

Corrections to Hythem Sidky's review of *Corrections in Early Qur'ān Manuscripts: Twenty Examples*

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Hythem Sidky, a post-doctoral fellow in Molecular Engineering at the University of Chicago, has written a surprisingly lengthy review article (*Al-Uṣūr al-Wuṣṭā* 27 (2019): 273-288) of my small book, *Corrections in Early Qur'ān Manuscripts: Twenty Examples* (hereafter “CEQM” or “*Corrections*”). It was indeed remarkable that Sidky and the journal devoted a whole fifteen pages to *Corrections*, as my book is not an academic contribution, but simply a brief popular-level introduction to the mere existence of physical corrections in the earliest Quran manuscripts. I also found it interesting that a journal would receive and advance to publication a book review that failed (among other things) in its own title to correctly state the name of the book reviewed.¹ The article is rather vehement.² At first I wondered whether to respond at all, because, despite some reasonable criticisms in this review, all too often I could recognize as my own neither the arguments Sidky criticizes, nor the presuppositions he relies upon. Moreover, Sidky's assertions are at times uninformed, revealing a lack of familiarity with what is a highly specialized field.

However, it seems any discussion of variations in the text of the Quran must be for some a reactive topic, for certain zealous polemicists with large social media followings have been

¹ Sidky's review is titled “Daniel Alan Brubaker, *Corrections in Early Qur'ānic Manuscripts: Twenty Examples* (Lovettsville: Think and Tell Press, 2019), xxv + 102 pp. ISBN 978-1-949123-03-6. Price: \$35 (paper).” However, the title of my book is *Corrections in Early Qur'ān Manuscripts: Twenty Examples*.

² My book, he states, “suffers from a number of critical flaws in methodology, analysis, and discussion” (273), I go to “lengths [...] in order to avoid suggesting scribal error as an explanation” (281), I “insist” that “every deviation from the standard *rasm* encountered in a manuscript is a deliberate one” (ibid.), I reach a “frustrating conclusion” (285), etc., etc.

loudly proclaiming Sidky's review to be conclusive evidence of my incompetence. As I write this, Sidky's Gish gallop has had more than 3,000 views on [academia.edu](https://www.academia.edu). So, I now take up the pen to offer a correction. Regrettably, the editors of *Al-Uṣūr al-Wuṣṭā* have denied me a right of reply, so I am distributing this response by other means. I have an article forthcoming in a prominent journal that will help readers understand the nature of these issues in a more general way, but what follows here is a specific answer to Sidky.

Now, to the substance. After some brief introductory statements, Sidky announces that he disagrees with my 'thesis', which he has "not found convincing" (p. 274). As I read this for the first time, I was intrigued. My book has a thesis? I had to wait to the end of the review to discover what my thesis was. It was, Sidky opined, that "the flexibility of the Quranic text persisted centuries beyond its standardization" (p. 287), for which he cites page 95 of CEQM. Here are my actual words from page 95:

What does the existence of these corrections mean? It is an open-ended question with many possible answers. Here are a few of my thoughts:

First, although it seems to have been reasonably demonstrated by now that (with the exception of the lower layer of the San'ā' palimpsest) most surviving Qur'ān manuscripts bear the signs of having been produced following a campaign of standardization basically consistent with that reported to have been directed by the third caliph, it is also clear that there existed *some* differences of perception about the correct words of the Qur'ān text at the times most of these manuscripts were produced, which were later revisited when these perceptions changed or standardization became more thorough. It is not impossible that some of these varying perceptions would have been tied to certain geographic regions or locales. This perceived flexibility exceeds the bounds of what is reported in the *qirā'āt* literature.

Second, these differences of perception were not confined to the earliest decades after Muhammad's death, but there was some flexibility extending for several centuries after. The flexibility does not appear to have been great.³

Readers will notice that my remarks are measured and carefully qualified, with attention to the scribes' apparent perceptions concerning what was the "correct" text at the time of manuscript production or correction. In contrast, Sidky's rendition of my supposed 'thesis' is a sweeping and unqualified claim of flexibility in an official text throughout those centuries. I would not be so foolish as to make such a claim.

The foregoing is, unfortunately, not the only instance where Sidky's representation of my views displays a lack of care. However, the more important point is that *Corrections* is hardly a formal

³ CEQM, 95, emphasis original

presentation of a thesis to begin with. As already mentioned, it is a simple introduction for laypeople to the little-known and under-reported existence of scribal corrections in Quran manuscripts of the 1st to 3rd Islamic centuries: at most, a prelude to an argument. True, it is a work in which I give a brief preliminary comment from the vantage point of having observed and recorded thousands of corrections. At the appropriate time, I shall willingly submit to critical assessment of both my methodology and conclusions, but it would be reasonable for critics to hold fire until I have had the opportunity to make a full presentation of both.

