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Comment on acp-2021-360

Anonymous Referee #2

Referee comment on "Hemispheric contrasts in ice formation in stratiform mixed-phase clouds: disentangling the role of aerosol and dynamics with ground-based remote sensing" by Martin Radenz et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-360-RC2, 2021

Review of the paper 'Hemispheric contrasts in ice formation in stratiform mixed-phase clouds: Disentangling the role of aerosol and dynamics with ground-based remote sensing' by Radenz et al.

Summary:

Radenz et al. use a comprehensive suite of observations for three contrasting ground-based locations in order to quantify and understand differences in ice formation. They conduct a comprehensive analysis of aerosol and cloud properties derived from the ground-based sensors in order to reach four main conclusions: (1) average backscatter is fairly close between sites; (2) ice formation at relatively warm temperatures is more common than previously thought; (3) clouds coupled to the aerosol-rich BL show more ice formation; (4) gravity waves form liquid clouds over Punta Arenas and bias the cold statistics. I commend the authors for their thorough analysis and note that the four key points I summarised above (From their abstract and conclusions) are all important conclusions.

Although I suggest 'major revision' due to one major comment directly below, I think that this can easily be implemented (along with the minor comments).

I would strongly support acceptance of the revised manuscript by ACP. It will be a very good contribution to the literature.

Major comment:

Line 263. On the use of the Fast Fourier Transform (FFT). The authors described how the vertical velocity time-series is the input into the FFT in order to calculate power spectral density. However, in lines 253-254, the authors state that 'vertical velocity is sampled at the pixel with the maximum backscatter out of the heights…', and illustrate this as the dashed line in Figure 2c. Note that this varies in height. In other words, the authors are mixing a height and time series as use for input for the FFT. The series for input into the FFT should be time- only: it is not correct to incorporate information from a range of altitudes in your time-series. While I suspect these effects are likely small and won't change the conclusions, the authors should reprocess their data, using the vertical velocity at a constant altitude, for each cloud event. I leave it to Radenz et al. to choose what the altitude is for each cloud, but if you would like a suggestion, then perhaps use the mean (or median) altitude of peak backscatter for each cloud. Please propagate these changes through the manuscript.

Minor comments:

Line 83: McFarquhar et al. paper is now published (2021), see https://doi.org/10.1175/BAMS-D-20-0132.1

Table 2: I struggle to believe that Limassol's climate is 'northern tropics'. For a start, it's at 35N, well outside the tropical region. I suggest you change this phrase to more accurately reflect the climate zone Limassol is in.

Line 190: You use ECMWF IFS analysis for temperatures over Punta Arenas. The lack of radiosondes is somewhat unfortunate, but I fully agree with the authors in using an analysis product instead. So, some comment on the uncertainty in temperatures at cloud top height should be included though. While I doubt these uncertainties would often push your results into adjacent 5C-wide temperature bins, the IFS temperatures will carry some uncertainties. Perhaps a Monte-Carlo simulation could be performed to test how a 1C or 2C uncertainty in cloud top temperature, of your thin stratiform clouds, changes the results. From my experience in comparing remote-sensing retrievals (radar and/or lidar), the (re)-analysis temperatures do not always match up with the melting level or cloud top inversion height.

Line 255 onwards: Can you add a figure, perhaps in an appendix, showing a typical example of clouds which are influenced by orographic waves please? It would be informative to visually see the differences as observed by your ground-based instruments.

Line 267: You need to explain why you select the 0.8 autocorrelation threshold. What happens when you vary this by e.g. changing to 0.7 or 0.9? How do the results change?

Line 268 (approx.): The autocorrelation method is a good idea to determine the influence of orographic waves on cloud phase, and, as the authors describe, necessary at locations like Punta Arenas. To provide the broader context for wave forcing of clouds over Punta Arenas, and an additional verification on their method, I suggest that the authors composite the synoptic scale meteorology. I'd suggest using a reanalysis (ERA5?) at the closest time-step to the observed clouds, and compositing surface pressure and 10 m wind fields (as vectors) for the two cases of orographically-forced waves (autocorrelation>0.8, using your threshold in the paper) and no forcing (<0.8). In general, orographic waves form when relatively strong near-surface winds impinge upon a mountain range approximately perpendicularly (within 45degrees of this, e.g. see Section 5 in Dornbrack et al., 2001, JGR, https://doi.org/10.1029/2000JD900194). This is commonly observed in the Andes further north which are aligned north-south, but, around Punta Arenas, as the authors know, the topography is very complicated. The southern tip of the Andes is likely a unique region for cloud phase in the whole Southern mid-latitudes, so I think that understanding some of the synoptic features which generate the waves and result in enhanced amounts of liquid water would be useful for the community.

Figure 8: This is a nice, and informative, figure. However I wonder whether it’s possible to increase readability, without losing the message, by plotting the average autocorrelations and PSDs in e.g. five degree temperature bands?

Line 354: You could cite McFarquhar et al. (2021) here to support this point, who showed the measured INP values over the Southern Ocean and who also noted the ~3 orders of magnitude lower values than were reported in the 1970s…

Line 402-403: You could expand your comments on Figure 7b as you only have 1 sentence at the moment.

Line 464: To support the statement that INPs in the BL are of biological origin over the Southern Ocean, how about citing Uetake et al. (2020) https://doi.org/10.1073/pnas.2000134117

Technical corrections:

Line 70: '…higher aerosol load allows us to advance…'

Line 84: 'But, apart from the year-long…'

Line 103: 'The goal of this…'

Line 113: '…and allow us to…'

Figure 2: Suggest you mention in the caption where the temperature is obtained from to avoid the reader having to dig through the text. You should also describe in the caption what the dashed line in Figure 2c is.

Line 241: '… horizontal extent of…'

Figure 5: Caption: 'red squares in (c)…'

Line 380: 'Limassol nearly all clouds…'

Line 380: Why not say '…fraction of (0.6 +/- 0.1) of shallow…'

Line 405: Well, 'The lack of ice-containing cloud layers…' seems pretty strong statement to me. Looking at Figure 7b, you still have them over Punta Arenas for about half of the clouds. Suggest rephrasing to 'The reduced fraction of ice-containing clouds…'

Line 446: 'absence of continental aerosol species…'

Line 453: 'Murphy et al. (2019) argue in a …'

Line 497: 'CloudSat'

Line 520: '… with orographic gravity waves…'

Figure A1 & A2: Add units to x-axes