of drawings made by Reisner may suggest that a few *yods* were penned in a continuous motion, and I. T. Kaufman argued that a dozen ostraca contain such *yods*.[72] Yet a few years later, Lemaire pointed out only one case.[73] In my own doctoral dissertation, I was not convinced by it.[74] Based on his own thorough examination, Rollston concluded that

> *pace* Kaufman, the enhanced Kaufman negatives reveal that these *yods* were *not* formed with a continuous stroke of the pen. Rather, the

[72] I. T. Kaufman, *The Samaria Ostraca: A Study in Ancient Hebrew Palaeography* (PhD dissertation, Harvard University, 1966), 46-47.

[73] A. Lemaire, *Les ostraca hébreux de l'époque royale israélite*, vol. 1 (PhD dissertation; Paris: Université de Paris III, 1973).

[74] M. Richelle, *Le royaume d'Israël durant la première moitié du VIII^e s. av. n.è. : Analyse critique des sources épigraphiques, bibliques et archéologiques*, vol. 1 (PhD dissertation; Paris: EPHE, 2010), 134-35.

> initial and terminal points of the horizontal strokes are clear, demonstrating that these were separate strokes, necessitating the lifting of the pen, not continuous ones.[75]

Checking again photographs of all the alleged cases today, I still do not see any convincing instance.[76] At the very least, this is a matter of debate.

The other parallel mentioned by Dershowitz and by Sass is a problematic form on a jar found at Tel Reḥov. A number of scholars believe it is a *yod*, but a hieratic numeral was also considered.[77] If one reads *yod*, the resulting expression is *lšqynmš*, translated "belonging to the cup-bearer, Nimshi." But the normal sequence would be *lamed* + PN + title. Also, the word *šqy* is unattested in Hebrew; if deemed an Aramaic loanword, the final *alep* is missing.[78] Even the scholars who read a *yod* do not hide the fact that the form is bizarre. Explaining the zigzag *yod* from the *Graphic* by this form would be explaining *obscurum per obscurius*. Generally speaking, ignoring the multi-converging evidence concerning 21 letters or so of the strips to focus on a single bizarre letterform from a single non scholarly source, with no assured parallel, is certainly, from a methodological perspective, a recipe for disaster.

The argumentation concerning the special *yod* is therefore based on shaky foundations. Still, let us continue the discussion a bit, assuming for a moment, for the sake of the argument, that the alleged parallels do exist. Even so, it is not as if the form on the

[75] C. A. Rollston, *The Script of the Hebrew Ostraca of the Iron Age: The 8th-6th Centuries BCE* (PhD dissertation; Baltimore: John Hopkins University, 1999), 77 fn. 148.

[76] Even what may be regarded as the best possible candidate, on ostracon N° 15 (illustrated in Dershowitz, *The Valediction of Moses*, 33, Fig. 28), does not convince me. Reisner's drawing correctly represents neither the junction between the top two horizontal strokes, nor the relative thickness of the strokes of the letter, as they (clearly) appear on the photo.

[77] S. Aḥituv and A. Mazar, "The Inscriptions from Tel Reḥov and their Contribution to the Study of Script and Writing during Iron Age IIA," in *"See, I will bring a scroll recounting what befell me" (Ps 40.8): Epigraphy and Daily Life from the Bible to the Talmud*, edited by E. Eshel and Y. Levin (Göttingen: Vandenhoeck & Ruprecht, 2014), 44-45; S. Wimmer, *Palästinisches Hieratisch: Die Zahl- und Sonderzeichen in der althebräischen Schrift* (ÄAT 75; Wiesbaden, 2008), 141.

[78] I thank Andrew Burlingame for discussing the philological aspects of this inscription with me.

Graphic was exactly the same. It consists in two rotated Us – not Vs – plus a horizontal stroke and a cursive tick. Even if one leaves aside the cursive tick, since it is "optional," to admit that this form is the equivalent of the alleged parallels requires a little leap of faith. Indeed, the latter comprise (at least) five strokes; they form two rotated Vs plus a horizontal stroke. It is not only the case on the Reḥov jar inscription, where the incision made after firing could explain the angular shapes; the Samaria ostraca suggest that on ink inscriptions too, the *yod* drawn in a continuous motion was penned with angles, Vs, not the same kind of curves or Us that one sees on the *Graphic* illustration. Why would the situation be different on the strips?

More importantly, the lower horizontal stroke of the *Graphic* form is surprisingly short, whereas on the alleged parallels it is so long that it extends to the right further than the top horizontal strokes. Which is also the case not only on the paleographical charts, but also on the other *yod* of the *Graphic* illustration, although it is clearly not a single-stroke *yod*. This is yet another surprising feature.

Precisely, the presence of two different forms of *yod* in immediate succession, on the illustration of the *Graphic*, constitutes a problem. Indeed, a standard *yod* appears immediately to the left of this zigzag *yod*. Yet it is doubtful that an authentic ancient scribe would have penned this letter in two such different ways consecutively. This might be conceivable in an inscription, but hardly in a long, literary work written on a leather document. As a matter of fact, the paleographical charts record a surprising variety of forms of *yod*. If the *Graphic* illustration correctly reflects the original forms of the *yod*, then it further confirms this variability, which is a possible hint that the strips are forgeries.

But it is also possible that the zigzag *yod* in the *Graphic* does not correctly reflect the original. After all, this form is unique: it is not corroborated by any of the other sources of the script of the strips (by contrast, my discussion in section 6 only concerns morphological features of letterforms that are attested by at least two sources). Moreover, it is a fact that the same illustrator failed to correctly reproduce two other letters in the same line: the second *resh* and the first *mem* from the right (see Fig. 7) – unless the strips

themselves bore these forms, which would be a clear hint that they are forgeries. If one wants to explain the latter two anomalous shapes by the difficulty of the task or the badly preserved state of the strips, then one is *ipso facto* admitting that the same could be the reason for the bizarre form of *yod*; in any event, it is hardly certain that this *yod* is a faithful reproduction of the actual form on the strip.

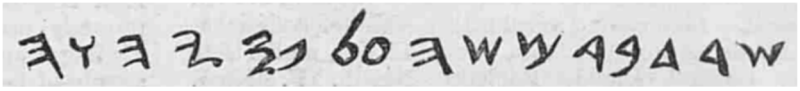


Figure 7[79]

A down-to-earth scenario may be explored: the artist might have tried two different manners of drawing *yod*, one being more successful than the other. After all, it is clear that he or she did vary the ductus to pen the letter *resh* (see the second and fifth letters from the right on Fig. 7). So it is possible that on the column of the strip, the first *yod* from the right was a standard form of *yod*, like the other, and that the illustrator decided to draw the top two horizontals in a single movement. Indeed, if the vertical shaft was slightly curved, the illustrator may have analyzed the upper part of the letter as a rotated U. In fact, the upper part of the other *yod* looks like a U. Afterwards, he continued with the lower part of the letter in the similar fashion.[80] To put it another way, he drew the

[79] Detail from the image available on Wikimedia Commons, where the source indicated is Dershowitz's book: https://en.wikipedia.org/wiki/Shapira_Scroll#/media/File:Dersh7.png.

