

## Authors' response to reviewer's comments

### Guyot et al. under review at EGU-AMT

We would like to thank the editor and the two reviewers for their constructive and insightful comments, which have helped us substantially improve the manuscript. For clarity, we have numbered the reviewers' comments; for example, comment 1 from Reviewer 1 is referred to as R1C1 below.

In addressing the reviewers' comments, we identified limitations in both the original implementation and the interpretation of the PyroScope analysis that warranted a broader revision rather than isolated adjustments. We have therefore substantially revised and re-evaluated the processing framework and reframed the interpretation of the radar–satellite comparison.

The principal changes are: (1) revision of the additional satellite spectral criterion, including a new sensitivity analysis and a more cautious interpretation of the resulting class-5 subset; (2) replacement of the original temporal object association with a lineage-based tracking framework capable of handling multiple seeds, object splitting and merging, temporary changes in classification, and continuity within the parent cloud system; (3) systematic comparison of the automated reconstruction against manual annotations for all eight case studies; (4) expansion of the radar analysis using full Level-1 volumes and additional diagnostics of vertical and internal reflectivity structure; and (5) reframing of the study so that satellite-defined pyrocloud properties and radar-observed reflectivity structure are treated as complementary observations, rather than interpreting either the satellite-object centroid or radar echo tops (ETH) as direct measures of the active convective updraft.

Following these changes, all eight cases were reprocessed from the beginning using a single frozen configuration, and all figures, tables and statistics in the manuscript were regenerated. The Methods, Results, Discussion and Conclusions were revised accordingly, and additional sensitivity and source-attribution analyses were added to the Supplement. Where numerical results have changed relative to the original submission, these changes are explicitly identified below.

The revised analysis preserves the principal observational result that satellite-defined pyrocloud structures and radar-observed convective structures can exhibit substantial and persistent spatial differences, including when corresponding lineage segments are compared. The revised full-volume radar analysis further shows that similar echo-top heights can be associated with substantially different internal reflectivity structures. These findings are now interpreted as evidence of the complementary information provided by the two observing systems, rather than as evidence that either sensor directly identifies the location or dynamics of the active updraft.

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Reviewer 1

#### Overall Assessment:

This study evaluates the Peterson et al. (2017) satellite-based pyroCb detection algorithm using Himawari-8, weather radar, and lightning networks data during the Australian "Black Summer" bushfires, and proposes an improved classification method. The research topic is scientifically significant, and the multi-sensor object-tracking approach clearly demonstrates the dynamic separation between satellite-observed cloud tops and radar-observed convective cores. This finding is crucial for understanding the pyroCb life cycle and improving existing satellite detection algorithms. The results are presented in great detail. However, the manuscript requires improvement in terms of

technical details, argumentation structure, and length control. I recommend **major revisions** before considering acceptance by Atmospheric Measurement Techniques . My detailed comments follow.

## Major Comments

1. In Sections 2.8 and 3.3, you use the geometric centroid of the satellite-derived object to compute the displacement between radar and satellite detections, and treat this as a primary metric of spatial agreement. However, in satellite meteorology, the most common method for locating the deep convective center is to identify the minimum cloud-top brightness temperature (BT<sub>min</sub>), rather than the geometric centroid. BT<sub>min</sub> corresponds to the highest cloud top, strongest convection, and most likely the region closest to the updraft core. In contrast, the centroid is sensitive to the object's shape and area, especially when the satellite detection includes a large, advected anvil – the centroid may then be heavily biased away from the true convective core. Please perform a comparative analysis using BT<sub>min</sub> as the satellite object center, at least as a sensitivity test or supplementary material. If you insist on using the centroid, please explicitly discuss in the manuscript whether this choice may underestimate or overestimate the radar–satellite position differences.

R1C1: We thank the reviewer for this important suggestion. We agree that the geometric centroid of a satellite-defined object can be strongly influenced by object size and morphology, particularly as an anvil expands asymmetrically or is advected away from the developing convection. We therefore performed the suggested sensitivity analysis using the location of minimum 11.2- $\mu\text{m}$  brightness temperature (BT<sub>min</sub>) within each satellite class-5 lineage as an alternative measure of satellite feature position.

For each matched radar–satellite lineage state, we retained the radar centroid as the reference and compared its distance to (i) the geometric centroid of the corresponding satellite class-5 object and (ii) the BT<sub>min</sub> location within that same satellite lineage. Importantly, these comparisons were restricted to corresponding lineage segments, so that differences between the two positional metrics are not confounded by switching between neighbouring cells.

Across 221 matched lineage states, the median radar–satellite separation was 16.3 km using the geometric satellite centroid and 26.5 km using BT<sub>min</sub>. The BT<sub>min</sub> location was closer to the radar centroid in only approximately 30% of matched states. Thus, the coldest cloud-top pixel does not systematically provide a closer correspondence with the radar-defined feature for these pyroconvective cases.

We have added this sensitivity analysis as new Fig. S1 in the Supplement and describe the result in Sect. 3.1 of the revised manuscript. We retain the geometric centroid in the main analysis because it provides a reproducible geometric descriptor of the satellite-defined cloud object and, for this dataset, provides better overall positional correspondence with the radar centroid than BT<sub>min</sub>. However, we agree with the reviewer that the centroid should not be interpreted as the physical location of the updraft core. We have therefore revised the manuscript to make this distinction explicit and to discuss the dependence of centroid position on object size and morphology.

2. Your tracking framework (Section 2.8) does not explicitly handle object splitting and merging. However, in several of your cases (e.g., Fig. 9, which shows multiple convective cells), splitting and merging occur frequently. For a pyroCb system containing multiple convective cells, how do you handle matching between radar and satellite objects? Could the current "greedy nearest-neighbor" association strategy erroneously link features from different cells? Please clarify the matching logic for multi-cell systems and discuss the potential impact of ignoring splitting/merging on your results.

R1C2: We thank the reviewer for raising this issue. We agree that the original greedy nearest-neighbour association was not sufficiently robust for multi-cell pyroconvective systems, particularly when objects split, merge, or coexist in close proximity. In response, we substantially revised the tracking methodology rather than simply adding a discussion of this limitation.

The revised framework is lineage-based and allows multiple radar and satellite lineages to coexist within the same event. Expert interpretation of the multi-sensor image sequences is used to establish the identity and spatial reference of the fire-associated features, after which automated component assignment is performed independently for radar and satellite using spatial proximity together with temporal-continuity and object-overlap constraints. Assignments are mutually exclusive within each sensor and time step, preventing two lineages from being assigned to the same component.

Merging is now represented explicitly. When previously distinct lineages merge, the resulting feature is assigned a new descendant lineage identifier and the parent–descendant relationships are retained. Temporary changes in the satellite classification are also accommodated through context-only states, which preserve continuity of the associated parent cloud when no class-5 pixels are present but are excluded from class-5 detection and positional statistics.

