

# ***Interactive comment on “Measurements of Ozone Deposition to a Coastal Sea by Eddy Covariance” by David C. Loades et al.***

## **Anonymous Referee #1**

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This paper describes coastal ozone flux measurements made at a location on the south coast of the UK. The paper builds on previous techniques to process & understand the data including its uncertainty. The paper includes a comparison of the data to estimates from oceanic ozone deposition models. The way that the paper is written most benefits readers who are very familiar with oceanic ozone deposition measurements and models. I would urge the authors to make changes in order to expand the readership. One way of doing so would be to better characterize what they are doing (and why) before the results of a given analysis are presented. There are also a lot of figures and information to take in – is this necessary?

My only major concern has to do with the footprint analysis, a large component of the paper. The footprint model used is for flat homogeneous terrain rather than a

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heterogeneous coastal site. I understand that a footprint model for the given land type may not be available, but I think the authors should explain more, with references, how a footprint model for a flat homogeneous terrain may or may not capture the footprint of a heterogeneous coastal site.

Detailed minor comments Abstract – does the percentage of the flux footprint being water change with tide, or the size of footprint? – readers may not know the Fairall model well. can the authors add some short description of this model to the abstract instead of, or in addition to, referring to the reference? – can the authors clarify whether they are talking about fluxes or deposition velocities when they refer to ‘deposition’? (this applies throughout the paper and the figures; I tend to think that ‘deposition’ refers to the flux’)

Line 26 – I think this a rather strong statement; only one paper suggests this Line 31 – briefly describe what is meant by ‘atmospheric and surface resistance values’ Line 31-31 – rephrase so as not to imply that we can’t learn anything from these lab and box enclosure methods Line 36 – references for this range of values? are the citations given in the previous sentences just for seawater? Line 52 – clarify the aspect of the depositional sink that needs to be better characterized, in line with the discussion in the previous paragraph; also, is it really a ‘tropospheric ozone cycle’? Line 55 – not sure this is the right usage of the term ‘natural variability’ Line 74 – can the authors describe more clearly in the text what Figure 2 shows and what the author wants the reader to do in referring to all the parts Line 105 – check sentence Line 112 – what is ‘dry ozone’? Line 130 – there is a negative sign missing Line 148-149 – what is ‘contrary’? are the authors implying that the dependences of Chang and Helmig are incorrect? Line 150 – new paragraph starting at “Footprint analysis” Line 159 – where is this estimate of roughness length from? is it appropriate for the location? Line 170 – removal ‘of’ Line 171 – clarify this sentence; what is the object of “contributing”? Line 171-4 – can the authors clarify what they are doing here? are they further filtering their data based on the roughness lengths or not? if not, is the justification only that

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they don't want excessive data removal? Line 175-180 – but does it mean anything for the authors' conclusions with regards to wind speed or friction velocity dependencies? Line 183 – what is being compared with the 20-min averaging? Line 186 – “Flux and deposition velocity values” Line 189-191 – say what this finding means Line 193-4 – say what this finding means Line 220 – is there a reference for this equation? Line 229 – is an assumption of constant Ts and [I-] fair? what's the 'relevant' time periods? Line 232 & 234 – what are the confidence intervals for  $m + b$ ? Line 237 – in terms of 'remarkable' I recommend the authors remain objective Line 239 – why consider only iodide reactivity? and I'm not actually sure what this means – I thought the authors were fixing [I-]. Does this mean that the authors are only considering the temperature dependence of A? generally, it would help if the authors gave brief descriptions of the Fairall and Luhar models, otherwise the discussion is not very useful for readers who are not well versed in oceanic ozone dep models. the authors do this to some degree in the discussion, but it would be nice to have this information closer to the beginning of the article. Line 240 – while the Luhar model underpredicts  $vd$ , it doesn't seem like the variability in the Luhar model is necessarily off, or worse than Fairall. Can the authors provide quantitative metrics for how well these models fit the data? Line 244 – what is the object of amplifying? Line 245 – is this deposition velocity for grassland from the models used in Hardacre et al.? or some observations used in the Hardacre model evaluation? regardless, the authors need to clarify and discuss the high uncertainty in using this value, and use references for the observations at grasslands if they are using the observations. Generally, I'm not sure what we are learning from the analysis with the Hardacre grassland value. Line 251 – confidence intervals for the land and sea values? Line 259 – I don't follow why ozone fluxes would be compared to emission inventories Line 260 – in contrast to what? what do the authors mean by 'aggregates'? Line 263 – why is this example 'extreme'? perhaps best to remain objective Line 265 – what could this mean in terms of the results? generally it might be better to have all the info about the tides in one paragraph, not two, with some of the info tacked on the end of a very long paragraph Line 266 – measurement height was adjusted

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how/where? Line 270 – where the authors expecting to see a diurnal cycle? would be helpful if authors set the stage for describing this analysis more Line 273 – describe method of Langford briefly Line 285 – in what relationship? Line 294 – similar to what literature? include references Line 295 – meaning that the authors do not use equation 12 to calculate the integral timescale? Line 299 – repeat empirical value here Line 301 – what do the authors mean ‘they defined twice’? Line 302 – clarify here that talking about variability within the averaging interval Line 303-4 – this sentence confuses me. random instrument noise in the ozone measurement or the wind measurement? Line 319 – say what the results with respect to block averaging vs. linear detrending means Line 324 – give the percentage for random uncertainty here Line 329 – does this choice of reaction-diffusion sublayer length have an impact on results? where is this estimate from? Line 333-4 – cut ‘significantly’ Line 353-5 – I’m confused by these sentences; rephrase Line 360 – why just discuss Helmig values here? Line 376 – give numbers here for instrument noise uncertainty Line 378-9 – clarify what the authors mean by larger (longer or additional measurements or both?)

Table 1 – say whether the data in the nth row is filtered by the criteria in the previous n-1 rows Figure 4 – it’s so helpful here that the authors point out what the reader should be “getting” from this figure – can the authors do this for other figures? Figure 5 – say what ‘DoY’ is Figure 9 – instead of saying “points omitted” (which to me implies that the authors do not include the data in the averages), can the authors say something like “points outside the yaxis range”? Figure 12 – I don’t know what I’m supposed to be looking at here/what this figure is telling me Figure 14 – ‘kaimal prediction’ is not very clear

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