Author Response to Referees 1 and 2

Referee comments are in black
Author response in red

Review of „Effective diffusivity of sulfuric acid in Antarctic ice cores" submitted to Climate of the Past by Fudge et al.

The diffusion of impurities within the ice matrix is an important mechanism that can lead to post-depositional change. These changes need to be recognized and constrained for proper interpretation of the climatic signals in the ice core impurity records. Sulfuric acid peaks related to volcanic eruptions are a prime example in this regard. In their manuscript Fudge et al. investigate the effective diffusivity of sulfuric acid along the full depth range in the EPICA Dome C ice core. In doing so they extend a previous foundational approach by Barnes et al. (2003) treating the Holocene sections (in the following, like in the manuscript, referred to as B03). The authors find an average value comprised of values for glacial and interglacial period that is an order of magnitude lower than the Holocene value by B03. This would suggest that records related to sulfuric acid are preserved longer and to greater depth, which is of relevance to the interpretation of existing and future ice core records. Although the manuscript is generally well written, at some central points it needs more work and clarification. At present the main result is difficult to appreciate without in-depth knowledge on the subject or reading the B03 paper first. It is especially important for the reader to understand where the discrepancy with the previous estimate by B03 stems from.

We thank the referee for their constructive review. As we will describe in more detailed responses below, our inference of the effective diffusivity in the Holocene agrees well with B03 (within 15%). Therefore, the lower effective diffusivity that we find when looking at multiple interglacial and glacial periods is the result of analyzing more data that spans a longer timeframe. The smaller effective diffusivities we find could differ from the Holocene values for two main reasons:

1) Processes may operate near the surface of the ice sheet, i.e. in the firn, that do not affect the record at greater depths. So the higher Holocene effective diffusivity may be accurate, but not appropriate to apply for the majority of the ice sheet depth.
2) The Holocene effective diffusivities may be inaccurate because of the challenge of noisy data and little time for diffusion to operate. We analyzed the Last Interglacial separately, and found that the mean scaled gradients became larger with age, suggesting inferences on short time periods may not be accurate.

We suggest that the most likely scenario is that the Holocene data is not sufficient to estimate the effective diffusivity, but we cannot exclude that different processes are operating in the Holocene. Regardless, the lower effective diffusivity values we find are more appropriate on orbital timescales.

We will add a section that more clearly compares with B03 in the Holocene and articulates these points more clearly.

Below we provide detailed responses.

General comments:

The difference with the previous estimate of the effective diffusion reported in B03 is a major result here, but comparatively little is said about how the authors explain this difference. Is this a result of differences in the method, the data, or lies within the range of uncertainty so the values actually agree (more regarding uncertainty below).

We will add additional text to address this question. We will add a new section at the start of results where we directly compare our Holocene results with B03. It will have this paragraph:

We first calculate the effective diffusivity in the Holocene to directly compare with B03. We find an effective diffusivity of 5.4×10^{-8} m^2 a^{-1}, approximately 15% greater than the B03 value of 4.7×10^{-8} m^2 a^{-1}. Like the small difference in the c_0 value, the larger effective diffusivity is likely due to revisions in the sulfate data set and thinning function. As shown below, a 15% difference is small compared to the difference in the inferred effective diffusivity when using data older than the Holocene.

We will also add a new first section to the discussion where we discuss the possible reasons for why Holocene effective diffusivity (B03 or ours) may differ from the estimates over longer timescales. As discussed above, we suspect the difference is primarily caused by trying to infer the effective diffusivity from a short time period with noisy data; however, we cannot exclude that processes in the near surface, i.e. firn, might result in a larger effective diffusivity.

In section 3.1 it is mentioned that the approach employed here was also tested for the Holocene considered in B03. In line 195 values for the residual concentration co are compared. The value reported by B03 is (0.54 +/- 0.04) (mM in the text and µM in the caption of Figure 4 in B03). What is the uncertainty for the new value reported here, co = 0.62?

Our c0 value is 0.62±0.06, which overlaps with the uncertainty of the B03 value. This will be added in section 3.1

It is stated that the small difference could stem from updates in the sulfate data and thinning function but this is not shown, e.g. by using the original B03 data.

