AC: We thank the Reviewer#2 for taking time to review this 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.

Reviewer#2

General Comments

1. The manuscript is well structured and starts with an overview on current comparable gravity field models, presents the methods used to derive the gravity field model, then describes the model characteristics and follows with internal and external evaluation and validation. Some figures can be made clearer so the information can be parsed easier. In my opinion, as this is a data description article, some parts of the methods and results section can be shortened while the model evaluation and validation can be expanded so that potential users get a clear picture of the model performance and characteristics. There are a number of experiments on certain aspects on the processing chain in the paper. It should be clear to the user what processing choices the final, published model comprises - here a concise description would help. The authors also do not present a clear use case of their model (i.e. why should we use their model for a specific application compared to already established data products). There are also some textual inaccuracies that need to addressed to provide proper context.

AC: Thank you for your insightful suggestion comments on our manuscript. According to your suggestions, we have revised our manuscript including replotting the figures and rephrasing the confusing sentences. As for the use case of our products, in our opinion, the long-term mean gravity field gives us a clear picture on how the gravity field shape over a long time period (i.e. a few years or a decade). Specially, our static gravity field is purely determined based on

GRACE/GRACE-FO observation data from 2002 to 2025, which can be regarded as the fundamental base for gravity field determination. In other words, the combined gravity field (i.e. GOCO series products) is also determined based on multi-source observation data, in which can surely achieve higher spatial resolution. However, the goal is achieved by the combination of GRACE/GRACE-FO data in fact. Our gravity field product is determined based on GRACE/GRACE-FO observation data. This can help us evaluate the contribution of GRACE-FO observation data easily and the contribution of LRI data especially, which is significantly different from the established data products.

Modified Sentence

In Introduction part (P3 L21-L23)

"… GRACE and GRACE-FO eras. In all, our model HUST-Grace2026s mainly focus on the construction of baseline for the long-time mass change variation research, bridging the gap of GRACE and GRACE-FO observation period, which is different from the current established static gravity field models."

In Conclusion part (P27 L11-L12)

"… thereby bridging the gap between these two satellite gravimetry eras and constructing a consistent baseline for long-time mass change variation studies based on satellite gravimetry observation data"

2. From a methodological point of view, I have two major issues which need to be discussed:

- The first one is the low maximum-degree of 60 for the co-estimated time variable coefficients. The authors even state that there is information in the observation data beyond degree 60. Truncating at such a low degree applies a hard constraint on coefficients above degree 60 which are subsequently fixed to the used background models. This introduces spatial aliasing and makes the model dependent on the background fields. I would argue that such an implicit regularization needs to be communicated more transparently.

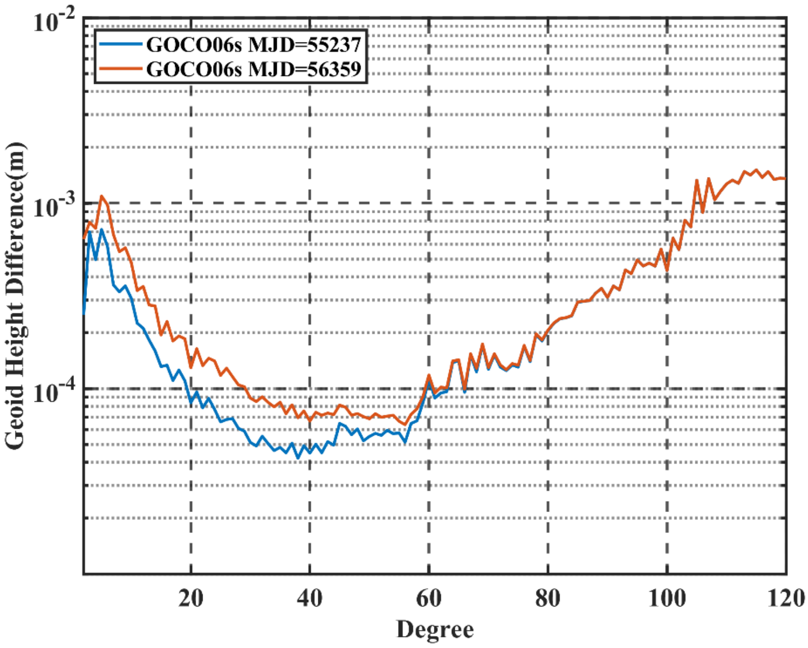
- Second, since the empirical covariance model has a major impact on the gravity field solution in the authors' approach, a more precise treatment of this processing chain component is crucial. Empirically derived (post-fit) covariance models depend on the parametrization, which should then obviously be consistent throught the processing chain. Especially time-dependent parameters such as N-CPR drastically affect the shape of the (stationary in time) covariance function. I realize that in practice some approximations are necessary, however, it would greatly improve the confidence in the model if this is presented in a strict and traceable way.

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.



Second, for the construction of empirical covariance model, we have to clarify something about the construction approach. During the gravity field determination process, we have two kinds of data, one calls "observation data" (i.e. the observation data derived from the satellite instruments onboard), the other calls the "stimulation data" computed by the background models (i.e. static background models such as GOCO06s, ocean tide model such as EOT11a or EOT20). Because of the imperfection of the background models and the temporal gravity signal in the "observation data", there are always some differences

between the "observation data" and "stimulation data", in which we can recover the interested gravity signal with the help of Least Square Estimation. During the gravity field determination, the empirical covariance model plays a role of the description of noise in both the "observation data" and the "stimulation data". That is the reason of we need to compute the post-fit residual after finishing the temporal gravity field determination. In other words, the more temporal gravity signal is determined during temporal gravity field determination, the more accurate estimation of noise. In all, the parameters estimated in our final solution are consistent during the derived post-fit residual process and the final solution process, that is no 1-CPR parameters used in our final processing chain.

