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Interactive comment on "NEMO-ICB (v1.0): interactive icebergs in the NEMO ocean model globally configured at coarse and eddy-permitting resolution" *by* R. Marsh et al.

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Overview of Responses

We thank the referees and editor for extensive and informative comments. Below, we list these verbatim, responding to each comment in turn. Rather than explicitly detailing "author's changes in manuscript" (as instructed in the auto-reply from editorial@copernicus.org), we indicate our intended changes in general terms. It is evident that major revisions are necessary, involving substantial further simulations and diagnosis. We request that the editor grants us sufficient time to complete this work, as requested in our response to the Editor's Comment.

Responses to Anonymous Referee #1

This paper describes simulations of an iceberg model coupled with the NEMO ocean model in two different resolutions (0.25 degree and 2 degree), using atmospheric forcing data. The results seem reasonable, although they often differ from those produced using the same iceberg model coupled with a different ocean model (Martin and Adcroft 2010, "MA"). An interesting result of the paper is the significant difference in Antarctic Coastal Current transport at the different resolutions, associated with changes in the density gradient and sea ice divergence. The paper is well written and contains a great deal of useful information, but several points need to be clarified.

1. My primary suggestion to the authors regards the comparisons with MA. In particular, a paragraph describing essential differences between MA's model configuration and the present model could be helpful for understanding these differences. It should include atmospheric forcing, calving rates, ocean mixing, and anything else likely to be important that is referred to in this paper.

P 5663 line 23. Why are MA's results so different? Is it because of the change in deep water formation mentioned in line 29?

Response: Note that MA implemented icebergs in a fully coupled climate model, with a model that is non-eddy resolving. As a consequence of using calving rates in balance with precipitation over ice sheets, quite different iceberg mass fluxes and distributions arise. We will clarify and discuss these differences.

P 5665 line 12. Are you using the normal year forcing or interannually varying?

Response: We use normal year forcing – this will be clarified in the revised manuscript.

P 5669 line 25. Please explain MA's calving rate.

Response: See comment above (relating to P 5663 line 23).

2. Another concern regards the way in which sea ice/iceberg dynamical interaction is

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addressed in the paper. (It is not addressed explicitly in the model.)

P 5666 lines 17-21. This is very vague and considering your later results, it's not clearly true. Hunke and Comeau (2011) estimate that the dynamical effect is no more than a few percent of total sea ice volume, but your results using thermodynamic coupling show that icebergs change total sea ice mass by only 4% (page 5672 line 12). I agree that dynamical iceberg/sea ice interaction effects will likely be smaller, but I'm not convinced that they are negligible compared with the thermodynamically induced effects.

Response: We will revisit our analysis of, and conclusions on, the iceberg influence on sea ice dynamics and thermodynamics.

P 5666 line 26. Using the need to recode as an argument for implementing the iceberg model in the ocean model seems irrelevant – now it would have to be recoded for a different ocean model. An argument for closer coupling with the sea ice model would be to allow the dynamical effects between them, similar to the physical argument for coding icebergs in the ocean model.

Response: Our statement is based on some technical aspects of the implementation, which we will clarify in the revised manuscript. In essence, the iceberg module in NEMO-ICB is not coupled, but rather interactive – providing flexibility/portability. In contrast, as stated by Martin and Adcroft (2010), "For computational convenience the iceberg model is part of the sea ice module SIS in CM2G."

P 5672 line 28. "Suggesting a dynamical effect" seems contradictory given earlier discussion about iceberg/sea ice dynamical interaction. It would be helpful to clarify here that you are referring only to dynamics internal to the sea ice model (or possibly forced by the ocean or atmosphere, but not by icebergs).

Response: Yes, we are indeed referring to the (indirect) effect of icebergs on sea ice dynamics, which we will clarify in the revised manuscript.

P 5673 line 1. How do icebergs increase sea ice convergence? Your sea ice model likely includes ice divergence as an output field, or it could be calculated. This would be interesting to see.

Response: This is presently an inference (that icebergs increase sea-ice convergence) – we will investigate the mechanisms, as advised, and clarify findings in the revised manuscript.

3. Some quantities need further explanation.

P 5667 line 12 and 17. Why do you choose much lower calving rates than used in the other studies cited here, especially considering the Rignot et al (2011) results? What rates do MA use?

