Dear Editor and Reviewers,

This is the review of our paper now entitled **"Identification of meteorological conditions driving the daily ragweed pollen release variability**". All reviewers comments were taken into account and, for each question or remark, a detailed answer is given below.

Globally, we found that the reviewers focussed on:

1- *a problem with the use of medium resolution modelled meteorological fields for the emissions scheme validation*.

We agree that for process-oriented studies local observations are preferable to model output instead of model data. In an ideal situation with many observations available over several sites one also should use observations in order to design model parameterizations. However (i) some of the weather parameters tested are not available from observations at sites close to pollen monitoring sites, (ii) direct pollen fluxes could not be measured and therefore measurements used (pollen counts) integrate emissions from some distance away of the sites, and (iii) our aim is to build a scheme able to model ragweed emissions fluxes with a three-dimensional eulerian chemistry-transport and over domains such as Europe (thus with an horizontal resolution of a few tens of kilometers, integrating various environments). For this, we have to use modelled meteorological fields and not measurements. We admit the resulting parameterization includes possible biases, but parameter tuning can easily be done when changing the model.

In order to give an accurate answer to the two reviewers remark, **we added in this paper a comparison of 2m temperatures time-series between the E-OBS data and the modelled values we used**. This is the only meteorological parameter we found, close to observations. The results showed that correlations are very high (more than 0.95) for all studied sites and years, with a low bias. We can thus consider that the modelled meteorological time-series we used in this study represent well the meteorology observed for all the sites and years.

Note **we also recalculated all scores using the new scheme with both observed and modelled 2 m temperature**.

An example is given in the Table below: for the year 2010, the E-OBS and modelled 2m temperature are compared from the 1st june to the 30th november. The correlation is high and always over 0.97. The mean bias is lower than one degree Celsius. The calculations was done for all studied years and the results are similar.

Site	MeanMod	MeanObs	Correlation	RMSE	Bias
HUDEBR	12.690	11.796	0.979	1.593	0.447
HUGYOE	15.932	15.970	0.974	1.501	-0.019
HRZAGR	17.748	16.813	0.976	1.678	0.468
ROUSSILLON	15.080	15.741	0.971	1.630	-0.331
VELIKA	16.298	17.277	0.978	1.668	-0.489
SAMOBOR	15.480	16.130	0.979	1.484	-0.325
IVANIC	16.239	16.964	0.978	1.521	-0.362
SLAVONSKI	17.306	16.652	0.978	1.484	0.327
BJELOVAR	16.328	16.637	0.979	1.355	-0.155

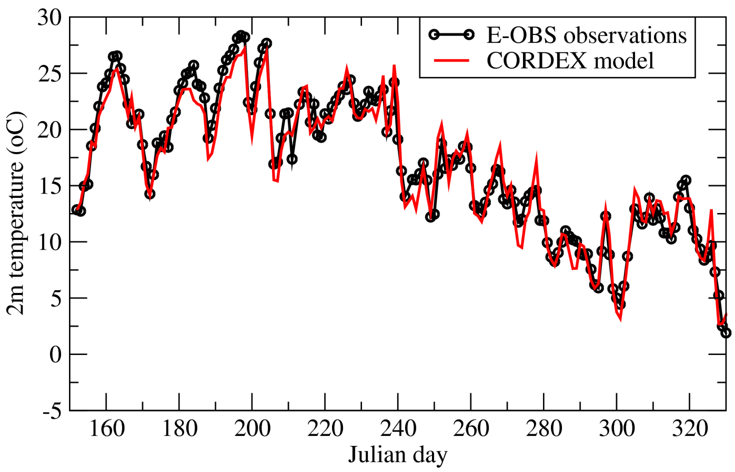

Illustration 2: Comparison of T2m from E-OBS and CORDEX simulation for BJELOVAR (2010)

