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Interactive comment on "On the impact of atmospheric waves on fluxes and turbulence statistics during nighttime conditions: a case study" *by* D. J. Durden et al.

Anonymous Referee #1

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Summary: This paper analyzes the impact of waves on turbulence kinetic energy and momentum and scalar fluxes in the nocturnal stable boundary layer. Turbulence observations from three levels of a very tall tower and from a microbarograph at surface level are subject to wavelet filtering and triple decomposition to separate the contribution of waves, mean flow, and turbulence on the instantaneous state and flow variables. While the authors attempt to address a number of interesting topics that are relevant to both the turbulence and applied biogeochemical flux communities, the draft is light on science, lacks in statistical rigor, and in my opinion is not a good fit for the journal Biogeosciences. The main criticisms include its a) poor stochastic relevance of the selected data, b) very descriptive nature of the interpretation, c) poor definition of motions analyzed, and d) overstretching interpretation of findings with respect to relevance for the applied flux community. Please see further comments below. The language is acceptable, but little technical. The organization of the paper needs to be substantially improved since there is a large degree of redundancy, and conclusions need to be separated from a summary of results. Figures could be improved. I suspect this paper is an attempt to publish a BS or MS thesis, which in its current form does not satisfy the standards of a high-quality peer-reviewed journal. In summary, I leave the decision to the editor to either reject the paper for publication (while recommending a more suitable outlet such as ACP, BLM) or to recommend major revisions.

Major comments:

1. Poor stochastic relevance: The data selection process is poorly described and confusing, filtering criteria were selected too narrowly. It is not clear to me how much data the authors have available, since they initially mention a period of 22 Apr to 9 June, but then they select the 2-hour period on 3 Dec as case study. Contradicting information about how many hours satisfied the selection criteria of period lengths between 3 and 30 min (why these arbitrary boundaries?) can be found in different part of the manuscript. Why did the authors only choose two 2-hour periods as case study? Why not include all periods, or even better widen the selection criteria? Selecting very few data using opaque filters gives the reader the impression that results are biased towards night when waves may have a large impact on turbulent fluxes and other statistics, but shed little light on the relevance of wave-contaminated fluxes for most applications. Either waves are an important, frequently occurring relevant atmospheric phenomenon that needs to be accounted for in seasonal statistics of land surface atmospheric exchange and this can be shown using a broad stochastic basis. Or, they are sporadic events with little importance for long-term statistics that may impact instantaneous fluxes, but not introduce bias into the interpretation of longer-term studies. The current manuscript does not lend any insight to answering this question because of its

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poor statistical basis of 4 hours of observations only. Even for a case study, the data set is too small. Ensemble averaged statistics for several distinctive nocturnal boundary layer regimes would be more appropriate and meaningful.

2. Descriptive nature of the interpretation/ results: Given the expertise of the authors, I am surprised about the lack of important elements such as e.g. discussion of vertical coupling between observational levels, quadrature spectra to analyze phase relationships, stability analysis beyond the bulk Richardson number (are these levels even in the same boundary layer?), Brunt-Vaisala frequencies, etc. The triple decomposition employed to separate the wave-part from the turbulence is adequate, but could be substantially improved by adjusting the averaging time scale used to compute $\widetilde{(x)}$ to the phase length of the detected waves, while the mean $\overline{(x)}$ is computed over the entire averaging interval (30, or 60min?). The authors' claim that the first night is quiescent while the 2nd night is turbulent is difficult to reconcile given the time series presented in Fig.4. Given the focus on turbulence statistics including TKE, friction velocity, and sensible heat flux (which all remain undefined), the paper does not fit the scope of Biogeosciences since trace gas fluxes play a subordinate role. Latent heat flux is mentioned once on the manuscript (page 5159, Line 4), but no results are presented.

3. Poor definition of motions analyzed: The authors do not provide a clear definition of the motions they so carefully filtered for. A physical description is needed to gauge representativeness and relevance of the studied motions in comparison with similar studies or sites. In the manuscript, terms including 'wave-like motions', 'waves', 'gravity waves', 'Kelvin-Helmholtz instabilities', 'density currents', and 'solitary waves' are used almost synonymously, while they represent very different atmospheric phenomena. These motions are difficult to separate and for most of them we lack a good physical understanding including their stochastic properties and generating mechanisms, but one shouldn't oversimplify and group them all together either. Some of the motions are thought of as 2-D motions (flat

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pancakes), so it is unclear to me how they would generate a similar signal at 3 heights with substantial separation distance exceeding hundreds of meters. I could not see a correspondence in signals shown in Fig.4 and the authors fail to provide any quantitative analyses such as lagged correlations or phase analyses to back up their claim the gravity waves are ducted between all three levels. As I understand, microbarometer data are only available at the surface, while the lowest turbulence statistics were collected at 34m agl. I would question the assumption that the surface pressure dynamics in strongly stably stratified environments are a good predictor of flux and other turbulence statistics at higher levels. Did the wind and sonic temperature traces lag or lead the pressure perturbations? Do wavelet spectra (Fig. 2) look similar for pressure, wind vectors, and scalar concentrations?

4. Overinflated interpretation of the results: As described earlier, the analysis of 4 hours of observations divided across 2 nights selected out of 9 months of data is no convincing argument that gravity waves have a profound, quantifiable impact of long-term turbulence or biogeochemical flux statistics. A more fundamental comment with regard to relevance of the findings to the applied flux community is that ideally one was able to include all motions contributing to the exchange between the land surface and the atmosphere to compute the net exchange of water, carbon, momentum, etc including waves, advective processes, storage, and turbulence. Of central interest is the question if motions introduce a random or systematic bias. The latter is of primary concern for seasonal and annual statistics, while the former is relevant for studies targeting turbulence specifically. The impact of waves on turbulence statistics is well documented as the authors admit. A sporadic false classification of nocturnal averaging intervals as being well mixed or decoupled depending on the friction velocity criterion will not impact seasonal and annual NEE.

Interactive comment on Biogeosciences Discuss., 10, 5149, 2013.