Response to Dr. S. M. Burrows

We thank Dr. Burrows for her carefully reading our manuscript and providing constructive comments. We are pleased that she has recommended it for publication in *Biogeosciences*. We have revised the manuscript based on the comments of Dr. Burrows, the second reviewer, Dr. Zink, and Dr. Sofiev. We believe the manuscript is much improved as a result of these revisions. In this response, we address the specific concerns raised by Dr. Burrows.

This manuscript deals with an important and interesting topic: the simulation of regional-scale pollen dispersal in a highly populated region of Southern California.

Pollen allergies are among the most common respiratory allergies, and they are likely to become an even greater concern as the incidence of allergies rises. Studies such as this one could potentially be used to guide public health efforts to reduce exposure to allergens. In addition, such a model framework could be used to study the interactions of pollen and air pollution, which may cause greater incidence of allergies.

The methods in the paper are generally sound, the study is clearly presented, and the authors have carefully treated several important sources of pollen. To my knowledge, there is not yet a well-established method for treating pollen emissions in regional fore- casting models. This study represents a further development of a parameterization proposed by Helbig et al. (2004), where the pollen emission potential has been more explicitly modeled as a function of meteorological variables. There are large uncertain- ties in the emissions parameterization, but the authors have acknowledged this, and it can be expected that the emissions parameterizations can be improved with further research. Given the complexity of the problem, it is to be expected that an initial at- tempt to build such a model will have both failures and successes, and the authors have carefully discussed these and pointed out some promising avenues for potential improvements.

As a final note, this manuscript relies heavily on the STaMPS model for regional pollen production. The STaMPS model has been described in a paper that is listed in the References as "submitted". Since STaMPS is an important foundation of this manuscript, it would be preferable if at least a discussion paper describing it were already available before the final version of this manuscript is published.

> We have revised the manuscript to include the full reference to the paper on STaMPS, which is now available on *Geoscientific Model Development Discussion* at:
> http://www.geosci-model-dev-discuss.net/6/2325/2013/gmdd-6-2325-2013-discussion.html

Duhl, T. R., Zhang, R., Guenther, A., Chung, S. H., Salam, M. T., House, J. M., Flagan, R. C., Avol, E. L., Gilliland, F. D., Lamb, B. K., VanReken, T. M., Zhang, Y. and Salathé, E.: The Simulator of the Timing and Magnitude of Pollen Season (STaMPS) model: a pollen production model for regional emission and transport modeling, Geosci. Model Dev. Discuss., 6, 2325–2368, doi:10.5194/gmdd-6-2325-2013, 2013.

General comments:

1. *Pollen size and settling velocity: The settling velocity of a pollen grain may differ significantly from the velocity calculated from Equation 6, because pollen grains are not smooth spheres. Many have very distinctive shapes, some of which are adapted to the function of increasing the probability of lofting or the time aloft. From the text, it seems that the values for the settling velocity in Table 1 were calculated from Equation 6, and that these values of the settling velocity were used to calculate dry deposition in the model. Please clarify this, and please also discuss how large the differences are between pollen settling velocities as observed and as predicted from pollen grain size. Could pollen clumping also modify this relationship? If I understood correctly, pollen grains are assigned a single diameter in the model (they are monodisperse)? Please clarify.*

As the reviewer correctly pointed out, pollen grains are seldom perfectly spherical but have different shapes. The diameter and density of the studied pollen grains listed in Table 1 are the representative values reported in the literature. The values for the settling velocity in Table 1 are indeed calculated from Eq. (6) using the diameter and density values listed in the same table. We have added text in the revised manuscript below Equation (6) to make this more clear: "For each genus modeled here, we assume monodisperse particles, with particle diameters and densities based on values reported in the literature; the assumed particle diameter d_P, density ρ_P, and settling velocities V_{dp} as calculated by Equation (6) are provided in Table 1. Table 1 also lists the measured settling velocity reported by Jackson and Lyford (1999). Uncertainty in model results associated with deposition velocities is addressed with sensitivity simulations (see Section 2.5) and discussed in Section 3.3." We also added a footnote in Table 1 to indicate that the settling velocities are calculated results using Eq. (6) and the provided mean density and diameter of each pollen genus.