[80] A possible analogy may appear on one of the (faked) "Hebron Philistine Documents," where there seems to be a *he* drawn like a "3," with two Us (see line one, sixth letter from the left, on the images in J. Naveh, "Some Recently Forged Inscriptions," *BASOR* 247 [1982]: 53-58, esp. 55, Fig. 2, or perhaps better, R. Reed, *Ancient Skins, Parchments and Leathers* [New York: Seminar Press, 1972], 180). Other forms of *he* like a mirrored E, with a rectilinear vertical shaft, appear in the lower part of the document. As J. Naveh demonstrated in the case of the other faked leather document, the forger took his inspiration from the Siloam Tunnel Inscription, copying sequences of it. So it may be a case where a forger, although inspired by an inscription containing only forms of *he* with a rectilinear vertical shaft, spontaneously drew a single-stroke *he* like two Us.

letter like a sort of "3," with a tail. The result differs from the original, notably because the way the two Us are joined cannot reproduce the vertical shaft. But this is not much more surprising than the transformations that the *resh* and *mem* were subjected to in the two problematic instances already noted.

In sum, there are two possibilities: either the zigzag form we see on the *Graphic* illustration reflects the original form on the strip, and this is shifty for several reasons mentioned above, or it is a clumsy reproduction (as in the case of two other letters in the same line). Moreover, all the alleged parallels to the single-stroke *yod* are debated.

6. The script

From the beginnning it has been noted that a number of letters on the strips are "normal," with shapes close to those attested on the Mesha stele: this is accepted by most scholars. So in the framework of this article, I will focus on the other, more unexpected letterforms, admitting for the sake of the argument that the rest of the alphabet is "normal." I will nevertheless include a few "normal" letters to show that some letterforms did not survive the Iron Age, which will play a role in the assessment of the hypothesis, defended by some, that the strips might originate from the time of the Dead Sea scrolls.

Our knowledge of the Paleo-Hebrew tradition has of course made much progress since the late 19th c. So it is not satisfactory to take the list of three letters that Guthe regarded as surprising and try to explain them away, as Shlomo Guil does in his interesting article.[81] According to Guthe, 7 letterforms (*bet*, *dalet*, *waw*, *zayin*, *kap*, *ṣade*, *qop*) do not have a parallel on the Mesha stele and 4 deviate in minor ways (*mem*, *nun*, *pe*, *taw*); he only identified three letterforms that were new to him (*ṭet*, *kaph*, and *qop*).[82] But Guthe was (understandably at his time) not attentive to a number of features that we now know are significant. For the same reason, Guthe's cautious conclusion regarding the script of the strips should not be

[81] Guil, "The Shapira Scroll," 21.
[82] Guthe, *Fragmente einer Lederhandschrift*, 66.

invoked today. He found the strips altogether different from obvious forgeries he knew, even "elegant" compared to them, and he did not take a stance on their authenticity based on the script.[83] But again, his rudimentary paleographical analysis is completely outdated.

Let me also note at this juncture that if we need to go as far as looking at coins from the Hellenistic or Roman period in order to find parallels, as Guthe[84] and Sirat[85] did, this is actually a problem since other shapes found on the strips did not survive the Iron Age (see below). Finding some parallels *only* in the 9th-6th c. BCE and others *only* in 2nd BCE to 1st c. CE would hint at the fact that the strips are forgeries.

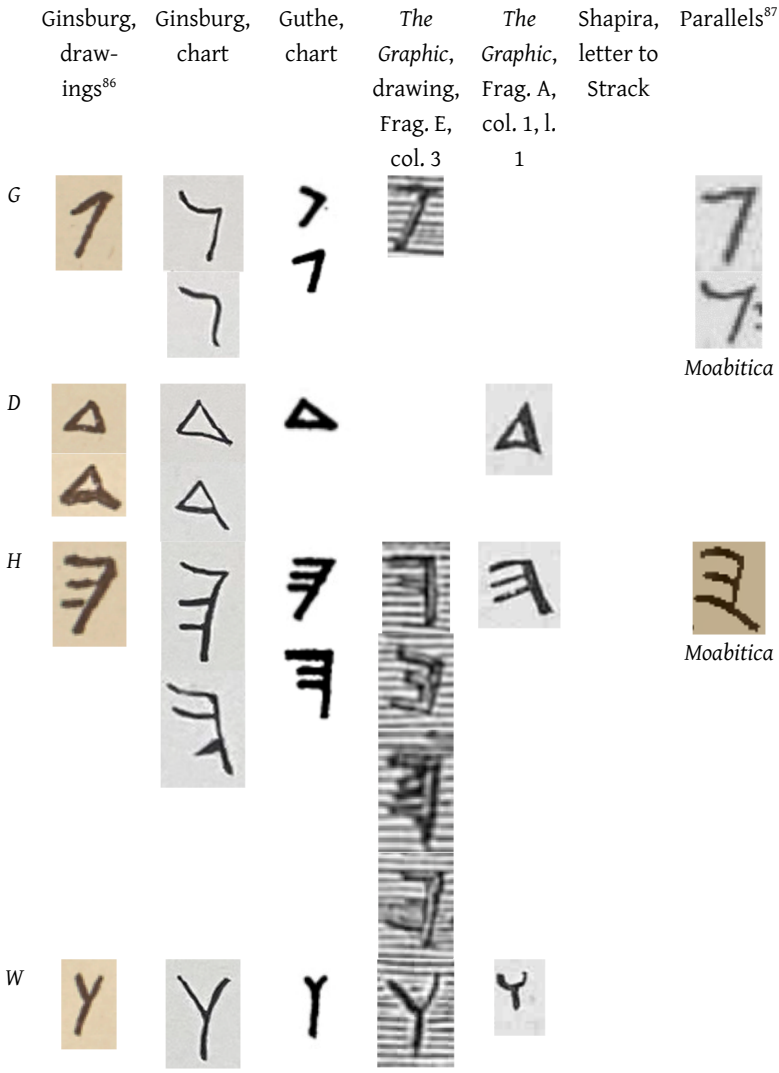
For the sake of the present demonstration, I will adopt a demanding criterion and limit myself to letterforms that are attested by several witnesses (see Table 2), so that it cannot be argued that the possible anomalies are due to errors made by an individual. In the discussion, I also note if the presence of the feature is corroborated by the fact that it explains a confusion between letters. Indeed, as indicated by Dershowitz in the very helfpul footnotes he provides below his transcription, Shapira, Guthe and Ginsburg sometimes mistook a letter for another because they were very similar. In two cases (*zayin* and *kap*), we have further confirmation of an anomalous form because it was first noted by Shapira himself. As a result, the features that I will discuss are attested by two, three or even four independent witnesses. I will show that even sifted with this criterion of multiple attestations, the evidence exhibits a significant number of problems. If I were to analyze all the letterforms visible on the various documents we have, a much greater number of other anomalies would immediately appear.

