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Interactive comment on "Dynamic Ocean Topography of the Greenland Sea: A comparison between satellite altimetry and ocean modeling" *by* Felix L. Müller et al.

Anonymous Referee #2

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The title says it all, really. I think that there is a lot of nice work here that should eventually be worth publishing, but it needs some thought, more work and revision before that is possible. My detailed comments follow, in order of occurrence.

p 1 L 8 delete "most" – a signal is either dominant or it is not

p 1 L 11 correlation coefficient r or r^2?

p 1 LL 15-17 "deeper comprehension of the Arctic's DOT" – this sentence is empty because the manuscript is a technical identification of similarities and differences between measurements and model – it offers no new insights into the behaviour of the Arctic Ocean (not in itself necessarily a problem).

p 1 Abstract: it is not clear to me that this manuscript contains properly formulated aims and objectives. It reads like a "look-see", and it does not need to; eg L 7 "to investigate similarities and discrepancies". If measurements and model agree, they are (probably) both right; if they disagree, you then want to identify which is "right" and which is "wrong", or even whether they may both be (differently) wrong. The authors should think more about how they frame the manuscript, therefore. See further comments on this below.

p 1 L 20 "freshwater inflow" – false as expressed – altimetry plus geoid can tell you about steric (density) changes, it does not specifically tell you about salinity (ie freshwater).

p 2 LL 15-16 potentially a very strong motivation for the study: you do not start by gridding, as others have tended to do. What are the benefits of this approach? It is hardly mentioned further but the manuscript really needs to draw out these benefits – assuming that they exist. If there are no actual benefits, then that too is worth knowing. I usually prefer not to suggest further work, but this is a case where it is necessary: please show the difference between a typical gridding approach to the altimetry and your finer-resolution approach. What matters and where does it matter?

p 2 L 19 delete "to" ("in spite of difficult")

p 2 L 25 delete comma ("conclude that")

Paragraph starting p 2 L 29 the justification for the use of FESOM is OK but I would like to see a line or two of context. What other models (if any) exist in this class (meaning spatial resolution, inclusion of ice and ocean physical processes, etc.), and how does it compare with them?

p 3 L 2 "eddy-resolving" – in most of the Arctic at most times of the year, but not everywhere and not always, see Nurser & Bacon (Ocean Science 2014). Near-zero wintertime shelf-sea density gradients can reduce the deformation radius below even

1 km.

p 3 L 16 "study area" – could use a more accurate description because you include the Lofoten Basin, discuss the Barents Sea, refer to part of the Arctic Ocean north of Fram Strait. The simplest solution is to replace "Greenland Sea" by "northern Nordic Seas".

p 3 L 16 reference to Figure 1. The figure is poor if you want it as a circulation sketch. Jan Mayen Current (south side of Greenland Sea recirculation), two branches of WSC, the baroclinic one runs further offshore along Knipovich Ridge, what enters the Barents Sea, the polar current around Svalbard? There are plenty of such sketches around.

p 5 L 2 query as to meaning: "The model does not include . . . tidal changes". Do you mean that the model does not include tides? Please be explicit – this is important.

p 5 L 9 we are told that the model runs from 2000 to 2009. Your analysis start date is determined by Envisat, your end date by the model run. Please state this explicitly.

Section 2.3.1 starting p 5 it looks like you have a completely different approach to determining SSH in the presence of sea ice to the (by now well-established) Laxon method, but you say nothing about how or why it might be better. You really need to compare the two approaches, which I recommend you to attempt using the gridded product that I suggest you create above, and then comparing with publications that use the Laxon method. This might entail further analysis using EOFs, or calculating eddy kinetic energy.

Comment on Section 2.3.3 you use the "highly resolved . . . OGMOC" geoid. A conference abstract is not an adequate reference for this product. More importantly, and since I cannot tell how it created, I strongly doubt whether harmonics to generate product resolution below 10 km is at all meaningful. Satellite gravimetry can only "see" signals at around 100 m resolution; and if you are looking at Greenland shelf seas (as you are), the issue of "leakage" (terrestrial signal contaminating ocean signal) cannot be ignored. At present, it reads like you treat the geoid uncritically, as a "black box",

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which is not sufficient.

Comment up to p 7. I have read to the end of Section 2 and there is nothing about tides, beyond a line in Table 1. Tidal corrections to altimetric SSH are critical, and all tidal models have weaknesses in the Arctic and the northern Nordic Seas because the M2 cannot propgate freely north of the critical latitude – and S2 is aliased by sun-synchronous satellites. Use of EOT11a, however good it is globally, does not avoid this problem. Have you tried, as Armitage et al. (JGR 2016) did, comparing the model tides with tide gauges?

p 9 L 14 you have identified a 3-day period artifact but you do not state what causes it; "irregular data sampling" is not an explanation.

p 10 last line "bins of 7.5 km length" – state reason for choice.

p 14 L 1 "These pattern originate from the altimetry DOT" – a factual statement without the implicit assumption would be "The patterns are seen in altimetric DOT but not in the model".

p 14 L 3 "insufficient sampling" – what does that mean, compared with my observations above about inherent weaknesses of geoids to do with resolution and leakage? What about tidal aliasing?

p 16 L 6 here we are told that model lacks tides; this needs to be stated at the start.

p 16 L 6 and following, concerning barometric effects. SSH corrections include the inverse barometer (your table 1) – why is this insufficient?

p 17 para beginning L 15 you finally talk about tides, but I am not persuaded that you have investigated fully. AOTIM is a good regional (Arctic) tidal model (Padman & Erofeeva, GRL 2004), for example. But while satellite-based model currently suffer from the sun-synchonous problem (mentioned above), even good regional models contrained by tide gauges lack infomration away from the coast. What signature might identify, actually or at least hypothetically, unresolved tides in the altimetry?

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p 17 L 32 spell Greatbatch

p 17 L 33 spell principal

So there is a lot of good work here, but I think that the authors need to do more to make this manuscript publishable. More context, more comparison with existing products and approaches, more thought about reasons for differences between measurements and model, and not just leaning on the positive sides of the comparison. We learn new things where approaches disagree.

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Discussion paper