Author responses os-2013-79. Wed 16 April 2014.

Note to self: Revised figures: 1, 5, 6 and 7. See ~/ice2sea/report/figs/global_paper_production_figs_revised_1
TODO: ensure that these are submitted along with revised m/s.

We wish to extend our thanks to our two anonymous reviewers and to Aslak Grinsted for their many constructive comments.

Anonymous Reviewer #1

Our ref	Reviewer comment	Author response
A01	The introduction should be shortened and better structured. The text of the Introduction now jumps apparently randomly from glaciological processes to dynamic sea level rise, AMOC changes and model complexity. In the revised Introduction please focus on the role of land ice melt on DSL: what has been done in the past in this field and what does this paper add to that knowledge. If that is what the title suggests, i.e. improved knowledge on the contribution of land ice melt to DSL, then please specify what the progress reported here entails. Also, references could be selected more carefully (see specific comments below for some examples).	The introduction has been substantially trimmed and focused towards the paper objectives.
A02	Please use 'high' and 'low' temperature rather than 'warm' and 'cold' temperature throughout.	Given that we also discuss sea surface height, we feel that 'warm' and 'cold' have less potential for confusing the reader.
A03	p. 125, l. 12 and 14: Bevan et al (2012) and Barrand et al. (2013) references out of place; the first paper deals with dynamics of Greenland outlet glaciers, the second with melting in the AP.	The reduction in the intro has removed these references. A check of all references has been undertaken.
A04	p. 125, l. 20: Van den Broeke (2011) reference out of place; that paper mainly discusses variability in surface mass balance. Please check appropriateness of references throughout Introduction!	The reduction in the intro has removed these references
A05	p. 127, l. 15: Specify average strength of AMOC to put listed changes into perspective, or simply add percentage between brackets.	Done.
A06	p. 127, l. 26: To aid the non-specialist reader, please specify 'Boussinesq' vs 'non- Boussinesq' model.	Reference to the response of sea level rise to ocean model formulation has been removed.
A07	p. 133: The sentence "There is no explicit representation of iceberg calving, so a prescribed water flux is returned to the ocean at a rate calibrated to balance the net snow-fall accumulation on the ice sheets, geographically distributed within regions where icebergs are found (Gladstone et al., 2001)." implies that the model is forced with 'balance' freshwater fluxes from land ice before the anomalies of Fig. 1 are imposed. Could you please specify the magnitude of these balance freshwater fluxes, i.e. how much snow is assumed to fall on the ice sheets and how much runs of as melt water or breaks off as icebergs? Are these	Added reference to Gordon et al. 2000, where this is discussed, as follows: The prescribed ice-berg freshwater flux amounts to 0.03 Sv from Greenland, larger than 0.2 Sv estimated from reconstructions (Hanna et al., 2011), and 0.09 Sv for Antarctica"

	fluxes realistic, i.e. how do they compare to recent estimates of balance mass fluxes from other techniques?	
A08	Sections 2.2 and 2.3: Please explain -already here- in short the irregular behaviour of the high-end GrIS and AIS freshwater fluxes, or refer to the section 2.4 for an explanation.	Irregular behaviour associated with warm water pulses around Greenland and partial collapses of West Antarctic ice sheet. Now expanded on in the text.
A09	Fig. 1b: please add unit (m) to y-axis label.	Done.
A10	My copy of Fig. 4 was of poor resolution.	Looks adequate to us – probably better than the resolution of the underlying model?

Second review on next page...

