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Biogeosciences Discussions

Interactive comment on "Turbulence in a coastal Mediterranean area: surface fluxes and related parameters at Castel Porziano, Italy" *by* S. A. Cieslik et al.

Anonymous Referee #1

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This paper is designed to give some background description of the meteorology and turbulence over scrubby vegetation within a coastal sand dune environment during the ACCENT/VOCBAS campaign. There are no substantial problems with the data or analysis presented; however, most of the analysis is fairly superficial and the final results are somewhat predictable. Most of this could just as easily appear as subsections in more substantial manuscripts from this study concerning H2O relations, carbon budgets or ozone deposition. There are also aspects of the manuscript that seem out of place (such as the discussion on the decoupling coefficient) that would fit better in other papers. The most interesting aspect of this study (the sea/land breeze and its impact on measured fluxes) is barely touched upon and there is no estimation of flux

footprints from the different measurement heights. Several specific comments address areas within the manuscript which could be aided by a flux footprint analysis. I would recommend publication only after major revision.

Specific Comments:

(1) Section 2, Site and Instrumentation: There is no description of how any of the ozone fluxes were measured. Nor is this referenced to some other publication. If you are going to show ozone flux data (Figure 2), then you need to describe or reference how these measurements were made. Conversely, CO2 flux measurements are described, but never shown.

(2) Section 2, How did the RM Young sonic operated by the IBAF group compare with the other 2 Metek sonics if they were run side-by-side at the same height? This seems a critical comparison to validate the subsequent conclusions that the constant flux layer for sensible heat flux extends down to z = 6 m.

(3) Section 3, page 3359, Last paragraph and Figure 2. The authors mention humidity "At the highest level (9.5m)", yet the profiles in the figure only extend to 6 m. The authors explain the large changes in absolute humidity above 4 m as indicators of the land/sea breezes (wetter air above during day, drier air above at night). However, this brings up the question of whether there is adequate homogeneous footprint for the eddy covariance flux measurements. How do these local circulation patterns affect the flux measurements within this flow aloft at 9.5 m? Are the measurements at these higher levels actually representative of the surrounding surface fluxes? As this is a paper to describe turbulence as it relates to micrometeorological flux measurements, this seems to be a critical question. At the least, some estimate of the flux footprint is warranted. Furthermore it would be helpful to have an aerial layout of the site included in figure 1 for reference during the subsequent discussion on wind direction patterns.

(4) Section 3, page 3359. From the standpoint of discussing dew formation – it would be more informative to see average profiles of relative humidity in Figure 2 as opposed

to absolute humidity. Most likely the same discussion points concerning inversions and drier/moister air aloft could be made by showing relative humidity instead.

(5) Section 3, page 3360 into page 3361. Discussion on the constant flux layer – It appears from Figure 5 that momentum fluxes (u*) agree quite well at all heights, with sensible heat agreeing only at the 2 uppermost heights. This is a bit unusual as momentum often is more affected by roughness than scalar fluxes. Again, do the flux values obtained at the upper heights truly represent the nearby surface exchange? Given the absolute humidity profiles in figure 2 and the "non-homogenously distributed vegetation", it would be informative to include a comparison of the latent heat fluxes at all 3 heights. An even more stringent test would be to look at the energy balance at each height.

(6) Page 3361 lines 15-20. I am not sure why there is a discussion of the eddy diffusivity here. It appears that the Mereu et al., reference given here does not use the flux gradient method. Even so, a discussion of the eddy diffusivity would have added weight with an analysis of flux footprints at the different heights. The H2O gradients show complicated behavior (Fig. 2) – depending up upon the heights chosen, the flux-gradient method could lead to either emission or deposition! How is this reconciled with the measured eddy covariance fluxes? To me, this is the crux of the problem in understanding surface fluxes at this site and the analysis here is superficial at best.

(7) Page 3362, line 7. How do you compute values of Ra and Rc. If this is using the big leaf resistance analogy? Then you also need Rb as well (to get Rc). None of this is explained or referenced. Furthermore, there is no discussion of what the measured values of the decoupling coefficient for this ecosystem tells you. As with the eddy diffusivity – the inclusion of the decoupling coefficient discussion seems a bit out of place and left for the reader to decipher. It is better suited in another manuscript.

(8) Page 3362, line 17. There are 3 different soil heat flux sensors located in different environments – How do the authors obtain an average G value in the energy budget

calculations shown in Figure 8? Are these measurements weighted by the percentages of surface cover?

(9) Figure 8. Is the linear fit forced to zero? If so, please state this. It appears that it a simple linear regression would have a small positive intercept (and likely a slightly smaller slope).

Technical Comments:

(1) Figure 2. The times reported in the legend are in AM/PM as well as the discussion in the text; however, in the caption, they are in 24-hour time. Please make these consistent. I would recommend staying with 24-hr time. Also, this is a very hard figure to read because all of the varying shades of gray and symbols are difficult to discern. A color-coded figure would be much clearer.

(2) Page 3359, line 19. Should read: night time

Interactive comment on Biogeosciences Discuss., 6, 3355, 2009.