Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-847-RC3, 2018

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Characterization of flow recirculation zones in complex terrain using multi-lidar measurements" *by* Robert Menke et al.

Anonymous Referee #3

Received and published: 2 November 2018

General considerations

In this paper, the authors use a wonderful data set (from the Perdigão-2017 field campaign) to describe the 'characteristics of flow recirculation zones in complex terrain'. Overall, the paper is quite well written and the provided material is useful to serve the purpose of the paper. However, some of the 'ingredients' of what is called an 'algorithm' to detect recirculation zones and some of the analysis tools need some clarification and reasoning (see the 'minor comments').

My major concern with this paper lies in the embedding of the obtained results in previous knowledge. The authors have decided to 'wipe this away' with a single comment ('. . . which are not well captured by a linear flow model', p1, l. 22) and therefore analyze the data with respect to each variable separately. First of all, linear theory is not all we

know about flow behavior over topography, but more importantly, this decision leads to a 'characterization' that is entirely specific to the Perdigao site: all the given 'numbers' (e.g., in the conclusions: 'recirculation is more likely for wind speed above 8 ms-1', p13, l.9) are not suitable to be transferred to any other site. In the end, the authors then (have to) conclude 'Because assessment with multiple scanning lidars, as presented in this study, is yet not feasible for commercial projects, efforts should be [made] to account for recirculation in the flow models used for wind resource assessment'. While this is certainly a valid conclusion, in my view, additional generality could (easily) be obtained when at least discussing (using) some of the principles arising from previous knowledge. In this sense, the present 'descriptive' approach appears to be a missed opportunity. Clearly, a review cannot require from the authors to change their strategy of analysis – but I think the least that should be done is to comprehensively summarize the previous knowledge and discuss the 'departures' of the present site from the conditions, for which we have some theoretical understanding (major comment below). If the authors decide to keep their approach of analysis with respect to dimensional variables, the title should be changed (into something like 'Characterization of flow recirculation zones at the Perdigao site using multi-lidar measurements')

Major comment

Occurrence of recirculation behind a two-dimensional or three-dimensional obstacle has been widely investigated for stratified flow. Baines (1995) provides an excellent overview – see especially his Chapter 5. The characteristics of the mean flow behavior (over topography) are largely determined by the relative importance of stratification and advection in combination with the 'obstacles' dimensions (half-width, height) leading to an assessment of the flow characteristics in terms of non-dimensional parameters, Froude number (using the half-width of the ridge) or non-dimensional hill height (using the height).

At least for the stable cases (which are not abundant at the present location), therefore, the authors' findings could be compared to previous knowledge with theoretical foundation. For example, Fig. 5.8 in Baines (1995) summarizes the occurrence of flow separation in a non-dimensional framework – and it would be extremely interesting to learn to what degree the re-circulation occurrence of the different cross-sections at the present site correspond to those (mostly ideal) results. Certainly, on the basis of this previous knowledge, it would be much more conclusive to analyze the present data in a non-dimensional framework, rather than producing 'thresholds' for the mean wind and stability separately – and finding of course results that are surely consistent with this (larger mean wind speed favors separation, stronger stability hinders it) and then speculating that 'These variations of recirculation occurrence may be related to the transects' elevation profiles within the valley', [p8, l.1]).

One of the relevant (and quite new) findings of the present study is certainly that re-circulation preferably occurs under unstable conditions (at the present site). This of course makes it more difficult to compare to previous knowledge for stratified flows. Even if the authors 'rule out' the value of linear theory (p1, l. 22), the Perdigao site is sufficiently ideal (and slopes may be steeper than desired for linear theory, but certainly not overly steep) that at least the consistency (in the trends) of the present results with the expectations from linear theory could be discussed. In fact, should be. The textbook of Kaimal and Finnigan (1994, their chapter 5) is an excellent source to start with – and Belcher and Hunt (1998) give a comprehensive overview of all the relevant resources. Again, stability is characterized in a non-dimensional framework using the terrain geometry (in this case the 'inner-layer depth', which could be determined from the present data) thus making the results more generally applicable.

The strongest 'departure' from applicability of previous knowledge is the 'double ridge' problem, i.e. the fact that at the Perdigao site not only one ridge is present but two – and those in a short enough distance so that they potentially influence the flow at each others' location. There is less systematic knowledge available for this flow type with respect to recirculation zones – except for the quite specific case of rotor formation (which, of course, is also some sort of 're-circulation zone', but not immediately

downstream of the ridge. See Grubisic et al. (2008) for some detail). Again, for the stable case, some information can be found in Baines' book (but clearly less, and less theoretically founded). I think, this aspect may indeed be used in the discussion of the present results in view of potential departures from expectations for a single ridge.

Minor comments

P2/l. 28 Section 4 (not section)

Fig 1 caption: 'Table 1' (when using in conjunction with a number, please capitalize: Tab. 1,

Fig. 2 etc.). Throughout - many occurrences.

