Response to Anonymous Referee #2

Referee #2

In general, the manuscript is well written and the manuscript addresses an important scientific question that is relevant for Biogeosciences: How growing seasons can be dynamically simulated and how this affects typical calculations with a CTM model. The authors use a well-established CTM Model EMEP (Simpson et al. 2012) that is well suited for the particular scientific question. The authors also show how certain output metrics from EMEP model are affected by dynamic calculation of the start of the growing season. Due to this, the authors argue that there is strong need for a dynamic description of growing season in CTMs. Overall I recommend that the article is published in Biogeosciences but also recommend that the authors both focus on the limitations of their approach (e.g. using birch as a model species for all deciduous trees) and that they provide some more detailed statistical information about the model performance such as seasonal ozone statistics.

Reply

We have addressed the need for more explanations and caveats surrounding the use of birch, as explained in the following sections. Concerning model performance, we have added some additional sentences, as follows:

Evaluation of the EMEP models performance for ozone concentrations has been presented elsewhere (e.g, Jonson et al. 2006, Colette et al. 2011, or for many individual sites for the year 2009, Gauss et al. 2011). Examining results from 43 EMEP sites included in the present runs, we found mean over-predictions of daily maximum ozone of ca. 5-6% in the winter (DJF) and spring (MAM) months, increasing to about 14% for the Autumn months (SON). Model performance changes considerably from site to site, however, and the reasons for this are often not so clear; likely sometimes model-related, sometimes problems with the observations. The issue of EMEP model performance with respect to the ozone-uptake parameters is even more difficult, but has been tackled in several previous papers (e.g. Tuovinen et al. 2001, 2004, 2007, 2009 or Klingberg et al. 2008).

We think that more detail on this would however detract from the aims of this manuscript. This paper is intended to investigate how the model results change when we use more realistic growing seasons. As we will also show below, ozone concentrations themselves are hardly affected by this change, whereas AOT and POD values are.

General comment to manuscript

The selection of data points for validation seems to be very reasonable. The same concerns the number of citations. This is in general well balanced and the cited articles are easy obtain.

a) I have one particular question which I think needs to be clarified. The question concerns the assumption that it is possible to use a model for birch flowering as a reasonable surrogate for deciduous trees in the EMEP CTM model. This assumption forms the entire manuscript with the respect to model simulations and which things that are discussed. However, birch is a light demanding species that has favourable conditions in only a fraction of the EMEP model domain (Skjøth et al. 2008). Other common trees are beech and oak. Furthermore, beech and oak are species that are common in the areas where the AOT40 values are high such as Central and Southern Europe. Will it make sense for the authors to do similar calculations for another species such as beech or oak or even better simulate several species at the same time? Can it be done or what is the needed before this can be done?

   Reply: We have added the following text to the manuscript:

   This work should be seen as a first step to including more realistic growing seasons in the EMEP model, with the introduction of a meteorological-dependent SGS being compared to the assumption used up to now, that SGS is a function of latitude only. To make the modelling of start of growing season as possible as simple, but still valuable, we chose a species (birch) whose start of growing season mainly depends on temperature (Körner and Basler 2010). Beech and oak are other characteristic species for a temperate deciduous forest (Allabay 2006, Skjøth et al. 2008), which we had considered for this study, but SGS for these species is complicated by a greater dependence on light conditions (Körner and Basler 2010). However, differences in SGS between different deciduous species are not so great. For example, Menzel et al. (2008) investigated the lengthening of the growing season in two European countries, and amongst other results they found differences in leaf unfolding of about $\pm$ 2 weeks.

   We plan to investigate species such as beech and oak in future, but these likely require more complex (e.g. light-dependent) formulations (Körner and Basler 2010). We have added text to the manuscript to make this point clearer.

b) The evaluation of the EMEP model: Why have the authors used annual mean values? it must be expected that only a very small period of time (e.g. during spring) there will be a difference in the model results. so averaging over an entire year will to some degree hide the changes in the model results. The authors should therefore focus on the period where there are changes: the spring.

   We have focused on long-term values (growing-season, not annual) because the metrics (POD1, AOT40) recommended by the UN-ECE Mapping Manual for estimating risks to vegetation are themselves accumulated values - determined over the whole growing season. We are not aware of any relevant metrics over shorter periods that we could use. However in order to illustrate the difference between spring-time and longer-term results, and between ozone flux and ozone concentrations, we have added a Figure:

   Fig. 1 here shows daily maximum ozone concentrations (at canopy top), and daily $\mathrm{POD}_{1,DF}$ values, for one site in Greece and one site in Sweden, using the base-case (LAT) SGS estimates and the T5-estimates. The use of the T5 SGS values is seen to have very little effect on O3 itself, (see discussion in Sect. 6), but a rather significant effect on POD1 at the start of the growing season.

c) It is very unclear to me, exactly where and how dynamic calculation in the start of the growing season is used in the EMEP. As I understand it, then the start of the growing season is used in the dry deposition scheme which is described by Simpson et al. (2012) and the BVOC emission. A simple but more detailed overview with the citation to the exact equations and Tables would make it much more transparent (e.g. equation 18,54 and Table 3 as given in Simpson et al. (2012) etc.)