I do not have the original B03 data. As articulated above in this response, we get a similar effective diffusivity for the Holocene, so the difference when using the full data is not sensitive to the small differences with the original B03 values.

If the values for the residual concentration and the peak width agree, what is the according value for the Holocene for the effective diffusion (Deff)? Is this different from B03 and if so, why?

We find an effective diffusivity of 5.4 x 10^{-8} m^2 a^{-1}, which is 15% greater than B03. If we use the B03 c0 value, we find an effective diffusivity of 5.3 x 10^{-8} m^2 a^{-1}. The difference in effective diffusivity with B03 is not driven by the difference in initial values, but instead by the difference in the calculation of the mean gradient. We suspect this is because the final data set is more complete. B03 note that "Points are removed when more than 65% of expected data are missing from a 10 m section" in the caption of Figure 5. This data set is missing a maximum of 12% of expected data points in any 10m section.

If I understand Table 1 correctly, the reported Deff is the value for the last two interglacials combined?

The Deff values in Table 1 use the median Holocene and median LIG mean gradients to calculate a single Deff value. The title will be revised to make this clearer. We do not calculate the effective diffusivity for each period because of the uncertainties in using short temporal durations and because we are interested in the effective diffusivity for longer (i.e. orbital) timescales

The more important implication of this is the following: If the values by B03 were an overestimation, it is important to understand where this is coming from. If only slightly other data and thinning functions were used as input, this would indicate that the calculations are very sensitive to such changes.

As discussed in response above, the calculations are insensitive to the new data and thinning function. The effective diffusivity for glacial-interglacial cycles is approximately an order of magnitude less than either the B03 or our Holocene estimate of the effective diffusivity.

The same argument holds for the case of slight differences in how Deff is calculated. I would have expected consistent values for the Holocene with B03, which would then have allowed to discuss why the values for previous interglacial and glacial periods are different by an order of magnitude, possibly indicating changes in the physical mechanism. I may have misunderstood this point, but strongly believe this issue should be addressed more clearly, thus helping the reader to appreciate the difference in Deff found in this study.

We will add new text to address this specifically. We find consistent values for the Holocene effective diffusivity with B03. While it's possible that the Holocene effective diffusivity is greater than the effective diffusivities averaged over glacial cycles, we note that the uncertainty of effective diffusivity estimates on data spanning only 10 ka are high because of complicating influences such as small variations at deposition.

The treatment of uncertainties for Deff needs to be clarified. In section 3.1 it is mentioned that the uncertainty results from the 95% confidence intervals in a fit to the individual data points (log of scaled mean gradient), while Deff is derived from a fit to the medians. What is the difference in values if you calculate Deff as a fit to the individual datapoints?

The unadjusted values show a small decline when using all of the data points instead of the medians, while the adjusted values decline a bit more. We prefer using the medians of the scaled gradients because periods with lots of volcanic activity can result in abnormally high mean gradients, which in turn causes the inferred effective diffusivity to be smaller.

		Unadjusted	Adjusted
Interglacial	Medians	5.9	3.4
Interglacial	All	5.5	2.4
Glacial	Medians	4.8	1.9
Glacial	All	4.7	1.4

What justifies regarding the two low values (notably for a glacial and an interglacial) as outliers?

The two low values are the two that fall outside of one standard deviation from the mean of the 12 estimates.

The differences in the two methods indicate substantial dependency on the way these values are derived, correct? This would be easier to assess if the values for the volcanic width method also had some uncertainty estimation.

We may not be understanding this comment correctly. We find that the good agreement of the effective diffusivity calculated with volcanic widths to those calculated with the scaled mean gradient indicates that there is NOT a substantial dependency on the way the values are calculated. Both calculations give similar results, and importantly are an order of magnitude less than the Holocene (B03) effective diffusivity.

My confusion about the uncertainty treatment translates into the following important question: After having treated glacials and interglacials separately, the authors suggest a general value of $(5+/-2) \times 10^{-9}$ m^2 a^-1. It is unclear to me where this uncertainty value comes from.

The uncertainty was not well described. It was from 2 standard deviations of the 10 non-outliner estimates. We will change this to 3x10-9, which is 2 standard deviations of all 12 estimates.

More importantly, this value is interpreted as representative for both, glacials and interglacials – implying that within the uncertainty of the method, potential differences in Deff cannot be recognized?

