**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.

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Multi-season eddy covariance observations of energy, water and carbon fluxes over a suburban area in Swindon, UK

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To distinguish between the author comments and original reviewer comments, author comments follow 'AC' and reviewer comments follow 'RC1/2' for reviewer 1/2. Where references to figures (equations) are made, these are the same as in the discussion paper. Additional figures (equations) included here in response to reviewers are referred to as Figure R (Equation R).

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—- Response to Anonymous Referee 1 —-

AC: We thank the reviewer for their thorough read of the paper and helpful comments. We have added more methodological details as requested. Both reviewers raised concerns about testing for stationarity and well-developed turbulence. We have not applied the tests developed for different environments to this suburban data, but instead have examined the possible impact of this decision – in general exclusion of data failing these tests made little difference to the results. Overall we agree with the reviewer's comments and have implemented the suggestions made and provide discussion and details below.

RC1: General comments 1

RC1: The measurements are not described well enough considering that this is the first publication from this site. The EC measurements are described well but others not. Provide at least the following information. AC: The following points are answered individually.

RC1: What is the measurement height of the WXT?

AC: The WXT is located on the same mast as the EC instrumentation at a height of 10.5 m. These details have now been added to the text.

RC1: What are the locations of the auxiliary measurements relative to the EC? Mark the locations on Fig. 1 or describe them otherwise explicitly (net radiation measurements, weather station, rain gauge, soil measurements, heat flux plates, IR temperature sensors, wetness sensor).

AC: The auxiliary measurements discussed in this paper were made at the EC site, with the exception of the rainfall data for 1-8 May 2011 shown in Figure 3 (hatched bar). Those data were was measured in central Swindon, approximately 2 km south and 1 km east of the EC site. The co-ordinates of this rain gauge have been added to the figure caption. The EC site is situated in the garden of a residential property.

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The net radiometer and weather station are installed on the same mast as the EC instrumentation. The other instruments are situated close to the base of the mast within the garden: the rain gauge is a few metres from the base of the EC mast; soil temperature and soil moisture are measured at a depth of 0.03-0.05 m (a) in a flowerbed and (b) below grass lawn; the heat flux plate used in this paper (Figures 4 and 5) is located < 5 m from the base of the mast, in sandier soil. Data from the infrared temperature sensors are not used here. The wetness sensor is positioned on the soil surface of the flowerbed and thus represents a pervious surface below canopy. These details have been added to the paper where relevant for the data discussed (Section 3).

RC1: What is the field of view of the four-component radiometer?

AC: The field of view of the downward-facing sensors encompasses a range of surfaces including: gardens, roads, pavements, grass verges, hedges and small trees, bare soil, gravel, roofs of garages, small sheds and single-storey extensions and brick and painted walls. The radiometer source area is therefore comprised of a similar mixture of surfaces to the expected average source area of the turbulent fluxes – although of course the source area of the turbulent fluxes will vary with time whereas that of the radiometer is essentially fixed. According to the method of Schmid et al. (1991), 80% of the radiometer source area lies within a radius of 20 m from the mast, and 95% within 44 m. Details have been added to Section 3 of the paper.

RC1: How many heat flux plates were there? What type is the soil they were installed in? What is the representativeness of this type of soil relative to the land cover in the study area?

AC: Three heat flux plates were installed at the EC site, one in clay loam in the flowerbed, one in slightly sandier soil and another under broken concrete. A mixture of soil types are found across Swindon (mostly clayey loams with some sandy areas). The heat flux from the plate under the mixed sandier soil was used to give the approximate

size and behaviour of the ground heat flux in pervious areas of gardens. Details of this plate have been added to the paper. The other heat flux plates were not used in this study. This is one component of the overall net storage heat flux which comprises the heating and cooling of all components of the urban fabric including soils, vegetation, water bodies and the large thermal mass of buildings and other built materials.

RC1: How many IR temperature sensors were there? What sort of surfaces were they measuring? Are the data used in this study?

AC: There are three IR temperature sensors at the EC site, directed at vegetation; an area of gravel and broken concrete; and a concrete path surrounded by grass. These data are not used in this paper so reference to them has been removed.

RC1: What sort of a surface does the wetness sensor describe?

AC: See response above.

RC1: General comments 2

RC1: The methods for gaining different energy balance components and CO2 flux components are introduced in the "Results and discussion" section. This is very confusing and decreases the readability. Figures are referred to before the actual variables have been introduced. (For example, QG is introduced on p29157, l27, but Fig.4 (including QG) is referred to already on p29156, l7).

AC: Both reviewers highlighted this point. To improve the readability we have moved the methods earlier in the paper (to Section 3). The soil heat flux, QG, is now introduced in Section 3 where details of the instrumentation are provided. Information about the anthropogenic energy release and carbon emissions has also been added or moved to Section 3, as has the discussion on different methods for estimation of the storage heat flux.