We additionally distinguish between event-scale and lineage-scale comparisons. Event-scale quantities such as total radar and satellite area and IoU are calculated from the union of all active lineages at each time. Positional comparisons, however, are made only between corresponding radar and satellite lineage segments and are expressed relative to the onset of each individual lineage. This prevents apparent radar–satellite displacement from being generated simply by switching between neighbouring convective cells.

The revised tracking was systematically checked against manual annotations for all eight analysed events, and all cases were subsequently reprocessed using a single frozen tracking configuration. We have added a same-lineage sensitivity analysis (Fig. S2), a lineage-topology diagnostic (Fig. S5), and spatial lineage trajectories in the main manuscript. These analyses show that finite radar–satellite centroid separation and incomplete spatial overlap persist when only corresponding lineage segments are compared. The radar–satellite differences reported in the revised manuscript therefore cannot be explained solely by erroneous association between neighbouring cells.

Sections 2.8 and 3 of the manuscript have been substantially rewritten to describe this new framework, and all tracking-derived figures and statistics have been regenerated using the revised methodology.

3. You propose a new spectral test based on  $BT_{11} - BT_{8.6}$  to separate "detached/decaying anvils" (class 4) from "intense pyroCb" (class 5), setting the threshold at  $-0.5$  K. What is the origin of this threshold? You mention that it is "based on the principles described in Strabala et al. (1993)". However, Strabala et al. focused on cloud phase discrimination (ice vs. water). Both orphan anvils and active pyroCb anvils are likely composed of ice clouds. The difference between them lies more in cloud thickness, effective particle size, or particle shape. What is the physical mechanism underlying this spectral difference? The manuscript does not describe how the threshold was determined. If the threshold is not sufficiently robust, it directly affects the credibility of your conclusions. Please provide the physical rationale and/or the determination process for this threshold. If this is not possible, please explicitly state the limitations of this test rather than presenting it as a mature, physically-based classifier.

R1C3: We thank the reviewer for this important comment and agree that the physical interpretation of the additional spectral test was overstated in the original manuscript. In particular, Strabala et al. provides a basis for the sensitivity of the 8.6 and 11.2  $\mu\text{m}$  channels to ice-cloud optical and microphysical properties, but does not provide a physical threshold for distinguishing an actively convecting pyroCb anvil from a detached or decaying pyroCb anvil. As the reviewer notes, both are predominantly ice clouds.

We have therefore substantially revised both the interpretation and evaluation of this test. We no longer present the BT8.6–BT11.2 criterion as a cloud-phase discriminator or as a physically unique boundary between active and inactive pyroconvection. Instead, it is introduced as an empirical partition applied only to pixels that have already satisfied the preceding P17-like pyroCb criteria. The broader class-4+5 mask represents the satellite-defined pyrocloud, while class 5 represents a more restrictive subset of that cloud-top signature for comparison with the radar observations.

We also performed a new sensitivity analysis to determine how the choice of threshold affects the resulting classification. All preceding classification criteria were held fixed while the final BT8.6–BT11.2 threshold was varied across the observed range. We evaluated radar-overlap precision and recall, the resulting class-5 area, and the corresponding F1 score. The response exhibits a broad near-optimal region. The numerical maximum in the pooled F1 score occurs at 1.5 K ( $F1 = 0.40520$ ), while the value at the adopted 3 K threshold is essentially identical ( $F1 = 0.40503$ ). The retrieved class-5 area also transitions toward a comparatively stable regime at approximately 2–3 K. We therefore retain 3 K as a simple rounded empirical operating threshold rather than tuning the classifier to the exact optimum of this selected eight-event sample.

This sensitivity analysis has been added as Fig. S3 and is described in the revised Methods. Importantly, the radar-overlap statistics used in this sensitivity analysis quantify cross-sensor spatial correspondence; we do not interpret them as an independent validation of pyroCb detection skill. We also explicitly state that the 3 K threshold should not be regarded as a universal physical boundary and that its generality will require evaluation using a larger and more diverse sample, including conventional convection and weak or marginal pyroconvective cases.

We thank the reviewer for prompting this clarification, which has led us to substantially moderate the physical interpretation of the additional test and to provide a quantitative basis for the adopted operating threshold.

4. From 154 candidate events, you selected 22 that met data availability criteria, and then only 8 for detailed object-based tracking. The selection of these 8 events introduces a clear sample selection bias. Consequently, the generalizability of your conclusions may not apply to all pyroCb types. The current analysis is essentially a case-study of strong, simple events rather than a statistically representative analysis. I strongly encourage you to increase the sample size to test the robustness of your conclusions. If this is not feasible, please explicitly state in the Conclusions or Discussion that the findings are primarily applicable to pyroCb events with strong signals and simple morphological structures.

R1C4: We agree with the reviewer that the eight events selected for detailed tracking do not constitute a statistically representative sample of Australian pyroCb events, and we have revised the manuscript to make this limitation explicit.

The initial event catalogue contains 154 pyroCb occurrences. Of these, 22 satisfied the requirements for coincident weather-radar, Himawari satellite, lightning and ancillary data within the study domain. Eight events were then selected for detailed tracking because they exhibited sufficiently strong satellite pyroCb signatures and suitable coincident multi-sensor temporal coverage for the cross-sensor analysis.

The resulting subset is therefore purposive and preferentially samples well-observed events with clear satellite signatures.

Rather than increasing the number of events while relaxing these observational requirements, we have clarified the scope of the present study. The objective of the detailed eight-event analysis is to investigate how satellite-defined pyrocloud features evolve relative to radar-observed deep echo structure, including in multi-cell systems, rather than to estimate population-level detection skill or to establish the statistical performance of the modified classification across all Australian pyroCb events. The revised lineage-based analysis also now accommodates substantially more complex multi-cell behaviour than the tracking framework in the original submission.

We have moved the full 22-event table to the Supplement (Table S1) and now explicitly state in both the Methods and Conclusions that the eight-event subset is not statistically representative of all pyroconvective events. We further identify evaluation across a larger and more diverse sample—including weak or marginal satellite detections, pyroconvection that does not develop into pyroCb, and conventional convection—as an important requirement for quantifying detection skill and evaluating the generality of the empirical spectral partition introduced here.

We therefore agree with the reviewer that the conclusions from the present sample should not be generalized to all pyroCb types, and we have revised the scope and wording of the manuscript accordingly.

## Minor Comments

1. You mention using the Satpy-based parallax correction algorithm. The cloud-top height is a critical intermediate variable. Please provide additional details on the accuracy, resolution, and potential systematic biases of the CTH product you used

R1C5: We thank the reviewer for this suggestion. We agree that uncertainty in the cloud-top-height (CTH) retrieval is important because it propagates directly into the magnitude of the satellite parallax correction.

