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Geoscientific Model Development Discussions
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Interactive comment on "The Simulator of the Timing and Magnitude of Pollen Season (STaMPS) model: a pollen production model for regional emission and transport modeling" *by* T. R. Duhl et al.

Anonymous Referee #2

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General comments:

The publication addresses an important issue in the context of the simulation of pollen concentrations, a topic that has become more and more important during the last years. In order to simulate the dispersal of pollen grains using numerical weather prediction systems, a model describing the beginning, end and course of the pollen season is essential. The present publication describes such a model for several tree and grass species. The presented model is used to investigate the influence of climate change on the start of the pollen season and the potential pollen production.

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Interactive Discussion
Discussion Paper

The topic of the publication is highly interesting and important in the context of numerical predictions of pollen concentrations. However, the publications shows some methodological drawbacks that should be addressed before publication.

First, I am missing a section describing the materials used. The data is introduced in the sections where it is used for the first time. It would be easier to have an overview at the beginning of the paper (after the introduction).

Second, apparently the STaMPS model was validated only indirectly by incorporating its output into a transport model. The resulting pollen concentrations are compared to count data. However, the simulated pollen concentrations not only depend on the output of the STaMPS model, but also on the emission parameterization and the transport/diffusion processes within the transport model. No numbers are given with respect to the accuracy of the STaMPS model regarding current climatic conditions. Cf. eg. Pauling et al. 2013, Aerobiologia, for phenological model performance assessment. In addition, this publication contains scores that can be used for comparison. It would be good to know how well the model can predict the beginning and magnitude of the pollen season. This should be done before using the model with a future climate. Lacking such a validation with current data, I think that it is not justified to make predictions in the future.

Third, I think that the chosen simulation period is not valid to support the interpretations drawn from the results. Even the authors admit that the differences in the potential pollen production between current and future climatic conditions could be due to a shift of the pollen season into or out of the simulation period. However, this is not further investigated. Hence, I recommend to use a more suitable simulation period.

Forth, the authors state that their model is flexible regarding different pollen species. But: for many species, they use identical formulations/fixed values with the hint that appropriate data for the given species is not available. Hence, the nature of the model might be flexible, but the use of identical parameters corrupts this flexibility. Especially

GMDD

6, C1019–C1026, 2013

data on birch seems to be scarce in Southern California since almost always data of other tree species is used for birch. I doubt very much that, e.g., for birches and olives the same parameters should be used since these trees primarily do not grow in the same climatic regions.

Overall, the paper leaves the impression that too many issues were tackled at the same time. The paper does not only present a phenological model describing beginning, end and course of a specific pollen season, but does so for several different taxa. Additionally, distribution maps for each of these taxa are generated. This paper could easily be divided into 2 or even more papers: one paper about the phenological model (or even one paper for each taxa including a thorough validation for each taxa) and one paper about the generation of distribution maps.

Concluding, I recommend publication after major revisions.

Specific comments:

Introduction: I am missing a few sentences about existing models for the timing and magnitude of pollen seasons and their performance. The introduction presents the motivation for the study and a summary of the paper, but is missing a paragraph about the scientific context (with relevant reference).

Page 2330, lines 1-2: what are the criteria for the selection of the species based on pollen count data?

Page 2330, lines 13-16: In some species (e.g. birch), the magnitude of pollen produced is not only a function of the meteorological conditions in the given season, but also depends on the previous season (the concept of masting). It is not clear whether this fact is included in the model or not.

Page 2331, lines 16-17: The authors exclude the year 2007 because of late-season rains. However, the section 2.1 addresses the prediction of the start of the pollen season. I don't see why late-season rains disqualify the year 2007 for the prediction of

GMDD

6, C1019–C1026, 2013

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the start of the season which should be before the rainy late-season period.

Page 2331, lines 6-13, lines 23-24: Needs more justification how the specific base temperatures and start-dates for heat accumulation were chosen.

Page 2332, line 1: The GDD thresholds are taken as the average GDD values that were reached on the day of the peak pollen concentrations. However, the peak pollen concentrations surely do not reflect the start of the pollen season. The start of the pollen season is a phenological date which depends on the meteorological conditions of previous days/months. What is the definition of the start of the pollen season used in the present study? The peak pollen concentrations depend on the current weather conditions and usually appear during the main season several days after the start of the pollen season. It should also be kept in mind that the start of the pollen season and the peak pollen concentrations could be a consequence of long-distance transport, not being related to the start of local flowering at all. Data should be corrected for these influences as far as possible.

Page 2332, lines 25-29, and page 2333, lines 1-2: It is mentioned that a 'variation-mimicking' parameterization has been developed and applied to several species. Details about this parameterization are not given. If I understand correctly, the parameterization normally distributes the available pollen over a period of 2 weeks. The authors state that such a behavior has been observed for a number of species, corresponding data is not shown. Looking at the pollen seasons in Europe, I cannot support the observation that typical pollen seasons have a length of only 2 weeks. For birch, e.g., the pollen season has been observed to be positively skewed instead of normally distributed (see e.g., Grewling et al. 2012, Grana). Additionally, it should be taken into account that the length of the pollen season is influenced by the weather conditions, e.g. cold conditions tend to result in a longer than usual pollen season. As I understand from the paper, this is not taken into account.