[83] Ibid., 68.

[84] Guthe found the best parallels to *ʾalep*, *bet*, *gimel*, *dalet* and *yod* on coins (ibid., 66).

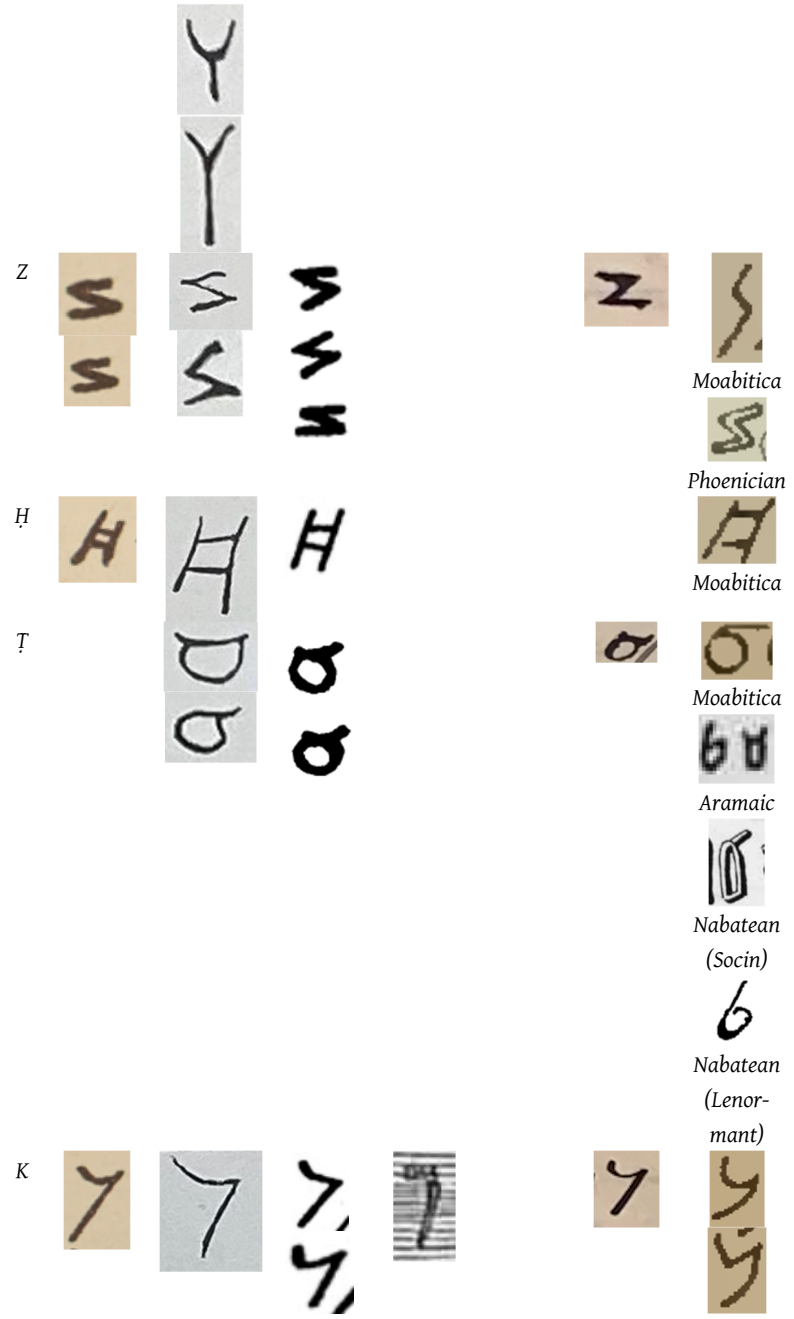
[85] Colette Sirat, "Les Fragments Shapira," *Revue des Études Juives* 1-2 (1984): 104-105.

Table 2



[86] All the images in this column come from what seems to be Ginsburg's "final draft" of Fragment E according to Dershowitz (BL Ms. Add. 41294, 34).

[87] See the notes to the paleographical discussion below for references.

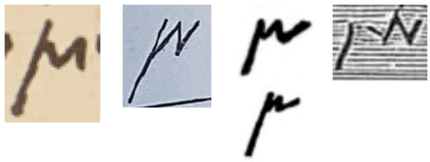

Z
Moabitica
Phoenician
Ḥ
Moabitica
Ṭ
Moabitica
Aramaic
Nabatean (Socin)
Nabatean (Lenormant)
K

Moabitica

Moabitica (Schlott-man)

Moabitica (Koch)

ṣ



Aramaic (Levy)

Phoenician

Moabitica

Q

Moabitica

Based on these multiple attestations, here are some remarks regarding the stance and the morphology of letters, as well as the presence of ligatures.[88] First, regarding the stance:

Gimel and *ḥet* lean to the right, whereas normally in Old Hebrew, they lean to the left or are vertical. Note that the stance of *gimel* on the strips is corroborated by the fact that this letter was sometimes mistaken for a *pe*.[89] As already noted, the stance of *he* is variable, it leans sometimes to the left, sometimes to the right. The normal stance of *ḥet* in the Iron Age is slanting to the left, although it is sometimes vertical; occasionally, it leans very slightly to the right, but not as much as on the strips, where this feature is striking. Re-

[88] For paleographical tables of the Paleo-Hebrew script, see J. Renz and W. Röllig, *Handbuch der althebraïschen Epigraphik*, 3 vols. (Darmstadt: Wissenschaftliche Buchgesellschaft, [2]2016), esp. Band II/2, 103-208 (letter by letter) and Band III, Tafeln 1-37 (in chronological order). See also C. A. Rollston, "Northwest Semitic Cursive Scripts of Iron II," in *"An Eye for Form:" Epigraphic Essays in Honor of Frank Moore Cross*, edited by J. A. Hackett and W. E. Aufrecht (Winona Lake: Eisenbrauns 2014), 202-234. For paleographical tables of the Moabitica, see K. Schlottmann, "Neue Moabitische Funde und Räthsel: Erster Bericht," *ZDMG* 26 (1872): 393–407, plate 1 on the page before p. 393; A. Koch, *Moabitisch oder Selimisch? Die Frage der moabitischen Alterthümer neu untersucht* (Stuttgart: Schweitzerbach, 1876), plates at the end of the book; Kautzsch and Socin, *Die Aechtheit der Moabitischen Alterthümer Geprüft*, Tafel 1. For paleographical tables of the Dead Sea scrolls written in Paleo-Hebrew, see http://www.paleohebrewdss.com, and for a discussion see A. Perrot and M. Richelle, "The Dead Sea Scrolls Paleo-Hebrew Script: Its Roots in Hebrew Scribal Tradition," in *The Hebrew Bible Manuscripts: A Millenium*, edited by E. Attia and A. Perrot (Textual History of the Bible Supplements; Leiden: Brill, forthcoming).

