First, my apologies to the authors of this paper for my slow reply to their changes. This revised manuscript now reads much more clearly and I appreciate the work done by the authors to address the reviewers' comments. I find it greatly improved and the authors have addressed most of my concerns.

I read the insightful comments of reviewer #2 with interest, as well as the reply by the authors. I'll leave it to the reviewer to do decide if their concerns have been addressed and focus on my own.

It may be that I am simply benefitting from a second close reading, but I found the manuscript significantly less laborious to understand after revision. Nonetheless, there are some aspects that remain opaque or bothersome for me.

I do think this paper should be published, pretty much in its present form. The results are interesting and important and are now presented in a comprehensible fashion. However, the interpretation of the data is very strongly driven by one site (GISP2). NGRIP seems to have an intrinsically small signal-to-noise ratio and Dome Fuji simply doesn't have a sufficiently rich dataset for subtle interpretation. Furthermore, the only way to explain the GISP2 data requires that a fractionation mechanism expected to apply to microbubbles must also apply to some other form of trapped air (whatever that may be). All of these concerns leave me very cautious about the robustness of these results. Nonetheless, they should be published so that other investigators can try to replicate or expand on this work. I do not need to see this manuscript again before it is published (or isn't).

Below are several substantive comments, followed by some very minor corrections.

Throughout, I would like to see a different designation for the two processes that are defined here. As the work is presented, it gradually becomes clear that the "microbubble/pressure-sensitive" process is not limited to microbubbles and the "normal bubble" process is very similar mechanistically and is also pressure sensitive. The big difference is really where the bubbles form. The "normal" bubbles form deep in the firn, so they experience only modest pressure increases before being isolated from open porosity. The microbubbles form in the shallow part of the firn column, and thus many of them become very highly pressurized (and in turn, highly depleted in the small, mobile species). That said, it seems unlikely that there is enough air in genuine microbubbles to explain the signals seen in the cores, so the authors invoke "other air" to explain the strong correlations to accumulation and temperature seen at GISP2. I suggest the two processes be renamed throughout the paper. Something like "shallow-trapped" and "deep-trapped" would be more generally correct and less confusing. It's fine to point out that normal bubbles are deep-trapped and microbubbles are shallow-trapped, but in the end, shallow-trapped applies to much more air than we would expect to find in microbubbles alone.

I'm still unhappy with Figure 9 (formerly Figure 8), panel C but at least I understand it now. The left axis should be labeled "fraction of air retained in microbubbles (%)" or something like that. The existing label is very confusing.

Page 22 lines 8-11 are very confusing and I wonder if the problem is deeper than just language. In these lines, you claim that normal bubbles are compressed (i.e. their volume is reduced) as they move deeper.

This is a clear statement, but it's true for *any* bubble (normal or micro-) unless clathrates are being formed (which is not what you're discussing here). You then say this leads to "generally smaller pressure build-up". Smaller than what? Isn't it all just governed by the ideal gas law? If the normal bubbles aren't at high pressure, it's only because they were formed close to the firn-ice transition. Or is there something more here that I'm missing?

Page 11, line 9: Your reference to datasets is a bit vague. When you say "we consider the Ar/N2 as the original values before coring" do you mean "we treat Bender's data (all points averaged together for each depth) as the true Ar/N2 values before coring." Please make this clearer.

Various minor corrections I stumbled across while reading:

On p3 line 21, should read "depletions of *these smaller* gases in the…" I think.

Page 5, line 7 should read "Dome Fuji data *are* new"

Page 5, line 16 should read "less than that *of GISP2 (0.24m* ice/year) over the…"

Page 10, line 20 should read "in ice *is* often depleted…"

Page 13, line 3: For clarity, please begin the paragraph "*The subset of the* GISP2 data *covering* the past 4000 years…"

Page 14, line 14 should read "are time, lag for temperature and lag for accumulation rate, respectively (all in years)."

Page 14, line 18-19 should read "Temperature records derived from d18O_ice can be quite noisy but stacking several d18O_ice records can substantially improve the derived temperature histories (White et al., 1997; Kobashi et al., 2011). Thus, we stacked…"

Page 15, line 4 should read "…as the one with the temperature and accumulation rate records for the last 4000 year based on Ar & N2 isotope values, but does slightly…"

Page 15, line 13-14 should read "The *observed* dAr/N2 variations…"

Page 23, lines 20-12 should read "…have higher correlation with temperature (r = 0.97) than with accumulation rates (r = 0.57) in the model (Table 3)."

Page 26, line 15 should read "is the carrier of the"

Page 27 line 6 should read "…changes. We note that dAr/N2…"

Page 29, line 2 should read "in each parameter in ice cores"

Page 31, line 3 should read “Several lines of evidence indicate”

Page 31, line 16 should read “We are grateful to G. Hargreaves…”

Page 31, line 18 should read “Polar Research for supplying ice core information.”