acp-2021-100
Responses (highlighted with blue) to Referee #1
31 May 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.

We appreciate Referee #1's helpful and constructive comments, which help us to improve the manuscript substantially. We have more emphasized the importance of our study and what scientific problems want to address, the capability of adopted datasets and their specific steps of data processing have been also clarified in our revised manuscript. A set of responses to the reviewer's comments is provided below and specific locations of revised portions were also noted as the number of lines.

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?

This is a particularly good comment. Except for the Olympic games were postponed, our selected event is one of two extreme wind events in the past decade based on the KMA (Korea Meteorological Administration) observational records. Besides, the persistent strong wind occurred frequently in narrow segment along the valley (i.e., near the DGW site) from the record. The climatological information also manifested the importance of the large-scale weather systems in this extreme wind event. Additionally, snice the dense observational network was built during ICE-POP 2018, this is a good opportunity to investigate this unique event. We have emphasized these in the introduction (Section 1, Lines 139-142) and provided detailed descriptions about the KMA record in last paragraph of Section 3.1 (Lines 352-367).

- 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.

  Various datasets were acquired in this study including conventional observations [scanning Doppler lidar, automatic weather stations (AWss) and soundings] and reanalysis data (ERA5 and LDAPS). In fact, the forecast outputs of the LDAPS does not adopted in our analysis and it does not to be the constraint in WISSDOM as well. This study is aimed to examine the evolution and mechanisms of an extreme strong wind event associated with a passing LPS (low-pressure system) in Korea with abundant data and reanalysis data. From this statement, the title was modified to "**An analysis of an extreme wind event in a clear air condition associated with a low-pressure system during ICE-POP 2018**" for clarity (Lines 1-2), and we have also emphasized this in abstract (Lines 37-41), introduction (Lines 133-137), and provided more detailed information about the ERA5 and LDAPS datasets in Sections 2.3 (Lines 220-241).
  Additionally, we explained the role of the LDAPS in WISSDOM in this revision. Its constraint is used to minimize the squared errors between the horizontal winds of LDPAS and synthesis winds of WISSDOM. Thus, the role of the LDPAS winds in WISSDOM is to improve the accuracy of the retrieved winds (the details have been noted in Lines 288-292). The accuracy of WISSDOM's winds was also discussed by previously studies, the retrieved winds reveal good relations and acceptable

discrepancies (maximum correlation coefficient is 0.86, minimum root mean square deviation is 1.13 m s^{-1}) compared with conventional observations (the descriptions have been also added in Lines 293-305).

- 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.

  The response to the first comment: to avoid the possible diurnal effects on the wind speed observations, we provided a better analysis to explain the changes of surface wind speed during research period. A sequence of figures (Line 368, Figure 3) shows clear evolution of surface wind speed in northeastern region of Korea and their relations with the moving LPS. The descriptions about these changes have been revised in third paragraph of Section 3.1 (Lines 339-351).
  The response to the second comment: since the station pressure and temperature can better represent the of local ambient (follow a Minor comment for "Figure 6" below). Therefore, we use station pressure and temperature for further analysis instead of original one (Lines 450-467, Fig. 6b, and Lines 623-632, Fig. 11b).
  The response to the third comment: data processing and the characteristics of these nationwide AWS observations were introduced in Section 2.2 (Lines 174-191). There were 727 regular and additional 32 AWS stations in Korea (mean distance is ~10 km for each station), the AWS observations have to interpolate to the given grids by objective analysis with the influences of radius in 10 km.

- 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.

Thanks for constructive comments. We have rearranged the structures of this manuscript to clarify the main purposes (i.e., the evolution and mechanisms of strong winds over complex terrain) of this study. Start from Section 2, the detailed introductions of conventional observations (AWS, sounding, wind profiler) and reanalysis datasets (ERA5 and LDAPS) were be separated into two different sections (Section 2.2 and 2.3). In the Section 2.3, the general bases of the LDAPS and ERA5 were clearly addressed, and their spatiotemporal resolution were also noted. The changes of AWS wind speed and their climatologic information have been revised in Section 3.1. In Section 4, we combined the contents from original one (Section 5). The descriptions about evolution of the strong winds in the leeward side of the mountain range, and the LDPAS analysis were switched to Section 4.1. The descriptions about evolution of the persistent strong winds in the upstream of the mountain range were switched to Section 4.2. The explanations for their possible mechanisms were moved to two subsections 4.1.2 and 4.2.2. Finally, the conclusion is in Section 5.