During the post-fit residual computation in our manuscript, we determine a temporal gravity field at a degree of order 120, in which we can obtain most temporal gravity signal in the "observation data". After obtaining the post-fit residual series, we use it to construct the stochastic model (that is "empirical covariance model" in your comments). Except the change of gravity field parameter space to be estimated, the background models during the long-time mean field determination are consistent with the process with the temporal gravity field determination before. In other words, the addition of "trend and oscillation" parameters to be estimated during the long-time mean gravity field determination will not contribute to the computation of the difference of "observation data" and the "stimulation data", and it means that the construction of stochastic model is consistent with the noise during the gravity field determination. What's more, during the gravity field determination (especially for temporal gravity field), the post-fit residual represents the signal obtained in the final solution. In other words, if post-fit residual is smaller observed, the more signal is obtained in the final gravity field solution and the accuracy of the final gravity field is better. So, in our opinion, the construction of covariance matrix based on post-fit residual is meaningful.

Modified Sentence

P4 L11-L23

“The post-fit residual is derived during the temporal gravity field determination, in other words, less post-fit residual is observed, and the accuracy of temporal gravity field is better. Therefore, it’s reasonable that the stochastic model is determined based on post-fit residual in our manuscript. More details about the stochastic model will be described in Section 2.2. (3) Beyond the static parameter determination, we also estimate the oscillation and trend parameters to degree and order 60. Besides, we need to address about this: both the trend/oscillation parameters and the static parameters are estimated simultaneously as independent parameters without any prior constraints from background models. As for the truncation degree of trend/oscillation parameters, we also design an experiment using GOCO06s (See Supplement S1). The experiment is designed as we allowed its time-variable coefficients to evolve with time and then superimposed them onto the static coefficients and calculate the geoid height difference between the base model EIGEN-6C4 and the new geopotential coefficients derived from GOCO06s. The result indicate that the variation of geoid height difference curves mainly is up to degree 60, while the higher degrees remain unchanged. This demonstrates that the temporal signals have negligible power beyond approximately degree 60, and therefore truncating the time-variable estimation at degree 60 is a physically reasonable dimension reduction strategy rather than a hard constraint.”

P8 L16-L24

“During the gravity field determination, there are two kinds of data: (1) “observation data” is derived from the satellite instruments onboard; (2) “Stimulation data” is derived from background models. Because of the imperfection of the background models and the instruments onboard, the

"observation data" is different from "Stimulation data" leading to residual vector $\mathbf{l}$ as shown in Equation 错误!未找到引用源。. It's should keep in mind that the trend/oscillation parameters will not contribute to the calculation residual vector. During the post-fit residual computation in our manuscript, we determine a temporal gravity field at a degree and order 120, in which we can obtain most temporal gravity signal in the "observation data". The post-fit residual not only represent the noise of the background models and the instrument observation data, but also represent the accuracy of gravity field. It's reasonable to construct the stochastic model based on the post-fit residual."

Specific Comments

1. P2, L28/L29: GOCO25 -> GOCO2025s

AC: Thank you for your comments. Actually, the GOCO25 gravity field is different from the final products GOCO2025s. As far as we know, the GOCO25 is the preliminary product version of final product GOCO2025s. We use the GOCO25 in order to distinguish it from the final product GOCO2025s.

Reference

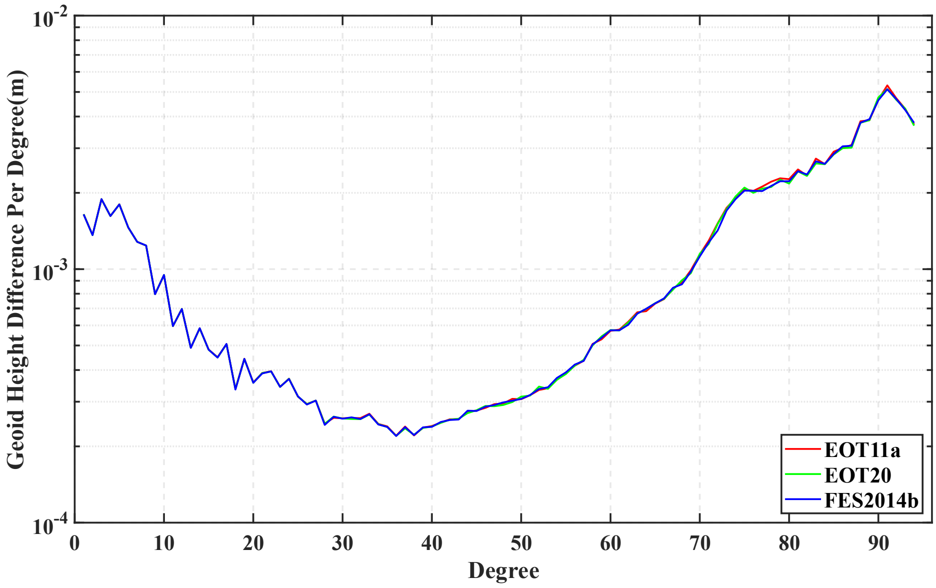
GOCO25: Öhlinger, F., Mayer-Gürr, T., Krauss, S., Dumitraschkewitz, P., Strasser, A., Süsser-Rechberger, B., and Tieber-Hubmann, C.: Towards a new release of the combined global gravity field model GOCO: Preliminary results, EGU General Assembly 2025, Vienna, Austria, 27 Apr–2 May 2025, EGU25-5676, https://doi.org/10.5194/egusphere-egu25-5676, 2025a

GOCO2025s: Öhlinger, F., Mayer-Gürr, T., Krauß, S., Dumitraschkewitz, P., Süsser-Rechberger, B., Strasser, A., Tieber-Hubmann, C., & Brockmann, J. M.: The satellite-

