Review of the paper entitled: "Semi-equilibrated global sea-level change projections for the next 10,000 years", by Jonas Van Breedam, Heiko Goelzer, Philippe Huybrechts.

In this paper, the authors aim to investigate the evolution of Greenland, Antarctica ice sheets and minor components of sea-level rise over a long period (10ky) using an integrative strategy.
A model of intermediate complexity, LOVECLIMv1.3, is coupled with ice sheets models (Greenland and Antarctica) that enable them to test different pCO2 and methane scenarios for the period of 1000 to 10000 years.
They investigate indeed the response of the Earth climate system to a large but short lasting perturbation. It is necessary to run long simulations that account for long time response of deep-ocean, ice sheets and CO2 evolution.
The authors first describe the tool they used, the originality of which is to account for feedback between atmosphere/ocean and ice sheet, then they describe the scenarios they chose and they finally present their results in terms of different contributions.
The paper is well written and the issues are interesting, nevertheless there is room for improvements on several points.

1. Discussion of the limitation of the study and its possible consequences:
The scenarios are prescribed from an initial perturbation based on the four RCPs of IPCC scenarios and 2 supplementary scenarios. This paper represents an improvement compared to previous studies because feedbacks between climate and cryosphere are accounted for. The discussion on the CO2 evolution, which is driven by different anthropic pathways, is only discussed from a "mathematical" point of view. The authors should discuss the limitation of such an approach. Indeed there are also interactions between carbon cycle and ocean and interactions between vegetation and carbon cycle, which are not limited to permafrost and clathrate destabilizations, and which are not accounted for. For instance, a dynamical vegetation model could be useful to account for the effect of desertification (albedo and water cycle). Another issue that is not discussed is the long-term climate evolution. Indeed all scenarios depict a complete melting of the GRIS. The simulated climates are similar to Pliocene climate and thus the impact of orbital forcing/orbital parameters may be drastically modified in comparison with Quaternary large glacial/interglacial oscillations.

2. Computation of grounding line evolution with coarse-grid modeling:
The response of the grounding line is very important and should be discussed in more details because it is difficult to compute it using climate change simulattion by a coarse-grid model. For instance, to test the capability of their model in order to compute correctly changes in grounding line, the authors could use the last deglaciation and they should validate their model over such a period.

3. Parametrization and scenarios:
Line 114-115: in addition to the fact that the authors wrote P11 in the text and P71 in the Table S3, this part of the paper is very unclear to me. The authors chose one set of parameters, and with this set, they provide 6 different simulations. All these scenarios do not need to be run again. But for the 2 extreme ones (RCP2.6 and feedback), we would like to have the result when using different parametrizations as far as LOVECLIMv1.3 needs parameterization to compensate the approximation made.
Indeed we would like to know how much the results are dependent on the parametrization.
Moreover, the scenario using methane from clathrate emission is important not only in terms of quantity of greenhouse gases emitted but these emissions could last several kyears, which is the duration of Paleocene-Eocene thermal maximum. The authors should explain the reason of their choice.
If the authors account for these main comments and more analytical comments below, I consider that this paper is a valuable contribution to an important issue and should be published in Earth System Dynamics.

More detailed comments:

- Title: the author use the term "semi-equilibrated", which is never clearly defined. I would prefer "quasi-equilibrium" but anyway the author should give an objective criteria for this term.
- Abstract: what is a semi-equilibrium? Over 10ka it may be important to account for astronomical forcing, especially for precession cycle (the period of interest here 1-10 ky is half the duration of precession cycle). I don't really understand the last sentence of the abstract: there is no geologic analogue for the next 10ky in Earth history - as far as I know.

How is it possible to reach more than 5800 GtC?

1. Introduction

The introduction if fine but 2 topics should be introduced or developed:

1. The last deglaciation, which lasts around 10 000 y, is an interesting period to validate the model used here. The authors should discuss this point, which is completely absent in the introduction.
2. The methane hypothesis should be clearly explained. There is a first short term feedback linked to the permafrost melting and a long term effect on clathrate destabilization. Concerning this second point, there are several unknowns: the quantity of methane, which is discussed by the authors but also the onset and the duration of these emissions, which they should discuss more.