Page 2333, lines 3-15: Instead of using TB values of another species, a species-

GMDD

6, C1019–C1026, 2013

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specific TB value for the studied region could be found by systematically varying the base temperatures and the starting date of the accumulation. Otherwise, is there a justification why the values of another species are valid for the given species?

Page 2334, lines 22-25: Any justification why you use the olive value for birch instead of the walnut value?

Page 2335, lines 3-6: Maxima/minima in pollen concentrations can have different reasons: e.g., precipitation events washing out the pollen in the air, turbulence and wind strengthening/weakening the emission and diffusion of pollen, long-distance transport, variations between individual trees. I find it implausible to assign observed maxima in airborne pollen concentrations mainly to different olive cultivars and individual thermal requirements.

Page 2337, lines 2-6: Relevance of these remarks? Should be part of the section 'Future plans'.

Page 2338, lines 16-19: Is it wise to use model precipitation to construct the relationship between precipitation and the pollen potential? It is well known that precipitation is one of the parameters that are usually not very well simulated in models. I suggest to use observations for that purpose.

Page 2338, line 22: Which other trees? Is the function also based on data of these other trees or is it taken from oak data? If taken from oak data: justification?

Page 2339, lines 22-25: Justification for neglecting the influence of precipitation on the length of the flowering season? Although the simulated grass species were not included in the mentioned paper, it is very plausible that the discovered influence also plays a role here.

Page 2340, line 10: Please add the p-value of the correlation.

Page 2340, lines 12-14: How did you calculate this?

GMDD

6, C1019–C1026, 2013

Section 2.4: I am missing the details: how is the fractional land cover assigned (percentages used for each class!), what is used for the weighting, horizontal resolution of the data sets. . .overall, I do not understand how the fractional land cover was produced. Maybe a flow chart would clarify the process? It would also be good to compare the methods with the literature about vegetation cover estimation (e.g., Sofiev et al. 2006, Int. J. Biometeorology, or Skjoth et al. 2008, Ecological Modeling, or Skjoth et al. 2010, Agricultural and Forest Meteorology, or Pauling et al. 2012, Int. J. Biometeorology).

Page 2342, lines 18-23: This is not necessary here, already described in formula 3. Section 2.5: Before applying the model on future climate, I would expect some sort of validation using current climate.

Page 2344, lines 10-17: How well works the model for current climate? In order to interpret results for future climate, it is essential to know how precise the timing and magnitude of the pollen season can be calculated under current climate.

Page 2345, lines ∼20-end of paragraph: Maybe the simulation period should be extended to represent the entire pollen season? It would help to interpret the results if the entire season was inside the simulation period!

Page 2346, lines 5-14: To evaluate the model, STaMPS output was incorporated into a transport model and simulated pollen concentrations were compared to pollen count data. The reader is referred to a companion paper for the details. However, I think incorporating the model output into another model and comparing the output of the second model to observational data is not a good way to validate the first model. Resulting pollen concentrations do not only depend on the output of the STaMPS model but also on the emission parameterization and transport/diffusion processes in the transport model. If available, it would be nice to compare the STaMPS output directly to phenological data of the start of flowering.

Page 2347, lines 18-24: I totally agree! Please justify why you did not simulate the entire season. Regarding the limitations (not representing the entire pollen seasons), I

GMDD

6, C1019–C1026, 2013

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am not sure what we can learn from the study about the future.

Page 2349, lines 8-9: No, the STaMPS model has been designed to simulate the timing and potential magnitude of the pollen season. The release of the pollen is simulated in the transport model (see companion paper) using an emission parameterization respecting the influence of wind.

Figure 3: What is “Days to flower” (y-axis)? The beginning of the pollen season? The length of the pollen season? If it is the beginning: what is the initial date? When does the counting of the days (x-axis) start? What is 7°C? Mean/min/max temperature?

Figure 4 and 5: What is the p-value of the correlation?

Technical corrections:

Page 2333, lines 20 + 23: (Betula, Juglans, and Olea) . . .(olive, walnut, and birch): unnecessary repetition, additionally it would be better to use either Latin or English words, not a mixture.

Page 2334, line 7: Losing a negative value (-0.56 chilling units) results in a net gain: double negative = positive. . .in my opinion, it should be: . . .above which 0.56 chilling units are lost.

Page 2335, lines 24-30: illogical use of the words ‘quantitative’ and ‘qualitative’: for me, the fraction of heads flowering is quantitative as it gives the amount of potentially available pollen grains.

Page 2339, formula 5: the precipitation-driven coefficient is now called gamma. Before, in formula 3, it was called alpha. There, gamma was the fraction of land covered with the specific plant.

Page 2349, line 22: Artemisia instead of Artemesia.

Table 1: phenological instead of phonological

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6, C1019–C1026, 2013

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Interactive comment on Geosci. Model Dev. Discuss., 6, 2325, 2013.

GMDD

6, C1019–C1026, 2013