Dear Editor and Reviewers,

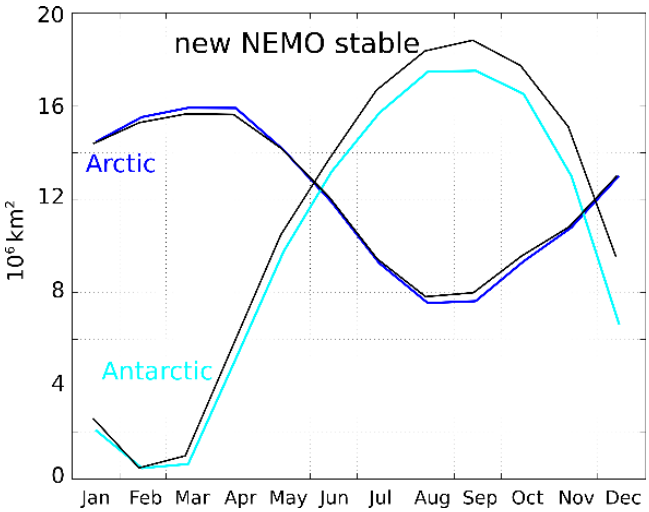
We thank you all for your time and comments that helped us to improve our paper. Answers to your comments are given in details hereafter, and we hope that you will find them satisfactory.

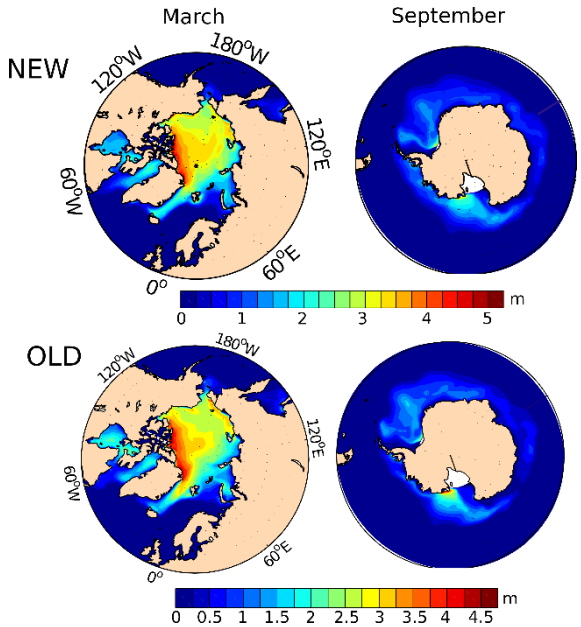
For the authors,

Clément Rousset.

General comments to both reviewers

1) The global simulation showed in this paper was based on a 6 months old NEMO3.6 (beta version), before its stable release (June 2015), that will form the basis for the upcoming CMIP6. The ocean model OPA has substantially changed since January, whereas the sea ice model LIM has remained nearly the same. Therefore we found appropriate to rerun the global simulation and present results from the new stable release of NEMO. Conclusions of this study are unaltered. The main changes are the larger ice thickness in the Arctic and the larger ice volume in the Antarctic. For illustration, we show the ice extent (left figure) and the ice thickness for the new simulation compared to the "old" one.





Moreover, three optional functionalities in LIM3 were added (virtual ice thickness distribution; flux redistributor for the ice-atmosphere thermal exchanges; embedded sea ice). They do not significantly change the physics of the sea ice code, but increase the possible uses of the code, hence we now describe them in the text. We have no intention to illustrate these capabilities further (they will probably be more detailed in the future), but we think this is important for LIM3 users to know about their existence. These modifications partly respond to the comment of Reviewer #1 on originality of the study.

- **Virtual thickness distribution.** We introduced parameterizations to virtually reproduce an ice thickness distribution while running the ice model with a unique ice category (mono-category), in order to have mono-category results roughly similar to the multi-category solution. It involves three additions that can be activated in mono-category: a) enhancement of the ice thermal conductivity b) explicit melting of thin ice c) simple pilling up of ice instead of ridging/rafting during advection. It makes the model computationally cheaper (which is a requirement for some users). LIM3 running in mono-category is roughly equivalent to LIM2.
- **Flux redistributor for ice-atmosphere exchanges**. This option enables the sea ice model to generate a thickness category dependence based on a single atmosphere-ice heat flux. This option is useful for some atmospheric models that are not able to see multiple surfaces, as the IPSL model for instance.
- **The effect of the ice/snow weight on SSH has been implemented (embedded sea ice).** In previous versions of the code, the ice weight had no effect on the SSH and hence on the surface pressure gradient. Now, this option has been implemented.

