Comments

1. Gaussian form

I think the discussion around the shape of the sulfate peaks could be improved. The manuscript states (on. Lines 73–74, Section 2) that "major volcanic sulphate peaks have a well-understood, reproducible peak shape very shortly after deposition, specifically a reproducible duration of deposition or peak width and a Gaussian form." More accurately, the form is only approximated by a Gaussian – examination of Fig. 1, for example, shows several measured peaks that deviate markedly from a true Gaussian form. Gaussians are great – they're completely specified by two values (peak height and width – more on the latter in a moment); they're continuous and smooth, and thus extremely tractable in numerical models; and they describe diffusion processes very well. 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.

In the manuscript, three different measures of Gaussian form widths are used: FWHM (full width at half maximum), FWTM (full width at tenth maximum), and σ (the standard deviation). Only one of these appears in the family of equations that describes the Gaussian form. The particular form used by the authors is not explicitly stated – I think it should be – but this works:

$$C(x) = C_P \exp\left(\frac{1}{2}\frac{(x - x_P)^2}{\sigma_x^2}\right)$$

Here $C(x)$ is the concentration at along-core distance x, C_p is the peak concentration (occurring at corresponding distance x_P), and σ_x is the standard deviation, the fundamental measure of the peak's width.

With that background out of the way (though we'll return to these ideas), 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.

Given the workflow – FWTM (or FWHM) peak widths are first determined from the ice core data, and then from these widths the standard deviation is determined – so I think Eq. 1 would be better if inverted, i.e.:

$$\sigma_z = \frac{FWTM}{4.29}$$

(Note that if FWHM is used instead of FWTM, the factor of 4.29 would be reduced to 2.35.) Switching to using FWHM in Section 3 would provide a more direct (if ultimately similar) linkage back to the data – i.e., that FWHM is between 2 and 3 years (as described in Section 2).

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.)

2. Description of the forward model

The forward model describes the time evolution of sulphate concentration in a moving medium, ice. This type of problem is typically solved using the "material" derivative D/Dt, which for a scalar quantity φ is generally expressed (in three dimensions) as

$$\frac{D\varphi}{Dt} = \frac{\partial\varphi}{\partial t} + \vec{v} \cdot \nabla\varphi = \frac{\partial\varphi}{\partial t} + v_x \frac{\partial\varphi}{\partial x} + v_y \frac{\partial\varphi}{\partial y} + v_z \frac{\partial\varphi}{\partial z}$$

For one-dimensional flow in the z direction, this reduces to

$$\frac{D\varphi}{Dt} = \frac{\partial\varphi}{\partial t} + v_z \frac{\partial\varphi}{\partial z}$$

This is essentially the Eq. 7, aside from the difference in sign of the second term on the right-hand side (more on that later). I recommend that Eq. 7 be re-written to this form (with the sign corrected). I also recommend that Eq. 6 be recast to reflect that it represents the partial derivative $\partial C/\partial t$.

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.

First, ice doesn't deform (and the ice layers don't thin) because of increasing pressure (i.e. mean normal stress); rather, ice flows in response to non-zero deviatoric stress conditions. Ice is nearly incompressible, and incompressibility is baked into essentially all glacier ice flow models (including that of Nye [1963], which frames the ice flow component of this study).

Second, it is not the assumption of "no lateral flow" that allows the move from the 3-D expression for the total derivative to the 1-D expression. Instead, we can collapse to the one-dimensional expression because the sulphate concentration in a given (horizontal) layer of ice is uniform across the general area of the ice sheet. In other words, this can be treated as a 1-D problem because $\partial C/\partial x = 0$ and $\partial C/\partial y = 0$, **not** because $v_x = 0$ and $v_y = 0$. If these horizontal velocity terms were zero, a necessary consequence (due to the conservation of mass) would be that v_z is also zero – and there would be no thinning of the layers.

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_{eff} is expressed as a function of time (i.e., $D_{eff}(t)$. However, given the description of the model in the text, it seems that the diffusion rate D_{eff} 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_{eff} values). Is this 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. Also, if this is the case, Eq. 6 can be simplified to

$$\frac{\partial C}{\partial t} = D_{eff}\frac{\partial^2 C}{\partial x^2}$$

If D_{eff} is indeed time-dependent (and therefore depth-dependent) within a given model run, this should be clarified, and a description of how D_{eff} is varied with depth should be included.

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.)

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.

- 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.

- Eq. 8 is dimensionally incorrect: the left-hand side has units of $\mathrm{m/m\,yr^{-1}} = \mathrm{yr}$, whereas the right-hand side is dimensionless (units: m/m); the equation is therefore a mathematical impossibility. It would be better to restate this as, for example:

$$\frac{\lambda}{\lambda_0} = \frac{z}{H}$$

  where λ_0 represents the initial thickness of a layer equal to one year's worth of time-averaged (and ice-equivalent) accumulation.

- Equation 12 has a similar issue with dimensionality: the argument of an exponent must be dimensionless, whereas $-a/H$ has units of yr^{-1}. The dimensionally (and mathematically) correct expression is

$$z(t) = He^{-at/H}$$

- The equation defining the downward velocity field (Eq. 9) has issues with sign convention; the correct expression is

$$v = \frac{\partial z}{\partial t} = \frac{-za}{H}$$

  This revision correctly defines the velocity.

- 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.

- As an aside, my preference would be to express the average accumulation rate as $\dot{b}$, as is traditional; this has the added benefits of (a) being immediately identified as a rate (i.e., a time derivative) and (b) allowing time (in years) to be expressed as "a" (Latin; "annus" or "anni").

3. Significant figures

Throughout Section 4 ("Results") of the manuscript, there are several mismatches in the number of significant digits given for the best estimate of a number and its uncertainty. For example, on Line 244, a diffusion rate is given as $1.0 \pm 0.31 \times 10^{-8}\ \mathrm{m^2\ yr^{-1}}$. Rewriting this

as $1.0 \pm 0.3 \times 10^{-8}\ \mathrm{m^2\ yr^{-1}}$ solves two issues: first, it states the uncertainty the same precision as the best estimate; second, it states the uncertainty to one significant digit only. (If the uncertainty's leading significant digit is 1 or 2, then the uncertainty can be stated to two significant digits; otherwise, it should be given to one significant digit. The best estimate should be expressed to the same significant digit as the uncertainty.) Please also replace the "x" multiplying the power of 10 with the mathematical "×" symbol each time scientific notation is used.

4. Section 5.3 ("Implications of results")
I'm not convinced this material needs to be presented separately from that presented in Section 5.1 ("Factors potentially influencing diffusion rate"), as the discussion as to whether and when the slower Barnes [2003]-type diffusion or the more rapid Gibbs-Thompson Ng [2023]-type diffusion might operate seems to fit well within that general topic.

I'm also not sure whether the study directly addresses whether Gibbs-Thompson diffusion might explain initial (high) sulfate diffusion rates, but not later (lower) rates of diffusion. This relates back to my earlier question regarding whether the effective sulfate diffusion rate D_{eff} is held constant for a given sulfate peak during the model runs. If $D_{eff}\ is$ 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.