Geosci. Model Dev. Discuss., 6, C1960–C1975, 2013
www.geosci-model-dev-discuss.net/6/C1960/2013/

Geoscientific Model Development Discussions
Open Access

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.

T. R. Duhl et al.

duhl@ucar.edu

Received and published: 21 November 2013

Please note that the author responses to all three referee comments are also viewable in the attached supplementary pdf.

Author response to Anonymous Referee #1, 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. (Geosci. Model Dev. Discuss., 6, C767–C775, 2013)

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

Please note that we have pasted sections of text from the original Referee report (Italicized and in a different font than our responses) containing the suggested revisions or comments and have responded to these with numbered Author comments.

"Regional applicability. The species considered in the paper are not all "natural" for Mediterranean type of climate. For instance, I cannot imagine that birch model can be developed or verified based on the data from such region. The habitat of this tree is located very far to the north of it. In Southern California, birch is heavily stressed by heat and water availability, so that the parameterizations based on the regional data have nothing in common with actual birch behavior. In particular, the base temperature of 9.1C suggested in the paper is confusing: typical range suggested in various works is 3-5C. The same is true for heat sum: typical value reported in the literature is around 100 degree-days (smaller in the north, larger in the south), which has nothing common with the baffling 620 dd suggested in the paper. This problem is also evident from the companion paper, which presents the observation results. Peak concentrations during the season about 5 pollen/m3 is negligibly small (about 1000 times smaller than in the main birch habitats). Therefore, I have to conclude that the birch model parameters are unrealistic and the model is not suitable for the main tree habitat. This is also confirmed by poor model-measurement comparison (discussed below)."

Author response 1: Pertaining to the birch parameterizations, we have added an explanation of why we chose the base temperature of 9.1°C and the particular sequential model that was selected by adding the following text (Section 2.1.2, lines 294-301 and 335-371): "The sequential chill-heating model developed for olives by De Melo-Abreu et al. (2004) is used to simulate pollen season for tree species in STaMPS with chilling requirements, since sequential models have been identified as being appropriate for phenological simulations of both birch and olive in similar climates (Jato et al., 2007; De Melo Abreu, 2004)." . . .

"For olive, walnut and birch species, we use the same optimum and breakpoint chilling temperature values as those selected for olives in De Melo-Abreu et al. (2004), since

GMDD
6, C1960–C1975, 2013

these values were calculated for a similar climate as the present study, and observations (e.g., Jato et al., 2007; Warmund et al., 2009) suggest that birch and walnut trees have similar optimal chilling temperatures as olives. A threshold chilling quantity of 58 chilling units was selected for walnuts (Warmund et al., 2009); 432 chilling units was assigned to olives (De Melo-Abreu et al., 2004) and was also assigned to birch (for which published values for birch growing in similar climates were not available). TB values close to 0°C have been suggested for birch growing in high latitudes although latitudinal gradients in base temperatures as well as chilling requirements have been observed in many tree species including birch (Myking and Heide, 1994) and grasses (Heide, 1994). We tested a range of base temperatures for birch using the Pasadena data but found little difference in terms of percent standard deviation in accumulated GDD on peak birch pollen count dates using a base temperature of 0°C compared with 9.1 for the years included in the analysis. Most studies of Betula have been performed on high-latitude ecotypes although Jato et al. (2007) studied and modeled the onset and length of flowering in B. pendula and B. alba populations in Spain using several modeling approaches and base temperatures and found that model parameterizations developed for olives (Galán et al., 2001b) resulted in the lowest deviations from actual versus predicted peak pollen date for Betula. Data regarding optimal TB values for birch and walnuts growing in climate zones similar to the study domain are sparse therefore, following a similar approach as Jato et al. (2007), we applied a model developed for olives (De Melo-Abreu et al., 2004) to birch and walnut species within the domain. It should be noted that Jato et al. (2007) used the olive model of Galán et al. (2001b), while we have employed the De Melo Abreu et al. (2004) approach which allows for the calculation of devernalization during chilling calculations. De Melo Abreu et al. (2004) evaluated the inclusion of devernalization against the same chilling calculation approach used by Jato et al (2007) and found results to be more accurate, and the model more physiologically meaningful, when devernalization was considered. It has been long known that species with chilling requirements can lose a portion of their accumulated chilling when temperatures exceed some threshold value (Richardson et

GMDD
6, C1960–C1975, 2013

al., 1974; Gilreath and Buchanan, 1981). The GDD thresholds for these species were determined using the Pasadena pollen counts as described in Section 2.1.1, with GDD accumulation dates each year beginning on the date when chilling requirements were met for each chilling species according to the Pasadena meteorological data."

