

This paper is very well researched and I am fascinated by the exploration of bubble free layers that has been carefully presented here. I think this paper is very suitable for publication in this issue. I am especially impressed with the diligence put into testing and developing this new proxy and I agree with the authors that it holds potential for future study and may contain valuable climate information. I also appreciate the authors' careful and honest interpretation of their results and the associated limitations. The writing quality of the paper is excellent.

Below I have outlined a list of specific comments that I hope will be useful to the authors in improving and revising the manuscript for final publication. Many of these items are suggestions or minor points. At a broad level, my primary critique is that I think some of the interpretations and discussion should be further developed. While I don't disagree, I don't think that some of the statements linking BFLs to broader climate or to depth of formation are as well- supported by the text and discussion as they could be. I also think that you can leverage the ERA5 dataset and all the work you've done to demonstrate its efficacy here to a much greater effect. The discussion in particular will improve substantially with some further analysis that refines and tests relationship between BFLs, snow accumulation and geopotential height fields using reanalysis data. I also suggest that you capitalize on the fact you have overlapping data from both reanalysis and an AWS on-site. Right now, the paper treats the AWS data and ERA5 data mostly in parallel, but I think some opportunity exists for more integration and validation between these datasets.

I hope the comments below are useful and I am grateful to the authors for developing this new and fascinating dataset. It is my recommendation that this manuscript be accepted for publication with minor revisions.

Sincerely, Dominic Winski

We thank Dr Winski for the encouraging and constructive comments.

Figure 1: The latitudes and longitudes in panels A and B on Figure 1 do not match.

We will replot this figure.

Line 73: "Upon physical inspection of the DSS1617 core, BFLs were found to occur in almost all cores" is a little confusing in this context. I suggest "found in almost all 1-meter core segments" or something.

We agree. We will rewrite this sentence as suggested.

Line 97: Need space before reference

We will add a space there.

Line 126: Pixels per image? Pixels per cm²?

We will clarify how the resolution of the image is determined, as this statement as it currently reads is misleading. Instead of:

'Ice samples were imaged by a computer-controlled line scan camera at a resolution of 2,048 pixels.'

This sentence should read:

'The images produced by the ILCS have a resolution of 19,932 x 2,048 pixels.'

We will change this in the revised version.

Fig. 2 caption: Check with the journal, but I'm guessing you'll need to change the next sentence and provide an actual DOI before the next round. Lots of data repositories can hold a link for you and only go 'live' upon publication or acceptance.

We are finalizing the data repository process, and will provide a DOI in the revised manuscript where the datasets produced in this study will be freely available.

Line 135: Can you explain why higher isostatic pressure would lead to a smoother boundary?

In the deep core, the high isostatic pressure will compress the BFLs, forming a denser bubble free ice layer with smoother boundaries. We will add this detail to the section at line 135.

Line 177: It's nice your statistical metrics come out significant, but you only have a sample size of 4 years. I'd be much more convinced if you break this down into all 48 months and show those correlations. That will also help validate your choice of monthly time horizons and accumulation values. The data in Figure A1, for example, looks like it has a very high correlation at much finer time resolution.

We agree. We will make four plots to show the monthly accumulation in each year in 1998-2001. The correlation coefficients between ERA5 and AWS accumulation for all 48 months will also be calculated for the revised manuscript.

Figure 4: This is commendable and certainly an improvement over using equal-intervals to assign months. However, as you are aware, accumulation patterns especially on month-to-month timescales can vary even over scales of meters. Have you made any effort to assess the accuracy of your monthly picks using data in the ice cores – chemistry with a well established seasonality for example. You can also use the weather station to validate the monthly snow accumulation too. Later on, you back off and end up only using seasons. So it would help to have some quantification of uncertainty regardless so that you have strong justification for whatever resolution you ultimately use to interpret your dataset.

Unfortunately, we only have the 4 years shown of AWS snowfall data, so we can't validate the monthly accumulation with the AWS over the full dataset. Because of the limitation, we opted to only use the more conservative approach of seasonal data.

While Law Dome has very high accumulation, we still don't think we can accurately predict monthly changes from the chemistry or isotope data. We do have some measures of sub-annual dating at Law Dome, especially in sea salts, however we

would advise caution in over-interpreting seasonal dating, and we don't advocate monthly dating using chemistry. This further reinforces our decision to stick with seasonal dating. We acknowledge that we weren't totally clear about our use of monthly versus seasonal data in the paper, and we plan to clarify our writing to rectify this.

Line 191: I'm glad you are conscious of the limitations here. Similar to my last comment, it would be helpful to quantify this if possible. I think you've got all the information you need to do this given the overlap with the AWS and previously defining seasonality in chemistry.

