Atmos. Chem. Phys. Discuss., 11, C8855–C8857, 2011
www.atmos-chem-phys-discuss.net/11/C8855/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of coherent structures and atmosphere-canopy coupling strength during the CABINEX field campaign: implications for atmospheric chemistry" *by* A. L. Steiner et al.

Anonymous Referee #2

Received and published: 12 September 2011

The paper addresses the role of coherent structures on the momentum and heat flux exchange at a deciduous forest in the USA. The study also aims at addressing the implications to atmospheric chemistry, although this is not the focus of the analysis at any momment. This is a relevant issue, as has been shown by numerous studies on the subject in the past decades. However, the wide variety of results obtained by such studies suggests that a new study on the topic must provide new insights and possibly methodological improvements, rather than merely applying techniques used in the past to come up with a new estimate on how important the coherent structures

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

are. In fact, it would probably be fine to simply show that these structures are very relevant, if such was the case, but the present study does not achieve this goal. The "flux contribution" by the coherent structures is a valid statistics, but only if directed compared to the "time contribution" of such structures. This has, in fact, been done by Lu and Fitzjarrald, whose study is cited by the authors as a reference for the flux contribution mathematical expression. However, the authors did not take the next step, as Lu and Fitzjarrald did, of comparing how much percentual time the structures take. In other words, claiming that 60% is a relevant contribution to the total flux is only meaningful if the structures responsible for such contribution took significantly less than 60 % of the total time. Alternatively, and more simply, instead of comparing "flux contribution", the study could compare the mean flux within the coherent structures to the mean series flux. The result would be equivalent to comparing "flux contribution", while taking the "time contribution" also into account. A very simple exercise can be performed from data in table 1 of the manuscript. Multiplying the average number of structures in each class by their average duration, one comes up with the average time of the structures (percentage contribution in parenthesis):

atable, wavelet: 1052 s (58,4%) stable, Q-H: 540.2 s (30%) unstable, wavelet: 507 s (28,2%) unstable, Q-H: 309 s (17%)

Now comparing these values to the "flux contribution" reported, one finds that the wavelet analysis finds structures that, in stable conditions, contribute to smaller percentage of both momentum and heat fluxes than the time they last (or, alternatively, the mean flux within the structures, is less than the average series fluxes). In unstable conditions, on the other hand, the opposite occurs. A statistical analysis is, however, necessary to tell whether these are significant differences in both cases. For the Q-H analysis, on the other hand, in all cases, the "flux contribution" is larger than the "time contribution". I have, however, serious doubts whether this would not be a mere consequence of selecting the extreme fluctuations from each variable and, therefore, forcing the covariance to increase. In fact, the very small average duration of the coher-

ACPD

11, C8855–C8857, 2011

ent structures from the Q-H analysis indicates that it is selecting extreme cases, very fractured, that do not actually correspond to flow structures, but actually to localized extreme values. This fact is, actually, acknowledged by the author (page 21024, lines 18-20).

With that in mind, I suggest that this manuscript may only be accepted for publication in ACP if a much more careful analysis of flux contributions, that considers their time contributions, is performed, and if it proves to be significant in a relevant way. As it is, I do not believe they are. Besides, as it is, the study does not address the implications for atmospheric chemistry, as claimed in the title. If that is done (I am not sure how), then something truly new may come from the study. However, if the study is restrained to an analysis similar to the one performed here (but improved), I suggest removing the words "atmospheric chemistry" from the title.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21013, 2011.