2. Model description and initialization

The authors should clarify how they downscale the large grid of atmospheric and ocean models to high resolution ice sheet models (GRIS or AIS).

Line 105: what are the range of corrected biases for present day climate?

Line 106: is the PDD really appropriate for this study? Why did the authors use a method based on present day (cold context) rather than a method based on energy balance? Concerning the choice of parameters (Table S3) the authors should justify this choice and its possible consequences.

Line 117: the tuning of parameters on the last 500y is not really appropriate to explore large changes of cryosphere. The last deglaciation is certainly a better but more complex target for the goal of this paper.

Moreover, the authors selected a parameterization "because of its mid-range contribution to sea-level at 2100 AD and 2300 AD in comparison with recent studies". Is it a correct criteria in science to be "mid-range"? I think the authors should favor more physically based parametrizations. We also would like them to use other parameterizations for the scenarios including feedbacks.

3. Scenario description

Line 150-153: scenario RCP8.5 leads to a maximum PCO_2 of around 5000 ppm about 20 PAL. But is there enough fossil fuel to be burnt to achieve such a value?

4. Climate response and global sea-level budget of individual terms

The last deglaciation is strongly nonlinear, with acceleration, as during meltwater pulse and reduced SLR during colder episodes.

The ability of the model used here to reproduce the deglaciation should be discussed.

A plot showing both terms accumulation and ablation in the different scenarios could be interesting to be depict and discuss.

For GRIS what is the ocean dynamics in North Atlantic? It seems that the AMOC recovers and is even stronger than for PD: does that mean that the GRIS could not be covered by perennial ice sheet for long periods?

6. Long-term sea level rise in the light of the geological record

Line 312 and Fig 6

This is an interesting comparison. Nevertheless, there should be an hysteresis between the values of melting GRIS and AIS in the simulations described here and the pCO2 values corresponding to the onset of the same ice sheets during Cenozoic due to the changes of surface albedos.

7. Discussion

Line 360: There are two sources of uncertainties that the authors should comment with more details:
1. The AMOC evolution because in PLIOMIP2, most of the models depicted a lower AMOC, which provided a context not favorable to the onset of GRIS
2. The authors should compare their highest scenarios to the resources available in terms of fossil fuel.
The authors pointed out the possible mechanisms that speed up the AIS melting as Marine Ice Cliff Instability MICI.
Moreover, the record of the previous deglaciation show much variability in SLR rise with acceleration and regression of the ice sheets. These simulations did not reproduce this variability in future scenarios maybe partly due to coarse resolutions. There are anyway hot debates on the prevision of SLR only concerning the end of this century and very few models are able to reproduce using transient experiments the SLR during the lest glacial/interglacial cycle. For all these reasons, the authors should be careful of uncertainties and limitations of their strategy even if it is a consistent approach for investigating long time scale.
Moreover, the evolution of the grounding line when using coarse grid model is difficult to capture and therefore, it is another limitation of the paper that the authors should comment.

8. Conclusion
Their main results are summarized:
A GRIS melting for all scenarios, this melting being irreversible in the time window of interest here whereas the AIS contributes to the SLR differently, depending on scenarios.
These results are consistent with a GrIS becoming perennial only when CO2 is around 300-400 ppm whereas AIS needs a higher CO2 level to become perennial around 800 ppm.
The authors should also discuss the consistency of their results when compared to the evolution of these ice sheets.

Comments on Figures
General comment for all the Fig. :
In fact, while GMSL rises to high values (env. 30 m) many regions that are considered in the simulation as land points shift to ocean points. By the way, the ocean/continent distribution is changing with SLR. This is certainly a minor effect for the coarse grid model for several meters. But it may be important when SLR reaches 10 to 30 meters. This effect is not accounted for in the figure and I believe also that this distribution is kept fixed in all simulations. Thus, the authors should discuss this approximation.
Fig 2: it could be interesting to show also the GRIS configuration for present day.
Fig 3: interesting to have AIS for present day. Moreover, it could be interesting to add a snapshot at 3ky with WAIS and EAIS separately.
Figure 7: the logic of the caption (a) (b) succession is not easy to follow at first sight.