Reply to Reviewer 2, Jeffrey L. Kavanaugh

We are grateful to Jeffrey L. Kavanaugh for his detailed and thoughtful review of our manuscript. We are confident that all concerns raised can be addressed in a revised manuscript. In particular, the description of our modelling approach will be much improved thanks to his queries and suggestions.

We do not reproduce the entire review here – sections of it are shown in italics. We note that some equations are not well-reproduced here. Our replies are given in regular font. Reviewer 2 also uploaded an annotated version of the manuscript, containing editorial suggestions that we are grateful for and will largely adopt in a revised version.

1. Gaussian form

…Their use here is more than justified
– but they remain just an approximation of the measured peak forms. (I'll also note that the description of stratospheric concentrations of sulphates following a major eruption (Lines 75–77) is decidedly non-Gaussian, being strongly asymmetric around the peak, and therefore it's reasonable for the reader to question whether sulphate concentrations in snow and ice are similarly asymmetric. Some additional discussion would help clarify this.

A similar point was made by Reviewer 1 and our reply is repeated below. We note that the manuscript text states peaks are Gaussian in form "very shortly after deposition". We do not argue that the direct deposition of sulfate from the atmosphere is perfectly symmetric.

The reviewer is of course correct that the accumulation of sulfur in the stratosphere and its subsequent deposition to the ice sheet will be asymmetric, with a relatively sharp onset and a decaying tail. This asymmetry is indeed seen in recent eruptions, but observations of eruption signals at EDC shows that the signals tend towards symmetry within a time (of order 1 kyr) that is relatively short compared to the 400 kyr of this study. For this reason we chose the mathematical simplicity of assuming an Gaussian shape from the start. Our sensitivity studies (Fig 3) where we changed the width of the initial peak between 1 and 5 years show that the exact width or shape of the initial peak does not materially effect the derived effective diffusion coefficients.

…it's unclear to me why FWHM is used to describe peak widths observed in the EPICA Dome C core throughout Section 2 (which describes the data), but FWTM is used throughout Section 3 (which describes the model). This isn't a major issue, to be sure (as the two are always related as $FWTM/FWHM = 1.83$), but it seems an unnecessary switch to make given that one value should be as easy to determine as the other from the data (but again, the data are presented in terms of FWHM, not FWTM). If FWTM is preferred, please include a brief explanation as to why.

We agree, the use of FWHM and FWTM is confusing and not necessary. Section 2 can be modified to use FWTM, or more generally 'width'. FWTM is preferred for the model because it is closer to the 'width' metric of volcanic events in ice core sulfate identified by previous studies, e.g., Sigl et al., 2013, and therefore useful for comparison.

Given that the objective here is to set up the numerical model, I recommend rearranging Section 3.2 somewhat, moving from expressing quantities in the time domain (FWTM/FWHM expressed in terms of years) to the spatial domain (FWTM/FWHM expressed in terms of distance) as quickly as possible, which could be accomplished by stating immediately stating that if the peak width is 3 years; moving Eq. 5 up to where it would become eq. 2; and then discussing relevant areas and fluxes (currently Eq. 2-4). (In my reading of this subsection, Eq. 2-4 needn't come before Eq. 5, but I might be missing something.)

Section 3.2 will be re-written following this advice and advice from Reviewer 1.

2. Description of the forward model

Reviewer 2 suggests re-writing equations 6 and 7. We agree we should more correctly use partial differentials in Eq. 6 and 7. The reviewer asks us to include a more fundamental intermediate equation. We are not convinced this is necessary and consider it less confusing for the reader to present the equation we actually solve including the parameter we try to derive.

Some clarification of the description of ice deformation at flow divides is also necessary. Lines 195-196 state "By assuming no lateral flow, the dynamics of the ice sheet consist only of one-dimension (vertical) flow, with ice layers thinning with increasing depth and pressure."
This misstates flow conditions in a couple of ways…

We thank Reviewer 2 for his suggestions on how to improve the section on ice deformation. We will update a revised manuscript with more appropriate terminology and remove the inaccurate statements.

It's important to also note that it is only because the same $\partial C/\partial x = 0$ and $\partial C/\partial y = 0$ conditions are met that diffusion (Eq. 6) can be treated as a 1-D problem here, rather than a 3-D one. With respect to Eq. 6, the effective diffusion rate D'((is expressed as a function of time (i.e., D'(((t). However, given the description of the model in the text, it seems that the diQusion rate D'((is held constant for each model run (with the best-fit diffusion rate for each sulfate peak determined independently from a set of 50 runs with log-spaced D'((values). Is this correct?

Yes, correct.

If so, this would mean that the sulfate diffusion rate is determined by the time at which the snow fell, rather than by the length of time the snow is resident within the ice sheet – which has implications for the interpretations regarding Gibbs-Thompson diffusion vs. slower processes discussed in Section 5.3.