2) The structure of the paper has been slightly changed. Implementation of open boundary conditions is now (more naturally) in the “new features” part, which is organized as follows:
==> part 3.1. conservation (time step, heat diffusion)
==> part 3.2 lateral boundaries
==> part 3.3 ice cat boundaries
==> part 3.4 virtual thickness distribution
==> part 3.6 ice embedding
==> part 3.6 surface flux redistribution
==> part 3.7 input/output.

The paper structure is smoother (intro, model description + changes, model experiments, discussion)

Anonymous Referee #1

General comments

The model describes LIM, in particular changes since version 3.0. The first part of the manuscript is a technical report. There are almost no features that have not been described elsewhere. Further the presentation of mass budget contributions, salt budget is novel, but there are no further insights based on their presentation (maybe not necessary for GMD).

The reviewer argues that the manuscript looks like a technical report with no true step forward and that the effects of the innovations are not illustrated.

First, we think part of the reviewer dissatisfaction might come from the fact that he is not very familiar with GMD publications. To us, our paper is appropriate to GMD. In agreement with the journal description, our paper includes a geoscientific model description with development and technical aspects. Moreover, even if this paper is admittedly not as fancy as others, it is necessary to describe and illustrate the capabilities of this model which will build the base of (hopefully) future fancy studies, especially in the context of CMIP6.

We see at least four reasons why there is a need to describe LIM3.6 in this NEMO GMD special issue

(i) LIM3.6 is the reference component of the 3.6 version of NEMO (just released in June 2015.
(ii) This is the first version of NEMO with LIM3 as a reference, and LIM2 will be discontinued
(iii) NEMO3.6+LIM3.6 is planned to remain a reference for several years before a new release comes.
(iv) The 3.6 version of NEMO, with LIM3 as described in this paper forms the base for the CMIP6 experiments to come and will be used practically as such in at least two families of Earth System Models (IPSL-CM and EC-Earth).

Action: We made the motivation even more explicit, with a specific paragraph in the introduction (3rd paragraph).

Second, we tried to make the innovation aspects of the paper more obvious. Open boundaries are really new (not as claimed by the reviewer, see later) and improving robustness was critical (see below). We tried to advocate for this in a clear way. We also incorporated in the paper the new features that were added meanwhile and that were not explicitly described in the original version of the paper.

Action: The introduction states the new aspects more clearly (4th paragraph), and section 3 which is dedicated to the new features of the code now includes 7 items.

Taken together, all these new features (category boundaries, exact conservation, open boundaries, mono-category capabilities, flux redistribution, embedded sea ice) make in our opinion a sufficient body of work that needs to be documented since they allow new research that was previously not possible.

The operating splitting in the time step is standard in all sea ice models that I know

The reviewer is right saying that splitting the time step between thermodynamics and dynamics is not new to sea ice models but we felt it was important to explain that the splitting

was necessary for conservation purposes, and the rest of the developments. We agree, that this was overemphasized in the paper, though.

Action: We have reorganized the manuscript, so that the change of time stepping is not stressed as a major change and only appears as a tool to reach conservation. We now use "operator splitting method" as proposed

Conservation of properties, while not necessarily strictly enforced in all models, is not really a novelty

Showing the robustness[1] of the code in a scientific paper is extremely important in our opinion and GMD is just the good place to do so. Robustness has been achieved by developments described in the paper, which hides a very large amount of development work, including a thorough rewriting of many of the model routines. Strict conservation of properties is the most important achievement and gives very strong confidence in the model.

Open boundaries and regional models are not new to sea ice modeling (Dumont et al, 2008; Schodlok et al., 2012).

We think this statement by the reviewer is partly true and partly misleading. What we describe in the paper, a proper handling of open boundary conditions within the sea ice zone with a Flather radiation technique is new and opens new possibilities with NEMO. Besides, the two references used to strengthen the statement are not appropriate. Dumont et al use closed boundaries. Schodlok et al. is out of scope: it contains no information on sea ice modeling, even though the model domain may include some sea ice. It does not describe the technique used for handling open boundaries in the sea ice zone, it hardly mentions sea ice (one instance) and includes no single plot of sea ice.