Response: This derives from a mass balance calculation around 2000, before climate changes in melt and discharge from ice sheets began to increase significantly. It is effectively a conservative "pre-industrial" estimate, not a present-day one.

P 5667 line 14. I do not understand this at all. Are you using a different ice density?

Response: We are simply converting from volume flux to mass flux, for purposes of comparison.

P 5669 line 11. Is the mass of the giant icebergs included in the mass of smaller modelled bergs, or is this part of the 'missing' mass flux compared with other studies' calving rates?

Response: Giant icebergs are indeed unrepresented, although their absence does not account for differences in calving rates (see response above regarding P 5667 line 12 and 17).

P 5671 lines 21-27. Here you seem to be comparing apples and oranges, talking about P-E (without sea ice fluxes) or net freshwater fluxes (fig 4) in your model versus total freshwater flux in MA. Please clarify.

Response: In the revised manuscript, we will correct the diagnostic as the ratio of

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iceberg melt to total freshwater input, and plot this in (revised) Fig. 4.

P 5672 line 24. Is this thickness the actual thickness over the sea ice covered area, or the mean thickness over the grid cell?

Response: We use Fichefet and Marqueda (1997) definition of sea ice thickness Hi as “mean ice thickness of the ice-covered part of a grid cell” and Figures 5 and 6 show this parameter.

P 5676 line 23. Please mention here and in the abstract that the total mass changes by 4%.

Response: Will do

Table 2. Is the virtual coverage by icebergs subtracted from grid areas occupied by sea ice? I.e. is A_bergs + A_seaice + A_openwater = A_gridcell for areas A?

Response: No, A_seaice + A_openwater = A_gridcell for areas A, hence we refer to virtual iceberg coverage. This is consistent with the very small fractional area for icebergs in the size categories considered here, but would need to be re-visited for giant icebergs.

Figure 5. Do the differences here take into account the area occupied by icebergs?

Response: No – see preceding comment.

4. Other comments:

P 5664 line 19. What does ICB stand for?

Response: ICB is shorthand for ICeBerg - we will clarify this in the revised manuscript.

P 5664 line 5. Use ’simulate’ instead of ’stimulate’

Response: Thank you for spotting this typo!

P 5665 line 19. Treating icebergs as Lagrangian particles seems to contradict lines 3-5 on page 5663. I suggest adding "Collections of" to the beginning of this sentence.

Response: We will consider this wording.

P 5667 lines 23ff. The way this is written, the numbers seem to be for ice discharge only, not liquid water. I suggest changing "ice discharge" to "ice sheet mass discharge".

Response: Will do.

P 5671 line 8-9. Why do the larger bergs drift farther north?

Response: We suspect this is because the icebergs melt slower in the coarse resolution ORCA2, as they are not exposed to such strong currents, and there is no eddy motion to delay their journey equatorward.

P 5673 line 18. The differences appear to be negative everywhere near Antarctica in the lower resolution runs. Please be specific.

Response: Will do.

P 5675 lines 1-4. Which resolution and years are shown in figures 11 and 12?

Response: The T/S analysis in Figs. 11 and 12 is averaged over years 10-14, for the ORCA025 configurations (ICEBERG and CONTROL) – this will be clarified in the revised manuscript.

Figure 2. Please remove the "trajectories" labeling in the figure.

Response: Will do.

Figure 3. What limits the flux along the eastern side of the Antarctic Peninsula?

Response: There are relatively few calving sites between the Bellingshausen Sea and the tip of the Antarctic Peninsula. Also, in ORCA025, the Antarctic Coastal Current carrying icebergs westward in this sector is strongly constrained to follow coastal topography and there is essentially no offshore transport of icebergs.

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Responses to Anonymous Referee #2

