Review of "Post-deposition processes affecting water stable isotope records at Little Dome C, Antarctica: new records from two firn cores and virtual firn core modelling" by Samin et al.

The authors present an important study comparing new firn core data from Little Dome C with simulated virtual firn cores. The simulation is presented in great detail, and the addition of surface noise and mixing to improve agreement with observations is a meaningful methodological contribution. Addressing signal formation in firn and ice cores is an important and ongoing challenge in paleoclimate research, and introducing a surface mixing term that improves model-observation agreement represents a significant step forward. The manuscript is generally well written and figures are consistently labelled. The study falls within the scope of EGUsphere. I recommend revisions before publication, as outlined below.

Major Comments

Implementing a surface mixing is a great contribution to the discussion on the signal formation; however, the manuscript lacks any substantive discussion of other post-depositional modifications, particularly sublimation, wind-driven erosion and snow redistribution. These processes are known to affect isotopic records in low accumulation regions such as Little Dome C. The authors mention these processes in the introduction and the title also suggests that these processes are addressed in the study. These processes should be at least discussed regarding their potential influence on the presented records (if not modelled). Furthermore, I am not an expert in simulating long time scales; therefore, I am unable to assess whether the assumption regarding the input climate for the 80m simulation is accurate, and I hope that another reviewer will be able to address this issue.

It remains unclear whether the implemented surface mixing is an analogue to diffusion and the manuscript does not sufficiently explain how mixing is calculated. In line 176 it is written that the mixing is a "spatial moving average", but it is not mentioned if this is , for instance, a weighted average, and during which step of the modelling this mixing is applied. A more explicit description of the underlying assumptions and of the parameterisation would strengthen the methodological transparency of the study.

The authors acknowledge that one core was stored for some years and that Dallmayr et al. (2025) reported isotopic changes associated with storage. Given this, a direct comparison of both cores without applying a correction is not well justified. If storage effects are analogous to diffusion, back-diffusion algorithms may offer a viable correction approach and should at least be considered and discussed.

The manuscript relies on SO_4 concentrations alone for core dating. The authors might consider calculating non-sea-salt sulphate (nss-SO_4) concentrations and using a robust threshold criterion. This would yield more reliable stratigraphic tie points and improve comparability with other records and studies.

The study by Ollivier et al. is often cited, especially in the discussion. Yet, since this paper is not published, it is in parts difficult to follow the line of argument. The authors should briefly summarise the key findings of Ollivier et al. where relevant. Additionally, it remains unclear whether the results presented here (and those of Ollivier et al.) are specific to the Dome C region or whether they can be generalised to the broader East Antarctic Plateau. A discussion on the question of spatial applicability would strengthen the manuscript.

I am missing a data availability statement, links to a data repository to access the data and a link to the code of the model. I would only accept this study for publication if these links were included.

Specific comments (minor)

- L. 26: It is mentioned that the deposited snow preserves an imprint of local and global climatic conditions.I am a bit puzzled with this statement and am also missing the respective references to both, the local and the global signal.
- L. 169: Using 1 cm, are you just following Lapple et al. (2018) or did you also perfrom a sensitivity test to figure out what the best noise scale is (for your specific data)?
- L. 186: Are you applying the MTM method in depth or time dimension?
- L. 228: The unit "m" following "0.48 ‰^{2}" is unclear.
- Figure 5: what are the cycles than are mentioned here? I am missing the descirption and usage of them in the text.
- L. 309: remove**s**
- L. 342: Please clarify what physical process or quantity is represented by the surface mixing term.
- L. 421: "visual comparison" – can you try to quantify it or at least show the comparison (not only in the appendix)?