

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2021-100-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on acp-2021-100

Anonymous Referee #1

Referee comment on "An analysis of an extreme wind event in a clear air condition associated with a low-pressure system during ICE-POP 2018" by Chia-Lun Tsai et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-100-RC1>, 2021

Review of manuscript entitled "Observational study for strong downslope wind event under fine weather conditions during ICE-POP 2018" from Tsai et al.

Recommendation: Reconsider after major revisions

Summary

The manuscript discusses observations obtained during a downslope wind event in a coastal mountainous setting, the Taebeak Mountain Range in eastern South-Korea, during the winter of 2018. The manuscript aims to explain the acceleration of winds in the lee slopes in a coastal setting, using data obtained in an upstream environment that encompasses a valley that narrows towards the coast. While the data seems quite abundant, and the authors have clearly done an extensive job in figure creation and additional analysis, it is unclear what scientific problem the manuscript aims to discuss. There are also quite a few unclear steps taken in the analysis approach, which need to be addressed. I'm in between major revisions or reject, but want to give the benefit of the doubt at this stage to enable the authors to improve their manuscript substantially. Please refer to the comments below.

Major comments:

- Data and period selection. Although the authors state that this day was chosen because some parts of the Olympic games were postponed, it would be interesting to know how this relates to climatology of wind events in the area. That would emphasize better the importance of the study. Was this the strongest wind event in the lee slopes at WWG? Or was it just the only event that could be considered as strong?
- The study presents a mixture of model simulations and observations, but this is not clear from title, abstract or methodology section, and should be emphasized. More important in this comment is that at times it is unclear whether the authors present observations, simulations or both? In the end, once the WISSDOM is used, this is a mix of observations and numerical output and therefore the study cannot be presented as observations alone. Additionally, not much is discussed regarding the WISSDOM data (how accurate is the approach?), nor the inclusion of the numerical model data into the

WISSDOM. Science is about understanding the uncertainties in the data presented, but the authors do not seem to discuss any of it.

- Some critical explanation of data usage and data treatment is missing. For example, a trend of wind change represented as a percentage per hour is maybe a different way than normal, but just in the sense of diurnal variability it does not make sense. Wind speeds at the surface change over the course of less than 15 hours (the time frame the authors chose for this figure), and so this could also be clearly within the diurnal variability of winds. Is it just a trend based on hourly data? Second, it is unclear what perturbation pressure and temperature represent, as these are not defined. Third, there is a nationwide plot that presents AWS stations, but these are not introduced in the data and methods section. There are some other examples that I leave to the authors to read in minor comments below.
- A few statements made in the paper seem incorrect or speculative of nature. Please see comments below.
- The manuscript is quite poorly structured. Section 2 with 2.1 explaining lidar on itself is very dense, while 2.2 is a combination of brief explanation of AWS, sounding, wind profiler and model (!) simulations. Section 3, 4 and 5 basically contain the full results part and could possibly be combined in one section. In the present structure, it is hard to understand what problem the manuscript is trying to address.
- The manuscript is full of grammatical errors, and the phrasing is hard to read. I highlighted only a few, but please let it proofread by an English native speaker, or perhaps pass it through a professional editorial company. While one can clearly not argue on writing style, the text has to be comprehensible, and, unfortunately, in the current state this is not the case.
- There are a handful of studies that use lidar observations to explain downslope windstorm events. Please include these studies. A simple web search would suffice here.
- A textual note is that the paper is full of abbreviations. Please consider introducing a table that would summarize instrument platforms and locations. This would help the reader greatly to refer back to.

Minor comments:

Line 37. Fine weather... What is fine weather? Fair weather? Or just pleasant weather? In the latter, one would not expect much wind... Or is it related to cloudless skies? Please be specific.

Lines 60-61. It seems very strange to start a paper describing a downslope wind event with a precipitation statement. Suggest to delete this phrase.

Line 67. Fine weather... What is this?

Line 78. "usually occurs at the lee side". By definition, the downslope windstorm occurs at the lee side a mountain range. Please correct.

Line 80-81. "explained by hydraulic jump". Please correct to "accompanied with hydraulic jumps".

Lines 116-117. "Wider ... conditions." Redundant phrase. Please delete.

