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## ***Interactive comment on “Momentum and scalar transport within a vegetation canopy following atmospheric stability and seasonal canopy changes: the CHATS experiment” by S. Dupont and E. G. Patton***

**Anonymous Referee #2**

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Review of ‘Momentum and scalar transport within a vegetation canopy following atmospheric stability and seasonal canopy changes: The CHATS experiment’ by S. Dupont and E. Patton, published as ACPD-2012-44

**Summary:** This paper addresses an interesting and fundamental question in atmosphere-vegetation interaction namely how sensitive the flow and its turbulent momentum and scalar exchange are to vegetation structure and atmospheric stability. Despite significant progress from both observational and numerical studies over the past 4 decades with regard to identifying and separating different flow modes and their

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generating mechanisms, it still remains an active research question. To my knowledge, the CHATS experiment is unparalleled with regard to sensor density and number, and quality of the data recording and processing. The comparison of the leaf-on / leaf-off period offers a unique opportunity to separate the influence of stability from that of leaf area distribution. The more disappointing it is that this manuscript almost completely lacks new insights into the canopy flow dynamics, but primarily confirms results from previous studies reported in the literature that were conducted with significantly less experimental effort. The authors do not tap the potential of the experimental data set. In addition to lacking innovative and fresh ideas, analysis techniques, and novel results, I have large concerns with regard to: 1) The use of quadrant analysis as the main analysis tool, since individual structures representing different and often superimposed flow modes are dissected and time-scale specific analyses are impaired leading little or no insight into the physical generating mechanism and its relation to external forcings such as stability and canopy architecture; 2) The use of existing literature; the authors missed some significant contributions to canopy flow analyses, particularly with regard to the definition of canopy exchange regimes also based on flux contributions of sweeps and ejections to the sensible heat flux in a vertical profile (e.g. Thomas and Foken, BLM, 2007a); 3) The conclusions based on the interpretation of the profile quadrant and octant analyses. Many interpretations seems plausible, but are over-reaching since the authors don't present any analytical evidence; 4) The shear number of figures and wordy descriptions that offer little scientific merit; fewer, more pre-processed and condensed figures highlighting the most important results would help focus on the main findings; 5) The lack of any uncertainty or variability analysis poses a question mark on any discussion about seasonality and stability classes. Despite its weaknesses, the current draft makes two contributions worth noting: a) the space-time autocorrelations that are very detailed due to the large density and number of sensor deployed; b) the attempt to generalize flow regimes for different stability classes and associate them with generating mechanisms in Fig. 17 which are similar to Poggi et al (2004), although I do have concerns with regard to details. The language is often

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imprecise and casual, I found many contradictions between figures and descriptions in the text. Details on all general comments are given below. In summary, I recommend reconsidering a substantially revised, refined, and more focused manuscript for publication in ACP that addresses the concerns listed below.

Major comments: 1) Quadrant analysis: Despite its widespread use and utility in turbulence analysis, quadrant analysis has a major shortcoming that significantly diminishes its applicability in this study: individual turbulent structures representing different flow modes may be dissected into 4 (or 8) quadrants by simply using the temporal mean of a signal over an averaging period of arbitrary length which does not preserve the integrity of the structures. Recent canopy flow studies have shown that there are at least 6 flow modes worth differentiating between: 1) the small-scale stochastic turbulence represented building the inertial subrange, 2) Kelvin-Helmholz vortices shed by individual canopy elements such as tree trunks, branches, etc; 3) The mixing layer eddies whose vortices scale with the canopy depth, 4) The larger boundary layer eddies that scale with the depth of the atmospheric boundary layer, 5) submeso modes most pronounced in weak wind flows on scales of tens of meters to kilometers, and 6) wave modes of linear or non-linear nature. Some of these modes can be differentiated by time scale-specific analyses using wavelet decomposition or other orthogonal decomposition techniques; some exist predominantly in certain stability regimes. The quadrant analysis employed here does not differentiate between any of these flow modes, and recent research has shown that dynamic stability, here expressed as  $L^{-1}$ , is no sufficient criterion, and may not even be a necessary indicator of turbulence. Thomas and Foken (2007b) showed that quadrant analysis overestimates the flux contribution of coherent structures compared to time-scale specific analysis using wavelet analysis in combination with the triple decomposition. I suggest the authors include a comparison between their quadrant / octant analysis and a different, time-scale specific technique to demonstrate the robustness of their results. 2) Use of literature: In the past decade, much insight into canopy flows has been gained by innovative studies in flumes, field experiments, and numerical simulations which the