   Reply: Yes, both dry deposition (including ozone-fluxes) and BVOC and soil-NO emissions are affected by the growing-season estimation. We have added more detailed references in the text to clarify this.

d) The LPJ-CRU methodology (section 2.2) seems to be the physiological most reasonable method as it include both chilling and heating. So it is a bit surprising that this methodology has much more lower performance that the two other methods. To my knowledge, then the current version of LPJ-GUESS with a species dependency was published in 2012 by Hickler et al. (2012). However, this article did not contain a calibration of the parameters that describes the start of the growing season for birch. Instead, Hickler et al. (2012) cited an earlier article by Sykes et al. (1996) that provided the needed values for the LPJ-GUESS methodology. In that earlier article the situation was the same. There was no calibration of the start of the growing season. To me appears that the LPJ-GUESS parameters for birch are based on something unclear. Probably the authors should take this into account or better– have calibrate the LPJ-GUESS methodology on their own data before it used and compared with the two other methods and in a similar way as the TTM or the T5 methodologies.

   Reply: In section 2.2 we have given the procedure we used, which was based upon the references given, but also the LPJ-GUESS code to get definitions for the start of the chilling season.

   Calibration of LPJ-GUESS is best left to the LPJ-GUESS model developers we feel; that is beyond the scope of our study. As we discussed in the manuscript, LPJ-GUESS is a model intended for global application, so it is not so surprising that it does not perform so well for one particular area (Europe). Indeed, given its global scope, the performance is not so bad - good correlation coefficients although starting too early. In LPJ-GUESS applications, the early SGS is partly compensated by a long leaf-development period, but again, such considerations are beyond the scope of our study. (Colleagues from the LPJ-GUESS team at the University of Lund are also working to re-evaluate the growing-season parameters, but new values were not available at the time of our work.)

   We agree however that LPJ-GUESS provides in principle a more robust methodology for exploring future changes in SGS (e.g. due to rising CO_2, or N-deposition changes, as well as temperature). Indeed, we are working with LPJ-GUESS scientists to explore couplings between the EMEP MSC-W model and LPJ-GUESS; this is one of the main reasons to include LPJ-GUESS in the current study. We have added text in the manuscript to make this a bit clearer.

Minor comments to the text in the manuscript

Referee #2

Page 12140, line 25: Please proved at least one reference in each of three named examples in the use of GDD: SGS, flowering time and start of pollen prediction time.

Reply We have added additional refs, for SGS: Wang (1960); for flowering: Linkosalo et al. (2010); and for pollen prediction: Galän et al. (2001).

Referee #2

Page 12140, line 25: start of pollen prediction time. What does the authors mean with the sentence? Please rephrase.

Reply This should have been start of the pollen release by plants. We have clarified this in the text.

Referee #2

Page 12140, line 26-28. Probably the well known MEGAN model (Guenther et al., 2006) would be very relevant to cite here as well. MEGAN is very often used in connection with CTM model studies.

Reply We have added this reference.

Referee #2

Page 12141, line 16. According to Sofiev et al. (Sofiev et al., in press), the TTM model was developed by Linkosalo et al. (2010), while Sofiev et al. (Sofiev et al., in press) have calibrated the TTM model for SILAM model domain by using the data from Siljamo et al. (2008) and pollen data from the European Aerobiological Network (EAN, https://ean.polleninfo.eu/Ean/)

Reply We have changed the text.

Referee #2

Page 12145, line 5-6. The reference to the personal communication can be replaced with a reference to Linkosalo (1999)

Reply Done.

Referee #2

Page 12147, line 4-5. This statement is probably only valid for central and Northern Europe and for birches. Other trees like alder have quite different requirements respect to onset on leaves and flowering (Linkosalo, 1999).

Reply This second bullet point is describing the advantage of the T5 method. A number of caveats are mentioned in the next paragraph, and we have expanded this to cover the example of alder as mentioned above, and of some other species.

Referee #2

Page 12154, line 19-30. The authors discuss various methods for the end of growing season and argue that the most simple method is probably sufficient. However, the authors use climate change studies as an argument for using a dynamic calculation of the growing season. This arguments should also be taken into account for the end of growing season, despite that the modelling results between the different methods do not differ that much.

