

Before I start, let me apologize for the tardiness of this review. Catching COVID for the first time at the beginning of the term hit me extremely hard, and I've spent most of my time trying to catch up.

I was very excited to be invited to review this paper. The authors have done multiple smaller studies to address fundamental questions about the marine sedimentological record and potential biases. They have examined the potential of hiatuses impacting sediment rates, age-related trends in rates, created a model to explore the above, then finally used the preceding results to inform a 'corrected' sediment accumulation rate.

I have only a few reservations which I believe could be handled in minor revisions. I do not believe there is need to perform additional analyses, but instead these are considerations which I think should be discussed and could be fodder for future studies. I am not sure how long this review is going to get, but that should be seen as interest and enthusiasm for publishing what is an excellent contribution, rather than critical flaws. I agree with the authors statement on lines 416-419, quantitatively establishing this as a fundamental bias in our marine sedimentological record, is of very broad significance.

My initial thought, back in the abstract actually, was "how are they going to address compaction?", and so I was surprised when there are only three mentions of compaction (one in fig. 7 caption, one when discussing erosion, and buried in the supplemental). I think this is the largest flaw here, we expect older intervals, usually buried under considerable mass from the sediments above, to have undergone compaction (water squeezed out, etc) - thus deeper sediments (typically older) should tend to have lower sedimentation rates when compared with those more shallow (thus younger). I would have expected this to be a discussion at the very least, or demonstrated that this isn't a feature of their "Within Section SAR vs. Time" analysis. Dealing with this does not need another analysis, but discussing how this impacts the results is certainly warranted.

Age model resolution - I was left with a question about the underlying resolution of the age models and how that changes through time. Being most familiar with the foraminifer biostratigraphic zonation scheme, the resolution is very different throughout the Cenozoic, with very short durations post KPg and in the latter portion of the Miocene and Pliocene-Pleistocene, and long zones in the Oligocene, for example. While that's certainly not 1 to 1 with the age models in NSB, I would imagine there are intervals which tend to have very highly resolved age models and those with less resolved. That might contribute to a few of these questions, though especially a trend in time with respect to the number of hiatuses. If there are intervals of time with poorly resolved age models, one would expect the probability of missing

a hiatus during that interval to be higher. I do think this is a finer point than it sounds, I generally agree with the statement made at 397-399 about age model quality, except that there could be pernicious systemic biases rather than the general uncertainty/diachroneity we usually worry about with biostratigraphy.

Sediment type - Probably my bias as a carbonate-focused worker, but I had also expected some discussion of two things: 1. paleoceanographers tend to fixate on finding considerable carbonate sediments due to their potential for lots of geochemical proxies, thus skewing records. 2. (and more importantly), there wasn't a lot of discussion about the different types of sediments found, other than distinguishing between clays below the CCD and carbonate above. Again, not suggesting to add an analysis of %carbonate or data from core description, but discussing the consequences (or lack there of) of different sediments in the potential environments seems valid.

Organizational - I was surprised by the "workflow" section, as it read to me as a methods+results summary prior to methods. I found it a bit jarring, but after coming back at the end of the paper, I understand it's likely there because this is essentially 4 small studies built together and either the authors or reviewers were expecting folks would get lost. I do, however, like the figure 2 associated with it quite a bit.

Caveats - Around L290 there's a discussion of the global pelagic biogenic sediment flux. I really like this section, but I would like there to be a short discussion of how to use those numbers or how to assess the uncertainty there. Given the analysis and uncertainties therein, should we only be interpreting the broad step around the EOT as real, should we be interpreting the broad Oligocene hump as real, or are the higher frequencies useful?

Supplemental files:

I have read the supplemental files, examined the figures. I am python illiterate (sadly, an R person only at the moment), so was not able to fully evaluate the code. I think I understand it somewhat, but without the skills to virtually-kick the tires that's the best I can do. I have not reviewed all the multitude of age models in SOM2, but NSB is the appropriate database to do this work on and these age models have been a part of many previous studies.

Line - by line

Line 40: I apologize for being a grammatical pendant but ending a sentence with "with" isn't appropriate, this should read: "There are, however, many general limitations with which studies of this type must deal."

41: first comma isn't necessary.

49, 101: Earth should be capitalized.

124: SAR isn't defined yet (done on 137)

127,128: I bristled at the "typically with only a few, limited duration hiatuses." That's a statement they back up later, but I would have preferred to have a definition of what the authors were considering a hiatus (e.g., >0.5 myr).

145: Typo

147-149: I don't understand this sentence, and I'm not sure if it's because of missing words or what.

178: missing reference? "2001, ; geologically young"

179-180: You could probably show this quantitatively by plotting by expedition year, but I doubt that'd be interesting in this context (very interesting in others though!)

212-213: Isn't the 'unbiased selection' fairly subjective at this point?

248: space missing between sentences

255: typo

259: typo "insignificantly"

332-333: I think this sentence is too strong. I'd be ok if instead of "likely" the authors were to use "potentially", but I don't think the analysis here is specific enough about this question. It's certainly a testable hypothesis, however.

376: Typo "burg, 2010, , see Figure"

400: typo "documented. he one"

408-410: I don't know that this statement is true. I think that most of us are pretty aware that the record we have is biased in a tremendous number of ways, even just starting with IODP favoring carbonate sediments over others for paleoceanography, the loss of ocean crust through subduction leading to a tiny fraction of our exposed rocks being Cretaceous aged, and so on. Maybe it's because I'm an import from paleobiology (more-or-less), but I think this is accepted, but perhaps under discussed?

I, however, will defer to the authors with the phrasing here. This isn't a big deal.