We have therefore added further information on the operational Australian Bureau of Meteorology CTH product used in this study, which is based on the NWC SAF cloud-top-height methodology described by Kerdraon and Fontaine (2021). A new Table S2 in the Supplement summarises the product characteristics, published validation statistics, and principal retrieval limitations. The CTH product has a nominal spatial resolution of 2 km at nadir and a temporal resolution of 10 min.

For opaque clouds, published validation against CALIOP and CloudSat reports CTH biases of approximately  $-0.82$  km and  $-0.56$  km, respectively, with corresponding standard deviations of approximately 1.21 and 0.96 km. Uncertainties are larger for semi-transparent clouds, for which reported biases are approximately  $-1.41$  km relative to CALIOP and  $+0.50$  km relative to CloudSat, with standard deviations of approximately 2.19 and 2.06 km, respectively. Retrieval performance can additionally be degraded by thin or semi-transparent cirrus, multilayer or fractional cloud, cloud-type classification errors, and errors in the NWP atmospheric profiles used by the retrieval.

We have also clarified in Sect. 2.3 that CTH uncertainty propagates into uncertainty in the calculated horizontal parallax displacement. Consequently, residual radar–satellite positional differences after correction may partly reflect CTH retrieval uncertainty, in addition to temporal sampling differences, radar geometry, cloud morphology, and the different atmospheric volumes sampled by the two observing systems. The CTH product is therefore used as an input to the geometric correction and not as an independent validation quantity.

2. Although the scientific content is substantial, the manuscript is overly long, which affects readability. I suggest the following streamlining:

Merge Sections 3.2.1 and 3.2.2: The two case studies follow nearly identical narrative patterns. They could be merged into one integrated case study, with the second case moved to supplementary material.

R1C6: We thank the reviewer for this suggestion and agree that the two case-study sections in the original manuscript contained substantial repetition. We have therefore substantially shortened and reorganised this part of the Results.

The two previous case-study subsections have been merged into a single section (Sect. 3.2, “Representative lineage evolution: Grose Valley and Wulgulmerang”). Rather than presenting two parallel narratives, the revised text uses the cases comparatively. Grose Valley illustrates a comparatively simple situation dominated by a single tracked lineage, whereas Wulgulmerang illustrates a more complex multi-cell system involving several simultaneous lineages and subsequent merger transitions.

We have retained both examples in the main manuscript because they now serve distinct methodological purposes and provide a compact visual demonstration of why the lineage-based tracking framework is required. In particular, Wulgulmerang provides an important example of the splitting/merging and multi-cell behaviour highlighted in R1C2. The detailed quantitative evolution across all eight events is subsequently presented in the multi-event analysis rather than repeated within the individual case descriptions.

The corresponding text has been substantially reduced, and redundant descriptions of the two cases have been removed.

Redesign Figure 3 (flowchart): The arrow structure is not entirely clear; some decision arrows cross thresholds or are drawn in a cluttered manner. Also, the satellite names (Himawari-8) are inconsistently labelled in the input boxes. Please redesign the flowchart for clarity.

R1C7: We thank the reviewer for this comment. We agree that the original flowchart was unnecessarily difficult to follow and contained inconsistencies in the labelling of the satellite inputs.

Figure 3 has therefore been redesigned to clarify the sequential decision structure of the adapted P17 algorithm and the additional test introduced in this study. The input channels and Himawari-8 labelling have been made consistent, and the decision arrows have been reorganised to avoid the crossings and ambiguities present in the original figure. The figure now also makes explicit that the additional BT8.6–BT11.2 test is applied only after the preceding P17-like pyroCb criteria have been satisfied and that the combined class-4+5 mask represents the broader satellite-defined pyrocloud, within which class 5 represents the more restrictive subset.

We have also revised the figure caption and accompanying text to define the meaning of the decision arrows and the resulting classes more explicitly.

Move Table 1 to supplementary material: This table contains basic information about the selected cases and could be placed in the supplement to reduce the main text length.

R1C8: We thank the reviewer for this suggestion. We agree that the full event table is not essential to the flow of the main manuscript. We have therefore moved the table listing the 22 events satisfying the multi-sensor data-availability criteria to the Supplement, where it is now presented as Table S1.

The main text now provides only a concise summary of the event-selection procedure: 154 candidate events were identified from the Australian pyroCb register, 22 satisfied the required multi-sensor data-availability and radar-proximity criteria, and eight were selected for detailed lineage-based analysis. The criteria and limitations associated with this purposive eight-event subset are discussed in the revised Methods.

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## Reviewer 2

Review of Guyot et al., “Multi-sensor tracking of pyroconvection reveals discrepancies between satellite cloud-top detection and convective dynamics “

Reviewer: Mike Fromm

Guyot et al. explore how augmenting broadband satellite image data with (primarily) weather radar and lightning data can refine and improve pyrocumulonimbus-storm detection. They also introduce a new satellite-based infrared brightness temperature difference metric to a published pyroCb-cloud detection algorithm with the aim of distinguishing the part of the pyrocloud associated with the causal convective updraft from aged and/or detached pyroCb blowoff.

The authors blend lightning, radar, and meteorological data with geostationary satellite image data in their novel “PyroScope” platform. Therein they have constructed a method for identifying horizontal “objects” having certain distinctive cloud and pyrocloud qualities. This is a strategic and potentially invaluable tool for realizing an optimal pyroconvection-monitoring capability.

This is an important initiative and appropriate for the AMT community.

The authors fairly describe the Pyroscope tool and several sources of uncertainty with respect to their approach to “object” definition as well as the many nuances inherent in the wildfire/pyroconvection environment. Thereby the authors appropriately offer their findings in recognition of the need for much more investigation.

With that being stated, my overall assessment is that this manuscript does not merit publication until/unless several substantial shortcomings are addressed. In short, the manuscript heavily promotes radar-centric clues into convective and pyroconvective dynamics by virtue of echo-top analyses exclusively. Even though weather radar represents an independent (and potentially valuable) remote-sensing data source, echo tops are inherently no different than broadband satellite cloud-top metrics. In other words, both represent conditions on the upper skin of the cloud; neither the echo-top nor satellite data they report from Pyroscope offer insights into underlying “convective dynamics,” as implied in the manuscript’s title and focused on in the body of this work.

Below I list specific major concerns. Appended is the manuscript PDF containing more specific and technical issues.

