Authors response on Referee Comment tc-2021-63-RC1

May 21, 2021

1 General comments

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. From 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.

   Author response: We agree that this part has not been addressed enough in the current version of the manuscript and we will include a more detailed explanation on the parameter regularisation in the revised version. The regularisation was done as follows:

   - The kink height is allowed to vary within the ice thickness, i.e. $0 < h < H$. Anything outside this range would physically not make sense. We did not regulate this parameter further because we do not have any prior knowledge on the vertical velocity distribution along the flow line.

   - The accumulation rate was set to vary within ± 2 cm from the initial accumulation rate, i.e. $\lambda_{H,0} - 0.02 < \lambda_H < \lambda_{H,0} + 0.02$. The initial accumulation rate obtained from Eq. (12) fits quite well with field observations at GRIP and EastGRIP. Nevertheless, small deviations can be expected because the local layer approximation is not valid in the study area. We estimate that this deviation should be within ± 2 cm from the initial guess.

   - The basal sliding was allowed to vary ± 15 % from the initial parameterisation, i.e. $f_{B,0} - 0.15 < f_B < f_{B,0} + 0.15$, resulting in a maximum sliding fraction of 95 % in the vicinity of EastGRIP.

   - The threshold for the basal melt rate was set to 0.03 m a^{-1}, i.e. $\lambda_{B,0} - 0.03 < \lambda_B < \lambda_{B,0} + 0.03$ which allows a maximum melt rate of 0.06 m a^{-1} at EastGRIP. The choice of this upper limit is based on our preliminary modeling results which showed that much higher basal melt rates lead to erosion of the deepest observed isochrones.

Basal melt rates suggested by previous studies were obtained from the Holocene stratigraphy only (e.g. Fahnestock et al., 2001; Keisling et al., 2014; MacGregor et al., 2016) or represent present-day estimates (Zeising and Humbert, 2021), while in our model the basal melt rates are averages over the entire modeled time period (∼50ka). However, we agree that the upper limits of the basal melt rates are reached in flow line A and B and that these thresholds should be loosened for more accurate sampling of the posterior probability. We will address this in the revised version of the manuscript by running additional models with more loose parameter regularisation.
Any parameter perturbation at a certain point along the flow line affects the isochrones at that location and further downstream, i.e. the model parameters are not uncorrelated. A drag-down of the isochrones due to enhanced accumulation, for instance, can be compensated by enhanced kink height, low basal melt rate or a decreased basal sliding fraction. Because the kink height is the least regulated parameter it can fluctuate a lot and compensate for effects caused by other parameters. The parameter distribution at EastGRIP thus depends a lot on the parameters in the upstream area and a deviation between the flow lines can be expected even if the observed isochrones are identical in the downstream area. We will add a few sentences in the revised version of the manuscript to discuss the parameter correlation and their effects on our results.

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.

   Author response: Thanks a lot for pointing out the plotting error in panel f) of Fig. 3. The differences in the strain rates are related to a slight shift in the coordinate sampling between the two lines. We apologize for this and will correct it in the revised version of the manuscript.
   As you correctly stated, the differences between the modeled isochrones of flow line A and B in Fig. 4 arise from the different choice of model parameters obtained during the Monte Carlo inversion. As flow line A and B differ in the upstream area, the parameters found are different. This affects the modeled isochrone characteristics but also the parameter sampling in the area where the two lines overlap. It is thus difficult to compare the isochrone fit of the two lines only in the overlapping part since it is also determined by the parameters further upstream. We agree that this should be discussed in the paper and we will insert a few additional sentences in section 4.1.

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.

Author response: The main purpose of this study is to determine the source characteristics of ice found in the EastGRIP ice core. As the ice flow along flow-lines is two-dimensional a 3D model is not necessary to determine the source location. Among other 2D models we chose the Dansgaard-Johnsen model due to its few model parameters which make it well suited for the Monte Carlo inversion, as is described in line 151–158. A Monte Carlo inversion of a similar 3D model would be computationally unfeasible as the number of parameters would increase immensely. As you also mention, we state that the largest limitations is the lack of RES data along the EastGRIP flow line. We believe that a 3D model would not overcome this issue as no further information can be obtained without additional constraining data. We will point out the strengths and limitations on our choice of model more clearly in the revised version of the manuscript.

2 Specific comments

- 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?

  Author response: The high ice-flow velocities are mainly responsible for the strong upstream effects at EastGRIP. However, other characteristics of the ice stream could also play a role, for example the higher spatial variability in accumulation rates due to the surface topography (i.e. across the shear margins). In the revised version we will discard the 'due to high ice-flow velocities' as it is included in the 'location in an ice stream'.

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

  Author response: We agree that this is a valuable clarification. However, we will mention this in the introduction in order to keep the abstract to the current length.