We would also argue that model performance for birch wasn't 'poor' as suggested by Referee #1. As stated in the companion paper (Zhang et al., BGD, 10, 3977-4023, 2013) the model exhibited reasonable agreement with both birch and mulberry (which are both non-native to the study region) pollen observations as well as for olive. The fact that birch concentrations were low in the study domain reflects the relative scarcity of this species within the region and also reflects the fact that fractional vegetation cover is much lower in the arid study domain compared with the heavily-treed regions found in many higher latitudes. The B. pendula trees occurring in the domain are found only in the urban tree inventories according to the input datasets (i.e. none are present in forests according to the FIA database) for the region, and our research has shown that this species is a popular landscaping tree in S. CA. Additionally, B. pendula is not the only birch tree found in the study domain; there is at least one birch species (B. occidentalis) whose native range does extend into the study domain (including both California and Nevada; NRCS USDA Plants Database, http://plants.usda.gov/java/). Table 1 now reflects the fact that other birch species besides only B. pendula are present in the model domain.

"From the above, it is evident that the model presented in the paper has much narrower applicability than it is claimed. That needs to be corrected and the ambitions scaled down to the actually delivered results. Non-natural species in California should be excluded (first of all, birch). Strict binding to Southern California should be made clear already in the title, abstract, and introduction. I understand that the basic approaches are universal – but also trivial and known for decades (e.g. many references go back 20-30 years). The devil is in details: it is the data existence and availability, as well as the possibilities of generalization of local and regional findings that presently limit the pollen model development worldwide. And from that point of view, current study is strictly South-Californian."

Author response 2: We agree with Referee #1 that the parameterizations developed in StaMPS are explicitly for species occurring in the particular climate zones characteristic of the study domain, and we did not intend to convey in the ms that the exact model approaches, GDD thresholds, base temperatures, etc. selected for each species should be used to simulate pollen season in climatic regions differing substantially from those represented in the study. We have made numerous changes, including to the abstract, moving the text "in a study domain centered over Southern California (S. CA)" to the first sentence, "climate zones represented" was inserted in line 39, as well as "in the study region" (line 47). We have also inserted the following text into the Introduction (lines 95-105):

"The relationships between meteorological parameters and pollen season described here are only applicable to this study region (or to study domains in similar climate zones) since observational data (including pollen count data and phenological observations) taken from within the model domain as well as literature-derived relationships from studies conducted in similar climate zones were used to develop the parameterizations. Plans for development of modules for additional species as well as considerations for simulating species occurring in other climate zones are discussed in Section 4.2." We have endeavored to more fully describe that the STaMPS modeling framework is in fact flexible with respect to simulations in different climate zones because it includes several types of budburst models (thermal time, sequential, and alternating; Section 2.1, lines 157-182), and that in principal, GDD and chilling thresholds and base temperatures obtained from published studies of the selected species growing in other climatic regimes could be applied, and the choice of budburst model modified, in order to perform simulations in other locations (Section 4.2, 814-826). However it is also noted that for some species extensive additional phenological and aerobiological datasets would be required to inform and validate simulations performed in other regions (Section 4.2). STaMPS is designed such that modules can be modified and substituted not only for individual Plant Functional Types (PFTs), but also for species occurring in different climatic regions. Plans to incorporate additional climate-zone simulation capabilities as well as new species are discussed briefly in Section 4.2 (lines 819-826).

“Methodology The authors accept many values from unconnected studies, often very old ones. This is normal practice in science but still requires care and is outright dangerous in case of pollen: natural variability is extremely high, as well as the sensitivity of the results to the setup of the field and lab studies.”

