Reply to Anonymous Referee #4

Referee #4 The paper presents new data about the accumulation/ablation rate of the Adamello Glacier, the largest in Italy, estimated from a new ice core. Results are interesting, however some weaknesses need some improvements.
Authors: We thank anonymous referee #4 for the useful suggestions to improve our manuscript and as follows we address the recommendations.

Referee #4 Abstract. It seems there is a contradiction when stating that the surface is clearly old and that the drilling is in the accumulation zone. Scientific literature about the Adamello glacier mass balance indicates that the area is not in the accumulation zone.
Authors: We rephrased into "former accumulation zone" to be more consistent.

Referee #4 Line 45. I am quite surprised by this conclusion. Being the altitude at Pian di Neve 759 m below the Alto Ortles Glacier we expect a 4◦C-5◦C mean temperature below and so definitely stronger temperate glacier conditions than Ortles.
Authors: We agree that at Pian di Neve, compared to Ortles, stronger temperate glacier conditions are likely for the reasons pointed out by the reviewer. What we concluded about is the expected similarity in the trend between the two sites, not about them having the same temperature or even equal temperatures at the same depth. The trend we refer to is "temperatures around 0°C in the top part" (cannot be higher at Adamello because ice does not exists above 0°C) with temperatures of the ice being (at least) slightly lower below, thus a "cold deeper part". Of course, because of pressure due to the (large) ice thickness and potential geothermal heat, temperatures being higher again in the very bottom part cannot be excluded and will only be known once borehole temperatures in a deep core borehole will be available. Anyway, temperatures around 0°C in this part though would imply the presence of water there. This was however not confirmed by the analysis of Picotti et al. (2017).
To clarify, this section was changed. Thereby the reasoning pointed out by the reviewer was included: "With Pian di Neve (3100 m a.s.l.) being located in the same region, affected by similar climatic conditions, but with a far larger ice thickness, a similar trend in ice temperatures - the presence of temperate ice in the upper part and colder ice temperatures below - is not unlikely. While seismic analyses, do confirm the absence of melt water at the base of the glacier (Picotti et al., 2017), temperate ice conditions are however likely to exist to greater depth compared to the Alto dell'Ortles Glacier considering their difference in altitude."

Referee #4 Line 45 Maragno et al. 2009 indicated an area loss of 19% and not a mass loss in the period 1983-2003. A more precise description of the meteorological and mass balance context is recommended also based on a more complete literature review of mass balance in the region.
Authors: "mass loss" has been corrected to "area loss". Additional information from and references about regional mass balance studies was included in the revised manuscript (see related comments/answers by/to the other referees).

Referee #4 Line 126 Because of the melting conditions at the surface I ask to comment how the exact timing of the radionuclides can be ensured. I have doubts about the correspondence between ice core depth and age.
Authors: We are not entirely sure if we correctly understand the referees concerns here. The relation between the activity of radionuclides and time/age is given be the law of radioactive decay. We thus think the referee rather refers to the possibility of relocation of particles in the ice to greater depth by percolating melt water. However, if this is indeed the issue, we are a bit puzzled about this comment. Using different dating approaches to overcome the challenges imposed by post-depositional bias, as described and discussed in the manuscript to be undeniably present to some extent for each of the parameters used, is the strength and main message of our study. So to answer in short, in our study, the agreement between the independent dating using Pollen and rBC and the dating based on the radionuclides does argue against significant relocation of the radionuclides. That rBC, pollen and the radionuclides used are reasonably well preserved, i.e. not easily relocated or strongly affected in the presence of percolating meltwater is in agreement with findings of previous other studies already cited in the manuscript. Specifically for the radionuclides, we here would like to refer again to Gäggeler et al. 2020 (^{210}Pb) and for Pb and Cs to Avak et al., 2018 and Avak et al., 2019 who showed that these trace elements are reasonably well preserved in the ice in case of melt water percolation. The references to the studies by Avak et al. were now also added to the manuscript (in the Introduction).

Referee #4 Figure 2. I do not see a clear correspondence between Pollen&Spores and rBC in Figure 2A and 2B if any was expected. The timing seems to be fairly kept but the correlation seems to be very weak. Can the authors plot a scatter plot with the two variables.
Authors: No correlation between Pollen&Spores and rBC is expected, at least not for the industrial period. While pollen and spores are of biogenic origin, BC (soot) is then to a large part of anthropogenic origin (see e.g. Sigl et al, 2018). Nothing about correlation, neither strong nor weak, is claimed in the manuscript (we even never used the word "correlation" or "correspondence"). Of relevance in the context of this study is only that for maxima and minima "The timing seems to be fairly kept" as the referee agrees on. In other words, what is important and the only point we make is the observation of synchronicity in pollen and spores and rBC peak maxima and minima (in the revised manuscript even better visible with the new Figure 3 added). This synchronism is mainly caused by vertical transport (stronger in spring/summer-strongest/fall) and time of highest emission (there likely is a shift in the exact time of year between highest emissions of pollen and spores and BC; because of different sampling resolution used, this might however not be possible to investigate in more detail), but this is already out of scope of the manuscript. Important is, that seasonality in their signal exists (and is preserved) allowing to count annual layers with both parameters yielding a comparable number of peaks (years), see related reply to referee Bohleber. For the reasons outlined above, a scatter plot would thus not be helpful or make much sense in the context of this study.

