Reply to Anonymous Referee #1

I deeply appreciate the reviewer's insightful thoughts and constructive suggestions. I revised the manuscript and updated figures to address the comments as summarized below. I am especially grateful to the reviewer for the first two comments, which I believe greatly improved the methodology to construct the composite wind field. The reviewer's comments are repeated in blue, followed by my response in black along with additions/changes to the revised manuscript cited in red.

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.

Reply: The composite analysis for Section 4 has been entirely redone with the anelastic continuity equation. The methodology section (Section 4.1) was updated accordingly. The resulting plots (Figures 9 and 10) will be shown in response to the next comment.

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 wa,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.

Reply: This is a great suggestion. In fact, the first option (radiatively driven subsidence) came to mind months ago when I was playing around with experimental design, but the problem was that I didn't have any handy observation dataset of Q_{rad}. I plan to get back to this idea as a next step with JAXA's EarthCARE radiation product.

For now, I decided to adopt the second suggestion (closed circulation model). Figures 9 and 10 of the revised paper presents the results for the original zero-ambient-motion model and the closed circulation model, respectively.

Figure 9: Zero-ambient-motion model.

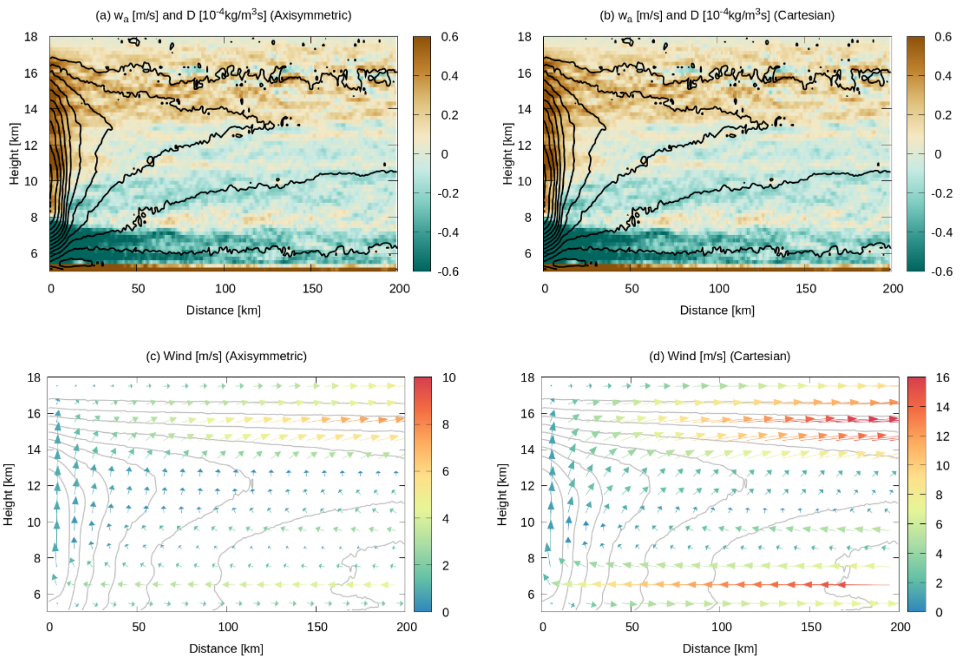
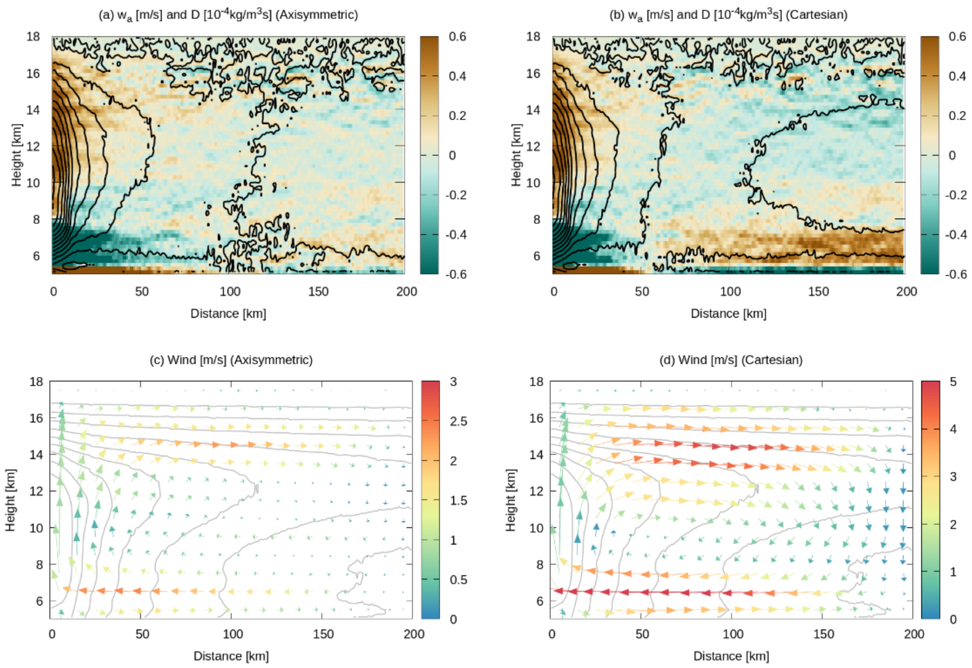


