Response to Anonymous Referee 1
Referee comments are in regular font
Our responses are in italics

Review of the paper « Characterization of in situ cosmogenic 14CO production, retention and loss in firn and shallow ice at Summit, Greenland

The crucial point in this paper is to put additional constrains on the muon induced 14C production rate in ice as function of depth. A recent study (Dyonisius 2023) indicated that this production channel, which is most relevant in depth below the LIZ, seems to be less pronounced than previously predicted in the literature (by a factor of 6 and 4 for negative muon capture and fast muon interaction respectively). These observations were confirmed in the current study. This may have important implication for ice core research, but potentially also for other research areas where cosmogenic isotope production (not only 14C) in the underground becomes relevant. In the conclusions, this aspect could be highlighted more. This holds also for hypothesis regarding the possible underlying reasons for the resulting disagreement, (such as variations in fast muon energy spectra with depth and cross-sectional considerations etc).
We would argue that the novel / more important contribution of this paper is actually the detailed characterization of in situ ^{14}C behavior (retention, loss) in the firn column, which had not been previously done. Our results are indeed consistent with Dyonisius et al (2023) muogenic production rates for ^{14}CO. However, this study only considers ^{14}CO, which at most amounts to 1/3 of total in situ ^{14}C in ice. We therefore do not think that this manuscript is the right place for an in-depth discussion of the ice vs quartz discrepancy in muogenic ^{14}C production rates. Referee 2 had some similar suggestions (with more detail); please see our response to Referee 2 also. To address this comment and the associated comments from Referee 2, we will nevertheless add some more discussion of this issue in the revised manuscript.

The paper is very well written and technically sound (with some minor issues as listed below). The 14C extraction and analytical methods, which follow well established procedures described in the literature are well documented. Data interpretation was done in the frame of existing production- (Balco 2008) and ice transport models (Buizert 2012). This excellent contribution is basically ready to be published as it is.
We thank the referee for their positive and constructive review.

Some uncertainties persist in the parametrization of the partitioning of 14CO in the ice reservoirs. The introduction of a two-domain approach, involving rapid and slower release rates/reservoirs, may seem somewhat arbitrary. While this approach might align well with the data (as expected with the inclusion of extra model parameters), there remains a need for some additional physical justification in the text, although some hypotheses are mentioned in L 526 ff. The small resulting retention of ~0.5% makes me wonder how "worst" the model fit would be without consideration of R1?
Physical justification / mechanism for the main, rapidly-leaking ^{14}CO reservoir / process in the model (R_0, L_0) is hypothesized to be gas diffusion through ice and is presented in detail in the manuscript in section 4.2, starting on line 493. Regarding the physical justification / mechanism

for the small, slowly-leaking ^{14}CO reservoir / process in the model (R_1, L_1), we can add some further details, for example highlighting the evidence for microbubbles (closed porosity) in the shallower part of the firn, both from our results and prior studies.
While the fraction of in situ cosmogenic ^{14}CO that is initially retained in the firn matrix (≈0.5%) is small, it is not possible to explain the results without this smaller slow-leaking reservoir (R_1). If R1 is ignored (R1 = 0), then there is no retention of 14CO in the firn matrix, and all the model curves on Fig. 4A would have 14CO = 0 in the depth range from 0 to ≈60 m. So there is no way that the model can fit the data without considering R1.

The leakage coefficient L0 was fixed at 1 yr-1 (corresponding to a half-loss time of 0.7 years). Where is this value coming from?
We thank the reviewer for pointing this out. This is an error in the manuscript description of this part of the model (though not in the model calculations). The primary (larger, fast-leaking) ice grain reservoir is defined in the model in such a way that all of the ^{14}C in this reservoir is lost to the porosity during each time step (time step = 0.5 yrs). We will correct the error and clarify this in the manuscript.

What exactly is meant with "indicating slow leakage of 14CO from the ice grain (L 421 p 13) in relation to the concentration decrease with depth? Please clarify.
As firn layers move downwards in the firn via the processes of continued snow accumulation at the surface / firn densification, the layers would carry with them the ice grain ^{14}CO content they acquired above. Further, ^{14}CO production continues (by muons) at intermediate firn depths (20 – 60 m). Therefore, the fact that ^{14}CO content in the ice grains decreases rather than increases with increasing depth between 20 and 60m indicates slow ^{14}CO leakage (loss) out of the ice grains. ^{14}C radioactive decay is far too slow to explain this, as the ice layers traverse the entire firn column at Greenland Summit in only ≈200 years. We will clarify this further in the revised manuscript.

Minor issues
P14 L455ff: there is something wrong with the grid search interval for Fn. I guess the step size should be 0.01 instead of 0.05
We thank the referee for catching this typo -- this should indeed state 0.01; we will correct this in the revised manuscript

Page 16, L515ff: Also, here is something wrong. It seems that the diffusion time was calculated with a grain radius of 3mm. Either this is a typo or the resulting diffusion time for 0.3mm should be ~4.5 h instead of 18 days (what emphasizes the assumption of nearly complete 14C loss on timescales of 1 year even more).
We thank the referee for catching this typo as well -- we indeed calculated the diffusion time using a radius of 3 mm; this will be corrected in the revised text