We would definitely be happy to cite the references describing open boundary conditions in the sea ice zone, but to our knowledge, there are very few of them and they are not detailed at all. Having tested several options, we know that it can be technically complicated to handle open boundaries for sea ice in a clean way. It was a recurring problem in NEMO, and probably in other systems, as illustrated in the very recent paper by Sein et al on a regional European climate model with a boundary in the Arctic sea ice zone (JAMES 2015). They explain in the introduction that lateral boundary conditions are quite problematic. They confirm in their results that the solution close to the domain limit has to be discarded, and when they show a sea ice map as simulated by the model, they move their domain boundary condition out of the sea ice zone.

Other sea ice models (CICE & MITgcm) document how they handle open boundary conditions very briefly and it is hard to tell what they do. Hence, we avoided citing them.

CICE documentation, page 71: "In CICE5, the treatment of open boundary conditions derives from POP and is based on simple restoring of state variables".

MIT-GCM documentation: "In MITgcm, the ice concentration, thickness, snow depth and ice salinity are relaxed using Orlanski radiation conditions".

Action: we added "The treatment of open boundaries in sea ice model is not very much documented in the literature, hence we found it hard to compare this new approach to what

[1] By robustness, we mean that heat, mass and salt are conserved bitwise and that very-high resolution experiments are supported by the code.

is done in other models." at the end of the first paragraph of section 3.2

The presented simulations do not illustrate the effects of the new features (because there is no comparison to simulations without them), but simply describe two different model simulations

We understand the point of the reviewer. We had considered this when preparing the paper, but came to the conclusion that it is either not possible or useful.

Comparing the new version (LIM3.6) versus the old one (LIM3.0) is nearly impossible. We would need to plug in the old LIM to the new version of the ocean model (OPA), or the new LIM into the old OPA, which is very complicated since both OPA and LIM evolved simultaneously.

In addition, most of the changes described here do not really affect model behavior. Rather these changes expand the range of possibilities given to the user. These new possibilities are hard to illustrate using differences between experiments. For instance, how could we illustrate the fact that there are open boundaries capabilities, whereas there was no such thing before? We have also new category boundaries. It is now easier to handle domains where ice is thin, but that does not significantly change the results of the model. We do not think such experiments would be worth showing.

Action: we made the focus of the paper clearer in the introduction. We explained why it is timely and relevant to describe LIM3.6 and its possibilities now. We also explained that we wanted to document the new characteristics and possibilities of the code and that the simulations are only illustrations of the new capabilities.

The term "reasonably realistic" that is used to qualify the results is not convincing. There are simpler models that can do as well or even better, so that applying complicated models like LIM3 is not really justified a-posteriori.

There are two points in this statement.

1) "Reasonably realistic is not appropriate wording". We agree we could have done more efforts to tune our model in the Antarctic, which would have avoided us to justify the quality of the simulations. The model simulations are not precise, but the errors are not so large either. Besides, the concept of quality of a simulation is subjective: it depends on application. We better explain that we did not target specific tuning (which is user-dependent) and avoid quantifying the simulations too precisely. We also briefly discuss how the biases could be solved.

Action: We added this sentence in Section 4.1, explaining the experimental setup: *"The simulation presented here is the standard simulation that can be performed with the most recent 3.6 version of NEMO after downloading the code. It uses the main reference NEMO configuration (ORCA2-LIM3), and is forced by the reference CORE-NY forcing provided with the code. This is not the best simulation that can be produced, but rather the one that a user starting with the model could perform very quickly."*

We also added this sentence in the beginning of Section 4.2:

"*Neither the model nor the atmospheric forcing are precisely tuned to get the most realistic sea ice simulation, because this depends on forcing, resolution and user wishes."*

And at the end of 4.2:

"This simulation could obviously be improved through careful calibration, which depends on resolution and forcing. Calibration can be achieved by adjusting the atmospheric forcing and the vertical ocean physics as well as by tuning the most influential ice parameters (minimum lead fraction, ice strength and ice albedo). For instance, the Arctic ice thickness can be increased substantially by increasing the albedo, decreasing the minimum lead fraction or decreasing ice strength."

2) Simple models are as good or better. This is a very plain statement on something that is actually more of a scientific question: the required model complexity for a "good" sea ice simulation is under debate. There are no "good" or "bad" models in an absolute sense. The quality of a sea ice simulation depends on the scientific question, hence on what needs to be evaluated. It is indeed well accepted that simple models can do a good job for ice extent. For instance, a simple Hibler 2-level model with Semtner 0-layer easily fits the observed mean ice extent (Notz et al., JAMES 2013). However, such models will hardly capture the ice thickness variability in the Arctic, the variability of the ice volume and extent as well as the seasonality of ice temperature, which all require a more complex model to be simulated correctly (e.g. Massonnet et al, 2011; Holland et al., 2006). Versatile models such as LIM3.6 make assessable the question of model complexity for various scientific applications.