P2/l.10 many studies…: there are quite some others, e.g. discussed in Kaimal and Finnigan (1994).

P2/l.18 …the orography of the Perdigao site is more complex…: likely, the Perdiagao site is as close to an ideal 'two-dimensional ridge' [more precisely, a valley between two two-dimensional ridges] as Askervein is to an ideal 3d hill. What is different is the dimensionality of the obstacle(s) and the slopes.

P3/l.7 SW and NE ridge, respectively.

P4, l.1 ….for the exact directions: directions are not really provided in this table (of course, one could determine them by additionally using Fig. 1…).

P4/l.18 …due TO missing…. However, until now we have only learned that two 100 m masts were used with sonics (p.3, l. 1) – now, all of a sudden, we have temperature measurements (which failed). Given the well-known problems with (absolute) accuracy of sonic temperature measurements, I do not hope that the authors have used the sonic temperatures to calculate Ri (also, it is hard to imagine that the sonic produces wind but no temperature…). It seems that the list of instruments (on p. 3) should be completed.

Eq (1) In the equation, the authors write 'T', and after the equation they explain 'T overbar' (i.e., mean temperature). This must be consistent. However, the buoyancy term in the definition of the gradient Richardson number, in fact, should be defined with the potential temperature (not T), see, e.g., Stull (2012). While usually in Surface Layer micrometeorology, this is not really relevant, over the height of 100 m, this makes a difference.

P7/ l. 8 'In overlap regions only measurements of the lidar further downwind are used'. I am not sure whether I understand this. What I understand then looking at Fig. 4a, the 'lidar further downwind' for the entire overlap region (which is the purple part) is the blue one, right? So, the second lidar is almost obsolete (only the really small part of the 'rim' is from this instrument…). This needs some reasoning. Also, looking at Fig. 4 suggests that the two lidars did not scan the same range (opening angle of the RHI). This should i) be mentioned where the scan strategy is introduced and ii) be motivated.

P7/l.11 '…we do not attempt…': this seems to be at odds with l.7 (Cartesian coordinate system ….with the abscissa). The authors certainly need some rotation to do this, right? Does it mean that only one elevation angle for each RHI is used? I cannot see why this should be any easier to handle (and I cannot see what 'complex terrain' has to do with this). Can the authors please explain this procedure in some more consistency?

P9/l.15 … is in general more …

Fig 7 Unfortunately, the color coding in this figure is the opposite to that of Fig. 4.

Fig. 8 'reverse flow speed': this should be specified (median, average over the recirculation zone, etc.).

P11, l.11 'also influences [the] flow ...': given the purely descriptive approach taken in this paper, in a case study one cannot conclude from one case that 'the leeward recirculation zone and wake ….. decrease the mean wind speed…'. The wording has to be much more cautious (something like 'for this case with a strong recirculation

zone we observe a reduction in mean wind speed…'). Clearly, other cases with similar conditions but no recirculation zone would be needed to allow for a conclusion like 'due to the recirculation zone wind speed is smaller and turbulence intensity larger'.

P12/eq (2) I am not convinced that the ratio of the median values is an extremely useful measure to demonstrate the slow-down (increase in turbulence intensity). This would be a (statistically) appropriate measure, if wind speed were normally distributed (but often it is not). Is there any reason not to use a proper 'delta' (U_ds - U_us)/U_us ('ds'=downstream, 'us'=upstream), and average over all cases? If the sign of this measure were significantly different from zero (and significance can be tested…) – and even 'more negative' for recirculation occurrence than for no occurrence, this would be a strong indication that there is a reduction in mean speed (increase in intensity) due to the recirculation zone.

Tab. 4 If the 'delta's are defined as in eq (2), the given information is not in % (if the median ratio for 'recirc' is 0.42, say, and that for 'no recirc' is 0.50, the indicated difference amounts to -0.08 – and not even multiplying with 100 makes this to be an 8.2% reduction….). Definition of those 'delta's and their use should be made clear and the wording adjusted.

P13/l.4 'Algorithms are developed…': In fact, it is only one – and it is not really an algorithm, but rather a straight forward ad hoc procedure.

P13/l.21 should be made

References:

Baines PG: 1995, Topographic effects in stratified flows, Cambridge University Press, Cambridge, 482 pp.

Belcher SE and Hunt JRC: 1998, Turbulent flows over hills and waves, Ann Rev Fluid Mech, 30, 507-538.

Grubišic V, Doyle JD, Kuettner, J et al.: 2008, The terrain-induced rotor experiment, Bull American Meteorol Soc, 89, 1513–1533.

Kaimal JC and Finnigan JJ: 1994, Atmospheric Boundary Layer Flows, Oxford University Press, New York, 289 pp.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-847, 2018.