We have also added the still-air measurements of the sedimentation velocity from Jackson and Lyford (1999) to Table 1. From the revised Table 1, it can be seen that the differences between the calculated settling velocities and measured ones (if available) are normally within ±10%. In our study, only a single diameter value was assigned to each genus in the model. It is expected that changing the lumping scheme could cause variation of this mean diameter value since the species within each lumped group might have different morphologies. However, this variation is hard to quantify given the current state of knowledge. Note for our sensitivity simulations ('DVHI' and 'DVLO' in Table 2), we modified by particle diameter by ±10%, which corresponds to ±20% in V_{dp}.

2. Discussion of uncertainties: p. 3990, lines 8-12: How large (roughly) are the uncertainties in species composition and fractional vegetation cover expected to be? On p. 3980, lines 22-24, you state that the vegetation distribution maps are "subject to large uncertainties". Do you expect these to be larger or smaller than the uncertainties you investigated in your sensitivity simulations? What about uncertainties in the pollen grain size and/or settling velocity?

This issue was discussed in detail in the companion paper (Duhl et al., 2013). For the reviewer's convenience, we present that information here as well.

Addressing the uncertainties in species composition quantitatively requires an estimate of the uncertainty from each source of species composition data that was used. For the urban tree distributions, we only had municipal tree species inventories and only for a handful of communities. Cells containing urban tree cover located in the southern half of California were assigned average weighted species compositions based on inventories for Long Beach, Riverside and Los Angeles, while those located in northern California and southern Oregon were assigned weighted averages based on inventories from Berkeley, San Francisco, and Modesto. All urban cells located in Nevada and its neighboring regions to the north and west were assigned the weighted average species composition computed using a Reno inventory. Several issues arise from city-based species composition assignments. For example, how representative is assigning a few inventories to an entire region? One way to address this is to look at how much the inventories within a region differ from each other. The table below shows a comparison tree species composition between the communities of Berkeley and San Francisco, in CA:

Common Name	Sci_name	San Francisco %	Berkekey %	Pct_stdev
Monterey cypress	Cupressus macrocarpa	3.8	1.0	80.9
Olive	Olea spp.	3.1	0.3	119.3
London planetree	Platanus acerifolia	2.1	7.4	79.1
Oak	Quercus spp.	0.5	8.2	125.2
Coast live oak	Quercus agrifolia	0.1	6.4	137.1

Even between neighboring communities, there are substantial differences in tree composition (for municipally-maintained trees only). In addition to these differences there were also many species present in the Berkeley inventory that were not present at all in the San Francisco inventory; this is typical of the differences observed between other communities as well.

In addition to the uncertainties of assigning these inventories to whole regions, there is also the question of how representative the tree inventory is of the actual urban canopy. Our analysis in section 3.3.3, for example, suggests that either the pollen production capacity (ε_{sp}) assigned to walnuts was too low, or that walnut trees may be more prevalent within the domain than indicated from the land cover data available for the domain, or both. While agricultural walnut orchards are known to be present within the study region (mostly within California's Central Valley), urban tree inventories suggest that walnuts comprise <0.1% of urban trees. Urban tree inventories generally only include trees maintained by municipalities (such as trees growing along city streets). If private property owners favored walnut trees on their properties more than municipalities, the actual fraction contributed to the total urban canopy by walnuts would be higher than indicated by the inventories. To evaluate these potential differences quantitatively would require a detailed urban tree assessment. We know of only one study that actually sought to characterize tree composition in an urban location (Hope et al., 2003), and in that case there is no comparable tree inventory that would make a comparison possible. Suffice it to say the uncertainties are probably >100% in urban species composition alone.

Uncertainties associated with the other datasets used to assign species composition to the domain cells (FIA, NRCS, etc.) may be significant but have not been quantified. Indeed they cannot 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 (most likely these are within 15 %; Duhl et al., 2012). However, the accuracy assessment has still not been completed for the 2001 NLCD canopy and impervious cover datasets that were used to assign tree and non-tree vegetation coverage levels to cells with non-urban land use types (http://www.epa.gov/mrlc/accuracy-2001.html).