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authors do not include or are unaware of. One particular contribution is the definition of exchange regimes based on the penetration depth of coherent structures into the plant canopy using a detailed analysis of the magnitude and sign of the flux contribution of sweep and ejections to the sensible heat flux (Thomas and Foken 2007a). A similar analysis applied to the CHATS data may shed new light or confirm the speculations about vertical communication of scalars and momentum in canopy in this study, which are solely derived from the ensemble-averaged profiles. One major shortcoming in the discussion of flux contributions in the current manuscript is the normalization procedure expressed in Eqs. 3 and 4, which eliminates the sign of the flux and does not allow for estimating uncertainty. It is exactly the change in sign and magnitude of scalar flux contributions in sweep and ejection motions that is of critical importance to diagnose vertical mixing and coupling. Goekede et al. (2007) expanded on the exchange regimes and showed that dynamic stability ( $z L^{-1}$ ) and turbulence intensity ( $u_*$ ) scale to some degree with increasing vertical coupling in the canopy as identified by the exchange regimes, but that critical thresholds in either parameter don't exist. I therefore recommend dropping the current fixed boundaries of the 5 stability regimes (dynamic stability evaluated above the canopy may not be indicative of mixing in the canopy and sub-canopy layers anyway), but to employ some scheme to diagnose vertical mixing and penetration depth and use those to construct probability density functions of  $h L^{-1}$ . Ruppert et al. (2006) deployed a time-scale specific spectral correlation coefficient to evaluate scalar similarity, while Scanlon & Albertson (2001) used outliers in scalar-scalar quadrant analysis of CO<sub>2</sub> and water vapor to identify and separate scalar sinks and sources. Both approaches may be useful for the purpose of this study. 3) Overreaching conclusions: Much of the current draft contains speculations about the existence and merging of small plumes into larger thermal eddies to form ejections, or triggering of canopy waves and Kelvin-Helmholtz vortices by the unstably stratified sub-canopy at night. All this remains speculation as no analytical evidence is presented, the draft lacks time- and space-scale dependent and phase-angle flow analysis as mentioned before. Given the wealth of experimental data in CHATS

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and expertise of the authors, this could easily be accomplished and greatly benefit the merit of this contribution. I doubt that K-H modes in the stably stratified layer above the canopy can be triggered by thermal plumes rising from the statically unstably stratified sub-canopy since these generation of flow instabilities shedding these vortices require sufficiently strong flows and speed shear, which both are absent in the stable layer. We have performed any fog releases in stable conditions are never seen clear K-H modes.

4) Figures: I recommend significantly reducing the number of figures and subfigures to focus the reader's attention on the most important features. I don't see the utility of presenting profiles for all quadrants including the inward interactions for all stability regimes. As mentioned before, filtering using a meaningful diagnostic of vertical mixing and flow modes would be preferable and offer physical explanation. The figures that are contained in Dupont & Patton (AFM, 2012) need to be removed, this level of redundancy is unnecessary and offers little merit.

5) Uncertainty: The authors need to include some measure of variability into their discussions. Without estimating variability and by eliminating the magnitude of the flux contribution through normalization, the discussion of seasonal dependencies and differences across stabilities is statistically meaningless. In fact, analyzing variability in both space and time may make a valuable contribution to our understanding of canopy flows since the authors have such a high density of observations.