### Major Concerns

In section 2.4, “Radar data and selection of cases,” the authors state as future plans the exploration of additional radar metrics inherent to dual-polarization and Doppler systems. Presumably these future plans also involve exploitation of full volumes of radar returns within pyroconvective storms. These would be the minimum necessary to get at internal convectivedynamics underlying the pyrocumulus and pyroCb cloud tops. If the authors intend on showcasing these unique and strategic quantifiers of pyroconvective dynamics, such a work should precede this one, which is reliant on echo-top data. Again, echo-tops are generic cloud signals, inherently containing no information peculiar to pyroCbs.

R2C1: We thank the reviewer for this important criticism. We agree that the original manuscript placed too much emphasis on echo-top height as a proxy for “convective dynamics”. Echo-top height provides useful information on the vertical extent of radar-observed hydrometeor structure, but it is not a direct measurement of vertical velocity, updraft strength, or the internal dynamics of a pyroconvective cloud. We have revised the manuscript throughout to make this distinction explicit, including revising the title to refer to “radar-observed convective structure” rather than “convective dynamics”.

We have also substantially expanded the radar analysis using the native Level-1 radar volumes. In addition to echo-top heights, the revised analysis now includes the 95th-percentile crossing heights of the 18 and 30 dBZ echo surfaces (ETH18 and ETH30), together with the Cartesian volumes occupied by  $\geq 18$  and  $\geq 30$  dBZ reflectivity above 8 km MSL. From these quantities we derive an equivalent upper-level echo depth, by normalising the  $\geq 18$  dBZ upper-level echo volume by the horizontal area of the tracked radar system, and a strong-echo fraction describing the proportion of the  $\geq 18$  dBZ upper-level volume occupied by reflectivities  $\geq 30$  dBZ. The underlying unnormalised volumetric quantities and upper-level reflectivity statistics are additionally provided in the Supplement.

These diagnostics demonstrate that echo-top altitude alone does not uniquely describe the radar-observed structure. Events with similar ETH18 or ETH30 can exhibit substantially different upper-level reflectivity volumes and strong-echo fractions. The full radar volume therefore provides information on the internal reflectivity structure that is not contained in echo-top altitude alone.

At the same time, we have deliberately avoided interpreting these additional radar quantities as direct measurements of convective dynamics. They remain reflectivity-based structural proxies whose values depend on hydrometeor properties and radar sampling geometry. Their principal role in the revised manuscript is therefore to characterise changes in the vertical and internal radar echo structure of the tracked pyroconvective system and to provide an observational perspective complementary to the satellite cloud-top classification.

We have revised the Abstract, Introduction, Methods, Results, Discussion, Conclusions and title accordingly. The central comparison is now framed as one between satellite-defined cloud-top properties and radar-observed vertical and internal reflectivity structure, rather than as a comparison between satellite cloud top and a direct radar measurement of the convective updraft.

Moreover, the authors did not make a compelling case that echo tops by themselves serve as an independent pyrocloud indicator. In each example they illustrate (Figures 6-9), the echo-top pyrocloud objects (green and orange) do not regularly conform to or match up with dashed (ETH30) and dotted (ETH18) enclosures, which presumably indicate even more restrictive echo intensity than ETH10. Even more perplexing is that there are some green/orange radar-pyrocloud objects not connected with fires, raising the question of how the decision is made as to which objects are classified as pyroCb while others are not.

R2C2: We thank the reviewer for identifying this ambiguity. We agree that the original figures and description did not make sufficiently clear how radar echoes were associated with the fire-generated pyroconvective system, and this could give the impression that individual echo-top objects were independently classified as pyroCb on the basis of their echo-top properties.

This is not the role of the radar classification in the revised framework. Radar echo-top thresholds are used to describe the vertical structure of radar echoes belonging to the tracked fire-associated system; they are not treated as independent pyroCb-identification criteria. Consequently, the presence of an ETH18 or ETH30 object elsewhere in the radar domain does not by itself imply that the feature is

pyroconvective. Such echoes may correspond to neighbouring meteorological convection and are not included unless they belong to the tracked fire-associated lineage.

The revised lineage framework makes this distinction explicit. Fire-associated features are identified using the multi-sensor image sequence and their spatial and temporal continuity, after which the automated tracker follows the corresponding radar and satellite components through time. Multiple contemporaneous lineages can be retained where required, and component assignments are mutually exclusive. This is particularly important in scenes containing neighbouring convective cells, where proximity alone would otherwise permit association with an unrelated echo.

We have also revised the figures and captions to distinguish the tracked radar-system masks from the ETH18 and ETH30 contours. The latter are structural diagnostics within the radar observations and should not be interpreted as independent classifications of individual echoes as pyroCb.

The Grose Valley/Gospers case highlighted by the reviewer provides an important example of this distinction. We revisited this event using native Level-1 dual-polarisation radar observations together with temporally local FIRMS fire detections (new Fig. S6). The low-level radar morphology shows spatial continuity from the active-fire/plume region toward the northern convective feature, whereas the southern convective cloud does not show comparable continuity with the fire/plume sector. We therefore associate the northern feature with the fire-generated pyroconvective system and treat the southern feature as neighbouring meteorological convection. This source-attribution analysis is used as a diagnostic check and does not imply that echo-top height, RHOHV, or ZDR alone provides a unique pyroCb identifier.

We have revised the Methods, figure captions, and discussion accordingly to clarify the distinction between source attribution/tracking and the radar structural diagnostics applied to the tracked system.

Additionally, my impression is that the authors focus on the centroid of both radar- and satellite-sourced pyrocloud-objects as comparable markers of a pyroCb's cloud footprint. Sensitivity to cloud particles, especially those microphysically perturbed, is vastly weaker at radar wavelengths as opposed to satellite IR wavelengths. Then, considering the satellite's continuous coverage and approximately invariant satellite-pixel size in relation to any given radar's sweep limits, it is natural for a satellite's cloud top footprint to expand far beyond any single radar's ability to resolve. Consequently, the size of an individual radar's echo-top object will almost certainly be smaller than a comparative satellite IR-image-based cloud object, with consequences for a resultant centroid-location bias between the two measurement techniques. So in short, tying these two objects together over the life of a pyroCb—or any Cb—is an ill-posed proposition in my view.

R2C3: We thank the reviewer for this important point and agree that radar- and satellite-defined cloud objects should not be regarded as equivalent representations of the same physical cloud volume. Satellite infrared observations describe cloud-top radiative properties, whereas weather radar samples hydrometeor backscatter through a three-dimensional volume and is additionally constrained by radar sensitivity, range and sampling geometry. The resulting radar and satellite footprints can therefore differ substantially even when both observations are associated with the same pyroconvective system.

We have revised the manuscript to make clear that the centroid separation is not interpreted as a measure of geolocation error, nor do we assume that the two centroids should coincide. Instead, it is retained as a reproducible geometric descriptor of how the corresponding radar- and satellite-defined structures evolve relative to one another. Similarly, intersection-over-union is used as a descriptive measure of spatial overlap rather than as an expectation that the two masks should be geometrically equivalent.