Author response 3: We include a lengthy discussion of the observed variability in the various input datasets used in the model (Section 4.1). We acknowledge that the first version of the ms excluded some much-needed descriptions of and justifications for the specific choices made regarding which studies/models, forcing threshold values and base temperatures were assigned. We hope that in the revised ms it is clearer that we only applied literature-derived relationships between meteorological variables and flowering, as well as and critical forcing and heating thresholds from previously published studies that are relevant to the species in question and representative of the climate zones within the domain (e.g. lines 95-105 185-188, 294-302, 335-371). We also have placed more emphasis on the fact that the chosen thresholds, base temperatures, etc. are observationally constrained (lines 748-752) which should serve to increase confidence in the model. We acknowledge that these descriptions were deficient in the first version of the ms, but in this revision we have now added significant additional descriptions of and justifications for the approaches used for the species (e.g. the base temperature and modeling approach applied to birch, as described in Author response #1, above, and in Section 2.1.2 of the ms) and we have also placed greater emphasis on the fact that the relationships are valid only for simulations performed in similar climate zones (throughout the ms as described above in Author Response #2). Please note that Author response 5 is also relevant to these comments and includes

GMDD
6, C1960–C1975, 2013

additional instances of revisions made in response to them.

“For instance, heat-sum threshold is known to vary by a factor of two or even more at a spatial scale of just a few tens of km, especially in complex-terrain regions. Taking a single value for the whole region is much too crude approach.”

Author response 4: As described in Section 2.1.1 (lines 243-253) and Appendix B, STaMPS does include algorithms for calculating variable heat-sum thresholds and assigns them to species (Quercus) in which they have been observed in similar climates as our study. Table 1 includes the equations used to determine forcing thresholds for oaks.

“In several cases the values are extrapolated across species “due to lack of data” without justification and verification. This is not acceptable. The species, for which the data are not available – and again birch is to be mentioned first – should be excluded from consideration. Pollen counts is a poor type of input data for determining the start of flowering. The authors paid no attention to vast amount of publications analyzing early- and late- season long-range transport (LRT) episodes, which dramatically change timing of the pollen season, i.e. period with substantial pollen concentrations in the air, as compared with local pollen release season, i.e. the flowering period, the goal of the study. The difference can be as large as a month! The impact of LRT episodes is more moderate only for the species native in the area. For taxa with the main habitat outside the region, the pollen season can be almost entirely decided by a few LRT episodes, which have little connection to regional developments. This is the probable reason for poor model performance for several species (as shown in the companion paper). Phenological data should be used instead for more accurate model parametrization.”

Author response 5: We hope that in the revised ms it is much more clear that instead of simply accepting “many values from unconnected studies” or assigning values “due to a lack of data” we only applied literature-derived relationships between meteorological variables and flowering, as well as and critical forcing and heating thresholds from previously published studies that are relevant to the species in question and representative of the climate zones within the domain. See Author Responses #1-3 (above) for the exact revisions made. We agree that phenological observations are the ideal datasets to inform predictive models of budburst, and many of the studies that we used to parameterize these relationships for the selected species were based on phenological observations (e.g. Bromus: Gleichsner, J. A. and Appleby, A. P. Weed Sci., 44, 57-62, 1996; Quercus: phenological observations provided courtesy of Dr. Walt Koenig of Cornell University; and Olea: De Melo-Abreu, J. P., Barranco, D., Cordeiro, A. M., Tous, J., Rogado, B. M. and Villalobos, F. J. Agr. Forest Meteorol., 125, 117-127, 2004). We have now included a discussion of the possible contribution to observed pollen from long-range transport (LRT) episodes and the steps we took to consider LRT in our simulations as well as acknowledging the limitations inherent in using observed pollen counts as proxies for dates of peak pollen release (Section 4.1). The following text was inserted into Section 4.1 (Lines 732-754):

