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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.

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Final author comments on Biogeosciences Discussion Paper:

Have ozone effects on carbon sequestration been over-estimated? A new biomass response function for wheat

By: H. Pleijel, H. Danielsson, D. Simpson, and G. Mills

We would like to thank the reviewers for their comments, which will improve the manuscript. Below, please find our responses to all the comments and questions of the reviewers.

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Reviewer 1

This paper highlights a significant source of error in the current practice of estimating changes in agro-ecosystem carbon balance under elevated ozone from crop yield responses. It quantifies the consequence of driving a model of carbon balance across Europe with the biomass rather than yield response to ozone of wheat. This deserves to be published in my opinion. However, as written, the paper implies that biomass response of a crop to ozone is a direct measure of changing ecosystem carbon balance. It needs to be edited to reflect that standing biomass is not a measure of net ecosystem productivity and that comprehensive understanding of carbon cycling responses also depends on knowledge of downstream effects on the other elements of net ecosystem productivity e.g. heterotrophic respiration and soil organic matter incorporation.

Response: We agree with the reviewer. At the end of the Discussion the following text will be inserted which should cover the aspects raised by the reviewer: "It should be noted that the standing biomass effects shown in Figures 5 and 6 do not represent direct estimates of the carbon storage effect. Comprehensive understanding of carbon cycling responses depends on models which take into consideration downstream effects, such as heterotrophic respiration and soil organic matter build up/decomposition in the agroecosystems, but also the further use and longevity of agricultural products. Such an analysis is beyond the scope of this paper. However, our analysis shows the considerable difference between biomass effects and grain yield effects, which has strong implications for modelling of effects of ozone on carbon storage in agroecosystems."

Reviewer 2

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

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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 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.

Response: We agree that Sitch et al (2007) and Collins et al (2010) used grain yield response to simulate loss of photosynthesis and that this does not become clear in our manuscript. We will add information to the Introduction to clarify this. However, the use of the grain yield response function to derive effects on photosynthesis by assuming that the relative effect on grain yield is the same as the relative effect on biomass (“plant production”), although not strictly the same thing as using grain yield for biomass estimation, suffers from the same fundamental flaw by ignoring the significant effect of biomass partitioning on crop yield, which is a key message of our manuscript. At the end of the discussion we will also highlight the limitations of assuming that ozone effects on biomass are directly proportional to ozone effects on photosynthesis: “Finally, apart from the consequences of the partitioning effect which is highlighted in this paper, future use of the effect of ozone on biomass to estimate the ozone effect on photosynthesis should be supported by evidence that this is a valid assumption.”

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

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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.

Response: There are advantages and disadvantages to the use of filtered or non-filtered air as reference. A disadvantage of filtered air for example is that the effect of current ambient ozone no longer becomes visible in the figures. However, since this is not a main focus of the paper, we have taken the reviewer's comments and made use of filtered air as the reference in revised calculations. Actually, the revised figures are very similar to the ones using non-filtered air as the treatment. It should be noted that the populations of experiments included will change a little, since there are a few experiments having a filtered air treatment, but no non-filtered air treatment (now included in Figures 1+2, but not in the earlier manuscript) and a few experiments having a non-filtered air treatment but no filtered air treatment (now excluded, but earlier included in the figures). Anyway, it will be obvious from Table 1 which experiments are included. The revised Figures 1R and 2R are exhibited below. A number of mostly minor consequential changes of the text will be made along with replacing the older figures with the revised ones.

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.

Response: The stomatal conductance model has been developed and described in detail in earlier papers. It was compared with observations from the literature and with our own data. This has been described in Pleijel et al (2007) with important additions

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in Grünhage et al (2011) and the methodology in general was presented in Mills et al (2011). All details of the conductance model are available in the Mapping Manual of the Convention on Long-Range Transboundary Air Pollution, to which direct reference will be made in a revised version of the manuscript. Pleijel et al (2011), where most of the calibration/test of the conductance model is described, was not included as a reference in the earlier version of the manuscript. We will in a revised version of the manuscript include it and explain more clearly in the text where information about the modelling can be found. It seems inappropriate to repeat in this manuscript the information that has already been published, since no deviations were made from the existing and already described methodology.