- 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.

  Thank you for pointing out this problem, we have already checked the grammatical errors carefully. The manuscript has also been edited by professional editorial company.

- 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.

  Thank you for this suggestion. We have included four additional studies in the introduction of this manuscript (Lines 120-122). They all utilized lidar observations to document the downslope windstorm.

- 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.

The names, instruments, temporal resolution, locations and altitude of adopted stations have been summarized in Table 1 (Line 211). The temporary and permanent observations were also noted in the table.

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.

We changed these words to "clear air" for clarity based on glossary of AMS (American Meteorological Society). Since there are only strong winds but no precipitation in our selected event, the scanning Doppler lidars and the observations allow us to collect more wind information under clear air condition. All of the words (fine weather) have been replaced by "clear air" throughout the manuscript.

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.

Removed as suggestion (Line 61).

Line 67. Fine weather… What is this?

Reworded to "clear air" throughout the manuscript.

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

Corrected as suggestion (Line 78).

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

Corrected as suggestion (Lines 81-82).

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

Removed as suggestion (Line 119).

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".

Revised as suggestion (Lines 119-120).

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

More complete and detailed introductions for observations, reanalysis datasets and the principle of WISSODM have been improved in following sections (from Section 2.2 to 2.4).

Line 165. "100 km". This is probably not true, please address.
The redundant descriptions have been removed for clarity (Lines 166-167).

Line 169. "0.04". Why this value?
We have done with many tests from 0.01, 0.02, 0.03 ~ 0.1, and this value can appropriately remove most noises and retain sufficient meteorological signals. This explanation has been added in Lines 171-173.

Line 170. LDAPS is derived from model simulations? This needs to be emphasized, as it looks now as if this is an observational dataset.
We have improved the introductions of the LDAPS dataset detailly (as Section 2.3). The forecast outputs of LDAPS dose not used in this study, the capabilities and spatiotemporal resolution were also addressed in this section (from Line 220). Please refer to our responses in second major comment above.

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.
We have provided detailed information about adopting datasets in Sections 2.2 and 2.3 (Line 174 and Line 220), and these information have been summarized in Table 1 (line 211).

Line 177. "are" is "were". Five soundings at one time at all locations? Please let someone proofread.
Revised as suggestion (Line 195), this sentence has been rewritten for clarity (Lines 194-197).

Line 180. What is an "environmental wind"? Please define "very fine-scale".
We want to explain such dense sounding observations, which can represent local horizontal winds in relatively small scale (~15 km). The descriptions and the definition of these two words have been improved in Lines 201-205.

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.

The Korea map has been inserted in Figure 1 (Line 212). The color bar in Figure 1 were modified to be corrected one. The names of each station in Figure 1 were also summarized in Table 1.

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?

We have modified the title because the conventional and reanalysis datasets were used in this study (Lines 1-2). We have emphasized what kind of the datasets were adopted in the abstract as well (Line 37-41). The LDAPS reanalysis dataset was assimilated by various platforms with high resolution of wind observations (like lidar, AWS, sounding, wind profiler and satellite). The errors between conventional observations and LDAPS have been minimized conscientiously by the KMA and the quality of wind information is able to resolve small-scale weather phenomena over complex terrain in Korea. The detailed descriptions about the LDAPS dataset have been revised in Section 2.3 (Line 220).

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

Reworded to "clear air" throughout the manuscript.

Line 251. This must be plural, please address.

Revised as suggestion (Line 307).

Line 258. Stronger than what?

This a redundant word and it has been removed (Line 308).

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

The caption of Fig. 2 has been modified (Lines 320-324).

Line 271. "Consequently."
Already checked, and it looks a correct use (Line 325)

Line 276. "The other … from China". Awkward phrasing, please address.
The sentence has been rewritten (Lines 330-333)

Line 278. "northerly winds". Is this in figure 2e and 2f? Please make a reference.
The reference has been made, and more clear description was also added (Lines 332-333).

