Reponses to reviewers' comments.

Please note, that Figure numbers and Sections given in the following refer to the manuscript as it was revised in May 2023. If they would change in the potential next manuscript version this is assigned "now Fig. X" and or "now Section X". Line numbers refer to the new version of the manuscript.

Reviewer 1

Thanks for having reviewed our manuscript a second time.

The caption of Figure 1 could use more information - can the arrows be color coded such that portion of the crevasse seen in both a) and b) are the same, and the lighter arrows then define extension of the crevasse in a)? I find it hard to tell what portion of the feature is the same in the two photos, and what portion of the feature is new in a) compared to b)

Thanks for your comment. We agree that it is hard to bring both photos together since they were not taken from the same position. a) was taken from the helicopter and b) from the slope which leads to the Vallot Observatory. We color coded common crevasse features as you advised (see figure and caption below).

- describe the difference between the solid and dashed line in c) in the caption

thanks, this was done (see figure caption below)

- c) looks like a topographic map to me the caption describes it as an aerial photograph. i think it should be reworded to indicate that the topographic map is from aerial photographs

Yes, c) is a topographic map of the glacier surface and sorry if this was not clearly identified in the caption. We've also the bedrock topography in Figure 1c (now Figure 2c). The caption is updated now and clearly states that c) is a topographic map. This part of the caption now reads:

(c) Topographic map of the Col du Dome and Dome de Gouter together with the underlying bedrock topography (adapted from Vincent et al., 2020). Contour lines are spaced at 5 m intervals.

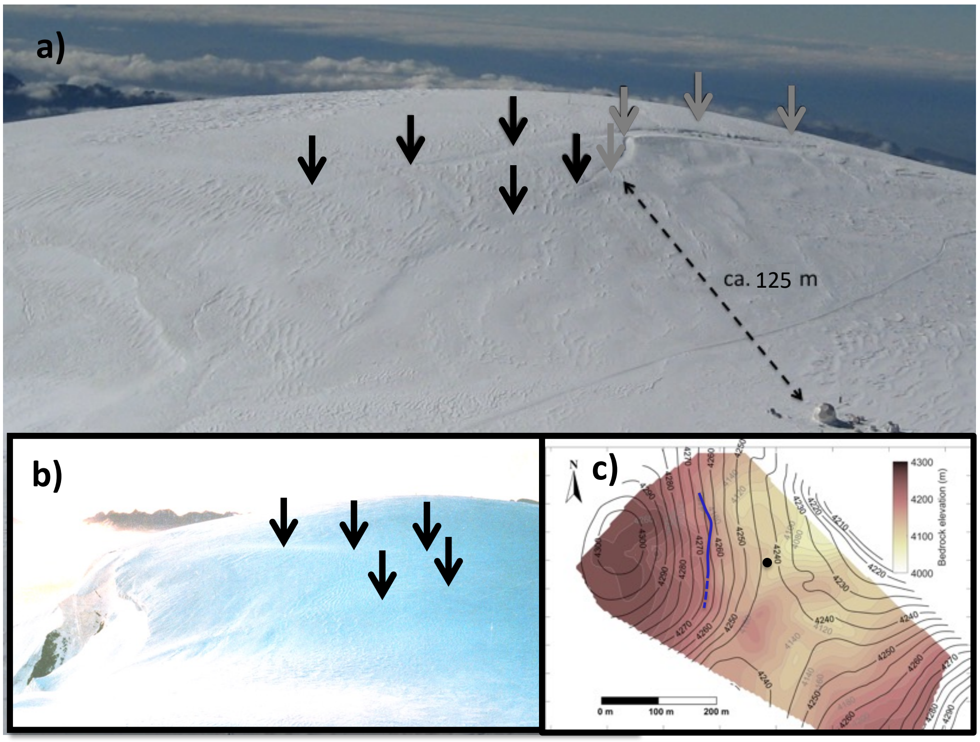

Figure 2: View of the South-East flank of the Dome de Gouter and Col du Dome saddle including the drill site of 1994, 2004, and 2012 situated downslope of Dome du Gouter. Note that the three drill sites are located within about 10 m of each other and thus are indicated by a single dot in (c). (a) Picture taken in summer 2012: A large crevasse extends across the upstream catchment area of the drilling site. At the time of the picture the distinctly visible crevasse was mainly snow-covered. A potential second crevasse also is visible on the southwestern slope of the glacier. (b) Picture taken in summer 1999: Evidence of one to two crevasses limited to the southwestern side of the Dome du Gouter. Black arrows in a) and b) indicate parts of the crevasse which are suggested by the surface features in a) and b). Grey arrows in a) mark the part of the crevasse which was only visible in 2012 (c) Topographic map of the Col du Dome and Dome de Gouter together with the underlying bedrock topography (adapted from Vincent et al., 2020). Contour lines are spaced at 5 m intervals. The main crevasse highlighted in (a) and (b) is reported in (c) based on an aerial photo from Institut national de l'information géographique et forestière (IGNF) taken at 30th June 2004 (blue solid line in (c) indicates the part of the crevasse which was clearly visible, and blue dashed lines demark the part which was less clearly visible).

Reviewer 2

Thanks for having reviewed our manuscript a second time.

The responses to my comments are not satisfying. There are again a number of scientific inconsistencies and lack of scientific thoroughness as outlined below, which prevent assessing the scientific quality.

210Pb lead record: The spatial resolution for most of the samples was included now, but this is not sufficient. A consistent temporal averaging has to be done to ensure data comparability. Now additional questions arose: 1) why is the spatial resolution not shown for all samples? 2) Why are there two data points at 60 m depth of CDM?

Thanks for this comment. We agree in principle that consistent temporal averaging would be good to compare the ^{210}Pb records. However, for our application and in view of the small depth coverage in the upper part of CDK and CDM this is of no advantage. We clarified in the text that ^{210}Pb measurements were focused on the part where the ^{210}Pb profile is disturbed and that in the upper part only sporadic ^{210}Pb measurements are available.

In Section 2 (now Section 2.2 line 199) it reads:

… We stress that whereas ^{210}Pb was measured continuously on discrete samples covering the whole C10 ice core, ^{210}Pb measurements in CDK and CDM were focused on the ^{210}Pb anomaly starting around 80 m depth. Therefore, only point-wise measurements with sample lengths between 0.6 to 1m length, i.e. covering less than one year, were made in the upper part of the latter two cores, with the exception of two CDM samples which were integrated of over core depths of 10 m each (covering 2 and 4 years).

And in of Section 3.2, line 393:

We stress that whereas ^{210}Pb was measured continuously on the C10 ice core, ^{210}Pb measurements on CDK and CDM were focused on the ^{210}Pb anomalies starting below 80 m. Therefore, only a few samples with limited depth and time coverage are available in the upper parts of CDK and CDM. However, comparing ^{210}Pb levels of the shorter CDM samples with the two samples integrating 2 and 4 years, and in view of the limited seasonal variation (with the exception of the outstanding hot summer of 2003) observed in Fig. 7, we assume the sample lengths and coverage is good enough to depict the order of magnitude of ^{210}Pb activities in the upper parts of the cores. …..

Concerning the two questions of this comment:

1) As outlined in the Fig. 5 caption (now Fig. 6) (*…For CDM (a) and CDK (b) the depths covered by the samples are plotted with thick black lines, whereas the thin lines are given to guide the eye and were used to calculate the ^{210}Pb inventories…..)*
the spatial extension of all CDM and CDK samples was added with thick lines. Since typically ice core sections of 0.6 to 1.0 m long ice core sections were analyzed the spatial extension is not very large for some samples.

2) For CDM, in addition to the samples with a length ranging between 0.6 and 1.0m, two parallel samples were measured integrating several meters of depth. This was done to get an idea of the representativeness of the shorter samples.
This is explained now in the manuscript, together with the fact that the ^{210}Pb measurements in CDK and CDM were focused only on the parts of the cores where the ^{210}Pb profiles were disturbed.

