Response to Referee Comment 'RC1'

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This is the authors' response to Dries Allaerts' (RC1) review of our article **"From shear to veer: theory, statistics, and practical application"** (WES 2022-119). We include the referee's comments *in italic script*, followed point-wise by our responses in blue.

The authors are thankful for the time and effort spent—and constructive criticism made—by the reviewer.

Overall review

The paper uses the one-dimensional, averaged governing equations of atmospheric boundary-layer flows under the assumption of horizontal homogeneity to analyse wind veer. The theory is used to derive mathematical expressions relating wind veer to wind shear and turbulent stress, and it is also used to analyse the physical mechanisms giving rise to wind veer. The theoretical results are then evaluated based on canonical RANS simulations and real observations (i.e., including for example effects of stability and heterogeneity) from 4 different sites. The authors also propose a practical approach to estimate the wind veer based on typical measurement data. The paper presents a detailed analysis of wind veer in both idealised and realistic conditions, and it provides a lot of insights and explanations for why a certain behaviour is observed. As wind veer is likely to become more and more important for wind energy purposes, I believe this work provides valuable insight into the theory and the statistics of wind veer. My main concern is that, as the paper is quite lengthy and heavy on math, more structure is needed to guide the reader. Please find below more detailed comments and suggestions as to how to further improve the manuscript.

The authors appreciate the constructive comments and overall suggestion here; although we removed quite a bit of material (and math) for submission, you have helped to show how it can be more readable, understandable, and ultimately usable through more explicit indication of its structure and intent.

Main comments

1. *It is not clear from the introduction what the objectives of the study are and how it is organized. After summarizing the literature on the importance of veer, the authors quite abruptly dive into the mathematical derivations and formulations in section 2, and it took me about 20 pages until halfway section 3 to realize what the paper was actually about. The paper is quite lengthy, so more indications of how the paper is structured are needed in the introduction to guide the reader. For instance, it is by no means clear from the introduction that the authors will test the theoretical formulations of veer against RANS and observational data at 4 different sites.*

   We have adjusted the introduction to clarify the structure of the paper and its intent. The last paragraph of section 1 has been expanded to include this:

   "In this paper we investigate wind veer, showing its joint behaviors with and connection to shear and key parameters used to describe atmospheric boundary layer flow. In Sec. 2, after reviewing expression of the shear exponent and its relation to stability and turbulence, we derive new relations for veer; we show veer to be composed of shear-driven and Coriolis-associated stress gradient contributions. The theoretical behavior of veer is also derived for canonical cases such as Ekman and surface-layer flow, as well as the effect of shear-stress misalignment on veer. Further, in Sec. 2.4 practical relations from micrometeorology are elucidated, towards evaluation of the expressions developed for veer. Section 3

includes analysis of veer, exploring and connecting the developed relations to both computational modelling and observations. Section 3.1 gives RANS (mean) simulation results over flat terrain in neutral conditions for hundreds of combinations of surface-Rossby number and ABL-depth Rossby number, showing the dependence of veer on the latter as well as the counteracting behavior of veer's two primary components. Section 3.2 begins with analysis of multi-year observations from six different flow regimes across four sites showing the statistical behavior of shear with stability, and subsequently that of veer, also providing new empirical relations for the probability of occurrence of larger veer (due to the effect of stable conditions) and for the variability of veer with wind speed. The observational analysis concludes in Sec. 3.3 with simplified practical relations for veer based on observed shear, including comparison with joint distributions of veer and shear across the six flows analyzed. Finally the results summarily discussed and conclusions given, with ongoing and future work also described for the reader."

