Review of **Characterization of Free Tropospheric Layers With Polar Radio Occultation Data** by Kubar et al.

This paper presents an analysis of upper tropospheric and lower stratospheric structure using polarimetric radio occultation data, focusing on cloud-top heights (CTOP), lapse-rate-derived metrics (LRMAX, LRMIN), and the cold point tropopause (CPT). They find that LRMAX aligns closely with the tops of heavily precipitating tropical clouds and propose that it serves as a reliable proxy for the TTL base. Additionally, the authors introduce a "modified CPT" definition based on the sharpness of the lapse rate profile.

In its current form, the manuscript is not yet ready for publication, but I believe it has the potential to be a valuable contribution pending substantial revision. There are some worthwhile analyses throughout the study and have no doubt that polarimetric RO data is a valuable tool deserving of increased attention in the coming years. However, this paper spends a lot of its time focusing on tropopause definitions in ways I find to be problematic, and the overall purpose of the study gets lost in this.

We very much appreciate the thoughtful and critical comments by the reviewer. We have spent considerable time implementing comprehensive modifications guided by your feedback and that of reviewer one. Just as a general note, our responses are in red Italics in this PDF.

General Comments:

1. In the title, "Polar" should really be "Polarimetric" in order to not be confused with the polar regions. Also, "Characterization of Free Tropospheric Layers" doesn't feel like it accurately describes most of the work presented in this paper.

Thank you for bringing this to our attention, and we agree with you – it turns out the title on the submitted manuscript was an oversight. It was supposed to be "Polarimetric" all along. To your other point, we have a significantly revised the working title to the following, which better emphasizes both the relationships and the constraints of upper-tropospheric lapse rates especially on upper-level cloud top heights: "Global Upper-Tropospheric Lapse Rate Constraints on Cloud Top Height Using Polarimetric Radio Occultation Data." While we continue to explore different criteria associated with the tropopause heights, part of our objective is to show that convection over the tropics, and the uppermost precipitating clouds over the extratropics, are more modulated/controlled by LRMAX rather than the tropopause itself. It is important in showcasing this to have a robust working definition of the tropopause, hence our effort spent on making sure we have a strong grasp of it.

We note as well that we have modified our description of the new criteria of the sharpness-based tropopause, and we now also include a relative cold point tropopause, which, as we state *in our abstract, is a "tropopause coincident to the smallest (∂LR/∂z) among cold points between 6 and 20 km." Outside of the tropics, the sharpness based tropopause $(\partial LR/\partial z)_{min}$ and the WMO tropopause agree with each other quite well, whereas the relative cold point (with sharpness component) tropopause is about 1 km or so higher than the WMO or $(\partial LR/\partial z)_{min.}$*

2. While your introduction is a thorough literature review of relevant work, it is quite dense and difficult to follow at times, and it doesn't provide strong motivation for *why* you are performing this work. Why does this study matter? What is missing from the previous literature that you are working to rectify? I would suggest revamping this entire section to feel more cohesive and to ensure the motivation for your study is clearly defined.

Thank you, and after separating ourselves for some time from that section (the period between our original submission and receiving your review was > five months), we feel that being more objective is crucial to improving the readability of the Introduction, and you have brought up some excellent points/questions here and throughout your review. The other reviewer also critiqued that some of the exploratory analyses pursued in our study were at times too broad and not as coherent as they needed to be or as we had hoped they were.

Our original Introduction was 1363 words; we have significantly amended the Introduction which now contains about 132 fewer words (1231), but more importantly, it is more responsive to addressing the background more directly related to the topline topics/priorities addressed in the manuscript. We have included the tracked changes in one of the versions which does demonstrate that this portion of the paper was heavily edited. The clean version, with all of the edits accepted, has been submitted as well.

We want to make very clear as well that the Introduction has been substantially revised; some of the points made in the original submitted manuscript, particularly in the Introduction, were repetitive, and furthermore, we now draw a much stronger connection for why polarimetric RO profiles are essential to make the direct collocated comparison between thermodynamic properties and constraints of the upper troposphere, including the Tropical Tropopause Layer (TTL) and Extratropical Transition Layer (ExTL) and thick cloud tops as a function of inferred precipitation strength.