We have undertaken two additional analyses to test whether the reported separation could primarily result from the choice of positional metric or from erroneous association between neighbouring cells. First, following Reviewer 1's suggestion, we repeated the positional analysis using the minimum 11.2- $\mu$ m brightness-temperature location (BTmin) within each satellite class-5 lineage instead of its geometric centroid. Across 221 matched lineage states, the median radar–satellite separation was 16.3 km using the satellite centroid and 26.5 km using BTmin, with BTmin closer to the radar centroid in only approximately 30% of states (new Fig. S1). Thus, replacing the satellite centroid with the coldest cloud-top pixel does not systematically improve the spatial correspondence.

Second, the revised lineage framework permits positional comparisons to be restricted to corresponding radar and satellite lineage segments. The resulting same-lineage analysis (new Fig. S2) continues to show finite centroid separation and incomplete spatial overlap, demonstrating that these differences cannot be explained solely by switching between neighbouring convective cells.

We agree, however, that these analyses do not make radar and satellite footprints physically equivalent. Rather, the persistence of these differences is itself part of the observational result: the two sensors describe different aspects and atmospheric volumes of the evolving pyroconvective system. We have revised the Methods and Discussion accordingly and have removed language that could imply that centroid agreement is expected or that either centroid represents the physical location of the convective updraft.

The authors have interpreted the “intense pyroCb” (IPCB) cloud-object designation (and specifically the object’s centroid) of Peterson et al. (2017) as associated with active pyroconvection’s updraft column. Conceding that P17’s “IPCB” terminology may suggest a general cloud/updraft connection, it is clear from P17 that the anvils and orphans flagged as IPCB are simply opaque ice clouds exhibiting a definable microphysical peculiarity, originally anchored to an active fire hot spot. The pyroCb anvil evolves similarly to those of Cb origin; the portion associated with underlying active convection remains near the upwind extent and diminishes in time size-wise relative to the overall opaque exhaust. If the authors concur with this characterization, a revised method of satellite pyrocloud/pyroconvection sectioning for the purpose of radar-data comparison is imperative.

**R2C4:** We thank the reviewer for this important clarification. We agree that the original manuscript attributed a stronger physical meaning to the P17 “IPCB” mask than was intended in Peterson et al. (2017). In particular, P17 did not define the IPCB object centroid as the location of the active convective updraft, nor did it require the complete IPCB footprint to represent actively convecting cloud. Rather, the satellite classification identifies opaque ice cloud with the relevant spectral characteristics in association with an active fire.

We have revised the manuscript throughout to correct this interpretation. We no longer describe the P17 IPCB footprint, or its centroid, as a direct measure of the actively convecting core. Correspondingly, the objective of the study has been reframed. Rather than testing whether the P17 satellite object correctly locates an assumed radar-defined updraft core, we investigate how the satellite-defined pyrocloud signature evolves relative to the vertical and internal reflectivity structure observed by weather radar.

This distinction also changes the interpretation of the additional BT8.6–BT11.2 test introduced in this study. We no longer present this test as a physically unique discriminator between an active pyroCb and an orphan or decaying anvil. Instead, the combined class-4+5 mask represents the broader satellite-defined pyrocloud, while class 5 is treated as an empirical, more restrictive subset of that cloud-top signature. The purpose of this partition is to examine whether a more restrictive satellite

subset provides closer correspondence with the radar-observed structure, not to redefine the physical meaning of the P17 IPCB classification.

We agree with the reviewer that the fire-proximate and upwind portion of the cloud is particularly relevant when establishing source attribution. In the revised lineage framework, however, we do not impose a fixed upwind subsection of the satellite anvil as a universal proxy for the active convection. Instead, source attribution and temporal continuity are established from the multi-sensor sequence, after which corresponding radar and satellite lineage components are followed through time. This accommodates changes in morphology, multiple simultaneous convective elements, splitting/merging, and temporary absence of a class-5 satellite core without assuming that a particular geometric portion of the anvil always corresponds to the radar-observed convective structure.

We have consequently revised the Abstract, Introduction, Methods, Results and Discussion to distinguish clearly between (i) the satellite-defined pyrocloud signature, (ii) its empirical class-5 subset, and (iii) the radar-observed reflectivity structure. We thank the reviewer for this comment, which prompted an important correction to the conceptual framing of the manuscript.

The title and argumentation within the manuscript indicate that the authors have found limitations in the P17 pyroCb-detection output in terms of representing “a direct measure of the actively convecting core” [Abstract, Line 30] or its lack of distinction between anvil and orphan pyrocloud. P17 at no point interpreted the footprint of their “IPCB” object as anything more than the location of microphysically perturbed opaque ice. No special meaning was given to the centroid of P17’s IPCB clouds. Rather, the greatest significance was reserved for the microphysically perturbed pixels in the vicinity of a nearby fire hot spot, which defined the condition of formal pyroCb identification. Hence it may be advisable for a radar-satellite pyrocloud analysis to focus on a subset of the satellite pyrocloud representing the upwind, fire-proximate portion of the anvil instead of its centroid.

**R2C5:** We thank the reviewer for this clarification and agree that Peterson et al. (2017) did not interpret the centroid of the IPCB mask as the location of active pyroconvection or of the convective updraft. Our original manuscript attributed a stronger interpretation to the P17 output than is supported by that study.

Rather than attempting to support this interpretation through additional literature, we have refocused the scientific objective of the manuscript. The revised study does not evaluate whether the P17 IPCB mask or its centroid correctly identifies the actively convecting core. Instead, we treat the P17-like classification as a satellite-defined pyrocloud signature and investigate how its spatial and temporal evolution relates to independently observed radar reflectivity structure.

Accordingly, centroid separation is now used only as a geometric descriptor of the relative positions of corresponding radar and satellite lineage components. It is not interpreted as an error in the P17 detection, nor is either centroid interpreted as the physical location of the updraft. The revised lineage framework further restricts positional comparisons to corresponding radar and satellite lineage segments, thereby avoiding associations between neighbouring convective cells.

We have also revised the motivation for the additional BT8.6–BT11.2 test. Its purpose is no longer presented as correcting a failure of P17 to distinguish active convection from orphan anvil. Rather, it provides an empirical partition of the broader satellite-defined pyrocloud that can be evaluated against the radar-observed structure. As described in our responses to R1C3 and R2C4, we explicitly state that this partition is not a universal physical discriminator between active and inactive pyroconvection.

The Abstract, Introduction, Methods, Results and Discussion have been revised accordingly. We believe this reframing better reflects both the intended interpretation of the P17 algorithm and the complementary information provided by satellite and radar observations.

