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

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

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.

Line 97: Need space before reference

Line 126: Pixels per image? Pixels per cm^2?

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.

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

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.

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 you 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.

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.

Lines 212-217: Excellent!

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.

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.

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.

Line 256: TIS or tis? DT1 or dT1?

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?

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.

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?

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

Line 330: Should be Table 4.

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

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.

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.

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

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

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.

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.

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

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.