Let us turn to another example from the introductory pages of how Sidky erects straw men only to knock them down. In his second paragraph, Sidky states, “[w]e are told that a survey of these manuscripts in a little more than a hundred pages will ‘challenge the traditional assertions about the transmission of the Quran in several ways’ (p. xxi) and have much to say about ‘the pious enhancement of the Quran’s textual history’ (p. xxii). Unfortunately, the bold claims are left unsubstantiated.” (p. 273) But, did I really make such claims? Again, I quote the actual passages to which he seems to refer:

This book, as I have already said, will not speak to the larger issues of Islam. But it does focus on tangible historical objects that, because of their particular characteristics as described here, do challenge traditional assertions about the transmission of the Qur’ān in several ways. If you are a curious and inquisitive person, [...] this book might be of interest to you not only for what it says about the matter at hand but also for what it says about what might be termed a pious enhancement of the Qur’ān’s textual history.”⁴

Here, Sidky’s report lacks precision. I did not suggest that my survey would challenge traditional assumptions. What I said was that the characteristics of tangible historical objects (manuscripts) do so. My book was not the point; at issue was the existence and nature of corrections and other features of manuscripts. Of course these features require analysis and interpretation, but supplying that was not the purpose of CEQM. Furthermore, and contra Sidky, I did not claim that *Corrections* would have “much” to say about “pious enhancement of the Qur’an’s textual history.” That was gratuitous embellishment on Sidky’s part.

It is most unfortunate that throughout Sidky’s review there are many such misrepresentations of my views, presuppositions and actual statements. What follows is a list of some ways in which the reader of Sidky’s review would not get accurate information about my book.

⁴ CEQM, xxi-xxii

1. Sidky represents me as claiming that the earliest Quran manuscripts were not subject to censorship or destruction (p. 274). This account is careless, as I did not deny censorship, and regarding destruction, I only reported that, unlike New Testament manuscripts, Quran manuscripts were not *usually* destroyed upon discovery by ruling authorities.⁵ Actually, as it stands, Sidky's report is absurd, because he must be aware, and indeed I report it in *Corrections*,⁶ that there was censorship and destruction of non-conforming Qurans during more than one campaign of standardization, the first one being at the direction of the third caliph, 'Uthmān.

2. Sidky alleges that I "erroneously state[] that some of the earliest quranic manuscripts, and in particular those in the Hijazi style, were written without diacritical marks," and continues to state, "In reality, the very earliest Hijazi manuscripts certainly contain occasional diacritical marks." (274) The portion of my book to which he refers reads, "The earliest Qur'ān manuscripts, particularly those in the 'hijazi' or 'mā'il' styles, were written without diacritic marks *or with only occasional diacritic marks to disambiguate the archigraphemes.*"⁷

Among Sidky's weaknesses is that he does not seem to know what he doesn't know, and this leads him to make over-confident sweeping claims. Had Sidky personally inspected many early Quran manuscripts, or had he conducted a review of the scholarly literature, he would have known that there are indeed pages of manuscripts in the Hijazi style written entirely without diacritic marks. For example, I have studied the Codex Parisino-Petropolitanus in person in both St. Petersburg and Paris, on multiple occasions. Moreover, Déroche has noted in relation to this manuscript that one of the scribes used no

⁵ "It was not dangerous to own a copy of the Qur'ān in the Arab empires that by the latter part of the 7th century stretched across a huge swath of territory from the Iberian Peninsula and the Maghreb in the West to Azerbaijan in the East, nor were these manuscripts usually destroyed if discovered." CEQM, 3.

⁶ "With some very important exceptions — most notably the Qur'āns that tradition tells us the third Caliph, 'Uthmān, burned and that would therefore be forever lost — we seem to have a good number of early Qur'ān manuscripts..." CEQM, 2.

⁷ CEQM, 5, emphasis added.

dots at all, and another used only five dots in sixteen folios.⁸ Being familiar with such facts, I reported that the earliest manuscripts use no or only occasional diacritics.

Here, Sidky's surprising haste to patronize my careful statement about dotting as "a common misconception," (p. 274) was despite his knowing that I have studied a great many early manuscripts in person, and successfully defended a doctoral dissertation on this topic.

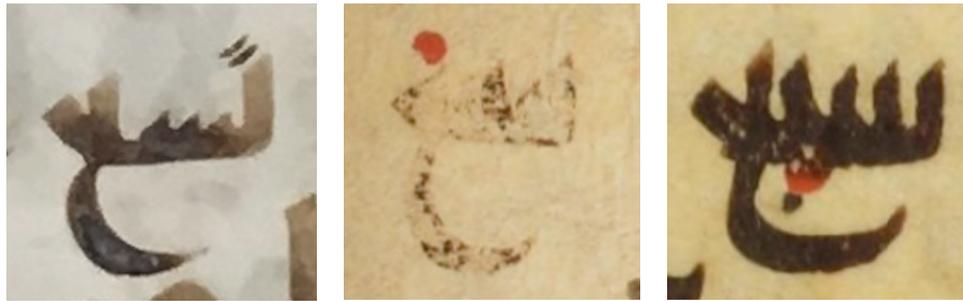
3. Sidky faults my claim that later development of script grammar allows for precise disambiguation of identical archigraphemes, calling this assertion "strange and ... incorrect." However, Sidky once again refutes a straw man; his answer to my allegedly incompetent claim is that script grammar "cannot disambiguate a single undotted word form." But, I said nothing about word forms.

