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Interactive comment on "Comparing a thermo-mechanical Weichselian ice sheet reconstruction to GIA driven reconstructions: aspects of earth response and ice configuration" *by* P. Schmidt et al.

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This is going to be an important paper, primarily for its component of thermodynamical ice modelling. It's to be acknowledged that ice modelling is intertwined with the solid earth response for two reasons; once, to get reliable modelling conditions for the topographic height (and thereby the temperatures) when/where ice is precipitating and controlling its melting and rheology etc.; second, to constrain mass and extension from solid surface deformation. For the latter, as to ice mass, it's been a staple to employ relative sea level and land-emergence data. For some years it has also become possible

to use contemporary surface motion inferred from GNSS networks, offering better coverage of land areas, higher resolution and eventually higher precision than terrestrial-only techniques. Unfortunately, the capability to resolve stages of ice accumulation and melting in time is very limited, so GNSS won't be but one observable in this context. A potentially strong contraint may come also from the Newtonian gravity pull and (dominatingly) elastic response of the ocean exerted by the ice mass. Observations of sea level fall during melting along e.g. the Norwegian coast might help. Of course, this is difficult because of simultaneous glacial dynamics elsewhere on the earth, a comprehensive analysis would be tedious. The appearance of the paper should not be delayed due to a request for further investigation. However, some remarks on present limitations and future work would be very helpful. And it would raise the merit of the paper for future reference.

I have a critical remark on the use of vertical-only GNSS rates (Bifrost data). Actually two remarks. The second first: The *most recent solution* should be used (Lidberg et al. 2010), since the history of some of the stations that came on lately was increased significantly. The first remark concerns the neglect of the horizontal rates in the ms's discussion. Although the comparison of model versus observation might not be successful, and the conclusion might be that the constraining power on ice mass and/or lithosphere+mantle properties is limited, it is still worthwhile to show and discuss this in a paragraph. In the present manuscript the problem is not made visible nor plausible. The data is there, not to use a part of it appears like cherry-picking (subjective, unconvincing). Milne et al. (2001) and Milne et al. (2004) show that horizontal rate information helps to disentangle the model parameters for mantle viscosity and lithosphere thickness (Bifrost data has become better since). If the modelling procedure has problems in producing horizontal rates of displacement, this should be mentioned (and arguments presented why the vertical rates would still reliable). (I'm not certain whether this is the case; however, the ms could make a clarifying point here, regardless of the circumstances.)

In summary, I suggest documentation and discussion in the paper on details as to why horizontal motion is of limited value, regarding aspects, the ice model and the solid earth model. Gratefully, I also acknowledge off-line correspondence with the first author which helped clarify my understanding on other matters of the ms.

Interactive comment on Solid Earth Discuss., 5, 2345, 2013.