RC1: General comments 3

RC1: There are 16 figures with altogether 41 subplots. This is an unusually high number. Please keep this in mind for future manuscripts. No actions are required here.

AC: No action required, but comment taken on board.

RC1: Specific comments

RC1: p29150,l21: Jarvi et al., 2012 has a more complete list of annual CO2 budgets than Helfter et al., 2011

AC: Reference to Jarvi et al. (2012) added and sentence rephrased in light of the additional sites mentioned.

RC1: p29151,l15-25: Make a clearer statement of the aim of this paper. What is the scientific question? And respond to the aim in the Conclusions. Currently there are many vague statements of the content of the paper: "investigating energy and water exchange", "discuss the climatology", "consider the trends and variability", "discussion of the energy partitioning, controls on evaporation and carbon balance", "influence of surface cover"

AC: In this paper, we address the following question: what is the role of temporal and spatial variability in surface cover on the energy, water and carbon exchange in suburban areas? Specifically, what are the controls on evaporation and the carbon balance, and to what extent are they modified by anthropogenic activities and urban land uses? Further, how can these controls be best parameterised in models – what is the range and behaviour of the surface conductance?

The investigation focuses on local scale (102-104 m) spatial variability and changes at half-hourly to seasonal timescales using eddy covariance measurements over a typical UK suburban area. Attention is given to the representativeness of the 12 month study period (Section 4) and the role of local scale variability of surface cover on flux measurements is quantified (Section 5.4). The variation of observed fluxes with temporal changes in surface cover characteristics is examined and related to the physical pro-

cesses which govern the exchanges between surface and atmosphere (Section 5), in particular the influences of water availability, vegetation and anthropogenic activities. The observations are compared to simple models: potential evaporation rates, heat storage using the OHM parameterisation, anthropogenic heat and carbon fluxes from statistical inventories. Estimation of the surface conductance under different conditions offers empirical data for comparison with and development of models. Where possible, quantitative comparisons have been made with other studies to corroborate or widen the uncertainty on representative empirical values.

The paper has been amended to more clearly outline these scientific aims.

RC1: p29152: Could you give the population density for the area?

AC: The population density is approximately 4700 inhabitants km-2 (Office for National Statistics, 2010). This has been added towards the end of the fourth paragraph in Section 2.

RC1: p29153,l9: Give the spatial resolution of the land cover classification you have made.

AC: The spatial resolution of the geodatabase and lidar data is 1 m and this has now been added to the text.

RC1: p29154,l13: What are "soil measurements"?

AC: Now reworded also incorporating suggestions from point G1.

RC1: p29154,l14: Give the type (model number) of the Apogee Instruments IR sensor.

AC: The IR sensors are not used in this study so they are no longer mentioned in the text.

RC1: p29156,l1-4: What about flux stationarity or friction velocity screening? These are the most common variables used for flux quality screening over vegetative surfaces. Why haven't you used them?

AC: Friction velocity screening – Empirical thresholds or quality control criteria developed for natural environments may not be applicable for urban areas, or in some cases may not even be required (Crawford et al., 2011). Even over natural surfaces there is no consensus of a suitable threshold or method to use for friction velocity screening (Gu et al., 2005). In contrast to vegetated surfaces, additional energy release from anthropogenic sources or storage, coupled with increased surface roughness of the urban environment means that unstable or neutral conditions tend to prevail over stable stratification (Christen and Vogt, 2004). In Swindon, neutral conditions were most common. Friction velocities below 0.1 m s-1 were observed for less than 4% of data, so the impact on the results is likely to be very small.

Flux stationarity - There are a variety of possible tests to check for stationarity. Foken and Wichura (1996) recommend that the difference between the covariance calculated over the averaging interval (30 min) and the mean of the covariances over shorter averaging intervals (e.g. 5 min) is less than 30% of the 30 min covariance. Comparison of the measured integral turbulence characteristics (ITC) to similarity theory predictions is often used to test for well-developed turbulence (Foken and Wichura, 1996). However these similarity functions are generally derived over non-urban surfaces and may not appropriately describe the behaviour over urban areas (Fortuniak et al., 2013). We therefore decided not to implement these quality checks, but have explored the potential impact of this decision on the experimental findings. The EddyPro software (LI-COR) used to process the fluxes offers the option to provide quality control flags after Mauder and Foken (2004), based on the combined results of the Foken and Wichura (1996) steady state and ITC tests and a 0-1-2 flagging system where 0 is best quality and 2 worst quality data. Restricting the Swindon dataset by rejecting data assigned quality flags 1 or 2 did not significantly change the conclusions. As expected, most of the data failing these tests are close to zero so that the size of mean and median values of the fluxes increase slightly, more so at night than during the day. The monthly mean QH and QE values (Figure 4) are 0-3 W m-2 larger in magnitude using only the 'best' quality data (flag 0) according to EddyPro. The biggest difference due to these quality

controls is seen in the friction velocity (momentum flux): the distribution is shifter to larger values (mean increases by 0.03 m s-1), particularly at night (mean increases by 0.05 m s-1). A short summary of the impacts of not applying these tests have been added to Section 3.