[89] Dershowitz notes that "in the script of these mss., *gimel* and *pe* were similar" (ibid., 136 n. 45).

garding *he* and *ḥet*, the wrong stance already occurs on some Moabitica.[90] For *gimel*, this stance does not appear on genuine documents before the Hellenistic period (see notably 4Q123, 11Q1).[91] It also appears on some Moabitica.[92]

Regarding the morphology:

Dalet is generally a simple triangle, although in some instances it has a small leg. The triangle without leg is the earliest form of the letter, attested on the Mesha stele, and still present on inscriptions from the 8th c. The leg is already attested in the Samaria ostraca (early 8th c.) and is a regular feature afterwards. The form without leg does not survive the Iron Age and is entirely absent from the Dead Sea Scrolls.

Waw is penned in several ways: as a lowercase y, or as a vertical shaft with a head shaped like a V or like a cup. These are early forms that disappear after the 8th c. (and are notably totally absent from the DSS).

Zayin has the shape of an *oblique* and *mirrored* Z (like a thunderbolt), with no parallel in the history of Paleo-Hebrew. Only Shapira reproduces a Z-shaped *zayin*, a form that sometimes occurs during the Iron Age; it is possible that both forms occurred on the strips. Good parallels of the "thunderbolt" form, however, occur twice on

[90] See Kautzsch and Socin's chart.

[91] At first, it seems to be also attested on two Iron Age inscriptions, but they are the exceptions that prove the rule. The first is an abecedary from Lachish stratum III, evidently inscribed by a person learning to write and liable to make mistakes, as in the case of the stance of *gimel* (see Renz, *Handbuch der althebraïschen Epigraphik*, Band I, 74-75, Taf. VI). The second is a 7th c. BCE ostracon from Tel 'Ira (the "*mpqd* ostracon"): as Ada Yardeni notes, "the bold script written in uneven lines seems to be the work of a non-professional, unskilled scribe, who, although being familiar with the formal cursive Hebrew script, apparently did not have much practice in writing" (*The National Hebrew Script up to the Babylonian Exile* [Jerusalem: Carta, 2018], 160). Yet another possible instance appears on a Gibeon jar handle (Renz, *Handbuch der althebraïschen Epigraphik*, Band I, 261, Taf. XXVIII; assuming the drawing is accurate), but this may due to the difficulty of writing on such a medium, and the other jar handle inscriptions show that the normal stance of *gimel* is leaning to the left.

[92] This is so true that that *kap* and *gimel* are recorded in the same line in Koch's Tafel V.

the drawing of a Phoenician seal published by Levy in 1869,[93] and on some Moabitica.[94] Mirrored letterforms are quite frequent in the Moabitica.

Ḥet has two crossbars, which is an attested form in the Iron Age; note, however, that this feature is absent from the DSS. With regard to the stance, see above.

Ṭet is a circle with one or two ticks (in the latter case, it looks a bit like a knot), both completely unparalleled in the history of the Paleo-Hebrew script. Both forms are attested in the column illustrating the Aramaic script in a letterforms chart published by Levy in 1867[95]. Parallels to the form with one tick are found in Nabatean letterforms published years before the appearance of the strips,[96] and on some Moabitica.[97]

Kap: the head is penned with only one stroke departing from the shaft, although sometimes it turns upwards, so much so, in some cases, that there seems to be a second stroke departing upwards from its left end. Note that the anomalous *kap* on the strips is corroborated by the fact that this letter was sometimes mistaken for

[93] Levy, *Siegel und Gemmen* Taf. II, no. 2.

[94] See Kautzsch and Socin's chart.

[95] M. A. Levy, "Beiträge zur aramäischen Münzkunde Eran's und zur Kunde der älteren Pehlewi-Schrift," *ZDMG* 21 (1867): 421-65, see Taf. III.

[96] F. Lenormant, *Essai sur la propagation de l'alphabet phénicien dans l'ancien monde*, vol. 2 (Paris : Maisonneuve, 1872), Pl. XV ("alphabet nabatéen"); Socin, "Über Inschriftenfälschungen," Figure No. II printed before the article. For a modern chart with the same letterform, see A. Yardeni, *Textbook of Hebrew, Aramaic and Nabatean Documentary Texts from the Judean Desert and Related Material*, B: *Translation, Paleography, Concordance* (Jerusalem: The Hebrew University, 2000), Appendix: The Nabatean Script, 231, chart A (CIS 805). This appendix is available on Yardeni's academia.edu page.

[97] See Kautzsch and Socin's chart, under the rubric "phantasia." This letterform is so unexpected in Hebrew documents that these scholars were not able to identify it as a *ṭet*, which is also why the line devoted to *ṭet* in their chart is void.

gimel[98] and *pe*.[99] Shapira also confirms it in his letter to Strack.[100] Guil suggested a possible parallel on 11QpaleoLev,[101] but this is inaccurate; 1Q3 and 4Q12 would be closer as regards the head, but on them the head of the letter resembles that of a *nun*, with a shoulder – an extension of the shaft above the meeting point with the head.[102] Relatively close parallels of *kap* occur on some Hasmonean coins (although generally there are differences in the stance, the shape of the vertical shaft, or the shape of the head).[103] The best parallels occur on some Moabitica.[104]

[98] Dershowitz notes "*gimel* and *kaph* being quite similar" (*The Valediction of Moses*, 157 n. 53).

[99] Dershowitz notes that "*pe* and *kaph* are similar in the scripts of the V mss" (ibid., 152 n. 201).

[100] See the transcription of this letter by Ross Nichols on his blog: https://themosesscroll.com/moses-shapiras-9-may-1883-letter-to-hermann-strack/.

[101] Guil, "The Shapira Scroll," 21.

[102] The paleographical chart of the Horvat 'Uza inscriptions published in the excavations volume (I. Beit-Arieh, "Epigraphic Finds", in I. Beit-Arieh [ed.], *Ḥorvat ʿUza and Ḥorvat Radum: Two Fortresses in the Biblical Negev* [Monograph series of the Institute of Archaeology, 25; Tel Aviv: The Emery and Claire Yass Publications in Archaeology, 2007], 182, Table 41), might give the impression that a few instances of *kap* on ostraca No 22, 28 and 34 are more or less similar to the letterform on the Shapira strips. But I checked photographs of these ostraca and it turns out that the drawings of these letters in the chart are inaccurate; *kap* has normal shapes on these inscriptions. Note that on inscription No 34, line 12, the tail of the next letter (a *yod*) touches the head of the *kap*. Nevertheless, a close examination of a good photograph indicates that the head of *kap* has two teeth: the first departs perpendicularly from the vertical shaft, the second begins at the end of the first downwards. This is a normal shape for a *kap* of the cursive script, which we encounter also on inscription No 22. On inscription No 28, line 1 – according to Beit-Arieh's numerotation; in fact, there are traces of two letters of a preceding line – the head of *kap* is badly preserved, but on close examination and with enhancement of contrast, it turns out that it is a normal shape and the drawing is inaccurate.

[103] Meshorer, *Ancient Jewish Coinage*, vol. 1: *Persian Period through Hasmoneans* (New York: Amphora Books, 1982), 170.

