Reviewer 1

Overview:

The authors here present their chronology development for a borehole from Sherman Island in West Antarctica. They use multiple methods to determine their chronology, largely based on the constraints of their sampling and changes in the ice chemistry resolution with depth. They find their borehole covers back ~1200 years and propose that ice near the bedrock could be early Holocene.

Overall, I find that their methodology is sound (although I will admit that I do not have personal experience with flow or thinning modelling). It generally accomplishes its goal of presenting an age-depth scale for the location, but broader discussion is rather limited outside of extrapolating what the bedrock age of the ice might be. The writing is generally good and easy to follow, with only a few minor technical/grammar issues highlighted in the technical comments.

As a stand-alone manuscript, this comes across lacking in parts at times. This manuscript is clearly complementary to Rowell et al, 2022, but readers unfamiliar with that paper will feel rather lost reading the present manuscript. I personally didn't understand many of the mechanics and specifics alluded to in this manuscript until I had to go read Rowell 2022. While it is of course fine to point out that the details on certain methods and analyses are in Rowell 2022, this manuscript in review needs to be able to stand alone enough that reader can understand all the basic results and discussion in the manuscript.

Following on that, the overall discussion of the final chronology and work feels a bit underwhelming and undeveloped. I don't think that a massive expansion of discussion is required (or warranted), but it would be nice to see some broader impacts and discussion about the chronology results. Is the final accumulation rate different from what is observed elsewhere or expected from models? How does the quality/resolution compare to the SI Core or other core-based results from similar sites? Are there any broad lessons learned about in how this method could be applied or where it should be applied elsewhere? Note that these aren't questions I'm requiring the authors to answer; they are simply pointing out some ways that a deeper discussion could make the manuscript more impactful than just largely a technical report specific only to this Sherman Island site.

Altogether, the manuscript is generally a solid report on the results of a chronology production. Should the editor(s) desire more than this, I think the paper could be made more impactful by developing the discussion more (both what exists on the bedrock age modelling and adding some broader comparative context and/or applicable lessons).

We thank you for your thoughtful consideration of this paper and for your assessment that the methodology and conclusions we present are sound. Upon reflection it is clear that there was too great a need for referral to the Rowell 2022 paper for this manuscript

to be considered a standalone text. And further discussion, as suggested, would enhance the paper and give more impact.

We have added further explanation about the chosen drill site (why and how it was selected) and use of the RAID to the introduction as requested in your following comments. We have also elaborated in the discussion and addressed some of the questions you raise in your review. As a result of other reviewers' comments we are re-visiting some of the modelling work and re-running the model to use the WAIS Divide accumulation rate as the reference for the record, as you also mention this in a later comment. Furthermore we have recently obtained a radargram from the Icebridge radar data available for Sherman Island and would like this to be included in the revised paper because it will enable us to expand on the discussion of bedrock age and ultimately the potential for a future ice core from this site.

Major Comments:

Introduction: The first paragraph sets the scene well, but I struggled to understand exactly the point and details of the project in the next two paragraphs. For example, is the RAID system different from other drilling systems? Why was Sherman island chosen? It is serving as a constraint of what? Moreover, what is the actual point and objective of THIS paper? There doesn't need to be a ton of detail on this stage, but more clarity and structure to prepare the reader for what they are about to read.
Thank you for explaining clearly what is lacking from the introduction. Instead of depending on referral to other papers (e.g. Mulvaney 2021 and Rowell 2022) for details about the project background, we have elaborated more on this in the introduction (now lines ~34-38). Specifically, we have explained that LIG ice from Sherman island would constrain the LIG WAIS stability in an additional location (if present) and stable water isotope records could indicate temperature and elevation history at the site as well as providing further palaeoclimate records (chemistry).

35: The use and reasoning of the RAID aren't clear here. Since it isn't stated what the RAID is, or how it differs from normal drilling, I don't know why it would be chosen in light of the risk of Sherman Island. (Reviewer note: Later I see that Rowell 2022 has this information, but the basics need to be summarized here in this manuscript also).
To avoid unnecessary referral to other papers, we have added a description of the RAID and how it differs from conventional drilling (now lines 45-50).

35: Why was Sherman Island chosen, then, if it had such high risk of not contributing to the goals of the WACSWAIN project? Did it have other virtues that warranted the risk, or was is simply logistically easy?
We have altered the introduction (between lines ~34-55) to address this concern, including addressing the comments above and trying to stress that the use of the RAID was ***because*** *of the risk that Sherman Island presents - of not containing old ice - and that a useful and long (potentially LGM) "core" could still be obtained, and still from a new and interesting location in the WAIS.*

Drilling and measurements: Some more information is needed here. Although papers with detailed descriptions are cited, this manuscript should include the minimum details required to understand the rest of the paper without having to look up a second paper. Namely, what sort of samples is the RAID bringing up? If they are chips, are they in stratigraphic order? What is modern environment at the drill site (accumulation, etc)? What is the vertical resolution of samples if they are mixed? These are all critical to understanding the rest of the paper, and a reader shouldn't have to go find a second paper to get this information.
Thank you, you are right to suggest that we expand more here rather than require readers to look elsewhere. We have added more detail about the samples, specifically, a description of how the ice chippings were sampled, their resolution, stratigraphy and mixing, and some more details about Sherman Island as a drill site (e.g modern accumulation rate originally estimated from RACMO to be 46.8 cm, but ice core analysis of last 20 years suggests higher rate of 58.3 cm).