"Long-range transport of pollen may also complicate the interpretation of observed atmospheric pollen curves (e.g., Siljamo et al., 2008), especially for non-native species within a study domain that could have a much larger presence in regions outside of the domain, such as birch species. Jato et al. (2007) compared Betula pollen curves with phenological observations from two species of birch growing in Spain, and found that for the calculation of GDD requirements, using dates of peak pollen concentration yielded similar results as use of phenological observations, although lags between dates of peak flowering and peak pollen count have been reported for a number of species (e.g., Latorre, 1997). Although we cannot exclude the possibility that pollen transported over large ranges may have influenced the pollen count data used for the determination of heat thresholds, a multi-year aerobiological dataset such as that employed in the present study should be dominated by local signals over time, and numerous studies (of the same genera selected for initial simulation in STaMPS) performed in similar climatic regions were consulted during model development. In an effort to include the potential contributions from pollen transported into the domain during the validation simulations, a nested domain scheme was used for the 4km domain simulations (as described in Section 2.4) Our study is the first to predict pollen production for multiple species in the western half of the US, and is constrained with observations taken from within the study domain as well as built using numerous previously published relationships for the selected genera occurring in similar climate zones as the domain, therefore STaMPS improves capabilities for predicting pollen season in this region. Nonetheless, these limitations should be kept in mind when interpreting the pollen dispersal results."

Finally, as pointed out in our companion paper (Zhang et al., 2013), although LRT might be expected to affect "spatial distribution and magnitude of pollen concentrations at the local scale (Zink et al., 2011), there should be a correlation between the timing of pollen emission and that of concentration on the regional scale", and using birch performance as an example, the regionally-observed and simulated regionally-averaged mean peak pollen concentration dates both occurred in Mid-May with good performance also seen in mulberry, oak, and walnut.

"The authors have excluded the year 2007 without any justification, just because it looked differently from the others. This is quite shocking: such thinning of the datasets should have very strong justification. Actually, strong meteorological variation would rather help to parametrize the model and improve its ability to reproduce the phenological processes under varying external forcing. Existence of such non-trivial year should be considered as the advantage of the study rather than its drawback. How can the model be applied to future climate, where extremes are more probable, if even in the present situation part of the data is excluded at the very beginning?"

Author response 6: Perhaps we were not clear in how we used the pollen count data and why 2007 was excluded: we have re-worded the explanation (lines 216-217) for the exclusion of this year from the set of years where cumulative GDD values were calculated and averaged by genus to determine GDD thresholds. Rains were so frequent in the early spring period that for many genera peaks were not observed at all during

GMDD
6, C1960–C1975, 2013

the expected times (or others). In one of the helpful references in the list provided by referee #1 (Galán et al., 2001), we noted that a similar approach was taken in that study. In addition to hopefully making the exclusion and the reason for it more clear in revision 2, the following text has also been added (lines 218-220): “In evaluating several models for their ability to predict the start of Olea pollen season in Spain, Galán et al. (2001a) excluded two years out of an 18-year olive pollen dataset due to rain events occurring near the beginning of olive pollen season.”

“Credibility of the results and model evaluation Evaluation of the model is not presented at all. Instead, the reader is referred to another paper, in different journal and not yet accepted for publication. This is the major problem: the presentation of the model is bound to include its assessment. Companion yet-to-be-accepted paper in different journal does not qualify for that. Nevertheless, I have read the companion paper in order to understand how the above-criticized methodological problems affected the performance. “

Author response 7: As is the case for Geoscientific Model Development Discussions, articles accepted to Biogeosciences Discussions (i.e. the journal to which our companion paper was submitted) are citable upon publication, even in the Discussions version of the journals, and journals such as Geoscientific Model Development and Biogeosciences do screen submissions, only publishing those of acceptable quality. Given the breadth of the modeling effort, we knew that more than one manuscript would need to be prepared and submitted in order to publish a satisfactory description of the modeling framework, yet we could not identify a single journal that was appropriate to submit both papers. Therefore we opted to submit to two EGU Copernicus journals, both of which have high standards for quality. There are only two ways to evaluate the output from the STaMPS model: Phenological observations or pollen counts. Since phenological observations were unavailable (except the oak data that was already used to develop the model), comparing model output with aerobiological data is the only remaining option. Simulating the dispersion of modeled pollen produced in STaMPS and available for release into the atmosphere is non-trivial and requires accurate representations of wind fields, mixed boundary layer height, particle physics, etc. and obviously adds more uncertainty to the results. However since the final product of the dispersion model is what is needed for comparison with observations, we decided that the ms describing the dispersion model and results was the appropriate platform for discussing model performance and comparison with observed values. Again we suggest that model performance is not poor as Referee #1 asserts, but this may be a matter of opinion. In any case we have added additional descriptions of model evaluation in Section 2.4 (lines 588-599).