Whereas satellite-based pyroCb detection, automated or otherwise, employs two metrics (anchorage to fire and peculiar microphysics) for qualification, the radar-based method described herein has no such explicit qualification elements. Echotops by themselves are a generic radar-centric cloud metric. Given that the 8 cases studied herein are all identified as pyroCbs from the Australian pyroCb Register, the authors have not demonstrated an independent skill of radar-based pyroconvection detection capability.

**R2C6:** We agree with the reviewer. The radar observations used here do not, by themselves, provide an independent pyroCb detection criterion comparable to the combination of fire anchoring and satellite spectral characteristics used in P17. Echo-top height and the additional volumetric reflectivity diagnostics introduced in the revision are not unique to pyroconvection and can equally occur in meteorological deep convection.

We have therefore revised the manuscript to remove any implication that PyroScope demonstrates an independent radar-only capability for detecting pyroCb. In the revised framework, source attribution and structural characterization are treated as separate steps. The fire-associated pyroconvective features are identified from the multi-sensor temporal sequence and their relationship to the active fire, after which the radar observations are used to track and characterise the vertical and internal reflectivity structure of the corresponding system. Radar quantities are therefore diagnostics of the tracked pyroconvective lineage rather than independent pyroCb classification criteria.

This distinction is particularly important in scenes containing neighbouring meteorological convection. Such radar echoes may exhibit substantial ETH18 or ETH30 values but are not classified as pyroconvective solely on this basis. The revised lineage framework allows multiple features to coexist and restricts the analysis to the lineage associated with the fire-generated system. As an additional check for the complex Grose Valley/Gospers case, we examined native dual-polarisation radar observations together with temporally local FIRMS fire detections (new Fig. S6), which provide low-level spatial and temporal evidence linking the northern convective feature to the active-fire/plume region while distinguishing it from neighbouring meteorological convection.

We have revised the Methods, Results and Discussion to make clear that the role of radar in this study is complementary: it provides an independent observing modality for characterising the structure and evolution of an already attributed pyroconvective system, rather than an independent radar-only pyroCb detection algorithm. Demonstrating such a radar-only detection capability would require a separate validation framework including both pyroconvective and conventional convective events and is beyond the scope of the present study.

In Section 2.2, the authors introduce a modification to the P17 pyroCb-detection algorithm, motivated by P17 occurrences of orphan anvils falsely classified as IPCB. They propose a brightness-temperature difference (BTD) between 8.6 and 11 micron channels, and apply a threshold test for pixels that have passed both the cloud-opacity and ice-cloud tests. The rationale for choosing this wavelength pair is based on a prior publication on satellite-based cloud-phase discrimination. It is perplexing that the authors invoke this paper for a situation involving ice clouds exclusively. The authors do not argue that the cloud conditions they are questioning are misclassified as IPCB because of the presence of liquid hydrometeors. So I am left wondering why this particular innovation was selected and how to interpret the results

shown in Figures 6-9.

**R2C7:** We thank the reviewer for this comment and agree that our original use of Strabala et al. (1993) could incorrectly suggest that the proposed BT8.6–BT11.2 test was being applied as a conventional ice-versus-water cloud-phase discriminator. This was not our intention, and we have revised the manuscript substantially to clarify the rationale and, importantly, the limitations of this test.

The relevance of the 8.6 and 11.2  $\mu\text{m}$  channels here is not that one satellite population is assumed to contain liquid cloud while the other contains ice. Both the broader pyroCb anvil and its more restrictive subset are expected to be predominantly ice cloud. Rather, the spectral difference is sensitive to differences in the radiative properties of ice cloud, including variations in optical thickness and ice-particle properties. Strabala et al. provides the underlying basis for the differential spectral behaviour of ice clouds in these infrared channels, but it does not establish a criterion for distinguishing active pyroconvection from detached or ageing pyroCb anvil.

We have therefore removed the stronger physical interpretation of this test. The revised algorithm first applies the preceding P17-like pyroCb criteria and only then uses BT8.6–BT11.2 to partition the resulting satellite-defined pyrocloud. The combined class-4+5 mask represents the broader pyrocloud signature, while class 5 represents a more restrictive empirical subset. We do not interpret this partition as demonstrating cloud phase or as uniquely separating actively convecting from inactive/orphan pyrocloud.

The adopted 3 K threshold is also no longer presented as originating from Strabala et al. Its selection and sensitivity are now evaluated explicitly in the revised manuscript and Supplement (new Fig. S3), as described in our response to R1C3. The threshold should be regarded as an empirical operating criterion for the present analysis rather than a universal physical boundary.

We have revised Sect. 2.2 and the associated discussion accordingly. We thank the reviewer for prompting us to distinguish more clearly between the spectral sensitivity that motivates use of this wavelength pair and the empirical interpretation of the additional partition introduced in this study.

Given that the authors offer the 11-8 BTD innovation, there is yet another concern. It is that they 1. do not explain their choice of BTD threshold, 2. do not present a test method to ascertain its skill, and 3. they show no experiments such as an application of the 11-8 BTD to demonstrably generic Cb anvils and orphans. With none of these reported herein or defended by citation, the reader is given no foundation on which to be persuaded by the analyses in this manuscript.

**R2C8:** We thank the reviewer for identifying these three related limitations. We agree that the original manuscript did not provide a sufficient quantitative basis for the selected BT8.6–BT11.2 threshold and attributed more physical meaning to the resulting partition than could be supported by the available analysis.

We have addressed the first two points by adding an explicit threshold-sensitivity analysis (new Fig. S3). All preceding classification criteria were held fixed while the final BT8.6–BT11.2 threshold was varied across the observed range. For each threshold, we evaluated (i) radar-overlap precision and recall, (ii) the resulting class-5 area relative to that obtained with the adopted 3 K threshold, and (iii) the corresponding radar-overlap F1 score. The response exhibits a broad near-optimal region: the pooled F1 maximum occurs at 1.5 K ( $F1 = 0.40520$ ), compared with  $F1 = 0.40503$  at 3 K. The

difference is therefore negligible, while the retrieved class-5 area transitions toward a comparatively stable regime around 2–3 K. We retain 3 K as a simple rounded operating threshold rather than tuning the algorithm to the exact optimum obtained from this limited event sample.

We emphasise that these radar-overlap statistics quantify cross-sensor spatial correspondence and are not presented as an independent validation of pyroCb detection skill. The analysis demonstrates principally that the results are not critically dependent on a narrowly tuned threshold value.

