

Reviewer#2

General Comments

1. I thank the authors for taking the time to address my comments and suggestions. Especially the clarifications with respect to the use of post-fit residual derived covariance information and the inclusion of an extended contribution analysis make the manuscript easier to follow. I also thank the authors for updating the figures which I know can be a lot of work. Overall, the manuscript is - in my opinion – now in a much better shape.

Before publication I have a few remaining points that need to be addressed. Please find my responses to the author comments below.

AC: We thank the Reviewer#2 for taking time to review our revised manuscript. Your insightful remarks have helped us to identify parts in the manuscript which needed clarification and certainly allowed us to improve the quality of this paper.

Specific Comments

1. Original Author Response: AC: Thank you for your insightful comment regarding the truncation of the time-variable coefficients at degree 60. We appreciate the opportunity to clarify our methodology.

First, we would like to correct a potential misunderstanding: in our joint inversion scheme, the time-variable coefficients (up to degree 60) and the static coefficients (up to degree 180) are estimated simultaneously as independent parameters without any prior constraints from background models. That is, the coefficients above degree 60 for the time-variable part are not fixed to any background field; rather, they are simply not

parameterized because our analysis indicates that the time-variable signal energy is inherently concentrated at very low degrees. To justify this truncation choice, we conducted a dedicated experiment using the GOCO06s model. Specifically, we allowed its time-variable coefficients to evolve with time and then superimposed them onto the static coefficients. The resulting changes in the combined coefficients were found to be significant only up to degree 40, while higher-degree terms remained nearly unchanged. This demonstrates that the temporal signals have negligible power beyond approximately degree 40, and therefore truncating the time-variable estimation at degree 60 is a physically reasonable dimension reduction strategy rather than a hard constraint. It avoids introducing noise from high-degree temporal parameters without sacrificing meaningful geophysical signals. The following Figure is mainly about the geoid difference for GOCO06s at different time t with respect to the base model EIGEN-6C4, which can confirm our statement mentioned above.

Referee reports: I appreciate your attempt to clarify this point, but not estimating parameters above degree 60 exactly fixes these degrees to the background model (in your case GOCO06s because it is introduced up to degree 120). If there is a discrepancy between the information content in the observations and the background model, this difference will leak into the estimated parameters. This mechanism of aliasing is well documented and understood (e.g., Sneeuw & Van Gelderen, 1997 or <https://d-nb.info/960213988/34> pp. 55 – 56 for early references).

Truncating at any degree is a hard constraint for any parameters above the truncation degree. How the truncation degree is derived does not change this fact and “physically reasonable dimension reduction strategy” is just a different way of describing it. To put it plainly: “therefore truncating the time-variable estimation at degree 60 is a physically reasonable dimension reduction strategy rather than a hard constraint.” (P4 L21 – L24) is simply a wrong statement and hides the implicit regularization. This is not opinion, this is math.

You state yourself that “it avoids introducing noise from high-degree temporal parameters”, so the truncation stabilizes your solution, which is the exact effect of a regularization. Compare your approach to a Kaula-type regularization: it is also physically derived information on how different degrees of the gravity field behave. While the approaches differ in the degree wise behavior (your

approach: degrees 2 – 60 can vary freely, degrees 61 and above are fixed to the background; Kaula: inverse power law), they are conceptually equivalent.

I do not disagree with your approach and find it very practical; I just want that implicit regularizations are transparently communicated to the user community. Please correct this in the manuscript.

AC: Thank you for your comments. We agree with your statements about the implicit regularizations effect on our final solution due to the truncation degree for temporal parameters (i.e. annual trend/oscillation). However, it still should be noted that: the static parameters of 61-180 are not “copied” from the background, in other words, the static parameters from our final solution are “different” from the background. Your comment “degree 61 and above fixed to the background” sounds like our static parameters are “copied” from the background, which is not correct in our opinion. We agreed with your comments that our current statements are not that rigorous. Therefore, we have corrected the statements about implicit regularization in our manuscript according to your insightful comments.

Modified Sentence

P4 L18-L20 “... Besides, we need to address about this: both the trend/oscillation parameters and the static parameters are estimated simultaneously as independent parameters. In other words, the static/temporal parameters from our final solution are different from the background model”

P4 L25-L28 “... The lower truncation degree for temporal parameters do help to accelerate the calculation process under our in-house gravity field determination software. However, it should keep in mind that the different truncation degree for static/temporal parameters is a kind of implicit regularization for the static parameters beyond degree 60”

2. Original Author response: AC: For the second part, we don't use any low-pass filtering before down sampling since the original 2s LRI data has been performed the CRN filter (a kind of low pass anti-aliasing filter). And we also compare the different performance of the temporal gravity field at different LRI data resolution (i.e. 2s, 4s, 6s). The experiment reveals that there is no significant difference between the performance of 2 s and 4 s LRI data on temporal gravity field determination.

