This manuscript explores the use of internal reflection horizons (IRHs) to calibrate ice-sheet models. To support this goal, the authors trace multiple IRHs in two key regions of East Antarctica and then use these horizons to evaluate model outputs produced under different model setups. Through this work, they demonstrate that incorporating IRHs into ice-sheet models can provide an additional and powerful constraint on simulations of past ice-sheet dynamics, especially away from ice divides.

The paper addresses an important topic. The authors have done a substantial amount of high-quality work, particularly in producing, documenting, and sharing new IRH datasets in a standardized, model-ready form. I especially appreciate the careful uncertainties analysis. The modeling results are clearly presented and thoroughly discussed.

Overall, I believe this is a timely and well-motivated study at the interface of radar stratigraphy, ice-sheet modelling, and data–model integration. I will be happy to see the paper being accepted for publication after addressing the points below.

Major suggestions:

1. **Manuscript structure and readability**
    a. The manuscript presents a substantial amount of new IRH tracing, together with depth and age uncertainty estimates. While I very much appreciate the volume of new data and the thorough description, it is difficult to quickly locate specific information in such a long paper (for example, the uncertainty associated with a particular IRH in a given region). I strongly recommend adding a concise summary table that lists, for each IRH and region, information such as the nominal age, associated uncertainties, etc.
    b. The results and discussion are combined into a single section. This is acceptable in principle, and I appreciate that, given the integrative nature of this study, substantial description and discussion of the IRH data are needed before the model–data comparisons can be presented. However, in the current form this combined section becomes very long and dense, with few natural breaks, which makes it hard to navigate. I suggest either (i) separating Results and Discussion into two sections, or (ii) introducing clearer internal structure and signposting so that readers can more easily follow the progression from data description to modelling results and their implications.
    c. The section numbering and nesting are confusing in places. For example, there is a Section 3.2 under which a subsection is labelled 3.2.1, but there is no 3.2.2. I recommend revisiting the section hierarchy and renumbering, and possibly adding a short "roadmap" paragraph at the start of Section 3 to guide the reader through the data description, model setup, and data-model comparison.
2. **Limited model setup explanations**
    The different model experiments are generated by modifying the precipitation-scaling parameter and the till effective pressure parameter. These are important concepts for understanding the subsequent results, yet they are only briefly introduced, and the actual values used in each simulation are not very clearly summarized. I suggest:
    a. Providing a short explanation of the physical meaning of the precipitation scaling and till effective pressure parameters, and why these two were chosen as the focus of the sensitivity tests over other possible parameters.
    b. Adding a small table or a clear paragraph that lists the parameter values for each simulation, instead of showing such information in a figure legend panel, so that readers can easily see how the experiments differ.
    These clarifications would make the modelling setup more transparent and help readers interpret the model–isochrone misfits.

Minor comments:

- There is mixed usage of “isochrones”, “isochronal surfaces”, and “IRHs” throughout the manuscript to refer to the same features. I recommend adopting a more consistent terminology. Also, I suggest

avoiding “isochronal surfaces,” because “surface” is already used frequently in the manuscript to refer to the ice-sheet surface, which may cause confusion.

- The impact of interpolated (rather than directly mapped) bed elevation is discussed and highlighted. Could the authors also comment on the potential impact of interpolated IRH depths?
- Line #75: I assume “This” stands for the DML survey?
- Line #117, “WSL”: do the authors mean WSB here?
- Line #143, “Where necessary, we also made use of the 3-D capability of the software to find intersecting IRHs that were not visible in the 2-D view”: This sounds like an interesting and useful approach. Could the authors elaborate?
- line #161: is this depth from the ice surface or depth from the radar system?
- Line #185, “These three sources of uncertainties are then combined by calculating the root-sum-square error for each isochrone depth”: Do the authors mean the RMS error for each IRH represents the combination of these three sources of uncertainties?
- Line #191, “It is worth …… into account”: This would be a stronger and more useful statement if the authors could provide typical values for relevant model uncertainties together with references.
- Line #195 “accept that this could amount to several more metres in places”: A brief quantitative estimate/statistics, with a pointer to Appendix A, would make this statement clearer and more informative.
- Line #508: repeated “the”
- Figure 1: I appreciate that the authors include ice velocity as the background field. However, with the current colormap, the maps are dominated by deep colors, which makes it difficult to see some important features (e.g., survey lines with fewer existing IRHs, ice divides). I also recommend increasing the font size of the lat/lon labels, color bar labels, and place names.