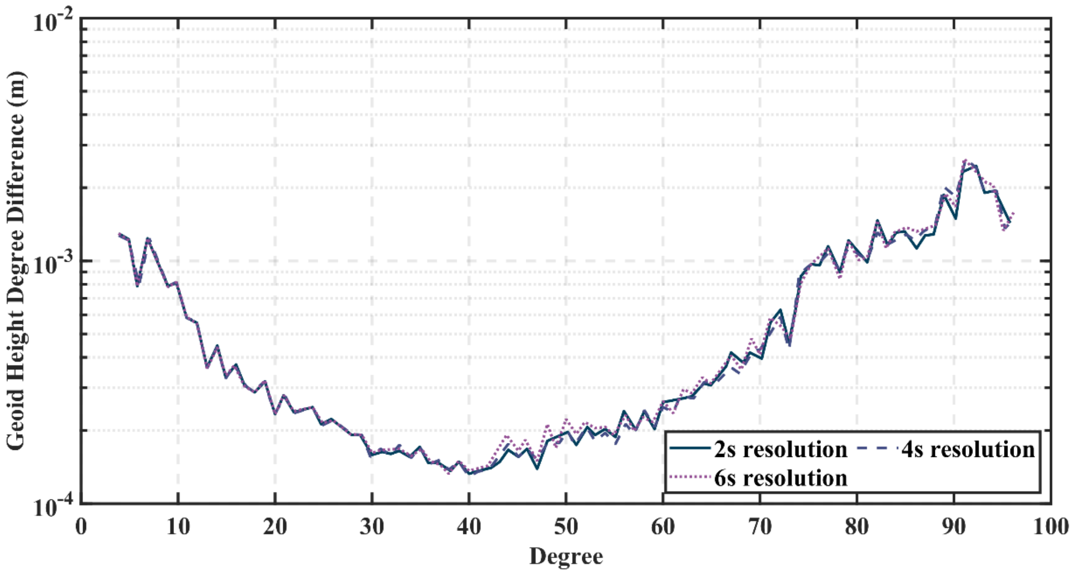
only gravity field model GOCO2025s [Data set]. Graz University of Technology. https://doi.org/10.3217/f48p8-8h651, 2025b

2. P4, Table1: EOT11a is quite an old tide model, have you evaluated more recent ones (e.g. FES2022, GOT 5.6, EOT20) and do you use admittance functions? I suspect that a modern tide model will have a large impact. Since you use 4 second LRI data: did you perform any low-pass filtering before down sampling? The maximum degree 60 for the parametrized temporal variations seems quite low. Have you conducted a post-fit residual analysis to see whether residual time-variable signal is present? Osulation -> Oscillation (also missing period/frequency); I guess the kinematic orbit reference should be https://doi.org/10.1016/j.asr.2022.03.014 as it is in the Data Availability section

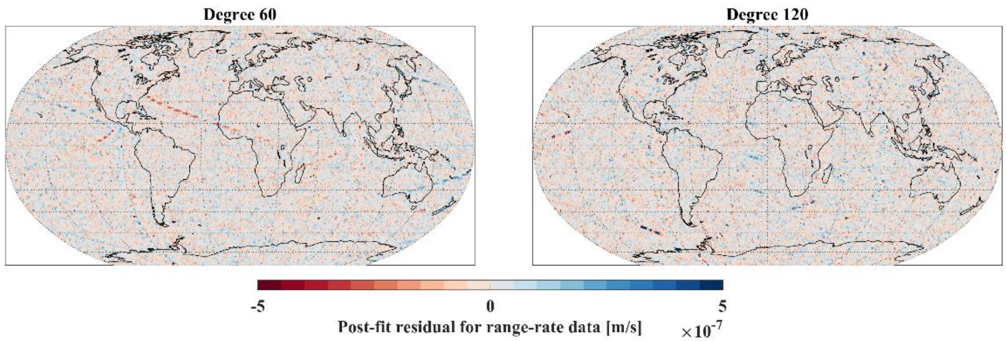
AC: Thank you for your comments. For the first part, we have evaluated the contribution of ocean tide model during the monthly temporal gravity field determination including EOT11a, EOT20 and FES2014b from January 2019 to December 2022 up to degree and order 96. (the experiment results not shown in the manuscript but shown as the following figure, the base model is EIGEN-6C4). We also use the admittance functions during the ocean tide force calculation process. The experiment shows the change of ocean tide model has a minor impact on gravity field determination, so that we still use EOT11a during the long-time mean gravity field determination.



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.



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.



For the fourth and fifth part, we have corrected the sentence in Table 1 and Data Availability section. "Static part" > "Static part over a solution period", "Osulation" > "Oscillation and Trend part yearly"

3. P5, L22/L23: How are the sensitivities computed? Please check the math notation: https://www.earth-system-science-data.net/submission.html#math

AC: Thank you for your comments. The sensitivities in P5 L22/L23 is the same as P5 L10, in other words, that is the contribution of orbital altitude for the geopotential coefficients. For a clearer definition, we have rephrased the sentence as below