Action: We tried to avoid to exceedingly state the "quality", or "goodness" of the simulation.

The language is mostly OK. Some formulations sound awkward to me (grammar) and could be revised.

The language has been thoroughly revised, thanks to both reviewers' comments and to internal reviews.

Detailed comments (from the annotated pdf)

p. 3404, l. 5 « robustness and versatility are not new to sea ice modelling »
p. 3404, l. 10 « If these are bug fixes, then they don't contribute to the innovation character of a scientific paper »

To address these two comments, as explained in the general statement, we tried to better explain the originality and the scope of the paper.

p. 3405, l. 15 Maybe cite Hunke et al (2010), J. Glaciology, for sea ice modelling challenges?

Done

p. 3405, l. 29 Are open boundaries anything new to the community?

We argue that yes and explained why in the general comment.

p. 3406, l. 19 Not clear, if that's an "advance". There is always noise in EVP solutions, that is hard to come by, see, e.g., Hunke (2001), Bouillon et al (2014), Lemieux et al (2012), Kimmritz et al. (2015).

Agreed that EVP is not an "advance" stricto sensu. It has been introduced because of its small computational cost. However, the EVP coded in LIM3 is the one revisited by Bouillon et al, (2013) which converges better and removes some of the numerical noise. Also, the EVP of LIM is on a C-grid, which is an advance in the NEMO context.

Action: we modified the text to explain that the interesting feature is mostly the C-grid, and did not use "advances" but "features"

p. 3407, eq. 2. This representation is misleading: the first term on the rhs is a mixture of advection (u\nabla g) and ice convergence (g\nabla\cdot u), see, eg. Schulkes (1995)

Yes, this is actually the divergence of the flux of g, and not the advection. It has been reworded.

p. 3408, l. 21 "charge" not sure about the terminology. What is charge in this context? I am used to talk about a yield curve. What is the relation to "charge"

This was pure Frenglish; yield curve is the correct term

p. 3409, l. 6 See above references about EVP: EVP does not improve anything except for the time to solution (if you don't use very many [>O(1000)] sub-time steps).

Indeed. We added in the text the appropriate Kimmritz reference.

Action: The added sentence reads: EVP has to be used carefully, however, since even the modified EVP of Bouillon et al. (2013) hardly converges to the VP solution unless a large number (>500) of iterations is used (Kimmritz et al., 2015).

p. 3409, l. 12 What is the physical meaning of diffusion within the ice pack? Do the properties actually diffuse from ice floe to ice floe? I don't see any reason for using diffusion except for stability reasons.

Diffusion is indeed used in LIM3.6 as a numerical artefact to smooth the ice fields and to avoid instabilities due to non-linearities between advection and ice dynamics. Therefore the diffusion term was removed from eq. (3). The text was adapted. Our last tests without diffusion lead us to think that it could probably be completely removed in LIM3.6, at least for low-resolution configurations. Note that here is some indication that a real (but weak) diffusion is associated with unresolved short-term random floe motion (see Thorndike, 1986), but this is not the diffusion that we represent in the model.

Action: Diffusion has been removed from the main equation, and the description of diffusion has been restricted to section 2.2.2. We also clarified that diffusion is purely numerical in LIM and that D should be as small as possible.

p. 3409, l. 22 Please clearify, if you use the new smooth participation function of Lipscomb et al, as the text implies or the older method of Hilber/Thorndike, as the parameter H*=100m implies.

The participation and transfer functions are two different things. For participation, we use Lipscomb et al (2007) + Haapala (2001) to determine which ice categories are involved in ridging/rafting. For transfer, we use Hibler (1980) and not Lipscomb (2007) formulation (exponential ITD), following Lipscomb (2007) suggestions. The wording is precise enough as it is.

p. 3410, l. 8. /Speaking of heat exchanges that are assumed purely vertical/ But there is horizontal diffusion?

We clarified that horizontal diffusion is purely numerical.

p. 3412, l.3. « Snow-ice formation requires negative freeboard, which occurs if the snow load is large enough for the snow–ice interface to lie below sea level » The proper reference (because much earlier) is probably: Leppäranta, M., A growth model for black ice, snow and snow thickness in subarctic basins, Nordic Hydrology, 14, 59-70, 1983.

