Specific Comments

1. Introduction: The introduction is well written with appropriate references. The authors may wish to add a recent paper on CO photoproduction and AQYs (Kitidis, Tilstone, Smyth, Torres, Law, 2011. Carbon Monoxide Emission from a Mauritanian Upwelling Filament, Marine Chemistry 127, 123-133).

2. Methods; Section 2.1: Please refer the reader to Table 1 for sample properties (salinity, DOC, CDOM). The sites are referred to as "coastal", but the salinities would suggest they are very much "estuarine". This may be a matter of opinion, so a reference to Table 1 would allow the reader to make up their own mind.

Reference to table one added to section 2.1

3. p. 6953, Line 23-27: Were any of the CO2-degassed samples irradiated for CO photoproduction? How did these compare with the standard protocol for CO? It is not critical if this was not done, but I would be interested to know in future.

Degassed samples were not analysed for CO photoproduction in this experimental set-up. As mentioned in the discussion, Miller and Zepp (1995) have performed one of the only dual irradiations on degassed samples, and did not find a significant difference in CO photoproduction in degassed samples, suggesting that the process of degassing does not affect CO photoproduction. Additional not yet published data from our lab (Powers and Miller) confirms this observation.

4. p. 6955, Line 18-20: What was the phase ratio for equilibration (Sample volume to headspace volume)?

The following text was added to section 2.3:
To provide an initial CO-free headspace for equilibration, 13 mL of room air was drawn slowly through a 50 cc column of Schutze Reagent (Fisher Scientific) into the spectrophotometric cells. The exact volume of each cell varied slightly (all ~30 mL), and thus the sample volume to headspace ratio varied around 2.3 and this was accounted for in the calculations by using the exact total volume for each cell.

5. p. 6956, Line 21-23: Please discuss Qa error as a source of error for the determination of AQY. These sam- ples are highly colored and according to Hu et al. (2002) the first order approximation for the error of Qa (given ag320 in Table 1 and 0.1 m path) would be 16-121% here. This will propagate through to the AQY determination and I am sure will be the biggest source of AQY uncertainty by far.

The reviewer is correct that using the first order approximation of Hu et al (2002) would lead to a large uncertainty in samples of this absorption range. However, we did not use the first order approximation of Qa (top of second column, page 1263 of Hu et al), we used the full expression of the equation (equation 1 in Hu et al, equation 4 in this manuscript). We specifically used this full expression of the Qa in order to account for inner filtering effects, as stated in the text.

6. Methods; p. 6960, Line 21: Figure 2 is very unclear. I couldn't separate out the lines. I strongly recommend redrawing this figure.
Have redrawn the figure to separate the lines further.

7. p. 6961, Line 26: I would make the preceding statement less definitive unless the authors can back it up with statistics. There is a lot of variability in Figure 4a.

The text has been generalized to say:
Measured production appears to decrease with increasing salinity for both photochemical products seen in Figure 4a but this is likely due to a negative correlation between CDOM and salinity.

8. p. 6962, Lines 4-6: Not so sure if CDOM is the "carbon fuel" for CO/CO2. Either back it up with references or remove. CDOM certainly plays a central role in CO/CO2 photochemistry, but I wouldn't go as far as saying it is the substrate (fuel).

Text has been generalized and a reference to Equations 4 and 5 added:
Because the absorption of a photon by CDOM must occur for a photochemical reaction to occur, a relationship between CDOM "concentration" and measured production should exist (see Equations 4 and 5).

9. p. 6964, Lines 19-21: Alternatively, the observed trend in lower CO photoproduction efficiency could be explained by bleaching (prior radiation exposure). Looking at Fig. 3, the seasonal trend seems to be driven mainly by summer months (June-July-August).

Text has been amended:
Additionally, this seasonal trend with lower efficiencies seen in the summer months could be driven by increased solar irradiation and pre-exposure of the CDOM to extensive sunlight, leading to photobleaching and potentially lowering the efficiency of the photochemical reaction. CDOM released in the fall and winter months would have less prior exposure to sunlight and this could lead to the higher efficiencies seen in those months. In reality, both of these situations are likely to co-exist.