[104] Schlottmann, "Neue Moabitische Funde," p. 411 (see also plate 1 on the page before p. 393). See also Levy, "Beiträge," Taf. III.

Ṣade has two surprising features. First, the downstroke is long, at least two, sometimes three times longer than the head; normally, the downstroke is much shorter than that in Old Hebrew.[105] Second, the head consists of a zigzag that departs obliquely from the vertical, sometimes making an initial angle of less than 45° with it, whereas normally it looks like a staircase that departs from the downstroke perpendicularly or nearly perpendicularly. Good parallels are found in Phoenician[106] and Aramaic[107] paleographical charts published years before the strips surfaced. This *ṣade* also resembles that of some Moabitica.[108]

Qop's head is a full and perfect circle, with a vertical shaft that sometimes intersects it, but never reaches its top; to the best of my knowledge, this has no exact parallel in the history of the Paleo-Hebrew script. On inscriptions 3.12-14 from Kuntillet ʿAjrud (abecedaries), the head is an ovoid, not a circle. On the Mesha stele, the head is normally not a circle, it is a "flattened" circle, and quite often its lower part is completely flat.[109] On a jar inscription from Tel Ḥazor dated to the 8th c. BCE, the head of *qop* is made of a semicircle drawn above a horizontal, straight line;[110] this confirms that the shape already attested on the Mesha stele is not an "accidentally" flattened circle but corresponds to a letterform in its own right. In a jar incision from Tel Reḥov stratum IV (9th c. BCE), we

[105] Possible exceptions (relatively long downstroke) seem to appear in inscriptions 3.11 and 3.13 on pithos B from Kuntillet ʿAjrud, but in reality, all the downstrokes penned by this scribe are very long, so this is an idiosyncracy.

[106] P. Schröder, *Die Phönizische Sprache: Entwurf einer Grammatik nebst Sprach- und Schriftproben* (Halle: Buchhandlung des Waisenhauses, 1869), Taf. B (before p. 77).

[107] M. A. Levy, *Geschichte der jüdischen Münzen* (Breslau: Schletter's Buchhandlung, 1862), 136, col. 1.

[108] See Kautzsch and Socin's chart.

[109] On a table in Renz and Röllig, *Handbuch der althebraïschen Epigraphik*, Band II/2, p. 193, on the line "j", the first *qop* (to the left) resembles the *qop* of the Shapira strips, but on close examination this form does not appear on Renz's detailed paleographical tables (letter by letter) in Band III. On the squeeze, the head of the *qop* in the middle of line 15 looks like a perfect circle, but on close examination it is not clear, and what is preserved of the head on the stone makes it uncertain.

[110] Yardeni, *The National Hebrew Script*, 101.

can see a *qop* where the vertical shaft extends above its intersection with the head but does not reach the top of the head (as in some of Ginsburg's drawings); however, the lower part of the same head is flattened.[111] Parallels of the *qop* of the Shapira strips with the vertical shaft not piercing the head are attested on Moabitica.[112] Parallels to both forms of *qop* appear on one of the *forged* "Hebron Philistine documents" that surfaced in 1970;[113] it is easy to see how a forger could spontaneously produce such problematic variants that only a careful paleographical inspection can spot.

Finally, ligatures between letters are recorded in Guthe's and in Ginsburg's charts (see Fig. 1 and 2). This is an anomaly in Old Hebrew. We are not just talking about letters that touch each other due to their proximity – which happens in Old Hebrew. In Ginsburg's chart, the downstrokes of the connected letters are *bent* in order to be joined, which is abnormal. Exactly the same kind of ligatures are abundantly attested in the Moabitica.[114] This feature may originally have been inspired by the Nabatean script, like, I suspect, a number of others (e.g. the form of *ṭet* discussed above).

The most significant conclusions to draw are the following:

- The stance of two letters (*he* and *ḥet*) is problematic, whatever the period considered.
- The shapes of four letters (*zayin*, *ṭet*, *qop*, *ṣade*) do not fit any period in the Paleo-Hebrew script; the only possible parallels to the shape of a fifth one (*kap*) appear on some coins from the Hasmonean period. Some of these letterforms have parallels in other scripts (Aramaic, Nabatean, Phoenician); all are attested on the Moabitica, and one (*qop*) is attested on the Moabitica only.[115] The mixture of letterforms from different scripts is typical of fakes, and is a clear proof that the strips were forged. But

[111] Aḥituv and A. Mazar, "The Inscriptions from Tel Reḥov," 44-45, fig. 7.

[112] See Kautzsch and Socin's chart.

[113] Rollston, "Non-Provenanced Epigraphs I," 142-45; see Fig. 2.

[114] See e.g. Koch, *Moabitisch oder Selimisch?*, Taf. I, tabelle II.

[115] Which confirms the general remarks made by Rollston, "Deja Vu all over Again."

it seems likely that the script of the strips was either inspired from the Moabitica, or made by the same circle of forgers.
- Some features are attested in the Iron Age only (*dalet* without leg, *ḥet* with two crossbars, early forms of *waw*), sometimes not even after the 8th c. (cf. *waw*). If one thinks, as argued in VM, that *yod* sometimes has a cursive reflex, then it is also worth noting that this phenomenon disappears from the documentation after the 8th c. In such circumstances, it is highly unlikely that the strips were written in the late pre-Exilic period or later. The script is vastly different from that of the DSS: as regards the paleography, the Shapira strips cannot be regarded as "yet another Dead Sea scroll." On the other hand, two striking features (the stance of *gimel* and the shape of *kap*) are not attested before the Hellenistic period. So we have a mixture of features from various periods, which, too, is typical of forgeries.
- The presence of ligatures is a complete anomaly in Old Hebrew script. It was inspired either by the Nabatean script directly, or by the Moabitica, or due to the forgers of the Moabitica.

Admittedly, some surprising letterforms do occur on inscriptions. This happens in the following cases: (a) when the person who wrote the text is not a proficient scribe (as in the Ataruz inscription, but not on such a scale in manuscripts of books); (b) when we are discovering, for the first time, a local script which deliberately deviates from others, perhaps because it was designed to be the script of a kingdom (this may have been the case of the Tell Siran bottle); (c) when the inscription is made by an engraver, not a scribe (in some monumental inscriptions); (d) when the medium on which the text is written is difficult to incise or engrave: it provokes some deviations of the tool. None of these conditions apply here. Moreover, there is an *accumulation* of eccentricities, worse, of eccentricities already found in the Moabitica.

Conclusion

The script of the Shapira strips exhibits an accumulation of anomalies that are indicative of forgeries (anomalous stances, letterforms unparalleled in Old Hebrew script but reflecting a mixture of letterforms from other alphabets or from the Moabitica, mixture of letterforms from different periods, ligatures). One could imagine that the strips were forged by someone who used a variety of script sources as models: the Moabite Stone, Nabatean inscriptions, Phoenician inscriptions, and so on (as was done before for the Moabitica). But there is a convergence of factors indicating that they were simply made by the same person(s) who forged the Moabitica. Which echoes the circumstances of their appearance: they were offered for sale by the same man who had sold the Moabitica.

