

We would like to thank the reviewer for accepting reviewing our paper and submitting his comments within the scheduled time. We appreciate the detailed comments and feedback with high level of expertise which very much helped to improve the paper. In general, we agree with most of the comments and we will further apply or have already applied suggested changes in the revised version. Also, there were parts in the paper where we were uncertain about some ideas, such as whether to add the list of the models in the Appendix and the reviewer just naturally provided us feedback on such items as well. We would like to thank him for his time and respond below to his detailed comments. The reviewer's comments are indicated in italic fonts whereas our responses are given in bold.

Detailed Comments

1. Introduction

The list of references in this section seems to be a little bit arbitrary. There are many other publications addressing the various items, which sometimes would be even better suited as they would be more of a review type. I would recommend to go through the list again and may be to cite mostly review papers. As a minimum I would add "e.g." in order to indicate that these are just examples of papers from a larger selection.

In general we tried to refer to the most fundamental (e.g. first of its kind, most cited) publications in the relevant topics but I could see this can be better linked in this section. We scanned through the references carefully and removed some of the references and introduced others that are more of review kind. The revised version should be more consistent in terms of the reference list.

Page 2, line 21 to 32: I would recommend to mention the gravity gradients and measurements of non-gravitational accelerations right before mentioning the satellite missions (i.e. at line 24). Otherwise it sounds like these measurements are not taken from the satellites. Then one could continue after the satellites with: "Other fundamental datasets are terrestrial gravity measurements from moving platforms . . . and collected on the Earth surface.

We have moved the "gravity gradients and non-gravitational accelerations" to the previous sentence and modified as recommended.

Page 4, line 9: I think the references to Drewes and Toth are not required here.

The content of Drewes and Toth are from ICGEM and we thought they would be good links to the available documents; but they could be removed as well which we did in the revised version.

2. Background of the ICGEM Service

2.1 History of ICGEM

Page 5, Figure 2, Caption: Please make clear that compared to the satellite models the EIGEN6-C4 is based also on terrestrial data. May be re-phrase the following sentence: ". . . . Note that the EIGEN6-C4 is not the truth but a better approximation to the real gravity field, because it includes terrestrial and altimetry derived gravity field information". Last sentence: I think it is EGM96S (and not EGM96).

We have revised the caption indicating clearly the content of the EIGEN-6C4. Thank you for your suggestion, indeed the model is EGM96S and not EGM96 which is also modified in the caption.

Page 6, Line 1: Currently? Please indicate a date because numbers are changing .

“By January 2019” is added.

Page 6, Line 10: Delete last part of the sentence: “. . . and promises future developments”.

Removed.

2.2 Scientific Background and ICGEM’s Data

In general I think this section should be shortened and reference to the ICGEM documentation should be made. It is impossible to completely write the geodetic gravity theory with all details in such a paper. I think the main purpose should be an explanation that global models are represented by spherical harmonics.

This was a previous discussion we eventually had while writing this section. We agree that the potential theory cannot be given in such a paper as complete. However, while writing the paper, we thought we would like to provide at least the equations which can explain the link between the global models and spherical harmonics. In order to provide a smooth introduction and transition later, we thought including the basics of the Newton’s law of gravitation would help to explain the potential theory. Thank you for commenting on this. Not to lose the focus of the paper and to make it more readable, the revised version has this part reduced. We only kept a few formulations representing the widely used functionals and their representation in terms of spherical harmonic coefficients.

Page 9, Line 6-7: Update sentence: “. . . . pure gravitational forcesthe Earth’s gravitational attraction (V) and potential of the centrifugal force due to Earth rotation”.

Updated.

Page 9, Line 21 to Page 10, Line 7: I think this is not needed here (including equ. (19 to (3). Instead reference to the ICGEM documentation or another book shall be made.

We removed the first 3 equations and referred to the Advanced Physical Geodesy Text book and Scientific report from Barthelmes, 2013.

Page 11, Line 12: Find a better wording. Proposal: “. . . normalisation is defined such that the average square . . .”.

Applied.

Page 13, Line 7-8: The geoid is introduced. In my opinion the definition is a bit misleading. Undisturbed in my view doesn’t include the MDT because it is a permanent disturbance and therefore the statement is not correct. I would write as follows: The geoid is an equipotential surface that in average approximates the mean sea surface.

The formulation of geoid is kept simple as recommended and only the equipotential surface is mentioned in the revised version. References are provided for the interested readers.

Page 13, Equation (10): I would recommend to write equation (7) and (10) in the same way.

Written as recommended.

Page 13, Line 24: Why can these quantities be computed approximately? The calculation is correct, just the models are incomplete. Gravity disturbances can be computed exactly, while for geoid one needs to do assumptions. Please re-phrase.

It is true that the gravity disturbance can be computed exactly. However, here in the paper we introduced the disturbing potential and summarized that the disturbing potential is used in the calculation of some of the functionals such as geoid which can be done only via approximations (especially for the grid calculations). Gravity disturbance however can be computed from W and U (not T), exactly from spherical harmonic coefficients with no approximation introduced. We clarified this in the text to avoid confusion.

Page 14, Line 5: “above the geoid” instead “over”

Indeed, applied as suggested.

Page 14, Equations (11) and (12): I don't think this is needed here. Just refer to the manual. Otherwise one should provide equations for all derived quantities.

We think having these two equations used in the calculation of two main functionals may provide good references for the rest. How the coefficients are included in the computations may be summarized with these two.

2.2.1 Static global gravity field models of the Earth

Page 15, Line 4: EGM2008 expansion to degree and order 2159 is for ellipsoidal harmonics. After conversion to spherical harmonics the expansion is up to degree 2190. Modify the sentence accordingly.