2. *Section 2 is quite heavy on math with a lot of different formulations for the same quantity, and it is not always clear what you are going to do with all these formulations. For example, there about 4 different formulations for the wind direction φ (or $\cos\gamma$) (Eq. 6, 13, 25, 29) and no less than 10 for the veer $\partial\varphi/\partial z$ (or $\partial\cos\gamma/\partial z$) (Eq. 9, 10, 12, 14, 15, 16, 21, 24, 38, 39)! More structure and guidance is needed in section 2 to make sense of all the different formulations. It could for example help to indicate upfront what expression you will derive in the various subsections and for what purpose (or alternatively, e.g. for 2.3.1 and 2.3.2, what aspects you will analyse and why), and it could also be useful to provide a summary or a table with the final expression which you will (mainly) use to analyse the data in section 3.*

   Along with the changes to the introduction section, following your suggestion we have added an explanatory paragraph at the beginning of section 2 to include guidance on the formulations:

   "In this section we define the shear exponent and veer, then derive relations for veer in terms of shear and vertical gradients of stress, as mentioned in the previous paragraph. Section 2.3 provides a number of expressions for veer; this is done to facilitate its calculation and interpretation in the different coordinate systems typically considered in wind energy flow analyses, and we also include forms that are independent of coordinate system. Because coordinates aligned with the mean wind for a given height of interest (e.g. hub height) are commonly used in wind energy, and because expressions for veer in such a coordinate system are simpler to express and calculate, we ultimately arrive at two forms in such a system (eqs. 14 and 16); due to its robustness, one of these (eq. 14) will later be shown in section 3.3 to be further simplifiable and usable (as eq. 38 or 39) in comparison with measurements."

   However, we note that (21) and (24) are not just generic veer expressions; also, (25) and (29) are not formulations for the wind direction but are angular differences $\Delta\varphi$ for the canonical Ekman and Ellison cases. We have updated section 2.3.2 (which includes eqs. 21–29) to more clearly and consistently include subscripts distinguishing particular cases (e.g. Ekman or Ellison) of the veer or wind profile, so that upon browsing the reader will not be confused or overwhelmed by what might otherwise appear as "yet another way to write veer."

3. *Eq. 9 and 10 show that, in the absence of baroclinicity, the veer only depends on the curvature of the stress profile. Later, it is shown that veer also depends on the shear (see Eq. 12, and 14–16). How is this possible? Is it perhaps so that the shear affects the veer by its impact on the stress profile, so that Eq. 9–10 are implicitly dependent on the shear? Please comment on the physics explaining these different forms in the paper.*

   Yes, these expressions implicitly include shear, as we now more explicitly point out below eq. 10; i.e., we add the text *"since the shear is implicit in the stress terms (and one would need to know the profiles of horizontal stresses to use these equations)"*.

4. *The authors propose two practical equations to find veer based on shear, equation 38 and 39. Eq. 38 only accounts for the shear, while Eq. 39 includes the contribution from the cross-wind stress. In Fig. 15 it is shown that both equations give comparable results, and for simplicity equation 38 is recommended. However, this seems to contradict with the earlier finding from fig 5, showing that the contributions*

of shear and crosswind stress are an order of magnitude larger than the veer and mostly balance each other. How is it then possible to get good results with Eq. 18 in which the crosswind stress is neglected? Please comment on thing apparent contradiction in the paper.

Basically the shear and crosswind stress contributions act similarly, which is part of how they nearly cancel each other out, leaving the veer as a sort of residual difference. The practical form (38) essentially just picks the 'easier' term to model. Admittedly we did not explain this in section 3, so now we add this around eq. 38 (also as motivation for such). E.g., the coefficient $c_{s\alpha}$ crudely accounts for this (in addition to that of stability, which was already written); the new/augmented text reads: *"The basis for the simple shear-driven form can be understood by recalling section 3.1 and Fig. 5, where we showed that the shear and crosswind stress-curvature contributions behaved in nearly identical but opposite fashions, with their sum amounting to $d\cos\gamma/dz$; (38) can be considered as a simple model assuming the veer behaves like either of its two components, but simply smaller in magnitude. The practical form of $|S|/|G|$ in (38) employs the log-law for wind profile and reverse geostrophic drag law (32) for $u_*/|G|$. The constant $c_{s\alpha}$ crudely accounts for the (competing) effects of stability on both $|S|$ and the geostrophic drag (and any other mechanisms affecting $|S|/|G|$), but also accounts for the smaller magnitude of $\partial\varphi/\partial z$ compared to its shear-driven component".*

Other scientific comments

1. Line 98: by definition $B = -z/L$. I believe this is incorrect and should be $B/\epsilon_0 = -z/L$.

   Yes, corrected.

