Response to Anonymous Referee #2

We are pleased that the reviewer found our work to be interesting and worthy of publication. We are grateful for Referee #2's constructive comments and have addressed all the concerns raised to the best of our ability and revised the manuscript accordingly. Please see our item-by-item response to each comment and the full list of changes made in response to the reviewer's requests.

General comments

The authors mention the keyword climate change in the title but don't pick it up later in the manuscript until the summary where they refer to the companion paper by Duhl et al. 2013. I suggest adapting the title if possible to reflect this.

This paper focuses on describing the framework development for modeling pollen emission and transport on regional scales and on using measurement data from the 2010 Children Health Study (CHS) campaign over Southern California as a demonstration to carry out model evaluation. This is a critical first step toward quantifying the impacts of climate change on pollen concentrations since this model framework is sensitive to the meteorological factors (such as temperature, precipitation and wind speed) as well as the land use, both of which would change with climate. In the companion paper by Duhl et al. (2013), we demonstrated the framework's utility for climate impacts research by quantifying the pollen production potential under current and future climate scenarios using the dynamic downscaling results of GCM to drive the pollen potential model STaMPS. We give a brief description of the major results of that demonstration in the Summary section of this paper. We also discuss the vision of using this model framework for investigating the impact of climate change on allergic airway disease (ADD) in the abstract and introduction. Hence, we think it is appropriate to keep the word "climate change" in the title.

To address the reviewer's concerns, we have added a sentence to the end of the introduction to clarify the connection between this paper and the companion paper (Duhl et al., 2013). We have also revised the last paragraph of the Summary section to make it consistent with our revised companion paper. The revisions are:

1. At the end of the last paragraph of the introduction section, we have added the following sentence: "The framework can also be applied to estimate the changes in the timing of pollen seasons and the magnitude of pollen production for selected allergenic species over California under current and future climate scenarios; description and results are available in the companion paper (Duhl et al., 2013)."

2. In the last paragraph of the Summary section, we changed the sentence from "… its capacity by qualifying the pollen production potential difference under current and future climate scenarios." to "… its capacity by estimating the pollen production potential difference under current (1995-2004) and projected future (2045-2054) meteorological conditions." We also added the following sentence: "The future climate simulation is the WRF downscaling results of the ECHAM5 global climate model under the IPCC A1B scenarios". And we changed a sentence from "… will occur an average of 5-6 days earlier than the current scenario with the amount of pollen produced under the two scenarios varies by species" to "… will occur an average of 5-8 days earlier with 0.1-10% less pollen production amount than the current scenario, depending on the species considered."

It might be interesting to mention a further motivation for your research in the introduction, namely the fact that biological particles such as pollen can have an impact on atmospheric processes. If you do so, you may want to cite one of the papers in the following special issue of Biogeosciences http://www.biogeosciences.net/special_issue31.html

In response to the reviewer's suggestion, we revised the fourth paragraph of the Introduction section. The revisions are:

1. We changed the first and second sentence from "… the framework of sophisticated regional air quality models. The advantages of this method are twofold." to "… the framework of sophisticated regional air quality models, which has multiple benefits compared to the traditional approach."
2. We have added two sentences after "… under the same modeling framework": "Third, pollen grains can serves as cloud condensation nuclei (CNN) or ice nuclei (IN) for cloud formation and further change the precipitation pattern, which has important impacts on aerosols and related pollutants (Möhler et al., 2007). The new modeling framework has the potential to quantify the feedbacks between pollen concentration and radiative forcing."

Specific comments

p. 3980 lines 23-24: You write that the patterns of the pollen sources are based on vegetation distribution maps. However, it's not completely clear from which of the cited sources you take what distribution. Do the different maps differ strongly in species composition and vegetation cover? And if yes, did you for example take the distribution of one plant species from one map and that of a second species from another, or how were those data homogenized? Also, it would *be interesting to know how the uncertainties in those maps differ from those investigated in your simulations.*

As described in Section 2.1, starting from line 1 of page 3983, we used different datasets to determine the fractional vegetation within each model grid cell, including Forest Inventory & Analysis datasets (FIA) and Natural Resources Conservation Service datasets (NRCS) and the Cropland Data Layer from National Agricultural Statistic Service (CDL/NASS). At the end of that paragraph, we have added the following sentence in the revised manuscript: "The homogenization process for combining these different datasets is documented in detail in the companion paper (Duhl et al., 2013)."

