We again thank the reviewers for their time analysing our manuscript and response. While we are disappointed that reviewer 2 still raised strong concerns, we are convinced that this is primarily due to misunderstandings and have made further changes to reduce the risk that new readers would share these misunderstandings.

Regarding the physical reasoning for our results, we have added a supplement to provide simpler examples and more explanation but believe that most readers would intuit the concept from the main text alone.

We wholly agree with reviewer 2 that accounting for 3-D radiative effects, particularly scattering into nominally clear-sky footprints from nearby clouds, is important. However, this paper overcomes a fundamental bias caused by the direct solar path alone, and so is an important step. We are gratified that reviewer 3 agrees that publication is still justified given that we describe these limitations and identify how to experimentally address them. For further specificity we re-titled the paper "New sampling strategy mitigates a solar-geometry-induced bias in sub-km vapour scaling statistics derived from imaging spectroscopy", edited the abstract and added some later text. The title is unwieldy, but still shorter than some other recent AMTD submissions.

We have changed one sentence requested by reviewer 1. Given the judgment by reviewer 3 we believe our paper has now fully addressed review concerns.

In addition, line- or marker styles were changed in Figures 5 and 6 after a colour-blindness simulator suggested some groups were hard to tell apart. The LES outputs necessary for reproduction have also been uploaded to Zenodo and linked in the paper. Finally, the copyright statement has not been removed from the main file – I need a form that the JPL copyright office can use to transfer copyright.

Reviewer comments stay in black, our general comments are in red and change descriptions are in magenta.

Reviewer 1
"manuscript version 3, p. 9 line 24:
"...our aerosol gradients induce a factor of 2 change in DRY_7200s and 10 % in other timesteps"
This sentence is not clear.
In which parameter is the change observed: TCWV or zeta_2 ?
Include "LES timesteps"."

We broke up this sentence and rephrased:
"In the DRY LES run our aerosol gradients induce a factor of 2 change in ζ_2 in timestep DRY_7200s and 10 % in other timesteps. This larger relative error may be related to their small spatial variability of TCWV and low values of ζ_2."

This is the only comment from reviewer 1.

Reviewer 2
Problems with the "Emulator": Although more details are added about their "Emulator" in the revised manuscript, the fundamental problems with it still remain. The authors argue that they can simply "emulate" the impacts of 3-D radiative transfer effects and the parallax effect

on TCWV retrieval without using the advanced 3-D radiative transfer model. I simply do not see how this is possible. The author can certainly derive a PIWV based on Eq. (4) and a LES filed, but that is not what the instrument observes (which is radiance or reflectance). A plane-parallel RT model (MODTRAN) is used to "generate the true forward radiance spectra" (page 6 line 9) in this study. But isn't the "true radiance spectra" the observed spectra that are affected by the 3-D radiative transfer effects and the parallax effect? How could a 1-D model generate the "true" observation? It is mentioned that "in this case, TCWVeff=TCWV" (page 6 line 12). To have TCWVeff=TCWV, we need to have Eq. (6) = Eq. (7) (BTW, Eq. (7) is wrong), which implies PIWV=PIWV_uniform in these simulations. But isn't the whole idea here is to simulate an ununiform PIWV? In summary, to me there are fundamental problems to the methodology of this study (emulator) which make the results based on it highly skeptical.

From reviewers 1 & 3 we judged that our description of the limitations and how future work could address them was sufficient to support publication regarding 3-D RT. Nevertheless, we made further changes to be more precise in our descriptions.

We suspect we are referring to the effect that the reviewer calls "parallax", but a standard definition of parallax is "a displacement or difference in the apparent position of an object viewed along two different lines of sight" (Wiki) but that isn't what's happening here with our single line of sight.

We show that the direct solar path, on its own, causes an insurmountable bias in derived ζ_2 when calculated in the solar azimuth direction. We have changed text (see below) to be explicit that we address *only* this bias, and that a perpendicular calculation overcomes it.

Our emulator is developed (or trained) using plane-parallel radiative transfer, effectively assuming horizontally-uniform water vapour fields. However, when applying our emulator, the input $TCWV_{eff}$ is derived from tracing through non-horizontally-uniform fields. There aren't substantial differences in the shape of $q(\boldsymbol{r}_{\downarrow\uparrow})$ between the training (plane-parallel) and forward-simulation (3-D field) sets so this emulator should work. We believe that our description is already clear enough on this, based on other review comments.