In Section 2 (now Section 2.2 line 199) it reads (as above):
… We stress that whereas ^{210}Pb was measured continuously on discrete samples covering the whole C10 ice core, ^{210}Pb measurements in CDK and CDM were focused on the ^{210}Pb anomaly starting around 80 m depth. Therefore, only point-wise measurements with sample lengths between 0.6 to 1m length, i.e. covering less than one year, were made in the upper part of the latter two cores, with the exception of two CDM samples which were integrated of over core depths of 10 m each (covering 2 and 4 years)…

And in Section 3.2. line 393 it reads (as above):
We stress that whereas ^{210}Pb was measured continuously on the C10 ice core, ^{210}Pb measurements on CDK and CDM were focused on the ^{210}Pb anomalies starting below 80 m. Therefore, only a few samples with limited depth and time coverage are available in the upper parts of CDK and CDM. However, comparing ^{210}Pb levels of the shorter CDM samples with the two samples integrating 2 and 4 years, and in view of the limited seasonal variation (with the exception of the outstanding hot summer of 2003) observed in Fig. 7, we assume the sample lengths and coverage is good enough to depict the order of magnitude of ^{210}Pb activities in the upper parts of the cores. …..

The new statement in the revised text that together with the systematic decrease of the winter to summer layer thickness ratio with increasing depth, seasonality counteracts the expected 210Pb decrease from radioactive decay is critical. If this decrease in winter snow contribution resulted in increased 210Pb values, how can this be disentangled from the evoked crevasse effect?

The winter to summer snow ratio versus depth at the drill site is detailed in Preunkert et al., 2000, Figure 9. The ratio decreases from ~1 at the surface to 0.6 at 75 m depth (well above the ^{210}Pb disturbance). This decrease of winter to summer ratio by almost a factor of 2 counteracts the decrease of ^{210}Pb, as stated in the text in Section 3.2. Below 75m and down to 111 m (i.e., over the depth of the ^{210}Pb anomaly), the winter to summer ratio does not change significantly but stays between 0.6 and 0.5.
Therefore, it will not contribute significantly to the ^{210}Pb enhancement observed in the cores below 80m.

This also is detailed in the revised text in Section 3.2 line 433:
… Together with the systematic decrease of the winter to summer layer thickness ratio with increasing core depth down to 75 m (from 1 to 0.6) at the drill site (see Sect. 3 and Preunkert et al., 2000), this pronounced ^{210}Pb seasonality at least partly counteracts the expected ^{210}Pb decrease from radioactive decay. ….

and line 449:

… Since winter to summer snow ratios lie consistently between 0.5 and 0.6 in the C10 core in the depth interval of the ^{210}Pb anomaly (Preunkert et al., 2000), these increases in ^{210}Pb cannot be attributed to changes in seasonal snow deposition.

The discussion on detection limits and uncertainties of 210Pb analyses with gamma spectrometry is still confusing. Both should be given in the same unit as the 210Pb values shown in Fig. 4 (mBq kg-1). In the new text both are given in mBq. The same is true for the blank values of the alpha-spectrometry. This is given in Bq. Without any information on samples size and counting time, this cannot be related to Fig. 4.

Thanks for this comment. We think you meant Fig. 5 now Fig. 6.
The blank values of ^{210}Pb and 3H measurements are given now in Bq kg-1 and TU, respectively.

The text in Section 2 (now Section 2.2) concerning the ^{210}Pb now reads as follows for the measurements made at IUP (line 165):
…. With typical sample masses of 300 to 1,000 g and measurement times of 2 to 6 days, mean ^{210}Pb measurement errors of 4 ± 4 mBq kg^{-1} were achieved on ice core drill chip samples spanning ice core depths between 0.6 and 1 m.

And for the ^{210}Pb data from C10 measured at IGE (line 192):
… we assume in the following a detection level of 10 mBq assigned by Pinglot et al. (2003) and a maximum uncertainty of 30 mBq for all C10 ^{210}Pb measurements. Taking 1 kg sample mass as an absolute lower limit, this would amount to a total error of 30 mBq kg^{-1}.

In addition, it is now stated that two additional 210Pb values of C10 were added to Fig. 4, which were not published in Vincent et al., 1997. Those are just the two highest values in the entire record. It would be more convincing to show the comparison between the inner and outer samples. The same is true for the separation of 137Cs and 210Pb: it would be convincing to see a spectrum. Especially because the C10 core is the one for which the 210Pb artefact is most obvious and that was analysed with the less sensitive method.

The reason why these high values were added after the publication of Vincent et al. (1997) is already given in the text in Section 2 (now Section 2.2). The method to measure ^{210}Pb with gamma detection was well established in the literature 30 years ago and is therefore referenceable. It is clearly beyond the scope of this manuscript to provide the analytical details here. In addition, ^{210}Pb values are not extremely high if compared to the ^{210}Pb peak values detected with alpha spectrometry in the CDK and CDM cores (measured by alpha spectrometry). Pinglot and Pourchet (1995) compared alpha and gamma spectrometry measurements of a sediment sample and achieved a fairly good agreement of both measurements. This comparison is now reported in the manuscript, and when comparing the ^{210}Pb inventories in Section 3.2 the difference in the two methods is noted.

This reads in the text as follows in Section 2 (now Section 2.2 line 174):

Although made on a sediment sample with much higher specific ^{210}Pb activities than found in core cores, Pinglot and Pourchet (1995) made a direct comparison of ^{210}Pb alpha and gamma-ray measurements. They found that the measurements made with α-spectrometry were only ~84 ± 11 % of the respective values obtained with gamma spectrometry and attributed the difference to insufficient acid leaching during the α-spectrometry sample preparation. However, both methods generally provide comparable activity values and the relative temporal variations in the activities should be robust. …

and Section 3.2. line 493:
… Furthermore, the CDK and CDM ^{210}Pb anomaly inventories (Fig. 6) are 4 times lower than in C10, which cannot be explained by analytical differences of the measurement methods (see Sect. 2.2). We stress that the ice originating at the crevasse had essentially the same travel time from the crevasse to the drill site for a given depth in case of all three cores....

The hypothesis of the two states of the crevasse is not supported by the data. The upper parts of the nitrate records (1990-1979) show the expected compression/thinning from 1994 to 2012 with a decrease in depth range covered by those 11 years from 35 m (1994), 21 m (2004) to 14 m (2012). However, the depth range from 71 m to 79 m, i.e. 8 m in C10 (1994) corresponds to the depth range of 78 m to 89 m, i.e. 11 m in CDK (2004) and probably 88 m to 96 m, i.e. 8 m in CDM (2012). Such a behaviour is highly unlikely for cores collected only 10 m apart from each other, since it would imply not only no thinning, but on the contrary thickening of CDK. This leaves doubts about the dating and also about the decay-corrected 210Pb activity in Fig. S1, which depends on an exact dating.

Thanks for this comment. We added a paragraph in Section 3.1.2 about that inhomogeneity between CDK and C10. In fact, a similar feature also was observed in CDM (Section 3.1.3). As in CDM this annual layer thickness increase is just at the starting limit of the discontinuity in CDK, which might have started already a few meters above 89 m depth, i.e. at 85-87m, just at the depth where the ^{210}Pb rises.
The resulting dating uncertainty (see text in Section 3.1.2) has, however, a very limited effect on Fig. S1 (now Fig. 7) since there is no ^{210}Pb datapoint in the range of 85 to 87m, i.e. the depth interval for which dating is more uncertain in the case that these snow layers were not integrated via regular surface deposition in the ice core.

