Response to Referees' Comments

We thank the referee for their comprehensive and constructive comments on our manuscript. Below, we address each specific point in turn:

L57. I would name this chapter as 2. Materials and Methods. And then having subchapters 2.1 Measurement location; 2.2. Experimental setup; 2.3 Flux calculation.2.4. Data selection; etc.

A 'Materials and Methods' chapter has been made as suggested, with subheadings 'Measurement location', 'Experimental set-up', 'Pre-flux processing', 'Data selection' and 'Flux uncertainty'.

L104-105. There is something missing at the end of the sentence. Please check it.

Sentence completed – corrections not needed 'for determining an accurate ozone mixing ratio'.

L114-127. Why to use such large windows for searching the lag, e.g. from 0 to 10sec as shown in Fig.4? I would use a very narrow window (e.g. 4±1) sec in order to reduce the scatter.

The large lag window is somewhat arbitrary, but if a clear peak cannot be identified above the noise in such a large timespan, then limiting the window to, say, 2-6 seconds will just result in a random peak in the noise being chosen (closer to the 'true' lag). We would prefer to set the lag time to a good estimate in such cases where no clear peak is observed, hence the large bounds. Note this is now Figure 3.

L130. A minus sign is missing from Eq.1.

Minus sign added, and rearranged putting flux on the left hand side.

L140 - Please report the percentage of excluded data for each criteria, and also the total percentage of data left.

Table 1 amended for each row to show only the effect of each filter on the total data set. A row at the bottom has then been added showing the combined effect of all filters on the data (with percentages provided in all cases).

L156-157. Why? I do not understand this point. Footprint doesn't depend on windspeed, rather on stability. I would be interested to see footprint estimates for different stability classes. Did the authors use the estimated roughness length for the footprint calculation?

The roughness length used in footprint determination was calculated by Eqs. (12-15). Depending on the chosen scaling approach, footprint parameterizations are expressed as a combination of variables that interrelate the strengths of horizontal and vertical transport processes. These can include atmospheric stability (but don't have to, e.g,. Kljun et al., 2004) and horizontal wind speed (e.g., Kormann and Meixner, 2001). Subject to similar solar forcing and owing to the differing albedo and heat capacity of the water surface, the diurnal cycle of stability is less pronounced in the coastal boundary layer compared to the boundary layer over land. On the other hand, due to a similar horizontal pressure gradient and differing roughness, the horizontal wind speed over the water surface is greater compared to over the land surface. We chose horizontal wind speed over atmospheric stability to provide the more selective discriminator for water and land surfaces, and classifier for footprint extent. Figures 4 and 5 demonstrate the robustness of this approach.

A clearer list of all variables used in the footprint calculation has also been added. While we recognise that stability and roughness length are used in footprint calculation, we would contest the statement that a footprint doesn't depend on windspeed, given that roughness and dimensionless stability are intrinsically linked to wind speed (owing to the use of friction velocity in their calculation).

References:
Kljun, N., Calanca, P., Rotach, M. W., and Schmid, H. P.: A simple parameterisation for flux footprint predictions, Boundary Layer Meteorol., 112, 503-523, doi:10.1023/B:BOUN.0000030653.71031.96, 2004.
Kormann, R., and Meixner, F. X.: An analytical footprint model for non-neutral stratification, Boundary Layer Meteorol., 99, 207-224, doi:10.1023/A:1018991015119, 2001.

L172-174. It is not clear when and where this filter based on wind speed was applied. For example, in Figures 9, 11 and 13 data points with U<3 m/s are shown. Please clarify it.

We have clarified (lines 248-249) that the filter is applied wherever overall medians are presented for the dataset or the models.

All filters are applied to flux and deposition velocity values presented in Figure 9 (now 7). Wind speeds below 3 m s^{-1} are shown in 7D to provide a complete timeseries. However, we have now clarified in the caption that there are no corresponding flux or deposition velocity values for these periods due to the filtering.

Figure 11 (now 8) includes these points to show the enhanced deposition velocity at low wind speeds. The omission of these points from the final dataset is made clear in the caption, with a shaded region indicating the removed region of values.

Figure 13 (now 10) similarly deals with exploring the unwanted influence of land, and the identification that deposition velocity was enhanced at low wind speeds is an important observation for this. We have added to the caption to clarify that the values below the wind speed threshold were removed from the final flux and deposition velocity dataset.

5.4 Measurement uncertainty. Most of this section describe the approaches to calculate the flux random uncertainty, and then it should be moved under the Materials and Methods chapter.

Descriptions of both the empirical and theoretical methods for uncertainty calculation have been moved into their own subsection of the Materials and methods Section. Discussion of their application to the data has been left in the Results so that presented values of uncertainty can be considered comparatively with the flux and deposition velocity values in the preceding section.

L280. For the estimation of total random uncertainty note also the Finkelstein and Sims (2001) method, which do not require the estimate of the integral timescale (which may be not so straightforward). See Rannik et al (2016) for a comprehensive review of existing approaches.

We recognise the work of Finkelstein and Sims (2001) as an alternative method for estimating flux variance, and note its use of covariance functions similarly to that of Langford et al. (2015) presented in this work. While not included in this manuscript, its implementation as an alternative method in the eddy4R workflow will be considered in the continuation of these measurements.

L284. What do the authors mean by “integral timescale for vertical fluctuations? ”This should be the integral timescale of instantaneous covariance timeseries w’X’ (see Rannik et al, 2016)

This wording was ambiguous, and has been changed as suggested for clarity

L316-317. What about the response time of the O3 analyser and the sensor separation? Could the authors provide more details?

The sample inlet was position approximately 20 cm below the anemometer – this has been added to the text. Although a precise value has not been accurately recorded in the field, lab tests determined the response time to be < 1 second, and the co-spectrum (Fig. 11) does not indicate high-frequency flux loss to be very large.

Figure 14. It is not clear for me how the cospectrum was normalized. Values seems to be one order of magnitude lower than what should be the reference Kaimal cospectrum. Units in the figure axis labels could be put between parenthesis.

An error was made in the initial normalisation – the plot has been updated to correctly assign the area beneath the data to be equal to 1 (now figure 11).