Of course, it may be argued that no absolute certainty can be reached about these documents, pending the discovery of at least one strip in some attic in Britain or Germany. This is a purely theoretical point: one could also say that no absolute proof exists of the fact that the Book of Isaiah or the Pentateuch are the result of successive redactions spread over centuries, since we do not have copies of Deutero-Isaiah or, say, P. And yet the evidence is sufficiently compelling to convince scholars in these cases. Epistomologically, the notion of proof is not the same in philology as in mathematics. The question is whether scholars have enough evidence to reach conclusions. In the present case, as far as the script is concerned, the evidence is overwhelming and the situation is clear: the strips are forgeries. At the end of the day, the main question is whether or not we should add the Shapira strips to the list of genuine documents that must be taken into account in historical and biblical studies. To this question, the answer is a "no." It is actually a shame, since it would have been exciting to have the text of a proto-Deuteronomy. But we will always have Coltrane and Proust...

Appendix: On the reliability of Ginsburg's drawings

As already noted, Ginsburg's drawings are not necessary to the paleographical analysis since we have a paleographical chart made by Ginsburg. However, it is worth discussing the reliability of the drawings, be it only to set the historical record straight. The arguments made by Dershowitz and Sass against them are the following:

a. Ginsburg's drawings are not facsimiles (in the strict sense of the word, that is, exact copies) so they are unreliable "by default"; they are comparable to transliterations (Sass).
b. Ginsburg was not a reliable maker of facsimiles, witness his facsimile of the Moabite stone (Dershowitz).
c. Ginsburg's drawings prove unreliable because they are mutually contradictory (Dershowitz).
d. There are differences in the letterforms between Ginsburg's drawings and Guthe's chart (Dershowitz).

I will address these arguments successively.

Drawings or facsimiles?

If by "facsimile" one understands "exact copy," (whereas the use of this word in the 19th c. was more flexible), then I agree that it is preferable to talk about "drawings." But the first argument seems partly based on an *a priori* notion about the degree to which Ginsburg's drawings were intended to reflect the exact appearance of the strips. Yet the only information provided by Ginsburg is that the drawings were actually made by a certain Miss Tennant and by W. St. Boscawen (an assyriologist);[116] both had "large practice in copying Assyrian inscriptions" and worked under Ginsburg's "strict supervision."[117] It is even conceivable that the drawings

[116] https://www.cambridge.org/core/journals/iraq/article/abs/assyriology-at-the-margins-the-case-of-william-st-chad-boscawen-18551913-1/C7003DA7106A721ECA55057938048118

[117] Ginsburg, "The Shapira Ms. of Deuteronomy," 304.

were not intended to be perfect reproductions of the strips, but were nonetheless made with a decent degree of care, so that they correctly reflect their script. Another imaginable scenario is that Ginsburg gave a letterforms chart as a model to his assistants so they could write down in Paleo-Hebrew the text that he had transcribed from the strips beforehand, perhaps in square Hebrew,[118] or some equivalent scenario. So it is possible, as Sass argues, that the drawings are akin to transliterations made with a sort of "font." Still, in that case, the drawings nonetheless give a helpful general idea of what the script looked like: they are reproductions of the letterforms contained in the paleographical chart, that is, "approximations" of this chart.

The downside, if this scenario is correct, is that it is not possible to be sure that the form of a given occurrence of a letter on a precise line of a precise column is reflected with precision in the corresponding drawing, because the script would have been normalized, thereby probably reducing its diversity and erasing some local peculiarities. While this would be a shame, it would, however, only underscore the fact that the drawings are a reflection of the letterforms chart, not a fantasy designed to give the general public a vague idea of the appearance of the strips. Yet another possibility is that the drawers did not use a chart as a "font," but nonetheless normalized the script by spontaneously having recourse to the same default letterforms, perhaps the most frequent or those they knew or liked the best.

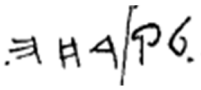
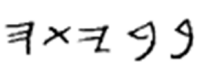
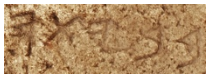
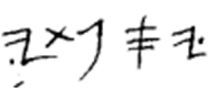
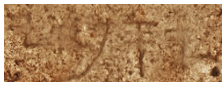
As a matter of fact, a close comparison between the drawings and Ginsburg's chart reveals that generally speaking, the letterforms one sees on the drawings, far from being airy-fairy, also appear in the chart. So while it is possible that they simplify the script by reducing its actual diversity, the drawings nonetheless exhibit actual letterforms from the strips. And what matters for the paleographical study of a document is the letterforms, *regardless of their location on the document*.

[118] I thank Matthew Hamilton for drawing my attention to this possibility (personal communication); this does not necessarily represent his view but it is a question he raised.

Is Ginsburg's facsimile of the Moabite stone unreliable?

The second argument is based on mistakes contained in Ginsburg's facsimile of the Moabite stone.[119] Dershowitz's article compares three sequences of letters in this facsimile with the squeeze of the latter kept in the Louvre museum (see Table 3).

Table 3

	Ginsburg's facsimile	Louvre squeeze[120]
sequence LQRḤH in line 25		
sequence BBYTH in line 25		
sequence YSPTY in line 29		

His article correctly points out the following mistakes:
(1) in the sequence BBYTH in line 25, the downstroke of B is rounded whereas it should be more angular;
(2) in the sequence YSPTY in line 29, the vertical shaft of S should not extend above the top horizontal stroke;
(3) in the same place, the stance of S is incorrect, the letter should lean to the left.

In addition, it is apparent that:
(4) in the sequence LQRḤH in line 25, the head of Q should be symmetrical.

[119] Ch. D. Ginsburg, *The Moabite Stone: A Fac-simile of the Original Inscription, with an English Translation and a Historical and Philological Commentary* (London: Reeves and Turner, ²1871).

[120] Images courtesy of the Louvre museum.

However, we shall see that only (1) can ascribed to Ginsburg. Note that since the downstroke of B is sometimes rounded on the Moabite stone, the lack of accuracy here is not something that would impact our knowledge of its script in a significant way. The situation (4) is more interesting. If we check the stone (see Fig. 8), it turns out that this part of the stele is broken, the head of Q is damaged, and the next letter is a modern reconstruction made on plaster.

Figure 8[121]

So to sketch this sequence of letters, Ginsburg would have needed to examine the Louvre squeeze. But did he have access to it? In fact, what material did he use for his facsimile?[122] In the introduction to his book, Ginsburg draws a list of the sources that were used to reconstruct the text (interestingly, he uses the passive voice here) but he indicates that he did not have access to them, be it only to check the arrangement of the fragments of the stone.[123] He complains that Clermont-Ganneau did not publish the fragments or the squeeze.[124] The only material Ginsburg had were photographs of

[121] Image courtesy of the Louvre museum.