Uncertainties associated with the datasets used to assign species composition to the domain cells (FIA, NRCS, etc.) may be significant but have not been quantified. Nor can they be quantified until there are relevant species-composition “truthing” datasets available to do so. The satellite-derived fractional vegetation cover estimates used to determine vegetation cover within the domain have much lower uncertainties (Duhl et al., 2012) but the accuracy assessment for the 2001 NLCD canopy and impervious cover datasets used to assign percent tree and non-tree vegetation cover to cells with non-urban land use types has still not been completed (http://www.epa.gov/mrlc/accuracy-2001.html).

In the revised manuscript, we have added following to the Summary section:

> With respect to uncertainty in the pollen emission potential, here we have only addressed the uncertainty associated with pollen pool size. As discussed in the companion paper (Duhl et al., 2013), the errors in speciation composition and in applying tree inventories for urban areas are potentially large but have not been quantified, nor can they be quantified until there are relevant species-composition ground-truth datasets available to do so. Thus, the uncertainty associated with pollen emission potential is potentially greater than those addressed in the sensitivity simulations performed here.

Chapter 2 and equation (1) p. 3982, line 20: You define the daily pollen emission potential as depending on precipitation and temperature. For some species however (e.g. the birch) the amount of pollen produced in one year depends also on the conditions of the previous season (“mast year”). Was this considered in the model somewhere?

This was indeed considered in the STaMPS model by introducing the chill-heating module of De Melo-Abreu et al. (2004) for the pollen species that have vernalization requirements (e.g., birch, olive, and walnut) and by accounting for precipitation rates of the prior wet season. This part of the model was briefly described in the last paragraph of the Section 2.1 in our manuscript and discussed in more detail in the companion paper

Duhl et al. (2013). To make it clearer that we have taken meteorological conditions of the prior winter and wet season into account, we have revised the last paragraph of Section 2.4. The paragraph now reads:

> The STaMPS model was run over the 4-km southern California domain (D2) for all cases and over the 12-km larger domain (D1) for the BCON sensitivity case (see Section 2.5 below). Even though the focus this study is March–June 2010, to simulate pollen potential P_a daily temperatures starting from six months prior and monthly precipitation starting from 15 months prior were used by STaMPS to model vernalization and chilling (vernalization) requirements and to model the effect of prior-year wet season precipitation on pollen potential (see Duhl et al., 2013 for details). For the October 2009–June 2010 period, daily temperature and precipitation fields from the WRF model were used to drive the STaMPS model. For computational efficiency, for the October 2008–September 2009 period 30-arcsecond PRISM monthly precipitation data (PRISM Climate Group, 2010) were used.

p. 3985, line 22ff: it would be helpful to also quickly mention here how the wet deposition process is treated.

At the end of the last paragraph of Section 2.2, we have added following description of the wet deposition process: "Pollen dispersion is also subject to wet deposition via cloud scavenging and precipitation. The algorithms for wet deposition processes in our framework are taken from the Regional Acid Deposition Model (RADM) (Change et al., 1987)."

p. 3989, line 24: how does this lumping work in the model? You don't have to describe it here but should refer to the companion paper by Duhl et al. so that one can look it up if necessary.

The lumping scheme for the oak genus is provided in the revised Table 1. The scheme works by separating the included oak tree species into two categories, one early-blooming and one late-blooming. The rationale for this adjustment was based on the bi-modal distribution of pollen counts observation over Southern California with one peak in March and early April and a second peak in May. The lumping was done by classifying the oak species present in the domain as 'early-blooming' and 'late-blooming' based on phenological observations and qualitative time-of-flowering data obtained from the NRCS USDA Plants Database.

To clarify the above, we have made the following revisions to the manuscript:

1. We changed the sentence reading "… was compared with observed pollen count data." to "… was compared with observed pollen count data to assess the general observed day-by-day temporal trend."
2. We changed the sentence reading "This comparison resulted in an adjustment made to the lumping scheme for the *Quercus* genus (i.e. earlier versus later-blooming oaks) … " to "This comparison resulted in an adjustment made to the lumping scheme for the *Quercus* genus by separating the considered oak species into early-blooming and late-blooming two groups (Table 1) according to the phenological observations and qualitative time-of-flowering data from NRCS dataset (Duhl et al., 2013)."
3. We revised the entry for oak species in the column 2 of Table 1 to make the format consistent with the companion paper (Duhl et al., 2013).
4. We changed the legend of Figure 4f to make it consistent with the description here.

p. 3992, line 2: I find an overestimation of surface wind speeds by 30% quite high! Wouldn't that be a problem when it comes to an accurate calculation of pollen emissions?

