Atmos. Chem. Phys. Discuss., 10, C8025–C8029, 2010
www.atmos-chem-phys-discuss.net/10/C8025/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Surface heterogeneity impacts on boundary layer dynamics via energy balance partitioning" *by* N. A. Brunsell et al.

Anonymous Referee #1

Received and published: 27 September 2010

A review of the manuscript

Surface heterogeneity impacts on boundary layer dynamics via energy balance partitioning

N. A. Brunsell1, D. B. Mechem1, M. C. Anderson2 1Dept. of Geography, University of Kansas, Lawrence, KS 2Hydrology and Remote Sensing Laboratory, USDA, Beltsville, MD

Status: under review for publication in Atmospheric Chemistry and Physics (ACP)

This manuscript presents results of LES of the convective atmospheric boundary layer (ABL) flowing over a real landscape. The authors evaluate the effects of changing bandpass-filter width (applied to landscape) and freestream velocity (forcing); the filter width is chosen such that the surface heterogeneities are much smaller (200 m), on the order of (1600 m), and much larger (12800 m) than the boundary layer height. This filtering is shown to change the contribution of surface-level fluxes of sensible and latent heat. The motivation for this work is to numerically investigate the effects of such changes on the ABL dynamics. I enjoyed reading the manuscript but can only recommend it for publication in ACP if the authors explain Item 1 satisfactorily, and if they address Items 2 – 8.

1. p. 17820, Numerical model description This is my central concern with the manuscript. On p. 17821 Line 20 and Line 21, the authors report that the v (transverse?) velocity component is set to zero. Can the authors please clarify what velocity this corresponds with, and whether this comment relates to the local transverse velocity or to the transverse Geostrophic forcing component? I expect that the authors are referring here to the transverse Geostrophic velocity component. If this is the case, it is preferable to instead refer to the forcing as Geostrophic, with if the streamsiwse and transverse velocity components are 10 m.s-1 and 0m.s-1, respectively, for example. If however the authors are imposing that the transverse velocity is globally set to zero everywhere, then I would recommend that the editor reject the manuscript. Such a constraint indicates a 2-dimensional simulation and is therefore of no scientific merit. Although I do not expect that this is what the authors have done, this information is very important and must be clearly explained. If the authors are simply imposing that Vg = 0 m.s-1, I would also like the authors to add to Figure 13 plots of the normalized transverse velocity variance ( ), where v corresponds with transverse velocity. Also, later in the manuscript when the authors discuss increasing the streamwise velocity, they refer to this with the parameter as (e.g,: p. 17824, between Line 10 and 15). To again use strict LES nomenclature, this commonly would indicate the local streamwise LES (spatially filtered) velocity, (x), and clarification is needed. How have the authors applied the forcing? Was it done by actively adding 3 m.s-1 to (x) at every location in the domain? Or rather by adding 3 m.s-1 to the Geostrophic flow?

Can the authors please provide the transport equations in the standard form found in an LES paper, for example, Eq. 1.1: mass, Eq. 1.2: momentum, Eq. 1.3: generic scalar (humidity/temperature). From this, it is appropriate to also indicate what different components of these equations represent. It would also be instructive to please provide some of the traditional nomenclature found in numerical ABL studies, such as clarification that u = (u,v,w), and x = (x,y,z), where x, y, and z, correspond with streamwise, transverse, and vertical direction, and subsequent indication of a filtered variable (i.e.,). The results show statistics for u, θ, etc, but in fact this should be , , etc, where tilde indicates spatial filtering. I am denoting a spatially filtered variable here with a tilde, although of course the authors need not using this symbol, but some symbolic indication of a filtered variable is necessary. In the present form, strictly speaking, the results indicate that the statistics are from a direct numerical simulation of the ABL.

2. p. 17821, "The allure of LES is that the SGS turbulent transports lie within the inertial subrange, so the only role of the SGS Model is to passively transfer energy scale."

