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Interactive comment on "A fully coupled 3-D ice-sheet – sea-level model: algorithm and applications" *by* B. de Boer et al.

P. Whitehouse (Referee)

pippa.whitehouse@durham.ac.uk

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General Comments

The submitted manuscript by de Boer et al. outlines a significant advance in our capability to model multiple glacial cycles on a global scale. Driven by the benthic δ18O record, de Boer et al. model global ice sheet changes over the past 410 kyr. The novel aspect presented in this manuscript relates to the fact that the influence of spatially-variable sea-level change on ice dynamics is accounted for via the coupling of an ice-sheet model with a glacial isostatic adjustment model. This is the first time that such feedbacks have been included in the reconstruction of global ice sheets over multiple glacial cycles, and, as such, this manuscript represents a major step forward in this field.

The submitted manuscript largely consists of a description of the technical aspects related to the model coupling; however, the preliminary results that are included provide tantalizing evidence that the consideration of realistic sea-level variations can have a significant impact on the evolution of a marine-grounded ice sheet, such as Antarctica. This result has previously only been explored in a couple of publications, and the study presented here considers a much larger temporal and spatial scale. As the authors rightly state, this manuscript therefore provides the foundation for some important future work.

The manuscript is well-organised and well-presented, and the content seems to be scientifically sound, but there are several aspects of the method that require clarification and I highlight these in my 'major points' below. There are a few areas where I feel that the modelling could be improved upon (if I have understood the methods correctly), but given the advance that is already made in this work, I think it sufficient that the authors simply highlight any deficiencies and justify their choices rather than trying to improve on their methods at this stage. The provision of supplementary information containing various subroutines relating to the coupling process was very useful in fully understanding the details of the method.

After my 'major points' I include a longer list of 'minor points', and finally also attach a pdf of the submitted manuscript in which I have highlighted a number of typographical errors. Despite the number of points I raise, I do not consider any of them to require 'major revisions'.

Major points

1. Page 3509, lines 21-27: There are a lot of ideas introduced in this paragraph, however, not all are clearly explained (e.g. the jump from bathymetry to precipitation and ice flow on line 25), and several provide the fundamental motivation for the advances presented in the manuscript. In particular, the link between relative sea-level (RSL) changes and ice-sheet dynamics warrants a much more detailed explanation. High-

lighting the importance of the processes that are neglected in current models will lead nicely onto the following paragraph.

2. Time-stepping is referred to in a number of different ways and it is not always clear whether you are referring to the time steps used within the individual components of the model (SELEN and ANICE), or whether you are referring to the frequency with which information is passed between the two components. I think it is usually the latter, but you only formally refer to 'the coupling interval' on page 3520; prior to this it is often just referred to as 'the next time step…' or 'at each time step the array is updated…'. It would be very useful if you could use specific terminology for the coupling time step, and also clarify early on what the coupling time step is (perhaps in the paragraph where you first refer to figure 2, on page 3515). As an aside, have you explored the sensitivity to the coupling time step?

3. Are any of the details of the standalone ANICE and SELEN models different in the coupled version, perhaps for the purposes of reducing computational time? I see that a 2-layer mantle is shown in Figure 5, while a 3-layer mantle is described in section 2.3 – which one is used in the coupled model? Could you also clarify whether the coupled version of ANICE accounts for solid Earth deformation (e.g. as described in section 2.1), or whether solid Earth deformation is only allowed in the SELEN component of the coupled model. The Earth models in SELEN and ANICE are very different, so allowing both models to respond to surface load changes would lead to some confusion.

4. Please clarify what information is passed from SELEN to ANICE. In different parts of manuscript it is variously referred to as RSL change or topography change, and it would be useful if you could clarify that, for your purposes, these are essentially identical quantities.

5. It is mentioned that ANICE is spun up for 1 glacial cycle in the uncoupled mode (page 3515, lines 19-20). What forcing (e.g. climate, sea level) is used during the spin-up? Is the solid Earth deformation component of ANICE switched on during this spin

up (I think it is, but this is not clearly stated)? Does it matter that the ANICE models are only run for part of the world; does this introduce a step between regions where the topography is updated and regions where it isn't?

6. In GIA models that I am familiar with, a subroutine is included to ensure that the final solution reproduces present-day topography, and this is used to 'back-calculate' palaeo-topography through time. However, I think you just run your model forward through time without imposing any constraints related to present-day topography. Despite this, how well does the final output of your model compare with present-day topography?

7. Please make it clear in all calculations of eustatic sea level whether temporal changes in ocean area are accounted for, or not. From your discussion of Figure 9 I think ocean area changes are accounted for in the results from the coupled model, but not in the results from the uncoupled runs. It is interesting that the differences in ice volume and ocean area between these two models lead to a trade-off such that the modelled eustatic functions are almost identical (page 3522, lines 13-15). Is this an artefact of the inverse model that partitions the benthic δ18O signal into ocean temperature and ice volume components? Also, do any issues arise from the fact that you only model the four largest ice sheets, e.g. how would consideration of smaller ice caps affect the benthic δ18O inverse model and hence the forcing of the coupled model?