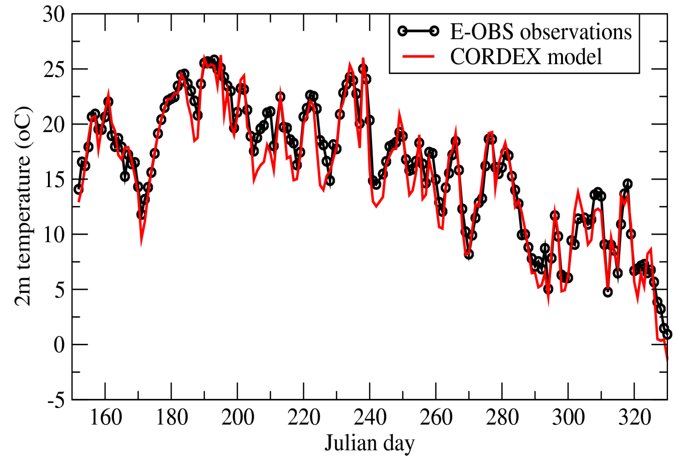

Illustration 1: Comparison of T2m from E-OBS and CORDEX simulation for ROUSSILLON (2010)

To see the comparison day by day, the following figure (added in the paper) presents daily time-series of the 2m temperature and for three sites: BJELOVAR and ROUSSILLON.

Note that all daily release values were recalculated with both these observations and the model outputs. These results replace the comparisons done with the [Sofiev et al., 2013] scheme.

<u>2- *a problem with the use of pollen concentrations measurements to constrain an emissions scheme.*</u>

Our study is focused on the daily release variability: if is important to note **we are comparing pollen counts and release factor in day to day variability (but not in absolute values)**.
Direct measurements of pollen emissions fluxes are not available. Our assumption is thus in most highly infested areas daily pollen counts should be roughly proportional to emissions within a corresponding spatial scale. The use of pollen counts as a proxy is not new, for instance a recent inventory of ragweed plants (Skjoth et al, 2014) used counts to spatialize the presence of ragweed (together with other informations). This is quite appropriate in absence of flux data as long as this assumption is clearly stated and kept in mind. We hope that the revised version better explains this assumption.

<u>3- *a problem with the use of the existing schemes for the comparisons and thus the validation*.</u>

We know that there is no existing scheme specifically dedicated to the daily ragweed pollen release, except the Efstathiou model. The [Sofiev et al., 2013], was developed and validated for birch pollen release only and, following the remark of the Reviewer#2, **we removed the results obtained with this scheme**, considering that its use is not relevant for this work. Other schemes exist, such as [Prank et al., 2012] and [Zink et al., 2013] but they are not adaptated to the comparisons we want to make in this study, based on the daily variability calculated using daily meteorological parameters. This is why they were not tested here. The use of the Efstathiou model in this study is just an indication to have some comparisons with other models, as requested by the reviewers.

<u>4- *The title of the paper does not reflect the content of the study.*</u>

For this new version, **we thus change the title** which is now: "Identification of meteorological conditions driving the daily ragweed pollen release variability".

Finally, and about the manuscript clarity, it was revised, many sentences were rewritten and corrected. If the table of contents remains the same, the text is really new. In addition: the model formulation was revised and is better explained. New references were added in the bibliography, as suggested by the reviewer #1.

We now give a detailed answers for all reviewers comments. Our answers are in blue in the text and after each reviewers remark.

Reviewer 1:

General comments:

This is an interesting paper that addresses one of the major knowledge gaps in numerical dispersion modelling of airborne pollen concentrations. The authors studied relationships between airborne ragweed pollen concentrations and meteorological factors, in order to determine parameters that govern pollen emission. Compared to previously described parameterizations (i.e. Sofiev et al., 2013; Efstathiou et al., 2011, Zink et al., 2013) this study emphasizes importance of temperature at 2m and shortwave radiation for pollen release. The manuscript does not present novel ideas and concepts.

We agree this study has some common goals with the cited papers: a model for ragweed pollen release. But we think some important things are new:
(i) our approach is based on daily variability. We mainly focus on the way to model the day to day release,
(ii) the approach is model for model. It means that we used the meteorological model and configuration finally used in general in transport pollen studies over Europe. This is important to have a scheme derived from model data, to finally reduce the uncertainty due to the different representativity between models and measurements.
(iii) in our scheme, there is no fit or tunable parameters, as often present in such kind of schemes.

We added comparisons to observed 2m temperature to show that this choice of "model to model" approach is robust.