- Line 9: the RES abbreviation is not re-used in the abstract and could be dropped.

  Author response: We agree and will remove the abbreviation.

- Line 10: As the model is solved along a flow line, wouldn't the source be a point rather than an area.

  Author response: Yes, we agree that this is confusing. The thought behind the 'area' is that due to uncertainties and the spread between the flow lines we can not determine an exact 'source point'. We will use source location rather than source area in the revised version of the manuscript.

- 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.

Author response: Thank you for pointing us towards these studies. We have rephrased this section as follows:

original:

Understanding the driving mechanisms of the NEGIS is essential to anticipate its future development and potential impact on the ice-sheet stability with large-scale ice-flow models (Joughin et al., 2001; Khan et al., 2014; Vallelonga et al., 2014). Yet, many unknowns remain in our comprehension of ice-stream dynamics (Tulaczyk et al., 2000; Robel et al., 2013), and the underlying processes governing ice flow are not sufficiently understood to successfully reproduce the NEGIS in sophisticated ice-sheet models (e.g. Mottram et al., 2019; Shepherd et al., 2020).

revised:

Large-scale ice-sheet models are essential tools to anticipate the future development of the NEGIS and its potential impact on the stability of the GrIS (Joughin et al., 2001; Khan et al., 2014; Vallelonga et al., 2014). However, results obtained from such models often show a significant deviation from observed surface velocitites in the NEGIS and its catchment area (Aschwanden et al., 2016; Mottram et al., 2019). In particular, the high ice flow velocities in the upstream area of the NEGIS and the clearly defined shear margins are difficult to be reproduced by ice flow models (Beyer et al., 2018). A recent study by Smith-Johnsen et al. (2020) showed, that the high surface velocities in the onset region of the ice stream could be reproduced with their model, which however required an exceptionally high and geologically unfeasible geothermal heat flux (Bons et al., 2021). This indicates that additional, yet unknown processes must facilitate ice flow in the NEGIS and that the driving mechanisms governing ice flow are yet not well enough understood.

- 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.

  Author response: We will replace 'reveals' with 'by revealing' in order to stress the ongoing process. Later in the introduction we also state that more data will become available in the future.

  original:

  The EastGRIP ice core sheds some light on the key processes, as it reveals unique information about ice dynamics, stress regimes, temperatures and basal properties, all of which are crucial components in ice-flow models.

  revised:

  The EastGRIP ice core sheds some light on the key processes by revealing unique information about ice dynamics, stress regimes, temperatures and basal properties, all of which are crucial components in ice-flow models.

- 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?

  Author response: With lateral flow we are referring to the flow away from the central ice divide. We agree that is unclear and will use 'horizontal flow' in the revised version of the manuscript.

- Line 39: I would prefer "spatial variability of the precipitation".

  Author response: We agree and will change it as suggested.

- Line 52: The fact that the model is 2D vertical should be mentioned here.

  Author response: Thank you for pointing this out. We will adjust this as follows:

  original:

  In this study, we use a two-dimensional Dansgaard–Johnsen model to simulate the ice flow along three approximated flow lines between the ice-sheet summit (GRIP) and EastGRIP.

  revised:

  In this study, we use a vertically two-dimensional Dansgaard–Johnsen model to simulate the ice flow along three approximated flow lines between the ice-sheet summit (GRIP) and EastGRIP.

- Line 63: The sentence starting on this line is unclear, perhaps "as a consequence of " should be dropped.

  Author response: We agree and will rephrase this part as follows.

  original:

  However, as a consequence of error propagation, minor uncertainties and bias in the data severely affect the tracking of flow lines along the velocity field (Hvidberg et al., 2020).

  revised:

  Minor uncertainties and bias in these data products strongly affect along-flow tracing and lead to deviations between flow lines derived from different products. These deviations become more pronounced with increasing distance from the starting point, as the uncertainties propagate along the line and in general become larger in slow-moving areas of the ice sheet (Hvidberg et al., 2020).

- 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.

  Author response: This is correct. We want to point out here that the flow line we used as the 'present-day' flow line (the black line in Fig.1) includes uncertainties and that it can look different if it is derived from other datasets. We also state that the uncertainties due to missing data is larger than the uncertainties of the flow line itself.

- 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.

  Author response: Thank you for this great input. We have revised this figure and added a scale. In addition to that, we slightly modified the color map in order to avoid confusion between 'white' velocities and the white background.

- Figure 1: As for Figure S1, Figure 1 would benefit from a more convenient scale legend.

  Author response: Same adjustments have been made as for Figure S1.

- Line 79: "the NEGIS trunk"

  Author response: We will adapt this suggestion.

- Line 80: Line C also presents a quite substantial data gap and that should be noted here.

  Author response: Yes, thank you for pointing this out. We will mention it in the revised version of the manuscript.

- Line 84: "centre" should be capitalised.

