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Discussions

Interactive comment on "Evaluation of Sub-Monthly Oceanographic Signal in GRACE "Daily" Swath Series Using Altimetry" *by* Jennifer A. Bonin and Himanshu Save

Anonymous Referee #2

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In oceanographic applications of GRACE, the question often arises whether the supposed gravity signal is really from GRACE measurements or is merely the prior ocean model used in the GRACE processing. The question is clearly important for a variety of ocean applications. This paper makes some useful progress in answering the question, based on three GRACE gravity solutions (although none is an "official" product from one of the U.S./German GRACE centers.

I recommend the paper be revised before it is published. Most of the items I list below are minor and easily dealt with (or dispensed with, if the authors decide). The one major item requires some data reprocessing. Another major item is: "Half the References

section is missing!" (Perhaps a problem with the OS website?).

Detailed items follow:

1. My major complaint is not with the GRACE processing, but with the altimetry – and more specifically it is with Figure 1, which reports a large "bias offset" between Jason-1 and Jason-2. This certainly will be a surprise to the altimeter community, and it contradicts what has been previously published – see, for example, papers by Ablain et al. (doi: 10.1080/01490419.2010.487805) and Beckley et al. (doi: 1.1080/01490419.2010.491029).

The Jason project teams and most users would be very concerned to see Figure 1 published as is, and for good reason.

In fact, this "bias" is merely caused by use of inconsistent versions of Jason GDRs. One cannot blindly combine different GDRs, based on different corrections and possibly other things (retracking?), and expect consistency. The authors should not rely on the "experienced aid of Don Chambers," but should carefully examine user handbooks and other documentation. They will find that there are other differences, too, not just the MSS model.

After I did some digging, I can add one thing in the authors' defense, which is a point about better data documentation. For some reason, the CLS group uses a naming (or non-naming) convention that is confusing. Their MSS evidently comes with a rate, and by changing the "reference time period" the MSS obtains different values, even though the fundamental MSS model is the "same" and retains the same name. (It is not a matter of the time span of data going into the determination of the MSS.) It would be much better if CLS didn't confuse users in this way, but that's the way matters stand. The GDR attributes give no hint of this problem, but the data handbooks do.

What the authors should have done, and should have written, is something like the following:

"We have used the best available Geophysical Data Records (GDRs) from Jason-1 and Jason-2, and applied consistent geophysical models to ensure a self-consistent time series of sea surface height anomalies across the missions. The source data are from Jason-1 version "E" and Jason-2 version "D" GDRs. Documentation for these different version numbers indicate the use of different processing standards, in particular ancillary geophysical models in the two sets of products. Most important for our investigation, we have used a consistent mean sea surface and ocean tide model. We have also used the ECMWF Reanalysis for the dry troposphere and inverse barometer corrections, as provided on the Jason-1 GDR-E, to mitigate any changes to the ECMWF operational analysis during our period of interest."

This does require some data processing. An alternative approach is not to use the GDRs at all, but instead use DUACS(Aviso) or MEASURES products, which are reprocessed data with consistent data handling since 1993.

2. The reference "Eumetsat,... (2016) for Jason-1 products isn't right, as Eumetsat had nothing to do with Jason-1.

3. Line 21: "as large as" -> "even larger than"

4. Line 23: How do you know the ocean models are poor in the Southern Ocean? If data assimilation has been used in their development, then I'd agree, but I thought OMCT and MPIOM had no assimilation. Is there another reason to think models are poor there?

5. Line 27: "predicated" is the wrong word to use here.

6. Line 163: "signal" -> "signal was"

7. Line 170: The authors here might wish to cite published work that has examined the barotropic circulation in this region. For example, work by Chris Hughes: doi:10.1029/2006JC003679

8. Line 210: Is MPIOM also forced by pressure? If not, how does this affect the

comparisons? Line 203 already notes that OMCT uses pressure forcing.

9. Line 214: The Lynch-Gray reference should be augmented (or even replaced by) Carrere et al.: doi:10.1029/2002GL016473

10. Caption to Figure 3. It would be useful to give the time intervals over which these standard deviations were computed. (In fact, I don't think I saw this in the main text anywhere either, but I may have missed it.)

11. Line 247. I would add "except for the middle and North Atlantic". It seems GRACE is not improving the prior model there.

12. Lines 280, 288: Are Figures 6a and 6b reversed?

13. Line 308: Could slightly more explanation be added here, or at least a reference? It is not obvious to me how Gaussian temporal windows are being used to form a band-pass filter.

14. Line 345: I would again suggest that it is mentioned that the Middle and North Atlantic are problem areas.

15. Section 7. Since this section already lists long URL addresses for data used, those things could be eliminated in the main text.

16. I much appreciate the color scales in (for example) Figure 5, where arrows point which way which model is superior. Very useful. Some of the figures are a bit hard to read, however, and a bit cramped. The fonts/resolution of Figure 2 seems especially fuzzy – a Word feature?

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