In this article, the authors propose a the structure of the age density in the layer-counted GICC05 chronology for NorthGRIP.

They model the structure of the age density (the number of layer per depth unit) as the sum of a 2nd-order polynomial term (representing the layer thinning due to ice flow), a d18O-related term, and a depth-related term that is unique to each stadial or interstadial interval.

The rest of the residuals is described as noise, which is best characterized by a AR(1) or AR(2) process.

It is then discuss whether there could be a systematic bias in the Maximum Counting Error (MCE), but no clear conclusion is drawn in this section.

Later, in an application section, the depths of the DO transitions are determined using a statistical framework, and the age uncertainties are derived from the depth uncertainties and from the age uncertainties previously derived.

The paper is well written, and the application of statistical tools is rigorous, as far as I could understand, but I have a few comments which could help to make the paper more relevant.

Answer: We thank Frédéric Parrenin for his helpful review. The comments will be addressed point-by-point below.

General comments:

1. The title is misleading, since here you really focus on NorthGRIP/GICC05, with its particular uncertainty structure. I would therefore use a more specific title. Moreover, the part related to the determination of the DO transitions is not mentionned in the title, while it is an interesting application.

Answer: We agree with both reviewers that the application of determining the abrupt warming transitions should receive more emphasis. We will change the title to ”Comprehensive uncertainty estimation of the timing of Greenland warmings of the Greenland Ice core records”

2. The paper pretend to model the GICC05 uncertainty, but I think it rather models the GICC05 age density. It is the first interest of this paper, to try to explain the GICC05 age density as far as possible with mathematical regressions, and to have an as small as possible residual term.

Answer: We think that a probabilistic age-depth model fitted to empirical age densities is an appropriate description of the age uncertainty. Similar interpretations can be found in other uncertainty frameworks such as the BACON (Blaauw and Christen 2011) where a first order

autoregressive gamma distribution is proposed for the accumulation rates (though without a regression component) and fitted to the data using a Markov Chain Monte Carlo approach.

3. The modeling of the thinning process as an additive 2nd-order polynome is questionable. First, the thinning function is not additive, but rather multiplicative. The analysis should therefore be applied to the log of the age density, so that multiplicative terms become additive terms. Second, there are more appropriate formulation of the thinning function, the like Lliboutry profile (see for example Parrenin et al., The Cryosphere, 2017). Although here I am not sure it will make a big difference, since the difference with a 2nd order polynome is important only in the very bottom section.

Answer: We agree that it is more suitable to apply our regression to the logarithm of the layer increments instead. Moreover, this would also ensure monotonicity by not allowing negative layers, which makes the model more realistic. We have conducted a preliminary analysis which seems to suggest that the residuals are more skewed and have a more apparent degree of heteroskedasticity. We have been able to find a way to model non-constant variance within our existing framework, but this might be too technical to be included in this paper. We are currently working on another paper related to this age-depth model that we aim to publish in a statistical journal where this might be a better fit. We will provide a comment on this in the revised manuscript.

4. The depth-related term in the age density is an interesting observation, but there is no physical explanation for it. It could be interesting to discuss some hypotheses.

Answer: We are not entirely sure if we understand this comment correctly. If the referee refers to the second order polynomial fit as the ‘depth-related term’ , then a physical explanation of the depth related term is included in lines 141-143.

5. The modeling of the stochastic residuals with AR(1) or AR(2) processes is questionable, since the age density is calculated every 5 cm (if I understood correctly). Therefore, an AR(1) process does not represent the same time memory in the top or bottom parts of the records.

Answer: Similar to other frameworks for uncertainty quantification, we do not model the actual climate process, but merely the thinning of the ice core. Hence the AR processes describe the interdependencies of different ice core slices and not climatic memory (for which we agree that a temporal axis would be more appropriate). Other uncertainty quantification frameworks treat memory similarly. A depth-varying memory would be possible to incorporate, and could possibly be addressed in future work.

6. The residuals are described as a gaussian process, but it seems from Fig. 2b that the standard deviation is not constant but rather depth dependent. This is not really discussed as far as I understood.

Answer: We agree that there is an apparent depth dependency in the residuals' amplitudes. However, the amplitude modulation is sufficiently weak, such that assuming a homoscedastic gaussian noise process is still a reasonable first order modeling approach. As already mentioned, in upcoming research we will incorporate the log transformation as proposed by the referee, which simultaneously will allow for better control of the heteroscedasticity in the residuals.

7. Regarding biases, when I read the abstract I got interested because I thought that such bias would be estimated. This could be the case by using a more accurate (in absolute ages) and independent chronology, like the U-Th dating of the Hulu cave speleothem record. But this is not the case here. I don't really see which message we get from this section on the biases, since there is no clear quantification at the end.

Answer: Knowledge about the counting process and underlying biases can be readily incorporated into our method if such information is available. We just give some examples of possible biases and demonstrate how such biases could affect the overall dating uncertainty. In a revised manuscript we will modify the abstract and will write:

"We show how the effect of a potential counting bias can be incorporated in our framework. Furthermore we present refined estimates of the occurrence times of Dansgaard-Oeschger events evidenced in Greenland ice cores together with a complete uncertainty quantification of these timings." instead of "We show how the effect of an unknown counting bias can be incorporated in our framework and present refined estimates of the occurrence times of Dansgaard-Oeschger events evidenced in Greenland ice cores together with a complete uncertainty quantification of these timings."

We hope that this change makes clear that in this study we do not intend to estimate the counting bias explicitly.

In principle, a bias can be estimated by synchronizing our timescale to tie points obtained from other archives. Synchronization with other archives requires additional, lengthy technical derivations, and will therefore be addressed in an individual paper on how to incorporate tie-points into this framework, which we hope to submit soon.

8. Regarding the identification of the DO transitions, it only appears as an application of the uncertainty quantification method, while I agree with Anders Svensson it has an strong interest by itself, in particular for the stacking of these transitions. Maybe giving more focus on this aspect could make the paper more relevant. I also agree with Anders Svensson on the interest of applying this method to other Greenland cores, other datasets or older time periods.

Answer: Thank you! We will in the revision put more emphasis on identifying the abrupt warming transitions, as we agree it will be of interest to the readers of the journal. However, the main aim of this paper remains to present the methodology. As such, we have, in an

effort to avoid overloading this paper, only applied it to one ice core. We will, however, consider applying the model to other archives in future work.

Specific comments:

l. 245: "of the GICC05 chronology"

l. 246 "over counted or missed." (missing dot)

fig. 6 legend, 2nd line: "linear ramp"

Answer: The specific comments will be addressed in the revised manuscript.