Figure 10: Closed circulation model

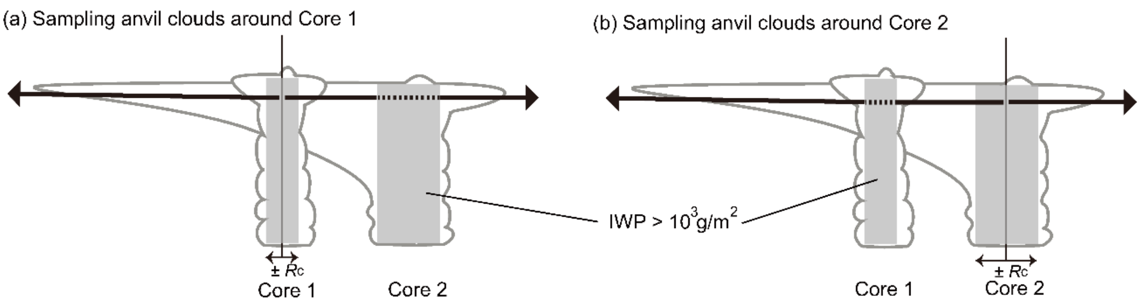


3. I am a bit confused about the compositing procedure described on lines 230-233. Since the retrievals are on a 1-km horizontal grid and since R_c = 1 km, a single isolated IWP measurement above 1e3 g/m2 is considered to be a convective core and therefore has r=0 by definition. So, how would columns 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.

Reply: A new paragraph with a schematic diagram (Fig. 7) was added to Section 4.1 to clarify the methodology:

Figure 7:



"It is noted that this horizontal distance is not necessarily the distance to the nearest core if multiple convective cores exist in vicinity (a few such examples may be found in Fig. 1). The search over horizontal distance continues up to ±200 km from the origin whether or not neighboring cores are met, with the caveat that the interior of the neighboring cores is not sampled. This caveat is intended to exclude external convective cores that happen to be present nearby from the compositing, as schematically illustrated in Fig. 7. In this schematic, an isolated convection (Core 1) intrudes into an anvil cloud belonging to another convective tower (Core 2). The current sampling procedure may properly attribute the anvil cloud to its parent core (Fig. 7b) or may not (Fig. 7a). Both cases are included in the composite statistics because it is generally difficult to unambiguously discern in actual observations which core is the source of a given anvil cloud.

Nadir observations from EC-CPR and ATLID are unaware of convective cores away from the sub-satellite track, potentially distorting the composite statistics of core-anvil pairs to a certain degree. As a strategy to overcome this problem, it may be possible to locate off-nadir cores in infrared imagery using the whole swath of EarthCARE Multi-Spectral Imager (MSI). This approach, however, has its own limitation that infrared radiance is not optimal for singling out convective cores especially when the cores are embedded in extensive high clouds. No attempt is made in this work to detect off-nadir cores for the sake of methodological simplicity. It is not attempted either for the same reason to put the sampled snapshots into the context of convective life cycle, although several sophisticated methods have been explored in the literature to monitor the anvil cloud evolution over life stages (e.g., Wall et al., 2020; Horner and Gryspeerdt, 2023).

