MS# 12-44 (11-25) author notes regarding revisions

Dear *American Studies* Editorial Staff,

I hope this message finds you well. Attached, please find a revised draft of my article Manuscript 11-25, ““Even the Most Careless Observer” Race, Visual Discernment in Physical Anthropology from Samuel Morton to Kennewick Man.”

I was very happy with the positive reader reports, and found the suggestions very useful for revisions. I have to add that this is probably the most collegial and constructively critical set of reports I have ever gotten for an article submission. It has been a pleasure to work with them.

I have made substantial revisions to every section of the MS. Below, I will go through the reviewers’ comments, which are cut and pasted below in the underlined sections of text. The numbering of the underlined sections is my own. The non-underlined text beneath each cut and pasted section is my response.

Please know that I am happy to make any other revisions that are deemed necessary. Thanks you so much for your time and consideration.

**Reader #1:**

1. There are obvious editing issues: pagination, large spaces between paragraphs. On page 17 there a mysterious “note xiv.” CITE

This has been corrected.

1. A major theme is the contrast between the standards of “science” in anthropology between the 1830s/40s and the end of the 19th century. Reference is made to Morton’s training at the University of Edinburgh (nt. 24, p. 38). Was his training in medicine and biology (fields in which there was at the time deep interest in comparative anatomy, plant physiology, and the physiology of the human brain)? Was Morton an economically independent, “gentleman scientist” not that far removed from the broadly educated “amateur scientists” (p. 21) to whom is writings appealed?

Morton was trained as a medical doctor, and practiced in Philadelphia. This fact and its relationship to amateur scientists’ reception of Morton, is clarified in the current draft, on pages 13 and 14 of the main text and in Note 14.

1. If memory serves, one intellectual root of early anthropology is traceable to late 18th and early 19th century anatomy and brain physiology. But the contrast between this earlier work and the late 19th century is a contrast between paradigms of inquiry. It is a contrast between inquiry still connected to moral philosophy and broad intellectual interests, and inquiry linked to the emerging research universities in Germany and the U. S.. The latter was the locus of the differentiation and professionalization of the disciplines (including anthropology). It’s not until the latter part of the century that one begins to find the use of statistical analysis in the biological and social sciences. I believe this historical theme needs greater illumination in the revision.

The role of changing paradigms of physical anthropology and the use of statistics was discussed in the previous draft, and I think is made clearer in this one. I have made the following revisions: The question of statistical analysis and its development at the end of the nineteenth century is now introduced on page 2. It is discussed briefly on page 17, and discussed in far greater depth in the section on Franz Boas that begins on page 28. The fact that Boas’ “four field” anthropology drew epistemological boundaries between different sub-disciplines is discussed with greater depth in this draft, and is meant to directly address the concerns quoted above.

1. But there is more to this contrast in the history of anthropology that relates to the author’s singular use of Franz Boaz to highlight the differences. I have no quarrel with Boaz’s importance for putting Morton’s work in proper perspective. Putting aside Morton’s racism and elitism, however, Boaz’s *cultural* as opposed to *physical* research points to other historical sources of the discipline which call attention to the narrowness of Morgan’s anthropological work. The author quotes Boaz: “It will be seen that that part of human history that are subject to physical anthropology is by no means identical with that part of history which manifests itself in the phenomena of ethnology and of language . . . The three branches of anthropology must proceed each according to its own method . . .” (quoted at pp 30-31). The intellectual roots of the cultural side of anthropology as opposed to the physical side are in the Scottish Enlightenment and, in Germany (especially for Boaz), in Kant, Herder, Dilthey, and neoKantianism. I don’t mean to suggest that readers and practitioners of earliest physical anthropology would not also have been interested in ethnology and language. The point to is to draw attention not only to different types of sources but also different methods of inquiry—which leads me to the next comment.

I’d like to point again to revisions made to the section beginning on page 28. One of the striking things about Boas is the eclecticism of his training, which ranged from more humanistic studies to the biology of Rudolf Virchow. Accordingly, even as he defined anthropology as a four-field discipline, he stressed that there were epistemological differences between sub-disciplines, which would produce different results from different realms of data. This division is crucial to the emergence of the modern discipline, is a boundary that was non-existent for Morton, and still tends to be blurred in public debates like the one over Kennewick Man.

1. Earlier the author compares “the boundary between present-day academic culture and popular culture to C. P. Snow’s “two cultures.” If memory serves, Snow’s contrast was between the natural sciences and the humanities (rather than popular culture).

The reviewer is correct here. I no longer make this reference.

