We hereby resubmit our manuscript 'Cenozoic pelagic accumulation rates and biased sampling of the deep-sea record' for your journal consideration. Apologies for the delay in sending this resubmission despite fairly minor revisions but in light of the broken agreement between Copernicus and the Leibniz-Insitut we had to make sure our institution will still cover the APC in case of acceptance, before doing so.

According to the last reviewer's comments, we added a note on compaction in-text and a figure showing the relationship between age model resolution and time in the SOM. Please find in the text below our answers to the reviewer's queries.

The authors,
Johan Renaudie and David Lazarus.

Reviewer 1

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 marinesedimentological record, is of very broad significance.

Thanks a lot for your thoughtful review.

My initial thought, back in the abstract actually, was "how are they going to address compaction?", and so I was suprised 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.

As explained to the previous reviewers, the concrete consequence of compaction is a change in the sediment density. As the computation for SAR is based on actually measured sediment density, and not an idealised version of it, compaction is in fact taken into account here. But indeed this explanation was missing in text, we have now added such a statement into the main text (lines 227–230).

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 Plio-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 missinga 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.

This is a fair point. We have thus added a figure in the SOM testing this issue. From what we know of NSB, while this issue would certainly be an issue in the Cretaceous, it is not, as the figure shows, an issue in the Cenozoic.

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.

Our paper is focussed on authigenic pelagic sediment. There are only three types that form substantial amounts of the deep sea sediment record – in pure form are: carbonate sediment, biosiliceous sediment and red clay. While red clay provinces are geographically large the average rates of sedimentation there are extremely low and these provinces thus contribute little to overall rates of sequestration. Much of the clay or fine silt in them also is from eolian transport of physical weathering products from land – the authigenic rates of sequestration are even lower than the bulk accumulation rates. Pure biosiliceous sediment is fairly uncommon in the deep sea, due presumably to the substantial amounts of weathering on land of carbonate rocks in addition to the more 'balanced' silicate minerals. Most biosilica is in sediments that also have significant, or are dominated by carbonate. Thus most of the biosilica record comes along automatically as the carbonate dominated sediment they occur in is selected by carbonate focussed drilling plannners. Some of the pure biosilica sediments have also been drilled. It is true that this last type of sediment has been to some degree undersampled by drilling (as radiolarian/diatom micropaleontologists ourselves we are acutely aware of this!), but the amount of undersampling of this fairly rare sediment type is not all that large, and due to its rarity, not significant for our study. Lastly we know of no reason why any bias would have

changed in relative intensity with geologic age. We thus feel that our calculations and the trends we have found are not significantly affected by biases in sediment type in our data.

> 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.

This was indeed a request from the previous reviewers.

> 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 interpeting the broad step around the EOT as real, should we be interpreting the broad Oligocene hump as real, or are the higher frequencies useful?

This is a very good question but we do not have an easy answer to this. We think to adequately answer this we would need to examine how many actual sections are being used to calculate the global rate for any given time interval, how many of these are coming from a particular geographic region, or from other subsets of the data that might contain a bias; and create simulations to see how much variation could be produced by the granularity of variability in the dataset. This is all do-able but also fairly complex, and would address only one source of possible bias – there may well be others we haven't thought of that could be affecting short term calculated rates. What we did however is test the effect of two different paleotopographic models, and what the results showed is that the first-order trend is the same but smaller amplitude versions are different enough not to be fully trusted. We thus have decided to be as conservative as possible and here only interpret the first order trends. Hopefully our work will stimulate follow-up studies where the reality of the finer scale features of our results can be confirmed or not, and if found to be real, appropriately interpreted.

> 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."

Modified accordingly.

> 41: first comma isn't neccessary.

Done.

49, 101: Earth should be capitalized.

Done.

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

Done.

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

Corrected

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

It was just a LaTeX oddity, which is now corrected.

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!)

Indeed.

248: space missing between sentences

Corrected.

255: typo

Corrected.

259: typo "insignicantly"

Corrected.

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.

This is a fair point. We changed for "potentially".

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

Same as above.

400: typo "documented. he one"

Corrected.

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.

Reworded to:

Although paleoceanographers have long known that older sediments are less commonly recovered than younger ones (Lisitzin, 1996) they have, to our knowledge, largely assumed that this phenomenon has not in itself introduced any substantial bias into the recovered representation of ocean sediments.