Author's response to comments from C Rodehacke (Referee 1)

The manuscript of Nowicki and others describes the foundation and reasoning of the Ice Sheet Model Intercomparison Project (ISMIP6) and its relation the coming CMIP6 exercise.

Under the frame of the Climate and Cryosphere (CliC) and World Climate Research Project (WCRP) model intercomparisons have been and will be a tool to project the future evolution of the earth's climate system. The understanding of how massive land ice masses — such as Antarctica and the Greenland ice sheet — will melt under a changing climate and contribute to an already globally raising sea level is crucial in the context of climate adaptation and mitigation efforts. The manuscript clearly contrasts the difference between various model experiment setups and motivates the usage of these experiments. It also highlights the relationship to former and coming "traditional" CMIP experiments. The manuscript will certainly act as the reference for the described ISMIP6 exercise.

The manuscript is very well written, has a clear structure and all tables and figures are necessary and well prepared. It was a pleasure to review this manuscript. I hope that the manuscript could be published soon, because I will be extremely helpful to have this information for the involved groups as well as the wider CMIP6 audience.

I recommend the publication of the manuscript after few minor corrections.

A detailed list of comments including the text above is given in the attached pdf-file.

We thank the reviewer for the helpful comments. We have now revised our manuscript in light of these and other comments that we have received. A point-by-point reply is given below.

Major Issues
None

Minor Issues
First I give general comments and afterwards specific comments.

General Comments
In the manuscript I prefer a consistent spelling of either "preindustrial" or "pre-industrial".

We have checked the manuscript and used a consistent spelling of "pre-industrial"

Since this manuscript will probably be a reference for the research groups participating, you may add a small table with the essential deadlines to the section 3.4 "Prioritization of experiments and timing". It could act like a "checkbox" table. Please share your thoughts about it.

We thank the reviewer for this idea. However, the dependence of most of the proposed ISMIP6 experiments on model output from climate modeling centers makes it impossible for us to foresee exact timing of the initiatives at this stage.

In the tables A1, A2, and A3 or/and in the appendix A "Variable Request", I would like to see the definition of the flux direction (sign convention). Is ablation (ice loss) a positive or

negative flux? Please clarify the text and add (also) please a short remark to the corresponding table captions.

We thank the reviewer for the suggestion. We have added general statements to the table captions to clarify the flux directions.
Table A1: Flux variables are defined positive when the process adds mass or energy to the ice sheet and negative otherwise.
Table A2: Flux variables are defined positive when the process adds mass to the ocean and negative otherwise.
Table A3: Flux variables are defined positive when the process adds mass or energy to the ice sheet and negative otherwise.

Specific comments
In the following specific comments are made, where "P3L23" means line 23 on page 3, for instance.

P4L21: The term ''offline'', may not be known to a general audience. You may rephrase item ii): 'standalone dynamic ice sheet models (ISMs) that are driven by provided forcing fields ("offline").'
We have done the suggested rephrasing.

P4L26: I personally find the suddenly appearing "XXX" confusing. You may add the sub-clause: where XXX stands for different forcing scenarios as described later.
We have added the suggested sub-clause.

P5L16: In the bracket the term "fixed is vague. You may mean "reference ice sheet extent and topography"? If so, please specify a possible reference for illustration.
We believe 'fixed' is the right term here, referring to the prescribed topography and albedo in classic GCM simulations. Referring to a 'reference' instead would add confusion, since there is really only one state of the ice sheets in these simulations.

P5L27: You may add "pre-industrial" to obtain:"… is meant to capture the pre-industrial quasi-equilibrium state of the climate system."
We have done the suggested rephrasing.

P6L25: You state to use the same ice sheet initial condition, which comes from the coupled XXX-withism run, for the XXX-withism and the ism-XXX-self simulations. Since the geometry of the ice sheet could be quite different between the standard AOGCM and the coupled AOGCM-ISM in terms of ice sheet elevation, for instance, the starting conditions and climatic forcing of the standard AOGCM may not be consistent with the XXX-withism ice sheet. Hence the forced ice sheet may show a considerable drift and ultimately this drift overprints the actually wanted impact of the difference between coupled vs uncoupled simulations for simulations of about 150 years. Could be please be so kind and comment.
We agree that there is a danger of a drift dominated by any signal forced by the difference between coupled and uncoupled simulations. We could lessen this drift by using SMB anomalies. However, the potential for a drift is one of the reasons why we are not relying on the coupled modeling for our projections, as we expect the results of the standalone ice sheet modeling to be more robust. The coupled modeling is primarily done so that issues (such as this) created by coupling climate and ice sheet models are exposed, and the community can start to work towards resolving them.

The ism-piControl-self will be used to quantify the drift, which we can subtract from the ism-1pctCO2-self, to get the effect of climate change. However, if the drift is large, this may not be satisfactory. We hope that the AOGCM SMB will be realistic enough and that the spin-up geometry between the standard AOGCM and the coupled AOGCM-ISM is not hugely different, and therefore the drift minimal.