Referee reports: The cutoff of the CRN filter used by JPL is 0.25 Hz, so when down sampling to 4 s aliasing will occur. If the LRI product you use, applies a different CRN filter, please specify the filter properties, if not please acknowledge that no further lowpass filtering occurred but you deem the impact to be minor. Showing the differences in absolute to probably EIGEN-6C4 does not make this impact visible.

AC: Thank you for your comments. As far as we know, the filter properties of CRN filter are the same for the LRI1B data used in our manuscript. For a clear demonstration in our manuscript, we have modified the sentences according to your comments.

Modified Sentence

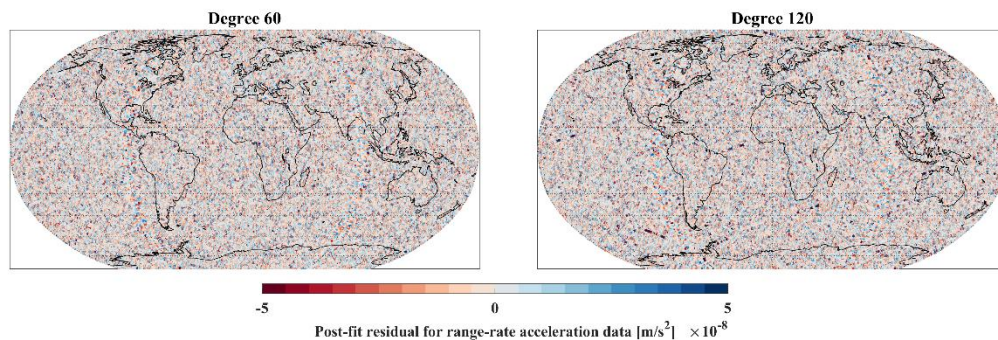
P4 L9-L12 *"... The LRI1B data used in our manuscript is performed by the CRN filter (a kind of low pass anti-aliasing filter) and the filter properties are the same. So, we don't perform any other low-pass filter before down-sampling the LRI1B data to 4s (original data in 2s) and we think the impact of this strategy is minor."*

3. Original Author response: For the third part, we perform a post-fit residual experiment based on the monthly temporal gravity field determination at different degree and order (i.e. 60 and 120). The result shows that even the recover order is set

as 60, little temporal signal is shown in the post-fit residual. The result is shown as the following figure but not shown in our manuscript.

Referee reports: Range rate residuals are not suitable for checking physical signals as they are dominated by long-wavelength accelerometer and background-model errors that hide small-scale geophysical features which are what you want to find. Range-accelerations are more appropriate, because the differentiation acts as high pass filter. This experiment therefore does not confirm that there is no temporal signal left.

AC: We perform a post-fit residual experiment based on the monthly temporal gravity field determination at different degree and order (i.e. 60 and 120). The result shows that even the recover order is set as 60, little temporal signal is shown in the post-fit residual. According to your comments, we perform a differentiation filter for range-rate residual to derive a range-acceleration residual. The result is shown as the following figure but not shown in our manuscript.

