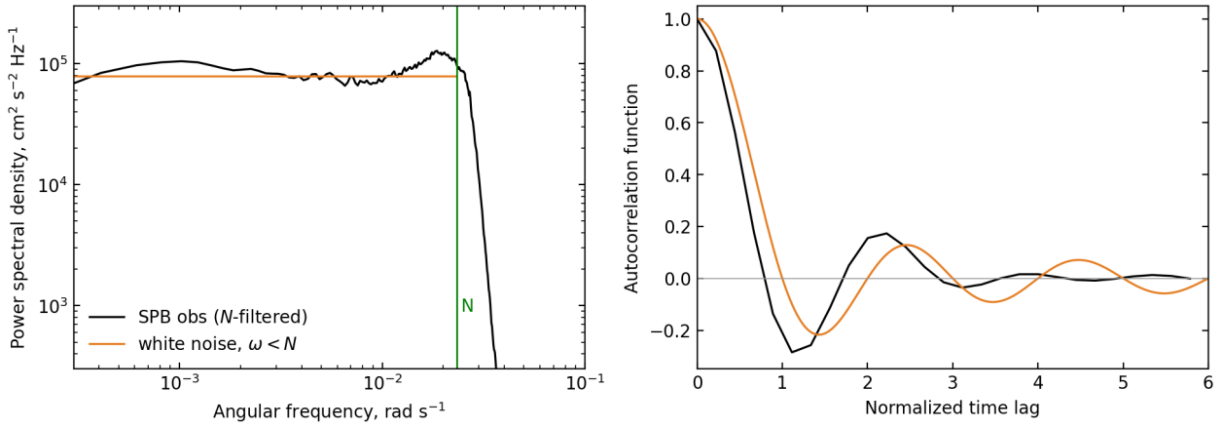
I appreciate this manuscript's thorough depiction of the vertical wind statistics from the Strateole-2 campaign and discussion of how these link to ice nucleation simulations. I would like to ask Bernd to please delve a bit further into the nature of the observed wind statistics, to make more concrete recommendations for modelers, and to refine the manuscript format.

Major comments

1) From Fig. 2 the filtered observations match a Laplacian shape to an impressive level, though I don't see such a match to any presented model for the autocorrelation (Fig. 3) or power spectrum (Fig. 4). There are statements about some of the models being more suitable than others here, but to me their level of bias appears similar. I'd like to lightly challenge Bernd to consider if the assessed measurements raise the need for a more suitable model. Along these lines, the current models follow fits for the autocorrelation function (triangular or exponential), but it does not appear to have been noticed that the data suggests the power spectra to be more fundamental. As I show below (noting I was not able to perfectly replicate the data), assuming white noise up to frequency N (with zero at faster velocities) produces an autocorrelation spectrum that has sinusoidal elements and is far more similar to the SPB obs than either the triangular or exponential function. Could something like this be translated into a more suitable analytical model? And I am wondering if the manuscript's current interpretation of the wave elements in the autocorrelation function, which cites trapping at wave duct boundaries, is perhaps more complicated than the actual reason.



2) The majority of the manuscript is in the "Data and Methods" section (Sec. 2), while the current results section (Sec. 3) is quite short. I think it would be easier to follow the flow of the article if there were, for example, a shorter section introducing the methods, then a first major results section on how the SPB measurements compare to statistical models, and then a second major results section on the implied ice nucleation distributions. I'm assuming that most of the material on the SPB statistics is novel enough to warrant presentation as a result. There is currently a daunting number of subsections in Sec. 2 and I think generally the organization of the manuscript can be improved.

3) I like the concept of linking the statistics of the vertical wind observations to those of available models, though am a bit concerned the ice nucleation section (Sec. 3) is very similar to analyses already done in Jensen et al (2026), especially that Fig. 6 here is extremely similar to Fig. 8 of J26. I'd like expressed in the manuscript why this analysis warrants a new iteration, including whether the use of DICE here is an improvement versus CARMA in the previous study. I appreciate that there is benefit of having the statistics (Sec. 2) and the ice nucleation outcomes from such representations (Sec. 3) together in one study, so I wonder if the latter (currently quite short) can be a bit expanded to link better to the preceding sections.

4) There's a stated aim that this study "offers pointers" for modelers, but being myself a modeler I'd appreciate some more concrete info and guidance. For instance: How advanced are the current approaches in standard GCMs compared to what is shown here? Do the Laplacian forms here represent a substantial path toward

improvement over simpler Gaussian representations in models? If so, how exactly would models ideally be altered? Conversely, could it be worth keeping Gaussian (or other simple) representations but tuning these to the SPB obs? Is there sufficient confidence that scaling distributions relative to Strateole-2 measurements are representative of cirrus in other regions and at other altitudes (noting a GCM is unlikely to have multiple cirrus schemes at once), or does there continue to be a large gap even if recommendations here were to be followed? There is a section on "Implications" but it doesn't address these. Also, coarse 100km-scale GCMs are gradually becoming dated, so it could be worth briefly expressing how implications translate to km-scale models (i.e. global storm-resolving or limited-domain convection-resolving models).