The manuscript is in urgent need of a thorough description of pollen release from ragweed plants, so that the results can be related to actual biological mechanisms. When assessing ragweed flowering and the resulting pollen release, the authors not to refer to some relevant papers that are sadly missing from this study (e.g. Bianchi et al., 1959; Martin et al., 2009, 2010; Ogden et al., 1969).

In general, the bibliography was updated and many references were added in the paper.

Including information about the biological aspect of emission would improve the parameterization by addressing local environmental conditions that lead to pollen emission (amount, frequency). Instead, the authors use a statistical approach in order to provide a relatively simple approach for deriving the pollen emission. A statistical approach in general can be a robust methodology in certain studies. However, the authors have used an approach in this study that has some conceptual problems that relates to the source of meteorology. These problems can both result in questions in relation to the actual quality of the results, but also would also cause the study to have limited value for other scientist. These conceptual problems must be solved before the study is relevant for publication in AtmChemPhy.

A large part of the calculations done in this paper are statistical. But these statistics are done for many years and many sites in Europe. So, we expect the results are usable for a model in a deterministic way.

I suggest that the text is thoroughly checked by a native English speaker because some statements are not very easy to understand. E.g. "For nine stations in Europe and six years of daily measurements, correlations were calculated between daily release rate and surface concentration measurements." (Page10910 Row26). I suppose the authors meant daily pollen release rate and surface pollen concentration measurements. However, I did not see that measured daily release rate is available for this study.

Yes, the sentence is for pollen. This was changed in the text. There is no "measured daily pollen release rate" and this is the challenge of the paper: establish correlations between "modelled daily pollen release rate" and "pollen counts" in term of variability. The text was changed to be more clear.

Specific comments:

Probably the major issue that would cause uncertainty of the results are assumptions made for the three out of four components of the emission flux (i.e. the ragweed density distribution in number of individual plants per square meter, the annual production in grains per individual plant and the knowledge of the start and end date of the pollen season).

The ragweed density distribution has to be modeled separately, and stands here as a calibration factor. We agree that there is a very large uncertainty on this factor. Our emphasis in this article is on the modeling of the short-term emission fluctuations (daily time scale). The season start/end is also to be modeled separately with a phenological model, which is beyond the scope of the article. This is the reason is also why we chose to make assumptions for these processes.

It is questionable whether pollen and meteorology data used in the analysis could be identified as local observations. Pollen traps when located at 10-20m above the ground are considered to be representative for area 30km in diameter (most likely even more under specific conditions in plain terrain). Also it was shown that for herbaceous pollen spores the height of the pollen trap from the ground would notably influence its representivity (Spieksma et al., 2000). Therefore, more detailed information about trap location is required (especially to support introduced assumption that the measurements are close to the emission sources in all study regions). Additionally, the selection of the sites is based on the assumption that if a station represents a local maximum, then this is due to a large local ragweed population, which then justifies the statistical approach. However, it has been shown a number of times and for a number of pollen types, including ragweed, that this assumption is not necessarily the case (e.g. Skjoth et al, 2009, Kasprzyk, 2008). A large local maxima can easily be due to a large population about 50-100km away, which during flowering tends to blow the pollen in certain directions.

Since we are aiming at modeling daily variability, our approach considers also a spatial scale of changes that is associated with this time scale. Considering a mean boundary layer wind of about 5 m/s the transport of pollen but also weather variables within a distance of 100 km takes about 6 hours so our model aims indeed at representing being representative of this spatial scale (0-100 km). This is now specified in the Introduction.

The authors therefore need to provide more convincing arguments, why the selected sites are usable for this kind of study. It is aerobiological data originates from EAN (European Allergen Network) the authors should not term pollen data as "surface concentrations". Please give some detail about methodology applied for collecting pollen data used in this study. Are these collected by the same team and if not, do the applied analysis techniques give comparable results?

From the above argument, our approach should select sites to be representative of emissions within a footprint of ~100 km but not of long-range transport. The measurements were collected from various institutes but all data were quality-controlled and homogeneized in the framework of the FP7

ATOPICA project.

