Thank you, Henryk, for your very helpful review. We've altered the text in several places because of it. Here are our more detailed replies. Please let us know if there's anything else you'd suggest changed.

-- Jenni and Himanshu

1.) It is stated at numerous places in the paper that the ITSG series are governed by external geophysical models whereas the swath solutions are free of a priori model information. I believe that both claims are not entirely correct and should be relaxed in the sense that both solutions utilize some external "information" (in certainly distinctly different ways). Note that such information might also include the assumptions that variability over oceans and land is uncorrelated, and that ocean bottom pressure variability in the tropics is very small. The inverse problem with just a 24-hour subset of GRACE data is ill-posed and needs to be stabilized in some way to obtain a reliable solution.

We have tried to be more careful in our wording. You are correct that some sort of stabilization is obviously required, and we now mention that in the paper. What we intended to say with regards to the CSR swath solution, was that there is no external geophysical model used to inform any constraints. The constraints are purely driven by GRACE information. The only external information used to define the constraints is the land and ocean boundary. Our information concerning your understandable question about "the variability over the oceans in the tropics is very small" is also derived purely from GRACE. While the inverse problem with just 24 hr subset of the GRACE data is ill-posed, the stabilization is helped by the fact that only the mascons under the ground tracks are estimated for the day. This inverse problem not as ill-posed as a global inversion from a 24 hr data. We have attempted to make this more clear in the paper:

> "The global mascon solutions and regularization are purely driven by GRACE without any influence from external models. The only external information used to inform the constraints is the land/ocean boundary mask. All the other information for constraints comes from expected signals in GRACE for that month from regularized spherical harmonic solutions (Save et. al. 2016) and the GRACE groundtrack. Since the daily constraints are derived from the respective monthly expected signals from GRACE, the regularization also allows for adjustment of unexpected signals that are captured the monthly solutions. The only submontly signals that will get constrained to zero in the swath solutions are the signals that may have a zero mean over 30 days throughout the mission but do have sub-monthly variability. The implementation of the swath estimation assumes that such locations are very rare. Thus, the time-variable regularization process used does not bias or attenuate future regional signals based on statistics from models or past GRACE months, but is intentionally designed to encourage no land/ocean correlation in order to reduce leakage. Further details of the data processing for producing the daily GRACE swath solutions is available in Save et. al. 2018 (in-preparation/in-review)."

There isn't a lot of detailed information about how the ITSG series are regularized, but everything we've read/heard suggests that they do it based on signals (RMS, etc) from apriori models. We sent an email to Torsten/Andreas asking for information, and this is what Andreas replied:

> The big picture of the daily processing has not changed much since Enrico's paper in 2012 (https://doi.org/10.1016/j.jog.2012.02.006). We still use daily GRACE normals (in spherical harmonics) and constrain them using a stochastic model derived from geophysical model output, so most of the conclusions from back then can still be applied today. What became more sophisticated over the years is the way how the constraints are computed. We put a lot of thought into how the covariance function of a high-dimensional stationary process can be robustly estimated. This mainly involves exploiting geophysical properties, for example, land/ocean masks.

We've updated the text to make this more clear, as well as adding the reference into it for those who want more information.

2.) It is nice to see that swath solutions show less noise in the tropics than ITSG, but it should be acknowledged at some point in the paper that reducing noise in regions where geophysical signals are expected to be non-existant can be very easily achieved by regularizing the solution towards zero. In case of an unexpected event at some later date (say, an earthquake), regularized solutions tend to underestimate or even miss that signal. Maybe the authors could elaborate a little further about the utilization of regularization (or related techniques) in the swath solutions when discussing the tropical oceans for the pleasure of their geodetic audience?

There is a main swath paper ready to be submitted for review shortly that will discuss all the details of the constraints, etc. We have now included a few details about the regularization in this paper, as well as adding a citation toward the in-progress work for further information in the future. The regularization matrix for the swath solutions are essentially an extension of the monthly regularization matrix design process (as described in Save et. al. 2016) but also includes information for the ground track.

As for your concerns about the tropics, you are correct that it's easy to "regulate away" errors by just driving them the full signal+error to zero. We see no indications that this is what's happening in the swath solutions, though, as said in above. The only way signal (or noise) in the tropics could be artificially driven to zero is if the monthly mean signal was zero, but the sub-monthly non-noise signal wasn't. That's unlikely to be commonplace, so if the tropics show low sub-monthly noise, it's because there's also low monthly-scale signal there within that particular month.

3.) The Low-degree Stokes coefficients not accessible from GRACE alone can be assumed

to vary rather slowly in time so that linear interpolation from monthly to daily sampling might be feasible. Have you tried this in some way? Would you expect any consequences for your conclusions? Which regions might be affected most?

If you're talking about the geocenter and J2 terms, we toyed with interpolating them, by fitting a trend/annual and interpolating based on that model. We chose not to add that complication in this paper, however, since we're looking at the sub-monthly signal. As you say, low degrees like the geocenter aren't likely to change rapidly, even assuming we had good daily geocenter data to represent reality with. And if we simply interpolated the change linearly between months, the change in the sub-monthly frequency band would be zero. Thus the omission. We've added a comment about this in section 3.2.