5) I tried to replicate Figs. 2-4 but found the paper does not currently have all needed info:
a) Which specific version of the Herzog 2019 dataset is used? (v3b?)
b) I don't see w' in the Herzog 2019 files, so how exactly all the w' values calculated? Is any other dataset used in this process?
c) What quality control is applied to get to 695,104 data points from the larger Herzog 2019 files? (the raw data across all flights has about 710,000 points, so maybe small segments are removed?)
d) How are gaps treated, and are all flights simply combined as if a single continuous dataset?
e) Is all needed info on the filtering approach included?
f) Is it possible to reproduce the ice nucleation plots (Figs. 5 & 6)? Is DICE publicly available online, and the needed details stated in the manuscript?

Minor/specific comments

- Lines 10-11: It is advertised as a significant result that, as the abstract states, "the causes of small discrepancies in ice concentrations are traced back to filtering of the balloon data necessary to remove non-geophysical artifacts and differences", but this seems to be speculative, I cannot follow the logic, and "small" feels a bit over-forgiving. Is it possible for some visual support to be added to the manuscript?

- Line 32: The "due mainly to" fragment doesn't logically follow the beginning of the sentence.

- Line 37: "Due to their smaller horizontal wavelengths […] high-frequency GWs are closely related to deep convection" – I don't follow a causal effect here.

- Line 39-41: Is there an explanation for why GWs of relatively high frequencies, but not more than N, are most important? Considering both the speed of an updraft and its duration, has anyone already formulated why this appears to be a 'sweet spot' and a citation can be added? This feels like a quite interesting and relevant problem worth addressing.

- Line 57: Section 2 feels like part methods and mostly results, and most figures are associated with this section. I'd suggest breaking off the statistical analyses of the SPB observations and their comparisons to simple fittings as a results section, and reorganizing the methods section accordingly.

- Fig. 1. Can the times shown in the inset be stated for replication purposes?

- Lines 77-78: Can it be explained physically why w' closely follows a Laplacian? Also, is this a very specific outcome of the TTL? I would think in much of the tropical troposphere there is asymmetry with stronger updrafts and more extensive weaker downdrafts.

- Lines 92-97: I'm wondering, along the lines of my first major comment, if the autocorrelation function for the filtered observations is perhaps more explainable than the speculation here conveys. I think it is important to get this right, because it is clear that the shape is fundamentally neither Laplacian nor triangular and so appears to signify a need for an improved model.

- Line 157: replace “short-term autocorrelation” to “triangular autocorrelation” to link better to the models shown in the preceding sections?

- Lines 194-195: How does OU30 compare to current GCM approaches? I would think even having a Gaussian distribution with hooks from this to ice nucleation is currently relatively advanced. Also, it might be better for the arguments of the paper to additionally show a cruder model than this if one is standard in GCMs, in order to point out more effectively what ability for improvement already exists.

- Line 205: I don’t see this parameterization type used anywhere so (if I haven’t missed something) am wondering if it warrants its own section. Also, generally it would be beneficial to know which ice nucleation schemes (and which GCMs that use them) are in each basket. And does Kärcher & Lohmann 2002 (and related versions) connect well to any of the categories presented here?

- Lines 210-254: The “notes” feel a bit detached from the rest of the manuscript and breaks the flow. I’d suggest either incorporating them more closely or moving them to an appendix.

- Line 268: While vertical winds are certainly a major control, do the simulations make assumptions on other controls worth mentioning? E.g. an assumed temperature, or an initial relative humidity?

- Line 273: What is “one ensemble, based on 39,038 data points”? This is many fewer than the 695,104 data points used elsewhere.

- Line 279; isn’t median < mean “right” skewed?

- Line 283: 144 cm-3 isn’t really “far off” scale given the axes are logarithmic - “off” would suffice.

- Fig. 6: Why not include OU30 in the inset? And please in answer to the relevant major comment reconcile the similarity to Jensen et al (2026) Fig. 8 with how this new version is warranted.

- Line 291: Can it be clarified if these are scaled to match the SPB obs and exactly what parameters are used for this?

- Line 319: the first Conclusion paragraph seems like it could be combined with the Summary subsection.

- Line 347-349: The causes of discrepancies would seem quite important for this work but we seem to be left with speculation, whereas I think these causes might not be so mysterious (c.f. major comment 1, and I’m sure other analyses would be able to pin it down further).

- Line 357: Fig. 3 looks more promising for the the exponential fitting in FL21 than the triangular one of KP19, because – even though the shape is neither exponential nor triangular – it does decline first sharply and then less sharply. And the ice nucleation simulations to me appear to have similar biases with KP19 and FL21. To say KP19 is more supported seems to me a slightly surprising conclusion.

- Line 366-373: Can some evidence be shown for the information in this paragraph about trapping and filtering, if there is confidence in the material?

- Line 480 – The data availability statement here hasn’t been filled in, even though the manuscript file has a separate data availability footnote that mentions Herzog 2019.