The text in Section 3.1.2 (line 301) was changed accordingly:
A closer look at the NO_3^- and NH_4^+ raw data in CDK shows that the depth interval from 80 to 89 m appears to correspond to 72 to 79 m depth in C10 and may be attributed to the years 1976-1971 as done by Legrand et al. (2013). However, these 5 years span 2 m more in CDK than in C10 corresponding to a relative thickening of layers by a factor of 1.28. Such an anomaly in the thinning curve cannot be explained by the systematic layer thinning at the drill site caused by undisturbed upstream inflow of ice (see also Fig. 3) along the same flow line for CDK and C10. However, refilling the void of the crevasse by inflow of ice from upstream may explain such a thickening.

and to Figure caption of Figure S1 (now Fig. 7) we added:

... To avoid a potential overestimation of ^{210}Pb activities between 85 and 92 m due to the dating uncertainty, the recent date limits of the uncertainties were assigned to the samples..

Discarding nitrate and ammonium data: The procedure of removing CEP peaks because they were not present in the DRI dataset is not convincing and not scientifically sound. Why is the DRI assumed to be correct? There are still CEP values in the figure, which are higher than the corresponding DRI value. You need an independent criteria to identify contaminated values. In addition, most of the data gaps are in the sections below 88 m, for which no DRI data are shown. What is the reason for those data gaps?

Thanks for this comment.
First, the reason for the data gaps below 88 m is that the ice core sections available at CEP were too small for reliable analyses.

We tried to better explain this and the reasoning underpinning the data comparison between DRI and CEP.

DRI dataset is assumed to be correct since working conditions were as they should be, i.e. the ice section had the regular size for routine CFA measurements using the DRI analytical system. This was not the case at CEP since the available ice core section for CFA measurements were at or below the lower size limit for the analytical system and, given the small cross section, we were not completely confident that the sample had not been altered by circulating laboratory air.

Taking the advantage of having a second dataset measured under regular conditions over a limited depth interval, we made a comparison of the two datasets in the depth interval covered by both of them, to get an idea whether the analyzed CEP data are reliable and useful for their application in this study. Note that further down in the CDM ice core where no DRI data were available, no additional datapoints were discarded from the CEP dataset. However, we used the CEP DRI comparison to conclude on the reliability of NO3 and NH4 data measured in these core depths. We state this now clearly in the text.

The corresponding text in Section 2 (now Section 2.2 (line 215)) reads now as follows:
... However, since the CDM ice core has only a 3-inch diameter, the ice available for the CFA analyses at CEP consisted only of a non-rectangular cross-section with maximum outer dimensions of 2.5 x 3.0 cm instead of the standard quadratic size of 3.5 x 3.5 cm for which the standard melt head at CEP is designed. Although a special, smaller melt head was constructed for the CDM analyses, it was not always possible to assure that the CFA melt water only came from an inner section of the ice material with no contact to the outer surfaces. This may have led to a higher risk of contamination of the inner sample melt water stream and with the smaller melt water flow available implied also a reduced analyte spectrum. Despite the undersized core section available for the CFA analyses at CEP, 86% of NO_3^- *and/or* NH_4^+ *raw data could be evaluated. To test the reliability of the CEP dataset, the nitrate profiles obtained at DRI and CEP (covering 97% in this depth range) were compared over the depth interval 45 to 86 m. Both datasets are in very good overall agreement, except for individual outliers in the CEP data.*

After having additionally discarded very high peaks (concentrations above 700 ppb) in NO_3^- values (1.5% of CEP data in the depth interval from 45 to 86 m), which were not present in the DRI dataset and could be attributed easily to contamination, mean NO_3^- values over this depth interval were 263±281 ppb (CEP) and 255±231 ppb (DRI) (Fig. 4). The agreement is somewhat weaker for NH_4^+ likely because for this species only 80% of this depth range is covered by CEP measurements. After discarding additionally 8 % of the CEP NH_4^+ data between 45 and 86 m consisting of high NH_4^+ peaks (concentrations exceeding 190 ppb), which were not present in the DRI dataset, the mean NH_4^+ values of 101±110 ppb (CEP) and 95±99 ppb (DRI) were in good agreement. As a consequence of the better reliability, we base our discussion mainly on the NO_3^- data. Below 86 m no additional data were discarded from the CEP NO_3^- and NH_4^+ datasets. However, because no further single NO_3^- peak values above 700 ppb were found below 86 m, we are confident in NO_3^- data below this depth. In the case of NH_4^+ we cannot exclude that a few peaks in the record below 86 m with a concentration higher than 200 ppb might be influenced by contamination.

Figure 3: I still think that you cannot show any data for C10 beyond the year of drilling (1994), because they don't exist. Shifting does not help.
We think you meant Fig. 4 (now Fig. 5). It makes no sense to compare annual layer thicknesses of the three ice cores on absolute age if they are not drilled in the same year. Thus, to overcome this problem we shifted the data of CDM and C10 in time so that all three data sets to simulate a common drilling year. This also was stated in the legend. Since this was not clear to the reviewer, we changed the vertical scale in Fig 4a to "Year before drilling".

Note that to increase data consistency in recent and the present publications we updated Figure 4 (without changing its scientific meaning) to no longer show the originally published data versions of Legrand et al., 2013 and Preunkert et al., 2003, but the actual data as they are archived in the database.

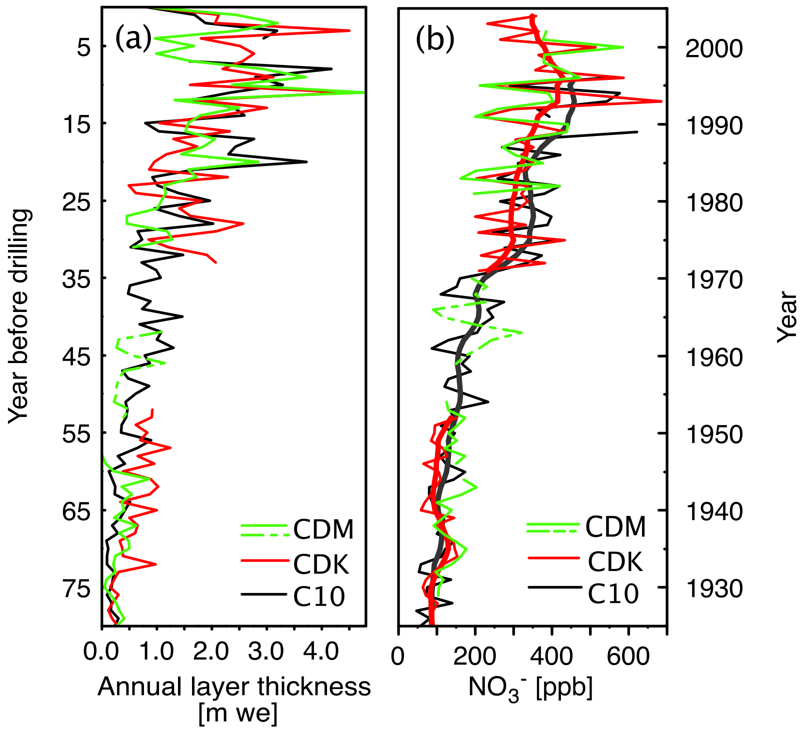

Figure 5: (a) Annual layer thickness of C10 (adapted from Preunkert et al. 2000), CDK (adapted from Legrand et al., 2013) and CDM. For CDM, the annual layer thickness is estimated via the ammonium stratigraphy back to 1980 and via the nitrate (and ammonium) stratigraphy further back in time (Sect. 3.1.3). (b) comparison of nitrate summer half-year means of C10 (adapted from Preunkert et al., 2003), CDK (adapted from Legrand et al., 2013) and CDM. The thick solid lines for C10 and CDK refer to the smoothed profiles (single spectrum analysis, see Legrand et al., 2013). CDM depth intervals for which the dating is uncertain (Sect. 3.1.3), are marked with dashed lines.

Confusing is that in the revised version new data from another core from 1994 are shown additionally in Fig. 5, which are not mentioned in Tab. 1 and which have a different labeling than in Vincent et al., 1997.

Thanks for this comment, sorry that we overlooked that.
Data in Table 1 are limited to the three cores which could be investigated in detail.

