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Atmospheric Measurement Techniques Discussions

Interactive comment on "Analysis of Flow in Complex Terrain Using Multi-Doppler Lidar Retrievals" *by* Tyler Bell et al.

Anonymous Referee #2

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The manuscript presents interesting analysis approaches on the topic of spatial variability wind effects, which is an important one. The text is often hard to follow, though, and I had to reread the paper a couple of times to understand even the main thrusts. I believe the manuscript needs a major rewriting, especially in the following areas: a significant rearrangement of the text, elimination of vague and qualitative statements, and addressing what seem to me to be places where the text does not match the analyses presented. In several places, the last point may be addressable (or at least clarified) by plotting difference profiles in addition to the mean profile, and adjusting the color scales so the reader can see what the authors are seeing.

Major recommendations: 1. Text rearrangement: This is not a long paper, so the brief descriptions of the three types of flow case studies in Section 3.3, followed by the

case studies themselves in Sections 4.1 and 4.2, seemed like jumping around and was confusing. The descriptions in 3.3 sounded more like introductions to sections than 'Methods.' This was made a bit more confusing because the cases are presented in a different order from the columns of Fig. 5, which is an introductory and summary figure. I recommend combining subsection 3.3 and Section 4âĂŤby making 3.3 into the beginning of Section 4, with the first 3 subsections being the three case studies. The first subsection (4.1, 12 June) would start with the text from 3.3.1 (lines 13-24 from p.9) and then add the text from p.11, lines 13-23; 4.2 (14 June) would start with text from 3.3.2 and add that from p.11, lines 24-32; and similarly for 8 May into a new 4.3. If Fig. 5 were rearranged to show 8 May in the last column, the text would go smoothly into the material in Section 4.3, either as its own section (new 4.4) or added to the 8 May material in the new 4.3.

2. Qualitative descriptions. Many of the descriptions are vague and qualitative, and need to be made quantitative. Some examplesâĂŤ'moderate to high wind speeds' (p.9, lines 13 and 25), 'became very weak' (p.10, line 7); 'very little vertical velocity;' 'slight differences,' 'agree qualitatively well with...' (p.11, lines 13, 30, 32); 'relatively good agreement well above ridgetop' [what levels?], and 'large differences,' and 'wind speed [sic.] all line up nicely' (p.14, lines 10, 11, and 13), etc. Small differences, strong or weak winds, all depend on application, so it is important to specify what these statements mean quantitatively.

3. Mismatch between text and analyses. In some places, the description in the text does not seem to me to be supported by the evidence presented in the analysis figures. Examples: - (p.11, lines 12 and 20) 'near perfect agreement between VT3 and VT2...' apparently referring to the red and green curves in Fig. 6, 'near perfect' seems like a big exaggeration, and these two curves seem to be off by $\sim \frac{1}{2}$ m/s through most of their depth. 'the areas with the least amount of vertical motion (e.g., around 120 m) have the greatest agreement...' There seems to be about $\frac{1}{2}$ m/s difference at 120m, which looks to be about as big as this difference gets. Smaller difference values seem to be

at -30 m and 180 m, where vertical velocities do not seem smallest, although it's hard to tell with the wind-speed scales shown. (p.14, lines 13 and 15) 'wind speed all line up nicely,' and 'spread in wind direction gets slightly larger.' These are very hard to see, and so it's hard to know whether they are significant enough to be concerned about. Some of these may just be difficult to see because of the scales plotted, and it's asking a lot of a reader to be able to see differences that may be not much larger than the widths of the lines. Such as (p.12, line 9): 'a consistent offset between VT3 and each of the other. . .' is hard to see. So I suggest plotting vertical profiles of the differences, where they can be seen on an expanded scale. Similarly, please compress the color scale in the top row of Fig. 5 (like, max values of 12 or 15 m/s instead of 20) to show more of the structure described in the text. Overall, please check carefully that all the claims in the text are supported by the Figures used as evidence.

4. Homogeneous. Homogeneous means invariant in space ('spatially inhomogeneous' is redundant), including the vertical, so the best that can be hoped for in the atmosphere is horizontal homogeneity. The flow within complex terrain such as Perdigão is very seldom horizontally homogeneous, especially at night. So, claims that either June case has horizontally homogeneous flow must have much more justification, or the wording needs to be changed (like, less inhomogeneous, or, less horizontal variability, etc.).

Minor comments: 5. The appropriate manner of scanning for and computing mean wind profiles depends on applicationâĂŤthere is not a 'one-size-fits-all' solution, and each manner (single lidar, multiple lidar, etc.) has advantages and drawbacks. As this is a relatively new field, I have not heard of robust studies that convincingly and quantitatively demonstrate that one approach is clearly better than others for all applications and circumstances. Authors seem to be favoring the virtual tower approach in their review, but I would encourage them to take a more neutral tone in their review, along the lines of 'there are several approaches, and each has advantages and disadvantages' as just stated. VAD has the possibility of inhomogeneities across the scan adding to

the uncertainty of the calculation, but according a recent Mann et al. paper, virtual towers can have difficulties in the scanning/ data acquisition stage with beam alignment and coordination, leading to uncertainties especially in complex terrain. 6. Throughout, there are a number of places where is seems to be assumed that the only way the speeds, and especially directions, of different scanning approaches can be different is because of vertical velocity. In a complex terrain setting, there can always be many different terrain-related and other reasons. The authors should list these reasons, and when discussing differences, say something like that the differences are consistent with a possible vertical-velocity explanation. 7. Add a 'Site Elevation ASL' column to Table 1. 8. Add a Table 2 of virtual towers, specifying which lidars are participating in each tower. It's sometimes hard for a reader to keep track of. 9. (p.3, line 10: 'To validate numerical models, detailed measurements of the flow at multiple scales are required.' I suggest references here, since in my experience many modelers are not necessarily in agreement about this. Banta et al. 2013 and Fernando et al. 2015, both BAMS, are a couple of papers that make this point, I think. 10. p.4, line 15 and p.5, line 6: 'lower orange site' what is this? No context for this. 11. p.5, about line 1: what is the minimum height (minimum range) of the CLAMPS lidar in vertical staring model? 12. p.7, Fig. 3: can hardly read the labels. Also true of many other figsâĂŤplease make sure labels are big enough to be legible. 'OU DL' in caption – is this CLAMPS? Please stick to one name for each lidarâĂŤthere are a lot of lidars to keep track of. 13. Section 3.2: The explanation of what lidar is doing what, and how that impacts the horizontal wind uncertainty is not clear to me. What do the vertical lines marked VT1, VT2, and VT3 mean – please clarify in caption. 14. Sections 3.3.3 and 4.3: Figs. 11, 12 are almost certainly hydraulic jumplike mountain lee-wave features, which at larger scales would be downslope windstorms. An early paper with lidar cross sections is Clark et al. 1994 JAS. 15. A couple of places had kts for wind speed, instead of m/s.

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