Anonymous Reviewer #2

Our Ref	Reviewer Comment	Author response
B01	Overall, it seems that a lot of words are needed before the authors finally make their point. It wasn't until I reached the conclusions before I had a clear idea of the actual point of the paper. Admittedly, this might be partly on myself, but as I think that as an author you should try to guide the reader trough the paper a bit more than you're doing now. I then needed to read back to also understand more of the middle part. On the positive side, this means that the conclusion is well-written. On the other hand, it means that the rest of the paper can be improved. First of all, by writing up some parts of text more efficiently (mainly Intro and sect 2-3), and by clarifying short or vague statements, as specified in the comments below.	The paper has been trimmed, particularly the introduction, ar improve logic.
B02	P124,L5: DSL is not the only type of local departure from GMSL due to ice melt, gravitational effects are at least as important. Although they are mentioned in the introduction, in the abtract it sounds as if DSL are the sole cause for local deviations.	Paragraph reworded.
B03	P124,L11 evolution in space or time? Or both?	"in time" added to text.
B04	P125, first paragraph: some reference(s) to regional projections paper(s) might be in place here; Milne2009 does not provide projections.	Added ref to Pardaens et al 2011
B05	Please clarify: are the ice sheet scenarios existing (P128,L9-10) or developed specifically for this study (P128,L15)?	Clarified and reference added.
B06	In P128L20 the freshwater fluxes are applied to HadCM3, and in P129L2 the fluxes are derived from ECHAM? This requires clarification. Do you mean that the ice melt contributions are determined with ECHAM and then implemented as freshwater fluxes in HadCM3? The subsequent sections (2.1-2.3) do not exactly make matters clearer: while GIS is projected using ECHAM5, this is not specified for G&IC and AIS	We're not sure that "determined with" is clearer than "derive We're not sure that "implemented as" is clearer than "applie Sections 2.1 and 2.3 include references to sources of further i regarding the G&IC and AIS components.
B07	P134L4: Why a simplified version of A1B, can you provide a reason for this? Also, the GIS (and presumably G&IC and AIS) are modelled with the full A1B, doesn't this lead to discrepancies?	This was a technical choice decided in part by the time-frame was much more straightforward, and cleaner, to ensure that t between our control and A1B simulations was due to the sim equivalent CO2 forcing than to reconstruct all of the technica in a 'full' A1B simulation. We tested the impact of this simpl terms of the 21st century changes in OHC, SST, salinity and I them all to be broadly similar to the 'full' A1B simulation, as Furthermore, I think it unlikely that any discrepancies betwee A1B simulation and the 'full' A1B are larger than other pote

		inconsistencies inherent in the unavoidable use of ice sheet si are not fully coupled to the GCM.
B08	P135L16: Please define 'three member ensemble'. How is it set up, what are the differences between the three members? I presume they are three different parts of the control run(?), but it should be stated explicitly since a lot of analysis follows from this ensemble.	The formation of the ensemble is now discussed in section 4. Here is a more lengthy, informal discussion: *In fig 3, (b) is spun off from one point in the control run. (c) is different point. (d) is spun off from the same point as (b) but from the ice sheets is a monthly pre-industrial climatology (s Sect. 2.2). We assert that the change to using a monthly pre climatology makes a comparable difference to spinning off fr point in the control. The evidence for this is: the different AM the 'Clim' simulations, as seen in fig 12, and the different π(t in fig 6 (b). In fig 6(b), the red and blue are the two that have dump [red corresponds to fig3(b), green corresponds to fig3(corresponds to fig3(d)] Arguably, red and blue start off simila measure) but diverge significantly (i.e comparable to the dive red and green) before the ice melt forcing begins around 199* In the revised article, in addition to the new discussion in sec ensemble members with common initial conditions are identi relating to figures 6 and 12.
B09	P135L25: 'the last 100 yr, when the forcing is strong' – the last 100 yr of the control run or of the ice-melt scenario? I presume the latter since the control run is not supposed to have any external forcing, but this should be clarified.	Rephrased, thanks.
B10	P136L15-16: Do you mean to say that the control run without land ice gives no DSL pattern? Please clarify.	Rephrased, thanks.
B11	There are a couple of situations in which the authors claim that there is similarity between patterns and conclude that therefore the patterns can be scaled for different ice melt scenarios (P138,L16 – fig 5) or used on top of different climate scenarios (P142,L7 – fig 11 vs fig 3), but the similarity/scaling is not really shown. Yes, maybe the patterns generally look alike, but it is very hard to judge by eye whether they actually scale, as the authors suggest. I would therefore like to see the ratios or differences between these patterns to strengthen these conclusions – especially since these end up in the final paragraph & conclusions of the paper (P145L13).	A discussion of MR vs HE correlation/regression/scaling is n section 4.1. Also panel (c) added to Fig 5. The correlation coefficients between the dynamic sea level cl with the HE ice melt applied to a simulation with fixed CO2 business-as-usual greenhouse gas warming scenario of increa quoted in section 5. For brevity we have not included all of the tests that we made assertion of MR vs HE similarity, included in the revised arti Global correlation with no mask = 0.83 N Atlantic correlation with no mask = 0.93 And Fig 5(c) But not included: N Atlantic correlation with mask of HE only = 0.90 N Atlantic correlation with mask of HE and MR = 0.82 Regarding our assertion that the dynamic sea level change as ice melt is similar regardless of whether the simulated ice flu simulation with fixed CO2 or under a business-as-usual green warming scenario of increasing CO_2, included in the revised

