

Interactive comment on “Integrating O₃ influences on terrestrial processes: photosynthetic and stomatal response data available for regional and global modeling” by D. Lombardozzi et al.

Anonymous Referee #2

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Overall quality of the discussion paper ("general comments"), This manuscript reports on a data set that fills a critical void in regional and global modeling under global change. The analysis provides the basis for integrating ozone responses into ecosystem models, and points out some key differences between the current modeling methodologies and the evidence from empirical studies. The manuscript is quite well written and for the most part is very clear (see details below) and is therefore appropriate for publication with some minor revisions. I would like to see the authors present a little more discussion on the impacts that using linear responses to CUO has/will effect predictions of global carbon uptake, with a little more emphasis on the potential solutions presented in the final section of the discussion. Also I think there should be

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more concise and emphasized statement of the novelty of this study relative to the work presented by Mills et al., and Wittig et al., as well as a more directly addressing the discrepancies between the previously documented responses of yield, and the new reports on gs and photosynthesis (see specific points below).

Individual scientific questions/issues ("specific comments") I would like to see a mention of the effects of ozone on other key biochemical processes such as the regeneration of Rubp. While the pool of data on the effects on Jmax is too small to include the type of analysis conducted here, there is potential that these effects are important, especially at high O₃ and when CO₂ is non-limiting (Martin et al., 2001; Long & Naidu 2002; Morgan et al., 2004; Fiscus, Booker & Burkey 2005; Betzelberger et al., 2012). There are also ecosystem-scale responses of water dynamics to O₃ documented in Bernacchi et al., 2011 and VanLoocke et al., 2012, that would be appropriate to mention here. Is it not confounding to correlate gs with CUO given gs is used in the calculation of CUO? In my opinion, the impacts of omitting the open air studies from the analysis should be discussed. As indicated by Wittig et al., 2007, there is evidence that O₃ have a stronger effect in open air experiments relative to chamber experiments. I understand the rationale for separating them from the studies with CF as a control; however I think it is critical to incorporate these data, as they are likely the most representative data for parameterizing regional models. In my opinion the importance of CUO on phenology is under explored here (e.g. Betzelberger et al., 2012). If many studies report significant decreases in yield with increasing O₃ exposure (Mills et al., 2007), but the data analysis here shows no such correlation of photosynthesis, than some other factor (e.g. phenology, LAI) must explain this discrepancy. Please expand on this point.

Compact listing of purely technical corrections at the very end ("technical corrections": typing errors, etc.). P6977 L2-5. The sentence on conductance and with links to hydrology is potentially ambiguous/confusing. Please clarify with a more explicit statement. I suggest that conductance is explicitly referred to as "stomatal conductance" through-

out the manuscript to avoid any confusion with other conductances. P 6990 L25. It is unclear what is meant by “stomatal responses in crops become damaged with chronic O₃ exposure. . .” P6976 L25. This is not always the case some crops show decreasing gs with greater O₃ exposure (Betzberger et al., 2012).

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