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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "The Alaiz Experiment: untangling multi-scale stratified flows over complex terrain" *by* Pedro Santos et al.

Anonymous Referee #2

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This study presents measurements from a recent field campaign in the Alaiz mountains, northern Spain. A range of sophisticated equipment, including wind profilers, sonic anemometers and masts for measuring wind and temperature profiles, is employed to probe the flow in transects across a valley between two mountain ridges. These measurements are used to characterize two flow regimes (a hydraulic jump and a valley flow stagnation situation) for orography whose combination of horizontal scale and steepness fills a gap hitherto relatively unexplored by previous field campaigns. Consisting of an essentially observational study, containing a large amount of original results, relevant for both better understanding orographic flows and the requirements for their accurate numerical modelling, this manuscript is relevant and appropriate to Wind Energy Science. The research seems sound and the manuscript is well written and well organized. My main objection, which is nevertheless minor and will be detailed below, concerns application of known theoretical insights, developed for idealized flows, to the measured hydraulic jump. I think that the paper should be acceptable for publication after minor revisions.

Main point

Use of the Froude number to diagnose super-critical or sub-critical flow, and therefore the occurrence of a hydraulic jump, is one of the most questionable aspects of the results presented. It is tricky to apply concepts developed for idealized cases to realistic conditions, but it must be recognized that by suitably choosing either the definition of Fr or values of the parameters included in it, Fr may vary within a wide range. D is defined as 500 m, based on the elevation difference between MP5 and M7. This seems a bit arbitrary, since D is defined by Rotunno and Lehner (2016) as the depth of a stratified layer and, for example, in Figs. 11 and 12 the atmospheric layer with stronger stratification seems much shallower. It makes more sense to equate D with the layer of high wind speed from the lidar measurements (as the authors do), but the connection with stratification still needs to be clarified. It is mentioned that the potential temperature gradient used to calculate the N included in Fr is obtained by linear interpolation between measurements at 2 and 80 m or 2 and 113 m, but this depth is much smaller than 500 m, so this value of N cannot be considered representative of the stratified layer D. I could not find any allusion to how the value of U included in Fr is estimated. All of these aspects need to be justified in a physically more convincing way, as the estimated value of Fr is very sensitive to them.

Minor points

Page 1, line 19: "several processes are included in the former but not in the latter" (when referring to meso- and micro-scale models). It would be good to be more specific here by briefly specifying what some of these processes are.

Page 3, caption of Figure 1: It should be mentioned in this caption that in the insets the blue colour represents low elevation and yellow represents high elevation. Is the colour scale arbitrary, or does it have some quantitative meaning? It is strange that the orography of T-REX is mostly blue.

Page 4, line 3: "northwestern part of Spain". Given that these mountains are near the Pyrenees, this should be "northeastern" instead, I think.

Page 4, caption of Figure 2: "CP" is denoted by light blue, but this colour (without a nearby white colour for comparison) looks rather white instead. Consider using a different colour, or a different description (but this is just a suggestion).

Page 6, line 9: "hectometer scale". If I am not mistaken, a hectometer is 100 m. It would perhaps be easier for the reader to understand if this was phrased as "100 m" instead of "hectometer".

Page 7, line 24: "equally distributed". "equally spaced" might be a more precise description.

Page 8, line 9: "(see figure 4)". It seems to me that the technical aspects about the RHI scan that are discussed in this passage are somewhat unrelated to the aspects that are depicted in Figure 4. Consider whether it makes sense to cite that figure here.

Page 8, line 14: "staring at M7's 80 m 3D sonic storing". Although I am unfamiliar with the terminology of field work, I wonder if the word "staring" is the most technically accurate one in this context. Please check.

Page 9, Figure 5: In the description of this figure within the text, information should be included on what the angular interval is for calculating the histograms that make up these wind roses. It should also be mentioned in the caption what the percentages labelling the dotted circles mean.

Page 9, lines 12-15: "the wind rose measured at the valley floor" is described. Above, when the measurements at the mountain top are described, a turbulence intensity of 7% is mentioned (line 6). If possible, the authors should say what the turbulence intensity in the valley floor measurements is.

Page 10, lines 4-5: "Results show that the NEWA-WRF simulations underestimate the mean wind by more than 1.5 m/s, which is indicative of unresolved speed-up effects in the meso-scale model" (a similar comment is made in page 19, lines 4-6). Do the authors believe that this underestimation is simply caused by insufficient resolution, or are there other factors at play?

Page 12, Figure 7: I am puzzled by the order in which the stability categories "nu" and "u" appear in the graphs. It would be natural to expect a sequence "u-nu-n-ns-s" in order of increasing stability, but it is "nu-u-n-ns-s" instead. Why is that? If this was a mistake, please consider correcting it (although this, of course, does not affect the correctness of the results).

