

This revised version has been significantly improved compared to the original manuscript. However it still needs substantial improvement before being considered as publishable. My main comments concern : (1) the writing, sometimes poor and unclear, with several vague or not argued statements (see below), (2) the lack of discussion on the data uncertainties, in particular on the steric component, and (3) the lack of acknowledgement to similar previously published studies on arctic sea level (just quoted in the reference list but without discussion/comparison) (see below).

- The abstract is vague and non informative. Nowhere in the abstract is explained what the computed reconstructed sea level consists of. The abstract says : '*as the first study...*', a wrong statement in view of the many previously published studies. In addition, it claims that NOT using GRACE is a progress, but why ? GRACE has a nearly global coverage over the Arctic and provides unique information about the mass component of the sea level budget in the Arctic. Why oppose GRACE to the approach considered here ? The only acceptable argument is the shorter time series of GRACE data. I recommend to rewrite the abstract and clearly explain the approach considered in this study and presenting the main outcomes (and eventually the novel results compared to previous studies). More detailed comments below :
- In the introduction, describe the main objective of this study and the approach used
- Line 31 : what is the meaning of 'difficult' in the sentence 'Previous attempts in reconstructing sea level in the Arctic have shown to be difficult' ? Explain. In the same sentence, the author writes : ... because satellite and in situ observations are less consistent'. Less consistent than what ? Curious claim considering that the authors ALSO use satellite and in situ observations...
- Line 35 : Clarify what you want to say by : 'the 10 mm/yr discrepancies between GRACE solutions'. In absolute value, this has no meaning. It should be compared to the amplitude of the signal. It also depends where.
- Line 37 : I disagree with the sentence 'some authors tend to choose the GRACE solution that closes the sea level budget. This is not true. Please remove this sentence.
- Method section, lines 63-64 and Equation 7 : What about local VLMs, unrelated to GIA and ongoing mass redistribution ? These could be important at some TGS
- Equation 6 : What about the GIA component for altimetry (the component of -0.3 mm/yr removed to altimetry data in terms of global mean)? It seems to be ignored here.
- Line 90. Fig. 1 should be quoted first.
- Section 3.1, Altimetry. What is the accuracy of the altimetry data set in this region partly covered by ice. The authors disregard GRACE because of its 'poor' accuracy, but what about altimetry ?
- Section 3.1, VLMs. The authors choose the option of using theoretical VLMs due to GIA and ongoing mass redistributions rather than GNSS data. Ok but explain why using the model is best and at least compare where possible the model VLMs with GNSS.
- 3.3, steric sea level. The interesting part of this study is the use of in situ T/S profiles over the Arctic. However, we have no idea of the data coverage nor on the accuracy of this data set. What is the integration depth H ? I strongly recommend the authors provide information on the T/S data set, a crucial aspect of the study.
- Section 3.4, ocean mass component. This section is hard to follow, with several unclear sentences, e.g., lines 163-165.

- Results section. I strongly recommend to put into perspective the results of this study with the published literature (e.g., Henry et al, Armitage et al., Carret et al., Raj et al). What is novel ? What is different ? etc.