

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 #3

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The paper is generally well written and provides a detailed and accurate description of the presented model. I agree with the highlighted importance of developing similar models to investigate changes in the presence of airborne pollen in the low atmosphere, however I have some general remarks of a certain importance. In my opinion, there are some flaws in the choice of model coefficients, as well as confusing references to a not clearly specified relationship between airborne pollen counts and pollen production/availability. Moreover, the cited literature is often outdated and should be replaced by more recent scientific articles, especially (but not exclusively) in the fields

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of general plant phenology and large-scale aerobiology.

General remarks:

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 ms, 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. The ms 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. 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 can be 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.

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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...". line 3: Is there need to specify "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". 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. p. 2328, line 3: change "simulated species" 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 a detected changes in airborne pollen burden, for example Ziello et al. 2012, Damialis et al. 2007, Garcia-Mozo et al. 2010. p. 2330, line 20: Not clear, maybe "separate" stands for "separated"? If so, in which ways are they separated? Are those modules sequential? Parallel? 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. p. 2349, line 22: change "Artemesia" to "Artemisia". Table 1, footnote: change "phonological" to "phenological".

References:

Menzel A, Sparks TH, Estrella N, Koch E, Asas A, et al. (2006) European phenological response to climate change matches the warming pattern. *Glob Change Biol* 12: 1969–1976. doi: 10.1111/j.1365-2486.2006.01193.x. Ziello C, Sparks TH, Estrella N, Belmonte J, Bergmann KC, et al. (2012) Changes to Airborne Pollen Counts across Europe. *PLoS ONE* 7(4): e34076. doi:10.1371/journal.pone.0034076. Damialis A, Halley JM, Gioulekas D, Vokou D (2007) Long-term trends in atmospheric pollen levels in the city of Thessaloniki, Greece. *Atmos Environ* 41: 7011–7021. doi:

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10.1016/j.atmosenv.2007.05.009. García-Mozo H, Galán C, Alcázar P, de la Guardia CD, Nieto-Lugilde D, et al. (2010) Trends in grass pollen season in southern Spain. *Aerobiologia* 26: 157–169. doi: 10.1007/s10453-009-9153-3.

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