Page 13, line 5: "Fr = pi U/2 N D". I am aware of the fact that the authors define the Froude number based on Rotunno and Lehner (2016), but it would be good to briefly explain the presence of the factor pi/2 in this definition.

Page 13, lines 15-16: The description of the hydraulic jump could be improved. It should be mentioned that the discontinuity that corresponds to the hydraulic jump occurs as a downstream consequence of the flow transition between subcritical and supercritical over the mountain, associated with a downslope windstorm. Perhaps a reordering of the text would do the job, since in lines 18-19 below high amplitude mountain waves and downslope flow are mentioned, and in lines 21-22, supercritical and subcritical flow regimes are mentioned.

Page 13, lines 17-18: "It differs from atmospheric gravity waves or lee waves since it involves a discontinuity and requires nonlinear dynamics to be described". Some gravity waves and lee waves also require nonlinear dynamics to be described. It is just that the nonlinearity of hydraulic jumps is more extreme.

Page 13, lines 23-24: The authors refer here to solutions that use "a Fr scaled with the

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maximum wave speed given by (g D)ˆ(1/2)". Clearly this is different from the Froude number defined previously and used in the present study. How do the conditions in which this alternative Fr apply differ from those considered in the present study?

Page 13, line 30: "We defined D $\sim$ H $\sim$ 500 m in section 3.2, which agrees with previous studies". Unless the flow is the same, it is irrelevant whether this value agrees with previous studies or not, since the height of the stable layer, or whatever is used to estimate D, varies between different flows.

Page 15, lines 3-4: "Fr at MP5 decreases from 1.5 to 1.2 and the M2 measurements evidence a recirculation zone in the lee-side of the mountain". This recirculation zone is usually called a rotor, and it would be worth mentioning that term, perhaps in connection to citation of one or two studies that address that flow structure.

Page 15, line 13: "quiescent easterly winds". "Quiescent" is often reserved for situations of calm. Do you mean to say that the easterly winds are "weak", perhaps?

Page 15, lines 14-15: "The potential temperature profiles (Figure 11c) from S1, M5 and sodar RASS agree and show a stable boundary layer". What is the height of this boundary layer? What is its relation (if any) with the height of the so-called "stable layer".

Page 16, inset in Figures 11(a), and page 18, inset of Figure 13(a): These insets (containing the legends for the 2m wind speed, 2m temperature and temperature difference between 2 m and 0.36 m need to be described in the caption.

Page 16, lines 5-6: "heterogeneous land cover, which causes unequal heat fluxes". How important is this effect compared with orographic effects (causing katabatic and anabatic circulations)?

Page 17, line 8: "Both layers are stably stratified, although with different intensity". Curiously, the apparently katabatic flow shown in Figure 12 extends over a substantially higher depth than the more intensely stably stratified layer in the theta profile. Is there an explanation for this?

Page 17, line 12: "The S7 and S9 positions show a recirculation zone". Does this correspond to pooling of the flow? If yes, it would be worth mentioning this explicitly, since cold air pools in valleys are an active area of research.

Page 18, line 2: "An elevated thermal inversion around 720 m asl". It would be good to refer back to Figure 12(b) at this point, so that readers can understand what is being discussed.

Page 18, lines 3-4: "depth of the red band within the valley observed in the RHI scan of 3:10Z". Again, it would be useful to the reader if the authors referred back to Figure 12(a), where this feature can be identified.

Page 18, lines 11-12: "ALEX17 yet poses a further challenge in numerical modeling efforts for its large domain size". I suggest replacing the word "for" in this sentence by "because of".

Page 18, line 14: "wind direction offsets up to 90°". I suggest replacing this passage by "the wind rotates by up to 90°".

Page 19, lines 1-2: I suggest that the reference at the end of the sentence spanning these two lines is moved to after "experimental report", since it seems to refer to that report.

Page 19, lines 13-14: "On the other hand, during stratified conditions with lower wind speeds, valley winds become decoupled from the mountain flow aloft due to thermal stratification". This should be connected to cold-air pooling and valley inversions, and one or two relevant references on these topics should be added.

Page 19, line 17: "in terms of its physical description". "its" should be replaced by "their", since the word it refers to, "flows", is plural.

Page 19, line 18: "i.a.". I interpret this as meaning "inter alia". Is this abbreviation

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standard? If not, write it in full form instead.

Page 19: The "Author contributions" and "Competing interests" sections seem to be in the wrong place, between the Appendix title and its content. They should probably come, either before the Appendix title, or after the Appendix.

Page 21, line 22: Please check if the volume number is correct, as it appears to coincide with the year (2020).

Page 22, line 8: "pp. 1-33". I believe the "pp." is unnecessary. Consider removing it.

Page 22, line 27: The page range appears to be missing from this reference.

Page 23, lines 21-22: This reference has no volume number or page range.

Page 24, line 6: This reference has no volume number or page range.

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