We updated the title of Table 1 accordingly to:
Table 1: Basic glaciological and radiometric parameters of the three CDD ice cores investigated in this study.

The Fig. 5 caption (now Fig. 6) was updated to read as follows:
… are compared to those from a 140 m long ice core extracted 30 m away from C10 also in 1994 (labeled as "ice core 2" in Vincent et al., 1997 and denoted here as C11, upper x-axis in c, orange)…

Reviewer 3
Alison Criscitiello
Review of: "Impact of subsurface crevassing on the depth-age relationship of high-alpine ice cores extracted at Col du Dôme between 1994 and 2012"
I will keep this third review brief in the interest of expediency. Preunkert et al. compare records from three ice cores from Col du Dôme drilled in 1994, 2004, and 2012. The 1994 (C10) age scale is intact, while the 2004 (CDK) and 2012 (CDM) age scales are disturbed during the 1950s/60s. Chronologies are largely established by annual layer counting of ammonia, with the disturbances identified by the H3 and 137Cs records. Disturbances are attributed to the presence of an upstream (to flow) crevasse, which did not intersect the flow path reaching the core site in 1994 (but it did subsequently). During times when a snow bridge covers the top of the crevasse, 210Pb accumulates in the crevasse and surrounding firn. I find the discussion and suggested mechanisms for both the observed layer doubling (or missing layers) and the impact of the two crevasse states on 222Rn and 210Pb accumulation novel and fairly well supported. This is an interesting and new theory for processes occurring at this site, and I enjoyed reading the mss.

In the revised mss, I can see huge improvements in the edits and additions made in response to the previous referee and editor reviews. Really like the added Fig.2 b and c panels. A few questions and comments below.

- I think there was a core drilled on Dôme de Gouter, correct? Is there an archive stick left, that perhaps 210Pb could be measured on (for comparison to CDD)?
  Thanks for this comment. In principle this would be a good idea. However, there is not much material left and the advantage we could get from that will be very limited since there are additional crevasses at the Dome de Gouter itself and very high ^{210}Pb values (28 Bq k-1) are reported by Vincent et al., 1997.
  We give this value now also in the text and have added a paragraph in which we roughly estimate whether the crevasse could be responsible for the enhanced ^{210}Pb values observe in the ice cores investigated here.

- Fig. 2 is rather confusing. Not sure how to simplify it or make it more readable. Perhaps the insets could be moved to a separate figure? Worth a think. I don't like the NO3 offset which is an artifact of drilling year, I understand, but could perhaps be corrected for so the records line up?
  We think you meant Fig. 3 (now Fig. 4). We are not totally sure what is meant with NO3 offset, there is an offset in the depth scale we have to apply because of the different drilling years. We tried to rearrange the Figure so that there are common depth scales for all three ice cores. However, this would crush the depth zones around the ^{210}Pb disturbances which are of main interest in this manuscript. We puzzled also about the 3H/137Cs insets in the figure, but in the end we found its best to leave them as is. Among others also since in the discussions e.g. Section 3.1.2 and 3.1.3 the 3H und 137Cs markers are used directly in connection with the ionic stratigraphies. Please note also that the annual layer thicknesses of the three cores, for which a comparison in Fig.3 (now Fig.4) is not obvious are reported in Fig. 4 (now Fig. 5).

- “In the C10 core, 210Pb was determined by gamma-spectrometry (Vincent et al., 1997), whereas for the CDK and CDM cores 210Pb was analyzed by alpha-spectrometry of its decay product 210Po after chemical enrichment, which is the much more sensitive method.” This is one of the more concerning aspects of the mss I found. Uncertainties arising from utilizing 210Pb data obtained by more than one method should be discussed in more detail.

  Thanks for this comment. This matter is now discussed in more detailed including a comparison between the gamma ray method used for the C10 measurements and a parallel measurement with alpha spectrometry. The differences amount to about 20%. We point out this difference when comparing the ^{210}Pb inventories of the three ice cores.

  The text in Section 2 (now Section 2.2 line 172) reads:
  … We note that the gamma-ray method is less sensitive than α-spectrometry due to the high conversion of the low energy γ-line at 46 keV (96% in the form of electron and only 4% in the form of γ emission) (Gaeggeler et al.2022), and may have systematic differences. Although made on a sediment sample with much higher specific ^{210}Pb activities than found in core cores, Pinglot and Pourchet (1995) made a direct comparison of ^{210}Pb alpha and gamma-ray measurements. They found that the measurements made with α-spectrometry were only ~84 ± 11 % of the respective values obtained with gamma spectrometry and attributed the difference to insufficient acid leaching during the α-spectrometry sample preparation. However, both methods generally provide comparable activity values and the relative temporal variations in the activities should be robust….

  The text in Section 3.2 (line 494) reads:
  … Furthermore, the CDK and CDM ^{210}Pb anomaly inventories (Fig. 6) are 4 times lower than in C10, which cannot be explained by analytical differences of the measurement methods (see Sect. 2.2). We stress that the ice originating at the crevasse had essentially the same travel time from the crevasse to the drill site for a given depth in case of all three cores. ….

- “After having additionally discarded very high peaks in NO3- values (1.5% of CEP data), which were not present in the DRI dataset and could be attributed easily to contamination, mean NO3- values from 45.3-86.0 m were 263 ppb (CEP) and 255 ppb (DRI). The agreement is somewhat weaker for NH4+ likely because only 80% of the depth range is covered by the CEP measurements. After discarding additionally 8 % of the CEP NH4+ data consisting of high NH4+ peaks which were not present in the DRI dataset, the mean NH4+ values of 101 ppb (CEP) and 95 ppb (DRI) were in good agreement.” I find the discarding of what amounts to quite a lot of data concerning. Was a threshold technique used? How did you determine that clear contamination had occurred? You assume the DRI CFA is the benchmark, and any large deviations that don’t align with that record must be contamination? It currently reads as a bit subjective.

Thanks for this comment.
We now explain more in detail difficulties that occurred during the CEP measurements, and tried to clarify the reasons for and approach used in the CEP data corrections. The DRI dataset is assumed to be correct since working conditions were as they should be, i.e. the ice section had the regular size for routine CFA measurements at DRI. This was not the case at CEP since the available ice core section for CFA measurements there were at the smaller size limit (and some core sections were below the limit) to work with the CFA and to work without contamination from laboratory air.

Taking the advantage of having a second dataset measured under regular conditions (the DRI dataset) over a limited depth interval, we made a comparison of the two datasets in the depth interval covered by both of them, to get an idea whether the analyzed CEP data are reliable and useful for their application in this study. Note that further down in the CDM ice core where no DRI data were available no additional data were discarded from the CEP dataset, but we evaluated the reliability of NO3 and NH4 data measured in these core depths. We state this now clearly in the text.

The corresponding text in Section 2. (now Section 2.2, line 215) now reads as follows:
… However, since the CDM ice core has only a 3-inch diameter, the ice available for the CFA analyses at CEP consisted only of a non-rectangular cross-section with maximum outer dimensions of 2.5 x 3.0 cm instead of the standard quadratic size of 3.5 x 3.5 cm for which the standard melt head at CEP is designed. Although a special, smaller melt head was constructed for the CDM analyses, it was not always possible to assure that the CFA melt water only came from an inner section of the ice material with no contact to the outer surfaces. This may have led to a higher risk of contamination of the inner sample melt water stream and with the smaller melt water flow available implied also a reduced analyte spectrum. Despite the undersized core section available for the CFA analyses at CEP, 86% of NO_3^- and/or NH_4^+ raw data could be evaluated. To test the reliability of the CEP dataset, the nitrate profiles obtained at DRI and CEP (covering 97% in this depth range) were compared over the depth interval 45 to 86 m. Both datasets are in very good overall agreement, except for individual outliers in the CEP data.
After having additionally discarded very high peaks (concentrations above 700 ppb) in NO_3^- values (1.5% of CEP data in the depth interval from 45 to 86 m), which were not present in the DRI dataset and could be attributed easily to contamination, mean NO_3^- values over this depth interval were 263±281 ppb (CEP) and 255±231 ppb (DRI) (Fig. 4). The agreement is somewhat weaker for NH_4^+ likely because for this species only 80% of this depth range is covered by CEP measurements. After discarding additionally 8 % of the CEP NH_4^+ data between 45 and 86 m consisting of high NH_4^+ peaks (concentrations exceeding 190 ppb), which were not present in the DRI dataset, the mean NH_4^+ values of 101±110 ppb (CEP) and 95±99 ppb (DRI) were in good agreement. As a consequence of the better reliability, we base our discussion mainly on the NO_3^- data. Below 86 m no additional data were discarded from the CEP NO_3^- and NH_4^+ datasets. However, because no further single NO_3^- peak values above 700 ppb were found below 86 m, we are confident in NO_3^- data below this depth. In

the case of NH_4^+ we cannot exclude that a few peaks in the record below 86 m with a concentration higher than 200 ppb might be influenced by contamination.