Detailed comments: a) Page 5, Line 12ff: Remove this section, as it doesn't belong into the introduction. b) Page 7, Line 4f: I would argue that the choice of 30 min as the perturbation length scale for sub-canopy fluxes for the free and forced convection and near-neutral regimes is too long. Did the authors perform spectral analysis to find adequate height-dependent time scales to define perturbations? c) Page 9, Line 8ff: I don't understand why the authors want to use different terminology for ejections and sweeps / upward and downward plumes for momentum and scalar flux. Ejections and sweeps are simply defined over their upward and downward, respectively, motions, and do not imply any sign or magnitude of the flux. Choosing a different terminology would make this study less compatible with terminology used in the existing literature, and

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frankly, I don't see the novelty or physical necessity of using plumes instead of ejections / sweeps for scalar exchange. d) Page 9, Eqs. 3 / 4: As mentioned, I believe that eliminating the sign and magnitude of the flux contribution masks important features of the flow and vertical exchange. e) Page 11, Section 2.4: It remains unclear to me how the authors compute a correlation coefficient from temporally discontinuous time series across the profile since the conditional sampling indicator selecting a specific quadrant is height specific. This leads to an unequal number of sampled events between levels, or do the authors use one level to as the reference and sample all other levels using the same indicator mask? This needs to be clarified. f) Page 11, Section 2.5: Remove this section and refer to the published manuscript in the text where appropriate. g) Page 14, Line 4f: 'With departures . . .' I don't understand this sentence. It seems like a contradiction to me. Maybe the authors can clarify? h) I was unable to reconcile features described in the text comparing to the figures. To give a few examples: Page 14, Lines 12-14; Page 14, Lines 17-23; Page 23, Lines 9-14; Page 24, Lines 26 – Page 25, Lines 2. Maybe the authors used different figures when writing the draft? i) Page 14, Lines 24 ff: This last paragraph should be saved for later and included in the conclusions. j) Page 15, Line 16 and thereafter: It is imprecise to say that ejections or sweeps 'transport'  $t_+$  or  $t_-$ . Heat perturbations are not a separate entity independently existing of the flow. Rather, use wording such as that ejections were correlated or inversely correlated with relatively warmer or cooler air. As mentioned before, a discussion not including uncertainty is irrelevant. k) Page 18, Line 20ff: I caution the authors using the number of events in a quadrant as an argument for significance of a specific motion for two reasons: First, fewer, stronger events can have a greater flux contribution than many weak events; second, the temporal mean used to define quadrants becomes somewhat arbitrary when the flow is non-Gaussian (which the rule rather than the exception of canopy flows). Events close to the origin are very sensitive to small errors in the mean resulting from non-Gaussian flow. Hence, many authors use hyperbolic criteria to decrease this uncertainty. l) Page 19, Line 2ff: As mentioned before, quadrant analysis does not allow for differentiation between time and space scales of motions. Hence, all

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of this interpretation remains pure speculation, although it seems plausible. m) Page 21, Section 4.3: The space-time correlations are very sensitive to the magnitude of the flux, so for the regimes representing the stability end members either the momentum or sensible heat flux become very small introducing a larger uncertainty into the momentum-scalar correlations. n) Page 24, section 5.2: Many of the described features cannot be found in the figures, particularly differences between the warm rising air and cool sinking motion, and seasonal dependencies. o) Page 25, Section 6: This is no discussion, but a summary of results. p) Page 26, Section 6.1: The differences in generating the large rising and sinking eddies for the free convective regime is obvious. Since downward eddies must originate in the deeper ABL flow and scale with its depth, they are large and efficient in transport heat. The upward eddies (ejections) originate from the radiatively active surfaces and probably are generated on the scale of the forest / walnut orchard. Can the authors present some estimates of spatial scales for these motions? q) Page 27, Line 24ff: It is incorrect to term water vapor a passive scalar, and sensible heat an active scalar. Recalling the equation for the potential virtual temperature, both quantities impact density and buoyancy. I recommend removing this argument and the associated sections as it promotes incorrect physics. r) Page 29, Section 6.3: No analysis of any wave modes has been presented, so any reference to the interaction between gravity waves and turbulence needs to be removed. s) Page 30, Section 6.4: Similarly to previous sections, the conclusion here are not supported by the analysis presented, so the authors need to either provide evidence or remove these speculations.

Additional references:

Goeckede, M, Thomas, C, Markkanen, T, Mauder, M, Ruppert, J and Foken, T, 2007. Sensitivity of Lagrangian Stochastic footprints to turbulence statistics. *Tellus Series B-Chemical and Physical Meteorology*, 59(3): 577-586. Scanlon, T.M. and Albertson, J.D., 2001. Turbulent transport of carbon dioxide and water vapor within a vegetation canopy during unstable conditions: Identification of episodes using wavelet analysis.

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