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Discussions

Interactive comment on "Declining risk of ozone impacts on vegetation in Europe 1990–2050 due to reduced precursor emissions in a changed climate" *by* J. Klingberg et al.

Anonymous Referee #3

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This manuscript presents a modelling study addressing the trends of risks of detrimental ozone effects on vegetation in Europe. Tropospheric ozone concentrations have changed as a result of changes in precursor emissions and in the future are expected to be increasingly influenced by climate change. As ozone is a key air pollutant, it is important to understand how much, and to which direction, these driving forces will affect the near-surface ozone concentrations and their impacts on vegetation. Thus the topic of this manuscript is highly relevant and the emerging conclusions carry significant policy implications.

Unfortunately, I cannot recommend publication of this manuscript in its present form. While the methods are sound in principle, there are some serious shortcomings in the

application of these methods for assessing ozone risks to European forests and crops. There is some very useful material in the manuscript that could be further developed. However, the revision should be substantial and go beyond just improving the existing text.

Major comments

1. Risk assessment

The risk of ozone impacts on vegetation is assessed in terms of the AOT40 index. As the authors explain, AOT40 is known to poorly reflect the actual stomatal uptake that is a prerequisite of any physiological impact. Thus the so-called POD (previously known as AFst) index, which is explicitly based on stomatal uptake, has been developed and is widely used in ozone risk assessment. In the present study, the environmental controls that are inherently included in POD are discussed based on meteo/hydrological data from a climate model. I have the following concerns about this approach:

1a. Why did you not calculate POD, which would make it possible to quantify the effects that in the present version can only be addressed in a (semi)qualitative manner? I cannot avoid the impression that AOT40 was adopted just because it is very easy to calculate it from the existing concentration data. Results of the model runs used in the paper have been previously published as mean ozone concentrations (Langner et al., 2012a), and it is generally known that AOT40 is more sensitive to changes than mean concentration. Against this background the value of AOT40 maps is not very high. The problem of poorly quantified results is obvious in the discussion of climatic effects on ozone risks (p 637, top). How is it possible to conclude that the climatic modifications of risk are small, if these modifications were not quantified?

I should point out that the present authors have previously published such POD results (for selected locations) based on the same chemistry-transport model, including climate change scenarios (Klingberg et al., 2010). Furthermore, the recommendation for future work (Sect. 3.4) may give an idea that nobody has ever carried out such work, while in fact European-scale risk estimates based on modelled AFst were published already in 2007 (Simpson et al., Environmental Pollution).

1b. The authors acknowledge that “the climate model is not optimized to model soil water” (p 636), implying limited confidence in the modelled soil water data, and present no comparison against observations that would indicate otherwise. Still these data are considered sufficiently reliable to estimate how frequently soil water content (SWC) limits stomatal exchange. While the results show no limitation in northern Europe, the authors conclude that in fact such limitation is likely, since another study has shown that to be the case. I find this logic confusing. A similar analysis was carried out for the VPD effects without any discussion of the quality of modelled air humidity data. The conclusions resulting from this sort of reasoning do not appear very convincing.

1c. It is explained that in southern Europe drought is an important additional factor for the growing season length (p 634). But you have separately considered the effect of soil water (Sect. 3.3), which must, at least partly, cover the growing season effects.

2. Model performance

While the long-term development of AOT40 in different locations (Fig. 4) is an interesting result, it is important to note that there is a major inconsistency between the model results and observations. At the Montelibretti station, for example, AOT40 decreased during the period of observational data (about 1995-2010) from approximately 40000 ppb to 30000 ppb, i.e. by 25%. The AOT40 observed during this period ranges from approximately 12000 to 40000 ppbh randomly without any trend. On the other hand, in the model results this AOT40 range corresponds to the period of 1960-2050. In other words, the observations do not reflect emission changes, while the model underestimates the observed meteorological variability. Given this striking difference between the modelled and observed trends, how is it possible to reach the key conclusion of the study, i.e. that the risk of ozone impacts is declining? Please discuss.