Figure 1. A close up map of the Sherman Island region and drill site would be beneficial to understanding the local geography and ice structure/flow.
Yes we agree, and this will be included in a revised paper along with some other details to the map that we think would be beneficial (e.g. pointing out the relative location of the Thwaites and Pine Island glaciers referred to in the text, and other reference points such as existing WAIS ice cores including WAIS Divide).

105: Why was EPICA Dome C chosen as the assumed proportional accumulation as opposed to, say, WAIS? Is there any evidence to think that Dome C and SI would be proportional? They are very different climate systems.
This is a fair question and we are re-visiting the modelling for this paper to address this and similar comments by other reviewers. We would like to use WAIS Divide as a reference accumulation record, in addition to the modelling already done. We expect that this will not significantly alter the conclusions or final age scale for two reasons: 1) the accumulation rate of both sites (WAIS Divide and Dome C) has not significantly changed over the last ~1000 years and 2) the model smooths the accumulation record. However you are right to point this out and we should test this thoroughly.

110: This sentences reads awkwardly. Better fitting than the markers? Better fitting judged in what way? "It became clear" gives us no insight into what decisions were made, statistics performed, or observations made.
At the request of another reviewer, this sentence has been moved and, at your request, rephrased to make the approach more clear to the reader. We explain that the annual layer counting markers (shown in Figure 5) are more closely matched to the flank-flow depth/age model than the divide-flow, providing justification for using this depth/age model as the basis for identifying volcanic markers deeper in the core.

113: Again, if it is clear, you don't need to state it to the reader. You should present what made it clear.
Thank you, this is a good point. We have been more specific in explaining that with an average annual layer thickness of 87 cm in the top 70 m, a sample resolution of 19 cm

throughout the borehole would mean sample resolution remains annual or greater until at least 250 m depth (through comparison with the depth/age from the flank-flow model), enabling resolution of individual volcanic events in the sulfate record.

114: Annual resolution isn't necessarily required to detect volcanic events. They are found at Dome C, for example, and the signal there is blurred over annual levels. In fact this raises an interesting question: since the sulfate fallout can extend over several months, did any of the volcanic peaks interfere with your annual dating by SO4 peaks?
This is an important consideration that you are right to point out. However, we deliberately stopped annual layer counting at 70 m depth, and the shallowest volcanic events that we identified were the 1815 and 1809 eruptions at 125-130 m depth. We demonstrate in Figure 4 that these eruptions do indeed appear over several RAID samples. To address your main concern, we know that this does not interfere with the annual layer counts here, however if in future use of the RAID if annual layer counting was carried out to deeper depths, this would need to be considered.

140: Note that Kuwae is still disputed as a source for the 1458 eruption (https://doi.org/10.1038%2Fs41598-019-50939-x). Should become more clear with some research over the next decade, but you could add a "commonly attributed to Kuwae" or "eruption in 1458 (possibly Kuwae)" style to be safe.
Thank you for this helpful comment, we have made your suggested edit to the text. We have also edited line 163 (now line 180) where Kuwae is mentioned again and corrected a typo for the date at this line from 1485 to 1458.

Figure 4: The legend is hard to see with its small size and placement. I recommend enlarging it and placing it at the top and/or direct labelling the y-axes and lines.
Thank you, you are right that the legend is small. In addition to some other changes to the figures (colour scheme and line thickness), we will adjust the legend to make it more clear.

233: What is known about the bed of the glacier here? Is it thought to have significant melt? Is the assumption about no basal melting simply for calculations, or is there geologic evidence supporting this assumption?
This is an important question and we will add more detail to the discussion here using the data we have available. Specifically, we have borehole temperature measurements to 323 m which extrapolating to the bed give a basal temperature of -6 C (Mulvaney 2021). This is worth reiterating at this point in the discussion and we will make these changes.

253: Again, there are references to the uniqueness or special nature of the RAID samples, but they were never described in this manuscript.
Thank you for pointing this out. We have re-phrased this section to remove the vague description of the "unique RAID samples". We have been more clear about the accommodations that were necessary - cutting off the annual layer counting at 70 m and comparing with a nearby and higher resolution firn core.