RC1: p29156,l28: It is stated that 59% of the time the wind is from southwest. Is this percentage when considering 8 wind direction classes (45deg windows)? Note that this is slightly confusing because the figures have 30deg windows.

AC: The percentages refer to the NE, SE, SW and NW quadrants, i.e. 59% of the time the wind direction falls within 180-270° (the 180-210°, 210-240° or 240-270° bins). The text has been amended to include the word "quadrant".

RC1: p29156,l14-19: calculation method of QF

AC: This section has been moved earlier, to Section 3 (see response to G2).

RC1: p29157,l14-24: calculation method of $\triangle$QS

AC: This section has been moved earlier, to Section 3 (see response to G2).

RC1: p29157,l21: What is the closure for this site, in per cents? I know it is tricky to calculate the closure for a suburban environment since QF and $\triangle$QS have been modeled. You have all data so why not report the number?

AC: The energy balance closure varies seasonally and with time of day. The ratio of the outgoing energy (QH + QE + $\triangle$QS) to the available energy (Q* + QF) is close to 100% during summer (95-100%). In winter the average closure is around 120%, due to the overestimation of the release of stored heat by OHM (see below).

RC1: p29158,l7: Could the discrepancy between RES and $\triangle$QS be due to different source areas?

AC: The different source areas of the radiometer, turbulent heat fluxes and the estimate of storage and anthropogenic heating based on land cover could be a factor contribut-

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ing to the discrepancy between RES and ΔQS and departures from energy balance closure. However there are likely to be more significant issues with RES and ΔQS than differences in source area, which are now discussed in more detail in the paper (see response to Reviewer 2).

RC1: p29158,l9: Do you mean systematic or random errors with "uncertainty"? Random errors could not explain the systematic difference that is discussed in the text.

AC: This sentence has been deleted and the systematic difference explored more fully in terms of RES and ΔQS estimated using OHM.

RC1: p29158,l.20: The night time QE values seem to be below the commonly reported detection limit of EC measurements (about 5 W/m2). Also, it is hard to imagine that these small fluxes could be stationary and not intermittent. Please discuss or quantify.

AC: It is assumed that the random uncertainty associated with the instrumental detection limit averages out in the mean and median values of a large number of samples. Rejecting data that failed the stationarity and well-developed turbulence checks for non-urban surfaces resulted in similar values: mean 8.4 W m-2 (c.f. 5.7 W m-2) and median 6.4 W m-2 (c.f. 3.4 W m-2) for the 'best' quality data (flag 0); mean 7.2 W m-2 and median 5.2 W m-2 for flag 1 denoting suitability for inclusion in long-term observations. The slight increase for the restricted datasets is due to exclusion of a greater proportion of very small fluxes. We point out the large uncertainty associated with these small night time values at the end of the paragraph.

RC1: p29158,l25-26: Beyrich et al. (2006) refers to Mauder et al. (2006) for the uncertainty analysis. Please refer to the original source. (also on 29162,l20)

AC: These have been corrected in the text.

RC1: p29160,l17-24: calculation method of QEq

AC: The methods for calculation of QF and ΔQS have been moved earlier in the paper, as they both require some explanation and reference to the appendices. For clarity we

decided to leave the calculation of equilibrium evaporation and surface resistance next to the relevant discussions to aid the reader in interpreting the results.

RC1: p29161,l26: Where does the 5% come from? Give a reference.

AC: Reference has been added (Blonquist Jr et al., 2009).

RC1: p29168,l1: How many days with snow cover were there?

AC: Of the two snowy periods in February 2012, the first lasted just one day (5 Feb) and the second lasted 3 days (10-12 Feb). These dates have been added to the text.

RC1: p29168,l8: What does the boundary layer height have to do with CO2 emissions?

AC: The reference to boundary layer height has been removed from here as its influence on the measured CO2 flux is mentioned at the end of Pg 29168. Further explanation has been added on Pg 29168, also in response to Reviewer 2.

RC1: p29717,l5: Were the Fc data gap filled in order to get 1,6 kt C km-2 y-1? The data coverage was said to be 73%. Please use SI units: kg C m-2 yr-1.

AC: The value 1.6 kt C km-2 y-1 (kg C m-2 y-1) was calculated by summing each of the twelve monthly mean diurnal cycles over the day and multiplying by the number of days in the month, then totalling these to obtain an estimate for the year. Adding up all the available data and dividing by the data coverage also gives 1.6 kt C km-2 y-1 (kg C m-2 y-1). There was no particular bias in the availability of FC by time of day. The effect of the variable source area on the EC measurements is discussed in the text in Section 5.4. Units have been changed to kg C m-2 y-1 throughout.