- Was the winter to summer layer thickness ratio obtained just from ammonium? Were other glaciochemistry time series used as well?

  Thanks for this comment, we should have written this more correctly in the text. The winter to summer layer thickness ratio is based as the annual layer dating, not just on NH4 but on different aerosol tracers which all show a seasonal variation. This is now reported in more detail in the text.

  The text in Section 3.1 (line 246) was corrected accordingly to:
  … and the winter to summer layer thickness ratio (which was calculated using the seasonal information embedded in the various aerosol tracers measured in the core, Preunkert et al., 2000), decreases from 1 at the surface to ~0.5 at 100 m depth.

  Based on the well-marked seasonality in the chemical stratigraphy for all cores, annual layer counting was used as the main dating tool over the time period of interest of this study, i.e., back to the 1950s. NH_4^+ has a very strong seasonal variation (factor of ~14 higher in summer than in winter) caused by the parallel seasonal changes in source strengths and vertical transport of NH_4^+ (Preunkert et al., 2000). However also other ions (such as nitrate and sulfate) show clear seasonal variations (factor of ~4 higher in summer than in winter). The annual layer counting, which was based mainly on ammonia, was reinforced by absolute time markers such as Saharan dust events (for example the prominent event in 1977) (Preunkert et al., 2000 for C10; Legrand et al., 2013 for CDK, Legrand et al., 2018 and this study for CDM) and radiometric analyses aimed at detecting fallout from atmospheric thermonuclear bomb testing via ^3H (Legrand et al., 2013 for CDK and this study for CDM) and ^{137}Cs (Vincent et al., 1997) for C10. ..

- "The dating of the C10 ice core back to 1925 obtained from annual layer counting of the ammonium record was initially established by Preunkert et al. (2000). More recently, the availability of additional measurements such as lead, cadmium and thallium allowed the dating to be extended back to 1890 without changing the original dating back to 1935 (Legrand et al., 2018)." This implies (with no mention of it) that you changed the original dating between 1925 and 1935, yes? Maybe say a bit more about this (how did you identify a dating error? etc).

  Thanks for this comment.
  In fact, when having measured lead (stable isotopes) and cadmium in the C10 ice core in 2016 (and two years later also in the lower layers of CDK), we recognized that the beginning of the industrial use of these metals in ~ 1890 was visible in the C10 (and CDK) core in form of a significant increase of these metals. With that we had an additional absolute time marker in 1890 and mainly the depth age relation between the last existing absolute time marker (the fluride increase in the beginning of the 1930s and this new time marker in 1890). The updated data

series were then used in publications made in the following, as stated in the text of the present manuscript.
Also, as also stated in the manuscript, that dating update changed only the very lower parts of the depth age relations, which were not in the focus of the present study. Therefore, we kept explanations about that topic rather short. On the other hand, for data consistency between the older publications and more recent ones (including the present one), we added a more precise explanation in the manuscript in Section 3.1.1 and also Section 3.1.2.
In addition, since Fig. 4 (now Fig. 5) shows data back to 1925 we updated it also (without changing its scientific meaning) to contain the actual data as they are archived in the database and not the originally published dataset versions of Legrand et al., 2013 and Preunkert et al., 2003.

The text in Section 3.1.1. (line 263) reads now:
… More recently, new measurements of toxic metals such as lead, cadmium and thallium underpinned identification of an additional absolute time marker, visible as a marked concentration increase in all three metals at the beginning of the industrial period, what allowed to extend the C10 chronology back to 1890 (Legrand et al., 2018). This additional information did not significantly change the original dating back to 1935 (i.e., only by one year back to depth 106.5 m and 5 years at a depth of 112 m), and these changes are within the estimated dating uncertainty of 5 to 10 years (Preunkert et al., 2000).

The text in Section 3.1.2. (line 298) now reads:
… Analogous to C10 (see Sect. 3.1.1), the CDK dating was updated in the lower part on the basis of additional measurements of trace metals such as lead and cadmium without changing significantly the original dating of Legrand et al. (2013) back to 1935 (Preunkert et al., 2019a).

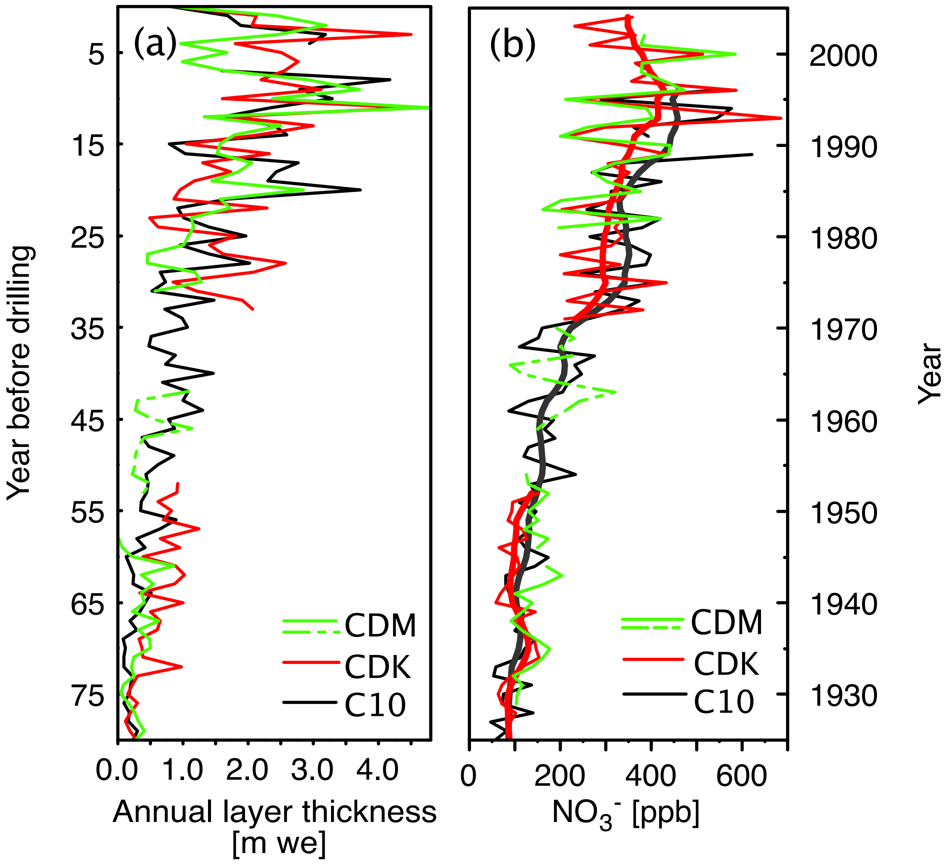

Figure 5: (a) Annual layer thickness of C10 (adapted from Preunkert et al. 2000), CDK (adapted from Legrand et al., 2013) and CDM. For CDM, the annual layer thickness is estimated via the ammonium stratigraphy back to 1980 and via the nitrate (and ammonium) stratigraphy further back in time (Sect. 3.1.3). (b) comparison of nitrate summer half-year means of C10 (adapted from Preunkert et al., 2003), CDK (adapted from Legrand et al., 2013) and CDM. The thick solid lines for C10 and CDK refer to the smoothed profiles (single spectrum analysis, see Legrand et al., 2013). CDM depth intervals for which the dating is uncertain (Sect. 3.1.3), are marked with dashed lines.