		Global correlation with no mask = 0.73 Global correlation with mask of Fig 3a = 0.94 But not included: N Atlantic correlation with no mask = 0.87 N Atlantic correlation with mask = 0.89 (Note to self: "review_comment_B11.pro" and fig_BV.e
B12	Related to this, the final conclusion of this paper is that all climate models may adopt the DSL pattern to add to their respective DSL patterns, because the linear addition works for this model. However, this relies on the rather strong assumption that all climate models have a similar response in DSL to freshwater forcing. Since the DSL without this additional land ice freshwater forcing is already rather variable (see e.g. Yin2012), and also the response to additional freshwater forcing varies (e.g. Swingedouw2013 shows different sensitivities to freshwater forcing for different models), this seems a very bold conclusion to make, as it might very well not be true. Please discuss this. Or maybe consider to add 'in HadCM3' to the title.	Agree this statement was overconfident. We have removed th confined ourselves to discussion of the DSL changes in our n context of model uncertainty.
B13	The use of 'case studies' and 'scenarios' throughout the paper seems at times to even confuse the authors. Is it ice-melt scenario or ice-melt case study? Or ice-melt case study scenarios (header sect.2)? Also, scenario can point either to climate or to land ice. It would be very helpful if clearer distinctions were made. Pick a term and stick with it.	Thank you for pointing this out. We now explicitly refer to 'i or 'greenhouse-gas scenario' throughout, reserving the phras the section focussing on North Atlantic changes.
B14	Sect7: One thing I'm missing here is how this fits in the bigger picture. Yes it's small (L23) but how does it compare to other regional sea level contributions? How important is it to include this effect (or is it important?)? might also be added to P124,L15: how does DSL compare to the change due to gravitational effects?	The following text has been added to section 4.1: "The size of this contribution is put into the context of some contributions to regional sea-level change by Howard et al. (2 Howard T, Pardaens AK, Lowe JA, Ridley J, Hurkmans RTW Spada G, Vaughan D (2013) Sources of 21st century regional along the coast of North-West Europe. Ocean Sci. Discuss, 1 2013 : doi:10.5194/osd-10-2433-2013
B15	P144L23: 'the mean DSL change' due to ice melt or in general? Probably the former, but please specify.	Clarified, thanks.
B16	Table1 1; This Table needs MUCH more explanation, because it doesn't make sense at all. What is A1B(m), A1B(s), PI climatology, PI(s)? All these things are barely explained or not even mentioned in the rest of the paper.	We agree that the table did nothing for the clarity of the manu dispensed with it altogether, instead describing each simulati
B17	Fig 6b; These blue lines appear a	We have extensively revised our description of Fig 7 (former

	little..out of the blue.. I suppose these are the 'independent samples' from the text? Why 8? Also, the point that is made in the last sentence of the caption is probably a point you should probably make in the text instead.	reiterating the point from the last sentence of the caption into suggested. We have also added the following text to the manuscript: "In panels (a) and (c) the unforced variability (noise) is samp member parallel control ensemble only. Using the whole of c simulation (1715 years) we are able to study a larger sample taking sets of three 240-year chunks (with initial times chose within the long control simulation) and treating them in the s three-member parallel control ensemble. We created eight su are the eight blue lines in each of panels (b) and (d) in Fig 7." Regarding the question of "why 8?": this was a compromise conflicting demands: • I wanted to show a larger sample, and using more of t the long control run was an obvious way to do this • I wanted the additional lines on the figure (the blue li distinguishable, and: • if the number of chunks were increased there would c the blue lines were no longer independent of one anot even the long control run is of finite length. I didn't perform a rigorous test of the independence of the eig number eight was a somewhat subjective choice that seemed illustration satisfying the first two criteria above, but I would one were to accept that, say, any two of the blue lines are ind imagine the other six erased), then together with the red line, broken black line (forced) is outside the noise (red and blue: be made. (In the interests of brevity we have not included the whole of the manuscript!)
B18	P124 L10: global mean sea	Done, thanks.
B19	P125,L10: Pritchard	Done, thanks.
B20	P125,L29: using -> use a full Stokes model to project	The reduction in the intro has removed this reference, c/f rev A01.
B21	P126,L13: take out ",which is" to make the sentence clearer	Done, thanks.
B22	Fig 1 caption: it would not contribute	Done, thanks.