Thanks for this comment. The first instance of snow ice reporting we could find is actually in the great book by Zubov (1945), page 95 of the English translation (https://archive.org/details/arcticice00zubo). The snow ice parameterization we use can indeed be attributed to Leppäranta (83), for the freeboard condition, to Fichefet and Morales Maqueda for the heat balance and to Vancoppenolle et al (2009a) for the heat and salt balance in a mushy-layer framework.

Action: The three refs are added precisely.

p. 3414, l. 15. It has a name, too: operator splitting method. All ice models that I know, use this method to begin with (except for previous LIM3 versions, obviously)

Agreed, and as explained in the general statement, this section has been partially rewritten for more clarity.

p. 3416, l. 16. There is no such thing as a tri-polar grid (because mathematically speaking the sphere has a topological charge of -2), see Murray (1996). The ORCA grid is really cool, but it still only has two poles (singularities). The north pole is stretched into a line. I wish, people would not use this misleading expression anymore.

We understand your comment, but it is just a matter of language after all. By tripolar, we meant three mesh poles (and not geographic poles). The ORCA mesh presents no singularity inside the computational domain since two north mesh poles are introduced on lands (see Madec and Imbard, 1996). We have removed the world "tripolar" from the text because it is not necessary here, although we think it is formally ok.

p. 3417, l. 20 /Speaking of the overestimation of ice extent in the Greenland sea that we attributed to resolution/ **No, you see it also in very high resolution simulations. it's a problem with the forcing or generally with ice rheology (unclear, if you want my opinion)**

The proposed items could also explain this problem. Actually, attributing this bias is beyond our possibilities and is not the goal here. We revised the text and only proposed a few elements of explanation.

p. 3417, l. 25 /Speaking of the explanations why the Antarctic sea ice extent is wrong/**. These explanations are too simple. It is very well possible to tune a model so that it produces non-zero ice concentration in austral summer.**

Indeed, our text was vague and somehow meaningless. We revised the text, stressing what could be the problem and how the model could be tuned to improve the results.

p. 3418, l. 21 I find the presentation of these not so exciting results awkward. What kind of tuning do you suggest to improve the representation of sea ice? I have seen simulations with much simpler models that look better.

The presentation of the results has been completely revised. We avoided quantifying the goodness of the results, we have clarified the goals and reduced the amount of description. We gave indications on how the model could be tuned to give better results.

p. 3422, l. 14 It looks like the model cannot represent fast ice and the polynya opens at the wrong place compared to observations. This appears to be a common problem, especially associated with EVP (see Koenig-Beatty and Holland, 2012).

Indeed, the model cannot represent fast ice (this arguably needs to be parameterized, see Lemieux et al., JGR 2015) and it could be one of the reasons why the polynya does not open at the right place. We have added this precision in the text. However, as far as the size of the polynya is concerned, we believe that atmospheric forcing is a key point, as the text says. By comparing two simulations with 2 different atmospheric forcing (NCEP vs MM5 high resolution), Skogseth et al (2007) clearly show that the weakness of the opening comes from a too coarse atmospheric forcing.

p. 3423, l. 7. /Speaking of the volume of ice production in the polynya/ Or the wrong size of the polynyas (too small)? Your explanation is very weak and general (not convincing).

Indeed, the explanation was too vague. Yes, the size of the polynya matters and a deeper analysis is needed. But once again, the simulation should be viewed more like an illustration than a real scientific study. The paper focuses on model developments (with illustrations). A

scientific study on the Storfjorden polynya will come.

Action: We added a sentence at the end of 5.2 "This could be related to the small size of the simulated polynya and / or to the lack of high-resolution, high-frequency winds in the ERAI forcing, and should be further investigated."

p. 3435. Figures are very small.

The size of figures is enlarged.

p. 3437. Ice thickness is far too low in the Western/Southern Weddell Sea where most sea ice is produced.

Indeed, and this is a common problem. The factor responsible is the wind forcing derived from NCEP which shows a divergence in the western Weddell Sea instead of a convergence. Ice is then driven away from the coast.

Action: We added the following sentence in the text: "The band of thick ice along the east side of the Antarctic peninsula is missing, which is attributed to misrepresented winds in NCEP (Timmermann et al., 2005; Vancoppenolle et al., 2009b)"

p. 3437. The ice appears to be fairly thin in Arctic for the period (mean of 1984-2000) in question. Also: is the strip of thick ice along the Siberian coast realistic?