In “NEMO-ICB (v1.0): interactive icebergs in the NEMO ocean model globally configured at coarse and eddy-permitting resolution” Marsh, Ivchenko, Skliris, Alderson, Bigg, Madec, Blaker, and Aksenov present an interactive dynamic-thermodynamic iceberg model component coupled to the well established NEMO-LIM ocean sea-ice model. As duly noted in the study, neither the iceberg model nor its coupling to an ocean model are new developments. Obviously, the study closely follows an earlier work by Martin and Adcroft (2010), in which the same iceberg model is applied to the GFDL's climate model CM2G. Nevertheless, interactive icebergs in NEMO are a novelty and as NEMO is used by many groups all over Europe this is an important step forward in both accepting icebergs as an important component of the climate system and testing the sensitivity of modeled oceans to realistic icebergs. The authors study the sensitivity of NEMO to icebergs by means of two control simulations with grid resolutions of 2 deg and 0.25 deg, respectively, where the former run features 105 years and the latter only 14. The very short spin up of the high-resolution run is unfortunate and the one major concern I have with this work. It would be desirable to have the ORCA025 simulation extended for at least another 16 years, or even better 36 years. This suggestion is based on my assumption that the authors have access to high performance computers. My experience is that 10 years of ORCA025 take 5 days or even less depending on the HPC system used. Otherwise the study is carried out thoughtfully and the well-structured manuscript is nicely written. The focus of the paper is entirely on the presentation of this new model set up and thus matches well the journal topic. I thus recommend this paper for publication in GMD after major revisions.

Response: We are planning to extend the ORCA025 simulations (CONTROL and ICEBERG) to at least 30 years, and to repeat the diagnosis on the last 10 years of this extended simulation (see below). This is a major undertaking, as we have also now identified and solved a technical problem, which previously limited the ORCA025 spin-up to 14 years. The work is underway, and we anticipate that new results will become

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available over the next 6-8 weeks.

Detailed comments: As mentioned above I have trouble believing your statement that the ORCA025 simulation is in equilibrium after only 14 model years. In Table 3 it is shown that calving and melt fluxes are not balanced in ORCA2 averaged over model years 10-14 and the difference is big compared to the (im-)balance in years 101-105. The greater imbalance compared to the CM2G may partly be due to the short averaging period of just 5 years for the ORCA results (100 years for CM2G). Moreover, the average iceberg mass for ORCA2 after 10 years is similar to that in ORCA025 after 10 years but from the upper panel in Figure 1 one can see that the iceberg mass further increases in ORCA2 after year 10 and levels off around years 40 to 50. This suggests that the iceberg mass in ORCA025 is not in equilibrium, yet. By the way, a spin up for iceberg mass of about 50 years agrees nicely with the 60 years noted by Martin and Adcroft (2010). However, the results of Martin and Adcroft support the notion that calving and total iceberg melt fluxes (not iceberg mass!) are in balance after about a decade. Nevertheless, 14 years of spin up are too short to draw conclusions for the deep ocean as you do later in the paper (Section 3.3).

Response: As clarified above, we are will extend the simulation to at least 30 years, and the extended version of Fig. 1 (time series of iceberg mass in NH and SH) will better inform us of the extent to which equilibration is reached. We will more clearly explain what is in balance (calving and melting fluxes). We will be more cautious in drawing conclusions about iceberg influences on the deep ocean.

By page/line:

5662/5 Here, it is briefly noted what the forcing in the control simulation without icebergs looks like, i.e. the freshwater forcing that compares to iceberg calving in the sensitivity run. This bit of information is unfortunately missing in Section 2 and I strongly recommend to more clearly state in Section 2 whether there is a freshwater flux comparable to calving in the control simulation and how it is distributed in the absence of an iceberg model component.

Response: We agree that this vital detail was missing, and we will clarify the experimental details.

5662/14 I believe that this conclusion has to do with above issue. Again, please explain clearly what kind(s) of runoff are accounted for in the control and the iceberg experiments and how they are handled. This is instrumental to understand the simulation results and to compare to earlier studies such as Jongma et al. (2009) and Martin and Adcroft (2010), which both are referred to in the text.

Response: Will do.

5662/20 The last sentence of the abstract is not clear to me without further knowledge of the main text. What is "eastward transport tendency" referring to? Please rephrase.

Response: Will do.

5663/23 Please add here, how Jongma et al. (2009) handled runoff in their control experiment. I believe their runoff from Antarctica was distributed globally in the control experiment, which is in stark contrast to the control run with CM2G and could explain the opposing sea ice trends caused by introducing icebergs to these different models.

Response: Will do.

5665/13 There is only a single 14 year experiment presented here; remove "s" from "experiments".

Response: There are two experiments, CONTROL and ICEBERG.

5665/15 Please describe here the runoff distribution scheme in CONTROL. Is there a flux in CONTROL that is redirected to iceberg calving in ICEBERG or does NEMO-ICB feature an additional flux of energy and mass due to calving?

Response: We simply specify a fixed fraction of the CONTROL runoff as the calv-

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ing flux, so the total freshwater fluxes from Greenland and Antarctica are identically equal in CONTROL and ICEBERG. We will clarify and emphasize this in the revised manuscript.