Given all these limitations, it should be noted that the averaged cloud structure in vertical-horizontal space may be a statistically noisy representation of anvil clouds. Nevertheless, a distance-based (instead of IWP-based) composite analysis is crucial for the purpose of deriving diagnostic horizontal wind through the mass conservation law."

As for the 10-km averaging, it might slightly smooth the IWP field but this would not bring about any substantial effect on the current analysis.

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.

Reply: Yes, the CPR scan geometry limits the ability to sample core-anvil pairs. This is addressed in the second paragraph of the quote cited above.

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.

Reply: Section 2.2 has been newly added as a new section describing JAXA's EarthCARE cloud products. The first paragraph briefly outlines the retrieval algorithm. The second to forth paragraphs, cited below, are intended to accommodate this suggestion.

"The CPR-only and CPR-ATLID synergy products contain the same sets of cloud physical variables, but estimated values are not identical for two major reasons. First, the CPR-ATLID algorithm updates the ice habit model identified by CPR alone in effort to optimize the cloud-type classification with all available measurements. Second, the radar-lidar synergy product can exploit an additional constraint on PSD where radar and lidar signals are both available, because the particle-size dependence of cloud back-scattering is systematically different between microwave and visible wavelengths (Okamoto et al., 2003).

Lidar pulses are so susceptible to attenuation that back-scattered lidar echoes are often lost entirely deep inside a thick cloud layer. The synergy impact is thus expected to be efficient near cloud top but is not necessarily confined there because the cloud PSD has control over the extinction of radar and lidar echoes as well as their back-scattering efficiency. Any difference in PSD parameters hence not only locally influences the retrieval but also in the layer immediately below through attenuation correction. A consecutive chain of this effect could eventually reach all levels beneath, leading to the propagation of cloud-top information deeper into the cloud as will be confirmed in Section 3.3.

Any systematic bias found between the CPR-only and CPR-ATLID estimates is attributed mainly to differences in the algorithmic assumptions, e.g., PSD models, between the products. As such, comparing the two products offers an incomplete but useful measure of uncertainties that could be refined by further updating the assumptions. The EarthCARE cloud products are ultimately to be assessed against in-situ measurements. Several field campaigns have been conducted and are being planned for EarthCARE product validation (Kubota et al., 2026)."

Just a side thought: One might naively expect that the radar-lidar synergy product is "better" than the radar-only product because the former enjoys additional observational information unavailable for the latter. This is true in a sense, but at the same time a more complicated algorithm structure could induce an extra source of error that is hard to track down. For example, the TRMM and GPM missions provide "radar-only" and "combined radar-radiometer" precipitation products, but after years of assessment efforts we are still unsure which is more reliable than the other. But comparing the two products at least gives us some measure of uncertainty, without knowing exactly where the truth is.

Minor/Line Comments

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

Reply: Corrected as suggested.

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.

Reply: The sentence now reads as follows.

"While the climatological mean high-cloud amount is about 0.37 when averaged over the globe, this number increases to nearly 0.5 at tropical latitudes (Stubenrauch et al., 2024)."

L26: "through ~~the~~ lower"

Reply: Corrected.

L28: working -> workings

Reply: Corrected.

L40: "succeeds ~~to~~

Reply: Corrected.

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)?

Reply: In ESA's cloud products, spatial averaging is applied along the track successively over four selected windows of 40 km, 30 km, 20 km, and 10 km in this order, according to Kollias et al. (2023). This information is added to the revised manuscript.

As a side note, ESA's goal with EarthCARE Doppler measurements appears to be sedimentation speed, rather than in-cloud air velocity. In fact, ESA's cloud product does not provide vertical air velocity, as it has been separated out by an upstream module in the L2 algorithm flow. Vertical air velocity is more like a ``noise'' to be removed for them. This contrasts with the JAXA product's design concept.

To my knowledge, the averaging is done universally regardless of environments. As the reviewer pointed out, vertical air velocity wouldn't be smoothed out to zero for convective clouds, in which case this algorithm would introduce some error in the sedimentation speed estimates.

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.