The core of why we are doing this work in part might be highlighted by this modified short paragraph (lines 67 – 72) - note as well that our revised Introduction and manuscript now includes the TTL and ExTL (Extratropical Transition Layer) to better represent the generality between the layering of the upper troposphere and thick precipitating cloud top height. We emphasize the last sentence for clarity just in our response to you here:

> *Because PRO profiles offer the opportunity to individually and systematically quantify the relationship between upper-tropospheric LRMAX and CTOP across all latitudes, understanding what drives the altitude of clouds, the global troposphere, the depth of the TTL/ExTL, and their response to a changing climate make the high vertical resolution of all-weather RO observations and the sensitivity of*

polarimetric RO (PRO) to ice (Padullés et al., 2023) good tools for quantifying subtle changes in stability and concurrent layers of deep convective clouds. ***It also makes the case for global criteria that enable proper identification of the transition between these vertical layers.***

Before that paragraph, however, we do more clearly present now in the third paragraph of the Introduction, a gap in knowledge that we aim to close with our work:

> ***Detection of the bottom and top of the substratosphere from observations, including the transition into the stratosphere with a single global criterion, have remained elusive.***

To your excellent points throughout, the goal of this work is more global and general in nature and less tropics-focused. While we toggle back and forth at times between the tropics and extratropics, the objectives remain having a set of criteria in general.

To that point about treating the tropics and extratropics equally:

> *An analog to the TTL exists in the extratropics, the Extratropical Transition Layer (ExTL), which also more clearly motivates the global approach to our analysis as a possible constraint of upper-tropospheric CTOP. Like the TTL, the ExTL, defined by the World Meteorological Organization (2003), is a region just below the thermal (LR tropopause), and is important since the square of the Brunt-Väisälä frequency, a surrogate of atmospheric stability, starts increasing ~2 km below the tropopause (Gettelman et al. 2011).*

3. Much of the focus of this paper (Sections 3.1 through 3.3) focuses on this "Modified CPT" and its use as a tropopause definition. It is unclear how this metric (minimum of the second derivative of temperature profile) represents a modification of the CPT, and it may be better framed as a novel tropopause definition altogether. Throughout these sections, you tend to compare this modified CPT with the CPT in scenarios where the CPT has no applicability. It is well known that the CPT decouples from the composition transition associated with the actual separation between troposphere and stratosphere outside of the tropics and therefore should not be used outside of the tropics as it has no real physical meaning. Therefore, any work showing how the modified CPT is better than the CPT outside of the tropics is not very compelling in my opinion. I strongly urge you to reconsider whether this new definition is necessary, and if other definitions may suit your purposes well enough (WMO, PTGT, etc). All of this aside, I don't think these sections feel super relevant to the rest of the work looking at CTOP.

Thank you for bringing up this concern, and as we noted above, our goal is less of introducing a new definition of the tropopause but instead laying out a criterion – the height corresponding to $(\partial LR/\partial z)_{min}$, *that offers a physically consistent working metric of using observational data such as high-resolution RO to locate the tropopause. We also realize that the term "Modified CPT" as it applied to what we were/are presenting was somewhat misleading, particularly since we have* *slightly refined our search and only use the CPT as a constraint against possible outliers such that we only search for $(\partial LR/\partial z)_{min}$ between 6-20 km (rather than 25 km as in some cases previously) and only up to 1.1 km above the CPT, given the CPT is the height of the lowest temperature within this altitude range. We also spend some space now stating that we wish for our metric to be consistent with the WMO tropopause in a globally-averaged sense, however, there is additional processing that usually must be done to locate the WMO tropopause (to your next point), and furthermore, it is possible, especially during polar night, for even the WMO tropopause to not be found where the tropopause actually should be, at least for some outliers. This is because LRs may remain above 2 deg/km well into the lower stratosphere due to a lack of stratospheric ozone heating, due to gravity waves, or several factors. We now include an example in the revised Appendix of our manuscript (also shown below) where all four tropopause metrics are unable to capture the top of where the true tropospheric air is.*