Modified Sentence

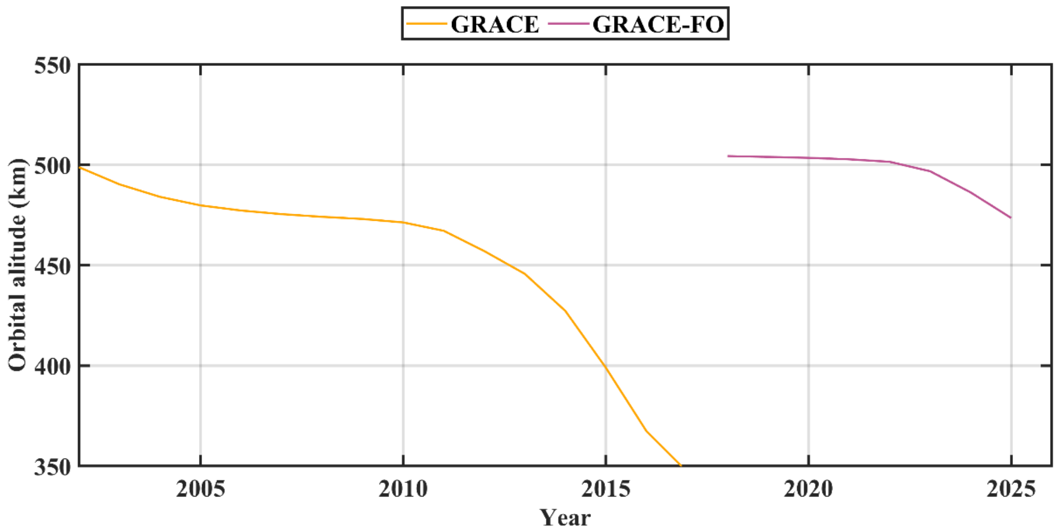
P6 L7/L8

"The progressive decrease in GRACE's orbital altitude led to a corresponding increase in the relative sensitivities of the geopotential coefficients (computed by $\left(\frac{R}{r}\right)^{l+1}$…."

4. P6, Figure1: y-label: Orbital altitude (singular); also please either align the y-limits or even better put GRACE/GRACE-FO into a single panel so the altitude can be directly compared.

AC: Thank you for your comments. According to your suggestion, we replot the figure and correct the error in the figure.

Replotted figure



5. P7: Please check the math notation: https://www.earth-system-science-data.net/submission.html#math

AC: Thank you for your comments. We have adjusted the math notation according to comments.

6. P8, L3 - L16 1) computational efficiency is very implementation specific:

I would not formulate such statements as general fact, but relate them to your approach/implementation.

2) post-fit residuals depend on the parametrization. You use a time-varible gravity field solution up to d/o 120 to determine the post-fit residuals. Your trend/annual parametrization goes up to d/o 60 beyond where time variable gravity field signal is certainly present. This mean that your stochastic model will be too optimistic and certainly not an optimal fit.

3) Using a correlation length of one month: A correlation length of one month is equivalent to a frequency spacing of ~4e-7. Is there a reason why such a frequency spacing is necessary? Shorter correlation lengths (coarser frequency spacing) and averaging within a certain time period (e.g. one month, one year, ...) increases the redundancy of the autocorrelation estimates and is certainly preferable.

AC: Thank you for your comments. According to your comments, we have realized that there is something wrong in our manuscript which makes you confusing and misleading. Let us explain point by point specific next:

(1) The static gravity field determination is a very time-consuming task. For instance, the static gravity field (i.e. only the static parameters estimated) with a degree and order 180 will cost 110 GB memory on a computation core. We need 2~3 months to finish the task based on GRACE/GRACE-FO observation data with the help of 10 nodes (using 2 computation cores per nodes, 250 GB memory per

nodes) in total on our in-house high-performance computer platform. If we extend the parameter space (i.e. the trend/annual parameters), the computation time will reach 6~7 month finally. We fully agree with some comments you mentioned, the computation efficiency actually relates our in-house software a lot. If we want to make our software more effective, we need to change our software fundamentally which will cost a lot time and we can't finish it shortly. (i.e the extension of trend and annual degree to 120 or higher)

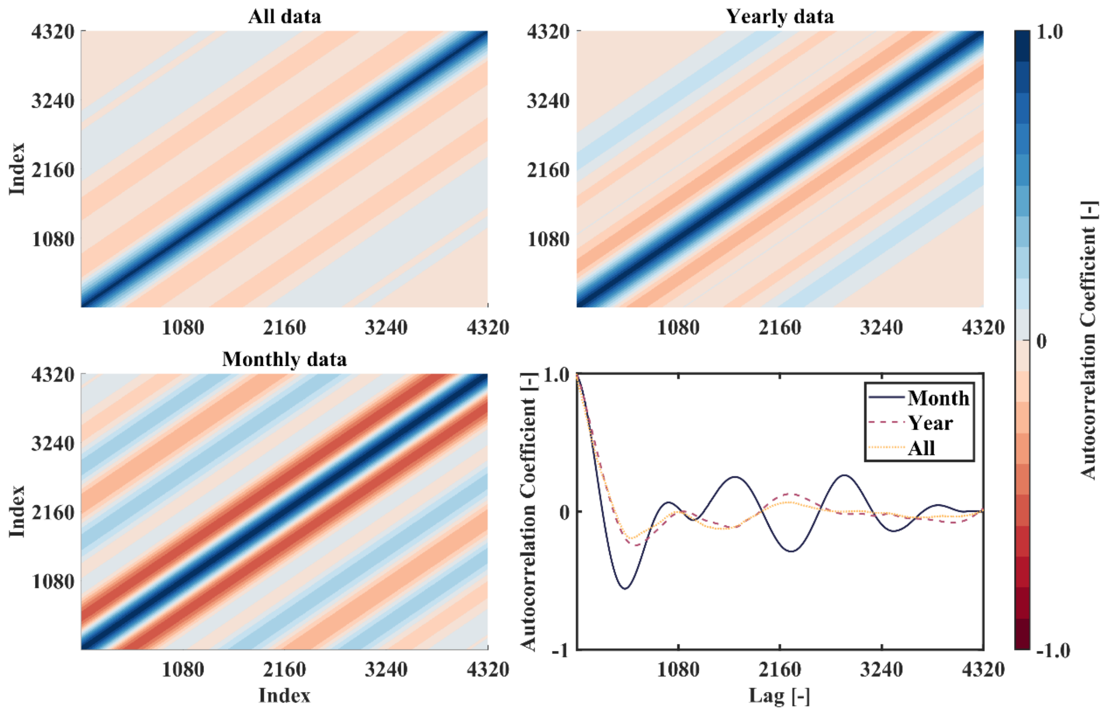
(2) During our gravity field determination, the post-fit residual is used to decorrelate the noise in the residual matrix l as mentioned in the response of general comments point 2. In other words, the residual matrix l remains the same between the temporal gravity field determination process and the final long-term mean gravity field determination process. Actually, the trend/annual parameters don't contribute to the residual matrix during the long-term mean gravity field determination. Our stochastic model is still fitted to the same residual matrix during gravity field determination.