Reply: I don't know of any specific optical-depth threshold for MS tails, but it should be so high that MS tails are limited to above deep convection. Figure 5 of Battaglia et al. (2023, https://doi.org/10.5194/amt-14-7809-2021) offers a demonstrative example for that.

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

Reply: Corrected.

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.

Reply: The snapshots comparing two cloud masks (CPR-only and CPR-ATLID) were added as a supplement figure (Figure S1, included also in the end of this document).

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

Reply: The suggested plots are now included as Figure S2 (also in the end of this document).

This new figure made me realize that inter-product differences can be found deep inside clouds for at least two reasons. First, the attenuation correction could make any microphysical difference propagate from upper to lower cloud layers as already discussed (now in Section 2.2). Another possible reason is that composite statistics are comprised of various cloud samples with different heights. For instance, an upper layer of relatively low-topped clouds is likely averaged together with the deep interior of higher-topped clouds in the composite diagram. This is mentioned in Section 3.3 of the revised manuscript.

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.

Reply: The following paragraph has been inserted in the revised manuscript (Section 3.2).

"Figure 2 remotely resembles the IWP histogram from CloudSat/CALIPSO observations in past work (e.g., Sokol and Hartmann, 2020) but differs in detail. The discrepancies, while partly because observation periods are not overlapping, are likely to arise from algorithm-specific biases. Different products, or different versions of the same product, disagree among one another in the IWP histogram even for the same input observations (Atlas et al., 2024). It is noted that the histogram in Figure 2 sharply tapers off toward the high end of IWP around 3×10^3 g m^{-2}. This upper limit is significantly lower than known in the literature, well outside the inter-product spread across multiple CloudSat/CALIPSO products (Atlas et al., 2024). The reasons for this bias are unknown at this point."

Line 169: plote -> plot

Reply: Corrected.

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.

Reply: The color scale in Figure 3c has been adjusted as suggested.

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.

Reply: The sentence has been updated as follows:

"Radar back-scattering strengthens so sharply with particle size that $w_{t,Z}$ could be dominated by large particles falling out of an ascending air parcel even though such precipitating particles are much fewer in number than smaller cloud particles."

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.

Reply: Cloud-fraction contours were added to Figure 4.

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.

Reply: A caveat has been inserted there that "this can be done only in composite space where the averaging operation statistically compensates the incomplete geometry of individual cloud samples."

Reply to Anonymous Referee #2

I greatly appreciate the reviewer's careful reading and helpful suggestions. I revised the manuscript in response to the comments as summarized below. The reviewer's comments are repeated in blue, followed by my response in black along with additions/changes to the revised manuscript cited in red.

Specific comments

1. As discussed in section 3.3, the results presented are highly sensitive to the assumed particle size distribution (PSD) when deriving particle fall speeds (w_t). This is nicely demonstrated by the difference between the two JAXA wt retrievals, but does not provide much context for the robustness of the wt retrievals more generally. I suggest the following additions:

(a) Presently, the reader is directed to Sato et al (2025) and Nakajima et al (2026) for technical algorithm details for deriving w_t and w_a", but I think the method description would benefit from more detail. Could you specify which observables are used to derive the coincident PSD, what assumptions are made, and how reliable this estimate is assessed to be (including estimates of the modelled PSD uncertainty and any planned or past validation activities of the input or resulting EarthCARE products)?

Reply: Section 2.2 has been newly added as a new section describing JAXA's EarthCARE cloud products. The first paragraph of this section is as follows:

"The retrieval algorithm behind the 2A.CPR_CLP and 2B.AC_CLP products (Sato et al., 2025) is briefly outlined in this subsection. The key input observables include radar reflectivity factor, Doppler velocity, and path-integrated attenuation from EC-CPR and back-scattering coefficient, extinction coefficient, and depolarization ratio from ATLID. The cloud properties such as IWC and cloud effective radius are derived so they match the observables by running an inversion model under prescribed assumptions on particle size distribution (PSD) and ice crystal habit. The PSD model is the sum of two modified gamma distribution for frozen hydrometeors, allowing a group of large ice particles such as snow (mode 1) and another of smaller ones (mode 2) to co-exist. The built-in inventory of ice crystal habit consists of 2D plates, 2D columns, bullet rosettes, snow aggregates, and several others. ATLID lidar ratio and depolarization ratio, CPR reflectivity factor and Doppler velocity, and air temperature help identify the optimal ice habit models. Once the ice habit and PSD parameters are determined, the terminal velocity of hydrometeors is derived with a built-in lookup table so it is aerodynamically consistent with cloud microphysics (Sato et al., 2009)."