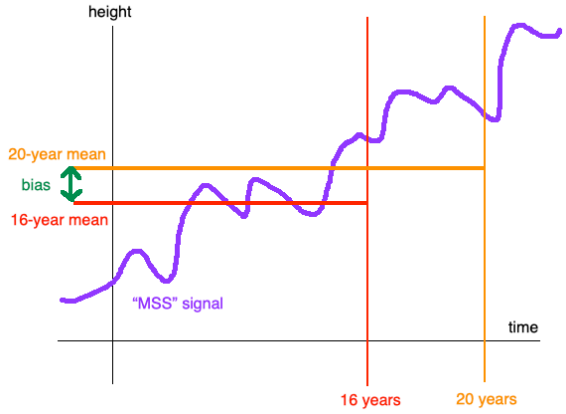
4.) The impact of change in the MSS model might be explored a little further. What is the difference between the 16y and 20y MSS? Is that effect perfectly linear, or do you see larger biases in regions where the MSS models differ most?

At some point in the future, I (Jenni) really would like to dig up my own MSS model and reprocess all my altimetry series correctly. But I haven't done that yet and it's no doubt going to be a pain. I've tried to look into the two MSS models used by Jason, but getting information about them has also proved unexpectedly difficult. The sum total of all the info I can find is from the AVISO website here:

https://www.aviso.altimetry.fr/en/data/products/auxiliary-products/mss.html

As you'll note, it's not extensive, nor does it link to any more useful papers. So I don't actually know the answer to your first question.

But I'm less concerned about short-term, mostly-regional differences where the two different models see slightly different signals, than I am about the bias difference. As best Don and I can figure, the main reason our Figure 1a sees such a huge signal isn't because the models are necessarily very different, but simply because they're centered at a different time. I'm attaching a little sketch I drew to show you what I mean. Even assuming the two MSS models were perfectly identical, because of the 16-year vs. 20-year time span, they're going to see different MEAN values. That's where the bias jump between missions (or rather, between MSS models) is coming from. It's way too big to simply be from real model improvements. They just didn't recenter the bias to the same timespan as the old MSS model. And also didn't tell anyone, which is even more frustrating – and why I wanted to explicitly mention it in this paper, so at least the information is out there someplace.



(I rather doubt that the journal would appreciate publishing my hand-drawn picture, alas.)

5.) The discussion of the signal in the Zapiola Gyre is interesting and deserves more attention. There has been previous work about the dynamics seen from both altimetry and gravimetry (see 10.1029/2018JC014189 and references therein), and it would fit well into the scope of the journal if some further discussion is added based on the swath data.

We absolutely agree that this is a fascinating area. There are some interesting results from the Gyre being included in the main swath paper. The summary is that GRACE swath solutions can clearly observe rotation in the gyre at a sub-monthly frequency that has been previously seen in the altimeter data. We're hoping to do more work in this region in the future – possibly with both ITSG2018 and CSR swath, since they both seem to give plausible localized results. That's a bit outside the scope of this paper, however – and probably shouldn't be published until after Himanshu's swath paper, anyhow.

Also, thank you for the link. That was an excellent paper.

6.) The assessment of the anomaly present in AOD1B RL06 in the South Pacific appears to be sound and forms valuable feedback for the development of general ocean circulation models. Our present-day understanding is that an overly simplyfied Ross Sea bathymetry in the MPIOM model run (i.e., all ocean areas covered by shelf-ice are treated as land) distorts the dominant eigenmodes in the larger region at periods around 3 to 8 days. I expect to see this problem reduced to a large extent in the next release of AOD1B.

Congrats on figuring out the bug! I'm glad to hear it. What a bizarrely localized issue.

Is this just for our own information, or would you like us to quote you in here so everyone knows? We assume the latter and have altered the manuscript accordingly, but if you'd prefer for us not to do so, that's also fine. Let us know what you'd like and we'll see it done.

Section 4: In terms of the language, I suggest to clearly separate between observations, which might "see" or "observe" signals; and on the other hand numerical models, which rather "predict" variations. I suggest to modify this throughout the whole manuscript, but in particular adapt the wording in Section 4.

Sorry. That's my bad. Srinivas has only been yelling at me about this imprecision since 2002, now. :) You'd think I'd know better. I've gone through and corrected it, as you suggest.

l. 135: The products used here have higher resolution in time but not in space, right?

Correct. We altered the line to make this more clear.

l. 137: The desire of labelling the CSR swath data as the "main" GRACE product is understandable, but not fully justified. Maybe just call it "your" GRACE product?

Agreed, this is definitely not the "main" GRACE series. We meant the main series used by THIS paper, not overall. We have altered the text to make that clear.

l. 159: ... results shown here represent the full non-tidal mass signal.

We have added a line confirming that the ocean tide model was removed and has not been restored.

l. 174: Who, in fact, is Norbert?

I am so sorry. I use Mendeley to organize my citations and somehow, in the submitted version of the document, the second half of the bibliography disappeared, including all authors with last names later than "M", and the ITSG citations shifted from the first author (Mayer-Gurr) to the last (Norbert). I have no idea how, but that has been corrected. Sorry again.