Let us consider Sidky's claim that script grammar "cannot disambiguate a single undotted word form." Is this true? We need not look far for an answer: The archigraphemic sequence BBBBE⁹ occurs in twenty-two places inside the Qur'an. In three of these, Q27:12, 17:101, and 38:23, it represents the letter sequence تسع "nine." (see Figure 1) This letter sequence, with script grammar but without dots, is unique from all the other nineteen occurrences of this archigraphemic sequence in the Quran.¹⁰ That is to say, script grammar *can* disambiguate a single undotted word form. Sidky's statement to the contrary is simply incorrect.

⁸ See, for example, François Déroche, *La Transmission Écrite Du Coran Dans Les Débuts De L'islam : Le Codex Parisino-Petropolitanus*, ed. Gerhard Böwering and Jane Dammen McAuliffe, Texts and Studies on the Qur'an (Leiden: Brill, 2009), 41. "A la différence des autres copistes, E n'emploie aucun diacritique;" also "The Codex Parisino-Petropolitanus and the Hijāzī Scripts," in *The Development of Arabic as a Written Language: Papers from the Special Session of the Seminar for Arabian Studies Held on 24 July, 2009*, ed. M.C.A. Macdonald (Oxford: Archaeopress, 2010), 113-19. "It is immediately apparent that E did not use any diacritical points at all on his two pages, and that C – the second best represented hand in terms of volume of text transcribed – only distinguished three letters, namely *thā'*, *dhāl*, and *nūn*, and this occurs only five times in sixteen folios" (116); "The distribution of the dotted letters in the codex does not seem to answer a specific purpose: one can find pages without any dot at all ..." (117); and "... one of them does not use any dots at all ..." (ibid.).

⁹ I here employ Thomas Milo's system for transcription of un-disambiguated archigraphemes.

¹⁰ In sixteen of the other nineteen occurrences, this sequence of allographs represents either the letter sequence سبع "seven" (Q2:29, 2:261, 12:43 (three instances), 12:46-48 (five instances), 23:17, 41:12, 65:12, 67:3, 69:7, 71:15); in one it represents شيع (Q15:10), in one it occurs within the word أُسْبَغَ "he bestowed," (Q31:20), a hapax legomenon. In one place it represents the sequence ينبيع "springs" (Q39:21), which has its own script grammar form unlike any of the other instances of this allograph sequence in the Quran (see Figure 1, middle image). Script grammar cannot disambiguate س from ش, or medial ن from ب from ت from ث from ي. But, it can disambiguate the former group from the latter, and this is often sufficient to clarify a text. On script grammar, see Thomas Milo, "Towards Arabic Historical Script Grammar through Contrastive Analysis of Qur'ān Manuscripts," in *Writings and Writing: Investigations in Islamic Text and Script*, ed. Robert M. Kerr and Thomas Milo (Cambridge: Archetype, 2013).



تسع of Q27:12
Marcel 2, 19v

ينبيع of Q39:21
BnF *arabe* 340, 90v

سبع of Q15:10
BnF *arabe* 340, 37v

FIGURE 1: Script grammar at work. The form at the left can *only* represent the letter sequence ع - س - ت inside the Quran. (Left: Brubaker photo, courtesy of National Library of Russia. Center and right: gallica.bn.fr)

4. One of Sidky's most bizarre objections stems from another misrepresentation. In *Corrections*, after defining and summarizing paleography and codicology among the available tools for analyzing and dating manuscripts, I write

A third method for dating is probably the most well-known: radiocarbon dating. This method can be applied to anything organic. Everything that was once living, that is, all plant or animal material, is organic. Parchment qualifies and can therefore be tested with this method. The reason radiocarbon dating works is that a radioactive isotope of carbon is present in all living things and begins to slowly decay at a predictable rate when the living thing dies. Subjecting parchment to this testing yields a series of date ranges based upon the probable time that the source (in this case most likely the goat or sheep) was alive.

Obviously a radiocarbon date range cannot tell when a parchment was written, but we generally assume that a parchment did not sit for decades before receiving its first writing.

Radiocarbon dating is not a foolproof way of determining dates of manuscripts. Some manuscripts of known date of origin (e.g. with a colophon or some other overt indication of the time of writing) have been radiocarbon dated a century or more off from the apparent actual date of production. Therefore, all these methods must be taken with a grain of salt, and in most cases *the best thing is to take all the various clues (paleography, codicology, and radiocarbon dating if available), and weigh them alongside one another.*¹¹

¹¹ CEQM, 6, emphasis added.

