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Geoscientific Model Development Discussions

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.

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Please note that author responses to all three referee comments may be viewed in a single document attached as a supplementary file.

Author response to Anonymous Referee #3, 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, C1029–C1032, 2013)

"In the manuscript there are appropriate references to a companion methodological paper, but the difference between pollen production at plant level and airborne pollen counts is not well explained here and this is necessary. A reader non expert in the field could assume, reading the current manuscript, that the pollen present in the air is simply the result of the pollen released by local plants, and this assumption is obviously wrong. Simulated pollen production data are in fact used as "input" data to be used by a transport model presented in the companion paper. I suggest to make a clear distinction between these two research aspects also in this ms. The confusion on this argument is also generated by the presence of Fig. 2, Fig. 4 and Fig.5, which report pollen counts while captions refer to pollen production, and the claimed use of pollen counts to calibrate a coefficient of the presented pollen production model (p.2338, lines 13-16). Such calibration also needs then a more accurate justification."

R3 Author response #1: We have changed the beginning of Section 2.3 (lines 448-452) to read:

"The amount of pollen emitted to the atmosphere is a function both of the amount of pollen available for release (which is what is modeled by STaMPS) and the various short-term meteorological factors that influence pollen dispersal into the atmosphere, including relative humidity, wind characteristics, etc. (as simulated by the companion pollen transport model described in Zhang et al., 2013a). "

In response to the issue with Figure 2, we cannot find a discrepancy between the figure caption and the figure itself. However, we have made the caption more explicit by changing it from 'Olive tree pollen counts data for Pasadena, CA, 2006 and 2008.' to 'Olive tree pollen concentrations observed in Pasadena, CA, 2006 and 2008.' In Figure 4, we have changed the caption to read: 'Relationship between wet season precipitation and pollen concentrations derived from Fairley and Batchelder (1986, who reported a p-value of 0.0002). This relationship is applied to pollen production by tree species in the domain without chilling requirements.' We have changed the caption of Figure 5 to read: "Relationship between pollen production (expressed as percent of average peak value) and dual vernalization-precipitation coefficient (as calculated using Eq. 6) derived from Pasadena, CA pollen concentration data and applied to STaMPS tree species with vernalization requirements for flowering."

Also see lines 376-392 where we have made it clearer to the reader that observed pollen concentrations can be affected by various meteorological factors. We have inserted the following text into Section 4.1 (lines 739-765):

"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 evaluation simulation period, a nested domain scheme in which the 4-km S. CA domain pollen transport simulations used boundary conditions provided by simulation results of a larger 12-km domain that covers the entire states of California and Nevada (Figure 1; as described in Section 2.4) was used; the simulations results indicate contributions from California and Nevada outside the S. CA domain to pollen counts at the observational sites were insignificant (Zhang et al., 2013a). 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."

As described in Section 2.3 of the manuscript, the pollen production potentials were derived from studies of actual pollen production; i.e. production of pollen grains was quantified and scaled to a per unit area production potential, which is then modified by a precipitation-only or a combined precipitation/vernalization coefficient. Admittedly the coefficients were derived from pollen concentration data, in our case peak concentrations were used, and the relationship between these and early-season chilling/precipitation were applied to the potential pool size of pollen produced. While we agree this approach leaps from production to concentration, the observation in aerobiological datasets that cumulative past-year meteorological conditions such as precipitation affect pollen concentrations is pervasive and overwhelmingly argues that these factors control the amount of pollen available for release while on shorter timescales local meteorological factors contribute to pollen rafting into the atmosphere (e.g. Laursen et al., 2007 from the manuscript). In Fairley and Batchelder (1986), the study whose observations of the relationship between pollen concentrations and wet-season precipitation we used to derive our precipitation coefficient, an excellent correlation was observed between median pollen concentration and wet-season precipitation a full year before the year of flowering, while median pollen concentration and wet-season precipitation in the year of flowering had no statistically-significant relationship. This behavior illustrates that meteorological factors occurring well before the date of flowering (i.e., during the times when flower bud differentiation is occurring) are controlling these processes, which has also been reported in phenological studies numerous times (Corden and Millington, 1999; Aerobiologia, 15; Miyazaki et al., 2009 from the manuscript; Kozlowski, 1971 from the manuscript), and which justifies the application of observed aerobiological relationships with prior-year climatic variables to predicted production.

We have made it more explicit that pollen concentration data were used to infer the relationships between meteorological factors and pollen production (see changes made above) in tree species (the grass pollen production coefficient was determined based on phenological data) and have also endeavored to more clearly discern between what STaMPS does (i.e., predict the amount of pollen available to be released) and what the pollen transport model described in our companion paper does (model the dispersal into the air).

"The manuscript refers to the interaction between airborne pollen and anthropogenic air pollutants such as for example ozone. But no information about the ozone cycle is given, even if the concomitance of peak ozone concentration and pollen concentration is mentioned. I would better describe this point, to highlight the occurrence at the same time of different airborne particles and justify the choice of the period March-June for simulations."

R3 Author response #2: As described in lines 584-589 "Initial STaMPS and pollen transport simulations were performed for a 4-km resolution model domain centered over southern California (Figure 1, lower right corner) for 1 March through 30 June 2010. This domain and time period coincided with an extensive set of pollen observations collected as part of the University of Southern California's Children's Health Study from which ambient pollen count data were collected and were used to evaluate and optimize STaMPS."