5666/13 Is such a simple drag law implemented in NEMO-ICB? Please make this more clear in this sentence.

Response: No, not at the present time.

5667/3-6 You correctly state that "icebergs [: : :] are largely submerged into the ocean" (5666/27) and thus implement the iceberg model as part of the ocean model (and not as part of the sea ice model as Martin and Adcroft did). However, I am surprised that you do not take advantage of this and force the icebergs by the surface instead of the available 3-D fields of ocean temperature and currents and you also seem not to feed 3-D fields of iceberg melt back into the ocean. Why?

Response: We are indeed contemplating some of the opportunities afforded by implementing icebergs in the ocean component. Colleagues elsewhere in the NEMO community have recently modified ICB to use depth-averaged currents in the iceberg momentum balance, and compared iceberg trajectories this obtained with those obtained (as here) using surface currents. There are some interesting differences in Weddell Sea in particular, where icebergs drifting with depth-averaged currents more readily take a short cut across the Weddell Sea. Vertically averaged temperatures are also being considered. In the revised manuscript, we will include these extended sensitivity studies with NEMO-ICB, in relation to ongoing development. However, we are not yet feeding 3-D fields of iceberg melt back into the ocean, with the following justification. Given that the size of our maximum iceberg is much less than even the ORCA025 resolution, and that the plumes from iceberg basal melt rise to the surface within a few hundred metres, applying the melting flux to the surface is inherently reasonable at these model resolutions.

5667/12 Please explain why you are using a seemingly small calving rate of 1140Gt/yr.

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As you elaborate in the remainder of this section this is a rather conservative number. How did you derive this number?

Response: Repeating a response to Referee #1, this derives from a mass balance calculation around 2000, before climate changes in melt and discharge from ice sheets began to increase significantly. It is effectively a conservative "pre-industrial" estimate, not a present-day one.

5668/23 Since you only show 10-year averages of the iceberg mass in ORCA2 in Figure 1, I cannot follow your conclusion that the calving and melting rates are in balance by year 10. Please revise Figure 1 as outlined below. You may also want to refer to Table 3 to support this statement (although I don't believe the numbers based on years 10-14 are fully convincing).

Response: Will do.

5669/1 In continuing this argument: By stating that SH iceberg mass in ORCA025 is in equilibrium after just 10 years you also state that ORCA025 has about 100 GT ($\sim$15%) less SH iceberg mass than ORCA2. Why? I think the iceberg mass is not in equilibrium in year 10 of either simulation. Figure 1 shows that these years belong to a transient period although the iceberg mass of the first 10 years is astonishingly similar to the later equilibrium state. Further, you state that semi-enclosed basins and embayments prolong the lifetime of icebergs in the NH. Why would the enhanced resolution of coastlines in ORCA025 (compared to ORCA2) not lead to more "grounded" icebergs and enhance lifetime in the SH as well? (also see line 5669/8).

Response: Calved icebergs are immediately placed slightly offshore in coastal gridcells. The stronger coastal current of ORCA025 advects the bergs away faster (so they melt quicker) and the higher depth resolution near the coast in ORCA025 (compared to ORCA2) may not make much difference to iceberg mass balance.

5669/8-11 I would expect most NH icebergs to drift into the North Atlantic and hence

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melt about as quickly as SH icebergs moving out to the ACC. Is such a comparatively large storage of icebergs in embayments in the NH realistic or a model characteristic? Out of curiosity: Are more icebergs “grounded” in ORCA025 than in ORCA2?

Response: It can be very difficult to get icebergs out of enclosed areas – Baffin Bay is an iceberg trap numerically, and there is some evidence for this in reality. We also doubt whether the currents in iceberg calving regions in the NH are as strong as the Antarctic Coastal Current, except along East Greenland.

5669/25 I am wondering if the short averaging period of just 5 years may cause some of the greater imbalance in NEMO-ICB compared to the number from CM2G.

Response: We will consider extending the averaging period to 10 years, but the 100-year averaging in Martin and Adcroft (2010) is clearly less viable as we are unlikely to extend the OPRCA025 run beyond 30 years.

5670/9 The different partitioning between iceberg erosion and basal melt may also be due to different SST and wind speeds in the forced ORCA runs compared to the fully coupled CM2G.

Response: We will mention this.