RC1: 29166,l15: The effect of surface heating on LI7500 analyzers should be discussed somewhere in the chapter on Fc. This is a known problem for CO2 fluxes but is not that important for QE (Grelle, Burba 2007, Burba et al. 2008).

AC: The 'Burba correction' can be an important correction to apply to open-path gas analysers to account for underestimation of the measured CO2 flux due to the heating

of the instrument itself. The impact of instrument heating is most important for cold climates, particularly when air temperatures are low and solar radiation loading high. The climate in the south of the UK is relatively mild and, particularly during the study period, there are few clear-sky days so radiative heating and cooling of the instrument is somewhat limited. Whilst the ambient conditions help to minimise the impact of instrument heating in this setting, a small underestimation of the CO_2 flux is likely. Regression of the air-surface temperature difference with meteorological variables (Method 4 of Burba et al. (2008)) suggests the correction is of the order of a few percent at Swindon. However, the uncertainty in applying this correction is large compared to the size of the correction itself. The regressions applied were derived for different sites, for vertically orientated sensors, and the performance is described as 'fair' as opposed to 'best' or 'good' (Burba et al., 2008). Järvi et al. (2009) test correction methods in an urban setting Helsinki and recommend a site-specific approach. To attempt an improved correction would require more instrumentation and detailed study – ideally a closed-path gas analyser for comparison. However, given the mild UK climate we assume that the effect on the fluxes is small and do not implement a correction. The following sentence has been added to the Section 3: "No adjustment was made to account for instrument surface heating of the open-path IRGA, however, the relatively mild UK climate means this effect is not expected to be significant (Thomas et al., 2011)."

RC1: p29171,l11-12: Give references for Melbourne, Helsinki and Montreal (2, 3 and 3 sites according to URBANFLUX website, respectively)

AC: The figures correspond to the sites at Preston, Melbourne (Coutts et al., 2007), the SMEAR III station in Helsinki (Järvi et al., 2012) and at Pierrefonds-Roxboro, Montreal (Bergeron and Strachan, 2011). These references have been added to the text.

RC1: p29172,l13: Is "active vegetation index" mentioned in the manuscript before the conclusions?

AC: This sentence has been reworded slightly and the context of the Swindon results

made clearer.

RC1: Technical corrections

RC1: p29156,l14: It is generally not a good habit to start sentences with numbers or variables. It decreases readability. Perhaps replace "2011-2" by "Years 2011-2"

AC: "2011-2" has been replaced with "The period May 2011 to April 2012".

RC1: p29161,l26: typo:"(ÂaËŻ5 %)"

AC: Now correct. Sorry this appears to have occurred in the typesetting – text checked and will correct at proof stage if necessary.

RC1: p29163,eq5: make larger brackets around s/$\gamma^*\beta$- 1

AC: Larger brackets used.

RC1: p29167,l6-7: The following sentence does not read well, please revise. "The response to increasing PAR is also less."

AC: Sentence rephrased as "The Swindon data also show a weaker response to increasing PAR."

RC1: p29168,l21: typo: "combusution"

AC: This has now been corrected.

RC1: p29169,l21: The following sentence does not read well, please revise. "To the north of the mast is most vegetated"

AC: Sentence has been rephrased as, "The highest vegetation fraction is found to the north of the mast, particularly directly northeast where there are mature trees and lush gardens."

RC1: p29180,l21: Gwilliam et al. is in the middle of publications by Grimmond et al.

AC: Now correct. Sorry this seems to have moved between versions of the ACPD

proofs - we will check again at proof stage.

RC1: Fig.7a: Write in the caption that the colored dots are 30min data.

AC: "(30 min)" added to figure caption.

RC1: Fig.11: Make the lines thicker or the patches transparent. It is currently very hard to see the lines. AC: Figure redrawn with the patches made transparent.

RC1: Fig.14: "In winter Fc is well explained by human activity; in summer photosynthesis dominates." This text does not belong to a caption since it is more like results and discussion.

AC: This text has been deleted from the figure caption.

—- Response to Anonymous Referee 2 —-

AC: We are grateful to the reviewer for their helpful suggestions and discussion of the work presented. The main issues raised were the estimation of the storage heat flux, $\triangle QS$, particularly with reference to its use in the further analysis of equilibrium evaporation. The paper now explores the performance and limitations of storage heat flux estimation via OHM and RES in more detail. Further discussion has been added to Appendix B to support the suggestion that the coefficient a3 in the OHM parameterisation may be too large outside summer months. Both reviewers requested information on the quality of fluxes with regard to stationarity and well-developed turbulence. These issues are addressed in detail below (see also the response to Reviewer 1) and additional information has been added to the paper where beneficial. We wish to provide some initial information useful for modelling in this paper but it is intended that the discussion on conductances will be developed further as part of a separate study. We therefore decided to leave the analysis of the 'observed' values here as one paper but have provided more information and a better assessment of the limitations and uncertainties. It was not our intention to assume equal roughness lengths for momentum and water vapour – this has now been corrected and is discussed here. We have

implemented the suggested changes and provide a detailed response to each point below.

