

Interactive comment on “Estimates of common ragweed pollen emission and dispersion over Europe using RegCM-pollen model” by L. Liu et al.

Anonymous Referee #1

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Review of the article Estimates of common ragweed pollen emission and dispersion over Europe using RegCM-pollen model by Liu et al (2015) The study present a model developments and simulations with the RegCM model during the period 2000-10 with focus on ragweed pollen

General comments:

1. The topic address a relevant scientific questions that is within the scope of Biogeosciences: Interactions within the biosphere-atmosphere system
2. The manuscript presents the RegCM model and the pollen emission estimates. This modelling framework was previously presented by Hamaoui-Laguel et al (2015) so there appears to be limited new knowledge with respect to concepts, tools and

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ideas. The study use 46 sites, but there is no information on the data from the sites except for a map (e.g. Fig 2) and scatter plots (e.g. Fig 4). This suggest that overall there is limited new information in this study.

3. The conclusion that are reached are substantial as they conclude that the simulations are possible with the RegCM model. However, as far as I know, then their approach on simulating pollen emission is not new. Similar methods are to my knowledge used in both European and American models such as COSMO-ART, CMAQ, SILAM, KAMM/DRAIS and a number of other models. So it would be good to be explicit on what is the difference with this method and e.g. the method published by Prank et al (2014) or Helbig et al (2004) and Sofiev et al (2013), which they rely on.

4. The atmospheric modelling techniques are well founded by using established methods. However, the observational record that is used in this study is largely undescribed, except for a map and the number. As a minimum a list with names, coordinates and a basic description of the observations record is needed (e.g. start dates, pollen index etc).

5. The conclusion appears to rely on a weak foundation as the observational data is not presented. Additionally, then the authors validate their results against the same calibration data set. A result of this approach is that the conclusion of the simulated pollen concentrations in Europe, are less well founded (page 17617, line 10 and onwards). As a minimum then the authors should have introduced an error estimate of their simulations (e.g. by using cross correlations) and probably, then the study should be limited to areas with a decent number of station coverage such as France and the region around Croatia.

6. The lack of data presentation makes it impossible to allow for a reproduction of their results. Without the calibration data it is not possible for me to assess the methods and verify the quality. The calibration data appears to be the most important component in the entire system(e.g. the substantial increase in correlations).]

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7. I have the impression that the authors do not give credit to the work that provided the observational record. There is no description of the data sites, a limited description of the observational method and the data source is limited to the acknowledgement section.

8. The title does not clearly reflect the contents of the paper. According to their results, then the observational record is the most important component (the large increase in correlations), but this is not reflected in the title

9. The abstract do to some degree cover the contents. However the parts on health risk appear to be unfounded as this require estimates against thresholds, which is not done in this study.

10. The overall presentation is clear and easy to follow.

11. The language is fluent and precise

12. The equations and symbols are well defined

13. The manuscript contain a lot of figures. Many figures could be removed if the authors used a few tables.

14. The used scientific literature is recent and relevant

Minor comments:

a) The figure on initial guess and calibrated ragweed density map appear to have very little similarity with related regional maps published by Karrer et al (2015), Smith et al (2013), Thibaudon et al (2014) in areas without a dense observational network. As an example then it shows that there is substantial infection in the UK even though it is well known that the UK has very few ragweed populations due to unfavourable ecology (Essl et al, 2015) and ragweed pollen are rarely found in the UK pollen counts (Pashley et al, 2015). This put a question to the foundation of the study and in which area the model results are usable. Also, their results leaves the impression that this study is an

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advanced way of developing an correlative model (although with an atmospheric model in between) where they have heavily tuned a model against a limited set of available observations and then validated their model against the same set of observations (e.g. Fig 7).

b) It is important that a number of relevant ragweed models are developed as it is unlikely that there will be one specific models that will be the overarching model that always perform the best. Due to this I consider it important that this study is fully published. However, based on the 14 points above as well as the minor comment, then it is my impression that major changes are needed in this manuscript.

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