Anonymous Referee #1

Review about the paper

GOCO06s – A satellite-only global gravity field model

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Authors: Andreas Kvas, Jan Martin Brockmann, Sandro Krauss, Till Schubert, Thomas Gruber, Ulrich Meyer, Torsten Mayer-Gürr, Wolf-Dieter Schuh, Adrian Jäggi, and Roland Pail

General remarks:

This paper describes the generation and the characteristics of the satellite-only gravity field model GOCO06s. The computation of this model was done in a joint work by the partners of the GOCO consortium. GOCO06s has been already published last year for the user community on the ICGEM data base and on the GOCO webpages. This paper now accompanies the publication of the GOCO06s data set and will help users to understand the background of this model. GOCO06s is one of the best global satellite-only gravity field models currently available and it benefits especially from the matter of fact that data from practically all recent gravity satellite missions were included and combined via a refined combination technique. I appreciate especially that the authors published along with the model very valuable supplementary material comprising the full variance-covariance matrix of the static spherical harmonic coefficients and estimated co-seismic mass changes. The content and the outline of this paper is convincing for me. The paper is written in good English wording. I recommend only minor revision according to my specific remarks below:

AC: We thank the reviewer for their insightful comments, which helped us to identify parts in need of clarification and undoubtedly allowed us to improve the quality of the manuscript. Below is the point-by-point response to the specific remarks.

Page 2, lines 34/35:

- The statement “Starting with the dedicated CHAllenging Minisatellite Payload satellite mission … different LEO satellites were tracked by GPS” is not 100% correct. There were LEO satellite missions before CHAMP with GPS receivers onboard, for instance Topex/Poseidon. CHAMP was the first satellite gravity mission with a GPS receiver onboard. Please modify this statement accordingly.
- Please correct this typo**: Reigbar** et al. 1999 → **Reigber** et al. 1999, please correct this name also in the reference list.

AC: We rephrased the sentence to:

“The CHAllenging Minisatellite Payload satellite mission (CHAMP, Reigber 1999) was the first dedicated gravity field mission tracked by GPS. Since then multiple other dedicated and non-dedicated satellites equipped with GPS receivers have been used derive static and temporal gravity field models of the Earth (…) in this so called satellite-to-satellite tracking in high-low (SST-hl) configuration.”

To correctly state that there were GPS-tracked satellites before CHAMP and fixed the typographical error in the reference.

Page 2, line 48:
The reference Battrick 1999 is right. Nevertheless, I suggest to give a more recent reference in addition to Battrick 1999, for instance Floberghagen et al. 2011:
Rune Floberghagen, Michael Fehringer, Daniel Lamarre, Danilo Muzi, Björn Frommknecht, Christoph Steiger, Juan Piñeiro, Andrea da Costa: Mission design, operation and exploitation of the gravity field and steady-state ocean circulation explorer mission, J Geod (2011) 85:749– 758, DOI 10.1007/s00190-011-0498-3

AC: We added the more recent reference to Floberhagen et al. (2011) in addition to Battrick (1999).

Page 3, line 83 – 85:

The statement "Basically, GRACE normal equations processed by GFZ (Dahle et al., 2019) are combined with GOCE normal equations assembled with the so-called direct approach (Bruinsma et al., 85 2014; Pail et al., 2011) and SLR normal equations." is not fully correct, since some of the GRACE and SLR normal equations used for the DIR-models were generated by CNES/GRGS. I suggest to modify this sentence as follows:

Basically, GRACE normal equations processed by CNES/GRGS (Bruinsma et al. 2010) or GFZ (Dahle et al., 2019) are combined with GOCE normal equations assembled with the so-called direct approach (Bruinsma et al., 2014; Pail et al., 2011) and SLR normal equations."

The additional reference is:
Bruinsma, S. L., J. M. Lemoine, R. Biancale, and N. Vales (2010), CNES/GRGS 10-day gravity field models (release 2) and their evaluation, Adv. Space Res., 45(4), 587–601, doi:10.1016/j.asr.2009.10.012

AC: We rephrased the sentence and added the appropriate reference.

Sections 2.2 and 2.3:

I see a contradiction regarding GRACE between the sections 2.2. and 2.3:

In section 2.2 the GRACE contribution is described. Here among others it is said: "The normal equations feature a static part parametrized up to degree/order **200** and **secular and annual variations up to degree/order 120**…"

In contrast, in section 2.3 GRACE is mentioned again, but here as one of the LEO satellites and you write: “For CHAMP, GRACE, TerraSAR-X, TanDEM-X and SWARM we set up the normal equations up to degree and order **120**…” and “For all LEO satellites, **only the static** gravity field was modeled…”
I think, GRACE is mentioned in section 2.3 by mistake. Therefore, you should remove GRACE from section 2.3. Then, in section 2.2 some sentences about the used GRACE kinematic orbit positions should be added, similar to such for the other LEOs in lines 143 – 145.

