Solid Earth Discussions

Solid Earth Discuss., referee comment RC1
https://doi.org/10.5194/se-2020-201-RC1, 2021

Review of the manuscript se-2020-201: "An upward continuation method based on spherical harmonic analysis and its application in the calibration of satellite gravity gradiometry data"

Anonymous Referee #1

Referee comment on "An upward continuation method based on spherical harmonic analysis and its application in the calibration of satellite gravity gradiometry data" by Qingliang Qu et al., Solid Earth Discuss., https://doi.org/10.5194/se-2020-201-RC1, 2021

Dear authors and dear editor,

please find below the review of the manuscript se-2020-201.

General comments

The paper "An upward continuation method based on spherical harmonic analysis and its application in the calibration of satellite gravity gradiometry data"' by Qingliang Qu, Shengwen Yu, Guangbin Zhu, Xiaotao Chang, Miao Zhou and Wei Liu studies the effect of an external calibration of gravity gradients as measured by the satellite mission GOCE. For that purpose, ground gravity data products - i.e. global gridded datasets - are upward continued and converted to gravity gradients in the reference frame of the gradiometer on-board the GOCE satellite. For the change of functional and the upward continuation of the ground gravity reference dataset, the authors propose a conversion to a spherical harmonic series. Within data windows of a fixed length, biases and scale factors are estimated to externally calibrate the measurements with respect to the reference dataset. The trace-free condition as well as rotational invariants I_2 and I_3 are used to access the quality of the calibration. The authors validate the calibration computing the trace reduction as well as the consistency of the higher order invariants with respect to the derived reference gradients.

In the current version of the manuscript, I see major issues in all main parts, i.e. the Introduction and Motivation, the Methods section, the Results and Discussion part as well as the final Discussion of the results and the derived Conclusions. For that reason I have

to suggest to *reject* the submitted manuscript.

In the first part (i.e. 1. Introduction) I am missing the motivation for the study. It remains open, why there is a need for new, respectively alternative, calibration procedures compared to the existing and published approaches. Although there have been several major updates in GOCE L1B gravity gradient processing, which might result in different conclusions for the external calibration compared to the existing published studies, the authors do not address the recent GOCE L1B updates and do not relate their study to the improved L1B data.

The methods section summerizes the upward continuation via a conversion of global gridded gravity data sets to spherical harmonics, for which well established procedures exist. The benefit of using global gridded data set, compared to directly an existing spherical harmonic model, remains open and is not shown. Furthermore, the presentation of the conversion to spherical harmonics is erroneous, there are some inconsistencies and errors in the presented equations. For the applied model for the external calibration, it is not explicitly stated how this relates to the existing and published approaches. Comparisons are missing.

The numerical results presented in the Results section rely on a single reference dataset and a single part of the time series. Consequently, the dependence of the results from the chosen reference dataset remains open, such that it is quite hard to draw conclusions. The numerical test of the upward continuation is not representative for the dataset used in the numerical analysis (in Sect. 3.2), as the real data set includes correlated data and systematic errors. Confirmation of the results with different datasets would be required to interprete the results correctly and reliable. Furthermore, it is not totally clear which version of the actual GOCE data were used.

The discussion and validation of the results is not clear. On the one hand, it is shown, that the trace-free criterion improves for the calibrated gradients. But, the other two chosen validation approaches are - from my point of view - not independent. If I understand the applied procedures correctly, the comparison using the TRF data is very similar to the first trace analysis and no additional data is involved. The higher order invariant analysis is - from my understanding - heavily biased, as consistency to the dataset used for calibration is checked. It is obvious and expected that the calibrated gradients are more consistent to the reference data compared to the un-calibrated gradients.

Finally, in the Conclusion chapter I am missing the actual conclusions. In the current form it is more a summary of the manuscript.
But what follows from the performed study? Are the conclusions and results of the older studies still valid? How will the calibrated data be used? Are there jumps in the calibrated gradient time series?

To address this points more directly, please find attached some more text specific

comments to emphasize/justify my overall impression.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-201/se-2020-201-RC1-supplement.pdf