This manuscript combines a 2.5-D ice flow mode with IPR sounding data to study the basal ice dynamics at LDC, which has important implications for locating old ice and interpreting the paleoclimate record it preserves. This study addresses an important science question and will be of interest to a broad community. The methodology is robust and well described, and the manuscript includes clear and informative visual illustrations. The authors have responded thoroughly to the previous reviewer comments and revised the manuscript accordingly. I have only a few minor comments and suggestions that I believe could further improve the clarity and quality of the manuscript.

My primary concern relates to the treatment of input data uncertainties and their influence on the model outputs, particularly in regard to the interpretation of critical features such as the inferred basal accretion layer. While the authors provide a good overview of the sources and ranges of uncertainties (e.g., in divergence rates, radar line positioning, IRH depths, etc.), the manuscript would benefit from a more explicit evaluation of how these uncertainties propagate through the inversion process and affect key model outputs. For instance, the identification of an accretion or stagnant ice layer relies on a relatively small difference among H_m, H_{obs}, and traced particle trajectories. Without an assessment of their sensitivity to uncertainties in IRH positions or surface velocity measurements, it is difficult to fully trust the robustness of this interpretation. Maybe a minor underestimation in IRH depth or flow line misalignment could alter the model results significantly? A formal sensitivity analysis or at least a discussion of plausible error margins and their implications on interpreting basal processes would greatly strengthen the paper and provide readers with a clearer understanding of the reliability of the model's conclusions.

Another concern relates to the approach used to estimate the accreted ice layer thickness. The authors derive this by comparing the depth of the deepest traced ice particle trajectory with H_m. However, this method seems to assume that subglacial meltwater travels through the bedrock along similar paths as ice particles within the ice sheet, eventually refreezing along those trajectories. This assumption is problematic. The movement of meltwater within or beneath the bedrock is controlled by very different processes than ice flow, such as basal hydraulic potential gradients, bedrock porosity and permeability, and subglacial hydrological routing. It is not physically reasonable to assume that meltwater would follow the same spatial paths as deforming ice, especially over bedrock obstacles or variable basal conditions. I suggest the authors clarify this assumption.

Line 146-152: This paragraph, especially the second half, is hard to understand. The methodological details related to GNSS measurements and pole installation lack sufficient explanation, making it hard for the reader to grasp the technical reasoning. No references are provided to support the statements or the methodological details.

Line 246-247: It takes a long time for changes in surface temperature to propagate through the ice column and influence the basal thermal condition and basal melt rate.

Line 347-352: The observation that the 1D model appears to yield age estimates closer to preliminary field observations than the more physically comprehensive 2.5D model is intriguing. The authors suggest that this may be due to past migration of the ice divide/dome, but this hypothesis is not elaborated upon. It would significantly strengthen the manuscript if the authors could expand this discussion:

- Why would ice divide migration lead to better performance of a 1D model that assumes no horizontal flow?
- Could this discrepancy be interpreted as indirect evidence supporting past dome movement or reorganization of the ice flow?
- What would be the implications of such migration for site selection or future modeling approaches?

A deeper exploration of these possibilities would provide valuable insight into the glaciological dynamics of the Dome C region and could open a useful discussion about the limits and contextual applicability of 1D versus 2.5D modeling frameworks.