The reviewer also seems concerned about a 3-D factor we do not address, namely scattering into nominally clear-sky footprints from other parts of the sky, primarily nearby clouds. However, we note this limitation and reviewer 3 concludes that this note is acceptable. Text is changed to emphasise this distinction between direct-beam and out-of-footprint factors:

The abstract has been rephrased, including with new text:
…accounting for realistic non-vertical sunlight paths. We trace direct solar beam paths through large eddy simulations (LES) of shallow convective PBLs, and show that retrieved 2-D water vapour fields are "smeared" in the direction of the solar azimuth. This changes the horizontal spatial scaling of the field primarily in that direction…
And:
…By only considering the direct beam we neglect 3-D radiative effects, such as light scattered into the field of view by nearby clouds. However, our proposed technique is necessary to counteract the direct-path effect of solar geometries and obtain unique information about sub-km PBL q scaling…

And in Section 1 the added text includes:

…We show that, in a set of 23 LES snapshots, the non-vertical direct-beam path prevents accurate retrieval of sub-km ζ_2 when standard methods are naïvely applied…
And:
…Diffuse sunlight is handled through a plane-parallel radiative transfer approximation, which means that complex 3-D radiative effects are neglected. In clear-sky areas near clouds, 3-D effects can brighten observed spectra (Várnai and Marshak, 2009), with induced biases of order ~0.25 % for VSWIR column CO_2 retrievals (Massie et al., 2021). The consequences for hyperspectral TCWV retrievals at 30—80 m horizontal resolution are not currently known, although the effect on retrieved ζ_2 would depend on the spatial scaling of these TCWV biases…

• As I mentioned in the first round of review, this paper only presents a phenomenological study of the simulation results, it provides little, if any, explanation of the underlying physics. I think this is due to the use of "emulator" which cannot provide any meaningful explanation of the simulation. Without a solid physical interpretation, these results are highly skeptical to me.
• In section 3.3, the authors found significant differences between the "parallel" and "perpendicular" direction retrievals. Again, there is no explanation of why. Nevertheless, the lack of physical explanation does not prevent the authors from proposing a new sampling scheme to all the possible sensors. To me this result is skeptical to say the least. Any point on the surface can be considered to be either "parallel" or "perpendicular" (or both parallel and perpendicular) to sunlight direction, isn't it? What is the fundamental difference between any two points in terms of geometry?

Our interpretation is that these two bullets boil down to "there is no physical explanation". We believed that we provided this explanation, and reviewer 1 correctly interpreted this as being "due to a solar light path traversing neighboring pixels" and found that the approach was "evident" with a "described bias reduction is what one would expect".

Our explanation was clear for some types of readers, but clearly not others. To try and be as widely understood as possible, we added a supplement and referred to it in the main text:
for a simplified illustration of the physical principles behind why our strategy is anticipated to reduce biases in ζ_2, see Supplementary Figures 1—3.

The new supplement and its figures show a simpler idealised situation, including a visualisation of the smearing effect and explicit calculations of spatial statistics: means, second-order structure functions, and ζ_2. We also make an analogy to "motion blur" in image processing, which is pretty widely known and shares much with our problem. For example, the appearance of objects in an image is "smeared" or "blurred" when there is relative movement between the imager and objects within the plane that is normal to the image-object vector.

In particular, objects are apparently "blurred" or "smeared" *preferentially in the direction of motion*, and the apparent spatial structure (which can be captured by statistics), is *also preferentially affected in the direction of the blurring*. Therefore, the direction in which you calculate statistics can matter.

Our problem is analogous: the TCWV field is "smeared" in the solar azimuth direction, in a way that shares features with motion blur. We use the term "smearing", rather than "blurring" since it is a synonym for the visual effect but “blurring” is associated with motion. As mentioned above, we do not call it “parallax” since we only have one line of sight.

We thought that this was an intuitive result, but our initial description was too terse. We think the added text following the initial review round, combined with the supplement, strike a good balance since for many readers (such as reviewer 1), the process should be intuitive and obvious, and for them the supplementary text would labour the point and distract from the main results.

Overall we now believe that with the supplement we have addressed potential confusion in a balanced way.

Reviewer 3

Reviewer 3 largely addressed the issues raised by reviewer 2 and judged that our response was adequate for publication.

They raised some further good points so we added two new citations; Varnai & Marshak (2009) and Massie et al. (2021). They are in the reviewer 2 response text above, and although they do not match the ones recommended by the reviewer, we believe they are the most appropriate references for the point we are making.