Sidky, however, summarizes my treatment thus: "Brubaker also discusses two other forms of dating, codicology and radiocarbon dating, the latter of which he calls 'not foolproof.' This skepticism towards radiocarbon dating is reminiscent of the discussion regarding the Dead Sea scrolls [sic]; there we find that the consensus has indeed converged on the method being foolproof." (pp. 274-275)

Sidky has a novel definition for the word "foolproof." Multiple rounds of radiocarbon dating the same page have at times returned significantly divergent date ranges.¹² Causes for such variance may include a poorly-conducted test, contamination of the sample, or other factors (such as regional differences in solar radiation) that may be contingent upon knowing *where* the object spent the last thousand-plus years, and therefore cannot be assessed for certain objects by any lab, since doing so would require a knowledge beyond the reach of science. It is also possible for a test result to be correct when the manuscript was actually written an extended time after the plant or animal comprising the substrate died, rendering the test "not foolproof" for determining manuscript age. This was part of my precise meaning in using the term "not foolproof for dating manuscripts." Also, there can be issues relating to regions or time periods (aridity, solar radiation levels, atmospheric factors, or other climatic conditions and cycles, etc.) that create a need for adjustment of calibration. Such factors have been particularly noted in objects from the southern Levant region.¹³ I have written about these things elsewhere. Am I a "skeptic," as Sidky alleges? Skepticism, in the sense of not taking everything at face value, but rather testing propositions and methodologies, is the very foundation of science.¹⁴ But, I am not a

¹² This method of dating yields results in the form of date ranges associated with different probabilities that the date the plant or animal was last alive falls within that range; as the date range widens, the probability increases. Good labs (e.g. Oxford, Stanford, and Arizona) will return similar results, generally with a variance from one another of not more than decades. Several early Quran manuscripts have been radiocarbon dated, and the results demonstrate that it is not safe to assume that they were always written within a few years of the death of the animal. One folio of the manuscript San'ā' 1, has been dated by four different labs, returning 95 percent probability date ranges 410-460 AD (Kiel), 388-535 AD (Lyon), 480-546 (Zürich), 504-550 AD (Oxford), all predating Muhammad's birth in 570, and his prophetic career which lasted from 610 to 632. As it seems unlikely that the manuscript was written with quranic material before Muhammad was born, this page is a clear example of the use of seasoned parchment in the production of Quran manuscripts, and as such is evidence that we cannot assume a radiocarbon date gives us anything more than a terminus post quem of the actual date of production of a Quran manuscript. Mark Durie, *The Qur'an and Its Biblical Reflexes* (Lanham: Lexington Books, 2018), 18-19.

¹³ See, for example, Sturt W. Manning et al., "Fluctuating Radiocarbon Offsets Observed in the Southern Levant and Implications for Archaeological Chronology Debates," *Proceedings of the National Academy of Sciences* 115, no. 24 (2018).

¹⁴ I completed my B.S. in Cell and Molecular Biology at the University of Washington (1995); I understand the scientific method and the principles of good experimental design and analysis. Good science requires both rigor and humility. When the latter is missing, the former is often compromised as well.

skeptic in the rebuking sense that Sidky alleges. Radiocarbon dating is not foolproof for dating manuscripts; it is a tool that, used properly, with scientific humility, and with due attention to possible sources of error, can be very useful.

Oddly, after leveling criticism at me for recommending a hybrid approach that takes into consideration all available data and methods of analysis as a means of better discerning a manuscript's date of production, several months later Sidky *himself* put forward essentially the same proposition in a Twitter thread,¹⁵ seeming to claim it as his own insight — and received hearty affirmation from his followers.

5. Sidky correctly quotes my assertion that “the readings are different from the *rasm*, and in most cases the one is not affected in the least by the other.”¹⁶ However, in the very next sentence he says “it is incorrect to state that they [that is, readings and *rasm*] are entirely independent.” (p. 275) That is, his statement seems to suggest that I claimed otherwise. However, “most” and “entirely” are not the same.

We now arrive at the first point in the review where I willingly accept correction: Sidky rightly notes that in my book I use the term “readings” where I ought to have written “regional *rasm*”:

A further matter of difficulty for the readings is that the consonantal texts of some of the important monumental early codices, such as the Topkapı, Istanbul, and Cairo *maṣāḥif*, do not reflect a single **reading**, but rather what might appear to be a combination of the different **readings**. This fact leads the preparer of their facsimile editions, Dr. Tayyar Altıkulaç, to describe these codices in terms of rough percentages when it comes to their adherence to the various **readings**. Such a circumstance is not necessarily irreconcilable with the existence of approved **readings**, but it does indicate a more complex picture that requires further inquiry and explanation.¹⁷

In each of the instances of the word “reading(s)” above, and in one instance in the following paragraph in my book, I ought indeed to have written “regional *rasm*,” because my intention was to accurately report the work of Dr. Altıkulaç, and he documented the latter, not the former, in his analysis. This point of detail having been admitted, I draw attention to a further important detail: When it comes to the codices being discussed, it may be anachronistic to speak of either

¹⁵ “Early Qurans can be dated in many ways using codicology, paleography, orthography, art history, and radiocarbon dating. Here I wanted to provide a peek at a new approach which we will be seeing more of in the future. It is the use of combined dating[.] The idea is to combine radiocarbon dating with other knowledge, like paleography, to construct a chronological model.” Sidky, Hythem, <https://twitter.com/therealsidky/status/1273679354418147329>, June 18, 2020.