  Author response: We agree and will adjust it accordingly.

- Line 109: "Greenland Stadial 2 (GS-2)"

  Author response: We agree and will introduce the abbreviation for Greenland Stadial here.

- Figure 2: It would be nice to show the different Greenland Stadials on the age axis.

  Author response: Thank you a lot for this very valuable input. We decided to indicate the Greenland Stadials in the figure background as colored areas, since the age axis might become too cramped otherwise.

- 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.

  Author response: Yes indeed, the depth is averaged over ± 250 m to smooth out local undulations of the isochrones. We will rephrase this as follows:

  original:

  The traced IRHs were dated by assigning the average reflector depth over ± 250 m around the trace closest to the GRIP and EastGRIP sites to the extended GICC05 time scale (Rasmussen et al., 2014; Seierstad et al., 2014; Mojtabavi et al., 2020).

  revised:

  The traced IRHs were dated at both drill sites by assigning the reflector depth at GRIP and EastGRIP to the corresponding time scale. In doing so, local irregularities were smoothed out by averaging the depth over ± 250 m around the trace closest to the ice core locations.

- Figure 3: On panel b the surface velocity legend is missing

  Author response: Yes this is right, thank you a lot for pointing this out. We will adjust it in the revised version of the manuscript.

- Figure 4: I think that "very well" to describe the fit of the isochrones is an over statement. 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.

Author response: We find that the modeled isochrones fit the observed isochrones well within the limitations of the method. Panel b, d and f in Fig. 4. indicate the misfit between the modeled and observed isochrones, which are both, positive and negative. It is true that the simulated isochrones in flow line B tend to be higher than the observed isochrones in the downstream part of the flow line but we do not quite agree that this is the case in general. We will however soften our statement by using 'well' instead of 'very well'.

- 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.

  Author response: Thank you for the feedback on this figure. We revised it and added the clarification regarding the basal sliding. We also changed the x-axis intervals such that they are identical for each parameter at the same location.

- Figure 6: There is a legend missing in panel (d).

  Author response: Thanks for pointing this out. The gray line represents the annual layer thickness obtained from the ice core stratigraphy. The black line is resampled to the same resolution as the modeled layer thicknesses to facilitate comparison. We will revise this figure and add the missing legend.

- 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.

  Author response: Yes, we are indeed referring to the accumulation rate at the deposition site. We agree that 'local' is confusing in this context and will discard it in the revised version of the manuscript.

- 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.

  Author response: Thank you for pointing out the lack of clarity here. The accumulation rates in the glacial period were generally lower than today due to the colder and dryer atmospheric conditions. The ice from that period, however, was deposited further upstream, where the accumulation rate is higher compared to EastGRIP, and hence the accumulation rate at the deposition site was higher than at EastGRIP today. The 'lower' should not refer to the present-day accumulation rate at EastGRIP but to the present-day accumulation rate at the deposition site, see Table 4. We have rephrased this as follows:

  original:

  Older ice was, due to climatic reasons, deposited under lower accumulation rates between 0.05 m a^{-1} in the stadials and 0.14 m a^{-1} in interstadials.

  revised:

  The accumulation rate at the deposition site for older ice varies between 0.05 m a^{-1} (GS) and

0.14 m a^{-1} (GI). The atmosphere in the glacial period was in general colder and dryer, and hence, accumulation rates were generally lower than today (Cuffey and Clow, 1997). However, due to the upstream flow effects, the ice from the interstadials was deposited under higher accumulation rates than observed at the EastGRIP site today.

- 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} .

  Author response: We assume that this comment refers to line 357, rather than line 377. We will adjust the sentence as follows:

  original:

  Melt rates in these order of magnitudes would require an unusual high geothermal heat flux, immensely exceeding the continental background (Fahnestock et al., 2001; Bons et al., 2021).

  revised:

  Melt rates in these order of magnitudes would either require an unusual high geothermal heat flux exceeding the continental background (Fahnestock et al., 2001; Bons et al., 2021) or an additional heat source (Zeising and Humbert, 2021).

- Line 388: "Propagate" might not be the good term here.

  Author response: We will replace 'propagate' with 'penetrate'.

- 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.

  Author response: The reasoning behind our model choice is described in section 2.3, line 151–158, where the model is introduced. As we mentioned in the answer to the general comment #3, a 3D model is not necessary for the purpose of this study, which is to determine the source location and provide estimates of the past accumulation rates at the deposition site of the EastGRIP ice core. The simple model we are using here has the advantage of having few model parameters which allows the use of the Monte Carlo method. A similar model in 3D would immensely increase computational costs and parameter tuning would become unfeasible. Furthermore, due to the limited availability of radar data, there is not much additional constrains of such a 3D model and hence, the information gain regarding the source location would be very limited. We will revise this section and point out the reasons for not using a 3D model in this context.

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