We have restructured the manuscript so that the discussion of the spin-up is now before the coupled experiments. In the new structure, the original first three paragraphs have been left unchanged. The fourth paragraph now discusses the spin-up, and it mainly based on the original sixth paragraph, with any change due to reading flow or needed to address comments about spin-up. The fifth paragraph states the ideal of using actual SMB forcing from the AOGCM, and is the bulk of the old fourth paragraph adapted to address your concerns about initial conditions and drift. The remainder of the section is then unchanged.

The fifth paragraph reads "Ideally, the ice sheet model should be forced with the actual SMB computed by the climate model, rather than an SMB corrected to match observed climatology. We accept that there may be biases in the atmospheric or land models that can lead to an unrealistic SMB, which could result in a steady-state ice sheet geometry that differs substantially from present-day observations. However, correcting for these biases can distort the feedbacks between ice sheets and climate that we seek to investigate. We hope to learn from and ultimately reduce these biases, in the same way that biases elsewhere in the simulated coupled climate system are reduced by greater understanding and improved model design. On the other hand, if the geometry of the spun-up ice sheet is greatly different from observations, then the initial ice sheet may be far from steady state with the SMB forcing from the standard, uncoupled AOGCM. As a result, the *ism-piControl-self* experiment could have a large drift that obscures the climate signal. If this is the case, or in general if the spun-up ice sheet in the coupled system is deemed to be too unrealistic, an alternative spin-up method would be to apply SMB anomalies from the AOGCM, superposed on a climatology that yields more realistic equilibrium ice sheet geometry."

P7L8: I'm unsure if a 'the' is missing:" The choice of the ice sheet model, …. ."
This has been changed as suggested.

P7L14: You may add:" However, any correction… ."
This has been changed as suggested.

P7L22-23: I'm skeptical about the implicit statement that internally computed surface mass balance (SMB) calculations are automatically mass and energy conserving while externally computed SMB are not. A wrong regridding from the probably coarse atmospheric grid to the finer ice sheet model grid could break the conservation (Fischer et al., 2014), regardless if the computation is performed inside or outside the AOGCM. I would like to suggest a more general phrasing such as:"… SMB is obtained from energy based method that conserves mass and energy. It facilitates interpretation of the drivers of SMB variability and change …."
We have done the suggested rephrasing.

P7L29: I guess I understand what is meant by a "realistic" state, but I would claim that this state is uncertain for the pre-industrial era and that an ice sheet state that is consistent with the driving AOGCM climate is more important. Hence you may agree in replacing "… to produce a realistic non-drifting coupled state" with "… to produce a consistent non-drifting coupled state".
We have done the suggested rephrasing.

P7L29: As mentioned above, the pre-industrial state is likely different than the contemporary observed state. Hence you may add the following sub-clause:"... to the pre-industrial (1850) climate, which is different from the contemporary state (Kjeldsen et al., 2015)."
This has been added as suggested.

P10L29: I would like to suggest a slight clarification:" … temperature changes using the relation of Rignot and Jacobs (2002) of 10 yr-1 °C-1 for temperatures above the actual ocean's freezing temperature." Please use "°C" instead of "C".
This has been fixed as suggested.

P11L11: For the first time initMIP is mentioned. Please either introduce it or mention where it is described below.
Following a suggestion of Reviewer 2, we have moved the initMIP description earlier in the text, which solves this problem.

P11L12: I'm sorry, but I do not understand or could not find the referenced section provided in the bracket. Please clarify. In addition this information seems to disagree with the information in the bracket below (P11L22).
Thank you for spotting this, there was a mistake in the referencing. This has been solved along with the changes described in response to the comment before.

P11L23: In my humble opinion a 1% raising atmospheric CO2 concentration has not a linear trend. Hence I suggest:" considers a 1%/year atmospheric CO2 concentration rise until quadrupled concentration and stabilization thereafter."
We have done the suggested rephrasing.

P11L27: Here you may add:" … to pre-industrial conditions, which is probably weaker, constrained than the contemporary state."
This has been added as suggested.

P11L31: Is the leading "to" needed?
This has been kept, as the sentence is "are likely to differ", if the "are" had not been used, we would have indeed removed the "to".

P12L28: You may indicate that some groups have provided longer runs by stating:"… each run for at least one hundred years."
This has been added as suggested.

P13L8/9: I'm not sure but maybe a pronoun is missing:"… geometric changes in these forward experiments." Please check.
Indeed a pronoun was missing and was added as suggested.

P14L31: I would like to suggest to add a more recent citation for the HIRHAM model: (Langen et al., 2015; Lucas-Picher et al., 2012)
This has been added as suggested.

P15L4: For the Greenland ice sheet a very valuable set of observations in the ablation zone comes from the PROMICE network. Therefore I suggest the following change:" … known as the GC-Net (Steffen and Box, 2001), PROMICE network with a focus on the ablation

zone (Ahlstrøm et al., 2008)".
This has been added as suggested.