There are so many ongoing/planned EarthCARE validation campaigns that it is not practical to name them all. I instead included a reference that summarizes the validation projects in the last paragraph of Section 2.2

"Any systematic bias found between the CPR-only and CPR-ATLID estimates is attributed mainly to differences in the algorithmic assumptions, e.g., PSD models, between the products. As such, comparing the two products offers an incomplete but useful measure of uncertainties that could be refined by further updating the assumptions. The EarthCARE cloud products are ultimately to be assessed against in-situ measurements. Several field campaigns have been conducted and are being planned for EarthCARE product validation (Kubota et al., 2026)."

(b) I would posit that the ESA particle sedimentation velocity product provides a stronger indication of the retrieval uncertainty of the CPR w_t than the CPR-ATLID w_t, as the assumptions used to derive it are different, while between the CPR w_t and ATLID-CPR wt only the input products, not the retrieval algorithm, differ.

i. Could you add a small section in which you compare the JAXA (or ESA) w_t and w_a retrievals?

ii. If this is infeasible, it would be great to detail why this was not done, how you expect the retrievals to differ, and how that might impact your results.

This would be immensely helpful to your audience. The Doppler w_a product is of great scientific interest, but having three different retrievals adds to confusion over its reliability. Context on when, how and why the retrievals differ (as nicely done here for the JAXA products) would offer an excellent basis for future work and its interpretation in our field.

Reply: I agree that comparing the ESA and JAXA products would be useful for quantifying uncertainties, as they offer independent estimates from the same observations. Unfortunately, however, it is not feasible to do so for vertical air motion because ESA doesn't provide $w_{a,c}$ in their EarthCARE cloud products. The ESA algorithm is designed to first filter vertical air motion out of Doppler velocity and does not include it in their Doppler product nor pass it on to downstream products. This is now mentioned in Section 2.3.

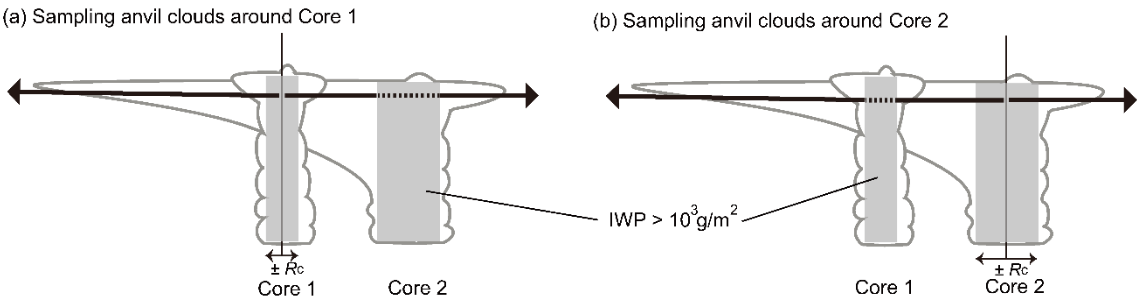
ESA's goal with EarthCARE Doppler measurements appears to be sedimentation speed, rather than in-cloud air velocity. I do not attempt to investigate JAXA-ESA difference in sedimentation speed (terminal velocity), as w_t itself is not the central focus of this work. In theory, one probably could reverse-engineer the ESA products to extract vertical air velocity that has been filtered out. But if this is not an intended use of their products, it might not be the way to go for general users.

2. Along-track composites have the potential to add important detail to analyses such as this paper compared to phase-space diagrams as explored for IWP. But they remain imperfect as they aggregate cloud lifetime, different locations in the cloud shield, both upwind and downwind anvil outflows, and multiple cores of different age. Can you please clarify how you handle multiple IWP "cores" in one track? You state that columns with IWP>10^3 g m-2 are excluded, but do you exclude parts of the track that are closer in

proximity to another core? (i.e., is it distance from **nearest** core?) It also would be nice to clarify that the composites should not be assumed to represent a typical convective cloud, as by aggregating we get a distorted picture unless more effort is made to control for differences in cloud sampling.