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.

Response: No, for the response functions shown in Figure 3 and 4 POD₆ is calculated for each treatment. It is mostly (very) low in the filtered air treatments, but POD₆ values for the non-filtered (i.e. not “ambient” - this treatment also exist in most experiment but was not used in any of the figures) are often significantly higher. It is stated in the manuscript that “The relative scale is based on the assumption that there is no ozone effect on above-ground biomass at zero POD₆ in each experiment”, which does not mean that POD₆ is assumed to be 0 in any treatment, but in reality it is near zero in most filtered air treatments. The assumption is that there is zero effect at zero exposure (POD₆) for each experiment. A relative scale is used where the biological response variable takes the value of 1 for POD₆ = 0. We will revise the phrasing and be more explicit in the Methods section to make sure that this becomes clear in the revised version of the manuscript.

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 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.

Response: We believe that it is good to base the Introduction on relationships that are fundamental to crop physiology and this is efficiently made by introducing Equations 1 and 2. If required we will move the quantitative aspects represented by the equations to the Methods part, but we believe that the presentation of the science behind our considerations will suffer from that. If the values of grain yield, harvest index and above-ground biomass, are known for the reference treatment (YGref, HIref, BAref) and a treatment with e.g. higher ozone (YG, HI, BA), f1, f2 and f3 can be calculated as BA/BAref, HI/HIref and YG/YGref, respectively. This information has now been added to the Introduction in association with Equations 1 and 2.

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

Response: Not sure we understand the question, since the word “constant” is not used in the manuscript. The three f variables describe the magnitude of ozone effects on the three response variables and the equation shows how the f variables and response variables are interrelated.

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?

Response: To make the kind of analysis presented in Figure 1 and Figure 2, showing relationships between different biological response variables, you only need the biological information – grain yield, harvest index and total above-ground biomass – in the different treatments, which are often all presented in papers describing experiments of ozone effects on wheat and other crops. To make the dose-response function pre-

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sented in Figures 3 and 4 you additionally need hourly data of ozone concentration, temperature, humidity and solar radiation for the growing season. We received this information for some (12) of the experiment; we have asked for the detailed ozone and meteorological data for several of the other experiment, but did not receive it. This is the simple reason that there are fewer experiments included in Figures 3+4 compared to Figures 1 and 2. We will rephrase to clarify this further in the revised version of the manuscript. The inclusion of data from the experiments for which hourly ozone and meteorological data were not available in Figures 1 and 2, increases the robustness of the presented relationships in these figures. We searched the literature using Web of Science for relevant for papers describing relevant, agronomically realistic experiments containing the needed biological variables.

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.

Response: We will clarify concepts, such as “monocarpic” and “anthesis”, the meaning of which is not obvious to all readers, in the revised version of the manuscript.

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

Response: Agree. It is true that much of the discussion is focused on BA and YG and their relationships. HI is the link between them, describing the efficiency with which aboveground biomass is converted into seed. Although it does not add anything in itself (other than influencing the critically important relationship between BA and YG) to the question of carbon storage, it deserves an extended paragraph in the Discussion as it is a key feature of ozone effects on seed crops and economic assessments of these. Thus it is relevant to our study. Further information on this will be added in the

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revised manuscript.