(3) We are sorry to make a misleading sentence about this. The monthly/long-time period observation data is divided by arcs to build the normal equations. During the construction of normal equation, our stochastic model is built along with the solution arc (6 hour in our manuscript). That's to say our correlation length is actually 6 hour and average within a certain period (i.e. a month, year, or even longer) as your comments mentioned. We have deleted the misleading sentences in our manuscript.

7. P10, Figure3: Please make the correlation matrices square and use a divergent colormap with symmetric axis limits [-1, 1]. It would also help to have the lag in a time unit and a horizontal line at 0.

AC: Thank you for your comments. We have replotted the figure according to your comments.

Replotted figure



8. P11: Table 2: I find it hard to parse the numbers in the table. If you want to present the relative improvement, I suggest adding a panel to Figure 4 where this can be plotted for each degree and not only selected ones.

AC: Thank you for your comments. We want to present the relative improvement based on different strategies, but actually the improvement can also be revealed by the Figure 4 (original). So, we have deleted the Table 2 in our manuscript.

9. P11, L10: The term "Rate-terms" is somewhat misleading as you not only parametrize a rate but also an oscillation. I suggest something along the lines of "time-variable".

AC: Thank you for your comments. We have modified the title according your comments.

Modified Title

“Trend/Oscillation terms estimation and its impact”

10. P11, L11: Calling a gravity field representation with trend and oscillation "static" is a little bit confusing, since purely "static" (i.e. only a long-term mean field) models also exist. This sentence should be rephrased.

AC: Thank you for your comments. We have deleted the confusing sentence according to your comments.

11. P12: please check the math notation

AC: Thank you for your comments. We have checked the math notation and corrected the error.

12. P12, L6: Most (all?) recent models contain time-variable coefficients - please rephrase

AC: Thank you for your comments. We have modified the sentence in our manuscript according to your comments.

Modified Sentence

P12 L3-L4 “Most recent gravity field models published on ICGEM website contain trend/oscillation terms (Ince et al, 2019).”

13. P12, L9/L10: see comment above. computational burden is subjective since the parameter space does not expand drastically

AC: Thank you for your comments. The actually point we want to present is the higher degree of trend/oscillation terms to be estimated, the heavier computational burden to be considered if we don’t change our in-house gravity field determination software. We have modified the sentence in our manuscript according to your comments.

Modified Sentence

P12 L4-L5 “As the estimated degree of trend/oscillation terms increase, the heavier computational burden will need to be met”

14. P12, L10-L12: in the ITSG and GOCO models, all parameters are solved in one single step - not multi-tiered

AC: Thank you for your comments. We have modified the sentence according to your comments.

Modified Sentence

P12 L5 "To balance model fidelity and practicality, we implement the estimation strategy based on Kvas et al (2021) …"

15. P12, L12-L14: this sounds as though this is the representation of the GOCO models, which is not correct

AC: Thank you for your comments. We have modified the sentence according to your comments.

Modified Sentence

P12 L7 "Our solution strategies are set as follow: the static coefficients … are solved to degree/order 180, whereas the trend and oscillation coefficients…"

16. P12, L20 - L23: as stated above the empirical covariance matrix depends on the parametrization. If you introduce or change parameters the model no longer fits, especially with time-domain parameters such as N-CPR. This is critical since the impact of the covariance model is substational (cf. Fig 4)

AC: Thank you for your comments. We don't use range-rate empirical parameters to mitigate 1-CPR effect in range-rate data except for the "Nominal" solution, and the "Nominal" solution don't use the covariance matrix constructed based on the post-fit residual. In all, our solution parameters are consistent during the gravity field determination process. In order to classify the parameter strategies used in the solutions, we have modified the sentence in our manuscript.

Modified sentence

P12 L13-L18

"… The "Nominal" solution mitigates the 1-CPR effect solely with empirical range-rate parameters (no combined covariance matrix used in this solution) and no trend/oscillation terms are estimated. In contrast, the "Static" solution uses a combined covariance matrix for 1-CPR mitigation (no empirical range-rate parameters used in this solution) and no trend/oscillation terms are estimated. The "Trend+Static" solution also uses the combined covariance matrix (no empirical range-rate parameters used in this solution) and additionally estimates the complete set of trend/oscillation terms alongside the static field parameters …"

17. P13, L1: Using centimeters would make this more readable.

AC: Thank you for your comments. We have modified the sentence according to your comments.

Modified sentence

P12 L27 "…the cumulative geoid degree difference for "Nominal", "Static", "Trend+Static", and "HUST-Grace2016s" are 7.30 cm, 6.44 cm, 3.81 cm, and 12.60 cm, respectively…"

18. P13 L10 - L13: Since you use an empirically derived stoachastic model, AOD errors are also accounted for in your autocorrelation function. The issue is that a stationary (in time) covariance model cannot fully capture the AOD error (which are more stationary in space, see Kvas et al. 2019).

Note that the same is true for ocean tide errors (Abrykosov et al. 2023)

It would be good to discuss this either in the section on the autocorrelation determination or here.

AC: Thank you for your comments. We have modified the sentence in our manuscript according to your comments.