The Arctic ice is between 2.25 and 5 m in the Arctic, which is in the range of uncertainty for this period. The mean is actually ok, but the spatial distribution is not, with indeed the spurious strip of ice along the Siberian coast.

Action: The sentence "The spatial distribution follows expectations, except a spurious band of thick ice along the East Siberian shelf " was added. We also avoided to give definite explanations when we cannot do so.

p. 3438. You can remove "dynamical growth" as it is (trivially?) zero, to make the plots a little simpler.

Indeed, advection is 0 but what we call dynamical growth is not zero, because it also includes the refreezing of the water trapped during ridge building. « Dynamical growth » is larger in the Arctic than in the Antarctic since ice is more deformed in the northern hemisphere (although we agree it is hard to tell from the figures).

Action: The sentence "Hence, in contrast with other models, the net thermodynamic ice production during convergence is not zero, since mass is added to the sea ice due to ridging" was added at the end of Section 2.2.3.

Reviewer #2

General comments

It would be useful if the authors, when describing the model, compared and contrasted its various aspects to the other state-of-the-art sea-ice models. This is especially true of the vertical halodynamics which differ significantly with other models and is one of the more advanced features of LIM.

We have added a pieces of sentences here and there to illustrate how different from its friends LIM is, but doing it in a systematic way is complicated and would rather have its place in a synthetic study.

Technically, all models share the same principles and structure, which were laid up by Coon et al. (1974) in the AIDJEX bulletin.

The specificities of each sea ice model is in the details of parameterization and implementation. It is hard to be systematic since little details matter. Besides, the other models evolve with time, and therefore, the comparison must always been done to a reference version.

Action: We added at the beginning of the model description: *"LIM3, as other multi-category models (e.g. CICE, Hunke et al., 2013), is based on the AIDJEX modeling framework (Coon et al., 1974). LIM3 mostly differs from other models in terms of parameterizations and implementation details."*

We also specified in the text where LIM differs from other models (C-grid model, ridge porosity, salinity equation)

2) The authors should describe the effect on the simulation results that moving from the 3.0 to 3.6 version of LIM has. Given these changes were mainly solver changes I would expect the effect to be small but it would be nice to have this discussed

We understand the reviewer suggestion, but there are two hardly surmountable difficulties to make a clean comparison.

First, to compare the sole effect of the ice model change, we must have the same version of the ocean model OPA below. And (un)fortunately, the ocean code and its interfaces to sea ice and the atmosphere have changed a lot over the last couple of years. To plug an old version of OPA on the new LIM or conversely is a real nightmare.

Second, the most relevant feature to look at would be the ice mass, salt and heat budgets. However these budgets are only implemented in LIM3.6 and would require a lot of work to be implemented in LIM3.0 (which would be a waste of time since this version is no more supported). The main problem comes from the change in the time step (splitting) and the complete rewriting of the code. All in all, we understand the curiosity of both reviewers (and maybe future readers) about what has finally changed in the simulations from v3.0 to v3.6, but isolating the sole effect of the ice is hardly feasible.

Action: We tried to illustrate that the scope of the paper is to illustrate the new capabilities of

the model, not to explain differences with previous version.

3) Some small discussion should be given on why this is v3.5 and not v4.0. Was left wondering what happened to 3.1, 3.2, 3.3, 3.4...

Versions 3.1 to 3.4 were working versions and have never been released. We use 3.6 in the end, just to match with NEMO standards.

Detailed comments

We detail the answers to specific comments when a detailed answer is needed. All other comments were readily accounted for.

Pg 3404, Ln 2: "novelty is" -> "novelty presented is"
Pg 3404, Ln 7: "enables to": incorrect English
Pg 3404, Ln 8: "online inspection ... of the code". I don't understand this sentence.
Pg 3404, Ln 14: "and is found ... done." -> "and is found to be reasonably realistic, although no specific tuning was performed."
Pg 3406, Ln 13: "moment-conserving scheme" -> "moment conserving advection scheme"
Pg 3406, Ln 14: "LIM2 as it was" -> "LIM2 when it was"

Pg 3408, Ln 3: "horizontal diffusion": Was confused by the addition here until it was revealed much later to only be numerical in nature.

