Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2015-995-RC1, 2016

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "The impact of the diurnal cycle of the atmospheric boundary layer on physical variables relevant for wind energy applications" *by* Antonia Englberger and Andreas Dörnbrack

Anonymous Referee #1

Received and published: 29 February 2016

Review of "The impact of the diurnal cycle of the atmospheric boundary layer on physical variables relevant for wind energy applications" by Englberger and Dörnbrack.

The authors present large-eddy simulations of a diurnal cycle loosely based on thermodynamic observations from the BLLAST experiment. They compare their simulations, which are tuned with subsidence and radiative cooling, to temperature profiles from BLLAST, but they increase the winds in their simulations by a factor of 3 from those in the observations. The authors do not acknowledge previous work on LES of the diurnal cycle (Kumar et al. 2006; Basu et al. 2008) and claim to be performing the first LES

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of the diurnal cycle (lines 85-86). Details of the simulations, including presentation of wind and turbulence profiles, as well as components of the TKE budgets are presented. Winds and turbulence in the lowest 200 m are discussed in detail "to expose the impact of the individual phases of the diurnal cycle on these physical variables which are relevant for wind energy applications", but novel insights are not provided and the authors fail to refer to previous simulations or observations which have explored the impact of the diurnal cycle on wind-energy-relevant quantities. Although there may be novel contributions in this work, the present manuscript does not highlight such contributions in a satisfactory way. Several concerns are outlined below, along with suggestions that could help the authors refocus a revised manuscript.

Part of the confusion in the presentation may be due to a lack of focus because the simulations are not placed in a proper context: instead of highlighting any novel aspects of these simulations, the authors instead focus on an interesting challenge that is unrelated to the simulations discussed in this manuscript. Specifically, much space in the introduction is devoted to a summary of large eddy simulations of wind turbine wakes (lines 63-95) although the present study does not include wind turbines. If this work is an intermediate step toward LES of wind turbine wakes, the present study should still be unique and novel enough to stand on its own. The authors could focus on the diurnal cycle of the ABL with their LES, providing more details on some of their technical approaches (nesting, immersed boundary method for canopies, subgridscale turbulence modeling) – these are important aspects of their approach that are neglected in the discussion. Further, previous contributions that have already carried out LES of a diurnal cycle are omitted from the literature review (Kumar et al. 2006; Basu et al. 2008). The authors should review these papers and consider how the present work provides a unique contribution.

Further, the correspondence of these simulations to the BLLAST observations is questionable. The authors compare their potential temperature profiles to the observed potential temperature profiles at only three points in the diurnal cycle (one profile is used for initialization, three for evaluation). No data other than soundings is used for evaluation although BLLAST included considerable instrumental deployments. They use two tuning parameters (subsidence and radiative cooling) to achieve approximate agreement with the profiles (but, as noted below, the authors do not refer to previous work on LES with subsidence). How should a reader develop confidence in the selection of subsidence rate and cooling rate? Are there any observations that support these choices of subsidence or radiative cooling? Second, the authors modify the winds in their simulations substantially from the observed 3 m s-1 to 10 m s-1 (a factor of three!) but still suggest that their simulations compare well to the BLLAST observations. It would be a cleaner comparison to first match both the winds and the thermodynamics (so that they can validate simulations with observed fluxes, aircraft data, etc.). Later, once the reader trusts the simulations, the authors could increase the winds if necessary. As the simulations stand right now, they are not really based on any observations with so many tuning parameters and vastly different winds. If the authors really require winds of 10 m s-1, they should find another experiment (CASES-99? Numerous studies from Cabauw?) that can provide adequate data for validation.

Finally, the authors do carry out a small ensemble of simulations with varying canopies and obstacles in the flow, emphasizing the novelty of surface heterogeneity in LES of the ABL. Again, they have not reviewed previous work in this area (Belcher et al. 2003; Kang et al. 2012, among others), nor have they supplied sufficient details on their implementation of the immersed boundary method for readers to understand if this is a novel contribution. Nevertheless, there could be novel aspects to this part of their study that would justify a publication on their LES.

Please note that although I am recommending rejection of the manuscript in its current form, I do encourage the authors to think carefully about how to reframe this work to identify and emphasize novel contributions.

Numerous comments regarding references, clarify, grammar, and scientific concerns are found below, listed by line number.

27: “its minimum level” should be “their minimum level” (referring to surface fluxes)

35: should read “The CBL is not only influenced from below, but also from above, as entrainment processes incorporate. . .”

37: updraft and downdraft instead of “updraught” etc.

40: why are these citations not in chronological order?

