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Solid Earth Discussions

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Interactive comment

Interactive comment on "The long-term GIA signal at present-day in Scandinavia, northern Europe and the British Isles estimated from GPS and GRACE data" *by* Karen M. Simon et al.

Anonymous Referee #1

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General comments

Simon et al. present a semi-empirical model of the glacial isostatic adjustment (GIA) signal in northern Europe, the British Isles and the Barents Sea. It is generated in a least-square adjustment method with the help of data of the Global Navigation Satellite System (GNSS) and the Gravity Recovery and Climate Experiment (GRACE), and additional input from GIA models. It is the first such model for this large area and is assumed to represent the GIA signal mainly induced by the last glaciation. The method has been used before by the main author and Hill et al. (2010) but for other or smaller areas. Data sets with longer time spans and interesting corrections are used to provide a model with realistic uncertainties, which are missing for most GIA models. Finally, the model is compared to other models and its performance for correction of tide-gauge observations in the North Sea is tested.

In principle, I would recommend publication of this study. However, there are many points and suggestions below the authors should work on before I would give my thumbs up. They are at a level where I suggest major revision as some reading and rephrasing is necessary. Despite the English is fine, the figures have high quality and the paper reads well at first glance, I identified some sloppiness and to be frank, the acknowledgement of related works on GIA in the investigated area is at a very low level. It appears to me that the authors consider many facts as known to everyone and thus refrain from explaining abbreviations and referencing whole paragraphs. The authors should also more discuss the reliability of GRACE in semi-empirical GIA models. The weakest part of the study is tuning GRACE results to fit expectations, and in the end the model is to less than 5% constrained by GRACE. It would be nice to read a discussion on how important these less than 5% are.

Specific comments

Title: I have four issues, (1) Scandinavia is part of northern Europe but you miss to mention the Barents Sea, which is largely discussed in your manuscript, (2) abbreviations should not be used in the title except they are well known, (3) although I understand that you want to distinguish the GIA signal you investigate from current climate change-induced GIA signals, “long-term”, i.e. its definition, is not the best word to me (see below), and (4) you estimate your signal also with help of GIA models. My suggestion would be: The glacial isostatic adjustment signal in northern Europe, the British Isles and the Barents Sea estimated from satellite positioning and space gravimetry data, and geophysical modelling

L11/L29-31: it seems “long-term” refers to “ice sheets… during the last glaciation” in your introductory lines 29-31. However, parts of this signal can result from previous

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glaciations, see e.g., Johnston & Lambeck (1999) and Root et al. (2015). This should be either specified or the word “long-term” be dropped.

L12: suggest change GPS to GNSS and introduce abbreviation; GPS is one of the GNSSs like Galileo, GLONASS or BeiDou. You can specify in the main text that both Kierulf et al. and Blewitt et al. use GPS only.

L12: explain GRACE abbreviation

L12: delete “Scandinavia,” (or do you mean northern Central Europe? – but my suggestion would be simply “northern Europe” which includes Scandinavia)

Introduction, i.e. L29-40: this is a rather short introduction that combines a paragraph without any references to a paragraph with references but already specifically focussed on the paper’s topic. I suggest mention a few “early” general studies on GIA in the first paragraph, e.g. Peltier & Andrews (1976) and Wu & Peltier (1982). Otherwise it sounds that GIA should be well known for the reader. A reference for the 1 cm/a and the location should be added. I am aware that GIA in Fennoscandia has been extensively studied so that it may be hard to find a good balance in summarizing previous work, however, there are a few review books/papers/reports that summarize many works (Whitehouse, 2009; Ekman, 2010; Steffen & Wu, 2011). These should be the backbones for another paragraph between the two on a brief overview of GIA (investigations) in Fennoscandia.

L32f: I suggest remove “that are tectonically quiescent”. They are thought to be but there was and is more activity than quiescence, see e.g., Lindblom et al. (2015) and Lund et al. (2017).

