Thanks to the clear explanations given by the authors, I think I now understand what they have exactly done. I consider the paper is now acceptable for publication. At the same time, I consider a number of modifications are still desirable, either in the scientific presentation or in the edition of the paper. Many of my comments and suggestions below could have been made on the previous version of the paper. I did not make them then either because they escaped my attention or because I considered they were of secondary importance at that stage.

1. As shown in particular by the fact that there is no increase of error in the forecasts that follow the assimilations (Figs 15, 16, 18), the system is extremely stable. Actually, the state of the system seems to be stationary over the time period (10-20 years) considered in Section 5 (the reference does not evolve in Figs 13 and 16). And the evolution of the reference is smooth (and presumably highly predictable) over the longer time periods (1000-2000 years) considered in Section 4. That makes the assimilation problem relatively easy. The authors stress that the originality of their paper is that it defines an approach for assimilation in a system with moving boundaries. That is true, and the paper is a significant contribution. But the problem would presumably much more difficult in a system where the evolution of boundaries depended on instabilities (as for instance in the motion in ice sheets with the possibility of sudden surges). I think it must be said that the problem studied in the paper is in a sense relatively easy. That does not degrade the value nor the significance of the paper.

2. Time scales considered in Section 4 and 5 differ by two orders of magnitude. It is not clear why (at least to me). Is it because the authors want to study in Section 5 the case of a rapid warming ? In any case, the similarity of the results obtained over so different time scales confirms the strong stability of the system.

3. Figure 3 shows the background error covariance matrices used in 3D-Var at analyses times t_1 = 500 yr and t_2 = 1500 yr. If the information is available, it would interesting to show how the actual background errors compare with the figure (just as, for ETKF, the spread of the ensemble is compared to the reference in Fig. 7 and other figures that follow). A similar remark applies to other figures showing estimated errors as produced by the assimilation, such as Figs 6 and 8.

4. Subsection 4.4. My understanding is that only observations of ice thickness are used there (at least, it is what the caption of Fig. 8 suggests). Say it clearly in the text from the start.

5. P. 24, ll. 2-3 (comparison between Figs 14 and 17), *We observe that the standard deviations for the node positions are smaller in the middle of the ice sheet than in the previous experiment.* Nothing of that sort is clearly visible from the figures.

6. P. 7, l. 2, *The Ensemble Kalman Filter* […] *aims to approximate the Extended Kalman Filter.* That is not really true. The Extended Kalman Filter is based on a local linearization of the evolution equation (16). The Ensemble Kalman Filter avoids any such linearization, and cannot be said to 'approximate' the Extended Kalman Filter.

7. P. 8, ll. 4-5, *Estimates obtained by* […] *using a moving-point numerical model provide more information* […] *than if we were using a fixed-grid method.* That is certainly true as concerns the position of the ice margin. But is it true of anything else ?

8. P. 6, l. 12. Say that the error in the background is assumed to be uncorrelated with the errors in the observations.

9. Eq. (19), $\mathcal{H}_k(\mathrm{x}) \approx \boldsymbol{\mathcal{H}}_k(\mathrm{x}_k^b) + \mathbf{H}_k(\dots)$

10. P. 8, ll. 15 and 16. From what I understand, the words *state variables* are to be replaced by *ice thicknesses*. The same correction is to be made in other places (*e.g.*, p. 7, last line). Check carefully.

11. Eqs (31-34). The authors assign numerically defined 'default' values (0 or $b(r_i)$) to observations when they actually mean there are no observations at all. In the case of Eq. (31) for example, the observation reduces to the scalar value h_i + … . (I do not think there is a real danger of confusion there, but it is preferable to be consistent).

The same correction may have to be made elsewhere. Please check.

12. Is it possible to give a reference for the rather complicated form of the discretization in Eqs (35-36) ?

13. The authors confirm that T_0 is expressed in °C in Eq. (A3). That is difficult to believe. It means that a slight variation in T_0 (which is said to be equal to -6 °C) would result in a large variation in Abl(t, r).

14. $T_{clim}(t)$ in Eq. (A3) does not seem to be defined.

15. P. 22, l. 8, *since the background state is smaller than the reference state*. I rather suggest *since observations may lie outside of the spatial range covered the background estimates of the node positions.*

And, three lines below … , *since experience shows that there is always at least one member of the ensemble which lies outside of the range covered by the observations.*

16. P. 14, caption of Fig. 15, l. 2, … *in Fig. **4**,* …

17. P. 4, l. 12, *One of the points is dedicated to the static ice divide r = 0 , while another point tracks* …. Do you mean *One of the integral upper bounds is the current point* r^(t), *while the other is the margin position* $r_l(t)$? But that remark seems to me of no real interest.

18. P. 18, l. 1, *This effect would not necessarily appear with another set of observations*. The same remark would probably be true of many other results in the paper. I suggest you remove that sentence.

19. Caption of Fig. 18, last line, … *3D-Var gives better results for the ice thickness at r = 0.* I think this is to be put in the text.

20. P. 24, l. 20, sentence starting *This can be achieved either by using an appropriate flow-dependent background covariance matrix* … My understanding is that this is empirical. I suggest *This can be empirically achieved* … Actually, both the maintenance of positive thicknesses and of an ordered mesh is obtained empirically. A similar correction may have to be made at other places in the paper.