5671/15 please add “: : : associated with local imbalances of precipitation and evaporation (P-E), and sea ice growth and melt.” as I assume the net freshwater flux (see caption of Figure 4) includes (virtual) sea ice freshwater fluxes.

Response: Will do.

5671/28 I suggest to rephrase: “In the Greenland Sea of ORCA2, negative values indicate areas where sea ice formation acts to increase salinity (not shown), i.e. causing a negative net freshwater flux, although both terms are locally small.”

Response: Will do.

5672/24 Do you show maps of actual ice thickness (m) or mean ice thickness (m3/m2), i.e. sea ice volume per grid cell area? The latter would be affected by changes in ice concentration. A greater (smaller) ice concentration in a grid cell with an unchanged actual ice thickness would yield a greater (smaller) mean ice thickness. The fields in Figures 5 and 6 look pretty similar, which hints at mean ice thickness.

Response: Repeating a response to Referee #1, we use Fichefet and Marqueda (1997) definition of sea ice thickness Hi as "mean ice thickness of the ice-covered part of a grid cell" and Figures 5 and 6 show this parameter. From the authors point of view, some similarity between sea ice concentration and thickness fields in Figures 5 and 6 is due to the fact that these two fields are generally correlated dynamically and thermodynamically, e.g., ice convergence increases ice concentration and thickness (ridging), whereas ice melting decrease ice concentration via lateral melting and thickness via vertical melting. Generally, except the western Weddell Sea, the plots are quite different, c.f. the eastern Weddell Sea and the western Ross Sea. Overall, the large scale-long term sea ice dynamics in the Southern Ocean is wind-driven, with gyres and alternating areas of on-shore/off-shore ice convergence/divergence in the Weddell and Ross Seas (Holland and Kwok, 2012), leading to areas of high correlation between ice concentration, Ai, and thickness. It should be noted that ice dynamics also operates with ice mass per unit area, Mi, defined as Mi=Hi*Ai. We will clarify these issues in the revised manuscript.

5672/28 Considering that the model does not account for iceberg sea-ice mechanical interaction (Section 2.2) I am not convinced that changes in sea ice thickness are due to dynamical effects. On the contrary, the spatial distribution of iceberg melt water in Fig. 3 (and its ratio to local freshwater sources, Fig. 4) show that areas of great iceberg melt also have great increases in sea ice thickness, which would indicate a thermodynamic effect: freshening by iceberg melt supports sea ice formation).

Response: Agreed – we will revise the text accordingly.

5673/18 This sentence needs an explanation in Section 2.4 Calving. How is runoff redirected in the iceberg runs? (see related comments above)

Response: Repeating a response to Referee #2, we simply specify a fixed fraction of the CONTROL runoff as the calving flux, so the total freshwater fluxes from Greenland and Antarctica are identically equal in CONTROL and ICEBERG, and the remaining (non-calving) part of the freshwater flux remains as runoff distributed around Greenland and Antarctica as in CONTROL. We will clarify this in the revised manuscript.

5674/10-21 I think these conclusions are not well supported by the results presented here for the following reasons: First, the calving rate is considerably lower in the NEMO-ICB runs than observed as stated in Section 2.4. Second, the model runs are based on a climatological cycle (Section 2.1) and comparisons are done for the "unrealistic" case of no icebergs versus one with icebergs. Hence, it is unclear what the impact of a transient climate on iceberg mass and melt distribution would be âĂŤ let alone that we don't know well, how the calving forcing should evolve in such a scenario. And third, as noted above, the spin up and averaging periods are short for investigations of the deep ocean. For instance, 14 years of ORCA025 run is even shorter than the period 1982-2000, which didn't indicate changes in iceberg mass from observations.

Response: OK - we can remove this text.

5675/12 Here is another thought you may want to add to the discussion of Figures 11 & 12: While freshening dominates above 500m in both seas, there is warming between 500 and 2000m in the iceberg run. This warming indicates that there is likely a decreased upwelling/mixing of these deep, warm waters with the cooler but fresher upper ocean due to the stabilizing effect of the upper ocean freshening. Hence, the distribution of melt water by icebergs helps to stabilize the Weddell and Ross seas making open ocean deep convection more unlikely.

Response: Thank you for this thought - we will consider this in revising the manuscript.