Correct. There likely are differences in the effective diffusivity between the climate states – glacial maximums appear to have a slightly lower effective diffusivity – but they are small compared to uncertainty.

I am not sure if I am following, however considering the differences in climatic states, impurity concentration, grain size and temperature this seems surprising and deserves further discussion. Ultimately the physical mechanism remains unclear, and as a reader I had hoped that the results

presented here would give more insight. Since the localization of sulfuric acid in the ice matrix remains unknown, it would have been beneficial for this discussion to include, at least partially, a similar analysis for sodium – like in the B03 paper. In the case of sodium growing evidence suggests that it might be predominantly located at the grain boundaries. Including at least exemplarily sodium could also provide an additional route for comparison with the results of B03. Would be very interesting if a similar discrepancy prevails.

We agree that including sodium would be interesting and it is something we are considering for future work. However, we believe it is outside the scope of the current manuscript. Sulfate was selected because of the volcanic forcing creates variability that is somewhat independent of climate forcing – at least within the extreme climate states of interglacials and glacial maximums. In this work, we wanted to establish that the diffusivity estimates were consistent whether using the scaled mean gradient method, or the width broadening of volcanic peaks. With this work showing agreement of the methods, we have provided a basis for further investigation using the scaled mean gradient for sodium which do not have the volcanic variations for an alternate approach.

Specific comments:

I suggest a better introduction of what the parameter Deff actually means earlier in the manuscript, as some readers may not be familiar with it. Maybe a simple sketch figure could help, also to illustrate the basics of the two methods.

We have drafted a sketch figure to illustrate the two methods. This will be further developed in the revised manuscript.
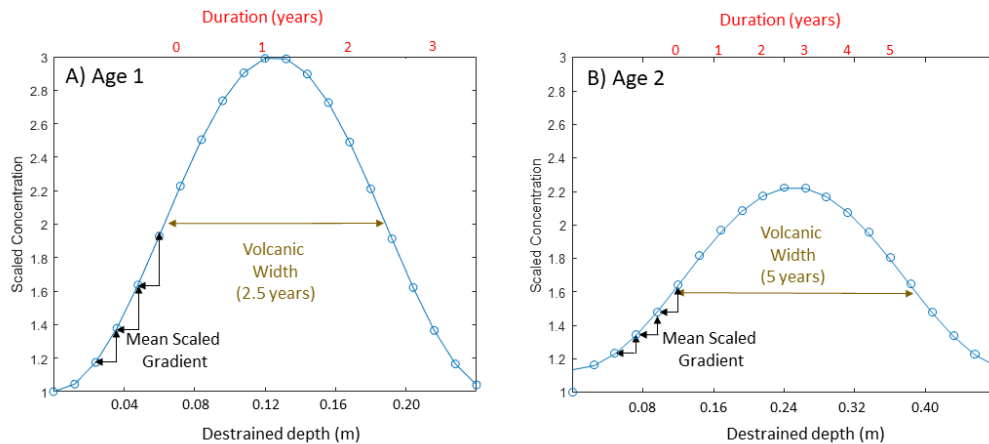

Figure 1: Example volcanic event at two different ages. Note that x-axis is unstrained depth and is a different scale in A) than B). A) shows a volcanic event at a young age and B) at an older age.

The same applies to the method used in Fudge et al. (2016), a bit additional detail would help.

We will add additional detail. A draft of a new paragraph is:

The effect of diffusion is modeled with a one-dimensional numerical model (e.g. Eq. 4). The model, described in Fudge et al. (2016), evolves an initial peak through time based on the effective diffusivity and the vertical thinning. At each time step, the diffusion is calculated first, and then the ice is thinned. The amount of thinning at each time step is calculated from the thinning function (Veres et al., 2013; Bazin et al., 2013). The ice temperature and effective diffusivity can be varied at each time step, although we only show results using a constant effective diffusivity here. We model the evolution of a gaussian peak. The initial width of the peak is determined by the sulfate data in the Holocene (0.128 m) and LGM (0.135 m). The initial thicknesses are similar but, because the accumulation rate in the LGM is a factor of 2 lower, the duration of the events are twice as long in the LGM. The amplitude of the peak is three times the background concentration although in practice the amplitude is not critical because we calculate the relative change. The peak width is compute as the width at the half-maximum amplitude, the same method as used on the sulfate data.