Thank you for reminding. Corrected.

Page 15, Line 9: Write: “As an example one of the high resolution . . .”

Modified.

Page 15, Line 18: Is the gravity attraction really stronger? This depends where you observe it. If you stay on the equipotential surface the gravity attraction becomes smaller because the neighbouring equipotential surfaces are separated by a larger distance. Please rethink this sentence and be more specific.

Maybe our explanation was not clear. The point of observation has to stay exactly the same, which would not be possible anymore under the circumstances introduced (e.g. switch on). Explanation of the features is quite sophisticated as one can imagine; therefore, we decided to remove the above mentioned part and simplify this paragraph, and refer to the dedicated study about the Indian Ocean geoid instead.

Page 15, Lines 19-24: This is confusing and in my view, specifically the sentence about the North Atlantic (why only there?). Please rephrase and leave out unclear statements.

This part has been removed from the text. The same principle applied to other areas as well, but North Atlantic was chosen as an example.

Page 17, Line 5: “over land” not “in the land”.

Applied.

2.2.2 Temporal global gravity field models

Page 19, Line 4: I think monthly gravity field models do not provide a resolution of 160 km. May be only when looking to the maximum degree of the SHS but not in the sense of real data content. Please make this clear.

Indeed ~160 km would only be possible with the highest degree/order available which is in fact not the case in practice due to the increasing error of the higher degree/order coefficients. Therefore, this part has been revised and ~300 km for monthly solutions is used instead.

Page 19, Line 16: It is not only water mass, but could also be geophysical signals (solid Earth).

The geophysical signals added in the sentence.

Page 20, Line 9: Reference Wahr, 2007 is missing.

Thank you for noting this; the reference is added to the list.

2.2.3 Topographic global gravity field models

Page 21, Figure 10, Caption: a) and b) is not indicated in the sub-plots. Instead write left and right.

Letters are brought to the front, now they are visible.

Page 21, Table 1: Monitoring sea level variations is a temporal gravity field signal if the pure mass variation is meant. If just the geometric change of the sea level is meant it is no gravity field signal at all. Please correct.

Correct also the Atmosphere section (no underlining).

We have replaced “level” with “mass”. We don’t mean to go into details with steric and non-steric sea level changes. Underline in Atmosphere is also removed.

3. Services of ICGEM

3.1 Calculation Service

Page 23, Line 7-8: I think the semi major axis is missing here.

Thank you for noticing and noting this. Radius corresponds to the semi-major axis here, we added this information within parenthesis in the text. We will eventually need to replace it in the service as well since this may cause confusion.

Page 23, Lines 19-27: This section is quite confusing and I would recommend to rephrase it in simpler words as it is not very clear to non-experts. May be it would be again sufficient to refer to the ICGEM manual.

Some reductions are applied for clarity purposes.

Page 25, Table 2: For second_r_derivative one could also write vertical gravity gradient. This is a more convenient name.

Added in parenthesis. However, in fact it is not the same for the computations introduced in the ICGEM Calculation Service. “Vertical” (plumb line approx. by normal direction) is not radial (spherical approx.). Therefore, second_r_ direction is an approximation of vertical gravity gradient.

Just a proposal for future development: Why not offering also the horizontal gravity gradients. These might be useful for some purposes.

This has been in our to-do-list. We also added this to the future work in the paper. However, we suspect that providing this in ICGEM is not as easy as it sounds. Some standards (e.g. definition of horizontal, which coordinate system to be used) need to be clarified from the users point of view. The authors would be happy to hear about what would be interesting for the community.

3.2 Visualisation Service

Page 31, Figure 16, Caption: For b) I think it should be written “. . . Represent the mass change.” Instead of “distribution”.

Applied.

3.3 Evaluation of global gravity field models

3.3.1 Model evaluation with respect to other models in the spectral domain

Page 32, bottom: wrong font

Replaced with the correct font size.

Page 33, Figure 17: I think the green line are the “Cumulative difference amplitudes . . .”. Please correct

This terminology was recommended in the past, but for the geodetic community indeed the cumulative would be more familiar. In the revised version, it is edited as recommended.

3.3.2 Model evaluation with respect to GNSS/levelling derived geoid undulations

In my view this chapter either needs to be significantly extended or its value is very limited as the procedure to do comparisons with GPS/levelling geoid heights is much more complicated as it is done here. Therefore the numbers provided in figure 18 are not really meaningful, e.g. the omission error is not considered at all. In the last paragraph of page 34 the authors even explain that this is not a fair comparison. So why do they show it or why is it offered in ICGEM at all? There are also missing references to publications dealing with GPS/levelling comparisons. I would consider to delete the complete paragraph and even to consider not to offer this in ICGEM as long as it is not a fair comparison. It only provides misleading results to the not experienced user.

We agree with reviewer’s concern on the contribution of GNSS/levelling evaluation to the use of ICGEM Service. Taking into account the omission error, this method can for sure be applied in more sophisticated ways. However, in general, ICGEM would like to compare the models among themselves (e.g. among satellite-only models, among combined models) and does not seek for absolute comparisons for particular degrees. Therefore, we think this kind of comparisons still provide useful information of different models wrt the same external datasets. We try to make use of the GNSS/levelling series we have been delivered in the past which was scanned for outliers initially. We think the evaluation provided in this section is still valuable and should be kept in the ICGEM which may be improved in the future. References concerning the GNSS/leveling evaluation are added in the revised version.

Appendix 2: I am not sure if this is really needed. Why not setting a link to the web site with the models.

This is something we could not decide in the beginning. In the revised version we removed the list of the models.