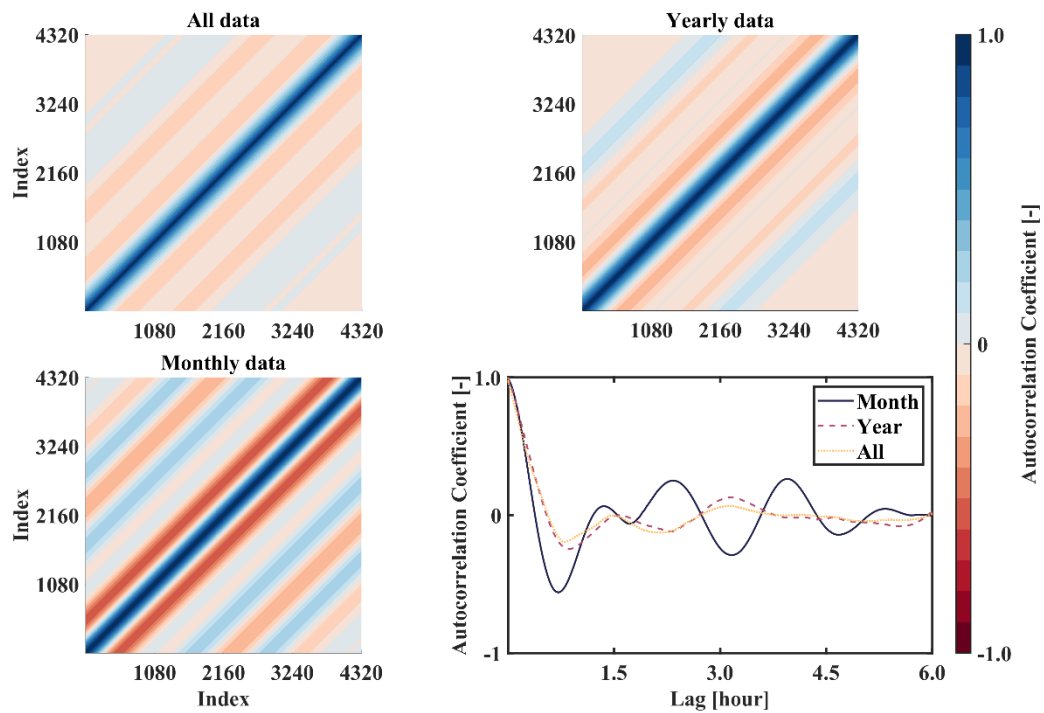


4. Original Author response: Thank you for your comments. We have replotted the figure according to your comments.

Referee reports: In Figure 3, the correlation matrices are still rectangular rather than square and the lag unit is still samples rather than a time unit. Since there are different sampling periods (5s KBR vs 4s LRI) this remains ambiguous.

AC: Thank you for your comments. The correlation matrices are something like rectangular due the figure option we chose, the correlation matrices are square actually and we have modified the figure option to make correlation matrices look like square in the modified figure. The correlation matrices shown in this experiment are calculated by only the 5s KBR1B data.

Modified Figure



5. Original Author response: Thank you for your comments. The constraint for the gravity field usually needs some prior/external information. However, the specific external information is dependent on the kinds of constraint, and our scope of manuscript is not the design of constraint. We can't specify the external information used in GOCO06s/ITSG-Grace2018s determination, and for a clearer classification, we add some related reference articles in our manuscript.

Referee reports: This statement contradicts P23, L3, where you state “due to the regularization imposed by external data in its estimation strategy”. If you do not know

what external data and regularization is used, how can you be certain that this is the sole reason this model performs better?

AC: Thank you for your insightful comments. As far as we know, the description of gravity field ITSG-Grace2018s/GOCO06s indicates the Kaula-type regularization is performed for the temporal parameters during the final gravity field determination and only the regularization method and external data are described for details in the manuscript for GOCO06s (<https://doi.org/10.5194/essd-13-99-2021>, pp104-105, section 3.1) but not for ITSG-Grace2018s which only the Kaula-type regularization is performed during the gravity field determination. As for the external data used in the regularization, only the GOCO06s describe what external data is used during the regularization approach, not for the ITSG-Grace2018s. So, we are not sure the improvement of ITSG-Grace2018s beyond degree 120 can be attributed by whether extension degree for temporal parameters (estimated up to degree 220, but regularize them from degree 120) or the regularization approach itself for temporal parameters solely. Since among the three solutions in our manuscript (Tongji-Grace02s, ITSG-Grace2018s, HUST-Grace2026s), the accuracy of Tongji-Grace02s and HUST-Grace2026s is similar over all the degrees while the accuracy of ITSG-Grace2018s is better than the other models beyond degree 120 significantly. A significant difference for the estimation parameters strategies is the estimation degree for temporal parameters and regularization approach. Anyway, we can't find the statements in the published articles for how external data is used in the regularization approach during the determination process of ITSG-Grace2018s. For a scientific and reasonable statement in our manuscript, we have modified the corresponding statement in the revised manuscript.

Modified Sentence

P22 L23-L24

“... More importantly, it applies regularization constraints specifically to the high-degree trend/oscillation terms (beyond degree 120). For the details of the constraints, please refer to Kvas et al (2021) and Save et al (2016).”

P23 L1-L3

“... (1) The global distribution of gravity anomaly differences for ITU-Grace16 or GGM05S is the most pronounced while that of ITSG-Grace2018s is least pronounced, signifying its closest agreement with EIGEN-6C4 and minimal residual noise.”

6. Original Author response: Thank you for your comments. According to your comments above, we add the contribution matrix result for trend parameters. The new result indicates that there is relationship between the static and trend parameters, while there is some prior information need to be introduced during the gravity field determination to decorrelate the relationship between the static and trend/oscillation parameters. In our solution, the static and trend/oscillation parameters are estimated simultaneously, not estimated independently and no external information is introduced during the gravity field determination. Therefore, our solution is not dependent on the background model. The change from Figure 10 to Figure 11 may be attributed to the relationship between the static and trend/oscillation parameters, not the truncation degree for the trend/oscillation parameters.

Referee reports: Please see my first comment on the issue of external information and regularization.

AC: Thank you for comments on the issue of external information and regularization. Our response about the issue can refer to the response #1 and response #5 for details. We have modified the relevant statements in our revised manuscript.