You are right that this is confusing and the other reviewer was also mentioning it. We have removed horizontal diffusion from this equation since this is the only term which is not entirely physical but mostly numerical.

Moreover, some of our recent simulations with no diffusion suggest that diffusion is not needed anymore in at least the low-resolution configurations. This indicates that LIM3.6 generates less noise than LIM2 and LIM3.0. But it is not clear why it is so. At this stage, we do not really know how long will the horizontal diffusion be kept in the code.

Diffusion of sea ice variables could also be physical, associated with unresolved high-frequency motions (see Thorndike, 1986).
Thorndike, A. S. (1986). Diffusion of sea ice. Journal of Geophysical Research: Oceans (1978–2012), 91(C6), 7691-7696

Action: Diffusion has been removed from the main equation, and the description of diffusion has been restricted to section 2.2.2. We also clarified that diffusion is purely numerical in LIM and that D should be as small as possible.

Pg 3410, Ln 4: "usually 50%": When would it not be 50%?

Usually in the sense that this is a choice subject to debate, but it is always 50% in the code.
Action: We have removed "usually"

Pg 3411, Ln 7: why is L=1m? Whats the justification for exactly 1 m?

This extinction coefficient is in the range [~0.5 - 2 m-1] of observed values for dry and melting ice (see Light et al., 2008). It depends also on the gas and impurity content, which are not represented.

Light, B., Grenfell, T. C., & Perovich, D. K. (2008). Transmission and absorption of solar radiation by Arctic sea ice during the melt season. Journal of Geophysical Research: Oceans,

113(C3).
Action: We added the reference to Light et al.

Pg 3411, Ln 15: Don't start a sentence with "And".

Pg 3413, Ln 14: "Brine drainage ... ocean salinity.": There is no exchange of water mass in the model maybe but surely there is in reality. Be clear with the distinction.

True. In reality, there is an exchange of water during brine drainage. In the model, it cannot be represented. This is because a mass exchange associated with brine drainage would have to be reflected by a modification in the ice density. However, the ice density in our model is assumed constant and breaking this assumption would involve a complete rewriting of the thermodynamics
Action: Added "Because the ice density is assumed constant, brine drainage cannot be associated with an ice-ocean water mass exchange (the ice density would have to change to be conservative). Instead, the salt lost by sea ice through brine drainage directly increases ocean salinity." at the end of 2.3.3.

Pg 3413, Ln 17: "neighbor" -> "neighbour": British English

Pg 3414: Seems section 3 should be renamed as something like "Changes in LIM3.6 » since section 3.3 doesn't fit with the title to section 3.

Agreed, it is a very sensible idea, indeed.
Action: we have changed the name of this section to "New features in LIM3.6". In addition, part 3.1 is renamed as "Control of mass, heat and salt budgets" since this is the real goal here (the new time stepping is only a necessary change to reach it).

Pg 3414, Ln 5: contributions to what?

Action: we changed to "contributions to the mass, heat and salt budgets".

Pg 3414, Ln 11: "however" -> ", however, »
Pg 3414: There is strange use of tense in section 3.2.
Pg 3414, Ln 24-25: It wasn't immediately clear that the authors were going to discuss how the scheme doesn't conserve momentum. Perhaps adding "As discussed next..."

Pg 3415, Section 3.2: The authors don't seem sure what CICE does in this situation in terms of conservation. A personal communication would seem to be in order if it isn't apparent in any CICE manuals or papers.

Action: We emailed our CICE friends (Elizabeth Hunke). They have nothing like that yet and admit to have trouble to reach perfect heat conservation. Elizabeth said she would add that to her to do list. We believe we do not need to tell this in the paper.

Pg 3415, Ln 15: "but is hardly adaptable": somewhat aggressive phrasing.
Pg 3416, Ln 1: "In and Outputs" -> "Inputs and Outputs »
Pg 3416, Ln 2: Remove « completely"
Pg 3416, Ln 7: "possibilities of variables manipulation" -> "possibilities for variable manipulation"
Pg 3416, Ln 9: "performance to output data" -> "performance when outputting data »
Pg 3416, Ln 10: "File system writing is totally overlapped by computation" -> "File system writing is performed concurrently with computation. »

Pg 3417, Ln 6: "no direct observations" Don't know what this means given the existence of icesat, cryosat, submarine data etc.