Reply As discussed in the text, it is more important to establish the start than the end of the growing season, not least as ozone concentrations are usually higher near SGS than near EGS. There are also studies suggesting that ozone-uptake at the start of the growing season in more important than towards the end (Pääkkönen et al. 1996, Ashmore 2005, and refs cited therein). Our results further suggest that predictions of EGS (from these methods at least) are not yet robust enough to warrant any conclusions as to the impact of climate change. Certainly, temperature alone is not driving EGS, and other factors such as light have an important role. This clearly warrants more study, but likely requires more advanced ecosystem modelling frameworks. We have, however, added additional explanations in the text to make it clear that these more complex methods are probably required for climate-change studies; the role of the T5 methodology is a first step towards quantifying the importance of using improved growing seasons.

Referee #2

Page 12157, line 24-26. Recent studies on BVOC emission suggest that temperature is not the only important parameter that should be taken into account (Baghi et al., 2012). These recent studies from Boulder suggest increased emission during spring time cannot be explained by temperature alone but maybe related to flowering. If this pattern is a general pattern, then the statement on line 24-26 holds with respect to existing parameterisations in emission model like MEGAN but also that this way of simulation BVOC emissions needs to be reconsidered.

Reply Yes, this is fair point. We have modified this sentence with reference to the Baghi paper.

Minor comments related to figures and tables in the manuscript

Referee #2

Fig 4. The scatter plots have a striking feature: The points with SGS after day number 125 lie more or less on the a straight line in all scatter plots, while the points with early SGS have much larger scatter. It could be very useful with a map as supplement to one of the scatterplots that shows both the simulated and observed SGS. Probably the highest agreement is found in the Finnish region (high SGS numbers) and low agreement is found in central Europe- an area where birches are much less frequent compared to Finland. If so then this is also needs to be discussed as this to some degree is related to the limitation of the methodology.

Reply Fig. 2 here shows the difference (in days) between the estimated (T5) and observed SGS at all sites. These results demonstrate that discrepancies between the T5 methodology and the observations are not generally a function of latitude. Rather, proximity to the coast seems to be a clearer indicator or problems, for example at all the Irish sites, and the isolated site in northern France. We will add this Figure to the manuscript Supplementary information, along with some text to discuss these points.

Referee #2

Fig 7. The characters on the large legend on Fig 7a and Fig 7b seems a bit smaller than the ones on Fig 8a and b. Fig8a and b are more easy to read.

Reply We have improved the Figures.

Referee #2

I suggest that table S1 is moved from the supplementary information and directly into the paper as the table can form an important data set for the future model developments.

Reply We have moved the Table as requested.

References

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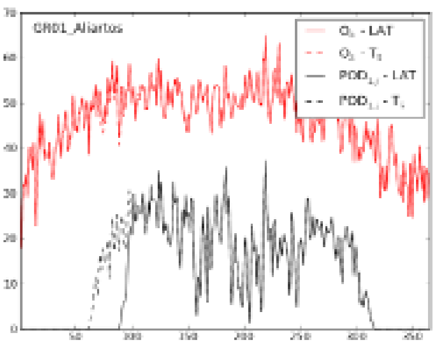

Figure 1: Calculated daily maximum ozone concentrations (at canopy top), and daily $\mathrm{POD}_{1,DF}$ values, for one site in Greece and one site in Sweden, using the base-case (LAT) SGS estimates and the T5-estimates.

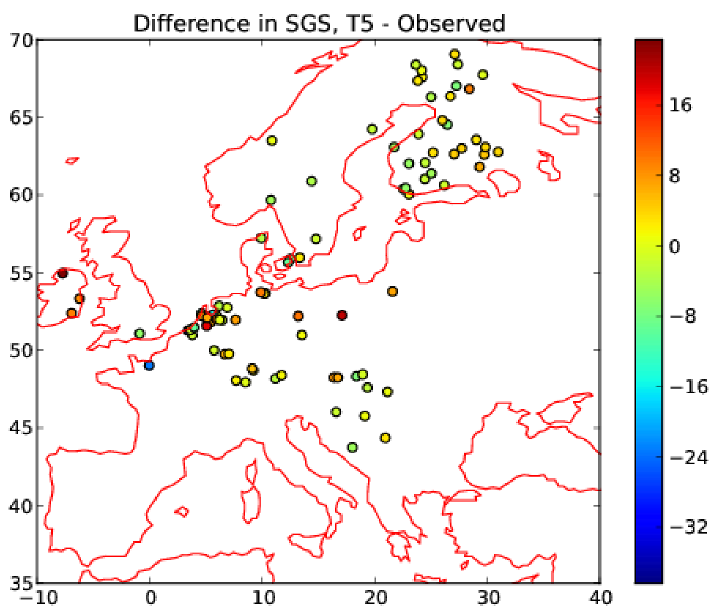

Figure 2: Difference (days) between T5-estimated SGS and observed values