Finally, there is the question of how accurate the potential production capacities assigned to the species or genera included in the simulations were. The ε_{sp} coefficients assigned to each species were derived from the literature, and there are large ranges in observed pollen production for some genera, e.g., three orders of magnitude for different walnut cultivars (Sütyemez, M., 2007). It is difficult to determine whether differences between simulated and observed pollen concentrations arise from uncertainties in species distribution data, incorrect pollen production coefficients, or from observationally-derived errors. Despite these limitations, our study is the first to predict pollen production for multiple species in the western half of the US, and therefore STaMPS improves capabilities for predicting pollen season in the long term. Nonetheless, these limitations should be kept in mind when interpreting the pollen dispersal results.

Since this discussion is included in the companion paper (Duhl et al., 2013), we have chosen not to repeat it our manuscript. Instead we have revised the Summary section to include the following text: "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."

3. Influence of canopy/release height: p. 4001, lines 24-26: could the overestimation of grass pollen and the underestimation of walnut and oak perhaps be related to their canopy heights and the parameterization of the "escaped fraction" (i.e. Eq. 2)? I notice that grass pollen has the lowest canopy height and oak and walnut have the highest canopy heights of all genera considered in this study (Table 1). According to Eq. 2, the model calculates lower emissions for species with higher canopy height. The reasoning seems to be that a higher canopy will more effectively prevent emissions? But what if the opposite is the case: a higher canopy also means that pollen grains are released higher above the ground, and so they might have the potential to be transferred farther before settling or impacting onto a surface. A pollen grain released from the top of an oak tree surrounded by grass must travel farther than one released from the grass, but Eq. 2 predicts the opposite.

The parameterization scheme for the pollen emission flux 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 Eq. (2) P_a/H_s is the characteristic concentration (grains m^{-3}) of pollen within the canopy, and is essentially a conversion factor between grains per land surface area and grains per canopy volume. Eq. (2) in combination with Eq. (3) indicates that the actual 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.

In order to clarify this point, we have included additional text in Section 2.2 of the revised manuscript. We have added the following sentence: "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 pollen of 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."

Technical and minor comments:

p. 3978, line 13: "representation of vegetation"

This has been corrected.

p. 3984, Eq. 2: is this equation applied per genus, i.e. Hs is a genus specific value here (average of the species–specific values?)

Eq. 2 is applied for each pollen genus, namely the five tree and one grass genera listed in Table 1. H_s here is the genus-specific canopy height of species considered in each genus, as documented in Table 1. For clarity, we have changed "… and H_s (m) is the average species-specific canopy height" to "… and H_s (m) is the average canopy height for species within each genus. Values of H_s used in this study are listed in Table 1."

p. 3985, lines 7-8: please rephrase sentence so that it doesn't begin with a symbol

We have revised this sentence from "α is introduced here to distinguish the different nature of sand erosion on the ground and the pollen release above the canopy height" to "A resistance factor factor, α, is introduced here to distinguish the different natures of sand erosion on the ground and the pollen release above the canopy height."

p. 3985, lines 22-23: The authors write that "The dry deposition ... is treated by calculating the settling velocity", but dry deposition can also be influenced by factors such as surface roughness. Is this taken into consideration? I recognize that these processes are calculated in CMAQ and are not the main focus of this study, but a brief explanation would be helpful. Similarly, please briefly describe the wet removal processes (in particular, are these also size-dependent?) For pollen, dry deposition is likely the dominant loss process, but wet deposition could still play an important role, so I think it should be described a bit.