¹⁶ CEQM, 8.

¹⁷ CEQM, 9, bold type for emphasis added here.

readings or regional *muṣḥafs* in relation to these codices, and in any event, that is very nearly the upshot of Dr. Altıkulaç' analysis. It is possible that these codices were produced at a time when concept of regional codices lay yet in the future. It is all but certain that they were written before the reading traditions were formalized. In both instances, a significant piece of evidence for the proposition is the very manuscript that Dr Altıkulaç presents. So, to speak of either "readings" or "regional *muṣḥafs*" as significant (except to emphasize that a 'regional' standard appears not yet to have been imposed) may even be considered a teleological argument on Sidky's part, or at very least a broad and possibly unjustified assumption based upon later sources.

6. The penultimate example I shall give of Sidky placing words in my mouth is found on page 275, "Brubaker ... asserts that the thousands of corrections he has documented appear to have nothing to do with the reading tradition literature and thus must be explained by another phenomenon, such as a greater degree of perceived flexibility in the Quranic text in the early centuries (p. 9)." Here is the passage to which he refers:

[M]any of the thousands of corrections I have documented appear to have nothing to do with the readings attested in the secondary literatures. So, corrections must represent in at least some cases another phenomenon, such as perhaps a greater degree of perceived flexibility of the Qur'ān text in its early centuries (the time of first production of these manuscripts) than is documented in the *qirā'āt* literature.¹⁸

Readers will understand that "many of" and "some" both refer to a subset of the members of a category. "Many of" a thousand could be fifty or a hundred. "Some" of a thousand could mean five. Here Sidky has told his readers, many or most of whom will not have read my book, that I wrote something I did not, in fact, write.

7. As a final example, Sidky says the following in his critique of my discussion of a correction:

In addition to the points made above, there are a number of other errors [...] Brubaker notes the addition of an *alif* to *li-llāh* in Q 23:87 to yield *allāh*, which, he says, "comports with Abū 'Amr's reading (and another)" (p. 286)

In fact, this is perfectly correct. The "other" is Ya'qub. Sidky continues:

¹⁸ CEQM, 9.

Brubaker then cites Michael Cook as observing that this reading aligns with the codex sent by 'Uthmān to Basra, which was one of the four regional exemplars. Brubaker also states that al-Dānī ascribes the insertion to al-Ḥajjāj. Both of these statements are inaccurate: Cook explicitly rejects this variant as belonging to the Basran exemplar, while al-Dānī very strongly rejects reports of this variant being a later addition. Nowhere in this discussion is al-Ḥajjāj mentioned. (Ibid.)

What I actually wrote was, "This conversion, at this particular verse, has been discussed by Cook, who notes the resultant reading as **allegedly** aligning with the codex sent by 'Uthmān to Basra, as described in al-Dānī..."¹⁹ This is a report of Cook's statement that, "the sources preserve accounts according to which two additional *alifs* of B were inserted at a secondary stage." Regarding al-Ḥajjāj, I added that just prior to publishing, at the suggestion of a respected colleague, but neglected to double check. The fault is mine.



These examples of misinterpretation and misrepresentation are by no means an exhaustive list. There is, however, another aspect of Sidky's critique which deserves comment. Time and again I noticed that, where there was an instance of potential ambiguity in something I wrote, with one way of reading the statement being reasonable and the other way of reading it sounding stupid and incompetent, Sidky generally chose to assert that I meant the statement in the latter, i.e. stupid and incompetent, sense. Following is an example:

With regard to my choice to use the 1924 Cairo edition as a point of reference, Sidky imagines that I was "conceiving of the Cairo edition as a targeted standard for the changes surveyed in the book." (p. 280) Here, Sidky takes my reference to the 1924 text as evidence that I somehow imagine the scribes and correctors in the 7th, 8th, and 9th centuries labored to conform their manuscripts to an edition that would not exist until 1924. This is an absurd proposition and not one that is required or even warranted by the way I reference the text in CEQM, but apparently the only one that Sidky imagines me capable of making:

Brubaker's general observations on manuscript corrections in the introduction contain perhaps the most significant methodological flaw that permeates his book. He notes that most often, changes in manuscripts result in "conformity of that manuscript at the point of correction with the *rasm* of the now-standard 1924 Cairo edition" (p. 10). Brubaker sees this as a pattern, which shows "a general movement