RC2: line132 – what about z0 calculated from logarithmic profile in close to neutral stratification. Is it similar? Is it angular dependent?

AC: The value of z0 given is based on the rule-of-thumb z0 = 0.1zH (Garratt, 1992), where zH is the average height of the roughness elements and has been calculated from the land cover map and lidar data for a 500 m radius around the flux mast. If instead z0 is estimated from the relation between wind speed and friction velocity, variability is seen with wind direction as at other sites (Grimmond et al., 1998; Pawlak et al., 2010; Nordbo et al., 2013). For Swindon, the range of anemometric z0 is about 0.25–2.00 m, with the larger values attributed to local effects of nearby buildings. Obtaining a value for the roughness length in this way requires the displacement height and the value of 3.5 m (0.7zH) was assumed here for all wind directions. In reality, the surrounding morphology will mean there is some variability in the displacement height as well as z0, with the two closely linked (e.g. Grimmond and Oke (1999)).

The anemometric z0 also exhibits some seasonal variation (greater roughness when there are leaves on the trees) and a dependence on atmospheric stability may also be expected (Zilitinkevich et al., 2008). To attempt to derive a precise roughness length (or displacement height) for each measurement interval is beyond the scope of this paper. Instead we opt for a simple rule-of-thumb approach based on the available information and prefer to analyse the resulting information keeping in mind the uncertainties on these input parameters.

For z0m of 0.25-2.0 m, the largest impact is on the aerodynamic resistances (the median value differs by 5 s m-1, or about 15%), whereas the surface conductances are less affected by the roughness length used (the median value differs by 0.02 mm s-1, which is less than 1%). The 'true' value of the roughness length representative of the suburban surface will realistically lie within a smaller range, e.g. 0.3-1.0 m, based

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on values in the literature (e.g. see Grimmond and Oke (1999)). A paragraph has been added to Section 2 summarising the above and providing more information on the source area (see response to the next point). The following sentence has also been added to Section 5.2: "Uncertainty in the roughness length has a sizable effect on ra but the impact on gs is small: increasing z0m by 0.5 m increases the median value of ra by 1.8 s m-1 and the average difference is 6%; the median value of gs is unchanged and the average difference is 2%."

RC2: Site description - Some information about source area for turbulent fluxes could be desirable.

AC: Based on the analytical footprint model of Hsieh et al. (2000), the peak of the footprint function from the EC mast ranges from about 12 m under strongly unstable conditions (ζ = -1) to 350 m under strongly stable conditions (ζ = 10) taking z0 = 0.5 m, zd = 3.5 m. For the majority of the data ($|\zeta|$ < 0.1, near-neutral conditions), the footprint model suggests that the probable peak contribution lies between about 30 and 80 m from the mast, with 50% of the contribution to the total measured flux from within 250 m and 80% within 700 m. As with all footprint analysis, more stable conditions give rise to larger source areas and more unstable conditions cause the probable source area to move closer to the mast. Over rougher surfaces the footprint moves closer to the mast: using an input value of z0 = 1.0 m would suggest the probable peak contribution lies between about 25 and 45 m from the mast, with 50% of the contribution to the total measured flux from within 135 m and 80% within 410 m (for $|\zeta|$ < 0.1). The land cover fractions (Figure 2) will vary for individual time periods (i.e. flux footprints) even within the same wind sector. However, there are clear differences between the higher vegetation fraction (to the northeast) and more built-up areas (to the southwest) in the land cover maps, wind sector analysis and flux measurements (see Figure 1).

To provide more information, the following text has been added to Section 2: "The footprint model of Hsieh et al. (2000) was used to determine the probable source area of the turbulent fluxes. During stable conditions the measurement footprint can extend

over many hundreds of metres; under unstable conditions it is much closer to the mast. For the majority (89%) of the data ($\zeta < 0.1$, i.e. unstable to just-stable conditions) the peak contribution to the measured flux is predicted to come from within 100 m of the mast and 80% of the source area lies within 700 m (using z0 = 0.5 m and zd = 3.5 m). Although the land cover fractions will vary for different flux footprints, even within the same wind sector, there are clear differences between the higher vegetation fraction (to the northeast) and more built-up areas (to the southwest)."

RC2: line 184 – Information about subsequent quality control is weak– what kind of test for stationarity and well developed turbulence was used?

AC: Please see the response to Reviewer 1 above.