1. The issue here is whether, by the mere use of statistical techniques, physical anthropology can be construed as a “hard” science like physics. Despite scientistic pretensions by some early practitioners, I don’t think so. But how about Boaz’s other two branches of anthropology? Even if quantitative methods were appropriate and currently used would there not also be involved a considerable amount of qualitative, interpretive, and/or hermeneutical analysis? And would not the use of these qualitative methods be considered “soft” by the standards of the “hard” sciences? True, during the early days of professionalization, the social sciences wanted to be considered genuine sciences and leading minds considered the physical sciences the disciplines to emulate. But doesn’t Boaz’s judgment about methods *appropriate to the problems and subject matter* to be addressed still ring true?

I think that these points are also addressed in the revisions to the discussion beginning on page 28, which is mentioned above.

1. The project of the professionalization of the social sciences included the notion of a value-neutral discipline. Normative questions should be left to philosophers and theologians. Even theory-driven empirical research should exclude value judgments. In highlighting Morton’s racism and moralism, the paper shows the contrast between Morton’s value-laden idea of science and the later ideal of professional practice. But despite Boaz’s desire and efforts to professionalize anthropology, Boaz was an anti-racist, progressive public intellectual whose own published work did not shy away from moral judgments. I think the contrast drawn between Morton’s and Boaz’s work on human origins and morphology is effectively presented. More needs to be done, however, in placing this contrast in the context of the history of anthropology and the professionalization of the social sciences.

Regarding the relationship of moral statements to scholarship, I have added a comment regarding Boas’ politics of race and ethnicity to page 28. However, as I also point out on that page, the main argument of this piece is precisely to focus on epistemological differences between Morton and the modern physical anthropology that transcend political agendas strictly speaking. The methodological and epistemological separation of physical and cultural anthropology that I have mentioned several times so far is central to this.

1. I wonder if this current “effort to explore different models for reconciling indigenous perspectives with modern science” is best described as a “democratizing impulse” (p. 35). It probably has more to do with anthropologists deferring to indigenous anti-intellectualism than democracy as a political principle. Three lines down this impulse is equated with “interpretive populism” (p. 35). This latter label is the better of the two.

I no longer use the phrase “democratizing impulse” in this draft, though the general argument remains as the reviewer characterized above.

**Reader 2:**

1. This “combination of quantitative rigor and subjective vision” in Morton’s work, the author contends, continues today and shapes discussions of race where physical anthropology is involved (such as Kennewick Man) and this, in turn, continues to shape popular culture and the perceptual practices associated with and generated by it. The last part of this thesis is the least persuasive and I would recommend revising the essay (as well as reorganizing this) so as to downplay the influence of this way of looking on contemporary practices of visibility (for which the essay does not provide sufficient evidence) and situate its claims more centrally alongside those of Fabian and Gould: esp. in terms of how it refines our understanding of Morton, his scientific method and how his contemporaries understood its validity in the 19c, and finally how an earlier physiognomic method persists into Morton’s work and may still inform visual perceptual practices today (both scientifically and culturally).

This was a very helpful observation. In fact, I am not arguing that Morton’s craniography had a direct influence on contemporary vernacular ideas of looking at faces. In the current draft, I have tried to clarify that my argument is that there is a well-documented tendency to privilege visual evidence in American culture, that this seems to have helped legitimate Morton’s work in the 1840s, and that it continues to play a role in popular-culture understandings of things like the facial morphology of Kennewick Man despite changes in the epistemological bases of physical anthropology. This is not a case of Morton exercising a persistent influence on popular culture, but of Morton’s work having been legitimated by popular culture assumptions about “seeing is believing” that continue to influence vernacular publics despite the fact that the epistemological bases of anthropological science have changed.

1. Onthis last point, I think the essay would benefit greatly from engagement with two recent intellectual histories on the subject: Ann Fabian’s recent book on Samuel Morton (*The Skull Collectors*) and Bruce Dain’s work on 19c race theory in the US (*The Hideous Monster of The Mind*).

Both of these books were very helpful (and very enjoyable reads!) and are now cited at several points in the text.

1. Fabian’s book is particularly relevant for this author’s subject since she explains how the

circulation of Morton’s *Crania Americana* owed more to Morton’s use of elite social networks than actual scientific arguments. In other words, it wasn’t just that his data was wrong, it just didn’t matter. Once Morton’s polygenist ideas reached the right audience (pro-slavery advocates and Northern racists), they took root (solid or not).

A very good point that I made revisions to elaborate on. See page 20 of the current draft.

1. Fabian’s closing chapters on Museum work and composite photography also seem pertinent to the author’s discussion of the blurry line between the practices of scientific statistics and vernacular looking.

Though this is a compelling point, I have shifted the emphasis on the nature of the visual in the current draft in such a way that this would be a somewhat tangential discussion.

