This paper presents an interesting analysis of anvil cloud dynamics using data from the EarthCARE Doppler radar and lidar. It is a nice conceptual demonstration and use case of these new measurements, and the treatment of the radar retrievals seems sound. The end result of the analysis is the derivation of a 2D wind field using vertical velocity retrievals and mass conservation. This derivation involves some big assumptions, one which is clearly described ($w_{a,a} = 0$) and another that is not addressed (comment 1). I credit the author for appropriately contextualizing the circulation result as a "proof-of-concept" rather than a precise analysis. Nevertheless, the analysis could be significantly improved with some very changes to the circulation derivation and core identification. If the circulation analysis is going to be done, it seems worthwhile to make it more realistic than its current form.

The paper is well organized and the communication and figures are clear. Altogether, this is exciting work that lays important groundwork for the future use of EarthCARE data for cloud-climate studies. The paper will be well-suited for ACP, and I anticipate fully supporting its publication once my comments are addressed.

Comments

1. Equations 2, 3, and 5 assume incompressible flow, which does not seem like the appropriate form of mass conservation for this case, since density variations at the scale of the circulation are large (factor 4-5). The anelastic form with a fixed reference density profile seems appropriate:

$$\frac{1}{r}\frac{\partial}{dr} r\rho_0(z)u_r(r,z) = -\frac{\partial}{\partial z}\rho_0(z)w_a(r,z) = D$$

The density term should lead to noticeable changes in Fig 7b-d. The circulation directionality shouldn't change, but the wind speeds in the upper (outflow) branch should increase relative to the lower (inflow) branch. This fix should be cheap and easy to do computationally, and seems important since this section will hopefully be used as a basis for future work.

2. While the assumption of $w_{a,a} = 0$ is discussed very clearly, there are alternative choices that are equally simple but much more realistic. For example, a simple fixed profile of $w_{a,a}(z)$ derived from a typical radiative cooling and lapse rates under WTG assumptions ($w_{a,a} = Q_{rad} / (\Gamma_d - \Gamma)$). As the author notes, this would mitigate the issue of very strong wind speeds at large r in Fig 7d.

An alternative would be to assume that the net mean tropical ascent is negligible, such that the Doppler-estimated in-cloud ascent (scaled by cloud fraction) must be exactly balanced by clear-sky subsidence at each vertical level. The downside of this option is the assumption of a closed circulation, but with an entire year of measurements, that might not be unreasonable.

3. I am a bit confused about the compositing procedure described on lines 230-233. Since the retrievals are on a 1-km horizontal grid and since R_c = 1 km, a single isolated IWP measurement above 1e3 g/m2 is considered to be a convective core and therefore has r=0 by definition. So, how would columns with IWP>1e3 be found "outside ±R_c"? I might be misunderstanding the procedure in which case clarification would be useful. Also, is each non-core column is assigned an r value based on the *closest* convective core? It would be good to state that explicitly, since the closest core is not necessary the actual source of the anvil cloud.

Also, the chosen value of R_c = 1 km is less than the 10-km averaging length used in the retrievals (section 2.1). Does this have any important implications? If you anticipate the averaging having an impact on convective core identification, it would be useful to note. If not, please disregard.

4. (Lines 215-220) The "curtain" geometry of the CPR-ATLID measurements means that a column may appear very far from convection when there is actually a core nearby that does not intersect the satellite track. So, while the distance-from-core metric is certainly more "spatial" than IWP, it is not clear to me how much more meaningful it is. Ideally, the coincident multispectral imager retrievals would be used to identify convective cores using cloud top height or other variables in those retrievals. Using the MSI to search for cores in the across-track dimension would be a clear improvement; ultimately, I leave it up to the author, since it is "a more significant undertaking; but at the very least, this large source of bias in distance-from-convection should be discussed.

5. From the reader's perspective, there are a few points made throughout the paper that I suggest consolidating into a new paragraph in the Introduction of Data section. I had these questions as I was reading, and while answers are eventually provided, it would be helpful to have that information at the beginning. Those points are:

- How does the addition of the ATLID constraint impact the retrieval? Is the impact limited to parts of the cloud detected by both instruments, or does it propagate throughout the entire column? This is nicely addressed already on lines 193-197.
- Why it is scientifically and/or technically beneficial to compare the CPR-only and CPR-ATLID retrievals, when one could otherwise argue that the CPR-ATLID retrieval should be used by default for research purposes since it has an additional constraint. Several good reasons are given throughout the paper already, but stating this concisely at the beginning would help motivate the analysis.

Minor/Line Comments

L6-7: I suggest "radar-only product" and "radar-lidar product" instead of "CPR-only" and "CPR-ATLID", since ATLID has not been defined.

L15: I suggest explicitly writing the tropical high-cloud fraction from Stubenrauch et al 2024, since it is always ambiguous whether a % change in cloud fraction refers to fractional or absolute change.

L26: "through ~~the~~ lower"

L28: working -> workings

L40: "succeeds ~~to~~

L90: (optional, since the ESA retrievals are not actually used in the paper) Some more details about this assumption would be useful here for those who are unfamiliar, e.g., what length scale is necessary to "average out" the vertical velocity? Can this approach only be applied to certain environments (e.g., within convection the vertical velocity would not average out to zero)?

L122-123: is there an approximate optical depth below which MS tails become much less likely? While most anvil cirrus have low/moderate optical depth, fresh anvils adjacent to deep convection are optically thick and precipitating.

Fig 1 caption: "longitude-height" -> "latitude-height"

Fig 1: It would be helpful to have a black contour on the CPR-ATLID panels that shows the portion of the cloud seen by both the radar and lidar. It may be too noisy and obscure the underlying data, in which case an example as a supplemental figure could be a nice option

Section 3.1: It would be nice to have plots showing the difference between the CPR-only and CPR-ATLID products as supplemental information (if ACP allows).

L155-161: I agree that the magnitude of the difference in IWP histograms between the two products is quite remarkable. Another aspect of these histograms that surprised me is the maximum IWP value of ~2e3-3e3 g/m2. This is almost an order of magnitude lower than the maximum of 1e4 g/m2 in DARDARv2, DARDARv3, and 2C-ICE (see Fig 3a in https://doi.org/10.1029/2023GL105868, for example). This might be an interesting difference to note between EarthCARE and the other retrievals commonly used by the community.

Line 169: plote -> plot

Fig 3: it would be visually useful to change the colormap in panel c to match the brown-green scale used for the other velocity fields in panels b and d, so that the three dominant terms in Eq 1 are all on the same scale and can be easily compared.

L183-185: “Doppler velocity….when even only a handful of hydrometeors are large enough…” – is there a way to be more precise here, or a reference that could be provided? This might be obvious to the remote sensing community, but it comes off as a bit hand-wavey.

Fig 5: It would be useful to overlay the cloud fraction contours from Fig 4 here, especially for panel d, to provide a reference for which values of r_e,2 are frequent and which are less common.

Line 295: While it should be obvious to most readers by this point of the paper, I suggest noting here that only *composite* horizontal winds can be derived, since the mass continuity assumption relies on having a large enough sample size for the sampling geometry of individual clouds to be ignored, i.e. to assume isotropic sampling.