Modified sentence

P13 L4-L10

"… The result shown in 错误!未找到引用源。 also depicts an interesting fact: although the trend/oscillation terms are estimated up to degree/order 60 or the combination of covariance matrix, its contribution starts to be dominant beyond degree 100. This phenomenon may be attributed to high-frequency non-tidal signals, as observed in prior studies (Kvas et al., 2019). Although the error of background models (i.e. AOD1B model or ocean tide model) is remained in the post-fit residual, the covariance matrix is stationary in time domain and can't fully represent the error characteristic of AOD1B or ocean tide model which is more stationary in spatial domain especially. In other words, the contribution of covariance matrix to the gravity field determination is limited (i.e. the difference between "Static" and "Trend+Static" solution in 错误!未找到引用源。)."

19. P13, L15: since "rate term" is both used in KBR parametrization and time-variable gravity paramtrization, please introduce a distinct nomenclature (e.g., "time-variable" for gravity field parametrization as mentioned above)

AC: Thank you for your comments. We have modified the sentence according to your comments.

Modified sentence

P13 L15

“Modelling via trend/oscillation terms …”

20. P13 L22 - L23: 1) this is not correct (e.g. Kvas et al. 2019 and Abrykosov et al. 2023 use model uncertainty estimates) 2) the four presented methods work differently

AC: Thank you for your comments. We have modified the sentence according your comments.

Modified sentence

P13 L21-P14 L7

“… atmospheric and oceanic non-tidal signals mostly follow a 2 or 3-day period, and the daily solutions can significantly reduce the influence of these frequency-aliasing signals (Daras et al., 2017). This method addresses the aliasing source directly but depends on external hydrological models as drivers, making its efficacy sensitive to the quality of those input data.

3. Updating the background model: Besides estimating of the daily low-order geopotential coefficients, updating the background model is also an effective approach to reduce the impact of aliasing signal on gravity field determination (i.e. the updating version of AOD1B model, Shihora et al., 2022).

4. Using the model uncertainty estimates: From the view of stochastic model construction, the stochastic model represents the stochastic noise originated from background models and instrument observation data. In other words, the impact

of model noise can also be reduced by using the model uncertainty estimation to updating the stochastic model (Kvas et al, 2019; Abrykosov et al, 2023).”

21. P14, Figure 6: Please state the reference gravity field in the caption.

AC: Thank you for your comments. We have modified the caption according to your comments.

Modified caption

“… The geoid height difference and its cumulative result derived from static gravity field result with respect to EIGEN-6C4, which is determined based on different agencies’ LRI1B data spanning from 2019.01 to 2023.06 …”

22. P15 L18: One issue I see here is that KBR and LRI (also kinematic orbits) contain common noise sources (accelerometer and background models). VCE in this condition will not give you an optimal solution. This should be discussed.

AC: Thank you for your comments. There are many approaches to combine the gravity field on the normal equation level, especially the solutions derived from KBR and LRI data which is concerned by the researchers since the GRACE-FO mission has been operated. There are many researchers using the VCE approach to combine the gravity field derived from different data source. As the comments state, the optimal combination approach needs to be further discussed. In order to avoid overgeneralization, we have modified the sentence in our manuscript according to your comments.

Modified Sentence

P16 L10-L15

"… Although many researchers use the VCE approach to combine the gravity field derived from different observation data source, in our manuscript, the gravity field solution derived from KBR1B or LRI1B has the same error source (i.e. the noise from accelerometer, background models, and so on). In other words, whether the VCE approach is the optimal approach for the combination of gravity field solution from KBR1B and LRI1B still need to be further discussed. Especially, the discussion of optimal approach for combining the solution from KBR1B and LRI1B is out of this manuscript."

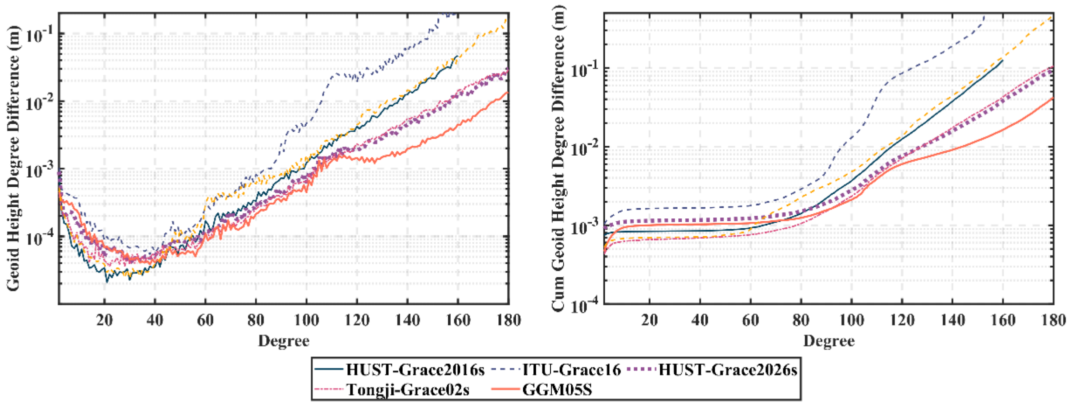
23. P16 L13-L14: it may also lead to degradation due to spatial aliasing

AC: Thank you for your comments. Due to the implementation of our in-house software, we can't extend the trend/oscillation to higher degree shortly. How the truncation degree will impact the gravity field determination still need to further researched. It's an important point to be discussed later on according to your comments.

24. P16, FIgure 8: Combine -> Combined

AC: Thank you for your comments. We have replotted the figure according to your comments.

Modified Figure



25. P18, Figure 10: since the "static-only" solution is not the final published model, this section can be condensed. I would rather see an analysis of the final published model characteristics (e.g., a contribution plot for the trend/annual coefficients)

P18, Figure 11: It would be interesting to see the contribution to the time-variable components (trend, annual). Also: a diverging colormap is not ideal for a sequential quantity, please adapt.