Discussion: Sections 5.1-5.3 are very short and do not add much discussion of the previous parts. I'd argue that they could simply be appended to their appropriate section in the results. Section 5.4 has some intriguing points, but is underdeveloped and feels a bit like an afterthought.
Thank you for your thoughts on the discussion. I agree that we could do more to develop this part of the paper. As a result of reviewer comments, we will be re-visiting the modelling (e.g. using WAIS divide as the reference accumulation rate record) and have obtained some recently processed radar data from the Icebridge project from flyovers on the island which we would like to add as further evidence for the conclusions we make in section 5.4. Along with these additions and changes to the discussion we will consider re-structuring the earlier discussion sections as you suggest.

Data availability: Are the data (d_2H, chemical species, etc) that led to the creation of the age model in an online archive or otherwise available? I didn't see a link at Rowell 2022 (unless they are contained in that paper's supplementary material).
You are right to point this out - the data are not currently available. We would like to make the data for the full records available and will submit them to Pangaea. However we are also in the process of writing papers which will present the full, dated, stable water isotope and chemistry records and would like to publish them as standalone datasets when these papers are published, rather than there also be an additional dataset of just the top ~70 m of data, as we think this would be confusing and not necessary. So one option would be for us to place the data on Pangaea with an embargo until these publications are available. Another possibility is of course to submit the data presented here as an additional supplement to this paper. I hope this makes sense and that either of these options seems reasonable.

Technical comments:

17: The acronym MISI is never used again in the manuscript, and therefore it is not necessary to define it here.
The acronym has been removed.

21: Both of these acronyms are only used once elsewhere. Remove acronyms and just spell out in second instance.
The acronyms have been removed.

26: LIG is used three other times. It may be better for readability to simply spell it out each time.
Due to expansion of the introduction and discussion, LIG is now used more frequently throughout the paper, so the acronym has been kept in line 26.

45: Suggestion to consider flipping the sentences in this section so that you give a brief summary of the important necessary information here, and then end with the sentence that "a detailed description…".
We have made other changes to this section as per your previous suggestion, so this is no longer needed.

Section 3: The numbering scheme is odd with 3.0.1, 3.0.2, etc. Since there is no 3.1, shouldn't these simply be sections 3.1, 3.2, etc.?
Yes thank you for pointing this out, the numbering has been corrected.

67: Is SI core the name of the ice core, or an abbreviation for Sherman Island? If it is the core name, perhaps adding a date and/or length to the end will make it more unique of an identifier. If it is an abbreviation, Sherman Island isn't abbreviated elsewhere (which I think is good).
Yes, SI:Core is the name we use to denote the 20 m firn core, to separate it from SI:RAID, which is named as such to make it clear we are talking about a "core" of RAID chippings, not a solid ice core. We name the RAID records from Little Dome C in this manner too (LDC:RAID1 and LDC:RAID2 for the two RAID boreholes which exist). For consistency and clarity over the type of ice we are referring to, we would like to maintain this naming system. You are right to point out that this is the first introduction of the naming convention in this paper, however, so we have introduced "SI:RAID" earlier in the text (now line 71) and clarified the naming of the firn core at line 83 (previously 67).

73: This phrasing seems odd to me. Perhaps "This variability could reflect the local geography of Sherman Island…"
Agreed, upon reflection the intended meaning was not clear, we have taken your suggestion.

74: "and IS then closely by…"
Corrected.

Figure 2 (and others): The line thicknesses are rather thick, which makes it hard to see small details where exactly indicated lines are falling. Consider making the thicknesses thinner.
Yes, we will reduce the thickness of the lines in Figures 2, 3 and 4.

102: Sentence starting with "That is" is a fragment.
Corrected.

111: a priori not prior, I think here.
Due to another reviewer's comment, this sentence has been moved and re-worded so this phrase is no longer needed.

Figure 4 (and others): This particular shade of red and green is difficult for many colorblind people. The colors aren't overlapping, so it isn't as big of a concern, but something to consider if you revise your figures. However, someone colorblind would not be able to make the connection from your legend to the lines.
Thank you for pointing this out. We will change the colour schemes used in the figures throughout the paper and be sure to check the figures again using a colour blind simulation tool.

178: This paragraph could be merged with previous.
Agreed, we have merged the first three paragraphs of this section into one.

Fig 7: Is this the best x-axis display for this plot? It seems to add more confusion and oddness since the yr BP axis goes negative and positive on the log axis.
Thank you for this comment. Presenting the age as relative to 1950 necessitates presenting negative values, and due to the desire to present this modelled data on a log x-axis scale to be able to visualise the range in the modelled outputs at an appropriate scale, this meant adding the negative values on the x-axis to -100. However, you are right that this could be confusing, so we will revisit the scale used here and either present the data as relative to 2020 (meaning no negative values) or re-consider the use of the log scale entirely. As there is more work to be done on the modelling as suggested by other reviewers, we will endeavour to address all the comments as closely as possible.