AC: Thank you for this observation, indeed the parametrization of the GRACE LEO orbits was not properly presented. To provide a clear picture, we amended the introduction to section 2.3.

Section 2.2, 2.3 and 2.4:
You estimated temporal gravity variations for GRACE and SLR only. But its known that other satellites like CHAMP, GOCE (here the long wavelengths only based on the kinematic orbits) and Swarm were also sensitive for temporal gravity field variations. You mentioned this in principle in the introduction. Do you think it makes sense to include both satellites here to improve the estimates of the long wavelengths, for instance in a future upgrade of GOCEO06s? Could you please add some sentences about this question e.g. in section 2.3 or in the introduction?

AC: The decision to not model temporal variations for the LEO orbits was made from the perspective of computational demands vs. impact on the solution. As you rightly state, the kinematic orbits provide information for temporal gravity field recovery and with an ever-growing data record through currently active and planned missions it is an interesting observation set. It is therefore certainly worth revisiting the inclusion of temporal variations from LEO satellites in future reprocessings.

We added the following sentences to clarify our motivation to not include temporal variations for the kinematic LEO orbits:

“We made the decision to not include secular and annual variations in the parameter set based on practical considerations. Even though SST-hl has skill in determining the time-variable gravity field, the expected contribution of the LEO orbits to the temporal constituents, with the presence of GRACE intersatellite-ranging observations and SLR, will be low. This results in a steep trade-off between the increase in solution quality and the computational demands required to set up the additional parameters.
“

Page 7, line 174:

I suggest to give the obtained relative weights. This would be of interest to others users which are processing SLR data.

AC: We added a Table with the obtained weights for the 10 processed satellites.

Page 8, formulae (11) and (12):
Could you please also give the relative weights applied for GOCE, GRACE and the LEO satellites?

AC: The GOCE, GRACE and LEO orbit contributions to GOCO06s feature a sophisticated stochastic model of the input observables. This means that the resulting normal equations already properly describe the accuracy with which each technique can determine the potential coefficients in an absolute sense. The weights determined in the VCE therefore do deviate from 1 only in the magnitude of 10^{-4} even without any pre-scaling. We added the sentence:

"Consequently, the weights of these contributions remain close to 1 throughout the VCE iterations."

to better reflect this circumstance.

Page 8, lines 201/202

You said, "… the SLR system of normal equations was artificially down weighted by a factor of 10 in each iteration step." Did you try other factors for down weighting? If you used only one factor you can never be sure that your empirical down weighting factor is optimum. Please comment.

AC: We determined the factor by analyzing the differences between the SLR solution and GOCO05s (which also contains an SLR solution). The gravity field signal cancels out and the remaining differences contain the noise of both solutions. This indicates the order of magnitude of the true errors compared to the formal errors. We agree that such a manual weighting scheme is not ideal and might result in a sub-optimal solution in a statistical sense. Ideally, one would model the normal point observation noise as it is done in the GRACE, GOCE and LEO contributions, which is something worth revisiting in future iterations.

Page 15, line 320 and Figure 7:

"… evaluated close to the epicenter of the 2004 Indian Ocean earthquake…" Please give the location of this point. Is it east or west of the fault line? Please give the precise geographical coordinates ore indicate the position in the map of figure 6a.

AC: We added the geographic coordinates to the Figure caption and additionally included an overview map with the point indicated to the Figure itself.

Figure 7:

The caption for this figure is inaccurate since the location is not mentioned. I suggest to write: "Comparison of estimated secular variation from GOCO05s, GOCO06s (including estimated co-seismic mass change) and filtered GRACE monthly solutions in terms of equivalent water height (EWH) **for a location close to the epicenter of the 2004 Indian Ocean earthquake (c.f. figure 6a)**

AC: Please see the response to the previous remark.

Page 16, lines 331 – 341

Your discussion about figure 8 is partially not clear for me:

You statement “The other large differences between the compared models in degrees below 60 are primarily explained by the respective reference epochs (for example, 2010-01-01 for GOCO06s and 2010-09-01 for GOCE DIR6)” is a little bit misleading: Even the spectra of Goco06s and GOCE DIR6 are closely together below degree 60. Please discuss e.g. the difference between GOCEO06s and GOCE TIM5 which is significantly larger.