Referee comment: Actually, in the surface layer (z < 0.1zi, where zi is boundary layer height), and especially for the first few grid points, this is not true. The grid width, Δ, is in the production range and the full range of scales are unresolved. This attribute is inherent in LES of high-Reynolds number boundary layers (such as the ABL), and has motivated a variety of approaches such as tuning of the SGS coefficient in the near wall region (Mason and Thomson, 1992) and development of hybrid RANS-LES schemes (Senocak et al., 2007: Boundary-Layer Meteorol., 124 405âĂŤ424). Also, the SGS model does not actively pass energy between scales but rather balances the turbulent energy production with SGS dissipation (S. Pope, Turbulent Flows: "Large-Eddy Simulation"). Please update the manuscript.

3. p. 17816, between Line 15 and 20: I could not understand this sentence:

"Air temperature is less sensitive to surface heterogeneity than water vapor, which implies that the role of surface heterogeneity in modifying the local temperature gradients

ACPD
10, C8025–C8029, 2010

in order to maximize convective fluxes."

This needs to be clarified.

4. p. 17824, between Line 10 and 15 The authors offer clear reasons and justification to run LES over surfaces bandpass-filtered to 200 m, 1600 m, and 12,800 m: to investigate boundary layer dynamics when the scale of heterogeneity is much less, approximately equivalent, and much larger than the boundary layer height. However another important part of the study involved running LES with increasing streamwise velocity, (Author Comment # 1), by adding 3 m.s-1. Presumably the observed sounding data for the base case corresponded approximately with Geostrophic flow – can the authors make note of this? More importantly, can the authors add some physical arguments for the choice of adding an additional 3 m.s-1 and 6 m.s-1; were these values based on some statistical expectations for potential changes in the expected streamwise velocity, or perhaps based on some other rationale?

5. Figs. 1 and 3 The figures show x-y visualizations of fields relevant to the work. The x- and y-labels of the figures are confusing to the referee. Perhaps the authors have plotted 'x' and 'y' to indicate spatial location (in m?), but the numeric values indicate computation meshpoints. The meshgrid intrinsic command in MatLab very conveniently associates computational mesh values with spatial location.

6. p. 17848, Fig. 10 The size of the figure text is very small and almost impossible to read; please use a larger fontsize. Many of the other figures have very small fontsize for the text labels – I would recommend that the authors enlarge the fontsize for the benefit of the readers.

7. p. 17829, Line 16 The authors offer Table 1 data but the discussion is slightly limited in terms of explaining the trends in variance of temperature and soil moisture. For example, lower <Ts> (where <. . .> indicates plane-averaging) with increasing wind speed is an intuitive result, however I also notice that the difference in variance of Ts and θ between resolutions (especially between the fine-scale and observed) is almost

an order or magnitude, and that the variances are monotonic with filter width. And that at all wind speeds the final variance of both quantities is roughly 50% to 80% of the observed (100 m) results. I do not believe the dynamics responsible for these differences and trends are fully explained, and such an explanation would be interesting to readers and relevant to the manuscript. Also, spatial mean or plane-average is more commonly indicated with <. . .>. The nomenclature used here for and , is confusing because on overline is often used to denote time-averaging. Also, in LES an overbar (or \widetilde{}) can indicate spatial filtering (in the case that the results indicate a time-average statistic). For this reason I recommend the authors amend “2.1 Numerical model description” as discussed above, to clarify the LES nomenclature to be exactly consistent throughout.

8. Results (p. 17838âĂŤ17851): Numbers should be given dimensions or (preferably) normalized by characteristic parameter values -Table 1: Temperature and soil moisture dimensions not given -Fig. 2: Resolution (m). Typically all length scales are normalized by boundary layer depth or some other characteristic length, H -Figs. 7, 8, 9, 10, 11, 12, 13: Instead of plotting data at vertical points z(m), it is also common to plot as z/H. -Figs. 7, 8, 9, 10, 11: f (m-1) can be normalized with the H length scale as fH. -Fig 13 a, e, i: Velocity variance typically normalized by square of friction velocity

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C8025/2010/acpd-10-C8025-2010-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17815, 2010.

ACPD

10, C8025–C8029, 2010