5676/15 These negative differences only indicate an intensification of the MOC by icebergs in case of a negative MOC streamfunction. Is this statement referring to enhanced northward transport of bottom water in ICEBERG? Please add information on sign convention and the sign of MOC in CONTROL for clarification.

Response: Yes, we do refer to enhanced northward transport of bottom water in ICEBERG - this will be clarified, along with sign convention, in the revised manuscript.

Table 1: This table is the same as Table 1 in Martin and Adcroft (2010), except that the length of the icebergs is not given here. Please add "(reproduced from Martin and Adcroft (2010))" to the caption.

Response: We will remove Table 1 and instead refer to Martin and Adcroft (2010).

Table 3: add "(100yr mean)" below CM2G to indicate the averaging period.

Response: Will do.

Figure 1: I strongly recommend to plot bars with yearly resolution instead of 10-year means for years 1-20 in the upper panel (results of ORCA2) in order to make this comparable with the lower panel (ORCA025 results). Alternatively, since I do not favor bar plots, I suggest to simply plot individual lines for SH, NH, and global iceberg mass; thin lines for annual mean and bold lines for 10-year running-mean; for both the upper and lower panels even though there will be only 5 data points of running-mean for the ORCA025 case. You may even consider not to stretch the x-axis of the lower panel and use the same 100 year axis for the ORCA025 run as for ORCA2. This way, the graphs will be comparable despite the very different extent of the simulations and the reader will hopefully be more easily convinced that the simulations have reached equilibrium with respect to iceberg mass. Moreover, you could add time series of calving and iceberg melt fluxes to show that these reach equilibrium much earlier than iceberg mass as you state in the main text (Section 3.1).

Response: We will consider these alternatives.

Figure 2: This snap shot of spatial iceberg distribution is somewhat confusing if com-

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pared to the 5yr mean of iceberg melt flux in Figure 2. For example, there are no icebergs north east of the Antarctic Peninsula in ORCA025 in Figure 2 despite a melt flux (Figure 3) of comparable magnitude to ORCA2. Is it possible to show a 5-year mean of iceberg "density" or, say, likelihood of iceberg presence?

Response: We will investigate this option, also in response to a comment of Referee #3.

Figure 6: Please indicate unit: "Sea ice thickness (in metres) : : :"

Response: Will do.

Figures 11 & 12: Please add longitudes used to define Ross and Weddell Sea sectors to captions.

Response: We will do this.

Responses to Anonymous Referee #3

This is an interesting paper discussing the implementation of an iceberg model into the NEMO climate model framework. The iceberg model is based on the work of Bigg et al (1996, 1997) and the Fortran code written by Martin and Adcroft, ported from the CM2G climate model. After an initial discussion of the iceberg model, the paper outlines some of the differences (e.g. SST, SSS etc) in Control integrations run with and without icebergs.

Overall I would like to have seen much more validation of the iceberg model, especially as this is one of the first times icebergs have been simulated in ocean models configured at eddy resolving ocean resolutions. This work needs to be done before any conclusions are drawn about how icebergs alter the physical properties of the ocean and also sea ice cover and thickness.

It would have been great to see some improvements to the iceberg model when porting the code over to NEMO. The iceberg model still only considers ocean drag forces at

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the surface level of the ocean, which is inadequate when simulating icebergs in ocean models that are eddy resolving as these typically have vertical grid spacing of 10's of meters in the upper ocean. This would allow for a much more accurate calculation of ocean drag and I suspect it would change the drift patterns. A similar issue relates to melting. It appears the melt scheme only uses SSTs, rather than an average of temperature over the entire keel. Making these changes would improve the model. Along similar lines, it would have been nice to see the code altered to include the interaction of icebergs with sea-ice, even if it was only 1-way so that icebergs in thick sea ice (>90%) drift with the pack ice (see Lighey and Hellmer, 2001). Such an addition to the model would probably be less than 5 lines of Fortran, and therefore minimal effort. Considering the extensive discussion of how icebergs influence sea ice growth and thickness it seems surprising that this was not done.