We used data assimilation- both analysis nudging at the outer domain and observational nudging at the inner domain- to get the best meteorological fields we could during the simulation period. The WRF model tends to overestimate surface wind speed, especially during calm wind conditions. WRF also still has similar difficulties as other regional meteorology models in fully capturing the dynamic over complex topography such as the modeling domain in southern California. Local mountain valley winds as well as land surface circulation can impact the synoptic wind field and make it harder to simulate.

In terms of the impact of the overestimated wind on pollen emission and transport, it was twofold. On the one hand, it tended to cause an overestimation of hourly emission flux since the simulated wind speed had a higher chance to exceed the threshold wind speed to cause pollen release. On the other hand, it tended to underestimate the ground-level pollen concentrations since higher wind speeds tend to distribute the pollen grains over longer distances.

In the revised manuscript, we have added following paragraph to the Summary section:

> The accuracy of simulated wind fields in WRF is one of the key factors for accurate pollen transport simulation. Data assimilation (both analysis nudging at the outer domain and observational nudging at the inner domain) was used in WRF to improve model performance. However, the model tends to overestimate the wind speed, especially during calm wind conditions. This leads to overestimation of hourly emission flux. On the other hand, higher wind speeds

> tend to distribute the pollen grains over longer distances and thus can lead to underestimation of ambient pollen concentrations.

p. 3993, line 8: Is the lumping scheme mentioned here the same as in the companion paper by Duhl et al. 2013 or a different one?

The lumping scheme referred to here is the same as the companion paper by Duhl et al. (2013). In order to eliminate ambiguity, we have revised the sentences describing the temporally-aligned pollen emission potential and observed pollen counts for oak pollen here:

> The temporal trend of P_a with the updated lumping scheme that separates oak species into early-blooming and late-blooming groups results in a much better agreement with the observed data (solid lines in Fig. 4f-1), compared to the results when all oak species are lumped into one group (dashed lines in Fig. 4f-1). This is consistent with the fact that the oak genus in the model contains several species with different thermal requirements for flowering (Table 1) (Duhl et al., 2013).

We have also changed the legend of Figure 4f to make it consistent with the description here.

p. 3994, line 24: If possible, give values for the simulated lifetime of pollen grains in the model.

The simulated lifetime of pollen grains should be inversely proportional to the settling velocity, but it also depends on the actual meteorological conditions. Sofiev et al. (2006) estimated the overall dry deposition velocity for birch pollen to be ~1 cm/s, which corresponds to a dry deposition half life of ~1 day in the atmosphere. For species studied here, such as olive and oak, the half life would be even shorter for the same meteorological conditions, only a few hours, since the calculated settling velocity is greater (Table 1).

To clarify this point, we have revised the manuscript to read: "Due to the relatively short lifetime of simulated pollen grains in the atmosphere- half lives of roughly a few hours to a day (Sofiev et al., 2006), the impact of pollen emission on a receptor is highly dependent on the PBL structure and meteorological conditions."

p. 3996, line 6: the observed peaks during March might have been missed if the meteorological conditions of the previous flowering season, which determine the current pollen production, haven't been taken into account.

Even though only four months (March to June 2010) of pollen emission and transport simulations were done using WRF-CMAQ, daily temperatures starting from six months prior and monthly precipitation starting from 15 months prior were used by STaMPS to model thermal and chilling (vernalization) requirements and to model the effects of precipitation on pollen potential. Please see our response to the previous comment on "mast years."

To make it clearer that we have taken meteorological conditions of the prior winter and wet seasons into account, we have revised the last paragraph of Section 2.4. The paragraph now reads:

> The STaMPS model was run over the 4-km southern California domain (D2) for all cases and over the 12-km larger domain (D1) for the BCON sensitivity case (see Section 2.5 below). Even though the focus this study is March–June 2010, to simulate pollen potential P_a daily temperatures starting from six months prior and monthly precipitation starting from 15 months prior were used by STaMPS to model vernalization and chilling (vernalization) requirements and to model the effect of prior-year wet season precipitation on pollen potential (see Duhl et al., 2013 for details). For the October 2009–June 2010 period, daily temperature and precipitation fields from the WRF model were used to drive the STaMPS model. For computational efficiency, for the October 2008–September 2009 period 30-arcsecond PRISM monthly precipitation data (PRISM Climate Group, 2010) were used.

p. 3997, chapter 3.3.2: I guess that another possible reason in the overestimation of pollen might lie in equation 2 which says that the lower a species' canopy height, the higher its escape fraction is. As grass has a very low "canopy", the escape fraction will be high and the emissions might thus get overestimated.