L43: introduce abbreviations

L50: I miss Müller et al. (2012) in the references, although they call it land uplift model, while mentioning the study by Zhao et al. (2012) does not seem to fit. They used GNSS data for determination of the subsurface structure.

L52: northern “Central” Europe

L61f: please add how many velocity results are taken from those two papers, respectively. Did you use all stations from Kierulf et al. (2014)? Note that especially the many in Norway have short time spans and thus their velocities should be used with care. I would have advised to use only those with at least 5 years of observations and your results in Fig. 8 (top) show differences for many Norwegian stations. Might be that these are the newer stations. Also note that Kierulf et al. point to possible neotectonics along the Norwegian coast emerging in the velocities which should be picked up in the discussion/conclusions.

Figure 1: the LGM margin is not correct in Denmark, Germany and Poland when compared to Figure 1 in Hughes et al. (2015); also mention that Iceland was glaciated but ice extent is not shown (or cut figure)

L80: Please provide an overview of the 31 common sites and their values. Which station shows the large difference?

L82: Which uncertainties from Kierulf et al. (2014) did you use? The ones from GAMIT/GLOBK are indeed very low but the authors also provide uncertainties from the time series analysis using CATS where a combination of white noise and flicker noise was assumed. The latter should be preferred in a modelling analysis.

Section 2.2: Did you add the degree-1 estimates for GRACE?

L94: There are quite large uncertainties in the higher degrees, especially from degree 60 and higher. Does the Wiener filter leave high-degree signals at all? If not much is left your spatial resolution is much lower.

L98: What about the aliasing effects from tides, see e.g., Ray et al. (2003)? Were these considered?

Section 2.3: I partially miss some information! How are the corrections calculated? Are they spatially variable in each region? What method is used to calculate the elastic signal in terms of vertical deformation from a mass balance signals? What time steps are used when acceleration is included? What (earth) model (if any) is used to calculate the elastic signal from current ice melt? You explain the input and the result but all intermediate steps are missing!

L117: Please add that the model considers anthropogenic changes only (as in the conclusions). I wonder why you do not use a global hydrology model like WGHM, which appears to perform OK in northern Europe (see e.g., Wang et al., 2013).

L119: Please specify the glaciated regions, i.e. in Scandinavia. I suppose you do not consider whole Scandinavia as a glaciated region. I also wonder if Scandinavia has glaciers of 2x2 degrees grid size so that the hydrological model has such large gaps? Jostedal Glacier is the largest with 487 km2 – much smaller than a 2x2 degrees grid.

L124ff: Please state in the beginning that you use and discuss published estimates. When reading I had the impression you do all the modelling yourself.

Table 1: I would like to see the detailed contribution from anthropogenic hydrology and glaciers for each area.

L152: altimetry results are also corrected for a GIA effect from GIA models, and those models might be erroneous. It's a bit chasing your own tail. See also Tamisiea (2011).

L162: Please refer to Fig. 2c.

L171ff: This is the weakest part of your study. You are not satisfied with your results as you expect something different. So, you basically tune your corrections, which are an average of published studies, until you end up with a result that you consider reliable based on your expectations. But what if the expectations are wrong or the real situation is much more different from the expectations. Wouldn't it be better to adjust the uncertainty for GRACE based on the, as it appears, rather problematic corrections? It might be that you end up suggesting the GRACE results as not feasible for usage in the semi-empirical model development due to all these issues and likely large uncer-

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tainties which would also allow a large range of suitable a priori models. As a matter of fact, Fig. 6 and Table 2 clearly show that the best result is mainly constrained by GNSS data. In the combined solution D3 the GNSS data are much more weighted than GRACE. The contribution from GRACE is minor and much less reliable. Hence, the question arises if GRACE should be used at all in this study and if this point is one of the main conclusions of this study!