The authors are correct that location of sites and high pollen counts would limit the influence of pollen transported from distant sources (Page10895 Row10). However, the Pannonian Plain is not homogenous with the respect to climate or with the respect to abundance of ragweed pollen sources (see ragweed pollen source inventory over Pannonia Plain by Skjoth et al. (2010) and a large pollen index at one particular site can easily be due to prevailing transport from sources 50-100km away (see arguments above).

See above answer
The references to Skjoth et al. works were added in the article.

If this is the case, then the authors will correlate local meteorology with emission of pollen that happens in another area, which thereafter takes hours to arrive to the pollen trap. Pollen captured around each trap could easily originate from other areas of the Pannonian Plain, and so correlating airborne ragweed pollen concentrations with meteorological conditions in the area of the pollen trap are unlikely to produce an effective parameterisation of pollen emission from the source. The number of pollen monitoring sites over a particular area could be suitable for estimation of regional emissions. However, the inclusion of a single site in Rhone-Valley does not seem to be sufficient to give an estimation of regional ragweed pollen emission over that heavily infested area. Similarly, the inclusion of a number of sites grouped in the southwestern part of the Pannonian Plain, one in the East and one in the Northwest does not seem representative for distribution of ragweed pollen sources in that region (Skjoth et al., 2010).

We agree that the “daily approach” cannot do better than modeling emissions within a radius of about 100km. In order to precisely model the emissions at, say, a hourly time scale, one should then use a more process-oriented model and use measurement of pollen fluxes, which are unfortunately not currently available to us. Some sites have measurements every 2 hours which could help achieving this, but the sites are not numerous, so this would be difficult to have a European coverage. Besides, temperature fields (which are the main precursors here) are usually smooth fields with weak variations at 100km distance except in complex terrain areas.

The temporal frequency of modelled data was three hours, but the temporal frequency of the airborne pollen data is unknown (was it a daily average or bi-hourly values?). There is no indication what exactly was correlated using Pearson's product moment correlation (daily values or bi-hourly values?). Also, what is the time frame of the correlated datasets? For example, correlating for period that ranges from the 1st Jan to 31st Dec would notably overestimate correlation coefficients (a lot of zero values for pollen concentrations out of the main pollen season).

The study is based on daily values for pollen concentrations as explained in the paper.

None of numbered studies (Page10893 Row23) analysed emission as the local phenomenon. They correlated airborne pollen to meteorological conditions without knowledge about the origin of this pollen and conditions at the location of its emission.

In the abstract authors wrote: "a new scheme based on temperature, specific humidity and precipitation rate is proposed". In fact, the proposed parameterisation also uses shortwave solar radiation. What is the biological/physical background of the positive correlation that is recorded for pollen concentration and shortwave solar radiation (linked to the day length)? The authors also wrote: "Recent studies have also shown that SWd is an important factor for ragweed pollen emissions." but did not supply references to support this statement.

OK, the abstract was corrected to also cite the use of the shortwave solar radiation. We agree with this remark and the bibliography was improved for the impact of these meteorological parameters on the ragweed release. For the shortwave solar radiation, the impact is indeed linked to the day length, i.e the magnitude of daily received radiation. References to [Thibaudon et al., 2014] and [Smith et al., 2013] was added in section 3.2.

The quality of meteorological input is critical in the study. Without that the authors would not be able to produce a statistical based emission model. The authors use model based meteorological data from the WRF model instead of local observations. The authors argue that the quality of the model has been validated in a previous paper by Menut et al (2013). This is however not sufficient. In the cited paper it is directly written in the abstract that all simulated meteorological parameters have a bias. It is a well-known fact that a bias in model based meteorology can cause large errors in emission models in relation to nature. This will therefore also be the case here, which is documented in the validation paper that the authors cite. A recent study by Liu et al (2014) covers this aspect quite well, and when model based meteorology is used it has always been recommended to use bias correction in such studies (e.g. Dosio and Paruolo, 2011). This has not been done here. Neither have the authors tried to assess the error they make by using data that are not bias corrected. This means, that the statistical model will be tightly linked to that particular setup of the WRF model. It is also a well-known fact that bias in meteorological models varies on things such as location and grid resolution. In fact, it has also been shown that the bias will change substantially by using another of the planetary boundary layer options in WRF (Coniglio et al, 2013). As such, the derived parameters in the emission model cannot be directly applied if the meteorological setup is changed, and the study would have to be repeated. Due to this, the statistical model and its derived parameters will have limited use as an emission model that can be coupled to a weather forecast model like WRF.