The fact that sensitivity simulations by varying the emission pool size estimation, which directly varies the emission rates, did not significantly improve the overestimation of grass pollen concentration over the domain suggests the possible deficits for geographic representation of gridded pollen production in the STaMPS model. Please also see our response regarding Equation (2) below.

p. 3998, chapter 3.3.3: The problem with the too low emission values for walnut trees might have the same reason as before. Equation 2 states that trees with a high canopy, such as walnut, have a low escape fraction and thus their emission might get underestimated.

Please see our response regarding Equation (2) below.

p. 3999 and 4000, chapter 3.3.6: After reading the discussion of the pollen concentrations for different species, I'm wondering if equation 2 needs to be adjusted somehow. After all, it seems to predict that plants with a higher canopy will have lower emissions. This is actually counterintuitive, as I would expect pollen emitted for a higher source to travel further and thus have a higher escape fraction. Could you please clarify this?

The parameterization scheme for the pollen emission flux given in Equation (2) was first introduced by Helbig et al. (2004) and is based on friction velocity, which has been widely used in other regional modeling studies (Sofiev et al., 2006; Vogel et al., 2008, Efstathiou et al., 2010; Zink et al., 2012). In Equation (2), P_a/H_s is the characteristic concentration (grains m^{-3}) of pollen within the canopy, and is essentially a pollen potential conversion factor between grains per land surface area and grains per canopy volume (see Equation (2) of Helbig et al., 2004). Equations (2) and (3) indicate that the actual amount pollen released each hour depends on the ratio of the threshold friction velocity and the friction velocity. In this formulation, for a given 10-meter wind speed, higher canopy height H_s implies a higher emission rate because the friction velocity is higher given that surface roughness and zero-plane displacement height are higher for higher $H_{s.}$

In order to clarify this point, we have revised the sentence after Eq. (2) as "… where the constant, C, is the conversion constant from day to seconds, and H_s (m) is the average canopy height for pollen from each genus. Values of H_s used in this study are listed in Table 1. In Equation (2), P_a/H_s represents the characteristic concentrations (grains m^{-3}) of pollens within the canopy." Later in the same paragraph we also added the following two sentences: "Equations (2) and (3) indicate that the amount of pollen released each hour depends on the ratio of the threshold friction velocity and the friction velocity. For a given 10-m wind speed, higher canopy height means higher emissions because of higher friction velocity as a result of higher surface roughness and zero-plane displacement height."

p. 4010, Table 1: it would be very interesting to see how the settling velocity of the different pollen types differs from model to observation. If such data for comparison are available, it would be good to include them. As the model idealizes pollen grains as smooth spheres while in nature they are irregularly shaped, I would expect different values.

The reviewer noted an important point that pollen grains are not smooth spheres but irregularly shaped. In the revised manuscript, we have added a column to Table 1 listing the measured sedimentation velocities reported in Jackson and Lyford (1999). The difference between our calculated settling velocities and the measured velocities (if available) are within ±10%, which is less than the ±20% in V_{dp} we performed in our sensitivity simulations ('DVHI' and 'DVLO' in Table 2) by varying the mean diameters by ±10%.

Technical comments

p. 3979 line 16 and 25: correct "Despés" to "Després"

This has been corrected.

p. 3987 line 11: correct "participant" to "participants"

This has been corrected.

p. 3988, line 17 (and all following instances throughout the manuscript): write "simulations" instead of "runs"

This has been changed throughout the manuscript.

p. 3988, line 24: to avoid confusion, mention that "YSU scheme" stands for "Yonsei University scheme" as not all readers might be familiar with the abbreviation.

We have changed the wording as suggested.

p. 3990, line 25: for a better legibility, rephrase the sentence so it doesn't begin with "10%..."

We have revised this sentence to read: "The estimated mean diameters in Table 1 for each pollen genus were varied by ±10% for the two sensitivity simulations, …"

p. 4001, line 23: correct "maximums" to "maxima"

This has been corrected.

p. 4004, line 31: change the citation of the Duhl et al. paper from submitted to already in discussion (Geosci. Model Dev. Discuss., 6, 2325-2368, 2013).

We have updated the citation.

p. 4009, line 11: As already mentioned by Katrin Zink, the citation of Zink et al., 2011 should be updated to Zink et al., 2012.

We have fixed the citation.

p. 4017, Figure 4: The labels of the axes in this figure are very hard to read due to their size, but this might be also a result of the typesetting in the discussion format of the journal. Just make sure that this is corrected for the final version.

We re-plotted this figure to make sure that the labels of axes are easy to read.

p. 4021, Figure 8: Same as above, labels are too small.

This has been corrected.

p. 4022, Figure 9: Same as above, labels are too small.

This has been corrected.

References:

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