P15L28: In addition to the common glaciological estimates I would like to add the following:" … can be compared with glaciological estimates of ice shelf melting around Antarctica (Rignot et al., 2013; Depoorter et al., 2013) as well as independent tracer-oceanographic estimates (Loose et al., 2009; Rodehacke et al., 2006)."
This has been added as suggested.

P17L27: You may highlight the coupled simulations in the conclusion by extending:"… no dynamic ice sheets, coupled AOGCM-ISM, and standalone…."
This has been added as suggested.

P19L16: Some glaciologists may feel more welcome when instead 'lost' the common term 'ablation' is also used. What do you thing about:" … and ablation to the ocean by either calving or melting."
This has been changed as suggested.

Tables
Here I refer to the table number
Table 2: Please correct the entry for the EC-Earth model. Here the Danish Meteorological Institute (DMI) in Denmark has expressed the interest in the name of the entire consortium.
This has been fixed as suggested.

Table A1, A2, A3: I believe the fractional quantities refer to the total ice covered area. Please clarify and mention it in the table caption.
For a gridded data set, these variables conventionally give the fractional area covered by the quantity in a grid cell. No further clarifications needed.

Table A1, A2, A3: Please indicate in the table caption the sign convention of the fluxes, as already mentioned the general comments section above.
Additional clarifications have been added to the captions. See also reply to general comment above.

Table A2: Please clarify what is the base line of the "Global Average Thermosteric Sea Level Change"? Is it the beginning of each individual simulation or since the historical period started in 1850, for instance?
The data request tables are thought to be universal and would apply equally to e.g. paleo simulations. The standard sea level reference is therefore the beginning of the individual simulation, but may have to be specified for certain cases.

Figures
The figure numbers are given.
Figure A2: Since runoff leaves the snowpack, I would prefer that the arrow points beyond the snowpack.
This has been changed as suggested.

References
Ahlstrøm, A. P., Gravesen, P., Andersen, S. B., van As, D., Citterio, M., Fausto, R. S., Nielsen, S., Jepsen, H. F., Kristensen, S. S., Christensen, E. L., Stenseng, L., Forsberg, R.,

Hanson, S., Petersen, D. and Team, P. P.: A new programme for monitoring the mass loss of the Greenland ice sheet, Copenhagen, Denmark. [online] Available from: http://www.geus.dk/DK/publications/geol-survey-dk-gl-bull/15/Documents/nr15_p61-64.pdf, 2008.

Fischer, R., Nowicki, S., Kelley, M. and Schmidt, G. A.: A system of conservative regridding for ice–atmosphere coupling in a General Circulation Model (GCM), Geosci. Model Dev., 7(3), 883–907, doi:10.5194/gmd-7-883-2014, 2014.

Kjeldsen, K. K., Korsgaard, N. J., Bjørk, A. A., Khan, S. A., Box, J. E., Funder, S., Larsen, N. K., Bamber, J. L., Colgan, W., van den Broeke, M., Siggaard-Andersen, M.-L., Nuth, C., Schomacker, A., Andresen, C. S., Willerslev, E. and Kjær, K. H.: Spatial and temporal distribution of mass loss from the Greenland Ice Sheet since AD 1900, Nature, 528(7582), 396–400, doi:10.1038/nature16183, 2015.

Langen, P. L., Mottram, R. H., Christensen, J. H., Boberg, F., Rodehacke, C. B., Stendel, M., van As, D., Ahlstrøm, A. P., Mortensen, J., Rysgaard, S., Petersen, D., Svendsen, K. H., A›algeirsdóttir, G. and Cappelen, J.: Quantifying energy and mass fluxes controlling Godthåbsfjord freshwater input in a 5 km simulation (1991-2012), J. Clim., 28(9), 3694–3713, doi:10.1175/JCLI-D-14-00271.1, 2015.

Loose, B., Schlosser, P., Smethie, W. M. and Jacobs, S.: An optimized estimate of glacial melt from the Ross Ice Shelf using noble gases, stable isotopes, and CFC transient tracers, J. Geophys. Res., 114(C8), C08007, doi:10.1029/2008JC005048, 2009.

Lucas-Picher, P., Wulff-Nielsen, M., Christensen, J. H., A›algeirsdóttir, G., Mottram, R. and Simonsen, S. B.: Very high resolution regional climate model simulations over Greenland: Identifying added value, J. Geophys. Res., 117(D02108), 16pp, doi:10.1029/2011JD016267, 2012.

Rodehacke, C. B., Hellmer, H. H., Huhn, O. and Beckmann, A.: Ocean/ice shelf interaction in the southern Weddell Sea: results of a regional numerical helium/neon simulation, Ocean Dyn., 57(1), 1–11, doi:10.1007/s10236-006-0073-2, 2006.

We thank the reviewer for the detailed comments. Most of the references have also been included in the manuscript.