"Several points are clear: birch is indeed practically not represented in the region. For comparison, typical concentrations in Central and Northern Europe during the main birch pollen season exceed 1000 pollen/m3, maximum going over 20,000-30,000 pollen/m3, whereas in the current application the counts never exceed 10. No surprises that the model failed it. Walnut and mulberry largely follow similar suite: their concentrations are very low and model predictions have little common with observations. As a result, only grass, olive and oak have substantial representation in the region and non-negligible pollen concentrations."

Author response 8: As mentioned in Author Response 1, and described in Section 2.4 (lines 588-599), we assert that model performance for birch wasn't poor as suggested by Referee #1. As stated in the companion paper (Zhang et al., BGD, 10, 3977-4023, 2013) the model exhibited reasonable agreement with both birch and mulberry (which are both non-native to the study region) pollen observations as well as for olive. The fact that birch concentrations were low in the study domain reflects the relative scarcity of this species within the region and also reflects the fact that fractional vegetation cover is much lower in the arid study domain compared with the heavily-treed regions found in many higher latitudes. Both mulberry and B. pendula trees occurring in the domain are found only in the urban tree inventories according to the input datasets (i.e. none are present in forests according to the FIA database) for the region, and the pollen monitoring station used to infer the peak pollen dates was also in an urban location. Although LRT may have played a role in the observations at certain times, the multi-year nature of the dataset would be expected to be dominated by local signals when viewed as a whole. See Author Responses 1-7 above for the specific changes that have been made to the ms to address these concerns.

"The evaluation is performed for a single 2010 season, which is insufficient for the model with climate-related ambitions. Difficulties with access to pollen observations also exist in Europe but it cannot justify application of untested models for predicting the future climate conditions. I included a few references that showed the climate response in pollen seasons is very complicated. Some species start flowering earlier, others show later season or appear neutral, and in many cases the response is region-dependent. This again stresses the necessity to evaluate the model for a large variety of conditions before making far-reaching conclusions at climate scale. And I again was missing the rainy year 2007 excluded from both parameterization and evaluation. Does it mean that the model fails it? If yes, why should the reader expect it to work for different climate conditions?"

Author response 9: We hope that the revised ms now makes it clear that we are not claiming to project climate effects on pollen season in climates outside of our study domain. Author Response 6 addresses the exclusion of 2007. We have added some additional discussion using several of the references that Referee #1 suggested which expands the discussion of our results into comparisons with other studies/regions (see Author response 10, below for specific changes made in the ms). Galán et al. (2001, from the list of references suggested by referee #1) used a similar approach in the development of their pollen forecasting model: a number of years worth of observational data were used to constrain the model and a single year was used in the model evaluation. Obviously, more years for comparison is better than fewer, however we had a finite dataset for use during the course of the study and we allocated the data in what we deemed to be the most responsible manner.

“Comparison of the model formulations with other models is entirely missing. How does the suggested parameterization meet / contradict / improve the existing models in Europe and the US? Several models are quoted in the companion paper, which includes some discussion. Why was it not done here in a systematic way? Finally, as seen from the companion paper, the model showed poor performance for the bulk of the considered species –except for olives and, may be, oak. With such scores, I see no way to approach climate studies. It is not possible to discuss 5 days of the shift of the season if the evaluation showed the error of as much as 1.5 months in the season start (e.g., grass).”