On re-reading this section, we think the section is quite confusing - our apologies to the readers/reviewers! We will change the section from lines 189-192 to read:

'Then, based on the estimated monthly dates transposed to the ice core accumulation series, approximate depths aligned to the commencement of each month were produced by adding up the scaled monthly accumulation amounts sequentially from the beginning of each year (Fig.4). We have high confidence in the annual (summer) layer dating in the DSS1617 ice core data (Jong et al., 2022), due to clear chemical and isotopic signatures. However, we have less confidence in being able to define 'within year' dates using chemistry or isotopes (especially at the monthly scale) thus we have used the ERA 5 accumulation scaled to Law Dome annual accumulation to date within year depths.'

As with the previous comment, we are unable to quantify the error in our monthly dating using chemistry or isotopic analyses from the ice core, because we only have complete confidence in their ability to depict annual layers, or in some rare circumstances seasonal features (e.g. low sea salt concentrations during summer.

Lines 212-217: Excellent!

Thank you!

Figure 5: The gray bar isn't visible unless I zoom in to 400%. I suggest either changing the symbology or using a less clustered example.

We will change the gray bar into more prominent colors.

Line 230-232: I was a little confused by this sentence, since it seems to contradict the previous sentence, until I read on to the next paragraph. I suggest rewording/reorganizing for clarity.

We agree that this is confusing and will reword to clarify that we are talking about two different scales of processes - the southern Indian ocean, and processes that are occurring over 10's to 1000's of metres at Law Dome itself.

Line 233-239: I can tell you are trying to be very clear and precise in your writing here, but I'm afraid I'm still a little confused. The examples that follow help a little, but I suggest revisiting this section and consider adding an equation, figure or conceptual flowchart or something.

We will add a flowchart here to clarify.

Line 256: TIS or tis? DT1 or dT1?

'TIS' is temperature inversion strength. We calculated TIS for every 6h timestep. 'tis' is the threshold we calculated, whereby a temperature inversion event is identified by $TIS > tis$. We will revisit this section to clarify.

Lines 258-267: You could use your AWS for this analysis too, right? Do you get similar answers for 1998-2001 with the AWS data vs. the ERA5 data?

The AWS data is only available for four years, compared to the 30 years of ERA 5 over the ice core period. We do compare the AWS to ERA 5 over the AWS timescale (Fig A1) and they match very well. However, we don't think we should use the AWS data for this analysis. This is because the AWS data contains a lot of missing data (and this may be biased due to the snow surface height sensor being overloaded during high precipitation events). In addition, the snow surface height data varies in both directions due to settling, ablation, redistribution etc etc. Thus, it would not be straightforward to interpret this data as suggested. The ERA 5 accumulation data is longer, has no missing data, and has the temporal resolution that we need. Ultimately, while the AWS data seems like the best quantitative dataset for the purpose, we think it is best in this study to treat it as qualitative data.

Line 291: So you are dating each BFL to within a month (as in Fig. 4), but you are only comparing to other meteorological variables on a seasonal basis, right? Stating earlier that your interpretive goal is only resolved to seasons (4/year) rather than months (12/year), might be helpful and would've allayed some of my earlier worries.

We will rewrite this part to make it more explicit as per our comments previously. We agree we weren't clear about why we opted to interpret only at the seasonal level, so we will revisit the methodological sections to ensure we are clear about our seasonal investigations. We will also revisit the earlier part of this paragraph to clarify.

Lines 309-310: I'm not sure that I am convinced of this statement. Because of the translation you are making from depth in the core to accumulated snow from your AWS, you will necessarily intersect with the AWS accumulation curve most often during periods of large accumulation increases. This might be fine if your AWS were at the core site itself, but unless the snowfall here is uncommonly uniform, I would certainly expect cm-scale differences in snow accumulation and redistribution over a few hundred meters. Looking at figure 7, it appears that differences of a few cm in one direction or another might affect your conclusions about the conditions under which BFLs are most likely to form. My suggestion would be to do your best to quantify the error associated with your assumptions in section 2.4. I know this is difficult to estimate, but any estimate would be a start (plus your thinking along these lines so far as been skilled and thoughtful). Then with these error estimates, I suggest doing some sort of sensitivity study using your approach in Fig. 7. If your depth/time estimates are off by X%, how many BFLs occur during storms vs. hiatuses for different scenarios within your error range?

We think that this comment really gets to the core of this study, and what the next direction should be. However we disagree with the reviewer about the next steps. While Law Dome is probably an ideal ice core to study BFLs given its high annual accumulation rate and the fact it is a well studied record, even at Law Dome differences of a few centimeters of snowfall will completely affect the *apparent* date of BFL occurrence. While we could try our best to quantify the uncertainties, we have opted to provide qualitative uncertainty by using the red and grey bands in Fig. 7 which give our qualitative estimate of the dating uncertainty. We don't think the data we currently have at hand would allow further refining of error around dates in a robust way.

Ultimately, this study is an exploratory investigation of events that coincide with BFL occurrence, and we make pains to differentiate between BFL *occurrence* compared to *formation*. A mechanistic study is required to investigate the *formation* of BFLs (and their preservation). We maintain that the next best step to investigate the mechanism of formation (and preservation) is a modelling study using the SNOWPACK model, within which we can simulate densification, vapour transport, etc. At this point, we will be able to re-visit the datasets from this paper with a clearer understanding of how to estimate uncertainty, because we will have an understanding of the atmospheric and snowpack processes that must be present to form BFLs. This will allow us to better define the climate process/es that BFLs represent.