Due the large diameter of pollen grains, the sedimentation of particles would be the dominant factor controlling the deposition rate. Hence, we treated the average settling velocity of each pollen genus as the dry deposition rate and followed the Regional Particulate Model (RPM) algorithm (Binkowski and Shankar, 1995) to calculate the hourly dry deposition flux. We added a sentence to explain the method used to calculate dry deposition: "Due to the relatively large diameter of pollen grains, the contribution of canopy resistance to bulk dry deposition rate is small." Also, in the last paragraph of Section 2.2, we added following information regarding wet deposition: "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. 3985, Eq. 5: maybe this is explained in Helbig et al., but it would help to have a brief explanation in a sentence or two of this equation. Why should it have this particular form, how is the value of U_{10e} determined? I believe U_{10e} is a single constant value (the same for all plant types? Should it depend on canopy height?); but this could be explicitly noted here.

We have added the following to the revised manuscript:

> "The resistance factor, α, is introduced here to distinguish the different natures of sand erosion on the ground and the pollen release above the canopy height. It is a ratio of an empirical threshold wind speed, U_{10e}, and the modeled 10-meter wind speed, U_{10}:
>
> $$\alpha = U_{10e}/U_{10} \tag{5}$$
>
> For the base case, the value of U_{10e} was set at 2.9 m s^{-1} for all pollen genera following Helbig et al. (2004). As discussed below and listed in Table 2, we also performed sensitivity simulations using different values of U_{10e}."

p. 3985, line 24: insert "the" before "slip" and "viscosity"

We have corrected this in the revised manuscript.

p. 3985, line 25: "The hourly ..."

This has been corrected.

p. 3986, line 12: "climate change on air quality"

This has been corrected.

p. 3987: "levels of 950 child participants…"

> This has been corrected.

p. 3989, line 6: "while the horizontal"

> This has been corrected.

p. 3990, line 2: "sensitivity runs" -> "sensitivity simulations" (here and wherever else this occurs)

> We have changed "sensitivity runs" to "sensitivity simulations" throughout the manuscript

p. 3990, lines 4-6: Does this refer to boundary conditions for the pollen concentrations? This is what I infer from Table 1, but it is unclear here.

> The reviewer is correct that this refers to the boundary conditions for pollen concentrations. We changed those two sentences to read:
>
> > The first set of sensitivity simulations ('BCON') was designed to test the impact of boundary conditions on the simulated pollen concentrations. In this case the pollen concentrations from the outer domain were also simulated by CMAQ to provide dynamic boundary conditions for the inner domain instead of using zero values.

p. 3990, line 19: "wind conditions"

> This has been corrected.

p. 3990, line 25: please rephrase sentence so that it doesn't begin with a number or symbol; "mean diameters"

> 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 runs, …"

p. 3991, line 28: "show" -> "shows"; perhaps delete "pretty"

> This has been corrected.

p. 3992, line 16: "domain-wide" -> "domain-averaged" (I think this is what is meant?)

This has been corrected.

p. 3992, line 17: "tree"-> "trees"

This has been corrected.

p. 3993, line 1: "concentration" -> "concentrations", "single site, CALT,"

This has been corrected.

p. 3993, line 10: "level" -> "levels"

This has been corrected.

p. 3993, line 12: "a new species lumping scheme was developed" – new compared to what previous scheme? Are both schemes introduced in Duhl et al. (2013, submitted)? Would there be a more descriptive way of naming these schemes, something along the lines of "with and without separation of early bloomers" rather than "new and old"? (similarly in Fig. 4f-2).

The schemes we present are the same as those in the companion paper by Duhl et al. (2013). To remove ambiguity we have made these changes:

1. We rewrote the sentences to describe 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)."
2. We listed the oak species in the column 2 of Table 1 to be consistent with the format of the companion paper (Duhl et al., 2013).
3. We also changed the legend of Figure 4f-1 to make it consistent with the description here.

p. 3994, line 24: "Due to the relatively short lifetime of pollen grains in the atmosphere ..." – about how long are the simulated lifetimes of the pollens in this model?

The simulated lifetime of pollen grains should be inversely proportional to the settling velocity but also depends on the actual meteorological conditions. Sofiev et al. (2006) estimated the overall dry deposition velocity for birch pollen will be about 1 cm/s, which is comparable with the species with a half life of ~1 day in atmosphere due to dry deposition. For species studied here such as olive grain and oak grain, the lifetime should be even shorter under the same meteorological conditions because the calculated settling velocity is even higher (Table 1). Hence we reword this sentence as:
"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. 3997, lines 16-17: if there are spikes in the concentration due to a nearby source, wouldn't these influence the observations as well as the model?

