"Microphysical variability of Amazonian deep convective cores observed by CloudSat and simulated by a multi-scale modeling framework" by Dodson, Taylor, and Branson.

Summary:

The authors use the Amazon as a testbed for assessing the internal structure of deep convection observed by CloudSat. Deep convective cores are shown, through a "double arc" structure in CFADS, to be composed of either highly reflective graupel and hail or weakly reflective snow. Cloud structure is contrasted between day/night and wet season/dry season to modest effect. The authors then compare their CloudSat results with those from two SP-CAM runs. These simulations are conducted with different versions of the model which results in the simulations of differing cloud structure between the simulations themselves and between the simulations and CloudSat. The authors report new results, but these are incremental. There are several aspects of the paper that need improvement: 1) the "double arc" is not plainly obvious yet the authors make a point of discussing it at length; 2) the analysis of the simulations seems to lack an obvious direction. I would recommend acceptance if the issues below are addressed.

Primary items:

1) The "double arc" is not especially obvious in any of the panels of Fig. 3. It took me quite a while to fully recognize what structure the authors were talking about and to convince myself that it was not just a result of the contour intervals used. I'm not sure what the remedy is for this, but the double arc structure needs to be made clearer through either some enhancement of the figure, a schematic, or particularly lucid writing.
2) I don't understand why the authors feel they can ignore graupel in SPV4. The model seems to include graupel to the same degree that it includes any physical species. It seems to be just as much a part of the precipitating ice category as snow.
3) I don't think you have shown sufficient evidence to draw the conclusion you do on Line 285 (even if we all hope that this conclusion is true). Figure 6-9 show only that SPV5 behaves more logically. We do not know how the real world binned variables (reflectivity, SWC, etc) depend on W_{max}. And, I'm not sure I agree that the SPV5 CFAD is more like the CloudSat CFAD than the SPV4 CFAD; they share more characteristics with each other than they do with CloudSat. Perhaps you could add the difference between the CloudSat CFAD mean and those from both model runs to Fig. 5d. Or maybe you could compare the variance at each level.

Other items:

Line 34: But Fu et al (1990) uses passive sensors. It should be made clear that the second two citations use models.

Line 42: "There exist lidars" is awkward phrasing.

Line 57 to 63: These lines describe the most significant impact this paper will make several pages later.

Line 88: You might want to rephrase “temporal data domain”.

Line 107: Up to this point, you have not explained what scientific purpose you have in focusing on DCC. Is there a reason we expect Amazonian DCC to be especially microphysically variable?

Line 110: You may want to make it clearer that Fig. 2 does not use your DCC selection.

Line 122-123: These two sentences seem contradictory.

Figure 3a: What does “Quad” signify?

Line 150: How do you know that neglecting to separate the data is the reason the double arc has not shown up before? It seems like you could show your selected-CFAD and an all data-CFAD.

Line 167: I’m not sure what the author is implying through the end of this paragraph.

Figure 3j: The standard deviation of what? Or is it the difference of the standard deviations?

Line 192: Why not show Wet-Dry differences in an additional column (like PM-AM differences are shown in an additional row)? Well, OK, the authors answer this question by the end of the paragraph by punting on seasonal differences. But if they want to do this (which they can), they should probably change the title of section 3.3 to just “Day versus night variability”.

Line 220: How was the CRM sampled? Did you sample individual CRM grid columns in a way similar to CloudSat’s sampling (i.e. approximately north-south at 1:30am/pm)?

Line 226: I’m confused about what the authors mean throughout this paragraph. In SPV4, according to Wang et al (2011), SAM diagnoses all its microphysical species from predicted precipitating and non-precipitating water. Why do the authors feel they can “disregard diagnosed graupel” (and by that I assume they mean when running the radar simulator) and not, say, snow which seems to be diagnosed in the same manner as graupel? It seems like the authors should include graupel in their calculations and change their conclusion that SPV4 underestimates graupel. This would also make the paragraph beginning on Line 230 unnecessary.

Line 238: Except SPV4 does “represent” graupel.

Figure 5: why use the dry season when the wet season produces the more significant “double-arc”? The vertical velocities seem oddly low in magnitude. There is barely any weight to the PDFs above 1m/s (which would usually be considered “convective” in a CRM).

Figure 6: Realistically, I think one has to question the sampling at high W_{max} when there are intermediate bins with zero samples. All the panels should probably be cutoff at ~3m/s. What are the different colored lines in panel (c)? Also, the label on the y-axis should probably be something like “fraction” rather than “count”.

Line 264: This discussion of the “jump” needs to be clearer.

Line 270: Or this logical thread about how the double arc structure might arise isn’t correct. I don’t have a different hypothesis to offer, but the reasoning seems appealing but unproven.