Section 3.3/Table 3: Just to check – by seasonal you mean 4 values/year, right?

Yes. We will clarify this in Table 3 caption.

Line 330: Should be Table 4.

Yes, We will change this.

Line 338: Suggest replacing 'numerous' with 'two'.

Yes, we will change as suggested.

Line 347: Can you expand on this idea? Why would summer/autumn BFL formation be climate- related as opposed to winter formation being surface process related? How does seasonality alone suggest a climate-related process? Connecting this logical framework is important, because your claim in the following sentence that "BFLs have the potential to provide past climate information" is critical to the paper.

We are a bit unclear about the reviewers comment here, as Law Dome BFLs occur in autumn/winter, and we are saying that Law Dome (autumn/winter) BFLs are climate related (not surface process related). Summer BFLs (e.g. WAIS) may be surface process related, if they are local radiation/melt layers. This is harder to understand at Law Dome, where BFLs predominantly occur in autumn/winter (unlikely to have a strong radiation/melt link).

On re-reading this paragraph however, we think our wording was a little vague, and gave an unintended meaning to our statements. In particular, this sentence:

“This seasonality suggests that BFLs may be related to regional climate variability, rather than local surface processes.”

We propose changing this sentence to:

“This seasonality suggests that BFLs may be related to *seasonal* climate variability, rather than *localised* surface processes.”

Line 357-375: Much of these paragraphs repeat from earlier. My suggestion would simply be to mention up front that you will only interpret to the detail level of seasons and lay out your justification then, in the methods. This will save time in the discussion and show readers earlier that you are aware of your dating limitations early on.

We will add line 357-372 to the methods to clarify the ice core monthly dating limitation. In line 373-375, we would like to point out that DSS1617 BFLs seasonality is different from BFLs in previous studies, and this difference does not come from different dating strategies. So we will keep line 373-375 here. We will however, review this entire section for clarity and brevity.

Line 405-424: I suggest condensing/reducing this paragraph significantly.

We will reduce this part.

Line 425-426: Why do you conclude that the BFLs are formed so close to the surface? This was never really discussed.

Previous crust studies have suggested that those crusts should form at, or below the snow surface. In this study, the hypothesis is that the DSS1617 BFLs are formed through the vapor condensed at the cold snow surface. Furthermore, scientists who have worked around the DSS sites have qualitatively observed there are thin ice crusts at the snow surface. These ice crusts could be newly formed BFLs. Thus, we suggest the BFLs may be formed close to the snow surface.

We will clarify this in the discussion.

Line 449-454: You don't have to hedge here – you have all the data in the ERA5 dataset you need to say something really concrete. Does a correlation between accumulation at Law Dome and 500 mb GPH produce fields similar to SOM1 or SOM2? If you think moisture transport is important, ERA5 has variables you can use in your analysis. You could also isolate times during accumulation hiatuses and determine how atmospheric characteristics during hiatuses differ from baseline circulation patterns. Finally, if you want to relate BFLs to regional climate patterns, you should at least consider running correlations between BFLs in your ice core and different ERA5 variables. This will add a lot more specificity and detail to the discussion overall. Lots of this can be done in 10-15 minutes using online web apps. I use <https://climatereanalyzer.org>.

We are encouraged by the faith in our study! However, given our response to comments above about lines 309-310, we think we still do have to hedge a bit here, and we think the next best step is to switch to the mechanistic investigation of formation using the snowpack model.

In addition, an in depth study of accumulation at the Law Dome and Mount Brown South ice core sites during different synoptic types/situations is currently well-advanced and will be submitted elsewhere (Udy et al., in prep). This work will be very complementary to this study, and will certainly aid in the development of the BFL proxy, so we would prefer to develop this work in depth as a standalone manuscript, rather than add it as a small section here.

Line 465-469: This feels a little more like a thesis proposal than a paper. All you have to say is that if the mechanism behind BFL formation can be determined (perhaps through modeling) then they may have potential as a climate proxy.

We will reduce/clarify this section, however we would prefer to keep some of the discussion of the snowpack model, as we are actively working on using SNOWPACK to investigate BFL formation.

Conclusions: Since all of this repeats from previous material, I suggest removing, greatly reducing, or bolstering the discussion with additional information.

We agree. We will reduce/clarify this section.

Figure A2: This figure is very important – at least panel AH, which you conclude is most meaningful. I suggest putting this into the main body of the paper. This is potentially true for A3 and A4 as well. Also, not having the Udy et al. 2021 paper on hand, I wouldn't mind a plot showing the SOM1 and SOM2 patterns, at least in the appendix.

We will move figure A2, A3 and A4 in the main body. We will also add a schematic plot describing SOM1 and SOM2.