Line 118. "the best solution". Arbitrary statement. Please change to "one approach to obtain more complete wind data is the use of Doppler wind data".

Line 157-169. This is a nice overview that is somehow lacking for any of the other observational platforms.

Line 165. "100 km". This is probably not true, please address.

Line 169. "0.04". Why this value?

Line 170. LDAPS is derived from model simulations? This needs to be emphasized, as it looks now as if this is an observational dataset.

Lines 171-177. Please provide more detail on measurement height and other instrument details. For some reason, these are only provided for the doppler lidar. Were the sounding stations only added for the field experiment time, or are these permanent stations? Was there missing data? Were the soundings always launched at increments of 3 hour? There must have been some discrepancy in release times, but there is no information. It would also be nice to show a table with available observational platforms that accompanies figure 1, for example.

Line 177. "are" is "were". Five soundings at one time at all locations? Please let someone proofread.

Line 180. What is an "environmental wind"? Please define "very fine-scale".

Figure 1. Please include an inset map of South Korea to indicate where this is (figures should be standalone). Presumably the white area in (a) is the ocean? The colormap suggests this is a mountain. It would be good to have a table in addition to this figure to indicate the abbreviations and the platforms used.

Lines 192-197. LDAPS is a numerical model. It is misleading to have this included in an observational paper without really emphasizing this. The title of the paper reads "Observational study", besides the model simulations are not mentioned in the abstract. It also remains unclear whether this is based on reanalysis, or whether this is a forecasting tool. This is important, as the results are presented as an observational study, but the model at 1.5 km grid spacing will never represent the terrain in such an accurate manner that one can present these results as observations. How is the data corrected regarding the terrain smoothing in the model?

Line 249. "fine weather condition". See above.

Line 251. This must be plural, please address.

Line 258. Stronger than what?

Figure 2. Please modify the caption such that it reflects (a,b,c, etc).

Line 271. "Consequently."

Line 276. "The other ... from China". Awkward phrasing, please address.

Line 278. "northerly winds". Is this in figure 2e and 2f? Please make a reference.

Line 280. Not sure why this is important in "fine weather conditions". Was there precipitation elsewhere on the peninsula?

Lines 282-294. It is arbitrary to use a trend for wind speeds at the surface over the course of only 15 hours, this is clearly within the diurnal variability of winds. How was this

calculated? Also, where does this data come from? See also comment on figure 3 below.

Line 284. "leaving". Awkward, please rephrase.

Line 286. "these two stages *are* shown in Fig.". Please use present tense when you refer directly to the figure, and past tense when you describe the event that occurred in the past. There are many grammatical errors like this, please address.

Line 290. "described" should be "shown".

Line 291-294. "That is, ... 3b)." This is very hard to understand.

Figure 3. It seems like the figure in this data encompasses AWS data for the full country. Correct? This was not introduced in the Data and methods section. How many stations are here? It is impossible to know this since the authors seem to have used some interpolation technique that is also not explained.

Line 301. Ambiguous subtitle. Perhaps change to "Upstream environmental conditions?"

Line 306-307. "Three scanning lidars were deployed at ...". Three at each site? I know what the authors want to say, but it should be clear from the sentence directly.

Line 307. "Five soundings ... coastal area". Something is missing in this sentence.

Line 308. "The sounding ... side (GWW)". This should be in methodology section.

Line 311. Are BKC and GWW also sounding stations? It is unclear.

Line 316-317. "Furthermore, ... symbols." Redundant sentence.

Line 320. Please remove "Instead ... site," as it is redundant information.

Lines 331-332. Awkward phrasing.

Figure 4. What is the wind direction in the wind barb plots? Degrees from north, or across the panel? Figure 1a indicates that MOP and JSC are not aligned along this cross section. How is this corrected for? Otherwise, this needs to be acknowledged for somehow: either that data is or is not corrected for the location. Given the WISSDOM dataset doing some interpolation, it seems crucial information at this point of the manuscript. Also, please discard the filled contour for terrain elevation (or make it lighter in color) as it obscures some of the wind barbs at lower elevation.

Line 339. DDG is upstream from the lee slope, but it seems there are more stations even further upstream. Why was this site chosen here?