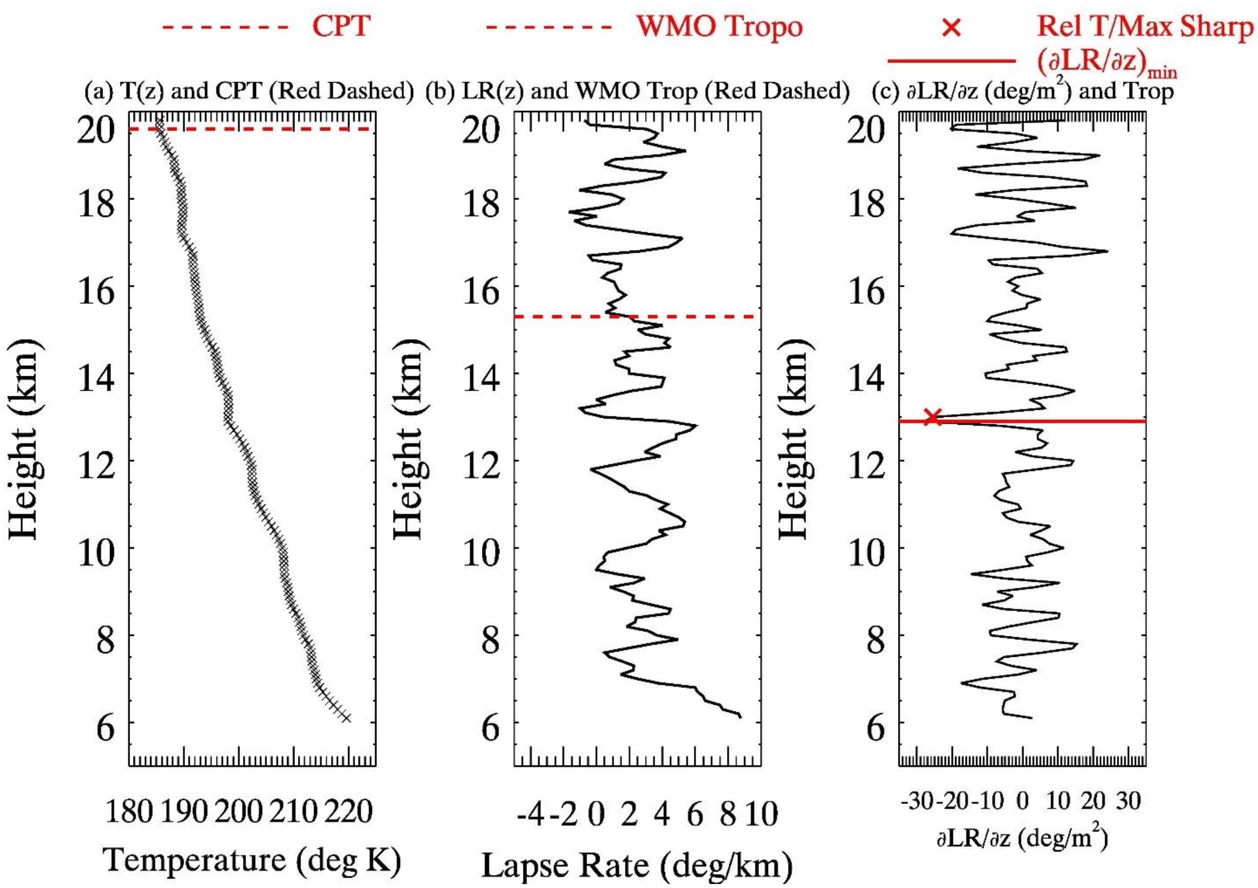

Figure A. Example of a profile from PAZ (26 July 2018) in which none of the four tropopause metrics adequately describe the tropopause over polar night in the Southern Hemisphere at 72°S. (a) Temperature profile and CPT, (b) LR(z) and the WMO tropopause, and (c) ∂LR/∂z profile with the $(\partial LR/\partial z)_{min}$ indicated as well as the relative minimum temperature with the smallest (∂LR/∂z) (red X). All tropopauses are searched for between 6-20 km. The real troposphere likely is below about 7 km, as below that altitude higher LRs are observed.