I am not sure if "artificial diffusion" is a good term. The point is that decreasing sampling resolution introduces additional smoothing to the data. Maybe "bias by artificial smoothing" would be better?

Thank you for the suggestion. We noticed that this aligned better with the language in the paragraph. We will make this change throughout the document.

The results by Traversi et al. (2009) suggested that, in the deep ice, there is distinct lateral variability of sulfate, and this raises some question regarding the 1D approach used here – would we expect systematic bias in the estimation of Deff in this case? Could be worth mentioning.

We agree that the loss of lateral homogeneity is important and is part of the rearrangement of impurities that occurs. We will add this to the discussion in 4.1.

Line 109: is z not the unthinned ice equivalent depth as in B03? Please clarify.

Yes. Thanks for the catch. We will specify this.

Line 117: "k is the wave number" – of what? This is a bit abrupt and is not clear until line 127.

We will add a pointer to Eq. 7 which describes the wave number.

Line 149: Why do you choose the largest 25 events? How sensitive are your results regarding changing this value? What if you use the largest 50, 100 or all?

The results are minimally changed using the 50 largest events. The interglacial effective diffusivity value becomes 2.0×10^{-9} m^2 a^{-1} compared to 1.6×10^{-9} m^2 a^{-1} and the glacial effective diffusivity value becomes 3.8×10^{-9} m^2 a^{-1} compared to 4.0×10^{-9} m^2 a^{-1}.

Line 278: Would the neutralization not affect also sulfuric acid?

ECM measures the H+ ions able to carrier a charge. When there are lots of particles (dust or tephra), these H+ ions have reacted and are no longer free. The sulfate is largely unaffected.

Line 295: "The scaled mean gradients have not been corrected for artificial smoothing" – why not?

We will change the sentence to: The scaled mean gradients have not been corrected for artificial smoothing to ensure the increase is due to the measurements and not the correction.

I am not sure how Climate of the Past is handling data availability and if "available upon request" is accepted.

We will post the remainder of the sulfate data upon acceptance.

Technical comments:

Line 95: "is uses"

fixed

Line 240: "increses"

fixed

Line 403: "B03)"

fixed

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Review of manuscript egusphere-2022-1219 by Fudge et al.

General comments:

The manuscript presents estimates of sulfate diffusivity mostly centred on the EPICA Dome C ice core. Two mathematical methods are used and lead to fairly consistent results. Such diffusivity estimates are important to better understand and constrain long term signal preservation in ice cores.

We thank the referee for their constructive comments and provide detailed responses below.

In my opinion, some implicit assumptions should be clarified and better discussed, especially the chemical interactions of sulfate (or sulfuric acid) with other impurities and possible role of non diffusive processes on the apparent effective diffusivity. It should help building a more in-depth discussion of uncertainties, the effects of temperature and differences between ice formed during interglacial or glacial periods, and differences between ice cores. Suggestions are provided below.

The manuscript title and most parts of the discussion assume that all sulfate is in the form of sulfuric acid. Although the fact that part of the sulfate could be in the form of salt micro-inclusions is briefly mentioned lines 57-58, it is re-formulated immediately after in terms of where sulfuric acid is located. The chemical form of sulfate and its relation with sulfate mobility in ice should be better introduced (as for example in Ng et al. 2021 and references cited) and discussed.

This is a good point. We will add justification for our assumption that the sulfate record is dominated by sulfuric acid both in general (Legrand 1995) and specifically at EDC (Figure 2 of B03). We note that in Greenland, the glacial values may not be dominated by an acidic source as evidenced by the drop in conductivity as measured by ECM. However, in Antarctica, there is no similar drop conductivity and thus acidity in glacial ice. We chose to test both interglacial and glacial periods to help address this question of whether the form of sulfate would affect the effective diffusivity. The small difference in the inferred effective diffusivities supports that there is not a significant change in the form of the sulfate.

The manuscript uses a method by Barnes et al., 2003 on the same ice core but different time periods. Diffusivity values one order of magnitude lower than in Barnes et al. (2003) are obtained but no clear explanation is provided, although an attempt to replicate the calculation by Barnes et al. (2003) could be performed.