Line 343. Awkward phrasing. It is the air that becomes drier and warmer, not the temperature.

Line 346-347. This is clearly a wrong statement. The authors refer to an elevated inversion at around 800 hPa in a profile that starts at 900 hPa (Figure 5). Stable boundary layers that develop overnight rarely exceed 300 m agl. Besides, there is clear neutral layer between the surface and the elevated inversion. Thus, this elevated inversion has some other origin, perhaps large-scale subsidence? The authors could address this by simply mentioning that the origin of the elevated inversion at time of writing has not been investigated.

Line 348. What is a "good condition" for generating hydraulic jump and downslope windstorm in the lee side? Please be specific.

Line 350. What are environmental winds? Perhaps the authors mean to say "the upstream environment encompassed westerly winds".

Line 352. "dramatically". Please remove.

Line 358. Headers should be objective titles such as "Lee slope winds" rather than "stronger winds in the lee slope". Please address also for other subtitles.

Line 360. Perhaps rephrase to "the prevailing wind direction". Why is this "likely" the wind direction? Wasn't this observed?

Line 370. Fluctuations of what?

Lines 370, 380. What are perturbation temperature and pressure?

Figure 6. This is quite a nice figure, but perhaps a lower density in the wind barbs (vertically) would make a clearer picture. Perturbation temperature and pressure. What are these perturbations of? A difference from one-day average at a single station? Or a time-difference across a station-average? Why not just present local ambient and dew point temperature evolution?

Line 395. Which two sites?

Line 420-421. As the boundary layer height changes over time, this cannot be a fixed value by definition.

Line 433. What is a sub-synoptic scale feature?

Line 443. What gusty wind are the authors referring to here?

Line 452-453. Perturbation of what?

Line 458. Surface or sea-level pressure?

Line 469-470. What would be the rationale between an enhanced PGF and subcloud cooling and/or warming? Wasn't this study performed in fine weather conditions, meaning there are no clouds involved? Also, just because term B and PGF "trend" overlap, this doesn't necessarily mean that "subcloud warming" is the critical factor explaining the enhance pressure gradient. The "warming" can also come from adiabatic compression as a result of mountain waves involved. In other words, the correlation does not necessarily mean causality here. Is the pressure gradient not just merely a result of the low and high pressure systems going through the area, that with some critical upstream upper-air environment led to some warming down the lee slopes?

Line 504. "Maximum values". What do the authors mean with maximum values?

Line 506. What sensitivity test are the authors referring to here?

Line 506-507. "increasing the topography height between 1000 and 2000 MSL". What does this mean?

Line 509-512. It is hard to follow this.

Line 513. "surface velocity". Surface winds perhaps?

Figure 10. This figure probably provides explanation for the adiabatic compression leading to a warming in the lee slopes. A recommendation is that it would be better to show theta every 2 or even every degree. Figure 10a also raises the question whether rotor behavior was involved or not. By any means, it looks like the development of winds and temperature at GWW could also be influenced by the fact that this location is close to the ocean, which makes the presented analysis a little more tricky. There have been quite a few studies to downslope windstorms in coastal mountainous environments (Corsica, Southern California, Adriatic Sea), maybe have a look at those.

Figure 10. This is clearly numerical simulations, but the title of the paper says "Observational analysis".

Line 523. Please change the title of this section to something that is addressed in the section, rather than "stronger winds".

Line 526. "westerly". Please change to "westerly winds". This accounts to all occurrences in the manuscript.

Line 533. "Can sustain". Please change to "sustained".

Figure 11. Change figure caption to "Same as Figure 6, but for DGW site". See also comments on figure 6.

Lines 627-629. This is quite an interesting analysis, but this statement seems off. Regarding the minimum and maximum values at roughly 4.5 and 8.5 km, respectively, one sees a similar increase in wind speed of roughly 40%. 10 vs. 15 m/s and 6.5 vs 4.5 m/s. Why, if the wind direction is the same, would this ratio be different in different wind speed conditions?

Figure 14. The y-label says averaged wind speed, but the figure also shows shading. Is that also averaged? Probably it would be better to just change the y-label to wind speed. Does the channel width actually mean "valley width"?