I appreciate the review of this paper provided by Dr. Baumgardner. He raises several important points and I have tried to address them in my reply and with additional or clarifying language in the manuscript. In the following, I copy each of his comments and follow that with a reply.

- Line 76: "Nd (re) has uncertainties of 0.16(0.16) and 0.55 (0.18), respectively." What are the units/dimensions of these uncertainties? They can't be in units of cm-4 and they are too small to be percentages. Can you explain?

Reply: These are fractional uncertainties that would be realized in the idealized situation where we have 1 meter and 5 meter lidar vertical resolution. I agree that the values for 1 m are optimistic since other uncertainties would become dominant. But a 55% uncertainty at 5 meters for Nd seems reasonable in my mind and somewhat startling since most lidars don't collect data at this resolution. This would mean that for a 100 cm^{-3} value for Nd the error bar would range between 55 and 155. The cloud top effective radius uncertainty on the order of 15-20% is quite substantial given the typical dynamic range of this quantity (8-15 microns for most non precipitating liquid clouds). I've attempted to add a bit more nuance to the text to explain this.

- Line 211: "Observed thermodynamics". I am assuming that this refers to soundings that document the T/RH vertical profiles? I return to this question further down when I ask for more information on how the uncertainty in this profile impacts the derived Nd.

Reply: Correct. I have added a bit of clarifying text to this statement in the revision.

- Line 229: "For the prior estimate of Nd, we reason that coincident cloud condensation nuclei (CCN) measurements provide an upper limit on the droplet number in each situation.". This requires additional discussion because the CCN measurements are only relevant as an upper limit to Nd if the cloud base temperature and maximum updraft velocity is known, since the number of droplets activated depend on the CCN spectrum of concentration versus supersaturation (SS). Two sentence further a value of 0.2% was mentioned, but where did this number come from? Unless I overlooked it, no where in the article are vertical motions discussed. Yes, given the cloud base T/P you can estimate maximum LWC but not maximum % SS. Given the very nice correlation between the in situ measured Nds and those extracted from the remote sensors, maybe this is a moot point. Perhaps 0.2% is a good guess for the clouds studied on the Southern Ocean; regardless, a bit more discussion about the properties of the CCN in this region would be useful with regard to the conditions that activate them.

Reply: We found that the use of CCN as a broad constraint on the iterative algorithm was necessary to achieve convergence and agreement with independent data. As we point out in the discussion of the Kx matrix (Jacobian) and elsewhere in the paper, the amount of information from the lidar regarding Nd is just barely sufficient for our purpose and CCN provides a useful and necessary starting point. I chose to use CCN at 0.2% based on a figure in McFarquhuar et al., 2021(Figure 5) and from discussions with colleagues who have worked with the Socrates data. We also performed our own analysis of the cloud probe and CCN data and found that 0.2% had slightly better correlation than 0.3. Of course, at a microphysical level the magnitude of the updraft also matters as you point out. However, the updraft velocity is a quantity that we cannot know form surface-based remote sensing at sea. However, I don't think this is a critical point. We simply require a consistent starting point for the iterative algorithm. The CCN are always an upper limit on Nd and most of the time the algorithm converges on a value 2/3 or ½ of the CCN especially in drizzle. I have added clarifying text to the revision on this point.

- Figure 4 and line 316. "Ramp" is a term that I rarely see when aircraft measurements are conducted. I understand the intent but thin a single sentence that explain that a "ramp" is when the aircraft does a vertical profile through a cloud. What isn't clear is if these are multiple passes through a cloud at different altitudes or a constant climb or descent?

Reply: Clarified in the revision. These are approximately constant rate climbs or descents from base to top.

- When looking at the uncertainty analysis, I was unable to tell if uncertainty in fad and the associated uncertainty in derived adiabatic liquid water is included. The adiabatic LWC is sensitive to the LCL, i.e. cloud base temperature and pressure. These can vary throughout the day and even from initial values derived from radiosondes. How does this uncertainty impact the subsequent uncertainty in Nd and re?

Reply: Yes, these are factored in via the adiabatic liquid water lapse rate. See this in the expanded derivation that I included in the revision just below what is now equation 15. The adiabaticity of the layer is a critical parameter since it essentially tells the algorithm what the liquid water content (LWC or q) is at the point where the lidar attenuated backscatter reaches a maximum. This 2nd piece of information, the LWC, allows us to solve the equation for Nd.

- My last point is trivial but from my perspective as one who provides the community with instrumentation I would like the model and manufacturer listed along with the instrument discussed. The author explicitly mentions the Vaisala ceilometer but not the manufacturer or models of the micropulse lidar (MPL) and cloud droplet probe (CDP). I ask that these are added.

Reply: I appreciate the need for this. I have added this information to the extent I could find it in publications and reports. Where I wasn't sure, I added references. We could not do the science without these advanced instruments even though sometimes we errantly take the effort of the developers of these instruments for granted.