We agree that not including a direct comparison in the manuscript has created confusion and we will add a new section to the results. As discussed in response to referee 1, we find an effective diffusivity of 5.4×10^{-8} m^2 a^{-1} in the Holocene, which is only 15% greater than B03. We suspect this slight difference is because the final data set is more complete. B03 note that "Points are removed when more than 65% of expected data are missing from a 10 m section" in the caption of Figure 5. Our data set is missing a maximum of 12% of expected data points in any 10m section.

Barnes et al. (2003) restricted their study to the top 350m of the core which has undergone only weak temperature variations after the dampening of the seasonal variability (in the top ~20m), weak progressive thinning due to ice flow and weak background (non volcanic) chemical composition change. The implicit assumptions made by extending the Barnes et al. (2003) calculation to much longer periods should be better introduced and discussed. Numerical tests performed with the diffusion model presented in Section 2.3 could be helpful. For example, the manuscript analyses ice deposited during several glacial and interglacial periods which have

experienced ice temperatures progressively rising from -55°C (or less in glacial periods) to -10°C or more. Diffusion speed and diffusivity are generally very sensitive to temperature, as could be estimated for example by the equation below Figure 4 in Fudge et al. (2016), and the much higher diffusivity inferred from the WAISD ice core (2.2×10^{-8}) by Fudge et al. (2016) than from EDC or Dome Fuji in the manuscript (about 5×10^{-9}). The role of temperature and temperature gradients on diffusive and non diffusive processes should be better introduced and discussed.

We agree that one of the surprising findings is that the effective diffusivity is not more strongly temperature dependent, at least until the ice becomes quite warm (e.g. -10°C). We discuss this in section "4.1 Relation with temperature and grain size" where we show the temperature profile. This could be because the temperature gradient (Figure 6) is relatively constant. It's only about a factor of 2 greater at the bed than the surface. We have chosen to highlight the change in mobility that seems to occur at about -10C, or possibly a bit higher following Wolff et al., 2023, and coincides with the onset of larger grains because this seems to be of greatest importance to the preservation of climate records. We agree that the lack of temperature sensitivity is interesting and we've tried to address this with other ice cores records, but the differences in the climate of deposition seem to dominate compared to the effects of different temperatures and temperature gradients among cores.

We will add additional discussion to section 4.1 highlighting that the temperature does not play a dominant role, and that it may be more important for altering the sulfate signal as compared to diffusing it.

Analysing the diffusivity values obtained for each of the five investigated glacial cycles (which could be provided in Appendix A) could also help improving the discussion of uncertainties as well as the roles of temperature, chemical composition of the ice etc.

I'm not sure I understand this comment. I think this stems from confusion about how we have calculated the effective diffusivity. We do not calculate effective diffusivities for each individual period, e.g. the Holocene, as B03 did. Instead, for instance, we calculate the effective diffusivity assuming the Holocene is the starting condition, and the LIG is the ending condition, and we find the effective diffusivity which explains the difference between the two. This will be clarified by beginning the results with a new section that finds the Holocene effective diffusivity to compare to the B03 value. We will then more clearly describe how are technique differs in calculating the effective diffusivity.

Specific comments:

Lines 46-47: the implicit assumption that all sulfate is under the form of sulfuric acid should be clarified and discussed.

As discussed above, we will add more introduction of why we primarily consider sulfate to record sulfuric acid. Legrand (1995) notes that sulfuric acid dominates the present-day ice. In Greenland, the glacial ice has an increase in terrestrial sources of sulfate, but this is much more muted in Antarctic ice. Thus, the EDC record is likely dominated by sulfuric acid

Lines 66-71: the reason why the diffusivity value of 4.7 x 10^{-8} for EDC (Barnes et al. 2003) is viewed as considerably higher than the WAISD value of 2.2 x 10^{-8} and the expected effect of temperature should be clarified.

We will remove "considerably". The lower effective diffusivity at WAIS Divide was unexpected in the Fudge et al. 2016 because we thought the ~20°C warmer temperatures should lead to greater diffusion. As described in the response above, the role of temperature and temperature gradient does not appear to be significant until warm (~-10°C) conditions are reached, thus the factor of 2 difference in effective diffusivities at Dome C and WAIS Divide should not be regarded as considerably higher.