In the revised manuscript, we have two working tropopause metrics and our goal remains using the discussion about tropopauses to characterize the top of the TTL/ExTL, which typically is separated from the tops of clouds, which are more constrained by the TTL/ExTL base. Part of the impetus of using the second derivative of temperature, or sharpness of the Lapse Rate, was to assess the tropopause as an analog of sorts to the PBL, which the first author of our study has used to define PBL top (inversion base) with conventional RO data (and using refractivity, N, rather than the lapse rate). It is encouraging that much of the time the WMO tropopause and $(\partial LR/\partial z)_{min}$ *do agree with each other. During the revision process, in order to be true to more of a "modified CPT" (a term we no longer use in the paper), relative CPT criteria with sharpness component instead is introduced which gives credence to the fact that the CPT has physical meaning, but is not useful outside of the tropics; using our approach here shows that except for some outliers, it can be applied nearly anywhere. While we introduce a relative CPT here in terms of an additional metric (and with the constraint of a minimum ∂LR/∂z), the general concept of a local cold point is not new, and has appeared in the literature before, particularly the extratropics:*

> *"In particular, high-resolution radiosonde measurements of the thermal and wind structure of the extratropical tropopause region exhibit a strong increase of temperature just above a sharp local cold-point tropopause." (Science Direct: Stratosphere/Troposphere Exchange and Structure - https://www.sciencedirect.com/topics/agricultural-and-biological-sciences/tropopause)*

Finally, we have revamped Figure 2, which instead of scatter plots, now is a figure of the contours of LRs from ~6 km to 25 km; it more clearly highlights the layers of interest of our study, including how the uppermost clouds are entwined to regions of high LR. We feel that this is a more intuitive way of showcasing where the uppermost cloud top heights are with respect to the upper troposphere.

a. Somewhat associated with this, I want to provide a note of caution to ensure that you are implementing the WMO definition correctly. Multiple times in the paper you describe the definition as being "the lowermost height at which an LR threshold of 2 C/km is sustained for at the least 2 km", while the exact definition from the WMO is "the lowest level at which the lapse rate decreases to 2 C/km or less, provided also the average lapse rate **between this level and all higher levels** within 2 km does not exceed 2C/km". I only mention this because the WMO definition is unfortunately frequently misapplied in published works.

*Thank you for bringing this up, and we completely agree that it's essential to not only be consistent with regards to the WMO definition but also to be thorough in its description. We have scaled back the use of the instances in figures in which the WMO definition is presented to Figure 1 (in which we calculate it ourselves) and in the now Figure 3 (old Figure 4). In the now Figure 3, the WMO tropopause comes directly from the Spire UCAR output, and indeed you are correct that the *average lapse rate* between the first level and a minimum of 2km the above* *must have a lapse rate below 2C/km. On line 133, we also include a more precise definition of the WMO tropopause and include a portion of your statement in quotes (with a proper reference as well). Here is what we now write:*

> ***The WMO definition of the tropopause, the lowermost height at which the LR decreases to 2°C/km or less, "provided that the average lapse rate between this level and all higher levels within 2 km does not exceed 2°C/km" (World Meteorological Society, 1957) usually captures the real tropopause, though occasionally, high latitude gravity waves (e.g. Figure A) can lead to a mischaracterization of the tropopause height as in the lower stratosphere.***

4. I find the sections of the manuscript focused more on CTOP comparisons with temperature profile structure metrics to be more compelling, but this gets a bit lost due to the length and somewhat disorganized nature of the paper. Additionally, I think these sections would be even more compelling if it was more thoroughly motivated in the introduction.

