The Cryosphere Discussions

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Reply on RC1

Marie Bergelin et al.

Author comment on "Cosmogenic nuclide dating of two stacked ice masses: Ong Valley, Antarctica" by Marie Bergelin et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-18-AC2, 2022

We appreciate the supportive and helpful comments by RC 1, and for the most part agree with the recommendations. Please, see below our detailed responses to specific review comments.

Specific comments

I think there are several specific aspects of the paper and its structure that could be developed to improve the clarity. Firstly, I think the overall contextual information given regarding the core is not sufficient. The core clearly displays a varied stratigraphy, evident in both the visual appearance and measured debris content of the ice. This stratigraphy seems critical to the interpretation of the subsequent cosmogenic nuclide data but it is only very briefly described in the results section. The sub-division of the core into individual units should be based on the stratigraphy (i.e. descriptive) and not on the interpretation of the cosmo data (i.e. interpretative). It would also provide an opportunity to introduce some of the potential complexity within the core (e.g. potentially more than one ice mass/ice deposition event) given the observed stratigraphy. This would then seem to give a foundation for describing what units were sampled and why/what they might inform upon. I also think the core description should come much earlier in the paper, perhaps after the study site section, as is often done in ocean core studies.

We agree that the physical description of the ice core should be expanded and a natural location for that would be after the description of the field site as suggested. However, it does not change our general approach to analyzing the core for three reasons: 1) originally, we started the analyses with the simplest assumption that the core covers a single glacial advance. The variations in the sediment concentration and the signs of banding that were observed in the core, are assumed to reflect the fact that the ice originates from near the bottom of the ice sheet and close to the perimeter of the glacier. 2) there are no major visible differences in the characteristics of the sediment in the core. 3) For our cosmogenic isotope analyses we used all mineral matter available in the core. In other words, our sampling scheme was not dictated or affected by the core physical characteristics.

The results section is very short and doesn't describe that data in enough detail for the reader to subsequently follow the paper. The cosmogenic data from the core is the central data-set but is described in less than five lines. The down core variation should be specifically described and quantified. The variations have implications for the subsequent

application of the model.

We agree and will expand and add more details to the results section.

Another comment regards the structure, both the overall structure and structure of individual sections. I appreciate that this may have been quite a tricky paper to write as it involves a number of approaches (surface exposure dating, burial dating, depth-profile dating) and modelling.

We agree wholeheartedly. This was a tricky paper to write.

I always find in similar cases structure can be hard to decide upon but one way I find helpful is to start with the simple(!) parts and add complexity. Currently it feels the paper sometimes tries to address all the complexity at once. For example in Section 4.3 the overarching principles of nuclide production at depth are described after the complexities of evolving mass shielding and depth. This seems like the wrong way round. Additionally, sentences describing key concepts are scattered throughout the paper; for example in section 6.2 (522-523, 526-536). The reader needs to be as up-to-speed as possible before the model results are introduced. I wonder whether the bulk of the model description should be moved to supplementary and only the key concepts described in the paper (perhaps in the discussion section).

The modeling approach and application are a novel contribution to this paper, therefore we feel that it needs to be in the body of the paper rather than in an appendix.

We agree that we need to provide the reader with a clearer roadmap on how the modeling unfolds in the paper. In general, the organizing principle of this paper is that we want to clearly show how the unusual characteristics of the observations led us to choose methods of data analysis that we would not have initially expected to be useful. The use of burial dating to constrain the age of the ice is probably the best example. Describing burial dating in detail before the reader has seen the observations would be misleading to the reader, because it would imply that somehow we knew in advance of the study that burial dating would be a viable approach to determining the age of the ice. This is not the case – given only the geologic setting of the site and nothing else, neither we nor anyone else would expect that burial dating would be possible or useful. The usefulness of burial dating only became clear after the surprising observation that some of the englacial sediment must have experienced prior exposure before being entrained in the ice. The purpose of the paper organization was to make this chain of reasoning clear: there are many different ways to interpret cosmogenic-nuclide data to gain age information, the correct approach is not known in advance, and one has to choose the right approach based on both the geologic context and the nature of the observations. However, it is true that we did not specifically state this organizing principle in the paper. We can improve this by stating this explicitly early in the paper.