- The bomb test horizon insets shown in Fig.4 are really lacking. I certainly understand only looking in certain sections of the core for bomb horizons (where they're expected), but why are there so few measurements? There are so few that it isn't actually possible to confidently pick 1963 (or <1954) at CDM or CDK. Were only wings (bag averages) available?

  Thanks for this comment, I think you meant Fig. 4 (now Fig. 5)?
  Commonly, 3H was measured on integrated samples (bag averages, as supposed by the reviewer), on 0.6 to 1 m length to identify the location of the 1963 peak and the start of the bomb tests. Such a sample resolution (covering around 1 to max 2 years) is "normally" enough to identify the 1963 peak and to localize the start of the bomb tests to ± 1 - 2 years. The expected depth of the horizons was determined via annual layer counting, so that we could avoid an overcharging of the capacity of the 3H lab. Note that the maximum of 1963 itself has "normally" a width of 2 years in rain (Global Network of Isotopes in Precipitation. *The GNIP Database* Accessible at: http://isohis.iaea.org) and snow (Schotterer et al., 1998) deposition records. Therefore, we feel, that if the peak in CDK would have been

included in the record we should have detected it. This was the only aim why the analyses were made.

Reviewer 4

This paper discusses dating anomalies in 3 cores drilled at different dates at Col du Dome. An earlier version of this paper has been commented on by two other reviewers. They made important points about the need to clarify the proposed mechanism. This is my first review of the paper, so I can see that significant improvements have been made, but I retain the concern of the previous reviewers. It is still written quite confusingly in places, and the proposed mechanism is still not really explained in a way that makes sense to me. Having said that, I do appreciate this focus on the difficulties of dating Alpine cores, and I also acknowledge the strength of using the evidence of missing layers along with the evidence of enhanced 210Pb to try and find an explanation. I think the paper can be published but would benefit from one further round of changes. It will still be a bit unconvincing and speculative but if well-explained, this paper can form the basis for a better appreciation of what is needed to use and date alpine cores well.
Specific comments (both detailed and general)

Line 40 “entirely cold”. What does this mean. Do you mean it’s polar and not temperate, ie has no meltwater? If so please spell this out.

Thanks for this comment. Yes, you are right cold meant here that there is no melting influence from the upper into deeper ice layers.

We updated the text as following (line 39):
… Although it has experienced significant warming in response to climate change since the 1980s (Vincent et al., 2007; Gilbert and Vincent, 2013; Vincent et al., 2020), the glacier has been shown to be entirely cold (i.e. the ice temperature is below freezing point at all depths), with the exception of sporadic surface melting and refreezing in the uppermost centimeters during summer……

Line 50 and onwards. The entire paper rests on us believing that it is possible to get a really good age scale from annual layer counting if the stratigraphy is continuous and that this is achieved for C10. This is asserted with reference particularly to Preunkert et al (2000). However, the casual reader is going to see the nitrate profiles shown in Figure 3 (C10 panel) and really wonder if this is possible. I appreciate that dating was mainly done with ammonium (which makes me wonder why this is not shown). Perhaps to reassure readers less familiar with the previous work the authors could include a plot similar to Fig 7 and 8 from Preunkert (2000) in the supplement, to really pin down the reliability of the C10 dating. This might be referred to at line 192 where the assertion hat C10 is well-dated is most clearly made.

Thanks for this comment.
The present study relyies on well-established work and scientific results obtained and published over the last 25 years. Therefore, we might have passed a bit too quickly over the dating of C10 for readers who are not familiar with the glaciochemistry of alpine ice cores, in writing that the “dating was achieved on the base of ammonia profile” what is a bit oversimplified since NH4 was not the only available tool to date.
On the other hand, we cannot discuss all the already established features in the C10 and CDK cores but need to reference existing publications for this. The dating on the base of ionic profiles, and hereby especially NH4 due to its stronger seasonal variations is

known in literature and is also used for ice cores from other high alpine sites such as Colle Gnifetti (Eichler et at. 2023), Fiescherhorn (Schwikowski,et al., 1999; Jenk et al., 2014), Grenzgletscher (Eichler et al., 2000).

The publication in which the dating of C10 is detailed (Preunkert et al., 2000) is on open access, and the interested reader can quickly discover the glaciochemical setting of the drill site, the detailed (at the top Figure 3 and 5 and further down Figure 7) and mean (Figure 4) seasonality of ions and deuterium as well as the depth age relationship (Figure 8) established in this epoch. However, to convince the reader unfamiliar with these older publications of the age scale we added another figure in this round of revisions. As update of this original depth age relation achieved in 2000, in view of additional absolute time markers which confirmed the dating, and to document the recent update made on the depth-age relation over the time period before 1935 (even if that period is not in the focus of the present study), we added a figure in the introduction of the manuscript reporting the actual depth-age relationship of C10. Along with the annual resolution, absolute time markers identified in C10 are assigned including 6 horizons situated between 1986 and 1930 with maximal distances of 15 years between each other.

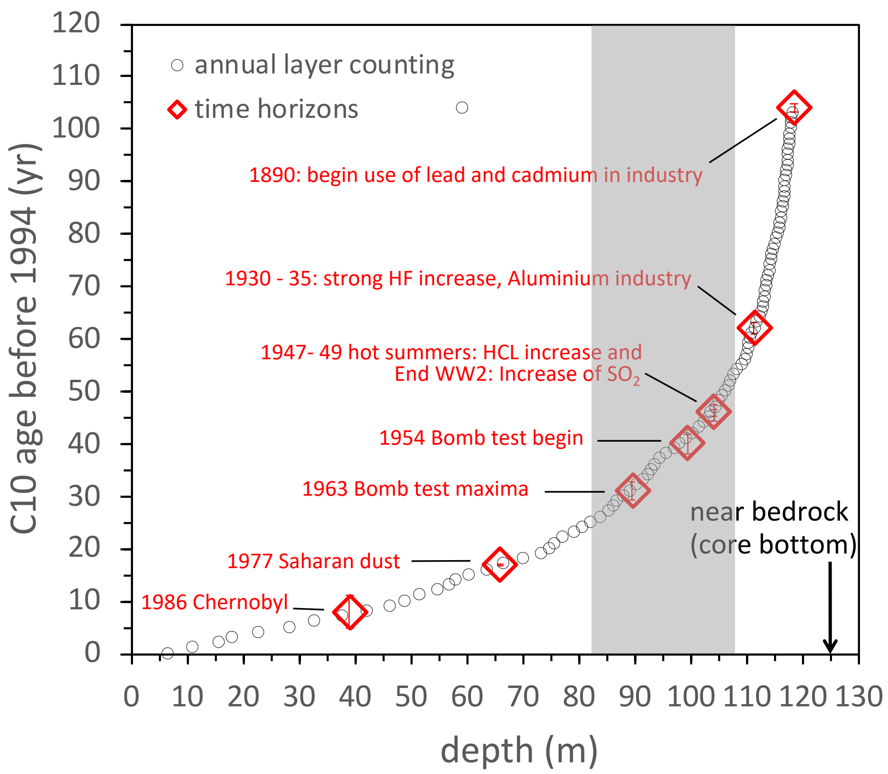

Figure 1: Depth-age relation established for C10 between 2000 and 2016. Data are from Preunkert et al., 2000, Vincent et al. 1997, Preunkert et al., 2001a, Preunkert et al., 2001b, Legrand et al., 2002, Legrand et al., 2018 and Preunkert et al., 2019a and the age scale is based on annual layer counting and absolute age markers. The shaded area refers to the depth zone where enhanced ^{210}Pb values (see Sect. 3.2 and Fig. 6) were observed.