[122] Ginsburg's facsimile was published in a book in 1871, he even wrote the preface of the 2nd edition on May 1, 1871. The stone was discovered in 1868; the main fragments were acquired in 1869 by Clermont-Ganneau who published his first extensive reconstitution of the text in 1870, including facsimiles, and materially reconstructed the stone in 1875.

[123] Ginsburg, *The Moabite Stone*, 14.

[124] Ibid., 14; see also the preface, 3. Even after the publication of Ginsburg's book, when Ginsburg visited Clermont-Ganneau in Jerusalem, the French diplomat, always the gentleman, refused to let him examine them (Tigay, *The Lost Book of Moses*, 100).

Captain Warren's drawings of the two main fragments.[125] So how could Ginsburg make a facsimile of the entire stele?

A comparison with Clermont-Ganneau's facsimile, published in 1870,[126] reveals that it is virtually identical to Ginsburg's. The arrangement of the fragments, their readings, and the lacunae are exactly the same (see Fig. 9).[127] Tellingly, while his commentary abounds in philological and historical remarks, it does not contain a single epigraphical discussion. In his facsimile, Ginsburg only changed the form of the lower part of the stone, since he believed it was rounded. Crucially, the two mistakes noted by Dershowitz on S in line 29, and the problem of the head of Q in line 25, *are there* (see Table 4). These shared – inherited – errors confirm what is already manifest when we compare the images: Ginsburg's fac-simile is not a fac-simile of the Mesha stele, but of Clermont-Ganneau's fac-simile! Unfortunately, he does not state it explicitly in his book, which is all the more misleading as the facsimile is the first thing mentioned in its subtitle.

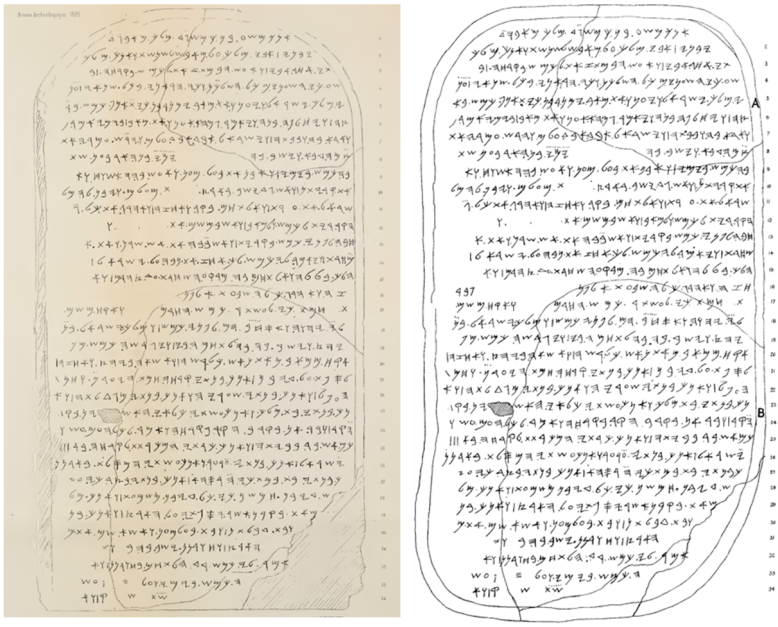
As a result, if we want to see if Ginsburg was careful when copying a text, we should compare his facsimile with Clermont-Ganneau's, not the squeeze. And it turns out that while Ginsburg sometimes made mistakes, they are relatively rare; overall, with regard to the morphological information relevant to paleography, and the stance of the letters, he is reliable. Based solely on his facsimile, we would have an excellent idea of what his model, that is, Clermont-Ganneau's facsimile, looked like.[128]

[125] Ibid., 32.

[126] Charles Clermont-Ganneau, *La stèle de Dhiban ou stèle de Mesa, roi de Moab, 896 avant J.-C. : Lettres à M. Le Comte de Vogué* (Paris : Baudry/Didier, 1870). The facsimile appears after page 4. This book, as well as Ginsburg's book, is available on archive.org.

[127] The only exception is that Ginsburg added one letter at the beginning of line 16, and three others at the end of the same line, but this is the exception that proves the rule: he did so not on the basis of his own examination but, according to his commentary of the text, because Clermont-Ganneau had said that he was now able to read these letters (Ginsburg, *The Moabite Stone*, 43).

[128] In this light, the claim that "his letterforms are written with little regard for the original characters' relative size, position, stance, shape, and ductus" (Dershowitz, *The Valediction of Moses*, 14) cannot be maintained. It was already an ex-



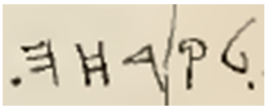
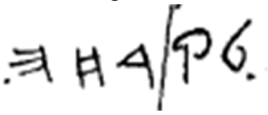
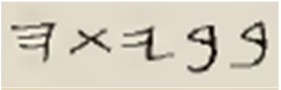
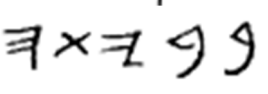
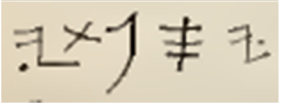
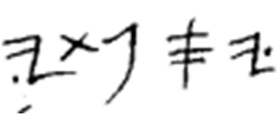
Clermont-Ganneau *Ginsburg*

Figure 9

aggeration to infer from his facsimile that "Ginsburg's drawings are utterly undependable for paleographic purposes" (ibid., 13); it is now shown to be completely inaccurate. We also read in Dershowitz's book: "In a world in which the unquestionably genuine Mesha Stele were lost and only the scholarly "facsimiles" remained – including that of Ginsburg himself – paleographers using the very same reasoning would condemn the Mesha Stele as a forgery. And they would be wrong." (ibid., 33)

This claim is incorrect. It would be correct if paleographers did not distinguish between any small deviation in the execution of the script and a deviation that would affect significant features of the characters. Or if the problems observed corresponded to typological developments that took place centuries after the supposed date of the inscription, thus betraying anachronisms. Yes, it is regrettable that Clermont-Ganneau has the downstroke of *samek* extending above the top horizontal, but no, this is not something that would lead paleographers who only had his facsimile to declare that the Mesha stele is a forgery. Yes, it is regrettable that the head of Q is asymmetric in the same facsimile, but no, it would not lead paleographers to regard the inscription as a fake.

Table 4

	Clermont-Ganneau's facsimile	Ginsburg's facsimile
Line 25		
Line 25		
Line 29		

What about the contradictions between Ginsburg's drawings?

This leads me to the third argument made against the reliability of Ginsburg's drawings. VM illustrates it by noting a problem regarding the place of some letters in a drawing of column 1 of Fragment E: a sequence of letters appears at the end of line 2 on this drawing, but at the beginning of line 3 on another drawing.[129] Dershowitz notes that there are a number of similar problems. At first sight, this gives the impression of a spectacular lack of care. But a more precise assessment of what happened seems possible.