RC2: 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).

AC: Although the sensible heat flux from the sonic can be affected by heavy rain, moderate rain rates are usually unproblematic. Values were removed when a threshold check on the standard deviation of the temperature measured by the sonic (σT > 0.9 K) suggested poor data quality. Although rainfall in Swindon was frequent, heavy downpours were relatively uncommon during the study period.

RC2: Why there are differences in the number of good data for QE and FC (measured with the same sensor)?

AC: For both fluxes, data failing physically reasonable threshold checks were excluded – slightly more FC failed these checks than QE (19 data points compared to 9). At the post-processing stage there were also slightly fewer 30-min FC data available than QE, which could result from differences in the amount of raw data passing initial quality control (e.g. despiking).

RC2: 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?

AC: Yes, the monthly statistics were calculated using all available data. This is assumed to be unproblematic as there are no major biases in the availability of data e.g. by time of day, and recalculation of the statistics using only periods when concurrent data are available does not significantly alter the trends observed. It is a known limitation that a large proportion of the IRGA data must be removed during and just after rainfall, hence open-path QE datasets inevitably under-represent these times. This is discussed in the paper (Section 5). Restricting other energy fluxes to times when the QE is available could introduce a sampling bias into the other energy fluxes as well (favouring non-wet conditions). We therefore decided against that approach, but illustrate here, and now also in the paper, the impact on Figure 4 (Figure R 1 below). Since availability of QE is the limiting factor this flux does not change, but average Q* and QH both increase if the statistics are calculated for times when all energy fluxes are available. Data availability had the biggest impact in Apr 2012, when frequent rainfall significantly reduced the number of QE data points. However, the general trends are unchanged.

RC2: line 221 – Here authors start to discuss fluxes including ΔQS , but there is no information how this flus was estimated. It should be specified here not later, in line 249.

AC: This has been moved earlier (to Section 3).

AC: On the subject of ΔQS, we recap the general points made by the reviewer (Page C12099 of interactive comments from Anonymous Referee 2):

RC2: 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).

RC2: lines 263-270 – In my opinion OHM does not work well in winter. Positive values of ΔQS 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 ΔQS is a sum of heat from the ground and some additional heat (probably anthropogenic). During the winter night this release (ΔQS 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 ΔQS (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 ΔQS is confusing. As it is negative its means heat release stronger than incomings. In my opinion RES seems to be more reasonable estimator of ΔQS in this case than OHM model.

AC: As suggested by the reviewer, the coefficient a3 may be less appropriate for wintertime than for the summer months. A wintertime bias in storage models based on Q* has been suggested by Best and Grimmond (2013). To date there have been few studies which enable the OHM coefficients for the wide variety of materials (and surface conditions) to be determined – these are clearly needed. In Appendix B, analysis based on comparison with the soil heat flux plate has been developed. Coefficients derived using Eq. B1 fitted to QG imply seasonal behaviour that is similar to the findings of Anandakumar (1999). Notably the constant term (a3) is found to be much smaller in winter.

Given the difficulty of obtaining an accurate estimate of the net storage heat flux in urban areas generally, we used OHM to calculate ΔQS and make comparisons with the residual term and the soil heat flux in Section 5.1. The performance of OHM is now discussed in more detail in the text. The storage term, as calculated using OHM, is presented in Figures 4 and 5 then used to estimate the equilibrium evaporation and

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aridity parameter (Figures 8 and 9) and the potential evaporation (Figure 11). We discuss the OHM and RES approaches further in response to the following points and have added the main points to the paper.

RC2: line 342 – It should be pointed that ΔQS used here is a function of Q* (a OHM modelled value). So, problems with estimation ΔQS 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?).

AC: This sentence has been added to the text: "Note that since the ΔQS term is calculated here using OHM, it is also a function of Q*; if instead the residual is used then the bracketed term reduces to the sum of the turbulent fluxes (QH + QE)." The slope of the saturation vapour pressure-temperature curve was calculated using the air temperature measured by the weather station at the same location as the EC mast (Section 3).

The main result from Figure 8 is the decreasing value of the aridity parameter with increasing time elapsed since rainfall, demonstrating the response of the surface to the availability of moisture. If RES is used for ΔQS instead of OHM, the slopes increase fairly uniformly by about 0.12 across the range of conditions, so that the decrease the aridity parameter is still observed. Whilst both methods have limitations, both clearly show the same behaviour over a range of surface conditions. As a simple sensitivity test, we adjusted ΔQS by $\pm$ 10% which resulted in a spread of ± 0.03 in αPT. Adjusting ΔQS by + (-) 50% returned αPT ranging from 0.74 to 0.23 (1.00 to 0.35) as the surface dried out (c.f. 0.88 to 0.28, Figure 8).