In addition, we added in Section 3.1 (line 251) more details about the seasonal variations of NH4, NO3 and SO4 to emphasize that there are, despite their different amplitudes, coherent in timing:

Based on the well-marked seasonality in the chemical stratigraphy for all cores, annual layer counting was used as the main dating tool over the time period of interest of this study, i.e., back to the 1950s. NH_4^+ has a very strong seasonal variation (factor of ~14 higher in summer than in winter) caused by the parallel seasonal changes in source strengths and vertical transport of NH_4^+ (Preunkert et al., 2000). However also other ions (such as nitrate and sulfate) show clear seasonal variations (factor of ~4 higher in summer than in winter). The annual layer counting, which was based mainly on ammonia, was reinforced by absolute time markers such as Saharan dust events (for example the prominent event in 1977) (Preunkert et al., 2000 for C10; Legrand et al., 2013 for CDK, Legrand et al., 2018 and this study for CDM) and radiometric analyses aimed at detecting fallout from atmospheric thermonuclear bomb testing via ^3H (Legrand et al., 2013 for CDK and this study for CDM) and ^{137}Cs (Vincent et al., 1997) for C10.

Sections 2 and 3. I found the structure, where some of the methods are folded into Section 2, very offputting. The authors are discussing the site characteristics and presenting a first summary of what they think might be happening (as requested by the editor), and then in a sudden gear change at line 112 they start discussing the analytical methods. I would strongly recommend splitting Section 2 into 2 main Sections: "Site characteristics", and "Ice core analysis". The analysis discussion could also be a bit more structured, perhaps even with a table, as I found the mass of different instruments and labs to be overwhelming as I read it. Section 3 is then simply two parts, "Dating" and "210Pb", not "Dating and Methods".

Thanks for this comment. We agree totally with you and split Section 2 in two parts. 2.1 Site characteristics and 2.2 Ice core analysis. Concerning the ice core analysis, we arranged the analysis now by parameters and not by lab as before. That should make this Section more readable. In addition, we added a table (Table 2) which reports analytical methods applied for samples used within the present study. Following your recommendations, the header of Section 3 was changed to "Dating and ^{210}Pb data".

Table 2 reads:

Table 2: Analytical methods of ice core analysis used for the present study

Core name	C10	CDK	CDM
^{210}Pb	gamma-spectrometry (IGE) (two samples, this study; all others Vincent et al., 1997)	alpha-spectrometry (IUP, Legrand et al., 2013)	alpha-spectrometry (IUP, this study)
^{137}Cs	gamma-spectrometry (IGE, Vincent et al., 1997)	-	-
^3H	-	gas counting (IUP, Legrand et al., 2013))	gas counting (IUP, this study) / liquid scintillation (CEP, this study)
NO_3^- and NH_4^+	ion chromatography (Preunkert et al., 2003; Fagerli et al., 2007)-	ion chromatography (Legrand et al., 2013)-	ion chromatography (IGE, Eichler et al., 2023) / continuous flow analyses (DRI, Legrand et al., 2018; CEP, this study)

Table 1 is useful. The bracketed value for CDM 3H maximum is not explained – I assume it is the two alternatives.
Thanks for this comment. Yes, you are right, the value in parenthesis assigned the depth of the 3H peak which is considered as not corresponding to year 1963. We updated Table 1 with a footnote that reads:
[a] Depth of shallower ^{3}H maximum detected in CDM, considered as not corresponding to the year 1963 (see Section 3.1.3)

Line 222. This analytical detail should be with the earlier discussion of methods.
Thanks. The respective sentence was moved to Section 2.2.

Line 263 – this was very confusing. You refer to the "second peak (89.5 to 96 m depth)" but the two peaks are at 88 and 93 m. The values you discuss here of 10-40 TU are also not the peak values. Please reword this to explain exactly what you mean, clarifying what you mean by the "second peak", and which time period and depth you are comparing the 10-40 TU values to.
Similarly line 279 "If the first 3H peak" - which is that – the one encountered at shallower depth or the one that was deposited earlier (ie deeper)? Please review all text from lines 263 to 283 to ensure you are clear about what you are comparing. I am sure this can be expressed more clearly so the reader can follow the different options.

Thanks for this comment. Indeed, the wording was not very clear.
In line 263 of the previous version of the manuscript we wrote "around the second peak (89.5 to 96 m depth)" and meant the ice layers above and below the deeper 3H peak. We clarified this now in the text (this line (line 355) reads now:
This is consistent with the observed value in CDM ice above and below the deeper ^{3}H peak (which is around 93.5 m depth).
and also changed the wording from "first" and "second" to "shallower" and "deeper", respectively in the complete paragraph.

Line 418 to 423. This is confusing. Surely the (partial) opening of the crevasse would most simply lead to a reduced buildup of 222Rn concentrations diffusing into the firn, and could therefore give any reduction you want (right down to background levels if completely open). I don't see why you need (even if you have) disturbed layers to get this reduction.

Thanks for your comment. You are right our conclusion was not entirely conclusive. We cancelled this argument and changed this part in Section 4 as following (line 581):
… A partial opening of the crevasse to the atmosphere would allow the bedrock-derived ^{222}Rn in the crevasse to mix with the much lower atmospheric ^{222}Rn concentrations (Pourchet et al., 2000). This would have led to a strong reduction of additional ^{222}Rn accumulation and ^{210}Pb production in the crevasse and in the snow and firn around the crevasse, starting from the moment of the opening to the atmosphere. In addition, disturbed isochrones also could lead potentially to decreased ^{210}Pb inventories since ice layers from the upstream side of the

crevasse with enriched ^{210}Pb activities could be missing, as suggested for CDK but not CDM.

Overall explanation. I think the authors have the elements of a solution but somehow the way they explain things doesn't quite work for me. I think this may be partly because neither I or they seem to be thinking in 3 dimensions. The problem for me is related to what they see as the character of the crevasse, because the question is how does ice get through it at all. If the crevasse only reaches partway through the depth of the ice sheet then presumably it flows with the glacier and would have reached the drill site by now. So the way the authors have drawn it with it open to the bed seems like the right way, but in that case no ice flows past it (assuming it is permanently open). So for any ice to reach our site it must be flowing round the crevasse and coming in at an angle to the flow line, and surely this is where the stratigraphy can get disturbed by taking a much longer flow path at some times than others. (I am not an ice dynamicist so perhaps this is wrong, but I would find it more convincing than what is written in lines 405-412 which seems to require ice to cross the crevasse).