Thank you. In fact, our paper now better highlights the LRMAX-CTOP relationships even more strongly, and in the Introduction, we include a more thorough primer about why this level, which might be defined by the TTL base in the tropics or the Extratropical Transition Layer (ExTL) base in the mid-latitudes, is important for the tops of tropical convective clouds or non-tropical thick precipitating extratropical clouds. While we retain some of the pre-existing literature review from before, we have consolidated where possible and sharpened the linkages between the importance between of three primary layers examined in the study – the high LR regions above the PBL to the TTL/ExTL base, where clouds are dominant, the decreasing LRs in the ExTL/TTL up to the tropopause, and then, to a lesser, the often present tropopause inversion layer (TIL) right above the tropopause, which as we now better characterize in the Introduction, has been observed at all latitudes. We have strengthened our description of this often ubiquitous inversion layer compared to the previous version.

But, more specifically, we address your primary concern earlier on, adding the following passage at the end of the first paragraph of the Introduction:

(Lines 32 – 35): "Polarimetric RO (PRO) data add the sensitivity to ice to the high vertical resolution of all-weather RO observations (e.g., Padullés et al., 2023). This enables analysis that quantifies subtle changes in thermal stability associated with deep convective clouds and, even more globally, non-convective raining clouds in the extratropical upper troposphere in a way not entirely possible or suitable with other passive or even active satellite sensors."

Later on, we have the following in our substantially updated portion of the Introduction, which in tandem addresses your appropriate concern about our previous hand-waving distinction between the tropics and extratropics (lines 67-71):

> *Understanding what drives the altitude of clouds and the CPT, the depth of the TTL, and their response to a changing climate make the high vertical resolution of all-weather RO observations and the sensitivity of polarimetric RO (PRO) to ice (Padullés et al., 2023) good tools for*

quantifying subtle changes in stability and concurrent layers of deep convective clouds. It also makes the case for global criteria that enable proper identification of the transition between these vertical layers.

We noted above as well in part of our response to #2 that we introduce the ExTL as the extratropical analog (albeit imperfect) to the (tropical) TTL which motivates the metric employed, the height of the maximum lapse rate (LRMAX) to test as both a physical and statistical boundary for upper-level precipitating cloud tops.

In other spots (in the Introduction), we also tighten up the connection between the enhanced stability from the top of convection to the tropopause height, in a way that, to your point, was insufficiently clear before, while still invoking appropriate sources. Here is an example on lines 55-57:

Water vapor has a strong peak in net cooling at or just below 200 hPa, near the top of deep convection, whereas CO_2 warming above stabilizes the layer between the top of convection and the tropopause (Thuburn and Craig, 2002).

5. The figures throughout the paper are quite messy and hard to digest, I would recommend thinking about different ways to show the wealth of information you are trying to convey in a more concise and digestible manner.

We appreciate this critique, and after reviewing the paper in its entirety with fresh, objective eyes, realize that too much information likely was trying to be packed into some of the figures, and many of them have been revamped or significantly updated. Others have been streamlined, or in many cases, simplified. In fact, only the original (and still existing) Figure 1 has not been revised at all! Here is a synthesis of some of the detailed changes that have been made:

Figure 2 now presents contours of LRs in the troposphere (and lower stratosphere) versus latitude as well as the mean height of the tropopause (using our sharpness-based criteria, $(\partial LR/\partial z)_{min}$), the 75th and 90th percentiles of CTOP, and mean LRMAX. This replaces the previous version which was a scatter plot, and might have been somewhat less clear (and for which there was a fair amount of criticism from the other reviewer).

The previous Figure 3 of the frequency of double tropopauses and the linkage with the meridional gradients of the height of min(dLR/dz) has been completely removed as double tropopauses are not a main thrust of our study, but more of a sidebar. However, when we checked the meridional gradients of the various tropopause metrics, including the newly introduced relative CPT (not shown), all of the results were broadly consistent with each other. That said, one question of the other reviewer had been why we hadn't just used reanalysis data to do much of the tropopause work, since the perception was that reanalysis data may filter out gravity waves. We still retain our Figure 1 which is an example of a double tropopause, but our primary response was that we have not explicitly attempted to removed the effects on temperature perturbations by gravity waves, but rather demonstrate that our tropopause *metrics are physically robust in picking out the tropopause signal for high-vertical resolution, potentially noisy temperature profiles.*

The old Figure 6 has been removed, as it was showing some of the differences between PAZ and Spire in terms of their relationships with CTOP, but such an intercomparison really wasn't the objective of our paper, and furthermore, the CTOP/LRMAX geographic relationships are already included in the new Figure 2, so the old Figure 6 was somewhat redundant anyway.