¹⁹ CEQM, 57, emphasis added.

over time toward conformity, though not immediate complete conformity" (p. 10).²⁰

Readers will notice that my latter statement regarding movement toward conformity does *not* suggest conformity with the 1924 Cairo edition, but rather conformity of the manuscripts *with each other* in a way that happens to match that of the Cairo edition, which is itself obviously rooted in older traditions. However Sidky chooses, again, to accuse me incompetence. He continues:

There are two major problems with this conclusion. The first is the evident anachronism of centuries-old manuscripts corrected to conform to a text from 1924 [...] In effect, this is a teleological argument for an end goal that did not exist at the time. The second is the presupposition that whatever standard the 1924 Cairo edition is based on differs from the standard that existed at the time the early manuscripts were written. However, corrections apparently in the direction of conformity to the Cairo edition are not evidence of a changing standard, but evidence of a standard in the first place! [...] Both of these problems stem from Brubaker's apparent lack of understanding of the nature of the Cairo edition.

Sidky at this point elaborates the history and basis of the Cairo edition and its reliance upon a rasm standard more archaic than that upon which "many manuscripts over a millennium older" were based, then continues:

Therefore, the Cairo edition in fact breaks away from the orthographic standard of classical Arabic that characterized nearly all *muṣḥafs* prior to its conception. Recognizing this aspect of the Cairo edition, which belies its use as a standard toward which Qurans evolved, makes apparent the anachronistic nature of the approach adopted by Brubaker.²¹

Once again, a full quotation from CEQM, including the part Sidky chose to omit, will suffice to demonstrate how wide of the mark Sidky's comments are:

Most of the time, I have found that corrections in a Qur'ān manuscript result in conformity of that manuscript at the point of correction with the *rasm* of the now-standard 1924 Cairo edition. This pattern is important and shows a general movement toward conformity, though not immediate conformity.²² There are interesting questions raised when a manuscript is corrected in one place but remains deviant (**the word**

²⁰ Sidky, 276.

²¹ Sidky, 276-7.

²² N.B. — Once again, observe that I do not here say "conformity with the 1924 Cairo edition."

“deviant” supposes a standard and I use it here merely as a practical matter) when compared to the 1924 Cairo edition in other places.²³

Note that I distance myself from the idea of the Cairo edition being a standard, yet in footnote 8 Sidky suggests, yet again, that I am arguing “for the development of a later standard,” something I did not do, and in so doing he raises the objection that the problem for such an argument would be that “[s]cribes across the entire Muslim world, for centuries prior to the 1924 Cairo edition, were entirely comfortable with orthographic fluidity yet somehow managed to refrain from making more significant changes.” (p. 277) However, my observations of corrections and variations have been confined to the earliest centuries of Islam, not more modern written transmission during which scribes “somehow managed to refrain from making more significant changes.” My modest general observation was that there was a degree of perceived fluidity, on the part of scribes and correctors, in the earliest decades/centuries. In fact, I would be surprised if such variants (i.e. beyond orthographic variants with which *rasm* literature is primarily concerned) continued to exist in Qurans written for many centuries after that.

It is regrettable that Sidky's tone and wording throughout most of the review is similarly dismissive. Absent prior reason to believe an author to be incompetent, a tendency to uncharitable interpretation is not only ungenerous; it will mislead readers.



At this point I shall turn my attention to Sidky's treatment of my presentation of the titular manuscript corrections, before concluding my response.

Sidky's commentary concerning the corrections I show generally falls into three categories: 1) Brubaker did not say enough, 2) Brubaker's description is wrong, and 3) nothing to see here; these are mere scribal mistakes.

BRUBAKER DID NOT SAY ENOUGH

I need not dwell on this objection that I did not say enough, as I have dealt with it already. *Corrections* never pretended to be the definitive statement of my work, nor even the presentation of any kind of argument. It was merely an introduction for laypeople, a taste of things to come. To have packed my book with hundreds of footnotes and an exhaustive bibliography would have defeated the stated purpose of the book.

²³ CEQM, 10, emphasis added.

BRUBAKER'S DESCRIPTION IS WRONG

I need not dwell on this objection, either. Sidky went through my book with a fine-toothed comb, addressing many small details. Although he concedes concerning Chapter 2 (the one detailing the corrections) that “Brubaker’s observations are generally sound,” (279) this is but a prelude to a litany of criticisms, many of which are merely differences of opinion concerning what is happening on the page or what might have been going on in the mind of the scribe. Sidky is welcome to his opinion, as are others who read the book and wish to engage with its substance. They are also welcome to take the material I have documented to the “next level.” Indeed, one reason I did this work is to help others have access to data that was formerly unavailable.