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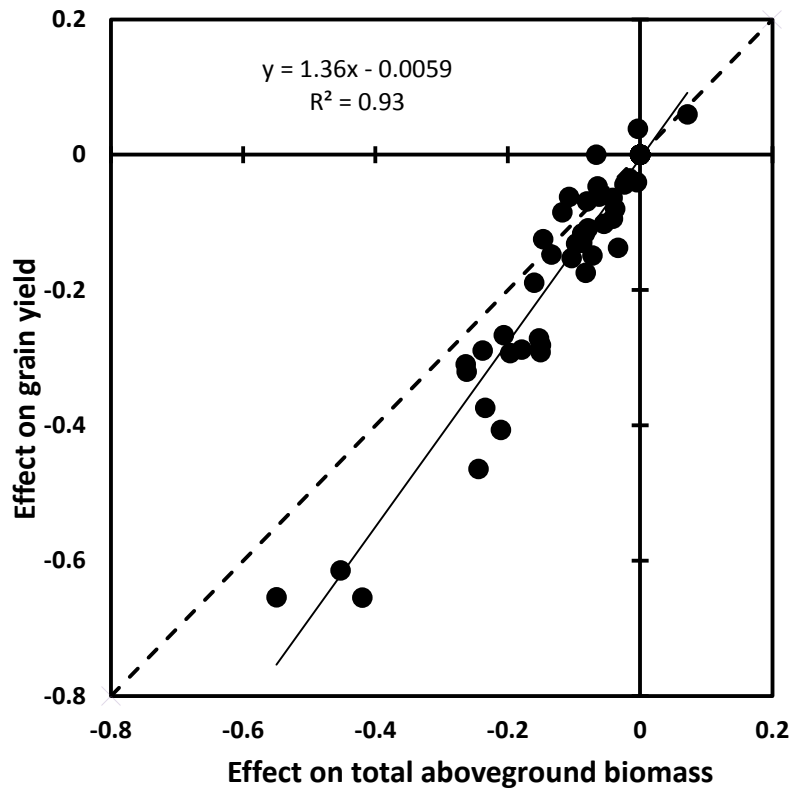
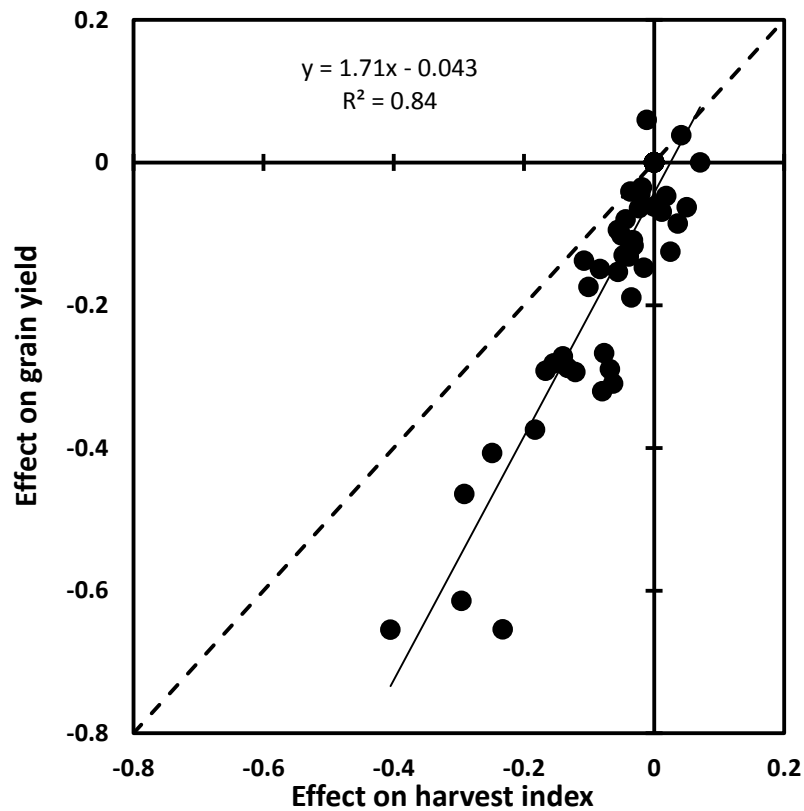
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Figure 1R

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Figure 2R

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