Thanks for this remark. Our view of the functioning of the crevasse is now explained in detail in Section 2 (now Section 2.1, line 91):
In fact, field observations and photographic evidence shows the existence of a large crevasse (clearly visible by a depression at the surface although the crevasse is not necessarily open to the atmosphere) east of the CDD dome which, dependent on its north-south extension, could also intersect the upstream flow line from the drill site. Unfortunately, we do not have direct measurements of the depth and lateral extent of the crevasse. The crevasse appears approximately at an oversteepening of the bedrock topography (Fig. 2c) along the flow line, suggesting that it is extensive stress at the bottom that leads to crack formation at a specific point and allows for opening of a deep crevasse down to bedrock (in line with ^{210}Pb evidence as outlined below). We stress that the crevasse is not necessarily open to the surface (see Figure 2 a and b), but that collapse of the snow bridge at the top in the past cannot be ruled out. We also note that this crevasse is not moving downhill with the surface velocity of several meters per year in the observations but is found approximately at the same location of the glacier surface every year. Despite this stationarity of the crevasse, the surface velocity field is not disturbed (Gilbert et al. 2014) implying that the ice flow is not totally interrupted across the crevasse. Together with the stationarity of the crevasse, this suggests that the subsurface void created by the crevasse is filled again by glacier flow after its opening (as also suggested by significant glacier thickness reductions of a few meters from 1993 to 2017 (Vincent et al. 2020) in the vicinity of the crevasse). Accordingly, we interpret the glaciological evidence as (recurrent) opening but also potential re-closure of the crevasse (Colgan et al., 2016) below the surface at the same bedrock topography-induced position. Comparing photos taken in 2012 (Fig. 2a) and in 1999 (Fig.2b), shows widening and northward extension of the crevasse from 1999 to 2012. Whereas in 2012 the crevasse is clearly visible as a snow-covered depression on the surface slope, the crevasse appeared to be limited to the southwestern flank of the drill site catchment area in 1999. Following Fig. 3, the crevasse is situated more than 100 m upstream of the drill site of C10, CDK, and CDM. Figure 3a shows the CDD glacier thickness changes between 1993 and 2017 overlayed with the modelled flow line indicating the calculated arrival depths at the drill site of C10, CDK, and CDM (Gilbert et al., 2014). Figure 3b and c represent vertical cross sections along the modelled flow line in Fig. 3a overlayed by simplified sketches of the upstream crevasse visible in Fig. 2. We sketch the crevasse in two hypothesized temporal states, as concluded in Sect.

4 on the basis of C10, CDK and CDM ice core data presented in Sect. 3. Table 1 summarizes the main characteristics of the three ice cores and basic findings related to radiometric analyses.

I do also get the argument about the 222Rn penetrating the firn, but some discussion of distance scales would be helpful here. 222Rn has a halflife of 3.8 days. I agree this means it can build up under a snow bridge, but it also surely implies that it can only penetrate horizontally into the firn by a few metres. This is then challenging for C10 where the authors require that the ice has not seen the crevasse. I would rather think that it must have seen the crevasse. I am then not sure how to explain the lack of flow disturbance. I don't have a solution but I think this should be discussed.

Thanks for your comment and thoughts. The fact that the crevasse has crossed the flowline already of C10 is evoked in the manuscript. in Section 4 (line 564):
Although speculative, we assume that the upstream crevasse of Fig. 2 and 3 already existed earlier in the 1970s and was capped at the top by the snow bridge. If the crevasse (1) did not intersect the upstream flow line at the time the ^{210}Pb anomaly was imprinted in the firn reaching C10 from the crevasse in 1994 (implying that the flow line was close enough to the crevasse for ^{222}Rn to diffuse to the flow line) or (2) did intersect the catchment area of the C10 drill site at that time but was so narrow that the chronology of the C10 ice core was not significantly disturbed before closing again, …

Just before this we added a new paragraph in which a rough estimate is made concerning the distance over which the 222Rn charged air of the crevasse would influence the firn and the order of contact time needed to produce the ^{210}Pb levels found at the drillsite.

It reads as follows in Section 4 (line 533):
Pourchet et al. (2000) observed mean ^{222}Rn activities of ~10,000 up to nearly 150,000 Bq m^{-3} in the firn of Mont Blanc (2 km from Dome de Gouter with the same rock mineralogy) and 0.7 Bq kg^{-1} of ^{210}Pb on average in a depth of 0.5 m in the annual snow/firn layer of the measurement year, both of which are orders of magnitude higher than typical background values in air or snow. They attributed these high levels to Rn emanation from a nearby crevasse. Vincent reported as much as 28 Bq kg^{-1} in a firn/ice core at the summit of Dome de Gouter which had contact with a subsurface crevasse. Since snow accumulation and ice flow velocities at the Dome de Gouter are lower than at the Mont Blanc summit, we assume that the firn air of the core drilled at Dome de Gouter was in much longer contact with the crevasse than the surface snow layer at Mont Blanc summit.

In the following we make a rough estimate whether such ^{222}Rn activities are sufficient to explain the ^{210}Pb anomaly in our cores. To keep the estimation simple, we assume a temporal constant ^{222}Rn activity in the crevasse of 50,000 Bq m^{-3}, which lies in the typical range observed by Pourchet et al 2000 and is equivalent to a ^{222}Rn number concentration of 2.4 10^{10} m^{-3} in the crevasse. As the half life of ^{210}Pb is much longer than that of ^{222}Rn we can assume that the amount of ^{210}Pb loaded into the firn is controlled primarily by the total ^{222}Rn entering the firn. We assume that ^{222}Rn loads the adjacent firn by diffusion and that after ten ^{222}Rn half lives the radiogenic ^{222}Rn entering the firn has essentially decayed to zero limiting its entrainment length to a few meters (see diffusion length discussion above). As a *first order estimate we assume a linear concentration gradient between the ^{222}Rn concentration in the crevasse and zero radiogenic ^{222}Rn in a distance of a diffusion length after ten ^{222}Rn half lives. We acknowledge that the true concentration gradient is not linear and that some ^{222}Rn atoms will enter deeper into the firn than this diffusion length, but to obtain the order of magnitude loading of the firn with ^{210}Pb this back-of-the-envelope calculation seems justified. Using the ^{222}Rn concentration in the crevasse as a measure of the concentration gradient driving the diffusive flux, this leads to a diffusive ^{222}Rn atom flux into the firn of 12,000 and 40,000 $m^{-2}\,s^{-1}$ for a firn diffusivity of 0.1 10^{-5} $m^{-2}\,s^{-1}$ and 1 10^{-5} $m^{-2}\,s^{-1}$, respectively. At an exposure length of the bedrock of one year and a firn density of 500 kg m^{-3}, this would result in a loading of the firn by approximately 800 to 2,500 10^{6} ^{210}Pb atoms kg^{-1}, equivalent to an initial ^{210}Pb activity on the order of 800 to 2500 mBq kg^{-1}. Given that the ice flows within 1 to 2 ^{210}Pb half lives from the crevasse to the ice core drill site, these numbers are of the same order of magnitude as the activities measured in the firn core. We note that an exposure time of the crevasse of one year may be at the upper limit of what is possible (given the stationary position of the crevasse which requires healing of the crevasse before ice flow has moved its position significantly) and an exposure time of only one month may not be sufficient to explain the measured activities. On the other hand, we only based our estimate on diffusive entrainment of ^{222}Rn into the firn. If there also are pressure differences between the crevasse air and the firn air (for example induced by synoptic pressure variations at the surface) this would also lead to an advective flux into the firn which may increase the initial ^{210}Pb activity after ^{222}Rn loading of the firn. In summary, while a more precise estimate would require stringent firn transport modeling in and around the crevasse which is beyond the scope of this paper, the overall order of magnitude of the ^{210}Pb anomaly can be explained by our simple estimate.*

Additional literature not referenced in the manuscript:

Schwikowski, Margit & Brütsch, S. & Gaeggeler, Heinz & Schotterer, Ulrich. (1999). A high-resolution air chemistry record from an Alpine ice core: Fiescherhorn glacier, Swiss Alps. Journal of Geophysical Research. 1041. 13709-13720. 10.1029/1998JD100112.

Jenk, Theo & Szidat, S. & Schwikowski, Margit & Gaeggeler, Heinz & Brütsch, S. & Wacker, L. & Synal, H.-A & Saurer, Matthias. (2006). Radiocarbon analysis in an Alpine ice core: Record of anthropogenic and biogenic contributions to carbonaceous aerosols in the past (1650-1940). Atmospheric Chemistry and Physics. 6. 10.5194/acpd-6-5905-2006.

SchottererU.,StichlerW.,GrafW.BürkiH-U.,Gourcy L., Ginot P. and Huber T. (1998), Stable isotopes in Alpine ice cores: Do they record climate variability, Techniques in the Study of Environmental Change, IAEA.

IAEA/WMO (2006). Global Network of Isotopes in Precipita- tion. *The GNIP Database* Accessible at: http://isohis.iaea.org, retrieved 11/2008