Line 280. Not sure why this is important in "fine weather conditions". Was there precipitation elsewhere on the peninsula?
Because lidar is not like radar, it will have severe attenuations when raining or snowing. So, it is a big challenge to collect the good coverage of wind information under clear air condition. Fortunately, many observational platforms were deployed at the time when the extreme strong wind occurred during ICE-POP 2018. We have emphasized this point of view in Lines 335-338.

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.
Instead of the trends for wind speed, we use consecutive wind speed analysis during research period to explain the relations between wind speed and the LPS. The results shows that the changes of wind speed have clear relations with large-scale weather system and reveals relatively weak relations with diurnal effects. In particularly, the wind speed was increasing when the LPS was passing and was decreasing when the LPS was moving away the Korean peninsula. This new analysis can also be sufficiently presented the uniqueness as the sustained (gusty) strong wind occurred over mountainous area (lee side of the mountain range). The new figure (Figure 3, Lines 368-372) and the detailed descriptions (Lines 339-351) about the changes of wind speed have been revised in Section 3.1.

Line 284. "leaving". Awkward, please rephrase.
This paragraph has been rewritten to clarify the changes of wind speed during research period.

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.
We have checked the grammatical errors and have been corrected throughout manuscript.

Line 290. "described" should be "shown".
We have corrected this kind of wrong usages throughout the manuscript.

Line 291-294. "That is, … 3b)." This is very hard to understand.
This paragraph has been rewritten to clarify the changes of wind speed during research period.

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.
Correct, the nationwide AWS data was used. There were 759 AWSs in Korea and their observational parameters were interpolated to given grid based on the objective analysis. We have added detailed descriptions about the characteristic of AWS data (Lines 174-191) and their distributions in Fig. 1 (Line 212).

Line 301. Ambiguous subtitle. Perhaps change to "Upstream environmental conditions?"
Revised as suggestion (Line 373).

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.
This sentence has been rewritten for clarity (Lines 377-379).

Line 307. "Five soundings … coastal area". Something is missing in this sentence.
The missing word has been corrected in this sentence (Line 379).

Line 308. "The sounding … side (GWW)". This should be in methodology section.
This description has been moved to the methodology section (Section 2.2, Lines 197-201)

Line 311. Are BKC and GWW also sounding stations? It is unclear.
Yes, they both are sounding stations, we have modified the description for clarity (Line 200).

Line 316-317. "Furthermore, … symbols." Redundant sentence.
The redundant sentence has been removed.

Line 320. Please remove "Instead … site," as it is redundant information.
The redundant part has been removed.

Lines 331-332. Awkward phrasing.
This sentence has been rewritten for clarity (Lines 397-400).

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.
The wind barb indicates the degree from north, this information has been added in the caption (Lines 404-405). Since the sounding were used here to represent local environmental condition in the scale around 15 km (cf. Lines 204-205). The sounding sites were perpendicularly projected to the cross line (in Fig. 1b) from their original locations, this description has been added in the figure caption as well (Lines 406-407). WISSDOM uses Cartesian coordinate system, thus, the input data have to interpolate to the same coordinate system first, this description has been revised in the introductions of WISSDOM (Section 2.4, Lines 263-265). The filled contour has been removed for clarity (Fig. 4, Line 401).

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?
The DGW is good upstream site than other two sites (MOO and JWC). First, the DGW have no missing data, and it have more tight relations with its downstream site (GWW). These descriptions have been noted in Lines 410-415.

Line 343. Awkard phrasing. It is the air that becomes drier and warmer, not the temperature.
The sentence has been revised (Line 418).

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.

We also agree this point, the large-scale subsident would possibly provide more contributions to the inversion in this event. However, it is not easy to clarify this issue completely from our present analysis and datasets, the numerical study would be good approach. The descriptions about his issue have been improved in manuscript based on the suggestions (Lines 422-426).

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

The "good condition" indicates perpendicularly upstream wind to the mountain range, and upstream inversion. In addition, we modified the word from "good" to "preferred". The specific description about the "preferred condition" has been noted in Lines 426-427.

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

Yes, we putted "upstream" prior to "environmental winds" (Line 429).

Line 352. "dramatically". Please remove.

This word has been removed.

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.

The subtitles have been revised throughout the manuscript (Line 439, Line 604).

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

This sentence has been revised as suggestion (Line 441).