The following sentence has been added to the text: "Despite the uncertainties in the energy balance terms, in particular ΔQS, the observed trend remains when the available energy is increased or decreased (adjusting ΔQS by $\pm$ 10% spreads the αPT values by ± 0.03)."

RC2: In further analysis (Fig. 9) it would be interesting to have the same analysis but with △QS estimated as RES instead OHM – especially for a wintertime.

AC: The aridity parameter calculated using both methods to estimate the storage heat flux (Figure R 2) illustrates the issues with both methods during winter. As would be expected from Figures 4 and 5, agreement is good in the summer but neither method performs well in winter. That the daily totals of RES remain positive all year round (Figure 4) indicates that this quantity must be larger than the 'true' storage heat flux (the urban surface cannot indefinitely gain energy without heating up) so the resulting QEq must be too small (the 'true' energy available is underestimated) which ends up as unrealistically large αPT estimates during winter daytimes. At night, since QE mostly remains positive, αPT < 0 because the sum of the turbulent heat fluxes is negative (QH is negative and larger in magnitude than QE). OHM predicts that the surface releases a considerable amount of energy at night and through most of the winter daytimes ($\sim$40 W m-2), described by the large constant coefficient (a3). Physically, this energy must go somewhere; according to the residual term a much smaller release is required to account for the outgoing energy, which again suggests that a3 is not applicable in this case (as discussed above). In terms of the aridity parameter, this overestimation of the release of energy from storage by OHM means the available energy is overestimated and therefore the resulting αPT are too small. Of course the coefficients a1 and a2 will likely show some seasonal dependence as well (Anandakumar, 1999). Comparison with the behaviour of the soil heat flux (Figure 5d and e) and the results in Figure R 2 suggest that daytime △QS may still be better represented by OHM than by RES even though the performance of OHM is worse in winter than summer.

The issues evident here make a strong case for further research into this area and we intend to develop the analyses in a future publication.

RC2: 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.

AC: We appreciate the reviewer questioning this statement as it was a mistake and not our intention to assume equal roughness lengths for momentum, heat and water vapour. The roughness lengths for heat and water vapour are assumed to be equal but different to that for momentum. Following Järvi et al. (2011), we estimate the roughness length for heat/water vapour using the formula suggested by Brutsaert (1982),

(Eq R1): $z_{0v} = z_{0m} \exp[2-a(u^* z_{0m}/\nu)^{0.25}]$

with the surface-dependent coefficient calculated using the parameterisation suggested by Kawai et al. (2009) for vegetated cities,

(Eq R2): $a = (1.2 - 0.9 \, lambdaV^{0.29})$

where ν is the molecular diffusivity of air and other notation is defined in the discussion paper. For the vegetation fraction of the study site (44%) this results in an average value of z_{0v}/z_{0m} of 0.05 (mean) or 0.03 (median). The description and results have been updated in the paper accordingly.

Although the estimated z_{0v} values are significantly smaller than z_{0m}, the impact on the surface conductance is reduced and the updated g_s values only differ by a few mm s-1 compared to the previous estimates, which equates to improved g_s values 10-20% larger in summer and smaller in winter. The effect on r_a is greater: the updated r_a values are around 20 s m-1 larger than before. Note that this adjustment of z_{0v} means the results of Figures 9-11 have been modified, however this alteration does not dramatically alter the conclusions.

RC2: line 558 – I understand that boundary layer height influent on CO2 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.

AC: The following explanation has now been added to the text: "At night the shallower

boundary layer restricts the dispersion of CO2, causing an increase in concentration. In the morning, as the boundary layer grows again with the increase in vertical mixing, an enhanced flux may be observed as CO2 is transported away from the surface (Reid and Steyn, 1997)."

RC2: 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.

AC: The language has been reworded more carefully – it now reads "the total evaporation measured by the EC system will be affected by the wind direction distribution".

RC2: 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.

AC: This sentence has been reworded and now reads, "In winter there are negative QH values (Fig. 5) that can be explained by the suburban nature of the site with relatively low building density, small QF and relatively small ΔQS terms compared to sites that are more built up – and more frequently studied."

Increased vegetation cover (as opposed to built or impervious surfaces) also usually means greater availability of water, more negative QH can result from increased QE at the expense of QH with limited energy available (Christen and Vogt, 2004).

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RC2: line 912 – should be: "Los Angeles and Vancouver"

AC: Checked.

RC2: line 946 – should be: "Miami, Florida"

AC: Checked.

RC2: Lines 1005-1057 Figure captions – please observe subscripts and superscripts.

AC: Checked.

—- Other modifications —-

AC: While responding to the reviewers comments we have also made the following small changes to the paper.

AC: The reference to the full paper of (Järvi et al., 2012) is now made (updated from the discussion paper).

AC: Since the description of the storage and anthropogenic heat flux has been moved to the methods section (Section 3), this has been renamed "Instrumental setup, data collection and data processing".