The 'time at which the snow fell' and the 'length of time the snow is resident in the ice sheet' are in effect the same value because the units of ice age ('time at which the snow fell') is thousands of years before present, if we understand the Reviewer correctly here.

We agree that using a constant effective diffusion coefficient negates the possibility of testing how a time-evolving diffusion coefficient (perhaps due to changes diffusion processes) impacts the outcome. This will be clarified in a revised manuscript, also following Reviewer 1's comment. In a future treatment we could attempt to derive the time varying diffusion coefficient under certain assumptions by starting diffusion from various depths or by adopting a finite difference calculation method, but that is beyond the scope of this paper.

Related to the discussion of the material derivative, it is not specified in the text whether the model is constructed in a Eulerian (i.e., fixed) coordinate system or a Lagrangian coordinate system (in which the coordinates track the deforming material). The framing of the equations suggests that a Eulerian coordinate system is used; this should be stated. (This is a relevant question because the sulfate peaks are advected downward through time.)

A Eulerian coordinate system is used to generate a frame of reference relative to the centre of each peak.

There are a few other concerns I have regarding the equations and phrasing in Section 3.3:
• The depth variable z is defined (on Line 202) as the "height above the bed," but is subsequently referred to as "depth" (e.g. Line 205, which describes Eq. 9, and Line 207, which sets up Eq. 10). This is unnecessarily confusing to the reader, as thinking of z as a depth-below-surface reverses the sign convention. It would be much clearer to refer to z as "height above bed" throughout the text.

Good point – manuscript will be modified.

In Eq. 6-12, the spatial variable switches back and forth between x and z. I suspect that z refers to "depth within the ice sheet" and x to "distance along the ice core," but didn't see this clarified in the text.

Again, good point. Thanks. x is the distance along core relative to the centre of frame of reference (L210). Manuscript will be modified to clarify this.

Eq. 8 is dimensionally incorrect: the left-hand side has units of m/m yr) = yr, whereas the right-hand side is dimensionless (units: m/m); the equation is therefore a mathematical impossibility.*

We should have clarified that both the layer thickness lambda and the accumulation rate a are in m yr^-1.

Equation 12 has a similar issue with dimensionality: the argument of an exponent must be dimensionless, whereas $-a/H$ has units of yr).*

Yes, the equation should indeed have at/H in the exponential – thanks for spotting this typo.

The equation defining the downward velocity field (Eq. 9) has issues with sign Convention

The equation suggested by the Reviewer will be included in a revised manuscript so that velocity (v) is positive.

Eq. 9 defines the ice velocity as being downward (i.e., negative). The negative sign ahead of the velocity term in Eq. 7 (as written in the manuscript) would therefore result in an upward (i.e. positive) velocity field. This is why the second term on the right-hand side of the material derivative equations must be positive in both 3-D and 1-D forms of the expressions for the material derivative.

We will correct the sign error to ensure consistency between Eq. 9 and 9-11.

3. Significant figures

We thank the Reviewer for his advice on significant figures, which we will follow in a revised manuscript.

4. Section 5.3 Implications

I'm not convinced this material needs to be presented separately from that presented in Section 5.1 ("Factors potentially influencing diQusion rate"), as the discussion as to whether and when the slower Barnes [2003]-type diQusion or the more rapid Gibbs-Thompson Ng [2023]-type diQusion might operate seems to fit well within that general topic.

We respectfully disagree with the Reviewer's opinion here. We will modify the sub-heading of Section 5.3 to make the distinction between Section 5.1 and Section 5.3 clearer.

I'm also not sure whether the study directly addresses whether Gibbs-Thompson diQusion might explain initial (high) sulfate diQusion rates, but not later (lower) rates of diQusion. This relates back to my earlier question regarding whether the eQective sulfate diQusion rate D'((is held constant for a given sulfate peak during the model runs. If D'((is held constant throughout each model run, the model does not directly answer the question: no "old" ice would have been modeled with high initial (Gibbs-Thompson) diffusion, followed by lower (Barnes-type) diffusion.

The Reviewer is correct in stating that the diffusion rate does not change as a function of time for each individual peak. Each peak is modelled with a constant effective diffusion rate (Deff). However, Deff does not represent the instantaneous diffusion rate in ice of that peak's age but a time-weighted rate of diffusion over the entire history of the peak (see L 234). By analysing each individual volcanic peak, and assigning the 'best-fit' Deff for each one, we are able to ascertain that effective diffusion rate (and therefore diffusion rate) decreases with time. We are not able to quantify the change in instantaneous diffusion rate with time.

In a future treatment we could attempt to derive the time varying diffusion coefficient under certain assumptions by starting diffusion from various depths or by adopting a finite difference calculation method, but that is beyond the scope of this paper. But the reviewer is correct, at present we have not modelled any ice as having a high initial rate followed by a lower rate at some depth, although this is what we suspect is happening.