Line 370. Fluctuations of what?

This sentence has been improved for clarity (Line 450).

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

We utilized the "station pressure" and "temperature" for further analysis, the descriptions

about the station pressure and temperature have been revised in this paragraph (Line 450 and Lines 460-463).

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?

Thank you for these good suggestions. The figure has been modified (Line 468) and we also use the station pressure and temperature for further analysis.

Line 395. Which two sites?

It should be "two different locations", the description has been revised for clarity (Line 527).

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

In this budget analysis, the mean of boundary layer height was usually used to represent the H. Furthermore, the values of H did not change too much during the research period. Thus, we used these fixed values to represent the boundary layer height here. The description has been modified in Lines 552-555.

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

It indicates small-scale. The description has been improved for clarity (Lines 567-568).

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

The gusty wind indicates the wind speed was increased suddenly (like ~3 to 12 m s^{-1} in this event) in short period. This description has been also noted in Lines 579-582 for clarity.

Line 452-453. Perturbation of what?

We used new analysis to evaluate the contributions of large-scale weather system in the PGF based on suggestions below. This paragraph has been rewritten.

Line 458. Surface or sea-level pressure?

This paragraph has been rewritten.

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?

We agree this point, and this is particularly an excellent suggestion. Thus, we did new analysis based on this approach. We evaluated the contributions from the large-scale weather system, and the results indicates that relatively lower pressure in the speed-up stage may have produced by adiabatic warming coupled with the LPS. Thus, the large-scale weather system indeed contributed some warming down the lee slope. The detailed descriptions have been revised in last paragraph in Section 4.1.2 (Lines 579-597), and the new figure is shown in Line 599.

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

We replaced the words "maximum values" to become "range". The "range" of Froude number ($F_r = U/NH$) was calculated when we assumed represent terrain height (i.e., H in F_r) between 1000 and 2000 m MSL (the averaged altitude of the TMR is ~1200 m). This description has been improved for clarity (Lines 505-509).

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

Except for the different represent terrain height, we also calculated the Froude number with different Brunt-Vaisala frequency (including saturated and dry). After this procedure, the range of Froude number in this event were estimated. This description has been improved for clarity (Lines 505-509).

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

Although averaged height of the TMR is ~1200 m, the Fronde number were estimated by adjusting represent height to check its variability in this local area. In addition, it should be 1000 and 2000 "m" MSL, this typo has been corrected (Line 508).

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

The descriptions have been improved for clarity (Lines 509-512)

Line 513. "surface velocity". Surface winds perhaps?
This word has been corrected (Line 513).

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.
The figure has been modified by following suggestion (Line 521). The rotors seem not clear showing when we increased the contours. Additionally, the ocean temperature was not changed too much during the research period when we checked the sea surface temperature of EAR5. Compared to the downslope windstorms in coastal mountainous environments in the other locations (Corsica, Southern California, Adriatic Sea), we may have assumed that the influences from the ocean would be small in our selected event. The descriptions about this statement have been noted in Lines 516-520.

Figure 10. This is clearly numerical simulations, but the title of the paper says "Observational analysis".
This figure was analyzed by using the LDAPS reanalysis dataset, this reanalysis dataset was assimilated with many of high spatiotemporal wind observations. We have addressed this in the introductions of LDAPS and ERA5 datasets (Lines 220-241). Therefore, the title was modified as well.

Line 523. Please change the title of this section to something that is addressed in the section, rather than "stronger winds".
The title of this subsection has been changed as suggestion (Lines 604-605).

Line 526. "westerly". Please change to "westerly winds". This accounts to all occurrences in the manuscript.
These words have been revised throughout the manuscript.

Line 533. "Can sustain". Please change to "sustained".
The words have been changed as suggestion (Line 614)

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

comments on figure 6.

The figure caption has been revised (Line 634, Line 661).

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?

This is a valid point. This ratio explained that the westerly winds indeed were accelerated in narrow segment along valley, however, there were different amplifications in the maximum wind speed with different strength of westerly. In this event, the maximum wind speed was amplified significantly (~10 m s^{-1} more than averaged) in the narrow segment of valley when the westerly winds were strong. The detailed descriptions have been revised in Lines 695-703.

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?

Thank you for pointing out the problem, the figure has been modified (Line 705).