Regarding the reviewer's third point, we have not added a control population of generic Cb anvils and orphan clouds to the present study. We agree that such a comparison would be required to determine whether the BT8.6–BT11.2 partition has discriminatory skill that is specific to pyroconvection. Consequently, we have revised the scope of our claims rather than presenting the present analysis as such a validation. Class 5 is now described as an empirical subset of the broader satellite-defined pyrocloud, and the 3 K criterion is explicitly not interpreted as a universal physical boundary or as a validated discriminator between active pyroconvection, orphan pyrocloud and conventional convection.

The need for evaluation against a substantially larger and more diverse dataset—including conventional convection, weak or marginal pyroconvective detections, and pyroconvection that does not develop into pyroCb—is now explicitly identified as a limitation and priority for future validation.

We have revised Sect. 2.2, the Discussion and Conclusions accordingly and added the complete threshold-sensitivity analysis to the Supplement.

In Figures 6-9 (c), referred to as a “feature space,” BTD11-8 versus BTD 11-13 scatter plots are shown. These presumably inform panels (b) of each figure, but much is left unexplained about panels (c) and (b). Panels (c) show gray dots that are unlabeled and undefined. These scatter plots show three labeled pixel classes whereas panels (b) have five labeled classes, including one (deep convection) that is not in the panels (c). Referring back to Figure 3, the algorithm flowchart, it is evident that the authors intend to color-code different cloud classes, but it is incomplete and conflicting amongst these figures. The result for the reader is great confusion and consequent skepticism about the method and results.

**R2C9:** We thank the reviewer for pointing out this lack of clarity. We agree that the original feature-space panels were insufficiently explained and that the relationship between the classes displayed in the scatter plots, the classification maps, and the algorithm flowchart was not clear.

We have revised these figures and their captions to make the class definitions and plotting conventions consistent with the revised classification framework. In particular, the grey points in the feature-space plots are now explicitly identified as pixels that do not satisfy the relevant pyrocloud classification criteria and are shown only to provide the surrounding spectral context. The coloured points correspond to the satellite classes relevant to the spectral partition illustrated in each panel.

We have also clarified why not all classes displayed in the spatial classification maps necessarily appear as distinct populations in the feature-space panels. The latter are intended specifically to illustrate the spectral tests used in the satellite classification and the final empirical class-4/class-5 partition, rather than to reproduce every category shown in the full spatial classification. The revised captions now state explicitly which pixels are included in each feature-space plot and how their colours correspond to the algorithm flowchart and classification maps.

More generally, Figure 3 and the associated classification terminology have been revised so that the sequence of spectral tests and the resulting classes are defined consistently throughout the manuscript. As discussed in our responses to R1C3, R2C7 and R2C8, the revised figures also avoid implying that the final BT8.6–BT11.2 partition constitutes a physically unique separation between active and inactive pyroconvection.

Regarding panels (a) of Figures 6-9, the authors plot dashed and dotted enclosures of echo-top objects, coloring in some as pyroconvective in nature, and not others. I could not find where or if the authors explained that. Moreover, they deploy an echo-top class, `eth_30`, that is not mentioned in the main text or the figure caption. Intuitively, `eth_30` and `eth_18` would seem to identify particularly high and robust echo objects, so it is confusing as to why many of these objects are shown but not discussed or classified as pyrocloud-related.

**R2C10:** We thank the reviewer for identifying this ambiguity. We agree that the original figures did not distinguish sufficiently clearly between the radar objects associated with the tracked pyroconvective system and the ETH18/ETH30 contours used to describe the vertical extent of radar echoes.

In the revised framework, ETH18 and ETH30 are not independent pyroCb classifications. They represent the heights of the 18 and 30 dBZ echo surfaces and are used as structural diagnostics of the radar-observed convection. Consequently, an ETH18 or ETH30 feature elsewhere in the domain is not classified as pyroconvective solely because it reaches a particular altitude or reflectivity threshold. As clarified in our response to R2C2, association with the pyroconvective system is instead established through the multi-sensor temporal sequence, fire association, and lineage continuity.

We have revised the Methods and figure captions to define ETH18 and ETH30 explicitly and to distinguish these contours from the tracked radar-system masks. ETH30 is now also introduced and defined in the main text, together with the additional full-volume radar diagnostics added in response to the reviewer's broader concerns regarding the use of echo-top information alone.

The revised presentation therefore separates two concepts that were insufficiently distinguished in the original manuscript: (i) which radar feature belongs to the tracked fire-associated lineage, and (ii) the ETH18, ETH30 and volumetric reflectivity properties used to characterise the vertical and internal structure of that feature.

Regarding the first case study, the Gospers Mountain event on 22 November 2019 (Fig. 6 and 7), I suspect that the cloud the authors are tracking is not a pyroCb. Indeed a pyroCb on this date is recorded in the Australia pyroCb Register and is listed in the Peterson et al. (2025) [P25] pyroCb database, but the upwind side of the radar and satellite cloud feature on which the authors focus is due south of the fire by more than 40 km (as reported by the authors). The authors are encouraged to verify that the prevailing winds at this time were northwesterly from the boundary layer to the upper troposphere. One can tell by the orientation of the subject cloud that it is oriented along that northwesterly axis. It is also noteworthy that the fire-position labeled in Fig. 6 and 7 is at the southern perimeter of the Gospers Mt. fire, which is illustrated by MODIS hot spots below.  
(Credit: NASA Worldview)

**R2C11:** We thank the reviewer for raising this concern. We agree that cloud-top position alone was insufficient to establish source attribution in this complex multi-cell scene. We therefore revisited the Grose Valley/Gospers event in detail using native Level-1 dual-polarisation observations from the Terrey Hills radar together with temporally local FIRMS fire detections. This additional analysis is now presented as Fig. S6 in the Supplement.

We examined low-elevation horizontal reflectivity (ZH), copolar correlation coefficient (RHOHV), and differential reflectivity (ZDR) at 03:20 and 03:50 UTC, together with FIRMS fire detections within  $\pm 90$  min and the contemporaneous northern and southern satellite class-5 components. At both times, the low-level radar observations show an elongated plume-related echo extending from the active-fire region toward the northern convective feature. This spatial continuity is identifiable not only in reflectivity but also in the coherent structure of the polarimetric fields.

A distinct low-level smoke/plume echo is also present farther south, between the northern and southern convective features, but this intermediate plume feature is not associated with a satellite class-5 core. Importantly, the southern convective cloud does not show comparable low-level spatial continuity with the active-fire/plume region. Taken together with the temporal evolution of the system, these observations support attribution of the northern convective feature to the fire-generated pyroconvective system, while the southern feature is interpreted as neighbouring meteorological convection.

We emphasise that this attribution does not rely on a unique RHOHV or ZDR threshold for identifying smoke or pyroconvection. Rather, it is based on the combined evidence from fire anchoring, low-level radar morphology, temporal continuity, and the spatially coherent multi-field polarimetric structure.