Short comment (Aslak Grinsted): (our ref C01) "Please cite and compare to Stammer et al. 2011. They find a much greater hosing response in an AO coupled model compared to forcing an ocean only model."

Our response:

Thank you for your suggestion. As a first look I have attempted to make a very crude like-for-like comparison of the range of the sea-level change pattern between the

Labrador Sea and North East Atlantic in the Stammer (2011) paper (his fig 3, middle panels – years 26-30), and ours (our fig 5a) as follows:

We don't have a Greenland-only simulation so, for a first look, let's suppose the signal in the Labrador Sea and North Atlantic in our simulation comes from the Greenland component of our forcing only (!). Further suppose that the strength of the sea level pattern behaves, as a first approximation, like the time-integrated forcing (i.e. the number of Sverdrup-years from Greenland). Stammer's forcing is 0.0275 Sv so his time-integrated forcing in his middle panels (26-30 years) is around 28years x 0.0275 Sv = 0.77 Sv-years.

Our time-integrated forcing is shown in our fig 1b. The Greenland component is well approximated by a straight line from zero at 2000 to 0.08 m SLE at 2100. We average over this full period so let us take our result as representative of a time-integrated Greenland forcing of 0.04 m SLE = 0.5 Sv-years. So, very loosely, to get from Stammer's result to ours, if the two models behaved similarly and subject to the crude approximations above we would expect Stammer's SL change divided by 1.5 to look like ours.

Stammer representative value in Labrador sea (CGCM): 14cm?
Stammer representative value near UK (CGCM: 2.5 cm
Diff: 11.5 cm. Divided by 1.5 = 7.6 cm

Our representative value in Labrador sea: 5cm
Our representative value near UK: 2.5 cm
Diff: 2.5 cm

Stammer representative value in Labrador sea (Ocean only model): 9cm?
ditto near UK: 1.5 cm
Diff: 7.5 cm. Divided by 1.5 = 5 cm

So it seems from this crude comparison that Stammer's model response is considerably stronger than ours for this simple test, either with his ocean-only or his CGCM.

Taking a second look, this time ignoring the strong signal 'spilling out of' the Labrador Sea and this time looking at his bottom two panels (41-46 years) and adjusting the scaling factor accordingly (from 1.5 to 1.5x(43years/28years)=2.3), and this time comparing:

the difference between his most negative N Atlantic value and his typical NW Europe value

with:

the difference between our most positive N Atlantic value and our typical NW Europe value

I estimate:

		N Atl extremum	NW Euro	Diff	Scaled diff
Stammer	CGCM	-6 cm	4 cm	10 cm	4.3 cm

	Ocean only	-4 cm	1.5 cm	5.5 cm	2.4 cm
Our	CGCM	5.5 cm	3 cm	2.5 cm	2.5 cm

Again Stammer's model response appears to be stronger than ours by this crude measure, and our CGCM response is nearer to his ocean-only response in both cases.

Since we do not have an ocean-only model response we cannot comment on the contrast between CGCM and ocean-only, but we note that in common with Stammer's coupled simulation, in our simulation (which is coupled) statistically significant sea level anomalies are apparent in all of the major ocean basins in fig3 panels b, c, and d (although not panel a, presumably due to the more strict criterion applied).

We have added the following text to the manuscript:

"Stammer et al. (2011) using the University of California Los Angeles model investigated the response of both a coupled ocean-atmosphere model and an ocean-only model to enhanced Greenland freshwater forcing of 0.0275 Sverdrups sustained for 50 years. This forcing is stronger than the Greenland component of our forcing, but even taking account of this (and even ignoring the other components of our forcing) Stammer et al. (2011) report a noticeably stronger response in the DSL pattern than the pattern which we see, particularly in their coupled simulation. Further, despite some similarities in the equatorial regions, the Southern Ocean and most noticeably the Labrador Sea, their pattern of response is generally quite different to ours."

Perhaps this is not too surprising in view of the large variations in DSL pattern seen in the CMIP3 or CMIP5 ensembles, and see also our responses to reviewer comments regarding inapplicability of our result to other models (particularly comment B12) – we have acknowledged this as a limitation.