46-55: these lists of citations are not put into context for the reader

58: this research question is not particularly innovative as LES of the diurnal cycle has been carried out (Kumar et al. 2006; Basu et al. 2008) (and these papers should be cited)

61: Why is a recent paper on wind energy research (Emanuel et al. 2015) referred to for discussion of the diurnal cycle of the ABL when many other older and more detailed studies of the diurnal cycle of the ABL for wind energy applications could be considered? should cite (Barthelmie et al. 1996; Walter et al. 2009; Storm and Basu 2010; Rhodes and Lundquist 2013, among others)

63-64: several papers have investigated the impact of atmospheric stability on wind turbine wakes (Aitken et al. 2014; Bhaganagar and Debnath 2015; Mirocha et al. 2015, for example)

73: wind turbine loading should refer to (Sathe et al. 2013)

79: the comment that “most papers assume NBL” is not consistent with the subsequent citations focusing on the SBL and CBL

87: The discussion states that no other papers have explored how the complete diurnal cycle influences wind turbine wakes, but that is not the point of this present manuscript. This paper focuses on parameters relevant to wind energy applications, which is different from wind turbine wakes. It feels like the authors are trying to define a specific niche, not very accurately, and without correspondence to the goal of the present paper. The authors should think carefully about the specific novel contributions of the specific work presented here, and emphasize those contributions, not necessarily the larger project.

144: the subgridscale model is very important but is not discussed in any detail here. Is it a TKE-based model? More details are necessary.

167: “satisfactory” rather than “satisfying”

175: Please explain that “MT” stands for “morning transition” and similarly for “ET”

194: “descent” rather than “decent”; check grammar in this sentence,

194: discussion of subsidence is interesting but very focused on recent work. Please also refer to previous work on subsidence such as (Mirocha and Kosović 2009). In fact, given that subtle changes in the specification of subsidence can significantly change surface fluxes and boundary-layer height, how can the authors justify their choice of w_sub (and the resulting choice for radiative cooling?)

233: more explanation or references are required for the “immersed boundary method” – many choices for how to implement the immersed boundary method are possible.

258: validation of LES is carried out only against radiosondes? What about flux measurements and the multiple other measurements possible during BLLAST?

Figure 1: difficult to interpret for red-green color-blind readers. Can the lines be thicker and labelled directly instead of relying on the legend? Are axis labels consistent with ACP requirements?

271: should be “W m-2” rather than “W m2”

283: it appears that the authors are attempting to match the thermodynamic aspects of the observations but for a difference range of wind speeds (10 m/s instead of the observed 3 m/s). Wouldn’t the thermodynamic profiles have changed with different winds? The utility of the comparison to observations seems lacking, and it is not clear that “validation” describes what is happening here.

300-315: this “validation” is really a qualitative comparison. Can quantitative metrics be calculated?

311-314: speculation on what tweaks could improve model performance are not appropriate: either make the changes and evaluate the simulations or avoid commenting on possibilities.

316-319: sentence unclear

334-349: these observations are not particularly novel or innovative for LES of a CBL.

356: Please refer to wind energy industry references for assertions of “typical” parameters. The Global Wind Energy Council has appropriate statistics.

366: The variability of the wind profile between daytime and nighttime is well known and documented in the literature already (Barthelmie et al. 1996; Walter et al. 2009; Rhodes and Lundquist 2013)

370-417: The discussion of the TKE budget does not introduce novel insights compared to previous studies such as (Beare et al. 2006)

440: The main conclusion seems to be the effect of obstacles on shear. Again, the novelty of this result is questionable (see previous work, such as (Belcher et al. 2003))

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Mirocha JD, Kosović B (2009) A Large-Eddy Simulation Study of the Influence of Subsidence on the Stably Stratified Atmospheric Boundary Layer. Bound-Layer Meteorol 134:1–21. doi: 10.1007/s10546-009-9449-4

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Rhodes ME, Lundquist JK (2013) The Effect of Wind-Turbine Wakes on Summertime US Midwest Atmospheric Wind Profiles as Observed with Ground-Based Doppler Lidar. Bound-Layer Meteorol 149:85–103. doi: 10.1007/s10546-013-9834-x

Sathe A, Mann J, Barlas T, et al (2013) Influence of atmospheric stability on wind turbine loads: Atmospheric stability and loads. Wind Energy 16:1013–1032. doi: 10.1002/we.1528

Storm B, Basu S (2010) The WRF Model Forecast-Derived Low-Level Wind Shear Climatology over the United States Great Plains. Energies 3:258–276. doi: 10.3390/en3020258

Walter K, Weiss CC, Swift AHP, et al (2009) Speed and Direction Shear in the Stable Nocturnal Boundary Layer. J Sol Energy Eng 131:011013. doi: 10.1115/1.3035818

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