Review of Evaluation of the EarthCARE Cloud Profiling Radar (CPR) Doppler velocity measurements using surface-based observations, by Kim et al.

This paper presents the first ever evaluation of the first ever 94 GHz Doppler measurements in space. In that regard, this paper will become a very important reference paper once published for a very large community of EarthCARE users. The paper is extremely well written, and the evaluation presented is thorough and convincing. I only have comments aimed at improving the description of the evaluation results, and I suggest the paper should be published once these relatively minor comments are addressed.

Comments:

1. Section 2.1: since you have to do do the same thing for both the ground based and spaceborne measurements, I don't really see the point of describing the relationship between terminal fall speed and Doppler velocity. In ground – satellite comparisons, you would find that the errors on Doppler are the same as the errors on fall speed, pretty much (that is also what is discussed in lines 110-113 by the authors themselves), and as a matter of fact, you don't compare fall speeds in the results section. Where is the added value of that section?
2. Line 124: I think it would be nice to describe in one sentence how this Kollias (2019) correction works. Also, I assume you are not correcting Doppler for the frequency difference? Maybe it would be worth adding that because as I was reading the results section, I wondered.
3. Line 130 (and line 165): what is the justification for 9 ms-1 ? Can't you use vertical profiles of horizontal winds that are changing from one day to another to be more accurate with this time – space conversion (wind profiler, Doppler lidar, radiosonde interpolations at the ARM site, even NWP, etc …). I wonder how much difference it would make.
4. Lines 155 – 159: Isn't there a contradiction here? Polar environments are indeed known to host the highest frequency of occurrence of mixed-phase and supercooled clouds that attenuate the W-band signal, but you start this paragraph by saying the opposite.
5. Line 163: So you use a 00km radius around the site and you develop some statistical corrections, which is fine. I have two comments / suggestions about that. Do you also have very close overpasses? Would it be useful to do a separate analysis of these near perfect collocations? Also, I was wondering if you could show comparisons of CFADs of reflectivity (after using the radar simulator) to make sure you are working with similar enough cloud statistics?
6. Figure 1: How close to the site is the closest profile of that figure and where is it on the cross-section ? I think there would be value in adding a panel where you compare the closest profiles of reflectivity and Doppler velocity together too (if the satellite was very close).
7. Lines 229-230: Are you stating that by looking at lidar data within these clouds ? The problem is that the lidar measurements are often extinguished before reaching these clouds by low-level liquid layers. My experience from the Southern Ocean tells me that this statement is not correct and that the impact of liquid attenuation cannot be discarded so easily in such comparisons.
8. Line 231: Removing clouds using bright band detection is a good idea, but when using this you are excluding precipitating cases with liquid and ice phases (frontal systems), not SLW or mixed-phase clouds.

9. Analysis of Figure 2 results: I think another important (and remarkable) point to make from this figure is that the standard deviation is very similar at all heights where you have enough samples. Doppler velocities from satellite are not only virtually unbiased estimates but do capture the Doppler variability accurately. I would highlight that too.
10. Line 346: So just to be sure, you are not trying to correct for frequency-induced Doppler differences in the radar simulator? This should be clearly explained both in the description of the simulator and maybe here as well.
11. Lines 370-371: that's really a missed opportunity not to compare cloud results for LWP >100 from that site. That was the perfect missing piece of information to infer if the differences you see at NSA are really due to the frequency difference or any other issue! I'd recommend doing that for sure or explore other sites where you have more data like that at 94 GHz. The reader here is left wondering if there's a problem or if that's just a limitation of these comparisons, but then if it is just a limitation, it's not a very interesting evaluation.
12. Figure 5: do you still use the 3% of total number of data points to define what is statistically significant or not ?
13. Line 385: Related to my point 11 above, the problem with this comment is that you have the difference in Doppler due to different frequencies that could mask some other issues. This is why it would seem quite important to do the comparisons at 94 GHz with a ground-based radar for LWP > 100. Also it would seem important to compare reflectivities too to document any statistical effect from liquid attenuation in these comparisons.

Good luck with the revision,

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20/07/2025.