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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A new model of ragweed pollen release based on the analysis of meteorological conditions" *by* L. Menut et al.

Anonymous Referee #2

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General comments.

The paper addresses an important topic of ragweed emission modelling. This is already the fourth study in the same direction during the last couple of years, i.e. the topic is evidently hot. The authors have benefitted from this advantage and tried to construct the model that would surpass the existing approaches. Unfortunately, the paper appeared not very convincing in this sense.

The paper declares a goal of constructing a new regional model for ragweed pollen emission but stays far short of this goal, apparently trying to solve a different problem.

Firstly, out of four factors controlling ragweed emission (equation 2 in the paper), the authors modelled only the last one, taking all others directly from the observations.

Secondly, the authors equaled the pollen emission and pollen concentrations, just selecting the monitoring sites in the vicinity of the pollen sources as a precaution. But it is evidently incorrect. For example, concentrations near the sources are strongly affected by wind speed, which blows pollen away. The authors found no correlation to wind speed, may be because better ventilation was compensated by stronger emission fluxes, i.e. the emission actually was related to wind speed. The list of such effects can be extended leading to the main conclusion: emission and concentrations cannot be considered as synonyms and compared directly as the authors did. A pollen transport and removal model has to be in-between. As a result, the paper in-essence solves a problem different from the declared one: it constructs a statistical model linking the meteorological proxies with daily pollen concentrations. The difference from most of similar papers is that the authors found a non-linear parameterization and covered several sites with one model (and varying success). This is an important result but it has little common with the declared goal.

The second problem is that the authors used a very poor meteorological dataset for driving their analysis. As they pointed out, usual approach to construction of pollen models is to use meteorological observations in the closest vicinity of the pollen monitor – to ensure connection between the meteorological conditions and pollen counts. The authors used meteorological model output instead, which would cause no problems if the data were of sufficient quality. But the dataset has very coarse resolution (almost 50km), which is bound to cause problems in complex-terrain conditions, especially for 2m temperature, one of the main parameters. It can be the reason for weak apparent dependencies between the meteorological parameters and pollen counts, i.e. the validity of the analysis is unclear.

In the evaluation section, the authors are comparing apples, oranges, and potatoes. They picked one (poorly validated) ragweed emission model applied in the US and one European birch emission model to compare with their ragweed concentration-predicting model. Two ragweed emission models developed and evaluated for Europe have been ignored. This selection is partly based on a wrong statement in some review paper about similarity of birch and ragweed emission models – but why not to read the original articles and see that they have nothing in common? This mistake came on top of the main problem: the new development is for concentration prediction whereas the models taken for comparison are indeed for emission and require transport to be properly calculated to obtain concentrations.

I have to suggest the analysis to be repeated with better meteorological fields and the paper to be rewritten bringing its wording in agreement with what actually is constructed: a non-linear statistical model for ragweed concentrations. These changes are admittedly monumental but such a model is worth saving, so my recommendation is "major revision" rather than "rejection".

Specific comments

p.10892, l. 16. It is not a good style to refer to reviews only (and dangerous, as shown below). Please provide references to the original works.

p.10892, l.19. References needed. The statement is very confusing and, if taken in its direct meaning, wrong. I see very little similarity between the spring-time perennial tree in Northern Europe and late-summer annual weed in Southern Europe.

p.10895, l.18-19. As pointed out in the general comments, the selection of birch emission model for the comparison is not correct.

p.10896, l.1 This number seems to be taken at random. The representativeness is a function of averaging, local conditions, distance from major sources and their configuration, local topography, etc. Without specific details and a reference this statement is hard to accept.

Section 2.1. What are the characteristics of the data? ACP readers are not familiar with Burkard trap, not aware of its features, temporal resolution, etc. The whole term "pollen counts" may be confusing and requires proper description. This section should

be rewritten.

Section 2.2. This is a poor dataset. The authors have just said that the representativeness of the pollen observations is just a few hundreds of meters – and still use almost 50km meteorological data. Much better datasets exist, including the archives of ECMWF, which could be used directly, still providing some 15-25km for the considered period. With downscaling the resolution as good as 10km would easily be in reach.

Equation 1 is a triviality and should be removed.

P.10901, l.10-11. The sentence suggests that this model predicts the total annual count. But the next paragraph admits that the observed values from the stations are actually used. The whole paragraph is a lengthy explanation that taking the station totals instead of climatologic value makes results for specific year better. But it is trivial and does not need so long explanation.

Section 4.2.

Eq.2 is a simple Gaussian curve, why not to say it? For readers it would be much easier.

P. 10902, l.7-18. This paragraph actually points out that the season start and end are taken directly from the observations following 5-95% rule. No fitting is made, start and end are directly taken from the data.

Section 4.3.1. The authors should have read the referenced paper rather than rely on a review. The Prank et al ragweed model is not based on birch algorithm. This statement is plainly wrong and the equations (4) and (5) have no relation to ragweed emission.

Equation 7 suggests that there will be no pollen release in neutral or stable conditions when w*=0. This is a very strong statement keeping in mind low correlations shown in table 1.

Section 5. As stated above, the comparison of ragweed model with birch model is baffling. Poor results of Efstathiou et al model is somewhat more surprising but limitations of that study was the thin evaluation (one station, one year, US), so it may indeed appear problematic for the purposes of the current study. But most-importantly, it predicts emission, which should be treated with transport model before comparison with observed concentrations.

Conclusions.

P.10910, l.6-15. I did not understand a lengthy paragraph regarding the diurnal profile of emission. It was not discussed, compared with observations, etc. All evaluation was about daily values. Apart from that, I am alerted by “hourly measurements showed the highest ragweed pollen emissions to occur in the morning”. To my knowledge, there is no regular hourly data for pollen in principle because Burkard trap has a temporal resolution of two hours due to construction of its nozzle and rotation speed of the drum. Do the authors actually have such data?

Figure 1. I did not understand its purpose and found the conveyed message confusing. Why cannot the local models be used in the forecasting mode? Less than a decade ago, all pollen forecasts were based on this approach and it is still widely used. Also: the term “local” probably implies “local statistical”, whereas “regional” probably means “regional deterministic”. But this changes the message: scale is of no relevance, only type of model. Local forecasts can be unified via some spatial interpolation to cover a region, whereas regional runs can be downscaled. Also, statistical models do not contribute to transport and deposition, the corresponding connector in the scheme is wrong. All-in-all, I would suggest to remove the scheme.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 10891, 2014.