We have revised the manuscript accordingly and now document this source-attribution analysis explicitly in the Supplement. The production lineage tracking for this event follows the fire-associated northern feature rather than the neighbouring southern convective cloud.

We thank the reviewer for prompting this re-examination of the event. It provided an important additional check on the source attribution and also reinforced the need for the revised lineage framework in scenes where pyroconvection and meteorological convection coexist.

Note the convective cloud in the midst of the large fire perimeter. This feature is mostly cut off in Fig. 6 but shows up wholly in Fig. 7's larger domain. It is this cloud that is documented in P25 as a pyroCb event. Even though Fig. 7 fully reveals this object, the authors maintain a focus on the convective cloud south of the fire. I would encourage the authors to follow their radar echoes back to the onset of both objects to determine which one is pyroconvective by its anchorage to the fire. If it turns out that the southern radar and satellite objects are generic convective clouds, that would reveal implications for the P17 and G26 detections being false positives in this instance. It also has implications for the deployment of the 11-8 BTD innovation. Regardless of the findings, the authors are encouraged to flesh out this "Primary case" much more fully.

**R2C12:** We thank the reviewer for this suggestion. In response, we revisited the evolution of both convective features in the Grose Valley/Gospers case rather than relying on their positions at the times shown in the original figures. As described in our response to R2C11, we examined the native Level-1 dual-polarisation radar sequence together with temporally local FIRMS fire detections and the corresponding satellite features.

This re-analysis shows low-level spatial and temporal continuity between the active-fire/plume region and the northern convective feature. In contrast, the southern convective feature does not show comparable continuity with the fire/plume sector. A separate low-level plume-related echo is identifiable between the two convective features, but this feature is not associated with a satellite class-5 core. On the basis of the combined fire anchoring, temporal evolution, radar morphology and polarimetric structure, we therefore associate the northern feature with the fire-generated pyroconvective system and interpret the southern feature as neighbouring meteorological convection.

The revised production tracking for this case consequently follows the fire-associated northern feature. We have expanded the discussion of this attribution and added the supporting analysis as Fig. S6 in the Supplement. Importantly, this reassessment also reinforces the distinction made throughout the revised manuscript between source attribution and the subsequent radar structural diagnostics: neither echo-top height nor the additional satellite BT8.6–BT11.2 partition is used independently to establish that a convective feature is pyroconvective.

Regarding the “Secondary case” (Figures 8, 9), it is worth pointing out that Peterson et al. (2021), a survey of the Australia New Year Super Outbreak, contains a figure relevant to Figure 9. P21’s Figure 5b shows the P17 pyroCb-detection results for the situation 20 minutes after this manuscript’s Figure 9. P21 qualifies certain objects in the scene as “remnants” of pyroCbs. One such labeled remnant is the primary object of Figure 9. Hence, P21 assessed this as an orphan of a pyroCb in spite of the fact that the P17 algorithm doesn’t allow for that granularity. The assessment relied on following the satellite imagery animations (in addition to radar imagery) throughout the two super outbreak periods and thereby adding nuance to the automated algorithm result. A take-home message here is that in published works such as P21 and P25, the role of the P17 algorithm is informative but not considered declarative of pyrocloud source or nature. The authors are encouraged to cite literature that supports their position that the P17 IPCB mask is misinterpreted as a marker of active pyroconvection. If such literature is present, that would provide a motive for the authors’ investigation. Absent that though, the authors are encouraged to re-focus their science objective.

**R2C13:** We thank the reviewer for this clarification and for drawing our attention to the interpretation of this event in Peterson et al. (2021). We agree that our original framing attributed a stronger meaning to the P17 IPCB mask than was intended in that work or in subsequent applications of the algorithm.

As the reviewer notes, Peterson et al. (2021) identified the relevant feature as a remnant of earlier pyroCb activity by following the temporal evolution of the satellite and radar imagery, thereby adding contextual interpretation beyond the instantaneous output of the P17 algorithm. We agree that this demonstrates an important distinction between the automated satellite classification and the interpretation of pyrocloud source and evolutionary state that can be obtained from the full multi-sensor temporal sequence.

We have therefore followed the reviewer’s recommendation to refocus the scientific objective rather than arguing that the P17 IPCB mask has generally been misinterpreted in previous literature as a direct marker of active pyroconvection. The revised manuscript no longer evaluates whether the P17 IPCB mask or its centroid correctly identifies an active convective core. Instead, we treat the P17-like classification as a satellite-defined pyrocloud signature and investigate how its spatial and temporal evolution relates to the vertical and internal reflectivity structure independently observed by weather radar.

This distinction is also incorporated into the revised lineage methodology. The broader cloud system can remain identifiable through time even when the restrictive class-5 signature is temporarily absent.

Such periods are represented as context-only (“C”) states, which preserve the temporal identity of the associated cloud system but are not counted as class-5 detections and are excluded from class-5 positional comparisons. Thus, cloud-system continuity is explicitly distinguished from the instantaneous satellite classification.

We have also revised the Introduction and Discussion to acknowledge the contextual interpretation used in Peterson et al. (2021) and related work, and we no longer present the motivation for this study as correcting a demonstrated misinterpretation of the P17 algorithm in the literature. Instead, the revised motivation is to quantify systematically, using object-based multi-sensor tracking, the relationship between satellite-defined pyrocloud evolution and radar-observed convective structure.

Figure 1. What is the white dashed line?

What is the wishbone dashed line?

Precisely where is the tropopause? Is it a thick layer between two annotated lines? If so, which ones? Why is the apparent cloud base (sharp transition from smoky brown to white) up near the stratosphere? Shouldn't the radar beams include one that hits the dome of the anvil?

**R2C14:** We thank the reviewer for pointing out the ambiguities in the original schematic. We agree that several graphical elements in the original Figure 1 were insufficiently defined and that aspects of the cloud geometry could be misleading.

We have therefore substantially redesigned Figure 1. The revised schematic focuses on the mature pyroCb stage and simplifies the surrounding graphical elements so that the relevant physical and observational relationships are clearer. The transition from the smoke-dominated plume to the glaciated pyroconvective cloud is now represented consistently with the 0 °C isotherm used in the conceptual framework, and the vertical temperature levels and tropopause are labelled unambiguously. The previous undefined dashed lines and ambiguous “wishbone” feature have been removed.

The radar geometry has also been revised to show explicitly that the radar samples different parts of the three-dimensional pyroconvective cloud, including upper-level echo structure, while the satellite observes the cloud-top radiative signature. The revised schematic additionally illustrates the downwind anvil/orphan-cloud component to clarify the distinction between the evolving cloud-top footprint and the radar-observed convective structure.

The figure caption has been rewritten accordingly so that all graphical elements are explicitly defined.