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## ***Interactive comment on “Have ozone effects on carbon sequestration been over-estimated? A new biomass response function for wheat” by H. Pleijel et al.***

### **Anonymous Referee #2**

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The analysis by Pleijel et al. highlights and quantifies a significant flaw in the method used by some studies that predict indirect ozone feedbacks to climate via plant damage. It is important to distinguish the effects of ozone on grain yield from those of biomass, as they are likely not the same. While these are important points, the authors will need to reframe the focus of their paper; the studies referenced (Sitch et al. 2007, Collins et al. 2010) use grain yield response functions to decrease simulated photosynthesis, not biomass as the authors indicate (see methods in Sitch et al. 2007). I think this study is important in distinguishing that responses to ozone differ based on the variable measured, and the authors should focus on this point rather than emphasizing that they have figured out the correct response function to use. Using a biomass

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response function, instead of a yield function, to modify photosynthesis will have the same problem that the authors are highlighting since leaf-level impacts (photosynthesis) are different than whole-plant impacts (biomass). It is still important to point out that studies should not use yield response functions to modify photosynthesis equations, but this study is not as comparable as the authors state.

There are some serious concerns with the methodology used to estimate ozone damage. First, the authors use non-filtered air as a reference. This is problematic because non-filtered air will have different ozone concentrations based on the location of the study, so the baseline for comparing the effects of ozone is likely to be different in the different regions. There needs to be some way of standardizing the baseline. The most obvious solution would be to use filtered air (near 0 ppb ozone), rather than ambient air, as a baseline. Second, the authors state that POD was calculated hourly and that stomatal conductance was estimated (not measured) based on environmental variables and phenology. Were the environmental variables (VPD, temperature, and radiation for stomatal conductance, and ozone concentration for POD) measured in the experiment? Several studies have shown that stomata respond sluggishly to environmental cues with ozone exposure (see work by Paoletti, Grulke), so estimating conductance can often be problematic. There is no indication that the authors were able to compare estimated conductance with observed conductance to evaluate their methods. Last, it seems (though is unclear) that POD is assumed to be 0 in the ambient air treatment, which might not be accurate depending on the ozone concentrations at each site, particularly since ambient ozone concentrations can be quite high in Southern Europe.

Overall, the paper is interesting and illustrates an important point that the scientific community needs to consider in future analyses.

Detailed Comments:

1) The equations presented in the introduction are quite confusing. Perhaps find a

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clear way of describing the impacts, and save the equations for the methods section. The authors should also describe how all the  $f$  variables are calculated.

2) In equation 3, are the constants somehow related to the  $f$  variables?

3) The introduction & methods state that you used 21 studies, however the analyses only use 12. How were these 12 studies selected out of the 21? Also, did you decide on the 21 experiments in a systematic way, such as sampling all available literature?

4) Please make sure that specific physiological terminology is defined. For example, you use “anthesis” and “monocarpic” in the discussion without defining these terms previously.

5) It is clear that both  $B_a$  and  $H_I$  are important for  $Y_g$ , but only the impact of  $B_a$  is discussed in terms of use in models. What is the impact (proportional or otherwise) of  $H_I$  on  $Y_g$ , and how might that alter the model? It seems that this would be important to include when discussing the economic impacts of ozone.

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Interactive comment on Biogeosciences Discuss., 11, 5511, 2014.

**BGD**

11, C1293–C1295, 2014

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