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Comment on egusphere-2022-1324

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Referee comment on "Missing sea level rise in southeastern Greenland during and since the Little Ice Age" by Sarah A. Woodroffe et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1324-RC1, 2023

General Comments

This paper compares a 300 year record of relative sea-level (RSL) change in southeast Greenland with a sea level budget developed from a combination of observations and modelling. The authors conclude that there is a significant mismatch between the amount/rate of RSL change inferred from the proxy-based reconstruction and the sum of modelled contributions, indicating errors in one or both of these datasets/approaches. Whilst the authors conduct some sensitivity analysis in an attempt to identify likely sources of error, they ultimately conclude that the source of the misfit remains unexplained, with the challenge for future work to reduce the 'budget residual' of +2.5 mm/yr since the end of the Little Ice Age (LIA).

The paper addresses a scientific question of relevance to this journal, presents new RSL data from a poorly studied location / period, and identifies a significant limitation in current understanding that will require new data and analysis to resolve. I cannot comment on the details of the modelling components of the study, but the methods used to produce the RSL reconstruction are sound and reasonably well-established. The substantive conclusion of a mismatch between modelled RSL budget and the field evidence for RSL change appears robust and I recommend this paper for publication. I have several questions relating to the more detailed inferences of elevated rates of RSL change in the early and latest portions of the record (see specific comments) and some suggested modifications to tables / figures. Consequently I recommend publication subject to minor revision.

Specific Comments

Calculation of palaeomarsh-surface elevation (PMSE)

The authors use a visual assessment of indicator species to essentially delimit vertical assemblage zones that are used to infer a particular PMSE with associated uncertainty (as in Table S2). The basic approach is sound but I have a few questions about its application to the particular dataset that the authors may be able to clarify.

The main elements of the visual assessment criteria appear well suited to identifying which side of the high marsh / upland boundary and low marsh / high marsh boundary a sample sits, but I have a question regarding how the finer-grained differentiations were established (e.g. the difference between PI 5%, PI 6%, PI 10%) which are based on very small percentage differences. Can these 'zones' be clearly identified on Figure 3A (if not, perhaps they are not reliable)? What was the count size of the diatom samples in both the surface and fossil datasets and is this sufficient to accurately identify a 1% difference? The reason for raising this question is that the current PMSE zones create the very smooth / progressive change in PMSE noted at the top of the core which contributes to the inferred 20th century rate change discussed in the text (also see comment on top of the core below).

The PMSE between 5 cm and 8 cm depth (corresponding to the 19th century, essentially stable RSL interval) is 1.65 m with a 15 cm error bar on either side. I'm not entirely clear where this comes from and wonder whether the authors could be slightly less conservative here? For example, at and below the low marsh / high marsh boundary, the proportions of *Achnanthes* species and *Navicula salinarum* rise to relative abundances similar to or greater than those used for the favoured indicator species PI and NC. According to Fig 3 B neither of these taxa are present in the core. Unless there are known issues of selective preservation which means these taxa a likely to have been preferentially removed, their absence would seem to argue for accumulation above the low marsh/high marsh boundary. This effectively precludes the 1.5 – 1.65m PMSE range associated with the lower error bars.

The potential significance of this is that, if correct, it would elevate the mid-point of the [PI below 5%, NC above 5%] zone, bringing it close to 1.75 – 1.8 m. In effect, the PMSE is now the mid-point of a high marsh zone delimited by the two grey shaded boundaries on Fig 3A (assuming that there are symmetrical error terms). If this is a correct interpretation of the data, this greatly reduces the apparent RSL rise at the bottom of the core which is purely driven by the magnitude of the change in PMSE (ie elevating the PMSE, reduces the inferred RSL rise).

Accuracy of the accumulation history and inferred RSL rate changes.

The chronology of the core for which there are PMSE estimates is derived from 3 dates (2 C14 and the inferred Hg dated horizon) spanning nearly 300 years of accumulation. This fact, coupled with the extremely low accumulation rate of saltmarshes in Greenland, makes it extremely challenging to produce 'high resolution' records. Consequently, whilst I think we can have some confidence in the century scale trends identified in this sequence, inferring decadal scale changes is much more equivocal. For example, I find it difficult to see the acceleration in the rate of RSL fall since the 1990s referred to in the text

(Ln448-449). This is not evident in Fig 3C and in the absence of age control for the 20th century, it is impossible to exclude changes in accumulation rate as a cause for any reconstructed PMSE variation. Unless the authors can provide additional supporting evidence to confirm sub-century scale changes I think these would be best excluded.

I also note that in Fig 3C there is a rapid jump in the apparent rate of RSL fall for the last (topmost) sample. Does the PMSE of this sample come from the observed core top height or is it based on the diatom assemblage? The reason for asking is that if it is based on the former it is possible to introduce a spurious jump in the PMSE due to a vertical offset between the mid-point of proxy-based PMSE estimates (at least when using proxies such as foraminifera which have edge effects toward the upper limit of marine influence). When calculating rates, it may be better to be consistent and use the same (proxy-based) PMSE estimate as the rest of the core. The 'true' elevation of the marsh surface should then be found within the error bars of the PMSE estimate.

Technical Comments

Ln 148: can bedrock 'prograde'? Alternative term may be better.

Ln 363: Pinnularia intermedia in italics

Ln 430: error term for +0.2 mm/yr?

Table 1: This is confusing - what is the CE/BP? Shouldn't this be all BP (or CE / BCE)? Values in the table don't seem to match the plot in Fig 4. Perhaps it would be better to have the reported C14 age and uncertainty in yrs BP and then a separate column for the calibrated output as CE?

Figure 3B: were diatoms analysed from the basal freshwater unit? If so (and they were absent) a note to that effect somewhere would be useful (or annotate on the figure 'barren')

Table S2: Depth error (6.25). Labelling errors in the lower 3 samples? 'intermedia'. Should this just be PI?