The WRF model does not always accurately capture the surface wind fields, especially under calm conditions. Due to the heterogeneity of simulated grass pollen emissions, the wrong wind direction prediction may result in a nearby source impacting a receptor site causing an instantaneous spike in the simulated time series that does not represent actual conditions.

p. 3999, line 19: "trend" -> "relationship"

This has been corrected.

p. 4000, line 1: "based on a dataset"

This has been corrected.

p. 4001, line 4: maybe "Pollens" -> "Pollen grains"

This has been corrected.

p. 4001, line 23: "maximums"-> "maxima"

This has been corrected.

p. 4002, line 3: "by the MEGAN/STaMPS model"

This has been corrected.

p. 4002, paragraph beginning line 14: here you discuss some limitations of the model and missing processes. It might also be appropriate to mention uncertainties in the pollen size distribution more generally, not only the uncertainty arising from not considering pollen fragments. Since the dry deposition rate is quite sensitive to particle size and settling velocity, how much does it matter that you use only a single average value for each genus (and apparently a monodisperse aerosol)? Is pollen clumping a possible problem? This is addressed in part by your sensitivity tests, but I think it is worth adding a few sentences. Also, it is worth mentioning wet removal here: rainfall location and timing is difficult to predict over uneven terrain such as the Sierra Nevadas, but a single rain event could effectively wash out most pollen from the air. So, uncertainties in rainfall prediction are another important limiting factor.

We have integrated the above comments into the limitation of the model discussion by adding the following sentences in the revised manuscript:

> The uncertainly in pollen size distribution and its impact on bulk dry deposition rate is not fully addressed in this study. Pollen grains are not ideal spheres. Many have very distinctive shapes, some of which are adapted to the function of increasing the probability of lofting or the time aloft. A single average value for the diameter and density for all the lumping species under each genus may not be representative.

Later in the same paragraph we also added the following:

> The uncertainties in rainfall prediction are another potentially liming factor. The location and timing of rainfall is difficult to predict especially over the complex terrain of southern California; a single rain event could effectively wash out most pollen from the air.

p. 4002, line 25: "trigger" -> "triggering"

This has been corrected.

Figures and Tables:

Several of the figures have labels that are very small and may be difficult to read, especially Fig. 4, Fig. 8, Fig. 9. This might be corrected simply by printing the figures in a larger format in the final typeset version, but please check this.

We have re-plotted these figures to ensure the labels of axes are legible. Further issues will be addressed at the proofing stage as needed.

Tab. 1: as noted above, these seem to be quantities calculated from the diameter, but the actual settling velocity may differ. It might be worth noting in a footnote that this is the case, i.e. that these are calculated from the diameter.

> We have added a footnote to Table 1 stating that the settling velocities are calculated results using Eq. (6) and the mean density and diameter of each genus.

Tab. 2: "compare"-> "compared" (four times in second column); there is a formatting problem in the third column which can be solved in LaTeX by using the command.

> We have changed "compare" to "compared". The formatting problem in the third column only appeared in the conversion to LaTeX. We will make sure that this is corrected in the final version.

Fig. 10: Please provide more information about the back-trajectories in the caption (e.g. "three-day back trajectories from HYSPLIT"). I'd also suggest writing something like "with pollen concentration boundary conditions from outer domain simulation (BCOND) and without (BASE)". Also, the bars in the right panel are difficult to distinguish. A different choice of colors could help, but it might be simpler to use a line or point graph instead.

> We have changed the caption of Figure 10 to "Sensitivity study for oak pollen simulation during the episode around March 27th, 2010 at Pasadena CA, with (a) three-day back trajectories from HYSPLIT; and (b) model evaluation with pollen concentration boundary conditions from outer domain (BCON) and without (BASE)." Fig. 10b has been revised using and the color scheme adjusted to be consistent with Figs. 8 and 9.

References:

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