AC: Thank you for your comments. We have replotted the figure according to your comments and the addition of trend figure. Also, we add some notes for new result in our manuscript.

Modified Figure

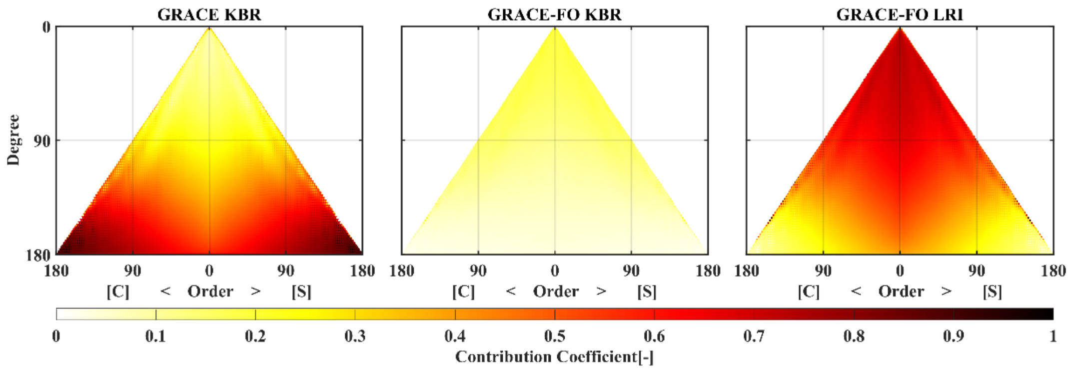


Figure 10 The contribution matrix derived from different normal equations, which are obtained from serval kinds of range-rate data for GRACE and GRACE-FO. Note that all results in this figure are estimated based on static-only parameters estimation strategy during the static gravity field determination.

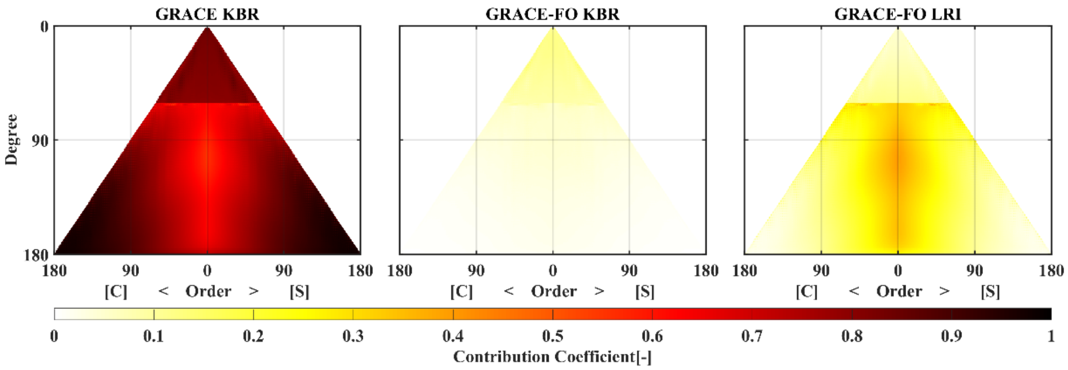


Figure 11 Similar to the Figure 10, but one should be noted that all results in this figure are estimated based on both static and trend/oscillation terms parameters estimation strategy during the static gravity field determination. Here is the contribution for the static parameters.

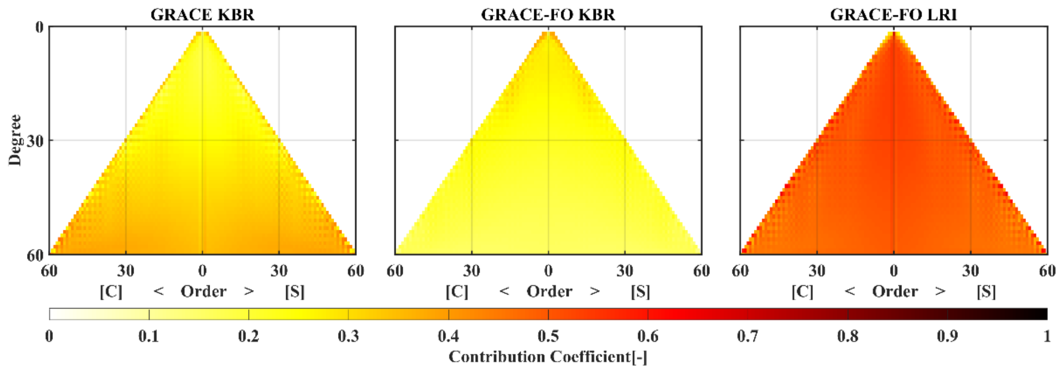


Figure 12 Similar to the Figure 11, here is the contribution for the trend parameters.

Modified Sentence

P20 L5-L11

“… In order to make further research on the change on 错误!未找到引用源。 and 错误!未找到引用源。, we plot the contribution matrix for the trend parameters as shown in 错误!未找到引用源。 (the contribution matrix for the oscillation parameters is similar to the trend parameters, but not shown in this manuscript). In this Figure, the contribution of GRACE-FO LRI for trend parameters is slightly higher than that of GRACE-KBR (i.e. 0.5 for GRACE-FO LRI and 0.2 for GRACE KBR). More importantly, the sum of contribution of GRACE KBR for static and trend parameters is near to 1.0. In other words, the contribution for static and trend parameters is not independent but correlated if no regularization information is introduced between static and trend/oscillation parameters during the gravity field determination.”

26. P20, Figure 12: Please add the reference model to the figure caption.

AC: Thank you for your comments. We have modified the figure caption according to your comments.