Referee #4 Figure 3 shows a fair correspondence. Can the Authors plot a moving average line to better identify the peaks in 210Pb at Silvretta and Adamello?
Authors: This is not so easy because of the different sampling resolution of the two records. What should an objective averaging window be? How to treat the points of very high activity at the surfaces? etc…
Since we see no benefit from adding a trend line for the purpose of this figure, we prefer to keep it as simple as possible and thus in the current version. We are encouraged in this decision because based on the current visualization the referee agrees that "a fair correspondence" between the two records exists. This is the main and only take-home message. In the manuscript we accordingly write: "The ADA16 ^{210}Pb record strongly resembles the ^{210}Pb profile of the nearby Silvretta (SI) ice core…" and "…a reasonable alignment of the two ^{210}Pb profiles was achieved, both showing a very similar, characteristic pattern…".

Referee #4 Figure 5 is quite problematic. With just three points in the 1-40 years range it seems difficult to fit the Dansgaard Johnsen flow model up to 10000 years also considering the morphology of the bedrock underneath Pian di Neve. So I agree with the Author's comment at line 260-261. I would add 'very crude'.
Authors: We agree. Also considering the altitude of the site, we consider an age of up to 10000 to be very unlikely (see Bohleber et al. 2020). We removed the according numbers from the manuscript text. Considering also the comments from the other reviewers (see related comments there), the text of this paragraph has been reformulated in order to more clearly portray the main message. It now reads:
"For an estimation of the potential age range accessible by the Adamello ice archive, the one-dimensional Dansgaard-Johnsen ice-flow model was applied (Dansgaard and Johnsen, 1969). For the resulting age-depth relationship estimate shown in Figure 5 (Fig 6 in the revised version), model parameters were as follows. Based on the bedrock depth determined by ground penetrating radar measurements by Picotti et al. (2017), the value for glacier thickness at the drill site was 265 ± 5 m (238 ± 4.5 m w.e.). The bottom shear zone thickness was assumed to be 15 % of the glacier thickness. This is slightly lower than the ~20 % typically observed for cold and polythermal high-elevation glaciers (e.g. Jenk et al., 2009; Uglietti et al., 2016; Gabrielli et al., 2016; Licciulli et al., 2020) but likely more reasonable for a temperate glacier (e.g. Kaspari et al., 2020). In any case, because constraining information from dated age horizons is lacking for the bottom part, a relatively large uncertainty of ±10 % was assigned. With these parameter settings, the value for the annual accumulation rate was found by tuning for a best model-fit to the dated 1986 and 1963 137Cs horizons (least squares approach). The dating uncertainty and the uncertainties of the pre-set model parameters as indicated above were employed to derive upper and lower bound estimates (to transfer the contribution form uncertainty in ice thickness to uncertainty in age, relative depths were used).
The model - nicely matching the determined bottom age for the ADA16 core and accounting for layer thinning (vertical strain) – provides us a best estimate of the mean annual accumulation rate at the ADA16 drill site for the period ~1946 to 1986 of 0.9 ± 0.03 m w.e. a-1. However, the assumption of steady-state conditions and the complexity of bedrock geometry and glacial flow in the deepest part of high-alpine glaciers strongly limits a realistic modelling of strain rates (and thus age) for the deeper parts, even with the most complex glaciological 3D ice-flow models. In our case, the lack of data for additional constraint in the deeper/older part, the assumption of steady-state conditions in annual accumulation rates (equal to an average value for the entire period contained in

the archive) which are further based on a relatively short time range covered by the 46 m core only, the derived model-based age-depth relationship can only yield a current best estimate. Anyhow, this is at least sufficient to reveal the potential of the site. Being contained in the major part of the total ice thickness (about the upper 240 m of ice; ~220 m w.e.), a millennial-long record should thus be accessible in high resolution. Also, there is reasonable likelihood for a few thousand more years contained in the remaining ~10 % of ice below. This is of high relevance in the perspective of an upcoming drilling campaign at Pian di Neve to retrieve an ice core down to bedrock."

Referee #4 Conclusions. In the conclusions I would better stress the estimated accumulation rate of 0.8-0.9 m w.e. yr-1 which is quite convincing than the Dansgaard-Johnsen model age estimate which is very uncertain.
Authors: As suggested, we added the estimated accumulation also in the conclusion and at the same time weakened the statement regarding the age of the bottom ice, not giving a specific number.

References not included in the manuscript

Avak SE, Schwikowski M and Eichler A: Impact and implications of meltwater percolation on trace element records observed in a high-Alpine ice core. Journal of Glaciology 64(248), 877–886, doi: 10.1017/jog.2018.74, 2018.

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Bohleber P, Schwikowski M, Stocker-Waldhuber M. et al.: New glacier evidence for ice-free summits during the life of the Tyrolean Iceman. Sci Rep 10**,** 20513 https://doi.org/10.1038/s41598-020-77518-9, 2020.