Reply: Yes, the CPR scan geometry limits the ability to sample core-anvil pairs. A new paragraph with a schematic diagram (Fig. 7) was added to Section 4.1 to clarify the methodology.

Figure 7:



"It is noted that this horizontal distance is not necessarily the distance to the nearest core if multiple convective cores exist in vicinity (a few such examples may be found in Fig. 1). The search over horizontal distance continues up to ±200 km from the origin whether or not neighboring cores are met, with the caveat that the interior of the neighboring cores is not sampled. This caveat is intended to exclude external convective cores that happen to be present nearby from the compositing, as schematically illustrated in Fig. 7. In this schematic, an isolated convection (Core 1) intrudes into an anvil cloud belonging to another convective tower (Core 2). The current sampling procedure may properly attribute the anvil cloud to its parent core (Fig. 7b) or may not (Fig. 7a). Both cases are included in the composite statistics because it is generally difficult to unambiguously discern in actual observations which core is the source of a given anvil cloud.

Nadir observations from EC-CPR and ATLID are unaware of convective cores away from the sub-satellite track, potentially distorting the composite statistics of core-anvil pairs to a certain degree. As a strategy to overcome this problem, it may be possible to locate off-nadir cores in infrared imagery using the whole swath of EarthCARE Multi-Spectral Imager (MSI). This approach, however, has its own limitation that infrared radiance is not optimal for singling out convective cores especially when the cores are embedded in extensive high clouds. No attempt is made in this work to detect off-nadir cores for the sake of methodological simplicity. It is not attempted either for the same reason to put the sampled snapshots into the context of convective life cycle, although several sophisticated methods have been explored in the literature to monitor the anvil cloud evolution over life stages (e.g., Wall et al., 2020; Horner and Gryspeerdt, 2023).

Given all these limitations, it should be noted that the averaged cloud structure in vertical-horizontal space may be a statistically noisy representation of anvil clouds. Nevertheless, a distance-based (instead of IWP-based) composite analysis is crucial for the purpose of deriving diagnostic horizontal wind through the mass conservation law."

Technical corrections

Title: Should it be "A preliminary analysis of tropical anvil-cloud dynamics [in/using] EarthCARE" (instead of "… from EarthCARE")?

Reply: The title was updated as "A preliminary analysis of tropical anvil-cloud dynamics in EarthCARE."

L4: I don't think it's quite right to say they are "… found to represent a smooth transition…" as this wasn't directly assessed, and smoothness is a result of aggregating.

Reply: The sentence was rephrased as "Composite vertical profiles of cloud fraction sorted by ice water path (IWP) are constructed as a statistical representation of anvil clouds and deep convection."

L14: "high-cloud **amount at any one time** is about…"?

Reply: No, the number given there is the climatological mean. This is explicitly mentioned in the revised manuscript.

L37: Why define "EC-CPR" if you just use "CPR" later?

Reply: The term "EC-CPR" is used several times later where it refers to the instrument itself in order to avoid confusion with CloudSat CPR. "EC-" is omitted for the product name (e.g., CPR-only product).

L40: This doesn't parse quite right. I think you mean "EarthCARE not only succeeds CloudSat, offering new temporal sampling, but benefits from enhanced technologies…" or similar

Reply: The sentence now reads "As such, EarthCARE not only succeeds CloudSat and CALIPSO, reviving joint radar-lidar observations from space, but benefits from enhanced technologies in satellite active remote sensing."

L90: Can you re-write this so that the language is more specific "averaged out" is not clear to me, and this is an important point.

Reply: The phrase "temporally averaged out over an adequate time interval" was rewritten as "temporally smoothed out to zero when averaged over an adequate time interval."

L146: "a striking updraft" Adjectives like "striking" are subjective (also L160 "excellent detectability" what's excellent about it?), I would reserve these for times when you really want to emphasise a result that is well evidenced (e.g., L175 "remarkably different patterns despite…").

Reply: "A striking updraft" was replaced by "An updraft motion." "The lidar's excellent detectability of semi-transparent clouds" was rewritten as "The semi-transparent clouds detectable only by lidar."