L196ff: I have difficulties to acknowledge such large corrections especially due to Greenland mass loss knowing that there is a plate boundary in between where already parts of the GIA signal are altered, see e.g. Klemann et al. (2008). Note that I do not question the value which I assume is based on a simple 1D elastic model. Is the value the upper bound or average of different models tested? What earth structure did you use to calculate the effect? Is it similar to the a priori model that best fits your observations?

L212: “mass loss” or “mass changes” or really “mass loss changes” (due to acceleration)?

L223: Just wonder why ICE-5G is used while the new ICE-6G_C is available for quite some time now.

L237ff: The second paragraph which comes without references. Please check Steffen & Wu (2011) for a list of Fennoscandian GIA model parameter and consider the studies by Zhao et al. (2012), Kierulf et al. (2014), Root et al. (2014), Schmidt et al. (2014 (they also use ANU as you do!)), Nordman et al. (2015) and Root et al. (2015 – for the Barents Sea). Please also refer to some literature for a few words on GIA models and Earth parameter for the British Isles and the Barents Sea.

L237: Why do you use a fixed lithosphere and why 90 km? See references above which partly show quite different best-fitting values. Also, Steffen & Kaufmann (2005) point to differences in the lithosphere thickness in each of the regions you investigate (British Isles, Norwegian coast, Gulf of Bothnia, Barents Sea), further subdivision of (parts of) northern Europe was done in Lambeck et al. (1998) and Steffen et al. (2014).

L239ff: Have a look into Nordman et al. (2015) who discuss this issue.

Table 2: Please check if the ratios are correctly calculated.

L329ff: There appears to be a misunderstanding and much information is misleading. It is important to go through the existing documentation (http://www.lantmateriet.se/globalassets/kartor-och-geografisk-information/gps-och-matning/referenssystem/landhojning/presentation-av-nkg2016lu.pdf). NKG2016LU is **not** a GIA model, it is as its name says a land uplift model. The observations are not corrected for any motion such elastic contributions from Greenland ice melt, hydrology, tectonics etc. The underlying GIA model is tuned to relative sea-level (RSL) data in northern Europe and GNSS data (with 80% weight on RSL data!), and is used as a gap filler in those areas where observations are not available, i.e. in the Baltic Sea. Hence, on land NKG2016LU represents the observed land motion - which has a very strong GIA component, of course. In addition, one should note that NKG2016LU is quite reliable in Fennoscandia (Norway, Sweden, Finland, Denmark) and performs well in the Baltic countries, but is not much reliable in Germany, Poland and eastern Europe as they are no or just a few observations (both for the semi-empirical and constraining the underlying GIA model). NKG2016LU largely relies here on the GIA model but which is tuned to give the best fit to the observations in Fennoscandia and the Baltic countries. The southern and eastern parts of the model are of less importance for the developers. As an interesting test, the NKG2016LU model could be on land treated as observation where the corrections of this study could be applied, and then used in the least-squares adjustment.

L353ff: Of course, the bias is 0.42 mm/a as NKG2016LU is the total observation where no elastic correction has been applied!

Section 3.4: I like this comparison as it nicely shows an important application. However, I wonder why you pick the North Sea where the GIA contribution is small. Would have been nice to see how the model performs in the Baltic Sea and along the Norwegian coast. I also note that in the documentation of NKG2016LU a comparison to tide gauges has been made for Fennoscandia and the Baltic Sea.

Conclusion: I wonder what the best-fitting Earth structures are for you models. Are they like those used in the generation of ICE-5G and ANU? Although your model is discussed as a data-driven you should mention and discuss how much the contribution of the a priori models is in the final models. According to Table 2 it is quite large at the level of the GNSS data.

L419ff: What implications has this for the results of Root et al. (2015)?

Where can your model be downloaded?

L595-598: Please add the website path for the model, http://www.lantmateriet.se/sv/Kartor-och-geografisk-information/GPS-och-geodetisk-matning/Referenssystem/Landhojning/

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