Review of the article entitled "Upstream flow effects revealed in the EastGRIP ice core using a Monte Carlo inversion of a two-dimensional ice-flow model"

1 General comments

This study investigates the ice flow on the upstream part of the NorthEast Greenland Ice Stream (NEGIS) in order to provide information relative to ice origin to the East Greenland Ice-Core Project (EastGRIP). The authors use a 2 dimensional, flowline Dansgaard-Johnsen model to simulate the ice flow upstream of the EastGRIP drilling location. The simulations are performed along three lines that roughly approximate the ice stream flow but have the advantage of being along measured radar profiles. The use of the radar profile allows to constrain the parameters of the model through a Monte Carlo inversion that minimises the misfit between observed and modelled isochrones. The model finally allow to compute trajectories of ice particles backwards in time from the EastGRIP borehole to define the location, accumulation rate and altitude at the time of deposition. These information will help to further analyse the results from the ice core and already show that the constant annual layer thickness observed in the first 900 meters of the EastGRIP ice core is mainly due to the increase of the accumulation rate as the ice source gets closer to the ice sheet divide.

In general, this paper is well written and nicely illustrated. The conclusions regarding the localisation of the deposition point in the past are well documented and seems to be robust given the methods that are used. It seems however that some key information are missing on the parameter selection for the Dansgaard-Johnsen model and more particularly regarding the regularisation of the data and discussion on their spread. I am also puzzled by some of the differences that can be observed on the overlapping flowline and this should be discussed further.

1. It is stated that a threshold is used to regularise the maximum deviation of the parameters and allow to keep those parameters in a physically feasible range. It is however unclear what this range is and how it was defined. Form Figure 5 it seems that the threshold is exceeded for the basal melt rate for flowlines A and B at

EastGRIP. However, the values presented here are lower than the maximum values for basal melt that have been recently presented at this location. It feels that for this parameter the threshold might need to be increased to allow the basal melt rate to increase. In any case, more information should be given on how those threshold and the "physically feasible range" have been selected. On the same figure 5 we can also see a very large standard deviation on the kink height at EastGRIP, together with a large spread of the mean value across the different flowlines. It would be nice to get more insight into the effect of this variation and the possible implication of this large spread if ever a parameter far from the actual value should be used.

2. Lines A and B share the same line close to EastGRIP, However in Figure 3 the velocity and stresses in the region where the line is common are different. From my understanding the velocities have been extracted from the MEaSUREs dataset along the radar profile so I don't understand why those plots are different. On Figure 4 we also see that the modelled isochrone on the region where A and B are on the same radar line show significantly different results. I expect that this arises from the different parameter selected through the Monte Carlo approach for the model but it would be interesting to discuss this point further and potentially identify a "better" set of parameter from these portions of flowline.

3. As a general point I think that the limitations of the model should be developed a bit more. The authors state that the lack of radar data is the major limitation of this study but perhaps applying a more evolve 3D model would allow to overcome this issue. I understand that there is a trade off here between using a simple model that allows the Monte Carlo approach and using a highly under constrained more physically accurate models but the limitation inherent to this choice should be more clearly pointed out.

2 Specific comments

Bellow is a list of more specific and technical comments throughout the manuscript given with line numbers:

- Line 5: It is stated here that the location inside an ice stream introduces non climatic bias. But isn't that redundant with the high ice-flow velocities, or is it meant that specific processes linked to the ice stream such as shear margin are causing some of those biases?

- Line 8: It might be worth noting here that the selected flowline emanate from the Greenland Ice Sheet summit but that the site as also specific interest due to the presence of the GRIP ice core.

- Line 9: the RES abbreviation is not re-used in the abstract and could be dropped.
- Line 10: As the model is solved along a flowline, wouldn't the source be a point rather than an area.
- Line 26: I think that the statement relative to the modelling of NEGIS is not completely true and that more recent modelling work like the ones of Beyer et al. (2018); Smith-Johnsen et al. (2020) should be discussed here.
- Line 27: Some results from the EastGRIP ice core have started to appear but it might be more fair to use the future tense here as much more results are to be produced.
- Line 37: I am not sure what is referred to here when using the term "lateral flow", is that to contrast with vertical flow? If that is the case perhaps "horizontal flow" would fit better?
- Line 39: I would prefer "spatial variability of the precipitation".
- Line 52: The fact that the model is 2D vertical should be mentioned here.
- Line 63: The sentence starting on this line is unclear, perhaps "as a consequence of" should be dropped.
- Line 67: The deviation of the different flowline does not seem that extreme to me, particularly if they are compared to the spread of the radar lines that are used.
- Figure S1: On this figure I am missing a scale more convenient than the one on the map border which would help to judge distances better on the map.
- Figure 1: As for Figure S1, Figure 1 would benefit from a more convenient scale legend.
- Line 79: "the NEGIS trunk"
- Line 80: Line C also presents a quite substantial data gap and that should be noted here.
- Line 84: "centre" should be capitalised.
- Line 109: "Greenland Stadial 2 (GS-2)"
- Figure 2: It would be nice to show the different Greenland Stadials on the age axis.

- Line 138: This description of the dating process is not the clearest. I am not sure of what the 250m represent, is that following the radar line up and downstream to smooth out any local bump in the IRH? This whole sentence should probably be rephrased.
- Figure 3: On panel b the surface velocity legend is missing
- Figure 4: I think that "very well" to describe the fit of the isochrones is an overstatement. To my eye it seems that there is a bias with the modelled isochrones being slightly higher up in the ice column than the observed one.
- Figure 5: I suppose that the basal sliding is expressed as a fraction of the surface velocity. That should be stated in the Figure or in its caption. On panel (i) and (j) the exponent on the x axis is confusingly placed.
- Figure 6: There is a legend missing in panel (d).
- Line 295: It is not sure to me what "local accumulation" means in this context. From the rest of the sentence I expect that it is the accumulation at the deposition site but somehow "local" here make it unclear.
- Line 300: The sentence starting on this line is not completely clear. If I refer to the present-day accumulation stated above (0.12 ma^{-1}) the (0.14ma^{-1}) given here for interstadial is actually higher then present day.
- Line 377: The sentence starting on this line should be modified. Zeising and Humbert (2021) actually state in the last part of their paper that "We are aware that these melt rates require an extremely large amount of heat that we suggest to arise from the subglacial water system and the geothermal heat flux." and that they are able to close their energy budget with a more reasonable geothermal heat flux around 0.25 Wm^{-2}.
- Line 388: "Propagate" might not be the good term here.
- Line 408:It would be nice here to have more information on the reason why this simulation is not attempted. Is it just due to the fact that the constraints on the model would be lacking, that it is not warranted for the specific goal of estimating the source location of the ice or for other reasons.

References

Beyer, S., Kleiner, T., Aizinger, V., Rückamp, M., and Humbert, A. (2018). A confined–unconfined aquifer model for subglacial hydrology and its application to the northeast greenland ice stream. *The Cryosphere*, 12(12):3931–3947.

Smith-Johnsen, S., de Fleurian, B., Schlegel, N., Seroussi, H., and Nisancioglu, K. (2020). Exceptionally high heat flux needed to sustain the northeast greenland ice stream. *The Cryosphere*, 14(3):841–854.

Zeising, O. and Humbert, A. (2021). Indication of high basal melting at eastgrip drill site on the northeast greenland ice stream. *The Cryosphere Discussions*, 2021:1–15.