The reviewer is right. Indeed, there are observations of ice thickness, mostly in the Arctic. However, these observations are not able to provide reliable estimates of the seasonal cycle of the global hemispheric ice volume (especially in the Antarctic). Submarine data are too sparse, whereas satellite data are limited to the Arctic Basin and very much dependent on the snow load which is poorly known, leading to large errors (+- 50 cm, see Zygmuntowska et al., 2014). But you are right, the sentence is misleading and we have rephrased it.

Action: We added the sentence at the end of Section 4.1: "To provide an observational context to the simulated ice volume, we do not use observational estimates, for which uncertainties are very large (e.g. Zygmuntowska et al., 2014), but instead the 1979-2011 reanalysis PIOMAS in the Arctic (Schweiger et al., 2011), and the NEMO-LIM2-EnKF reconstruction in the Antarctic (Massonnet et al., 2013)."

Zygmuntowska, M., Rampal, P., Ivanova, N., & Smedsrud, L. H. (2014). Uncertainties in Arctic sea ice thickness and volume: new estimates and implications for trends. Arctic sea ice altimetry-advances and current uncertainties.

Pg 3417, Ln 21: "to low an" -> "to too low an" (although this sounds clunky it is more correct English, you might want to change the whole sentence).
Pg 3417, Ln 21: "heat supply by" -> "heat supply in ».
Pg 3418, Ln 5: "too strong a vertical ocean mixing" -> "too strong vertical ocean mixing" C1163
Pg 3418, Ln 6: "classic" -> "common"
Pg 3418, Ln 23: "allow to split" Not correct English

Pg 3419, Ln 10: "slightly up to ": Don't understand this

Sorry. We meant "slightly larger than"

Pg 3419, section 3.4.3: Is there really that much snow-ice formation in the Arctic in reality?

You probably meant « in the Antarctic », because we do not mention the Arctic for snow-ice formation and our Figure 6 does not suggest anything significant. In the Antarctic, according to textural analyses, snow-ice formation makes about 13-25% of the sea ice mass balance (Worby et al. 98; Jeffries et al, 97). Based on a combination of satellite and reanalysis products, Maksym and Markus (2008) reach about 15-30 cm of annual snow-ice production. Our simulations are consistent with these results.

Pg 3420, Ln 12: "way similar" -> "similar way"
Pg 3420, Ln 23: "The category filling ... was used." Clunky sentence - reword.

Pg 3421, Ln 7: "recorded": when?

We changed the tense to simple present, because the tidal signal has lows and highs all year long (2 weeks frequency). So "when" is not really relevant.

Pg 3421, Ln 10: "role for climate" -> "role in climate"
Pg 3421, Ln 17: "uniform levels" -> "uniform ocean levels"
Pg 3421, Ln 24: "\kappa-\eta" -> "The \kappa-\eta"

Pg 3421, Ln 23: Why was this third order scheme used instead of FCT?

Action: We added "Using a third order advection scheme (like UBS) instead of a 1 order scheme (like TVD) is common at high resolution. It is more precise and removes the need of having explicit diffusion (diffusion is implicit in UBS). Diffusion is then minimal and depends on the ocean currents. Ocean structures can then develop without being impeded by homogeneous diffusion."

Pg 3422, Ln 10: "The opening of polynyas is generally well represented" In what sense?

Action: We added “The opening of polynyas, in terms of timing, location and size, is generally reasonably well represented by the simulation, in Storfjorden and elsewhere”

Pg 3422, Ln 24: "globally": Strange use of word. Maybe: "while it is generally weak in the Arctic basin »
Pg 3423, Ln 9: "considerably evolved" -> "evolved considerably »
Pg 3423, Ln 10: "were developed" -> "have been developed »
Pg 3423, Ln 10: "coexist up to now" -> "have coexisted up until now »
Pg 3423, Ln 12: "LIM2 was the reference model so far" -> "LIM2 has been the reference model to date. »
Pg 3423, Ln 20: "code is changed" -> "code was changed"
Pg 3423, Section 5. As noted above tense is not used well in this section.
Pg 3423, Ln 21: "which enables to discriminate": not correct English
Pg 3424, Ln 7: "showed" -> "present"
Pg 3424, Ln 27: "is yet to achieve" -> "is yet to be achieved"
Figure 2: "CICE and used ... is represented" -> "CICE, which i15s used in the former version 3.0 of LIM, is represented"
Figure 3: If you use "delimits" the sentence should be something like "The white line delimits the 15% ice concentration region." Or "The white line signifies the 15% ice concentration contour".
Figure 5: "at the time of the maximum": -> "at the time of maximum"