2. *Eq. 8: It seems to me that you only use the first form of φ for later derivation and interpretation. The derivation of the second and third form is not so straightforward, and, given that these forms are not being used (as far as I can tell), I think they make the math in this section unnecessary complicated. Moreover, I believe the second form (with S and S^*) is missing a minus sign (or equivalently, the factor i should be in the denominator instead of in the numerator). It is not clear to me how you derive the third form from the second one. Unless these complex forms are essential for later derivation or interpretation, consider removing them.*

   Yes, we originally had a number of complementary derivations which were cast in complex form, using the second and third parts, but removed them before submission; we forgot to remove the complex pieces from (8), but have now done so.

3. *The wording used to introduce and derive equations in section 2.3 is not always clear. For example, line 143, what do you mean with "taking alternately the time derivative of (8)"? Line 152: what do you mean with the "dimensionless deviation of the wind from streamwise"? Line 168: "We note that (14) and (15) are more direct alternatives to dealing with functions of φ_G". Not clear to me what this is about. It is also not clear how Eq. 16 is obtained.*

   Regarding 'alternately', this referred to deriving veer by using a different differentiation than that which had just been shown for the preceeding expressions; we have now clarified the text. On line 152, 'dimensionless' referred to the speed; this has been corrected. Regarding line 168: we have simplified to be more direct, as φ_G arises from the definition of γ.
   We introduce how to derive (16), but did not include the full details of its derivation because there are already so many equations in the document; we have added the word 'then' connecting the two sentences introducing (16), to better indicate the order of re-expressing terms following (12).

4. *I'm confused about Eq. 17: The text says that β_{ma} is the angle between $\partial S/\partial z$ and $\langle sw\rangle$, but Eq. 17 uses arg(S). Are you implying that $\arg(S) = \arg(\partial S/\partial z)$? I don't see how this holds mathematically. Please explain.*

   We have fixed the typographic error in Eq. 17.

5. Line 192: Please add a reference for the Rotta parametrization.

   We chose to just use the Wyngaard (2004) reference because it more clearly shows the Rotta form as commonly used, and because Rotta (1951) is in German; however, we have now added the latter reference following your suggestion.

6. *Eq. 18 is introduced to show the root of the misalignment between shear and stress, but it is not entirely clear to me from the surrounding text how this equation explains the root of the stress-shear misalignment. Line 197–199 only talks about the absence of misalignment when the flux-gradient relation is used, but a clear interpretation of Eq. 18 and how it explains misalignment is missing. Are you saying that misalignment is directly related to the turbulent transport of turbulent stress (the last term in Eq. 18)?*

   Indeed misalignment can arise due to the last term in (18); lines 197–199 further mention that advection and horizontal transport (in addition to vertical stress transport) can cause misalignment. Further interpretation (with accompanying derivations) is the subject of ongoing work/writing beyond the scope of this article.

7. *Section 2.3.2: I found the section title misleading. The section does not talk about alignment of shear and stress, but instead discusses two canonical solutions for the veer and the relation with shear.*

   This sub-subsection does have a deprecated title, due to re-organization and reduction before submission. It is now re-titled "Canonical solutions using an eddy diffusivity".

8. *Eq. 21: How is this equation obtained? Do you get it from Eq. 10? Moreover, on line 222 you mention it "defies analytical solution", but I'm not sure what you mean with that. The differential equation has two unknowns $\varphi(z)$ and $U(z)$ so you need an additional equation anyway to solve it.*

   We have now included this information and re-worded for clarity.