Response: Note that some of the following is repeated from a response to Referee #2. Regarding iceberg advection in the presence of vertically resolved currents, colleagues elsewhere in the NEMO community have recently modified ICB to use depth-averaged currents in the iceberg momentum balance, and compared iceberg trajectories thus obtained with those obtained (as here) using surface currents. There are some interesting differences in Weddell Sea in particular, where icebergs drifting with depth-averaged currents more readily take a "short cut" across the Weddell Sea. Vertically averaged temperatures are also being considered. In the revised manuscript, we will include these extended sensitivity studies with NEMO-ICB, in relation to ongoing development. Regarding sea ice, the reviewer is correct – icebergs in high concentration sea-ice tend to drift with the sea-ice. However, looking at motion of giant iceberg B31 over the winter of 2014 (in a recent project), this seems truest when the icebergs are frozen in to thick pack (essentially land-fast ice), rather than in areas where lead formation is common. Varying interaction with sea ice, as the referee suggests, is certainly an improvement that we should consider in a future version of NEMO-ICB, but verification is vital to get the parameterization right. We need a real time period and observed icebergs to do this, as planned in a new project starting now.

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The snapshot in figure 2 of iceberg distribution leaves the reader with little sense of how accurate the iceberg drift patterns are in NEMO. How are we to know if the iceberg model is accurately simulating iceberg motion? The drift patterns must be accurate in order to make meaningful inferences about how icebergs change sea ice cover/thickness. I would suggest plotting iceberg density over a 5-10 year period to highlight the main pathways of the iceberg drift. There is also a 100 year record of the number of icebergs passing south of 48N off the coast of Newfoundland. A figure comparing the observed number of icebergs passing this latitude to both the high and coarse resolution versions of NEMO would be very useful for such validation.

Response: We are very familiar with the 48N record (see Bigg et al. 2014), and we will attempt some comparison here. For the Southern Ocean, we will refer more to the published distributions for 2002–2010 of Tournadre et al. (2012). Direct comparisons of simulated iceberg density with the latter will be limited, as the satellite observations are used to estimate the average (2002-10) probability of icebergs in the Southern Ocean zone 45-66S, but will do what is practical in the revised manuscript.

Additional Comments: I was not clear if runoff from the ice sheet was partitioned into both calved ice as well as basal liquid melt. For example, observations at calving margins suggest that runoff at ice stream terminus can be >50% liquid runoff. From reading the paper it sounds like your 186 Gt ice calved from Greenland each year is released entirely as ice, without any liquid component?

Response: No, we do partition the mass flux between solid and liquid - this will be clarified in the revised manuscript.

What is the temperature and salinity of freshwater released into the ocean from icebergs? How is freshwater input from icebergs to the ocean treated? Do icebergs release cold water to the ocean and cool it when they melt? Is sea surface height altered in anyway?

Response: We add water at -4°C and 0 salinity, therefore cooling and freshening the

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ocean surface layer. The additional mass flux also alters the free surface height in NEMO. We will clarify this in the revised manuscript.

Over what period is ice calved from the ice sheets? For example, does the rate of calving increase in the summer or is it uniform throughout the year?

Response: The calving rate is uniform throughout the year.

Do the icebergs roll over? And if so, what stability criteria are used. Please state this. Ln 10, pg 5666: you write, "We also assume a given orientation for the iceberg relative to the wind...." What is it?

Response: Details are provided in the Bigg et al. (1997): see p. 117, end of section 3.1 for roll-over stability, with equation and reference. See earlier in that section for the direction discussion: effectively the berg is oriented at 45 degrees to the wind, with the wind to the left in the NH and right in SH.

Why is spin-up time so much shorter for ORCA025?

Response: We originally encountered a serious technical problem, related to advection of individual icebergs near a convergence of meridians at the North Pole. This problem is now resolved (in the course of developing these responses), and we are consequently able to run a longer spin-up with icebergs in the ORCA025 configuration.

Figure 1: Why is the total iceberg mass ∼1.25 times higher in ORCA2, compared to ORCA025?

Response: This difference is related to different ocean states: ORCA2 is colder, with slower currents, compared to ORCA025. See also our response to Referee #2 above.

Figure 2: As mentioned above, you should plot iceberg density instead of a snapshot of iceberg distribution. I was surprised to see so many icebergs clustering in central Arctic in ORCA025, which makes me concerned that the drift patterns are not realistic. I would have expected the icebergs to be more tightly constrained to narrow coastal

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boundary currents in ORCA025. In fact, ln 5, pg 5671 says "the majority of the icebergs follow the Labrdaor current", but this is not obvious from the figure. In general I find it hard to get a sense of how accurate iceberg drift is simulated in this model. Getting the drift correct has huge implications for accurately simulating where iceberg freshwater is added to the ocean model and therefore how and where the ocean responds to iceberg freshwater input.