1. Dain’s work would be useful for further contextualizing the fact that Morton and his brand of scientific racism (polygenist in nature) were neither the only nor the most popular race theory circulating at the time.

I now cite Dain in the relevant discussion on page 14, as well as several other points in the text.

1. The author might also want to consult Martin Berger’s *Sight Unseen* which considers the many ways in which race and visuality intersected during the 19c., esp. Berger’s provocative idea that race can be understood as a position from which one sees.

I am familiar with this book, and have added it to a note pertinent to Morton’s trip through the Caribbean. However, I felt that after the changes I made to this draft, a more extensive engagement with Berger would have been tangential.

1. I also highly recommend the author consider Francois Delaporte’s *Anatomy of the Passions*.

This is also a very compelling idea, but I felt that there was no space here to do justice to the discussion of emotion and expression in Delaporte, when what I am dealing with here are much more static elements of physiognomy and morphology.

1. Argument.Clarity. The central argument of the paper is not very clear. See p. 2 where the author refers to how “historical reflection” “provides useful insights,” what are these insights (and again on the bottom of p.8)? See p. 3, what is the relationship b/w academic and popular culture? How exactly does either the historical past (Morton’s craniology) or academic present (physical anthropology) shape contemporary visual perceptual practices?

As I mentioned before, I have made extensive efforts to clarify the argument based on these very helpful suggestions. I have completely rewritten the introduction. I think that opening with a brief discussion of Dundes’ classic “Seeing is Believing” essay clarifies my central point. I think that the following argument is now much clearer in this draft: There is a well-documented privileging of vision as a source of proof in American culture. Morton was writing in a time when this vernacular idea about looking at faces was at the heart of craniology, while later physical anthropologists would be more skeptical about the usefulness of visual examination of individual specimens. However, the logic of “seeing is believing” continues to have a powerful role in public discourse on things like the Kennewick Man debate. Thus, Morton represents an early point just before a rupture between popular and academic cultures in regard to understanding differences in human type, and this rupture is still evident today.

1. The ambiguity of that relationship allows the author to evade certain problems with the argument, such as the fact that the kind of looking that occurs in public involves discerning the faces of living people as opposed to looking at the skulls of deceased ones. What Morton scrutinized (skulls), in other words, would be invisible to the public today. I don’t believe this invalidates the author’s claim, but it might need to be rearticulated to how a physiognomic method informs both practices of visibility (looking @ skulls; and looking @ faces).

Physiognomy as such plays less of a central role in this draft, which focuses more on the more general privileging of sight. I have also added a discussion of how Morton reconstructed fleshy physiognomies and its relationship to contemporaneous forms of paleontology and comparative anatomy on page 15.

1. In addition, more evidence should be provided to account for why Morton’s ideas (as opposed to others) would be so influential to later racial perceptual practices, esp. when some scholars believe race was moving from biological to cultural markers at the end of the 19c.

This was not an argument that I was trying to make, and I hope that that is clearer in this version.

1. Argument. Coherence. This paper, at times, articulates two related, but distinct claims: (1) that the boundary between present-day academic/scientific and popular culture is fuzzy; and (2) that the boundary between academic/scientific culture of the 1840s was equally fuzzy. My sense is that the central claim of the paper is neither of these two claims, but the relationship of #2 to #1. But that relationship is not stated as clearly as it should be (see comment #1 above), nor is the paper organized around that central claim as effectively as it could be.

See comments # 16 above.

1. Argument. Illustration/Visual Evidence. I think the role of visual evidence, the elaborate and costly illustrations that went into Morton’s *Crania Americana* needs to be more centrally featured, perhaps moved forward in the argument and its discussion broadened. The author is correct to draw our attention to Morton’s illustrations (pp20+), but more needs to be said about how this presentation creates the very archive that scientists like Morton claimed to interpret. The author might wish to consider Allen Sekula and Francois Delaporte who have written persuasively about this with respect to physiognomy. The author might also want to situate Morton’s illustrations as an example of the visualization of knowledge itself, evidence is something to be seen (not something to be calculated or tested).

Which is exactly what I do in the current draft, from the very introduction. This was an excellent suggestion!

1. Argument. Clarity. The author suggests that “forms of discernment that would seem thoroughly subjective today are given a credibility” (24) in Morton’s day. But is that credibility a product of Morton’s status (white, educated, elite male) or a product of his method (that earlier scientific practices gave greater weight to individual observation) or some combination of the two?

As I hope is clearer in this version, this credibility is due to a combination of factors. One is cultural. There is a “default” tendency for Anglo American publics to privilege sight. Morton was credible as an educated, elite, white male, but he also lived in a time when the kinds of population analysis based on abstract metrical data had not come to dominate credible science. Thus, he lived in a time when the cultural emphasis on vision could be more easily passed off as credible science.