8. Could you also clarify how the ice model is developed – I think that within the coupled model ANICE is run for 1000 years at a time, this information is fed into SELEN, updated boundary conditions for the future are calculated, and these are fed back into ANICE, which is then run for the next 1000 years. Is this correct? Since your calculation of future boundary conditions can only consider past ice loading, and since ANICE is run independently for each of the four ice-sheet domains, I think this means that any RSL changes that occur in response to the evolution of the individual ice sheets during each 1000 year run won't have a chance to impact upon the evolution of the neighbouring ice sheets. How do you get around this - did you consider iterating to de-

rive the ice-sheet reconstructions, i.e. running each 1000-year ANICE reconstruction more than once to enable forcing due to the contemporaneous evolution of the other ice sheets to be included?

9. Is the mask around Greenland large enough to capture the maximum Greenland Ice Sheet extent? In the supplementary material (line 22) it is stated that this mask covers the land mask of Greenland, plus one grid point of ocean (the grid size is 20 km), however, in many places the continental shelf is much wider than 20 km. Any ice advance across the continental shelf will be captured in ANICE due to the larger rectangular domain of this model, but this information wouldn't be passed onto SELEN if the mask is too restricted. Also, in the supplementary movie it looks like the Greenland and North American ice sheets never join up – have I got this right, and is this realistic?

10. Section 4.1: It would be useful if the error incurred by using the moving time window method could be quantified more clearly. The normalised residual is shown in Figure 7, but this does not give any idea of the actual physical values that might be involved, which is important for understanding what errors might be passed onto the ice-sheet reconstruction as a result of inaccuracies in the RSL calculations. The method used to empirically derive your preferred value of L = 80 kyr should also be explained more clearly (page 3510, line 24), and this section would also be a good place to test/justify your choice of coupling time interval.

11. Section 4.2: First, the title of this paragraph could be revised to give a better description of its content. Secondly, it would be useful if you were able to include explanations for the differences between the modelled RSL at the different sites. For example, the dip in the US east coast curve during each deglaciation (Figure 8b) will be due to the collapse of the forebulge of the Laurentide Ice Sheet; the higher-than-eustatic LGM RSL at the West Europe and Antarctic Peninsula sites will be due to isostatic depression in response to increases in the local ice load; while the higher-than-eustatic values during past interglacials supports recent work by Raymo et al. (Raymo and Mitrovica, 2012). To aid comparison between Figures 8a and 8b, it would

be useful to include more dashed vertical lines (like the one at the LGM, which is not labelled in the caption) at key glacials or interglacials. It would also be useful to flag up the fact that the locations of the RSL sites are plotted later in Figure 10.

Minor points

1. Page 3506, lines 2-4: The first two sentences are a little awkward. I'd suggest rewording along the lines of (I'm paraphrasing) 'relative sea-level variations can only be reconstructed if we know about ice history, but the best way to learn about ice history is from relative sea level'

2. Page 3506, lines 12-14: There is quite a jump in logic from talking about inverting for ice volume and temperature, to inputting ice thickness variations into the sea-level model. A brief mention of how you determine the spatially- and temporally-variable ice thickness variations would give a clearer picture of what you have done.

3. Page 3506, line 19: '. . .of the ice sheets edges. . .' should have an apostrophe on 'sheets', but I'd actually suggest turning this round so that it says something like '. . .at the edge of the ice sheet. . .'. Throughout the manuscript the English and grammar are generally good, but there are quite a few places where this type of error has been made, and I'd recommend running the final manuscript past a native English speaker to pick up any small errors.

4. Page 3506, line 24: please clarify whether you are talking about ocean temperatures or atmospheric temperatures. This also needs to be clarified in a number of other places in the manuscript.

5. Introduction: The first and second paragraphs of the Introduction could be linked a little better. One suggestion would be to highlight any outstanding problems with your previous work at the end of the first paragraph, thus setting up the motivation for this study. You could then open the second paragraph by saying that you will use the example of the LGM to highlight some of the issues that remain in the field of

ice-sheet/sea-level reconstruction.

6. Page 3507, lines 19: I suggest starting this paragraph with 'However', since the ideas you introduce here are counter-intuitive to the near-eustatic sea-level changes discussed in the previous paragraph.

7. Page 3507, last paragraph: For completeness, I would suggest also briefly mentioning that many far-field sites will experience a mid-Holocene highstand. It may also be useful to include a reference that gives a more detailed insight into the processes that cause spatial variability in sea-level change (I don't have access to the Pirazzoli references, so ignore this second comment if they include all the necessary details).