Author response 10: We have included additional discussion of our approach with respect to other models and have included some comparison of how our results contrast with other model predictions (lines 640, 654-656, 658-660, 662-664) and observations (lines 639, 656-658, 681-689) As mentioned in several previous Author Responses, we disagree that the model evaluation indicated poor performance; with respect to the grass evaluation one would not expect the pollen counts to agree completely with simulations when just two grass species were included in the model whereas the observed pollen counts will reflect the contribution from all grass species flowering in an area at any given time.

“P.2330 line. 12-15. This is confusing. The TOTAL pollen produced by a tree during specific season is independent from the conditions during that very season. They are entirely controlled by the previous season when the male flowers are formed – as stated later in the paper. I guess, the authors have mixed-up the daily production and total seasonal production, the first one indeed being controlled by actual meteorological conditions. If yes, it should be stated clearly.”

Author response 11: We have deleted “in a given season” to avoid confusion since the point of this sentence was to list the variables known to govern pollen production but not the timescales on which they operate. In Section 2.3 of the ms we acknowledge that precipitation a full year prior to the start of pollen season in trees can affect the amount

GMDD

6, C1960–C1975, 2013

of pollen produced and that is in fact how STaMPS calculates the pollen production size for trees.

"P.2331, line.12-13. The so-called sequential model (heat accumulation starts after chill units are all collected) used by the authors is not always the best approach for explaining the flowering time of several trees. In many cases, parallel model with fixed start of heat accumulation has proven to be better. This problem should be at least discussed."

Author response 12: We have added discussion of this as described in Author Response 2.

"P.2331, line.15. It is a very well known that GDD threshold is a function of location, with its value varying by a factor of times for various parts of the habitat area. Therefore, the value(s) suggested by any specific publication is (are) valid exclusively in the region (possibly, small) around the study place. To the best of my knowledge, no extrapolation algorithm exist, i.e. the thresholds have to be determined by fitting the model output to the data at maximum number of points and interpolation between them has to be done with high care. This is among the biggest challenges of the pollen source terms developments."

Author response 13: (same as Author response 4) As described in Section 2.1.1 (lines 243-253) and Appendix B, STaMPS does include algorithms for calculating variable heat-sum thresholds and assigns them to species (Quercus) in which they have been observed in similar climates as our study. Table 1 includes the equations used to determine forcing thresholds for oaks.

"P.2331, line.16-20. Pollen counts can be very misleading when determining the start of flowering (see above). P.2331, line.16-17. Problems with the methodology are implicitly acknowledged by the authors themselves: they excluded 2007 because of rainy end of the season. But it "automatically" recognizes the fact that the model cannot deal with such conditions." Author response 14: this is dealt with in Author Responses 5 and 6,

GMDD

6, C1960–C1975, 2013

above

“Section 2.1. The authors claim that there is essentially no data for birch to parameterize the model. However, this is the most-studied tree in Europe. I roughly estimate that 30-40% of aerobiological publications are dedicated to it or use it as one of target species.”

Author response 15: we agree and we have clarified (lines 294-301, 342-371) in the ms that we meant that birch is not well-studied in climates similar to our study domain

“Section 2.3. It is a well-known fact that many trees have bi-annual cycle of total seasonal pollen release. Why does this model have no trace of it?”

Author response 16: This effect has not been included in STaMPS; we have added the following text into Section 4.1 (lines 705-706): “Additionally, some species exhibit bi- or even triennial variations in pollen production (e.g. Celenk et al., 2009), and these effects are not represented in STaMPS.”

“Section 3. Before going into the climate simulations, the model must be evaluated properly, which is not done. After reading the companion paper, I had severe problems believing the conclusions presented in this section. I would drop this section entirely until the model is improved and its ability to reproduce present climate is confirmed by detailed evaluation.”

Author response 17: This has been addressed in Author Responses 5-10, above

“Useful references”

Author response 18: Thanks for the references; we have included several of them in the revised ms (Siljamo et al., 2008; Ziello et al., 2012; Clot, 2003, Galán 2001, Emberlin, 1999; Linkosalo et al., 2008)

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/6/C1960/2013/gmdd-6-C1960-2013-

supplement.pdf

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

GMDD

6, C1960–C1975, 2013