At this juncture, in the article and in the book, Dershowitz refers to the first drawing, in passing, as "an early draft"; in a footnote that appears much later in his book, he refers to the second drawing as "what appears to be the final draft of Ginsburg's drawing," adding that "the earlier drafts are inconsistent with the final one and with Guthe and appear to be imprecise with regard to layout."[130] It turns out that his own Paleo-Hebrew reconstruction of column 1 of Fragment E is *virtually identical, in terms of layout as well as of readings* (98,7% of agreement), to Ginsburg's *final* drawing.[131] It

[129] Dershowitz, "The Valediction of Moses," 12.

[130] Dershowitz, *The Valediction of Moses*, 140 fn 89.

[131] Ibid., 169. The differences between Ginsburg's drawing and Dershowitz's reconstruction are the following:

is regrettable that he omits to tell the reader this. Instead, he writes: "we have seen that Ginsburg's drawings are unreliable and mutually contradictory,"[132] which seems to me misleading since he was comparing an earlier draft to a final drawing and since his own analysis indicates that the latter is pretty much reliable.

A comparison between the successive drawings of the same column shows that several sequences of words missing from the early draft were added in the final drawing. Yet the presence of lacunae is not indicated in the early draft. This suggests that Ginsburg's assistants initially wrote down, consecutively, all the letters that Ginsburg had been able to decipher so far. A corollary of this procedure is that the layout was wrong. Yet on the other hand, this problem is entirely absent from the *final* drawing. Obviously, what happened in the meantime is that Ginsburg had been able to fill in the gaps in the decipherment. But this time, care was also taken to faithfully reproduce the layout of the column.[133]

• two letters are read differently in line 6: Ginsburg's drawing has WHT where Dershowitz reconstructs MPT; this corresponds to two letters that Dershowitz regards as uncertain (with a superscript circlet and extensive discussion in a footnote);

• Ginsburg was not able to read the first letter in lines 1 and 9 (his drawing has hatching to indicate it);

• In line 6, between 'TM and L'T, Ginsburg uses hatching to mark the presence of a lacuna (or a couple of illegible letters); this is not represented in VM, but of course it is not possible today to examine the strips;

• At the end of line 10, Dershowitz adds 'LTM between brackets: this is a restitution and not a reading.

So out of 298 letters in this column (according to Dershowitz's reconstruction), there are only 4 different readings (agreement of 98,68%), 2 of which are due to letters that Ginsburg marked as illegible; there is no difference in the layout except that Ginsburg marks the possible presence of a brief lacuna in line 6. The differences are therefore marginal and some of them point rather to Ginsburg's precision in marking uncertain aspects, at least in this drawing.

[132] Dershowitz, "The Valediction of Moses," 31.

[133] Interestingly, in a lithograph containing reproductions of both drawings, someone – presumably Ginsburg – wrote to the side of the early draft: "Text to be the same as below," i.e., as in the final draft. He also made corrections to the readings of the final drawing. This lithograph can be seen at https://en.wikipedia.org/wiki/Shapira_Scroll#/media/File:Dersh4.png.

Whatever the precise history of the successive drafts, the criticism about the place of the letters concerns an aspect of the drawings that is relevant for scholars who want to *reconstruct the layout of the text*, as Dershowitz does in his book, it does not concern *the paleography*. Regarding the script, precisely, Dershowitz states that Lemaire based his paleographical analysis on the early draft of column 1 of Fragment E that I just mentioned. In fact, the latter drawing was used in *BAR* as an illustration to Lemaire's article, which does not mean that his analysis was based on this drawing only; Lemaire actually consulted the whole BL dossier.[134] More importantly, Dershowitz's remark would be pertinent only if the successive drawings were significantly different in terms of the script, that is, with regard to the morphological features of the letter-forms and the stance. At least in the case of the two successive drafts already mentioned, this is not the case.[135]

VM also points out differences between these drawings, taking a few letters as examples, but here again we should remember that Dershowitz is comparing *successive drafts*, and improvements in the decipherment and the readings are to be expected in the later drawing.[136] Such differences may impress neophytes, but epigraphers are used to see such differences on drawings of inscriptions that are difficult to read, for instance ostraca. The ink is often faded, and it is difficult to discern what is ink and what is just a dark trace left by some dirt or abrasion on the surface of the document. It is often hard to decide where a given stroke exactly begins

[134] Personal communication. In the article, Lemaire refers to the dossier of the British library and adds in brackets: "One of Ginsburg's drawings appears below" ("Paleography's Verdict: They're Fakes!"; https://www.baslibrary.org/biblical-archaeology-review/23/3/13).

[135] The sole interesting difference that I can see is that the form of *zayin* is problematic in the final draft (see line 7 and see my paleographical remarks) whereas it is relatively normal in the earlier draft, so perhaps initially one of Ginsburg's assistants "harmonized" it towards the expected form, and later, on closer examination, he or she (or Ginsburg) realized that the actual form on the strip was different. Which would mean that the script of the actual column was even more problematic than appears on the drawing published in *BAR*, and that care was taken to reflect the precise form of the letters in the final drawing. That said, it is also possible that the normalization happened the other way around.

[136] Dershowitz, "The Valediction of Moses," 12.

or ends. In the case of the Shapira strips, the letters on the dark leather were generally visible only when some alcohol was spilled on the manuscript, and only for a brief moment (!)[137] Alternatively, the differences between successive drafts may be due to normalization.

What about the differences between Ginsburg's drawings and Guthe's chart?

Finally, VM points out some minor differences between the drawings of Fragment E, and the column of Guthe's chart based on the same fragment.[138] The letter *waw* is consistently drawn as a lowercase y on the drawings, but as a small vertical line with a small v at its top in the chart. The shaft of *qop* pierces its head on the drawings, not in the chart. These observations are correct but their interpretation is debatable. It overlooks the fact that all of these variants are attested in Ginsburg's chart: it records two instances where the shaft of *qop* pierces its head, and one instance where it is not the case. It also records at least three different ways of penning the letter *waw*, some of them occurring in the same column.

So Ginsburg's assistant who drew Fragment E did not invent a form of *qop* and *waw* out of the blue: he or she simply used one of the forms that was attested on the strips, either on other fragments or a few times on Fragment E, if Guthe's chart is not exhaustive on this point, and he or she used it for all the forms on this fragment, thereby normalizing the letterforms, whether or not he or she used a chart as a model. In this case, while it is certainly a shame that the drawings do not give us access to the exact shape of *waw* and *qop* in every occurrence, it is nevertheless worth noting that they present to us actual letterforms that are attested in the

[137] Guthe, *Fragmente einer Lederhandschrift*, 21. This may also account for some minor divergences between Ginsburg's drawings and Guthe's letterforms chart noted in Dershowitz, "The Valediction of Moses," 13.

[138] Dershowitz, "The Valediction of Moses," 13.

strips, albeit not at the right place. This means that these letter-forms are relevant for paleographical analysis.[139]

To conclude, it is admittedly possible that the drawings simplify the picture due to normalization, and they are certainly far from perfect, but all in all, they (at least generally) exhibit actual letter-forms from the strips.

[139] Note also that there may have been different hands at work behind some fragments, and behind the two main copies of V (Dershowitz, *The Valediction of Moses*, 134 n. 21).