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Biogeosciences Discussions

Interactive comment on "Surface layer similarity in the nocturnal boundary layer: the application of Hilbert-Huang transform" *by* J. Hong et al.

J. Hong et al.

jkhong@yonsei.kr

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Thank you very much and we appreciate your constructive comments on our manuscript. As you have suggested, we have revised the manuscript by incorporating all of the comments provided by the referee 3. Below is the authors' response to the reviewer.

Page 9681 section 2: Since not all of the readers of Biogeosciences may be familiar with the mathematical procedures used in this paper, the authors are strongly recommended to give more user-friendly explanation of HHT. One example is that the explanation of HHT in pages 9681 through 9683 should have closer links with Figure 1 and 2, for example by referring to Figure 1 at each step of HHT, or by putting terms such as "m1(t)" in Figure 1. In addition, difference and/or relationship between HHT and HT, if there are, is needed to be mentioned. Furthermore, the authors seem to fail to present a concise explanation of HHT, though they mentioned its advantages, in the abstract, the introduction, or the conclusion sections.

> Reply: We incorporated the reviewer's comment into the manuscript. Like other papers to use the wavelet transform in boundary layer meteorology, the HHT is a research tool and is not an ultimate goal of this study. We think that too much detail on the HHT hinder readers from application of the HHT to their own study due to heavy mathematics. Because we did our best to refer most of the literatures on the HHT in our manuscript, readers who want to know more easily find all relevant information in the literature cited in our manuscript. Also we believe that we provided the information on the HHT as not-insufficient as other HHT application papers (e.g., Lundquist, 2003 in the reference). Thank you.

Page 9685, after line 22: The sentences starting with "The surface is defined as the layer where ..." is not consistent with the other part of this paper. The usual definition of the surface layer is, say, the lowest one-tenth of the atmospheric boundary layer, where MOS, or surface layer similarity, is valid, that is nothing to do with the atmospheric stability. If the authors does not follow it, then it is fine, though it contradicts with their texts about "surface layer similarity" in the introduction section. Moreover, the data presented in Figures 3 and 4 shows the range, $10^{-4} < z/L < 10^{1}$, that is not "the order of 1". Later in this paragraph, the authors seems to try to connect outer- and inner-scale turbulence with the stability? Is this what they want?

> Reply: In general, the surface layer depth has order of the Obukhov Length (L) and L is the ratio between shear-generated turbulence and buoyancy-generated (or destroyed) turbulence (Kaimal and Finnigan, 1994). That is, typically if height z is smaller than L, we regard that we are in the surface layer. Consequently, in the surface layer,

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the shear-generated turbulence dominates the buoyancy turbulence and turbulence is possible even in the stable boundary layer where buoyancy destroy turbulence. In this sense, we believe that our definition on the surface layer is consistently used in our manuscript and not different the reviewer's comment below (shear-driven turbulence). Accordingly, we can say that, as a rule of thumb, "surface layer is the lowest one-tenth of the atmospheric boundary layer" but this expression is not exactly true. In particular, please make sure that we didn't say that the depth of the surface layer is exactly L. Instead, the depth of the surface layer is order of L. The reason why we bring up outer- and inner-scale turbulence is that two different eddies contribute variance and covariance differently and therefore eventually make an impact on turbulence statistics used in our study. This is very important subject in boundary layer meteorology and beyond this manuscript. But we just wanted to give other scientists the fact that the HHT can be one of tools for such kinds of studies.

Page 9686 line 6, line18; Page 9687, line 14 "up to z/L 0.5" or else: The authors seems to misunderstand the z-less stratification which is valid under very stable conditions, namely $z/L \to \infty$. At least when z=L < 10-2, the turbulence can be regarded as near neutral, where the fluctuations are shear-driven. The fact that dimensionless moments are constant under near-neutral condition in Figure 2 is only due to that the shear only plays a role. Interpretation of Figure 5 is also incorrect. This reviewer thinks that the use of z-less does not help the authors to validate the use of HHT in this text.

> Reply: As the reviewer correctly pointed out, z-less stratification is the theory for the strongly stable boundary layer. Our interpretation in this study is the asymptotic behaviors of turbulence statistics. Recent studies contradict validity of z-less stratification in the strongly stable condition. For example, Yagüe et al (2006) argued that their data reveal the level-off of ϕ_m and ϕ_h in strong stable conditions and this level-off supports z-less turbulence. However, Grachev et al. (2005) interpreted this

level-off of ϕ_m and ϕ_h as an evidence of the breakdown of z-less stratification. Dias et al. (1995) showed that third-order moments remain constant with height, indicating the robustness of z-less stratification. Pahlow (2001 reported z-less stratification is not valid in general using the observation data. However, by differently analyzing the same data set and large-eddy simulation, Basu et al. (2006) supported z-less turbulence in the strongly stable boundary layer. Recently, Cheng et al. (2005) opposed z-less turbulence by analyzing turbulent and mean meteorological data collected over the flat Arctic pack ice in the SSL. Consequently, while we discuss the validity of z-less turbulence, it is important to know how some turbulence statistics (e.g., σ_w/u_*) become asymptotically constant or not in z-less stratification. Our interpretation on Figure 5 is related to this asymptotic behavior of turbulence statistics because Z-less turbulence predicts that the TKE transport term linearly decrease in the strongly stable condition (Dias et al., 2009). About the behavior of this transport term in the stable boundary layer, Wyngaard and Coté (1972) reported that $\frac{\partial \overline{ew'}}{\partial z}$ is zero in stable conditions but the normalized transport term should increase linearly in z-less stratification Dias et al. (1995). In our study, the contribution the TKE transport did not have any linear increasing patter and further study needs to be done for better understanding the TKE transport in the strongly stable conditions using the HHT. We believe that z-less turbulence is one example to apply the HHT to the boundary layer meteorological studies and that is why we simply bring up this issue in our manuscript. We revised the text for better readability of our manuscript. Thank you.

3. Technical comments Page 9681, line 6: “interplay between the surface layer similarities” does not make sense.

> Reply: The text was corrected.

Page 9681, line 20: “only inflection points” needs more careful explanation.

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> Reply: The text was revised for clear meaning of this sentence.

Page 9681, line 24: The word “no band-limited signal” is not totally understandable.

> Reply: We revised the text for better readability of the manuscript.

Page 9682, item (1) “the local maximum and minimum”: Aren’t they local “maxima and minima” (plural) ? The authors also need to explain explicitly the exact definition of the word “local”, such as the width of the time window if they are meant to be local in time.

> Reply: This is a typo and the text was revised to incorporate the comments.

Page 9682, equation (2): If m1;k and h1;k in equation are timeseries, not constants, then they better be appended by “(t)” in order for clear presentation. The same can be applied to other equations and texts in this section.

> Reply: We incorporated the reviewer’s comment into the manuscript.

Page 9686, equation (10): It is not known why the authors presented this equation, as it does not followed or preceded by description about it.

> Reply: We showed this equation for readers in the scientific community are not familiar with the original form of the TKE budget equation. We revised the text for more discussion on this equation. Thank you.

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Interactive Comment

Interactive comment on Biogeosciences Discuss., 6, 9677, 2009.

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