Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2017-46-RC1, 2017

Earth System Science Data
Open Access Discussions

Interactive comment on "Altimetry, gravimetry, GPS and viscoelastic modelling data for the joint inversion for glacial isostatic adjustment in Antarctica (ESA STSE Project REGINA)" *by* Ingo Sasgen et al.

Anonymous Referee #1

Received and published: 18 August 2017

Geodetic measurements in Antarctica measure a combination of Glacial Isostatic Adjustment (GIA) and snow and ice thickness changes in Antarctica. Combination of the different data sets in an inversion approach might be the best method to isolate the different components. Such inversion imposes requirements on the data sets. This paper presents analysis of data sets (altimetry, GPS, satellite gravity) and GIA model outputs to convert between the observables. The products can be used in an inversion to separate the different components which is done in a separate study. However, the data sets can also be a useful resource for studies relying on one of the data sets. It is com-

mendable that the authors have put great care in processing the data and making the results available. It will be a very useful resource for Antarctic mass balance studies. I reviewed an earlier version of the manuscript and I appreciate that comments from that review have been addressed in the current manuscript. There are in my opinion still several minor issues related to the description. The paper does not make sufficiently clear in the introduction what the processing adds to previous studies and what is required of the data sets to be used in the inversion in paper II. Such explanation would guide the reader of this lengthy paper. Given that the main aim is to present 'data inputs', the descriptions of processing and errors is sometimes ambiguous. I hope the specific comments below help to improve this.

Specific comments

74: the statement that forward models overpredict uplift rate measurements is not generally true, there are regional models that are tuned to the GPS data and there are instances where standard models underpredict observed uplift rates, see Wolstencroft et al (GJI 2015)

102: How are the response functions used in combining them?

Introduction: The introduction states that the data sets and modelling results are of value to address other research questions. But the paper itself does not yet contain a research question. In addition, it is not clear form the introduction why the processing is better than previous analysis of the data sets. For example, would you expect improvement compared to Thomas et al. (2011) or are differences merely 'small processing strategy changes' (line 272). It should also be summarized what requirements the inversion poses on the data and kernels, for example in terms of time period, resolution, error (= weight in the inversion). Such explanation would help the reader evaluate the (many) choices that are made in the manuscript.

121 and further. More information is given on the corrections, which is helpful, but not yet what the error in the corrections is (or if it is insignificant) and if it is added to the

height error.

138: ‘residual uncertainties’ is confusing as it sounds like the residual of the uncertainties? In any case it does not correspond to equation 1, which gives non-dimensional values as both e and x have the same unit. Also, it should be discussed why residuals are a good approximation for errors.

160: ‘the standard deviations of the rates’. Are they also calculated according to equation 1?

208: Errors could be important in the inversion to weigh the contribution from the different data sources. Neglecting model uncertainties because estimates are not available is not really a satisfactory solution.

243: This is the first time this data set is mentioned. Does it include error estimates?

339: Do you have any explanation for the difference?

361: What is the threshold and how did you weigh the average? This manuscript present data sets and their analysis so the procedure should be clear.

470: it is not clear what is meant. Is the search range for the parameters limited? Is the range of m limited to values higher than 10?

476 and further, it is confusing to use both interannual and non-linear because they can seem the same but are not necessarily so.

404 “zero difference”: better to write a full sentence here.

495: “the post-fit RMS residual for this known temporal signal variation”. This is not clear. In line 449 the residual is defined as GRACE minus ice elevation, fitting is not mentioned. figure 5: the axis label states ‘linear trend residuals’, but the text in page 503 states that also annual oscillations are removed. Please make the descriptions consistent.

509 and further. The procedure seems OK but the reasoning does not make sense. if you downweigh months with high post-fit RMS the post-fit RMS decreases. That seems to me a mathematical certainty and in that case it should not be used to say that the downweighting is beneficial.

514: What is meant by more accurate? A higher RMS when you include noisier months is still an accurate representation of the noise.

Section 4: it is not clear to me what is done with the signal corruption due to Swenson and Gaussian filtering. Is that added to the error? Or will the filtering be applied to the other datasets in the inversion? Line 924 states that there is no magnitude bias (in the geoid rate?), but that would suggest that filtering is not really necessary

594: it would help the reader to be more clear about why you need the response kernels in the inversion. Only in the conclusions on line 846 it is mentioned that you need the kernels for ratio of gravity and displacement.

631: Does the range span the values in the Priestley and McKenzie 3D viscosity model that you use later?

645: 10ˆ22 is quite low to be considered fully elastic. Such viscosity would still give noticeable response from ice load changes since the last glacial maximum.

657: make clear that it is the standardized ratio (i.e. it starts at 1)

658: according to appendix A.6 it should be 1/eˆ2

section 5.5: another assumption(mentioned in the appendix in line 932) is that the equilibrium has been reached. If load changes constantly, then at present you are not in a state of equilibrium with constant displacement rate. This is mentioned later but could also be added here. Another assumption is that upper and lower mantle viscosity are assumed known.

733: e-dot was used for geoid rate in line 673

842: The response functions in the paper are produced for a continuously changing load. It is not yet possible to draw conclusions about the exact timing of the load from that.

846: the ratio should be for rates, not the gravity disturbance itself.

848 and further: this is an important justification that should be mentioned in the introduction as well

935: and on elastic parameters and density

Typos etc

81: grammar ‘And thus to’

95: grammar, change ‘invasion’

115: I suggest adding this to the acknowledgements instead

150: ‘the’ before ICESAT

figure 1: when zooming in I see many different colors. That might be the result of lower resolution picture, but it makes it hard to interpret the colors described in the caption

caption figure 2: space before sigma

213: should it be 20 km grid?

219: typo? something wrong with the degree symbol here and further on

228: abbreviation should be introduced

229: typo?

246: kg/mˆ3 instead of km/mˆ3

302: can refer to section 3.2.2

caption Table 4: “Table Appendix A.4”

370: provide link?

533: expanded ‘to’

593: remove ‘a’

601: add ‘are’ before ‘a classic’

648: considered

649: parameter

801: compositing?

807: terms

813: change ‘over’ to ‘about’

834: ‘however’ implies a contradiction, I don’t see that

table A.2, better to write approx in full.

figure A.4 and text use both mm/a and mm/year

figure A.5 axis label: standardized

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