AC: You are fully correct, not only the reference epoch but also the used observation techniques play a role in this frequency band. The GOCE TIM models rely on GOCE kinematic orbits and gradiometer observations only and therefore cannot reach the noise level of combination models which use GRACE intersatellite ranging observations, which are far more accurate in this frequency band. To correct for this oversight, we restructured this part of the manuscript to:

“The differences between the compared models in degrees below 60 are on the one hand explained by the respective reference epochs (for example, 2010-01-01 for GOCO06s and 2010-09-01 for GOCE DIR6) and, on the other hand, by the used observation techniques. GOCE TIM6 only relies on kinematic orbit data and gradiometer observations, which do not reach the superior accuracy of the GRACE intersatellite ranging observations in this frequency band.”

Your statement “Concerning the GOCE gradiometer reprocessing we can see improvements from degree 150, where these data start to dominate the solutions….” is unclear for me, since the difference spectra of all five compared models are almost the same. Please explain better what you mean.

AC: To better highlight the differences between the models in the higher degrees, we added a second panel to Figure 8 which only shows this frequency band. Furthermore, we rephrased the corresponding text in the manuscript:

“In Figure 8(b), we can also identify differences between the solutions starting from degree 150. Here, we can clearly distinguish between models (GOCO06s, TIM6, DIR6) based on the latest reprocessing of GOCE gradiometer data (version tag 0202) and models based on the previous release (GOCO05s, EIGEN-GRGS RL04).”

Page 17, figures 8 and 9:

- In the captions of both figures it’s said that “8 degree polar cap (are) excluded” in the degree variances. This statement is unclear for me. Does it mean, that you start to exclude polar gap components beyond degree 8 up to the maximum degree? What is in detail excluded, some low order coefficients? Please explain. I suggest to explain the exclusion of the polar gaps for the spectra within the text of chapter 4 and not in the captions.

- Beside of the polar gap comment I ask you to modify the captions as follows:

Figure 8: Difference degree amplitudes of various state-of-the-art satellite-only models to the combined model XGM2016

Figure 9: Difference degree amplitudes to GOCO06s (solid lines) and the corresponding formal errors (dashed lines) for the individual GOCO06s components…

AC: We clarified our intentions behind the use of the polar cap and added an appropriate reference. Specifically, we changed the caption text of Figure 8 to

"Difference degree amplitudes of various state-of-the-art satellite-only models to the combined model XGM2016 (polar cap with 8° aperture angle excluded)."

and added the following text to the manuscript:

"Because of the GOCE orbit inclination of 96.7°, no data is collected directly above the poles. This polar gap has a distinct mapping to certain low-order coefficients of the gravity field (Sneeuw and van Gelderen, 1997). Consequently, these coefficients are highly correlated and less accurately determined in GOCE-only models, such as GOCE TIM6, where no other gravity field information is used. To avoid these low-order coefficients dominating the degree amplitudes and to ensure a consistent comparison, we excluded the coefficients corresponding to a polar cap with 8° aperture angle in all models, according to the rule of thumb given in Sneeuw and van Gelderen (1997)."

In Figure 9, we changed the caption text according to your suggestion and added the same information about the polar cap as in Figure 8:

"Difference degree amplitudes to GOCO06s (solid lines) and corresponding formal errors (dashed lines) of the individual GOCO06s components (polar cap with 8° aperture angle excluded). SLR is not shown because no standalone solution can be computed due to the ill-posed adjustment problem."

Page 18, lines 372 – 374:

"In addition, an independent GRACE/GOCE satellite-only combination model namely GOCEDIR6 is used for comparisons, which to a large extent is based on the same amount of GRACE and GOCE data as GOCO06s (Förste et al., 2019)"

This sentence is inaccurate and the place of the mentioned reference is disadvantageo. I suggest to rewrite as followes:

"In addition, an independent SLR/GRACE/GOCE satellite-only combination model namely GOCE-DIR6 (Förste et al., 2019) is used for comparisons, which to a large extent is based on the same amount of SLR, GRACE and GOCE data as GOCO06s."

AC: We rephrased the sentence according to your suggestion.

Page 2, Table 2:

Which maximum degree was used for the various Earth's gravity field models? Please give the number. It should be the same in all cases. If not, please discuss.

AC: All evaluated gravity fields were used up to degree 200. We added this information into the force model table (now Table 3).

The item "annual/secular variations" and the related model is given in brackets there. Why?

AC: We added an indentation, so it becomes clear that this is a continuation of the previous table line.