The new Figure 5 now includes maps of all the parameters shown before, but includes the high latitudes to partly address your point about the lack of global representation in some of the figures. This also better highlights some of the points about the very cold tropopause over the tropics and the lower and warmer tropopauses over the extratropics, a point that has been made many times before but is clearly highlighted here, along with some of the asymmetries of the extratropics between the northern and southern hemispheres. This figure also contained some errors previously, and now should be much clearer.

The new Figure 6 (old Figure 8), is similar to before, but because of some slight smoothing that we employ using three-width boxcar smoothing, some former LRMAX outliers were removed, slightly improving the correlation between LRMAX and CTOP when the Δϕ-0.8mm threshold is used; the 0.65 mm threshold for Δϕ is also now acceptable, but only in the context of presumed heavily precipitating clouds. This is because for such clouds, the signal below is strong such that there is more confidence that the inferred cloud top is really connected to the primary cloud.

Finally, both you and the other reviewer thought that too much information was being shown previously on the histograms, and we have removed all of the sub-regional analysis curves, as there is not sufficient space to discuss the importance of those analyses, and furthermore, we agree that they took away from the overall message(s) of the histograms. The new CTOP minus LRMAX versus $\Delta\phi_{max}$ histograms are now presented for PAZ (top) and IMERG/PAZ (bottom), with nothing else overlaying. We also have changed the x-axis to be logarithmic which expands the part of the domain with non-raining or lightly precipitating profiles.

*Instead of **10** main body figures as before, we now have **8** figures.*

6. Throughout the paper, it is unclear how much of your analysis is focused upon the tropics versus the extratropics. For example, in the third paragraph of the introduction, you go from talking about the TTL, which is in the tropical regions by definition, to the tropopause inversion layer, which is a primarily extratropical feature. Please ensure that in your methods, analysis, and discussion that you are clear if you are focusing on the tropics, the extratropics, or both.

One of the first changes we made was removing the reference of the TTL in the abstract, and replacing it, where appropriate, with LRMAX, which is the universal metric with which we evaluate against CTOP using polarimetric RO data. This actually also ties in to your valid point more broadly about the apparent *disconnect of the previous version of portions of our Introduction and a fairly significant swath of our paper; our analysis really represents more of a near-global one, and the Introduction has been substantially rewritten. We admittedly were focused a little too much on the low-latitudes previously, and while fundamental concepts of the TTL motivated the some of the goals of this work, we have bridged the tropics and extratropics more comprehensively now with better references and contexts to analogs of the TTL in the extratropics – namely the Extratropical Tropopause Layer (ExTL), to showcase some of the similarities between the two layers and why defining and using LRMAX even outside of the tropics is still an appropriate constraint to test of upper-level tropospheric cloud tops.*

We also alluded to it earlier in our responses, but we now include references and a description about how the Tropopause Inversion Layer is more of a global phenomenon than might be traditionally understood. But, in a nod to your point, the TIL in terms of nomenclature often is reserved for the extratropics, even though there is an equivalent layer in the tropics:

Lines 49-54:

> *Though radiative-convective equilibrium calculations often fail to capture the frequent presence of a thermal inversion, it has been reported at all latitudes just above the tropopause (Birner et al., 2002; Noersomadi et al., 2019). O_3 radiative heating, or sudden stratospheric warming events in the extratropics (e.g. Zhang et al. 2019) can give rise to what is often referred to as the Tropopause Inversion Layer (TIL) (Birner et al., 2002; Randel et al., 2007). In the tropics, this layer of maximum stability immediately above the CPT is often a very strong inversion (e.g. Sunilkumar et al. 2017; Noersomadi et al., 2019). We refer to the altitude of this enhanced stable layer as the minimum lapse rate height, or in our study, the LRMIN.*

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