10. p. 6964, Line 28: Spelling "dominant"

This has been corrected.

11. p. 6965, Lines 13-19: The authors suggest that "Pre-exposure of the CDOM to sunlight could explain the variation of CO2 to CO production ratios . . . since samples from riverine sources, presumably having had less sunlight-exposure showed higher ratios (see Fig. 3) . . .". Why not look at the ratio against salinity? If a positive correlation between CO2:CO and salinity was found, this would make the argument much stronger .

The relationship between CO2:CO ratio and salinity was examined, but as there was no relationship found, an additional figure was determined to be superfluous and was not added.

12. p. 6965, Line 19: Spelling "consistent"

This has been corrected.

13. p. 6965, Lines 19-20: The authors suggest that ". . . CO2 photoproduction is more variable than CO photoproduction (which) . . . is consistent with CO2 AQY spectra being more affected by pre-exposure to sunlight than . . . CO ". Fair enough, but earlier the authors suggested that CO2 photoproduction was more variable due to differences in the molecular

composition of source material (p.6963, line 20-23). I don't object to either explanation, but please make it more obvious that there are alternatives.

Text clarified to reflect both alternatives as:
This is consistent with both CO_2 AQY spectra being more affected by pre-exposure to sunlight than CO AQY spectra, and differing molecular compositions of the source material for the two different photoproducts as discussed above in section 4.1.

14. p. 6965, Line 26: "Conversely...Delaware River". Please also refer to Stubbins et al. (2011) here.

Reference to Stubbins et al added:
Stubbins et al (2011), also found an inverse trend between CO AQYs and salinity in the Tyne River estuary. This was not explained by simple conservative mixing and suggests that this relationship is more strongly tied to CDOM.

15. p. 6966, Lines 4-7: The authors suggest that the absence of a salinity-AQY relationship here may be due to the relatively limited salinity range of their samples (most fall between 29 and 33). Yet, 10 of 38 experiments have salin- ity <29 (26%) and as low as 0.1, so I don't think this is a valid argument. I think the presence/absence of such a relationship is more likely to be specific to the system of study. The St. Lawrence estuary and Beaufort Sea have much longer residence times (presumably). This may be comparable or longer than photochemical turnover, so that bleaching of CDOM and concomitant changes in AQY are apparent. In contrast, in short residence-time estuaries, CDOM may be "flushed out" faster than it is turned over photochemically, resulting in lower rates at high salinity (due to lower CDOM), but constant AQY. Alternatively, Xie et al. (2009) and Stubbins et al. (2011) have a much larger range of CDOM absorbance, 2-orders of magnitude as opposed to 1 here. This may be more important in separating out AQY differences than salinity and we know that CDOM and salinity are generally inversely related in estuaries.

29 was a typo, we meant 20 (has been corrected). Samples falling within the range of 20-30 represent 35/38 samples or 92% of the samples for which salinity information was available. The discussion of CDOM, which we agree is likely to be more important, follows this section immediately.

16. Conclusions, p. 6968, Lines 25-27: This statement may be missleading. Photochemical production efficiency under constant light (solar simulator) varries by 21.7% seasonally, but CO photoproduction "over all of Georgia coastal environments" will vary by more than that due to seasonal insolation differences.

This sentence was clarified to refer to the variation of the EFFICIENCY of photoproduction rather than TOTAL photoproduction:
The photochemical efficiency of CO production over all Georgia coastal estuarine environments studied varied annually within 21.7% with a seasonal pattern.

17. Table 1: Headings appear to be offset (e.g. DOC is above psu). Please correct.

This was a typesetting error. Corrected.

18. Figures 4-7: In the respective figure legends use the word "symbols", not "circles". Also for CO, "grey symbols". What do the lines represent? Presumably CO2 (black) and CO (grey). Make these solid and thicker and explain what they are in the legend.

The word “circles” was a typo, it has been corrected in all legends. The lines were calculated as a linear regression, however they are only there to guide the reader’s eye, and due to low R^2 values, they should not be used predictively.