**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Multi-season eddy covariance observations of energy, water and carbon fluxes over a suburban area in Swindon, UK" by H. C. Ward et al.

Anonymous Referee #2

The paper presents results of the observation of energy balance components and carbon dioxide flux for the suburban area of UK town, Swindon. The measurement period covers whole 12 moths from May 2011 to April 2012. The sensible and latent heat fluxes and net CO2 exchange were calculated with the eddy covariance method which is regarded as the most proper way for turbulent fluxes estimation. The choice of measurement site and methodology makes a data set unique – only a few examples of the long term measurements of turbulent fluxes for suburban areas can be find in a literature. Measurements were made with the present "state of art" including all commonly procedures for flux calculations and data corrections. In general the data are analyzed and discussed in the comprehensive way. All these makes a manuscript worth for

publication in ACP. The paper is well organized and argumentation is in general clear (but as I am not a native speaker, I am not able to evaluate language correctness). The number of figures is adequate and their quality is good. Abstracts gives a good information about the text.

Thus my main conclusion is that the paper can be published after minor changes only.

However, there are some point which authors could considered before publications. The first is an objective hysteresis method (OHM). The authors use OHM for estimation of the stored heat term (ΔQS). The results of this parameterization are considered to be as good as measured terms of energy balance. But, it is only a parameterization! First of all the OHM should be tested for Swindon conditions. In Appendix one can find information that many heat fluxes were used to measure flux to the "ground". Why these data were not used to verify OHM? As ΔQS is used in next calculations (see my next comment to line 342) the proper estimation of ΔQS is very important. At the present stage I have an impression that OHM dose not works properly in winter time (see my next comment to lines 263-270). The estimations of aerodynamic resistance (ra) and surface conductance (gs) are significant for modeling practice. Moreover, there is a lack in the literature such estimations for suburban areas (I am not a specialist in this but they are first such empirical data I have ever seen). As they are unique and can be referred by many others the methodology must be very carefully examined. In estimations authors assumes that "Roughness lengths and displacement heights are assumed equal for momentum, heat and water vapour". In general it is not a case for a urban areas. So, I would expect more discussion on the inaccuracies introduced by this assumption. Because of the above remarks I think that it could be better to split the paper on two or even three separate papers: the first one about energy balance only (with detailed discussion of ΔQS and OHM validation), the second one about evaporation (with more comprehensive analysis of possible evaluation of ra and gs) and third about carbon dioxide flux. However, I would like to stress that it is just a suggestion for thinking, not a strong recommendation – the paper can be published in

the present form with minor corrections only.

Below they are some more specific comments:

line132 – what about z0 calculated from logarithmic profile in close to neutral stratification. Is it similar? Is it angular dependent?

Site description - Some information about source area for turbulent fluxes could be desirable.

line 184 – Information about subsequent quality control is weak– what kind of test for stationarity and well developed turbulence was used? I am little surprise that as much as 96% of QH are available for analysis. Usually during the rainfall sonic measurements of QH are not correctly calculated (as QE). Why there are differences in the number of good data for QE and FC (measured with the same sensor)? Do differences in percentage of good data for QH and QE mean that different data sets were used for calculation of the monthly statistics of these fluxes?

line 221 – Here authors start to discuss fluxes including $\triangle QS$, but there is no information how this flus was estimated. It should be specified here not later, in line 249.

lines 263-270 – In my opinion OHM does not work well in winter. Positive values of $\triangle QS$ mean that heat is stored and negative that it is released in urban slab. With some simplification we can presume that during the night released $\triangle QS$ is a sum of heat from the ground and some additional heat (probably anthropogenic). During the winter night this release ($\triangle QS$ estimated from OHM) is on the level 40 Wm-2 (Fig 5d). But measured QG at the same time is on the level of 10 Wm 2 only. So, the question is where does this energy come from (even if we add QF there is a lack of energy)? Similarly at Fig. 4 for Nov-Jan – where does energy for $\triangle QS$ (which is very strong in these months) come from? Numerically, a strong negative value is a simple consequence that a3=-27Wm-2 and other components are small. But a physical meaning of such strong negative $\triangle QS$ is confusing. As it is negative its means heat

release stronger than incomings. In my opinion RES seems to be more reasonable estimator of $\triangle QS$ in this case than OHM model.

line 342 – It should be pointed that $\triangle QS$ used here is a function of Q* (a OHM modeled value). So, problems with estimation $\triangle QS$ in winter affects also these results. Particularly, I don't think that OHM can be used in dynamical processes like "rapid evaporation" (line 361). Moreover, in such situation is very difficult to estimate "s" in Eq. 2 (which temperature is taken for that?). In further analysis (Fig. 9) it would be interesting to have the same analysis but with $\triangle QS$ estimated as RES instead OHM – especially for a wintertime.

line 414 – The authors assume roughness lengths to be equal for momentum, heat and water vapor. In fact for the urban areas differences could be a few orders. Some discussion on the influence of this on the accuracy of the results (ra, gs) is needed.

line 558 – I understand that boundary layer height influent on CO_2 concentration, but how does it influent on the flux of this gas? My first guess is that more narrow boundary layer should result in smaller flux due to higher concentration and lower gradients. This needs explanation.

line 654 – The sentence "The wind direction may have affected the total evaporation" is literally false. The wind direction dose not really influent on evaporation, but the evaporation measured by the system depends on the source area which is not homogenous around the site. Therefore accuracy of estimation of total evaporation can be affected by wind directions distribution but not evaporation as a physical process.

line 676 (and line 17) – I do not understand why "considerable vegetation fraction" explains negative QH in winter. In my opinion negative QH in winter is a combination of radiation cooling of the surface and advection of warm air (eg. from the ocean). Of course, in the city centers this cooling is lower and additional heat sources exists which makes surface warmer than air and QH positive. But, vegetation cover is not a reason (it could rather reduce cooling due to reduced longwave losses and therefore it

could make QH positive or less negative). I understand that authors would like to point reduced built-up area in comparison to city centers as a reason of negative QH.

line 912 – should be: "Los Angeles and Vancouver"

line 946 – should be: "Miami, Florida"

Lines 1005-1057 Figure captions – please observe subscripts and superscripts.

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