We thank both reviewers for their time and the helpful comments which we address in turn, below. In general, we are confident that we can provide a revised manuscript that will satisfy their concerns and be appropriate for final publication in Earth System Dynamics.

Reply to Reviewer 1:

The paper describes an ensemble of simulations investigating the effect of stabilisation of CO2 levels at different global temperature anomalies using a coupled ESM/ice sheet model for Greenland and Antarctica including a scenario mimicking the effect of removal of the entire anthropogenic CO2. The experimental design follows the TIPMIP protocol and fills in additional levels at which stabilization takes place. The topic is highly actual and fits perfectly into ESD. In general, the paper is well written. The analysis could go more into depth, but the authors see this as a first paper, introducing the set of experiments and leaving the detailed analysis for later paper.

Thanks for reviewing and the positive comments. As you allude to, our wider group has a suite of papers in progress and under review with more detailed analysis of individual aspects of these simulations. We think an overview paper such as this is an important introduction and overview in its own right.

It is sufficient material to be published, but in some places not enough analysis to make me really happy. One example: Following your argument in section 3.6, the Antarctic SMB closely follows the GSAT (306/7) and stabilizes as soon as emission stop (307). This is obviously differently then the behavior of Antarctic SAT, which continues to warm even after stopped emission (Fig.2b and discussion). Here a bit more careful discussion and analysis of the different behaviors after stabilization and the causes behind would be essential. Why is the local Antarctic SAT not relevant?

This example is a good point and highlights an explanation we should have been more careful with. The local SAT over the ice sheet is indeed more relevant to SMB than either global SAT or emissions. In the case of higher GWLs, as noted elsewhere, GSAT continues to rise when emissions stop, as do local AIS SAT and SMB with it. At lower GWLs, where GSAT does basically stabilise when emissions cease, the high latitude SAT shown in figure 2 (the average of the whole 60-90S region) has a much smaller warming signal and the local AIS SAT itself has further regional variation, making it harder to detect and interpret a robust trend in continentally-integrated SMB. In all cases SMB is best correlated with the local SAT over the icesheet itself, and the relationship between the local SAT, global SAT and the cessation of global carbon emissions is more complicated than we implied at line 306. In a revised draft we will remove the line in question and clarify this point with additional explanation.

In general the paper can be published after a bunch of minor corrections.

Some general comments to the figs.:

1. The yellow line is almost invisible in my printed version, on the screen it looks fine. Changing this into orange could be a compromise.

OK

2. In the (mostly temp.) anomaly time series (1cd, 2ab, 4a, 7ab) a zero line should be plotted. That makes it considerably easier to assess potential drift.

OK

3. In some places PI is used as reference, in others ZE-0. This is rather inconsistent. I recommend to plot them both. This would also allow the reader to estimate, whether the drift in the ice sheet has an effect on the southern ocean climate (sea ice, temp) or not.

We will do this. Although we don't believe this will show significant differences, we think it is a good suggestion for clarity and to explicitly demonstrate that the drift in ice sheet evolution under the PI forcing does not have a major effect on the climate state, a concern also noted by reviewer 2.

Detailed comments

37 crucial ?that?

OK

62 specified emission of CO2 be more specific and give the number

OK

96-98 specify the length of the runs

OK

100-104 does not make sense to describe experiments specifically that are not used in the paper. Here a vague hint to more experiments should be sufficient

We note that Reviewer 2 would like an explanation of the choice of negative emission rate, which is most sensibly done by reference to the wider set of experiments. We will try to balance these two requests in the revised paper.

187 southern hemisphere SAT is inaccurate, you are discussing only the polar SAT. The PI runs shows similar multidecadal variability

It is true that in this view the event in question does not look so much larger than those seen in the warmest phases of PI variability. However, the event in question is exceptional since it rebounds to this level of warming despite the global SAT and pCO2 being lower than PI at this point in Dn4-3, so it is clearly worthy of comment. The comparison to PI variability is useful however, and we will expand on the similarities and differences between this event and those oscillations in the revised text.

192-196 max surface air warming .. is always constrained by the melting temp of the ice surface..

While I can follow this argument for Greenland and higher CO2 levels, where it is at least true in summer, this is almost completely irrelevant for Antarctica. Even in the highest scenarios the melting is restricted to coastal areas. The high elevation areas of the ice sheet are and will be far away from the melting temp and are obviously accumulating happily mass (see fig.7c). Give a better reason!

This is another point we should have been more careful with, and will expand on in the revised manuscript. There is literature around how the polar amplification signal differs between the northern and southern hemispheres, and especially how relevant heat transport and radiative feedbacks are influenced by the topography of the Antarctic ice sheet to limit warming (eg Salzmann 2017, ESD) that explains this feature more physically.

200 does the physics of sea ice depend really on the cumulated emissions or rather the Arctic SAT, which is linearly related to GSAT and the cumulated emissions? Please give a physically plausible reasoning!

We're not sure about this comment. We agree that the sea-ice state is physically related to local SAT, and that this in turn is correlated with GSAT and cumulative emissions. In this paragraph we state a correlation evident in figure 3 between our sea-ice simulated sea-ice area and GSAT, and that seems reasonable to us. The statement on line 200 that appears to be being queried however is "there is an observed linear relationship between cumulative anthropogenic CO2 emissions and Arctic sea-ice decline" - this is not our reasoning, here we are simply repeating the clearly stated conclusion of Notz and Stroeve (Science 2016) which supports the correlation we see in our simualtion. In the revised manuscript we will be more clear that it is not the increased emissions themselves that directly cause melt, rather the increase in SAT and heat content of the polar regions that does so.

220 How does the sign change of the GrIS mass contribution relate to the time, when the GSAT anomaly becomes negative?

This is an interesting question (closely related to the later comment about line 362), and one we will take the opportunity to expand on in the revised manuscript. Figure 4 shows that the gradient of the relationship between SMB and GSAT when the GSAT anomaly is near 0 is almost flat, and figure 9 shows that the GrIS mass contribution only barely becomes negative even in the Dn4-1 and Dn4-1.5 simulations where both the Arctic and GSAT temperature anomalies have dropped significantly below the PI level. We will include some analysis of the ice sheet behaviour at below-PI GSATs following carbon sequestration in the revised manuscript.

section 3.6 The effect of the residual mass loss on the climate is not shown at all. If it is negligible, great, than please explicitly state this. Showing ZE-0 also in the climate plots particularly in the south would remove remaining doubts.

We will do this, and as noted above will follow the suggestion of plotting both PI and ZE-0 lines on all figures.

339 THe typo

We will correct

362 here or somewhere else a small discussion would be helpful, that GrSMB does not lead to more ice production for negative GSAT anomalies (Fig. 7d). Please discuss the mechanism(s) behind this.

see our reply above re: line 220. We will include some analysis of the ice sheet behaviour at below-PI GSATs following carbon sequestration in the revised manuscript.