review of Young et al, “observed microphysical changes in arctic mixed phase clouds when transitioning from sea ice to open ocean”

This paper reports an experiment where an instrumented aircraft made detailed measurements of cloud and meteorological parameters in a widespread mixed-phase stratocumulus deck, contrasting the properties over the ocean, over the sea ice, and the transition zone between the two.

I felt that the paper was very well written, clear in its aims and in presenting the data (which can be challenging when presenting a complex case study). It is a valuable contribution to the arctic mixed-phase literature. I recommend publication, and have only a few minor suggestions for changes below.

1. Abstract – you contrast the cloud droplet concentration between sea ice and ocean as 80/cc over ice vs 90/cc over ocean.
   First point – this is not consistent with section 4.11 and 4.12 where you say there are 90/cc over the sea ice and 70/cc over the ocean. Please check.
   Second point - Do you think this difference is statistically significant? There is a lot of variability in this number over each run.
2. Abstract & elsewhere. “Evidence for crystal fragmentation” – I felt this was a bit speculative. The concentrations are clearly higher in this run as you say, but you don’t really show direct evidence that this is due to fragmentation – it’s just an argument based on the crystal habit and the fact that we are too cold for Hallett Mossop. It would be good in the main text to bring in a bit more of the relevant literature on breakup (Griggs and Choularton etc?). The other thing that would add weight to this argument would be to show any CIP-15/CIP-100 images which look like broken arms of dendrites.
3. Section 2.1 you say “all data are expressed as ambient with no standard temperature and pressure corrections applied”. It wasn’t clear to me what this meant, what kind of corrections did you not apply?
4. Section 3 – you say “In this study, the MIZ is not distinctly defined” and then proceed to give a very clear definition for what you are choosing the MIZ to represent. I think you could rephrase this.
5. Section 3.1 – I was a bit puzzled by the dropsonde data. Why are the peak RH values so low, when there is clearly liquid cloud present (hence RH=100%, or very very close)? Many sonde RH sensors have a small dry bias, but in figure 3D the cloud layers seem to have a peak RH of only 80%. Could you comment on that (in the text)?
6. section 4.1.1 – “subtly detected” – could phrase this better
7. section 4.1.1 – rosettes & aggregates fallen from Ci cloud above. Very interesting – can you say whether these are likely have to survived far enough to actually precipitate into the MPS layer itself (seeder-feeder arrangement)?
8. last part of section 4.1.2 – would be good to tell the reader at this point what they should take from the figure showing the evaluation of D10
9. section 4.2 and elsewhere. You talk about a “fog layer” here, but then of the droplets as “swollen aerosol particles”. I think it’s important to be clear throughout the paper

what this layer is – is it haze (ie unactivated solution droplets) or is it fog (activated cloud droplets)

10. section 4.3 and 2.2 – I think it is better to use a phrase like sea ice fraction rather than "ice concentration" (which makes reader think of snow particles in the clouds)
11. section 5.1 – talk about cirrus cloud and then "the possibility of another higher cloud layer" – makes it sound like there is a cloud higher than the cirrus, when in fact you are suggesting one in-between I think. Rephrase.