Although ozone season frequently occurs in or near the selected simulation period in this region, we felt it would be inappropriate to add a discussion of this in light of the fact that this is a complex issue (i.e. ozone episodes can occur at various times depending on numerous factors) and also because the extensive revisions that have been made to the paper (see Author responses to Referee # 1 and #2) have considerably lengthened an already long manuscript, additionally this really isn't the focus of the manuscript. One of the other reviewers even thought the paper could be submitted as two papers but as we discussed in response to that reviewer we didn't feel this was an option for

our case (mainly due to the existence of our companion paper which was submitted also to an EGU journal and which is under review now).

"I am very perplex about the use of the same chilling requirement for olive and birch. The authors have probably misunderstood the papers cited to justify such an arbitrary choice. There are several points that canbe mentioned in order to consider such an attribution wrong, among them a very different latitudinal optimum between these two genera (birch is more acquainted to higher latitudinal range -Northern Europe- than that considered in the presented ms, differently from olive for which the considered geographical extent could be optimal), as well as a different ecological classification (olive is a late successional tree, birch an early successional one). I would reconsider this aspect in order to modify the model."

R3 Author response #3: We have now explained that specific model approaches for birch (growing in the climates within our study domain, i.e., far from their latitude of origin) are chosen because these have proven to be the best for simulation of birch behavior in similar climates, by inserting the following text into the manuscript (lines 300-305):

"Of the tree genera selected for simulation with chilling requirements for flowering (Betula, Juglans, and Olea), olives (Olea europaea) have been best studied in Mediterranean climate zones since they are native to these regions and are important not only economically but are also highly allergenic (Rodríguez et al., 2001). 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)."

and (lines 338-374): "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 these values were determined within a similar climate zone 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 al., 1974; Gilreath and Buchanan, 1981)."

Additional specific remarks: "p. 2327, line 2: change "A pollen model..." to "A pollen production model...", or "A model of pollen shedding/production...", or simply "A model..."."

R3 Author response:We have changed this to "A model. . ."

"line 3: Is there need to specify "terrestrial"?"

R3 Author response:We have deleted "terrestrial"

"line 4: The model does not strictly study the interaction between pollutant factors and pollen, but simply simulates airborne pollen distribution. I would rather suggest "in order to investigate how pollen can interact with anthropogenic pollutants to affect human health". "

R3 Author response:We have changed this to "in order to investigate how pollen can interact with anthropogenic pollutants to affect human health"

"line 12: Are references to "Mediterranean zones" in Southern California appropriate? Would not be better a more general climatic definition? The adjective "Mediterranean" also recurs in other parts of the manuscript. When not referring to plant species, it would rather be better to use a different expression."

R3 Author response: This region is frequently referred to as being Mediterranean climate-wise; in fact much of the state of California is included in maps of Mediterranean climates. We have, however, removed numerous instances of this wording and simply replaced with phrases such as "from studies performed in similar climates", etc.

"p. 2328, line 3: change "simulated species" to "considered species"."

R3 Author response: We have changed this to "considered species"

"line 25-26: please also provide more recent literature about advances in phenology timing, for example Menzel et al. 2006, and about adetected changes in airborne pollen burden, for ex-ample Ziello et al. 2012, Damialis et al. 2007, Garcia-Mozo et al. 2010."

R3 Author response: We have made several additional references and comparisons to both other model results and observations (including Ziello et al., 2012 and others), see lines 643-646, 660-670 and 687-690 of the revised manuscript.

"p. 2330, line 20: Not clear, maybe "separate" stands for "separated"? If so, in which ways are they separated? Are those modules sequential? Parallel?"

R3 Author response: "separate" is correct; but we have now inserted the following text at the beginning of Section 2 (lines 160-185):

"A number of models have been devised to predict the timing of anthesis in trees and other PFTs that flower during spring to early summer (when temperature is the main driver controlling flowering); these include approaches that consider only the effects of forcing temperatures (such as the thermal time model; Cannell and Smith, 1983) and models that include both chilling and forcing temperatures: the sequential model (Sarvas, 1974), the parallel model (Landsberg, 1974), and the alternating model (Murray et al., 1989). Chuine at el. (1999) tested eight phenological models for their ability to predict flowering dates in tree species with differing abilities to adapt to local climatic conditions and found that no one model was best-suited for all species. STaMPS applies different models to different species based on whether the species has forcing-only or both forcing and chilling requirements for flowering. The thermal time model is applied to species without known chilling requirements (Section 2.1.1); a sequential model is applied to tree species with chilling requirements for flowering (Section 2.1.2). Section 2.2 describes an alternating model (based on Gleichsner and Appleby, 1996) that is applied to the Bromus grass species included in the initial model simulations. A particular model may perform well for simulating a species in one climate zone, but poorly for the same species growing in a different climate. The type of bud burst model assigned to a species in one location can be easily modified since STaMPS modules

already exist for thermal time, sequential, and alternating approaches. STaMPS lacks a parallel budburst model although this may be ideal for predicting flowering in some species/locations. Linkosalo et al. (2008) found that sequential and parallel models had similar prediction accuracies for several tree species including two birch species in a high latitude location (Finland), but that a simple thermal time approach performed best with independent data. Linkosalo et al. (2008) also noted that models including chilling parameterizations may be better suited to simulations under climate warming scenarios when chilling could potentially become a limiting factor."

"line 26: Please provide some literature to support the statement that "temperature is the main driver controlling flowering", for example Parmesan et al. 2007 or Menzel et al 2006."

R3 Author response: We already cited two studies the first time we stated that temperature was the main driver controlling flowering (lines 144-145 of the revised manuscript).

"p. 2349, line 22: change "Artemesia" to "Artemisia"."

R3 Author response: Thanks for catching this; it has been corrected

"Table 1, footnote: change "phonological" to "phenological"."

R3 Author response: Thanks for catching this; it has been corrected

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/6/C1996/2013/gmdd-6-C1996-2013-supplement.pdf

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