Figure 2: this is interesting, how does that compare to past estimates of IWP? It might be nice to add a statement about that.

Reply: The following paragraph has been inserted in the revised paper (Section 3.2).

"Figure 2 remotely resembles the IWP histogram from CloudSat/CALIPSO observations in past work (e.g., Sokol and Hartmann, 2020) but differs in detail. The discrepancies, while partly because observation periods are not overlapping, are likely to arise from algorithm-specific biases. Different products, or different versions of the same product, disagree among one another in the IWP histogram even for the same input observations (Atlas et al., 2024). It is noted that the histogram in Figure 2 sharply tapers off toward the high end of IWP around 3×10^3 g m^{-2}. This upper limit is significantly lower than known in the literature, well outside the inter-product spread across multiple CloudSat/CALIPSO products (Atlas et al., 2024). The reasons for this bias are unknown at this point."

Figure 3: I would make the colour bar identical between b,c and d, as these variables are comparable. Alternatively, I would at least make the colours the same (blue for c as in b and d).

Reply: The color scale in Figure 3c has been adjusted as suggested.

L198-205: It is not clear how these translate to the derived wt (see comment 1a), please add a sentence linking this section to the introduction of how the PSD is derived from the EarthCARE products.

Reply: Once the ice habit and PSD parameters are determined to match radar and lidar observations, the terminal velocity of hydrometeors is derived with a built-in lookup table so it is aerodynamically consistent with cloud microphysics. This explanation is included in newly added Section 2.2.

L289: “ATLID-CPR” you elsewhere use “CPR-ATLID”, perhaps double-check the full text to ensure consistency.

Reply: “ATLID-CPR” was corrected to “CPR-ATLID.”

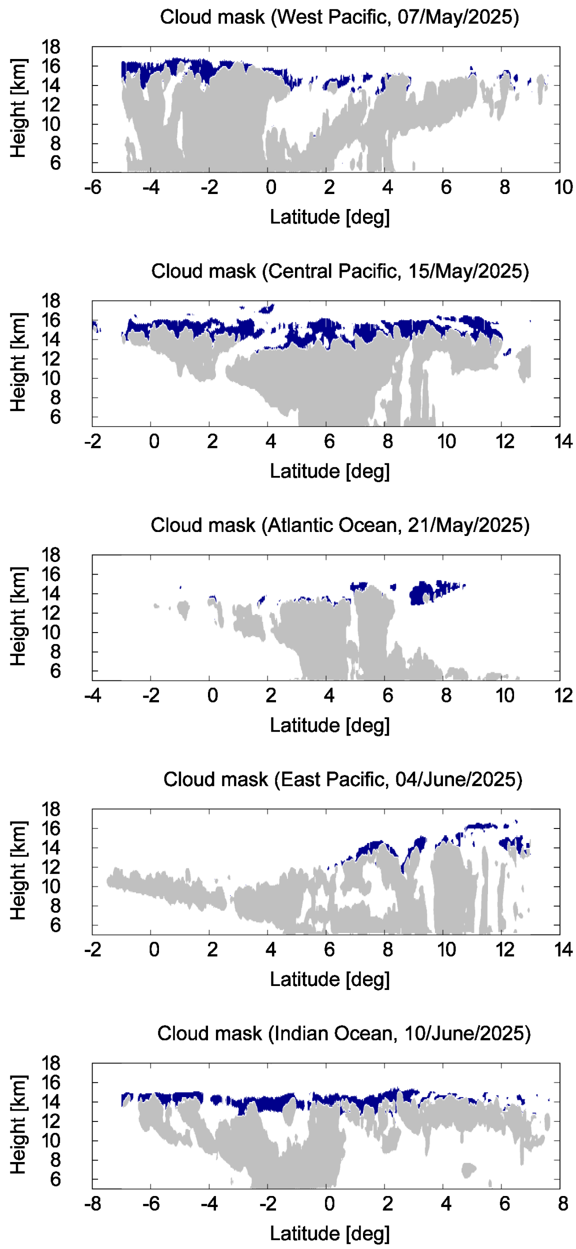


Figure S1: Cloud mask from the CPR product (grey) and CPR-ATLID product (grey and blue combined) for the selected snapshots presented in Fig. 1.

