

## ***Interactive comment on “Simulation of the time-variable gravity field by means of coupled geophysical models” by Th. Gruber et al.***

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The paper presented by Gruber et al is well written, well documented and clearly organized. The authors present an interdisciplinary study aimed to outline a global predictive model of the time variation of the gravity fields of the Earth. A realistic model of the changing during the time of the gravity field has a leading role in simulation studies on sensitivity of satellites. The proposed analysis is based on a coupling of different geophysical models correlated to different domains of the Earth. The final predictive model was obtained combining and converting in spherical harmonic series gravity field each individual model. The paper shows a clear organization with a good balance among the different sections. I enjoyed the paper and I think that it is worthy for the publication. I have only a few minor suggestions.

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Specific comments:

In the introduction the authors should include a few sentences about the existence, in the literature, of predictive model of the time variation of the gravity field similar to the one presented in this paper.

In the introduction a brief description of GRACE is needed. The authors should include a few part of text about the mission and the spectral resolution of the GRACE data

In the section 4:

The authors stated that the time series of surface mass balance (SMB) derived from the ECMWF data. At the contrary Van den Broeke et al., 2009 (also cited in the manuscript) used a different dataset for estimating of the SMB in Greenland. I would like to know why the authors have choice a different dataset.

Section 6.1 Global Isostatic Adjustment:

In this section the authors described the effect of the melting and the retreat of the late-Pleistocene ice. This effects is important in term of gravity anomalies and the response of the retreating of the ice shield provides important information about the physical properties of both the lithosphere and asthenosphere. The variation in term of heigh of geoid is strictly dominated by the nature of the crust and the viscosity of the mantle. This concept is very important and it needs a more exhaustive description. In addition, the authors should explain better which crustal-mantel model (in term of thickness and viscosity) they have used in the model represented in figure 10.

The authors have briefly introduced the concept of stokes coefficient. I would like to know if the authors have introduced in the stokes computation the effects of Secular trend in Earth rotations as recently proposed by Roy and Peltier (2011, GRL, Volume 38)

Technical corrections and minor remarks:

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section 2 "Atmosphere", page 31: re-analysis or reanalysis

section 2 "Atmosphere", page 32: line 2, ....our Software..., What kind of software is it ??please describe this software adding some references (if it is possible) section 3.1 "Adaptation of model for this study", page 33: line 27, after ....FAO digital soil map of the word ... an appropriate references is needed

Figures:

Please reformat all the figures (maps and plots) using a common style of the fonts. In addition, the labels and axis annotations of figures 5,11,12 and 13 are very small and they are almost unreadable.

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