Response: We will consider this switch (or simply remove Fig. 2).

Ln 12, pg 5673: Why does the presence of icebergs only lead to small changes in sea ice around Greenland/Arctic? And what about the Labrador Seas?

Response: There are fewer icebergs in the NH and these are quite scattered. They are not concentrated in one coastal current to the same extent as around Antarctica, where sea ice changes (due to icebergs) are consequently larger.

Ln 4, pg 5674: Please clarify what you mean by a 'strong warmer' by giving a percentage change. Similarly, on ln 7 of the same page you use the word 'extensive warming'. What order of magnitude classes as 'extensive' warming?

Response: We will clarify and quantify this statement.

Figure 6: Are +/- 0.1 m changes in ice thickness significant? And why are there such large differences in the changes at the two different spatial resolutions?

Response: We consider that these differences in sea ice thickness are locally important in relation to the mean (CONTROL) thickness, with implications for the freshwater input associated with sea ice freezing/melting, but different mean states are again important. Presumably, as the sea ice concentration is higher in ORCA2, there is more scope for local impact. A lot of the difference is also likely due to not resolving eddies in ORCA2, resulting in less horizontal mixing.

Responses to D. Goldberg (Editor)

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I am pleased to see that several knowledgeable researchers have taken an interest in your paper, and all seem to think it has merit.

All referees request a number of clarifications in the text, I ask that you make these clarifications or respond to each comment with your reasons for not changing the text. Reviewer #2 in particular has been very diligent and gives a very detailed list of comments, and I ask that you at the very least address them all in your referee response. Rev. 3 makes a nice suggestion to compare with a long observational iceberg record and I urge you to take this into consideration. Revs 2 and 3 suggest additional development work or more/longer model runs, I leave this to your discretion to weigh this additional effort against gains. However, in quite a few places the referees merely suggest that you present and/or add capability to generate additional diagnostics, I would urge you to follow these suggestions if possible. In my initial decision I questioned dynamic iceberg-sea ice interactions, and Revs 1 and to a lesser extent 2 have commented on this as well, I think you should give some thought as to how to address this issue in the manuscript and in future versions of the code.

Response: We are certainly willing to clarify the manuscript as advised, as outlined in specific responses above. Regarding comparison with a long observational record, we will compare simulations with the 48N record and Southern Ocean distributions for 2002–2010 (Tournadre et al. 2012), with caveats (see response to referee #3). A major decision, in response to the reviews, is to extend the ORCA025 simulation to at least 30 years, which will demand repeating most of the diagnostic calculations, and revising most of the tables and figures. In practice, we prefer to re-run from year 0 (ICEBERG and CONTROL), in line with ongoing NEMO development, and also because we have identified and now fixed a problem related to convergence of meridians at the North Pole, which previously caused NEMO-ICB to "hang". For this reason in particular, we request at least 2 months for major revisions. Finally, we will consider the (currently limited) extent of dynamic iceberg-sea ice interactions, and how this might be developed in the future. The revised manuscript will be attributed to the original authors plus four additional co-authors, in recognition of further assistance in addressing the extensive comments by referees 1-3.

To this discussion I would like to highlight this paper, should you choose to mention it (which I am certainly not requiring you to do): http://www.sciencedirect.com/science/article/pii/S0165232X11002436 In it, Morrison and Goldberg (but overwhelmingly for the most part J Morrison) estimate the degree to which momentum (input by sea ice wind stress) is transferred from sea ice to icebergs in the Weddell sea; as far as I know, it is the only in situ iceberg study which takes a look at this interaction in the Southern Ocean, albeit on a very limited spatial scale.

Response: We will look at this paper as we develop a revised discussion.

References (not cited in manuscript):

Bigg, G. R., Wadley, M. R., Stevens, D. P., and J. A. Johnson (1997). Modelling dynamics and thermodynamics of icebergs, Cold Reg. Sci. Technol., 26, 113–135, 1997.

Bigg, G. R., Wei, H. L., Wilton, D. J., Zhao, Y., Billings, S. A., Hanna, E., and V. Kadirkamanathan (2014). A century of variation in the dependence of Greenland iceberg calving on ice sheet surface mass balance and regional climate change. Proc. Roy. Soc. A, 470, DOI: 10.1098/rspa.2013.0662

Interactive comment on Geosci. Model Dev. Discuss., 7, 5661, 2014.