Response to RC2 [egusphere-2025-1566]

Black writing: review comments. Blue writing: authors' response.

RC2: Referee #2 (Anders Svensson)

The manuscript aims at quantifying the (effective) diffusivity of sulfate ions in the upper 2700m/~400ka of the EPICA EDC ice core. A nice theory for the involved processes is developed, at least for the upper part of the ice sheet, and based on results from previous work, the manuscript does come up an improved understanding of the topic and novel results. Overall, I mostly have a few comments to the manuscript, not any major concerns.

The manuscript is rather long, explaining every step taken in full detail. It would be an option to move some of the derivations into appendices. On the other hand, it is nice as a reader to be guided carefully through the various steps, so if it is not a concern of the journal, I would not recommend shortening it. In general, the manuscript is extremely well written and well illustrated. Obviously, a lot of effort has already gone into streamlining this manuscript.

The topic of determining the (effective) diffusivity at the EDC site has recently been the topic of two other papers in Climate of the Past. The current manuscript carefully explains why those studies reached deviating results/conclusions and elegantly develops a more advanced theoretical framework that is able to reproduce the previous results and explain where they went wrong/made too bold approximations. In particular, the diffusion of sulfate in the firn layer appears to be an important consideration that needs to be taken into account. The firn has been known to be important for diffusion of water isotopes for a long time, and it would be interesting to know which other components have important signal dampening by firn diffusion. And what about dust particles that are not supposed to diffuse importantly, can they teach us about what other processes are important for signal disturbance in the deeper ice?

Thank you for perusing our manuscript and your insightful review. Your comments on the broader science surrounding our study are much appreciated. They echo our thoughts and ongoing work beyond this study, e.g., some of the avenues outlined in the Discussion.

We are very happy to read your appraisal of our writing and your comments about its length. In a study that develops a new mathematical theory and applies it to reach new results as well as critiquing past methods, we feel the need to explain and report in detail.

Given our focus on sulphate and its diffusivity inversion, in the manuscript we have refrained from speculating/discussing how diffusion in the firn might impact other species or the role of dust on signal evolution. We agree that these topics are important for the field to address.

Still, with all the improvements introduced in the current manuscript it is remarkable to see in Figure 9 that all of the estimated contributions to the sulfate signal diffusivity are INCREASING with depth/age whereas the observed diffusivity clearly is decreasing with depth/age even with a temperature profile that increases with depth. Indeed, there are additional layers of understanding of the topic to disentangle in future studies. Clearly, a lot can be explained be letting the different suggested types of diffusion phase in at out at different depths/ages, but as I see it, there must still be something quite fundamental lacking for our understanding of the topic. For example, the effect of dust or other impurities, the development of size and/or orientation of ice crystals with depth. Let alone, the strange-looking sulfate peak shapes observed in the deeper sections of the EDC ice core (not something that has been observed in Greenland to my knowledge). Hopefully, soon-to-appear very high resolution records of ion locations and other properties in ice cores will enlighten us.

In a revision, we could inject the idea of possible additional factors regulating the diffusivity profile into Sect. 4.2 or 5. We shall keep this very brief, aiming mainly to sketch the challenges ahead and make our text more rounded.

As for the new cores and high-resolution records, we too look forward to learning what they teach us!

Although the study is concerned with sulfate spikes in the EDC ice core only, the developed theory can be applied to other ice cores provided high-resolution sulfate records and other essential records are available. As I understand it, the theory could even be applied for other types of ions or impurities provided the necessary records are available. The EDC core is of particular interest due to the recent arrival of the BE-OIC oldest continuous ice core from Antarctica that currently has a lot of attention and for which the preservation of signal strength in the deepest ice is a major point of interest. A logical next step for the present study would be to apply the developed theory to other ice cores and to get a more general overview of the signal preservation of impurities in ice with different properties. For example, it would be very useful to know which magnitude of volcanic eruptions that will survive to which depth/age in different ice cores, eg a figure 10 that covers other ice cores. It is understandable, however, that the current manuscript does not cover those topics.

Thanks for these helpful comments. The theory can be applied to other ions/impurities and other cores (as mentioned on Line 60). With the EDC core, nitrate and chloride may be the more obvious ions to study. Our immediate next focus, however, is on other cores, as the finding of high sulphate diffusivity in the firn intrigues us.

We read your idea of repeating Fig. 10 for other cores with enthusiasm, although we can see that estimating signal survival at their sites will require much bolder assumptions (about the vertical pattern of diffusivity) than those we currently use in Sect. 4.3 for the Dome C region.

Felix Ng
20 August 2025