3. Geographical domain

The study covers the whole Europe. However, some assumptions and parameter choices are clearly biased to the northern parts of the continent and may not be well representative of the region where the highest ozone concentrations typically occur.

3a. The limits adopted for SWC and VPD effects (15% and 0.8 kPa, respectively) are parameter values defined within the methodology adopted by CLRTAP (so-called Mapping Manual, MM; CLRTAP, 2004) for northern European coniferous forests only. What is the rationale for applying a Norway spruce specific value across the whole continent? It should be noted that the time fraction when SWC/VPD is below/above the limit value is 0% within the region defined in the MM as the domain of the northern European parameterization (Nordic and Baltic countries).

3b. What is the rationale for applying a common, simple temperature limit for the growing season across the whole continent? The citation provided for this criterion reports about a study that only covers a limited area (Sweden).

4. AOT40 calculations

There exist different definitions for the calculation of AOT40. For example, CLRTAP and EU define the growing season differently. There are inconsistencies in the definitions adopted in the present study.

4a. The AOT40 calculated for trees is compared to the critical level of CLRTAP, which is defined in the MM. However, the definition of growing season is inconsistent with MM, both with the default growing season and the recommendations for local exposure windows.

4b. According to the CLRTAP definition in the MM, the AOT40 for crops should be calculated over the period of active growth of wheat. There are large geographical differences in this period. The fixed period adopted in the present study is defined in the EU Directive, but that definition does not involve the critical level that is used as a reference. In addition, the daily accumulation period is fixed (8:00-20:00 CET), while the CLRTAP uses a variable time window (daylight hours).

4c. For both trees and crops, AOT40 is defined at the canopy height of the vegetation considered (CLRTAP, 2004). In the present model calculations, however, AOT40 is calculated from concentrations at a constant height of 3 m. Due to the large near-surface gradients, the concentrations are sensitive to this height, and AOT40 even more so.

5. Emission scenarios

The paper is about declining ozone exposure owing to emission reductions in Europe. These emission reductions stem from the global RCP4.5 scenario. All results are conditional to the realization of this scenario, which is repeatedly highlighted in the paper. Thus I find it strange that the authors do not provide any analysis/discussion of this scenario.

5a. As an absolute minimum, I would expect to see how large are the precursor emission reductions expected according to this scenario (cf. discussion on p 633).

5b. In addition, it should be explained how this scenario relates to the existing emission abatement legislation in Europe. Given the focus of the study, it is not sufficient to simply adopt a scenario without providing any further context.

5c. The assumption of constant boundary conditions is inconsistent with RCP4.5, which suggests significant changes in the ozone precursor emissions outside Europe. It seems unbalanced to conclude that regional emission reductions are efficient but the contribution of external sources simply “remains unclear” (p 638), when these contributions can be (and have been) estimated using models similar to that used in the present study. By the same token, the present background concentrations are hardly valid in 1960, when the simulations were started.

Detailed comments

page 628/lines 2-4: This sounds a bit trivial. Has anyone tried to ‘predict’ future emis-

sions? Emissions also depend on other factors than political decisions.

632/3-13: Results should not be placed in Materials and Methods.

632/8: What do you mean by ‘spatial correlation’?

632/23-24: Why do you expect vertical mixing to be strong over the sea? The idea is probably to say that the marine boundary layer is continuously well mixed as compared to land.

633/8-10: As above, the calculations are based on a scenario, which is not a prediction. Thus the concept of ‘credibility’ is misguided here.

634/8-11: Wouldn’t it be possible to discuss, and quantify, the changes in ozone episodes based on your own model simulations?

634/24-635/4: As above, it would be straightforward to use the present data to quantify this, i.e. to check how much the ozone concentrations during the additional growing season days contribute to AOT40.

Technical corrections

627/11: is -> are

634/1: overestimate <-> underestimate

635/23,25: kPA -> kPa

636/27: for SWC, underestimated -> overestimated

Table 2: Please define RMSE, spatial correlation and bias.

Fig. 1a: Inclusion of stations should be explained.

Fig. 2: Indicate crops.

Fig. 3: Indicate trees or crops.

Interactive comment on Biogeosciences Discuss., 11, 625, 2014.