AC: The following sentences have been added to Section 5.2, "Although not shown here, the remainder of 2012 was very wet and soil moisture remained high in contrast to 2011. As a result, observed Bowen ratios were lower during summer 2012 than 2011 (closer to 1 than 1.5)."

AC: A small error was discovered in the averaging of the meteorological data from 1 min observations to 30 min. This has now been corrected. The flux data have now been reprocessed with a correctly specified input height for the IRGA (according to the sign convention used by EddyPro). The impact of these corrections on the datasets and the results is negligible and it has made no difference to the conclusions. The figures have been redrawn and some values given in the text have been updated since

the discussion paper, but again, none of these changes were significant.

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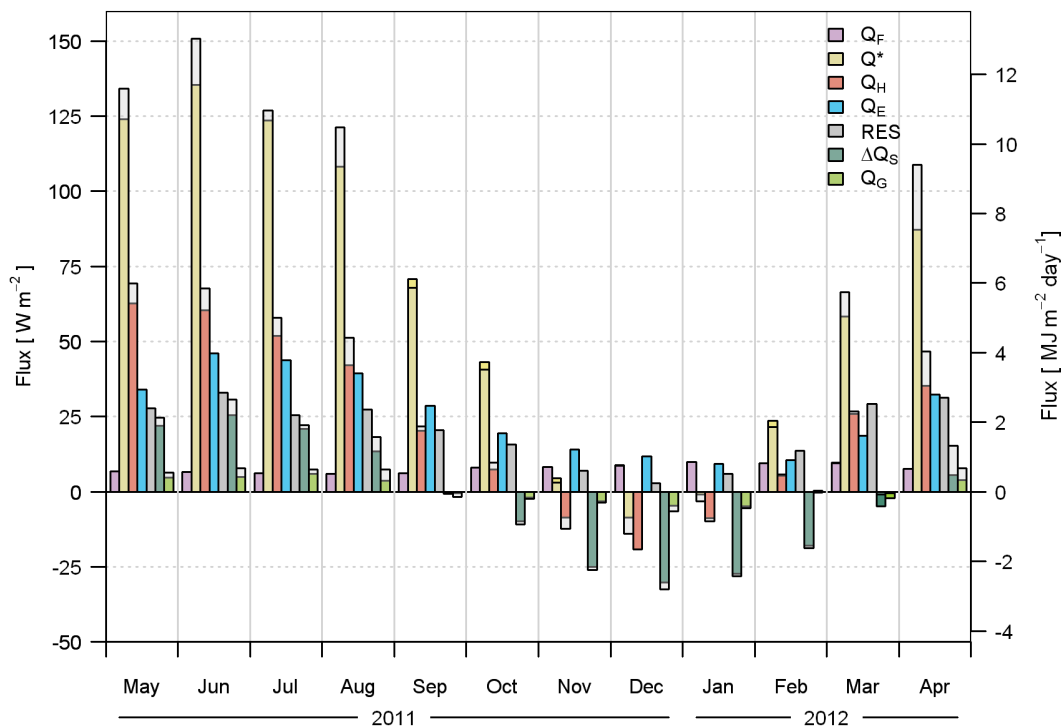Interactive comment on Atmos. Chem. Phys. Discuss., 12, 29147, 2012.

Fig. 1. Monthly mean fluxes for all available data (average of 24 h median values for each month), as for Figure 4 (colours), and for all concurrent data (unfilled bars). (Note the y-axis has been adjusted.)
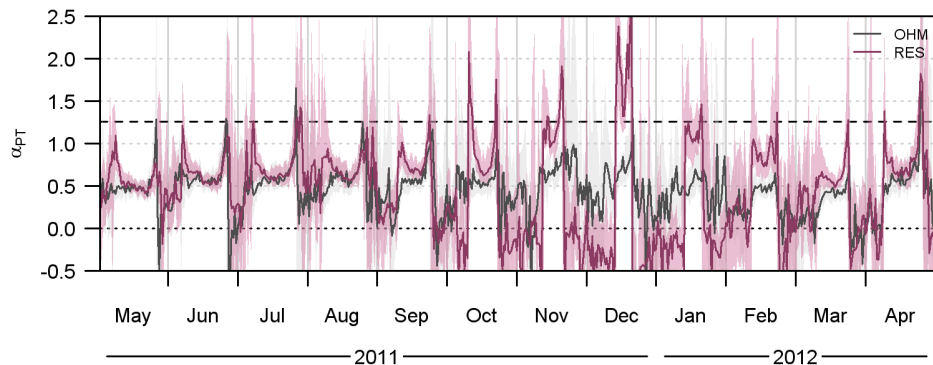
Fig. 2. Monthly median diurnal cycles (lines) and inter-quartile ranges (shading) of the aridity parameter, αPT, calculated using OHM or RES as the storage heat flux.