8. Page 3509, line 8: The phrase in brackets is not very self-explanatory, and since you've already used the term 'eustatic', this additional explanation may not be necessary.

9. Page 3509, line 28: I would edit this sentence to say that 'most' or 'almost all' transient solutions have been carried out using global average sea level. The recent study by Gomez et al. (2013), which you discuss on the next page, is one exception that springs to mind.

10. Page 3510, lines 5-6: It could be argued that ICE-5G provides 'a mutually consistent solution of ice volume and regional sea level over longer time scales'. However, I suspect you are thinking of cases in which the ice-sheet component has been determined using a numerical model, if so, it is probably worth specifying this.

11. Page 3510, lines 9-10: This sentence is a little muddled.

12. Page 3511, line 26: I don't think the Greenland data set is called BEDMAP. Also, the Bamber and Layberry (2001) reference is missing its third author.

13. Page 3513, line 5: The motivation behind the form of equation 1 is not really explained, in particular the reason for the offset when comparing modelled and observed benthic δ18O values.

14. Section 2.3: I think it is important to make the point that the sea-level equation must be solved iteratively. Does this cause any complications in the coupling process?

15. Page 3516, line 13: 'total numbers are provided in Table 1' – what do these numbers represent?

16. Page 3517, line 6: If it is important, then please could you clarify what you mean by 'until that very time'. E.g. If the ice thickness at location x is stated to be 'h' at time t, and temporal step sizes are Δt, then is this ice thickness maintained for the period [t-Δt,t] or [t,t+Δt]?

17. Page 3517, line 14: what are referring to when you say 'given the number…'?

18. Page 3517, line 28: On line 11 of this page, Δts is defined to be a time step length rather than the label for a specific time step or a specific time. I therefore found the terminology on line 28 ('for a specific time step Δts > 0') and line 12 of page 3518 ('… a time between t = Δts and t = L') a little odd.

19. Page 3518, lines 10-15: I found these sentences rather difficult to understand and I wonder if there is a clearer way to communicate the information. The method you describe is quite tricky to grasp on the first time of reading, but it is an important component of your work, so it's worth playing around with the text to get it as clear as possible.

20. Page 3518, line 17: I don't think H(t) has been defined.

21. Page 3519, line 5: This sentence doesn't make sense.

22. Page 3519, lines 6-8: 'the ocean functions are updated by overlapping the RSL changes…' A little more explanation is needed here to enable the reader to understand how the ocean function is determined. Also, since ice thicknesses will vary during each ANICE run, the ocean function will change as a result: Is the ocean function updated after each ANICE run, or is this dealt with at the start of the SELEN call?

23. Page 3520, line 2: 'To demonstrate the moving time window...' – what aspect of the moving time window is being demonstrated?

24. Page 3520: Please define all the terms in equation 3.

25. Page 3521, line 8: Do you really mean 'higher than PD' or do you mean 'higher than the eustatic curve'?

26. Page 3521, lines 22-23: This sentence is a little muddled.

27. Page 3521, line 27: 'Henceforth' is an odd choice of word as it means 'from now on...'

28. Page 3522, line 2: You reference the work by Gomez et al. on self-stabilization, but you have not really explained how these processes restricted the growth of West Antarctica in your reconstruction.

29. Page 3522, lines 13-14: Your references to Figures 9a and 9f need to be switched to be in line with the labelling in the figure.

30. Page 3524, lines 4-6: This sentence is a little muddled.

31. Conclusions, final sentence: It is already fairly well recognised that spatial variations in RSL can be large, so in your closing paragraph I would suggest emphasising the implications of this for ice-sheet reconstructions, as this is the new and important result of your study.

32. Figure 2 caption: Is there a difference between ΔTsurf (in the figure) and ΔTNH (in the caption)? Also what is 'the temperature module' referred to in the caption? And finally, when you mention 'ice loading on land', are you actually referring to 'grounded ice' (which may be grounded in the ocean or on land, both of which are a loading as far as SELEN is concerned)?

33. Figure 3: The different grids do not really show up, even when I zoom in!

34. Figure 4: I originally thought that the red dots were plotted too low because according to Figure 4(a) the amount of deformation after 1kyr is <1m. However, I then realised that each red dot is plotted just after the next load is applied, and hence it includes the elastic response to the second load as well as the viscoelastic response to the first load. It is probably worth briefly pointing this out.

35. Figure 6: Is this figure necessary – it is rather similar to Figure 4? But since you've included it, can you explain why the red curve flattens off around 100 kyr in this plot? Also, there is a mistake in the y-axis label.

36. Figure 10: captions (b) and (d) are currently not very clear.

References

Raymo, M.E., Mitrovica, J.X., 2012. Collapse of polar ice sheets during the stage 11 interglacial. Nature 483, 453-456.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C939/2014/gmdd-7-C939-2014-supplement.pdf

Interactive comment on Geosci. Model Dev. Discuss., 7, 3505, 2014.