Response to RC1 [egusphere-2025-1566]
Black writing: review comments. Blue writing: authors' response.

RC1: Anonymous Referee #1

1 General

In this paper, the authors analyze the diffusion of key climate signals in ice cores, taking sulfate as the primary molecule of interest. They pick up the thread of the recent analyses by Fudge et al. (2024) and Rhodes et al. (2024), where the results of those papers differ, in part, due to their distinct methods for estimating the diffusivity. Felix Ng and company describe these differences and place their origin on a firmer theoretical foundation. The current authors then take the new theory inference and perform a new inversion. The results of this effort are sort of mixed and could be clearer (but maybe the answer is that the data / inversion is just inconclusively unclear). They then perform a forward simulation, which matches the data nicely and gives new insight. Then there is a discussion about the rate of diffusion through the firn. This part of paper is a little speculative and lacks some of the clarity from the earlier part of the paper.

Overall, this is a really well-written paper – Felix and team are articulate, poetic, and precise in their wording – a masterclass in writing. I like that this is an all-star team – with 3 recent papers on the subject – joining forces to get to the bottom of this question. I also like that this paper is eminently readable. There is a nice description of the physics of the $t - z$ transformation to $\psi - \zeta$. With a few minor changes, I am happy to support publication.

Thank you for your valuable review. It is very encouraging and reassuring for us to read your summary of our manuscript's thread and contributions. We are happy to know that our writing communicates the science well.

Regarding the part that considers diffusion through the firn (Sect. 4.1), in this manuscript we have space to consider the potential mechanism only at a simple level. We hope you agree that Sect. 4.1 foreshadows a full study; we plan to tackle that elsewhere.

2 Remarks

1. I think more discussion of DR is warranted – it turns out to be important and having not read Rhodes et al. (2024) in enough depth, I am wishing that it was clearer in the text.

From reading the comment and visiting the manuscript, it is not clear to us what is missing (and where). How the method of Rhodes et al. (2024) works and what D_R measures are explained on Lines 81–90, together with Fig. 1b. Their findings for D_R are reported later in the Introduction on Lines 105–122 (with Fig. 1c); there, the decay in D_R is described – positioned for us to return to address later in the manuscript. Sects. 2.2.1, 2.2.2, and 3.1 also refer to D_R in the context of our theory and results in various places. Presently we have not decided on revision on this matter. We think that the text strikes a reasonable balance in terms of explaining what our study needs and recounting too much of Rhodes et al. (2024), and we rely on readers looking up their referenced study for more background.

3 Specific comments

1. Section 2.2.1 - it could be worth making the connection to the method of characteristics as motivation for the change of variables.

Thank you for this suggestion. We considered it but decide not to draw the connection. In this part of the manuscript, where we launch readers into the first heavy mathematical idea, it is better for us to focus on getting them safely to Eqs. (5) and (6) and the implications, without mentioning too much that is peripheral. We think that the concepts given on Lines 174 and 175 sufficiently signpost the motivations.

2. Line 192: what is the 4.2919 factor?

The factor converts the FWTM of the Gaussian to its standard deviation σ. We will extend Lines 192-193 to clarify this.

3. Lines around 320: is DE the same as Deff from before? I am missing the subtle difference.

DE is the effective diffusivity sought by the inversion approach based on mean absolute gradient, assuming D to be constant in the inversion interval, but not using the approximation made by Barnes et al. (2003) described on the last page (p12, lines 301–304). We will edit the writing near Line 320 to clarify the distinction.

4. Figure 4a: a legend or arrows could be helpful. It took some time for me to see the exponential curve.

Yes, we can add legends in Fig. 4a for the line types.

5. Where is the first inversion approach plotted? Figure 5 is the second approach only, correct?

Figure 5 does plot the results of the first inversion approach, as described on Line 398: "In contrast, D(t) from the first approach (dashed black curve in all panels, Fig. 5) shows a much more…". Figure 5 also clarifies the origin of the curve in its caption and legends.

6. I am very confused by the oscillations in D(t) shown in figure 5. Is some sort of regularization required?

D(t) oscillates because the inversion finds stretches of negative D as well as positive D, so we have focussed on explaining how negative D can arise. Potential reasons include noise on the D_R values (which derive from estimates from the sulphate record), uncertainty around the amount of spline-smoothing, and time-dependent variations in the real system (e.g. history of ice submergence velocity in the column). We outlined some of these on Lines 430-432; in a revision, we will try to improve their wording and link in the observed oscillations. Although regularization might prevent or suppress the oscillations, we don't think it is the right way to go, given the causes mentioned above.

Felix Ng
20 August 2025