Lines 80-85, 196 and 493: a reference (or references) should be provided for the EDC sulfate record, not only the analytical method. Is it the same as Traversi et al. (2009)? Updates in the sulfate data set are briefly mentioned at line 196 without a reference. I was surprised to find only part of the EDC sulfate record in a public database (down to 2094m, doi:10.25921/kgv8-cn35) and strongly encourage the authors of the manuscript to document and place the full record in a public database.
Yes, Traversi et al., 2009 is the reference and the data will be made publicly available.

Line 86: references should be provided for the data plotted on Figure 1.
The Traversi et al., 2009 reference will be added for the sulfate, and Veres et al. 2013 and Bazin et al., 2013 will be added for the timescale and thinning function.

Lines 100-104: several aspects of the replication and extension to longer periods of the calculation by Barnes et al. (2003) should be further discussed (see also general comments). Is the same diffusivity estimate obtained when replicating the calculation for the same period (Holocene)?

We find a similar value to B03, as discussed above. The results section will be restructured, as described in the response to Referee 1, to better explain the extension of the B03 methodology to longer periods.

Barnes et al. (2003) indicate some assumptions made in their calculation that are not valid on longer time scales (e.g. in their paragraph [16]: constant in time downward velocity in ice and effective diffusivity). They also discuss the scaling parameters in relation with the origins of sulfate (biogenic versus volcanic) in their paragraph [14].

The spreading of volcanic width calculations explicitly include the variation in vertical strain in time such that there is no assumption of constant downward velocity. The scaled mean gradient calculations use the net thinning (i.e. thinning function) which integrate the variations in vertical strain for the time period of the calculation. As to the origin of the sulfate signal – biogenic vs volcanic – this is why we chose two methods to calculate the effective diffusivity. The scaled mean gradient is sensitive to both, whereas the broadening of volcanic peaks is only sensitive to

the volcanic events.

Lines 123-127: this simplistic correction for the final thinning ignores the strong variation with time of the annual layer thickness. A step by step approach is used with the diffusion model (line 151 of the manuscript). The effect of neglecting the progressive thinning could be estimated with the diffusion model, such tests may improve the estimation of the uncertainty.

The thinning function is indeed a cumulative measure of vertical strain. Our goal is to "unstrain" the sulfate record so that we have values on a depth scale that reflects their depths when they are deposited (but after firn campaction), which this method does.

Lines 139-145: the references cited here for the identification of volcanic peaks cover a much shorter time period than the five glacial cycles considered in the manuscript. On the other hand, Fujita et al. (2015) stopped the volcanic synchronization of EDC and DF at 216kyr BP due to the smoothing of the peaks.

Matching volcanic events, with high confidence, between two cores separated by hundreds of kilometers is a more challenging task than identifying the volcanic peaks themselves. Distinct sequences, often of events with differing magnitudes, is needed for a confident match.

Traversi et al. (2009) find artefacts of non volcanic origin below 2800m depth in the EDC core, and the recent study by Wolff et al. (2023) found evidence of this behaviour at shallower depths: 2500m (300ka, see their Data Section and Figure 3). Here and in other part of the manuscript, a discussion of possible artefacts due to sulfate variability of non volcanic origin should be included.

Thank you for mentioning the Wolff et al study. We were not aware of this when we submitted the manuscript. We will include the following about anomalous sulfate peaks:

We primarily consider the median widths for each period to reduce the influence of either exceptional volcanic events or anomalous sulfate peaks. The anomalous peaks begin primary below 2800 m (Traversi et al., 2009), but may begin around 2500m depth (Wolff et al., 2023). The anomalous sulfate peaks are discussed further in section 4.1.

Lines 254-258: Figure 4 seems to suggest that a single diffusivity cannot explain both the smallest observed increase in event durations and the largest observed increase in event durations. The range of event durations should be discussed in terms of uncertainty on the diffusivity, possible role of non diffusive mechanisms and/or change in the nature of detectable events.

It is important to note that we cannot observe the increase of a specific event – we can only observe durations from different time periods. Thus, we expect considerable scatter in the durations as there may have been differences in the original duration of volcanic events. Because lines 254-258 are the figure caption, we do think expanding discussion would help clarity.

