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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.

Anonymous Referee #3

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1 General Comments

This paper presents the first application of Hilbert-Huang transform to turbulence data to validate the surface layer similarity for nocturnal turbulence. The topic is of interest for the readers of Biogeosciences. The authors intension is clearly stated and they also presented successful results in Figures 1 and 2. However, there are some misinterpretation of the results and the ambiguous or incorrect expressions that stems from the authors misunderstanding. The referee recommends that the paper should published after substantial revision.

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2 Specific comments

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 "$m_1(t)$" 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.

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?

Page 9686 line 6, line18; Page 9687, line 14 "up to z/L$\sim$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

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incorrect. This reviewer thinks that the use of z-less does not help the authors to validate the use of HHT in this text.

3. Technical comments

Page 9681, line 6: "interplay between the surface layer similarities" does not make sense.

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

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

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.

Page 9682, equation (2): If $m_{1,k}$ and $h_{1,k}$ 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.

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.

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