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Interactive comment on "Perdigão 2015: methodology for atmospheric multi-Doppler lidar experiments" *by* Nikola Vasiljević et al.

Anonymous Referee #2

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This manuscript is about multiple lidar measurements performed for the Perdigao 2015 campaign with two triple-Doppler units, both pulsed long-range lidars and continuous short-range lidars.

The first criticism is about the writing style. Rather than a scientist paper, this document reads like a romantic technical report, or maybe a long post on a blog. The manuscript is very very lengthy. The first lidar data is shown at page 20 (the last one at page 23). If I am not mistaken, no data from the short-range system is provided. I would rather recommend a more classical structure of the manuscript consisting of introduction, description of the site and setup, lidar scans and data retrieval, discussion of the results and conclusion.

Besides the writing, I have also some concerns about the novel results presented in this

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Discussion paper

manuscript. We have seen already dual-Doppler lidar measurements in wind turbine wakes or vertical transects for valley flows. Two figures are definitely not sufficient to describe effects of atmospheric stability on wind turbine wakes. Therefore, I suggest to provide a sharper focus on the data analysis and emphasize any new result.

Comments are provided below for a revision of the manuscript.

Comments: 1. P1L3: "...measure mean flow conditions over an entire region...", this sounds a bit too vague, maybe better stating the typical measurement volume of the two systems. 2. P2L3: "...it is unrealistic to sample...", actually it is real performing met-tower measurements. Maybe it is better mentioning the reasons why a multi-lidar system can be advantageous. 3. Sect. 1: It seems to me that this introduction is lacking to provide an overview of existing works on triple lidar measurements, such as J. Mann et al. 2009, Meteor. Z. 18, 135-140, Fuertes et al. 2014, JTECH 31(7), 1549-1556, or papers from the AMT special issue on the XPIA experiment (http://www.atmos-meas-tech.net/special_issue645.html), which was focused on assessing various multiple-lidar scanning strategies (Lundquist et al. 2017, BAMS 98(2), 289-314). Therefore, I suggest providing a more comprehensive introduction on the topic. 4. P3L7: Provide some references for the setup of the SRWS. 5. P3L14: Discuss the motivations on developing a hybrid system. 6. Sect. 3: these 10 steps are common for any (field) experiment and not specifically related to the LRWS and SRWS. Why these steps should have a special relevance or being different for this experiment? 7. Sect. 4 and throughout the paper: I understand the passion and excitement of the authors; however, this writing style is more adequate for a blog or a newspaper article rather than a scientific paper. Comments like "need to test both the equipment and human resources in highly demanding field experiments (P4L15)", "harsh conditions, high temperature and remote locations" ... This experiment was carried out in Portugal, I cannot image what scientists in Antarctica should write to describe their experiments! 8. Sect. 4.1-Sect. 4.4. This description is lengthy and unfocused. It would be easier to provide a classical description of the site and instrumentation. 9. Sect. 4.4: You suddenly introduce these

unexpected RANS simulations over the topography, without canopy performed with a commercial code, and finally just saying that the "...high complexity of the flow (Fig. 4) and large recirculation zone enclosed in the valley (Fig. 5)". I think these comments were highly expectable. Thus, I suggest removing the entire paragraph on the simulations and Figures 4 and 5. 10. P8L20: Maybe can be a personal issue of this reviewer, there is no way I can remember the names of these 3 lidars. I suggest to name them as LR1, LR2 and LR3 rather than with your nick names. 11. P8L22: What do you mean for "entailing the wind turbine"? Maybe a vertical plane along the line connecting a lidar with the turbine? 12. P8L25-29: Maybe add another table with distances among the different objects. 13. Sect. 4.5: this section can be completely removed. It provides only unimportant information. 14. P12L30-P14L3: If the measurement plane of this dual Doppler lidar was inclined, how is it possible you retrieved horizontal wind speed and direction? I guess you retrieve the 2 velocity components over the measurement plane. 15. P15L6-L11: The description of this scan is very confusing. You say that there was a time delay among the different lidars, and the delay was increasing with time. Therefore, you need to provide a statistical characterization of this delay and how you treat this time delay in the retrieval of the wind velocity components. 16. Sect. 4.7: A general comment for all the presented scanning strategies: for multiple Doppler lidar measurements, accuracy in the retrieval of the wind velocity components is affected by the elevation and azimuthal angles of the various lidars. A criterion for quantifying this error over a scan has been proposed in Debnath et al. 2017 AMT, 10, 431-444. A similar analysis should be provided for the proposed scans. 17. Sect. 4.7: A general comment for all the presented scanning strategies: you haven't provided any information on the data retrieval of wind velocity components from the lidar radial velocities. This part should be included in the manuscript. 18. Sects. 8 and 9 can be removed or summarized in the description of the setup. 19. Sect. 4.10 provides should significantly shortened. 20. P19L9: what do you mean for ... "show no turning of the wind for Northeast winds...divergency of flow lones"? This does not sound like a technical language. 21. Fig. 11 is not described accurately in the text. Why you were not able

to get measurements in the turbine wake? 22. Figs 12 and 13. You should provide more information about the wind condition, day, atmospheric stability, etc. No details are provided on the data retrieval. 23. P20L1-2: “The inflow and wake of the turbine during a one full day is well represented in Vasiljevic ÌĄ (2016b)”. Why then you provide these figures if a deeper analysis has been already published?

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