Lines 267-272: A better evaluation and discussion of the uncertainty could be made by analysing all individual estimates (for each glacial cycle) of the diffusivity. These values could be provided in Appendix A. I found interesting that the most reliable diffusivity estimates, for the last cycle, all lead to lower values in glacial ice than interglacial ice. This should be further commented in the manuscript (see also next comment on lines 275-278).

We are not quite sure what the referee means by analyzing all individual estimates for each glacial cycle of the diffusity. We think this is the same issue as the referee notes above "Analysing the diffusivity values obtained for each of the five investigated glacial cycles (which could be provided in Appendix A) could also help improving the discussion of uncertainties as well as the roles of temperature, chemical composition of the ice etc." We will reowrd Section 3.1 to make the source of effective diffusivity more clear.

Lines 275-278: the analysis of the Dome Fuji ice core ECM profile deserves more attention in my view. The increased reduction of the diffusivity in glacial ice compared to interglacial ice is attributed to the neutralization of acids where dust is in higher concentration. This process should have been introduced much earlier in the manuscript as it should affect the mobility of sulfate if it does not remain in the form of sulfuric acid (see also general comment and specific comment on lines 46-47). The ECM profile of the EDC ice core is publicly available with 1 cm resolution:

https://www.ncei.noaa.gov/pub/data/paleo/icecore/antarctica/epica_domec/
The Dome Fuji values using ECM are quite similar to the values from Dome C. We will reword this section to better acknowledge the complexity of using ECM as a proxy for sulfate

ECM primarily measures the acidity of the ice, and corresponds well with sulfate when the sulfate is primarily from sulfuric acid, which is the case in Antarctica (LeGrand, 1995). Calculating the scaled mean gradient as for EDC, the inferred effective diffusivity using the Holocene and LIG was 6.6×10^{-9} m^2 a^{-1}, and for the LGM and PGM was 3.2×10^{-9} m^2 a^{-1}. While the Dome Fuji results may be complicated by the different measurement, we are encouraged by the reasonable agreement. This suggests that the inferred effective diffusivity from EDC is applicable to similar East Antarctic sites.

It would be very interesting to check if it confirms the lower diffusivity in glacial ice than interglacial ice and increased difference inferred from ECM than from sulfate.

The Dome Fuji glacial value is about half that of the interglacial value, but both are within the ranges inferred at Dome C.

Lines 282-287 (and 321-323): this section is unclear. Does it just mean that the reduced and more variable diffusivity values obtained for the past 5 cycles are simply attributed to the correction of artificial smoothing without considering other sources of uncertainty? The uncertainty estimate should be clarified.

The uncertainty discussion will be clarified. As described above in response to the other referee, the uncertainty will be based on two standard deviations of the 12 estimates in Table 2.

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Lines 295-296: I do not understand why the scaled mean gradients are not corrected for artificial smoothing for the oldest ice, which should be the most affected due to its high thinning rate. The corrected values should be also provided, possibly on an additional panel of Figure 5.

The scaled mean gradients in Figure 5 have not been adjusted for artificial smoothing to emphasize that the increase in the deep ice is not a result of the adjustment.

lines 317-319: some test results should illustrate this important statement.
We will include a reference to Figure 3 which shows that the scaled mean gradient has become zero by ~100 ka.

Lines 329-330: this should be further discussed in relation with the findings of Wolff et al. (2023) (see comment on lines 139-145) and the neutralization of acids in glacial ice considered at line 278.

We will add that Wolff et al. support that the most recent glacial is unaffected by "sulfate sucking" (their term, not ours), while also adding that impacts may affect higher in the ice column.

Lines 337-338: the diffusivity contrast between interglacial and glacial ice deserves a more detailed analysis (se also comments on lines 267-272 and 275-278).
We will add discussion of why it is surprising that the diffusivity contrast between glacial and interglacial ice is not statistical significant, and also mention how the total impurity concentration may affect post-deposition alteration though.