NOTHING TO SEE HERE, MOVE ALONG

Concerning the corrections presented, Sidky has decided that “Apart from a few interesting variants [...] the majority of the changes are best explained as scribal errors.” (280) Further, he seems to accuse me of purposely misleading readers; for example, he speaks of “the lengths to which Brubaker goes in order to avoid suggesting scribal error as an explanation.” (281)

Now, parablepsis, assimilation of parallels, and other known causes of actual scribal error are not a threat to my work, as Sidky seems to imagine; they are part of it. I did not set out in *Corrections* to give twenty examples that could not be explained as scribal error. Indeed, I state openly:

In the preceding pages, I have shown examples of corrections from Qur'ān manuscripts that were produced in the first several centuries after the death of Muhammad. As stated earlier, I did not choose the most dramatic examples to present here, but rather a good group of samples to introduce the range of the phenomenon. In order to provide readers with the most value, **I've generally decided not to pick corrections that I've judged to be the result of correcting a scribal mistake** from the time of first production; the one exception in this book is (possibly) Example 8. Among the corrections I have documented so far in my research, **simple scribal error does account for some of them , and it is important for readers to understand that this explanation is the first factor I consider when trying to discern the cause.** These manuscripts were written by human beings, not machines, and so ordinary human error must always be taken into account.²⁴

²⁴ CEQM, 94-5, emphasis added.

In contrast to my accommodation of a variety of explanations for corrections, Sidky goes to great lengths to assert that scribal error accounts for almost every case.²⁵ In doing so, he misapplies terms with which he is apparently unfamiliar. Examples follow.

Sidky erroneously states that Example 9 and 18 are “standard parablepses.” (p. 282) This is technically incorrect: a piece of written text is not a parablepsis, so nothing in a manuscript “is” a parablepsis. Parablepsis (eye skip) is a cause, one that typically leads to one of two effects: haplography (missing script) or dittography (repeated script). There is furthermore little, if any, basis for claiming that either Example 9 or Example 18 were caused by parablepsis. The eye skip usually happens when a textual element (usually a word or word sequence) occurs twice in close proximity. If the earlier instance of the word is written down, the scribe looks away (perhaps to refill ink), and upon returning to the page accidentally skips ahead in the writing to the text that follows the second occurrence of the word or sequence, this is called haplography due to parablepsis. If the second occurrence of the word or word sequence is written down, the scribe looks away, and begins writing after the first instance of the word or sequence (thus repeating a portion of text), this is called dittography. In example 18, there are indeed two instances of the archigraphemic sequence BBKM in Q6:40, and the insertion of *al-sā'a* occurs after these. However, parablepsis was *not* the cause, because what follows the sequence BBKM in question as the text was first written is not what follows either instance of BBKM in the standard text; therefore this situation is not consistent with a parablepsis scenario — it is neither dittography, nor haplography. In example 9, the archigraphemic sequence preceding the insertion in Q42:5 is A LEFW R. Not only are there not two instances of this sequence in proximity of this verse; there are not even two instances of the sequence in the entire surah, nor the two suras before, nor the three suras after. I do not believe that parablepsis can reasonably account for either of these omissions, and it seems hard to imagine what could have caused Sidky to appeal to this explanation.

In his discussion of Example 12, Sidky again alleges that this is a case of parablepsis. He claims parablepsis has occurred here and can somehow account for the scribe initially writing *wa-llāhu* immediately after *wa-faḍlin*. But, nowhere else in proximity to this verse does the *wa-llāh* follow directly after *wa-faḍlin*. So, how is an eye-skip to account for the scribe making such a mistake? Perhaps Sidky means to suggest that the alleged eye-skip occurred after the scribe wrote the particle *wa-*, but this seems far-fetched. The word *allāh* does occur fourteen times on this page, but it follows the particle *wa-* only once,

²⁵ Sidky is welcome to assert what he wants, and has already found a ready audience for his claim that these corrections are mostly attributable to scribal error. However, he should support his claims.

nine lines above this spot. And, in that case (verse 167), the particle does not follow *faḍlin*, but *qulūbihim*. Scribal error may account for this correction at Q3:171, certainly, but I see no way it can be reasonably ascribed to parablepsis as Sidky claims.

A few further small observations with quick answers:

Sidky does not like that I presented nine instances of insertions of the word *allāh* as my Example 3. In particular, he does not like that I show a correction where there is not an apparent viable reading without the inserted word. But since my stated purpose was to show the range of the phenomenon of corrections in early Quran manuscripts,²⁶ why then should it come as a surprise that the examples I chose show a range of the phenomenon, which occasionally includes apparently nonviable texts as first written?

Sidky faults me for failing to mention Motzki and Schoeler in reference to well-known issues of the reliability of the Biography of Muhammad and hadith. But, once again, *Corrections* is not my academic book. As I state in the third paragraph of my Preface, "In the following pages, I've written to be understood [...] if you are an academic, you may find yourself wishing for more detail. If this is the case for you, I do hope you will [...] wait patiently as I finalize further publications."²⁷

Missing my purpose yet again, Sidky references a list of people and publications (apparently imagining that I have no familiarity with them), to argue that 1) there is evidence of an early single archetype, 2) stemma have been discerned in the manuscripts, and that 3) the evidence is consistent with the traditional narrative on 'Uthmānic standardization. But Sidky is tilting at windmills, since I have not argued against any of these things.