27. P21 L8: please specify extactly what external information is used

AC: 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.

Reference article

Save, H., Bettadpur, S., and Tapley, B. D.: High-resolution CSR GRACE RL05 mascons, J. Geophys. Res.-Sol. Ea., 121, 7547-7569, https://doi.org/10.1002/2016JB013007, 2016

Kvas, A., Brockmann, J. M., Krauss, S., Schubert, T., Gruber, T., Meyer, U., Mayer-Gürr, T., Schuh, W.-D., Jäggi, A., and Pail, R.: GOCO06s – a satellite-only global gravity field model, Earth Syst. Sci. Data, 13, 99–118, DOI: https://doi.org/10.5194/essd-13-99-2021, 2021.

28. P21, L9: Here it is worth to note that truncating the time-variable coefficients at 60 applies a hard constraint to coefficients above 60 which are then fixed to the background model (which makes your model dependent on the background model quality). This gives used a clearer picture of what is happening here.

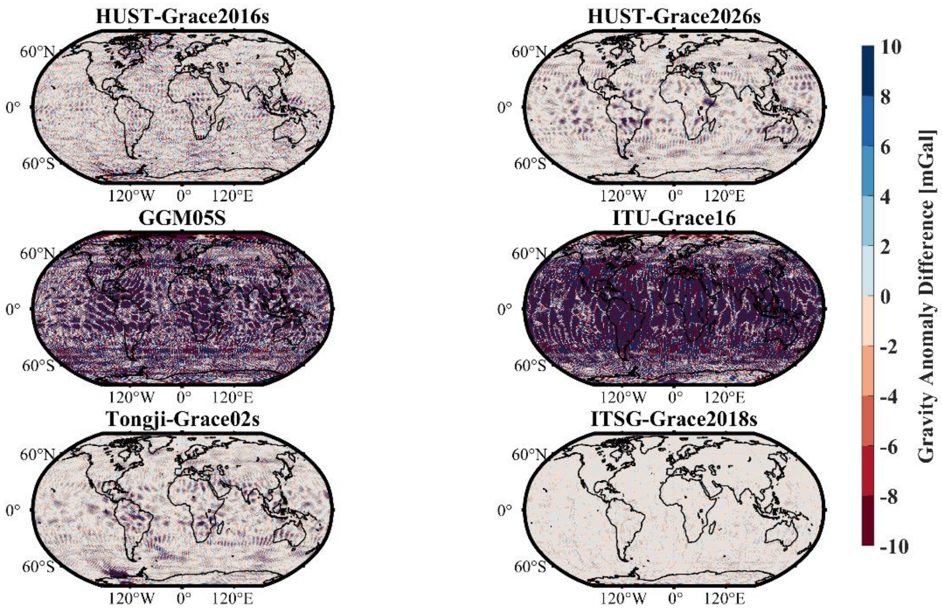
P21, L14 - L15: See comment above: truncating at an (in my oppinion) too low degree constrains all coefficients to the background model.

AC: 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.

29. P22, FIgure 13: Please do not use the Jet colormap (https://www.earth-system-science-data.net/submission.html#figurestables)

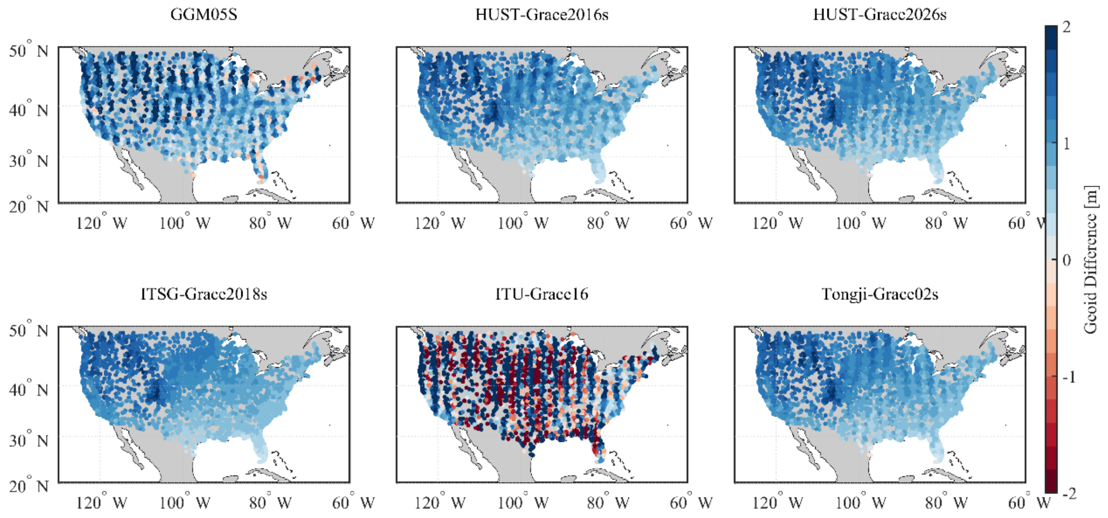
AC: Thank you for your comments. We have replotted the Figure according to your comments.

Modified Figure



30. P23, Figure 14: using a circular colormap is not ideal here. since these are differences with a well-defined mean, a diverging colormap is more suitable.

AC: Thank you for your comments. We have replotted the Figure according to your comments.



31. P26, L24: the static coefficients of ITSG-Grace2018s are not regularized, please be more specific

AC: Thank you for your comments. We have modified the sentence according to your comments.

Modified Sentence

P28 L7

" … our model shows an average noise reduction of 10–20%, performing closest to the unregularized static part of the solution ITSG-Grace2018s"

32. P27 Data Availability Statement: some links do not work, please check

AC: Thank you for your comments. We have checked the links in the Data Availability Statement according to your comments.