9. *Line 243: It would be more clear to indicate in the subsection title that this is the Ellison regime you referred to earlier. The current subsection title "Linear diffusivity profile: ..." is not clear and I had to search for where this Ellison regime is actually discussed.*

   The paragraph title has now been renamed "Ellison solution (linear diffusivity profile / surface-layer regime)".

10. *Fig. 1 left plot is not entirely clear to me. Which part of the curve is for low/high values of z? Line 260 says that you can see that "the Ekman solution produces less mixing away from the surface ... and consequently a higher shear exponent than Ellison's." Is this statement referring to the left plot? Difficult to appreciate without knowing where low/high values of z are in the plot.*

    In the left-hand plot, near-surface values are near the top, i.e. giving the highest values of α and $h\partial\varphi/\partial z$; we now note this in the text to clarify. The sentence of line 260 has also been cleaned up with regard to this, and to reflect what is seen from the different plots of Fig. 1.

11. *Where is Eq. 35 coming from? Please provide a reference.*

    It arises in our earlier (2020) paper; such reference is now included.

12. *Line 375: How did you find the exponent -1.4? Can this be derived mathematically from Eq. 33 or is this an empirical estimate based on visual observation?*

    We find it by 'empirically' by fitting to the output; this aspect is now included in the text.

13. *Line 429-430: "... follow a curve which resembles the nondimensional M-O shear function ...". Not all readers will know the shape of the Φ_M curve by hard. Please show the curve or provide a reference.*

    We now include the α-analogue to $\Phi_m(z/L)$ in the plot and text.

14. *Line 450: Not clear what the angular brackets in $\langle\alpha\rangle$ mean.*

    This denotes the mean, as indicated later in the sentence adjectively for veer; however we now explicitly indicate this also before $\langle\alpha\rangle$.

15. *Line 487: "The lowest [veer scale] corresponds to the offshore Høvsøre case, while the highest ...". It is not particularly clear how this can be seen. The veer scale defines the slope of the PDF in log scale, correct? As in, the higher the veer scale, the lower the slope?*

    Since the veer scale is defined as $\Upsilon_{\text{veer}}^{-1} \equiv \partial[\ln P(\Delta\varphi/\Delta z)]/\partial(\Delta\varphi/\Delta z)$, i.e. via reciprocal, then smaller Υ_{veer} correspond to steeper (more negative) log-slopes; the offshore case (blue dashes in Fig. 10) are the steepest, with $\Upsilon_{\text{veer}} = 0.07°/\text{m}$.

Minor/technical comments

1. *Variable Ro_0 is first used on line 264 but only defined on line 290. Please define upon first use.*

    This has been rectified.

2. *Line 274: Should $|S/G|$ be $|S|/|G|$?*

    Yes; this typo has been corrected.

3. *Line 307: "along with with (30) or (32)..."*

    The extra 'with' has been removed.

4. *Line 374: "... that is not inconsistent with" → Avoid double negation and just use "consistent with"*

    Done.

5. *Probability density function values are sometimes difficult to interpret. For example, in figure 7 (right), a value of $P(\alpha, L^{-1}) = 1000$ is not very meaningful unless you know the bin size $d\alpha$ and dL^{-1}. It would be much clearer if you could show results in terms of their relative frequency of occurrence.*

    We include frequency of occurrence in the captions to the plots where this was lacking (under Figures 7, 9, 13, and 14).

6. *Caption of Figure 15: what are the two empty brackets "()"? Should these contain a reference to an equation?*

    The equation references disappeared, and are now corrected/included.

7. *Line 604: "condtions" → "conditions"*

    Corrected.

8. *Line 667: "convering" → "covering"?*

    Corrected.

9. *Line 703: "underpreictions" → "underpredictions"*

   Corrected.

10. *Line 716: "stabiilty" → "stability"*

    Corrected.

Again we express appreciation to the Dries Allaerts for the review, which served to ultimately improve the scientific dissemination and support the scientific method at large.

with kind regards,

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