On pages 18 and 19 of *Corrections*, I run through several of the challenges that some features of the manuscripts create for popular impressions and common knowledge. The last item on that list is:

Fourth, the existence of manuscripts that were finely produced yet sometimes corrected after a long passage of time is interesting and presents a challenge to the notion that there was a strict uniformity and widespread agreement about every detail, every word and letter, such as one would expect to find if there were widespread agreement upon a

²⁶ "I've chosen the following examples for this introduction to the range of the phenomenon." CEQM, 27.

²⁷ CEQM, xvii-xviii. Furthermore, there are many people beyond Motzki and Schoeler that could be cited on the matter of the reliability of hadith, among them Fred Donner from Sidky's own University of Chicago. Since he is picking on such details, Sidky's failure to even mention Donner in this respect is telling.

standard form from a very early date, such as the time of 'Uthmān's caliphate.²⁸

Yet again missing my purpose and audience for this small introductory book, the single sentence above sets Sidky off on nearly a whole page of impassioned critique, accusing me of setting up a straw man, since the pre-modern Muslim world was comfortable with a degree of variance in the manuscripts, and the literatures document contentious debates surrounding various issues, mostly orthographic. But, most people, Muslim or otherwise, have gained the view that the Quranic text is unvarying in every respect, and I was writing my book for most people. If what I described was a straw man, I was not the one who set it up.

Sidky writes, "If Brubaker wishes to make the point that there was early fluidity and that manuscripts move toward the standard text over time, he would have to explain why the standard text is ubiquitous in manuscripts that are evidently paleographically older as well as those that are newer." (p. 280) There is a bit to unpack here. First of all, I do not "wish" to make any such point. A scientist does not begin with a conclusion; a scientist begins with a question, framing it as a hypothesis, and then designs a way to test the hypothesis. But, Sidky's argument seems to run thus:

PREMISE: Variant A exists in manuscript X, *but*

PREMISE: Earlier and later MSS contain the nonvariant form, *therefore*

CONCLUSION: Variant A cannot indicate fluidity in the perception of the "proper" text by the manuscript's producers or correctors

However, the conclusion does not follow from the premises. Very early in the history of Islam, there was significant geographic diffusion, a massive empire. The existence of a line of documents across time basically²⁹ consistent with the idea of an official standardized/archetypal *rasm* does point to the existence of an official standardized/archetypal *rasm*, but it does not preclude the possibility of simultaneous preservation and propagation of variant perceptions about what the "correct" text was among some people or communities. Sidky presumes that I am clumsily forcing the evidence toward a brash and sweeping revisionist conclusion. In truth, I am doing something much more subtle. Had he read my book in a less dismissive frame of mind, he might have noticed as much.

²⁸ CEQM, 19.

²⁹ The approximate nature of alignment is itself not to be overlooked.

CONCLUSION

At many points in my reading of Sidky's review, I had a twinge of fright. *Did I make such a mistake? Did I really say that?* But, time after time, when I went back and looked closely at what I had actually written, my fears were allayed. I do not reject responsibility for actual shortcomings, and I know there are some. However, I do note, with some chagrin, that Sidky's piece was more a demolition job than accurate review, more impressionism than realism, more bulldozer than scalpel.

In his review, Sidky failed to perform an essential task for any reader, namely to observe what a book is about. My small book's clearly stated purpose is to report and illustrate by means of examples the mere existence of corrections in early Quran manuscripts. Apparently Sidky wanted me to have written an altogether different book, so he reviewed it as if it was that book. Sidky, moreover, is protective of traditional views, such as the conventional account of the creation of an 'Uthmanic *rasm*, and pious narratives concerning the *qibla* turning toward Mecca/Ka'ba shortly after the *hijra*.³⁰ This defensive impulse and the misreading of my purpose caused him to launch a rebuking attack at the mere mention of a theory he considers to be beyond the pale. In his corrective passion he makes a litany of errors, reading meanings into my book which may match his agenda, but which my text itself cannot support. These misreadings reflect bias, not careful objectivity.

³⁰ Sidky states that David King has "written at length to debunk the thesis advertised by Gibson," (p. 277). I am aware of, and respect, King's venerable scholarship on *qiblas* over many years, but I'm not fully persuaded by the aforementioned "debunking" article. Regardless, I mentioned Gibson only to highlight an interesting and novel theory reconsidering the material evidence, one that I believe merits further inquiry. Regarding the North African and Andalusian mosque *qiblas* facing a line parallel to the trajectory from Petra to Mecca, I find King's explanation of astronomical alignment toward Canopus (like the orientation of the Ka'ba itself) to be compelling, though I am concerned that his sources are late.

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