Lines 338-339: the absence of temperature effect in the range -55°C to -10°C is a very strong conclusion in apparent contradiction with the physics of diffusion and the diffusivity calculated for the WAISD ice and should be further discussed (see also general comments).
See response above to general comments above
The lack of temperature response is indeed surprising, as we have pointed out and discuss. The WAIS Divide diffusivity calculation assumed a temperature dependence, but the data do not require it (at least until the warm temperatures near the bed as discussed here). Given the lack of understanding of how diffusion operates in polar ice sheets, we don't think it necessarily should be surprising that the effective diffusivity is not strongly dependent on temperature, but more work is necessary to understand the details, which can hopefully be done as new ice cores are drilled and analyzed as part of the multiple efforts at obtaining >1Ma ice.

Line 364: Figure 6 - it would be interesting to plot the expected diffusivity variations with temperature together with the EDC borehole temperature (see next comment).
We are not sure what the expected diffusivities should be. The following comment gives the values from the formulation of Fudge et al., 2016, but the functional form of the diffusivity was defined, not inferred. We believe this work provides better estimates of effective diffusivity

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Lines 393-421: this section comparing WAISD and EDC diffusivities is difficult to follow, the typing error in the WAISD diffusivity (2.2×10^{-9} whereas it should be 2.2×10^{-8}) adds to the confusion (see also comment on lines 66-71). A diffusivity of 2.2×10^{-8} is much higher than any value inferred from the EDC or DF records but the authors conclude that it neither supports nor refutes the lack of temperature dependence of the effective diffusivity below -10°C. I checked the orders of magnitudes with the temperature dependent diffusivity formulation below Figure 4 in Fudge et al. (2016) and found $2\ 10^{-8}$ at -30°C, $5.6\ 10^{-9}$ at -40°C and $1.4\ 10^{-9}$ at -50°C. The argument of a higher thinning rate at WAISD holds only for the last glacial cycle but not the previous ones at EDC. The effect of accumulation rate is considered in terms of grain growth but also affects the impurity concentrations in ice and possible neutralization of acids (see line 278). The consistency of a lower diffusivity at WAISD (about $5.\ 10^{-9}$) with an absence of sulfate peaks at ages older than 52 ka should be evaluated. Overall, the absence of temperature dependence of the diffusivity below -10°C may be an oversimplification.

Sorry about that typo. It created a lot of confusion and we appreciate you putting in the effort to determine the correct values. The thinning rate at WAIS Divide is always greater than at Dome C, so we think the reviewer means that the layer thickness is not greater for older Dome C ice. The accumulation rate does affect impurity concentrations, but since we don't observe a significant difference in effective diffusivity between glacial and interglacial cycles at Dome C, it does not make sense to discuss the difference with WAIS Divide based on impurity concentrations. For WAIS Divide, the absence of sulfate peaks (and ECM) for ages older than about 52 ka is related to the warm basal ice (i.e. ~-10C) as discussed.

Lines 424-425: the absence of diffusion during the Holocene disagrees with the conclusion of Barnes et al. (2003) using the same methodology. The reasons for this disagreement should be discussed.
See comments above

Lines 429-430: the recent study by Lin et al. (2022) could be mentioned here.
Added

Lines 476-477: this sentence should be reformulated in a less affirmative way (see comment on lines 393-421).
We will change the sentence to:
Our estimate of the effective diffusivity is MOST applicable for timescales of hundreds of thousands of years and for temperatures colder than -10°C.

Technical corrections:
Line 43: Kahle et al., 2021?
fixed

Line 95: variability uses
fixed
Line 215: Figure 3 is difficult to read, the two panels should be enlarged and symbols representing all gradients made more visible
figure sizing will be adjusted in the final manuscript, which will also have the effect making symbols clearer. We do not enlarge the size of symbols because it obscures other symbols because of the overlap.

Line 240: increases
fixed

Line 402: 2.2×10^{-8} instead of 2.2×10^{-9}
fixed
References not cited in the manuscript:

Lin et al., Clim. Past, 2022. https://doi.org/10.5194/cp-18-485-2022

Wolff et al., Clim. Past, 2023. https://doi.org/10.5194/cp-19-23-2023
Citation: https://doi.org/10.5194/egusphere-2022-1219-RC2

Both will be cited

References
Legrand, M. (1995). Sulphur-Derived Species in Polar Ice: A Review. In: Delmas, R.J. (eds) Ice Core Studies of Global Biogeochemical Cycles. NATO ASI Series, vol 30. Springer, Berlin, Heidelberg. https://doi.org/10.1007/978-3-642-51172-1_5