In the paper the forward model is introduced in the order it was built. The debris concentrations and the density of the material are the foundations of the forward model. The shielding mass determines the production rate at depth. The production rate is introduced later since the production of cosmogenic nuclides is based on the change in depth/shielding mass that a sample experiences. We agree that this section would benefit from the addition of a paragraph in the beginning which states the basic principles of shielding and production rates of cosmogenic nuclides at depth, and the importance of shielding mass prior to the introduction of details involving shielding mass.

The burial dating is introduced after the forward model (section 4. Method) and the resulting nuclide data set (section 5. Results). The reasons for this are currently stated in section 3. (Cosmogenic-nuclide applications relevant for dating Ong Valley buried ice) and,

we agree, should be described in greater detail. However, the burial dating is only necessary and applicable because of the downcore increase in nuclide concentrations and is therefore introduced after the results which reveal this. We think that a paragraph describing the reasons to use the burial dating as a constraint would be beneficial in section 6, to help the reader to follow along.

Is burial dating part of the model? Line 526 suggests not but line 611 suggests it is? I am really confused. I think the model results and burial dating results need to be more clearly defined as to what is what. Section 6.2 has forward modelling in the title but seems to refer entirely to burial dating units that the forward model wasn't applied to? The burial ages are only given at the very end of section 6 even though they are used as constraints for the model?

The model is not itself a dating method, it is just a forward model calculation that predicts the nuclide concentrations we should observe as a function of various parameters including the age of the ice. The concept of burial dating comes into the model optimization because of the constraint that, in effect, the samples are not allowed to have a burial age less than zero at the time they are incorporated into the ice. Then in a second step, after we have identified a best-fitting model for the nuclide concentrations produced after ice emplacement, we compute apparent burial ages for samples from the recycled surface material units. Thus, the concept of burial dating is used in the model optimization, but the calculation of burial ages for some samples is a separate calculation that takes place in a subsequent step.

There are two benefits of using burial dating: 1) to constrain the model as any given sample within the ice core cannot have been buried for lesser time than the deposit of the ice that encloses it, hence this burial constraint will provide us with a maximum depositional age of the middle ice, and 2) to determine the burial age for all samples in order to evaluate whether there is a general agreement between the burial age of the samples or if there is variation in ages which would indicate a more complex history or a variable source of the debris. Therefore, the model results come first which include the burial dating constraint and is based on such, and later the remaining results of the burial dating that appear in section 6. However, we agree that the structure of such could be more clearly defined in the forward modeling section where burial dating is introduced and discussed.

I am clearly not following what was done from the text. I think a much clearer structure that separates the measured results from the modelled results is needed. They authors need to be explicit about what is what throughout the paper. The paper needs to set up a logical structure and follow it throughout, to me it currently jumps about from one approach to another making it really hard for me to follow and subsequently review. The authors will of course be very familiar with the steps involved in deriving the results but for someone seeing this for the first time it is not obvious.

As noted above, the organizing principle of this paper is to show clearly how the characteristics of the data led us to choose approaches to data analysis. However, we agree that we have not specifically described this principle to the reader. We will explain this in more detail.

I would also ask that the authors think about some of the terminology used. The title describes dating of ice masses but the text commonly refers to ice mass (singular). Similarly i dont think the term "middle" ice is helpful when talking about a vertical core with potentially multiple ice masses within it. To me the term paleo-surface implies it is in situ which i dont think the authors are implying; i think this links back to the point about sub-dividing the core on descriptive grounds not interpretative.

The "ice mass (singular/plural)" terminology is dictated by the fact that the description of the site starts with the simplest assumption of a single ice mass. The physical appearance of the core gave no clear indication of more than one ice mass, only after further analyses and modeling we learned that the core contained ice from two separate ice advances at this location. Therefore, we need to refer to them as ice masses.

Middle ice refers to the ice that is found directly below the middle drift that is exposed at the surface. The reason we need to use the qualifier "Middle" in the name is that there is also "Young ice" under the youngest drift located in the lower end of the valley. We need to explain this more clearly in section 2 (Study Area). We also think that capitalizing the names Middle Ice and Young Ice will help to identify them as proper nouns and not as adjectives.

In reference to the paleo surface, we do not imply that it is in situ. We will clarify this in the manuscript.