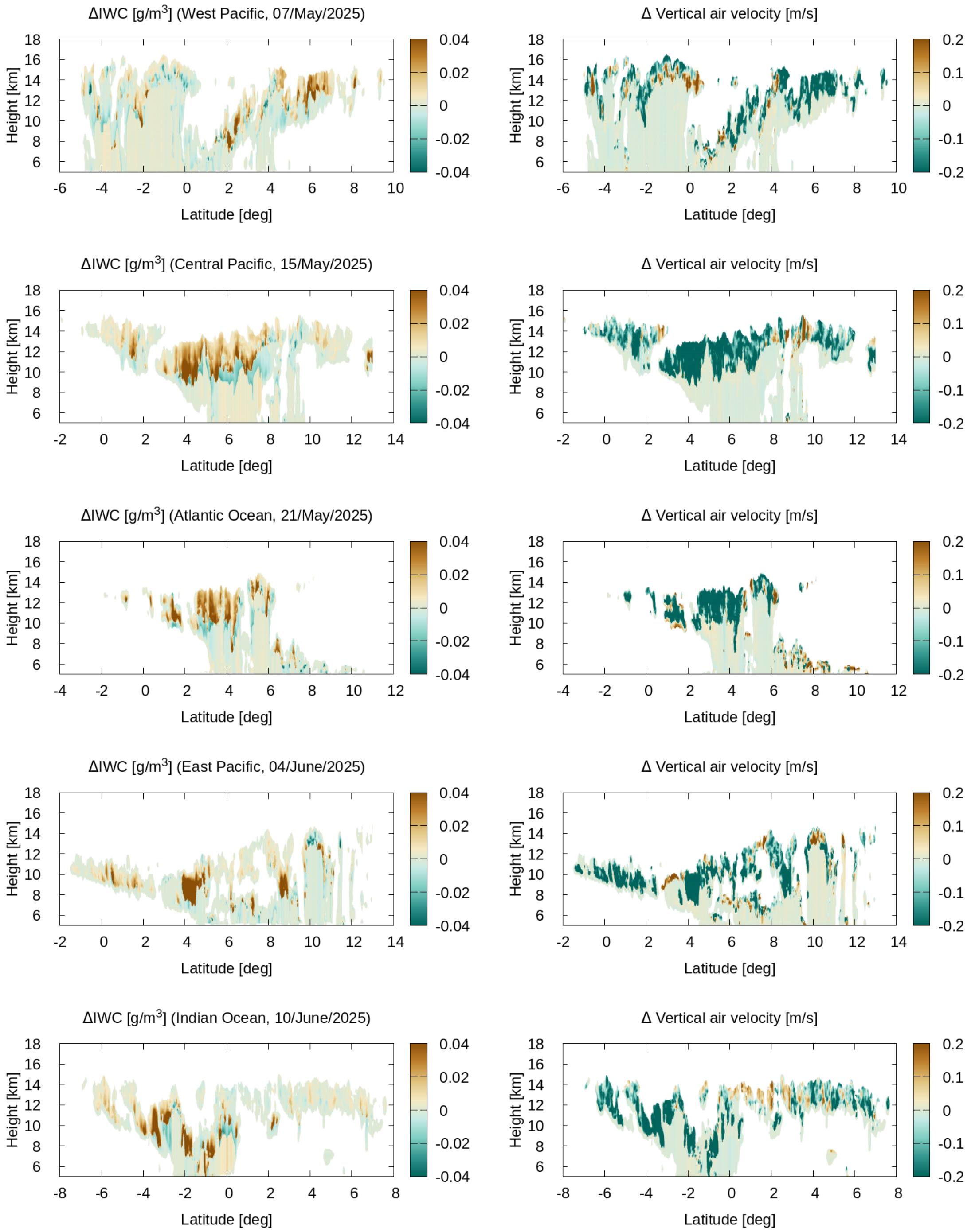


Figure S2: Differences in IWC (g m^{-3}) and in in-cloud vertical air velocity (m s^{-1}) between the CPR and CPR-ATLID products (the former minus the latter) for the selected snapshots presented in Fig. 1.