This is right that meteorological models are often biased and these biases may change depending on location, period and parameterization used. But you can see the comparisons added in the paper between observed and modelled T2m: the bias is always lower than 1 ºC and we consider this small difference is low compared to the day to day variability during the whole season and for the daily pollen release.

Secondly, there is an important point concerning the setup of the meteorological model. According to Menut et al (2013), which is the paper that contains the model validation, the setup is designed for regional climate model studies. The authors have used a grid resolution of 0.44 degrees. I compared this with a map over France in the Times Atlas of the World that shows both kilometres and degrees. I could see that in France such a grid resolution correspond to roughly 35 km x 57 km. In the WRF manual, I found the typical settings for regional climate runs. In that they write 10-30km. Why have the authors used such a coarse resolution in WRF? It seems to be outside the general recommendations for regional climate runs when the focus is on meteorological data. This must have had a negative impact on the model results. The original data set that was presented by Menut et al (2013) was intended for air quality modelling with a chemical transport model and not for statistical modelling that does not take atmospheric transport into account.

The WRF manual provides "typical" settings for regional climate runs with a resolution of 10-30km. Here, we are using an horizonatl resolution of ~50km. Between these two resolutions, there is no differences in term of meteorological processes nor that in term parameterizations. Our resolution also correspond a to regional climate setting and the WRF intervals is just an indication..

Our intention here is eventually to carry out climate simulations, hence the relatively coarse resolution to be able to run the model over decades. Note also that the current resolution of the ERA-Interim re-analysis is about 70km. The resolution of the model is that of all CORDEX climate projection exercises. EURO-CORDEX also proposed a higher resolution but the simulations were not nudged (or guided by re-analyses). These arguments are developed in the article.

The coarse resolution of the meteorological data will also have another impact on the study. It is generally accepted that data that is obtained from a pollen trap covers up to 30km away, when the studies cover long time periods. This means that it is expected that observed meteorology for statistical modelling should be within this 30 km zone. Preferably within 10-15 km. Interestingly, this fits very well with the recommended WRF settings (10-30km) on climate runs, but it does not match with this study. In fact, using a 35kmx57km setup must mean that the overall meteorology will cover a region that is more than twice as big as the pollen trap. It is difficult for me to see how such coarse meteorological data can be claimed to be representative for studies in relation to data that are obtained with a pollen trap. It must be something like trying to compare pears with apples. In my point of view, the grid resolution in the model must be below 30km in both x and y direction, which corresponds to 0.22 degrees before it can be used for this type of study. If the general recommendation on statistical modelling is followed, that the meteorological site should be maximum 10-15km away then this corresponds roughly to 0.11 degree resolution in the WRF model. Also, the study should include bias correction or at least if the authors can show and quantify that the error they obtain without using bias correction is very limited.

Why do the authors mention Parietaria in the introduction. It is not relevant to the present study.
Yes, we agree and this sentence was remoed from the introduction.

Technical corrections:

The manuscript gives a lot of information and the use of Tables and Figures is mainly appropriate. However, there are far too many figures. The amount of figures could easily be reduced without changing the results or the conclusion. Figure 5 showing time series is redundant because the results of evaluation of the relationships between modelled meteorological data and measured pollen concentrations have been already presented in Table 3.

The Figure 5 is important because this is the only moment when we explicitely show the daily correspondence between meteorological time series and ragweed pollens. The Table 3 also contains the same kind of informations but this is aggregated scores, thus a different message than in Figure 5.

Time series of measured pollen concentrations and measured meteorological parameters in the region of these two pollen monitoring sites would fit better to the statement made by authors: "Figure 5 focuses on two specific sites and periods in order to better understand the relationship between meteorological variables and observed concentrations."

Scientific names of plant species and genera should be in Italics.
Ok this was corrected.

Page10897 Row5: "tqhree" should be "three"
Ok this was corrected.

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Reviewer 2:

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.

We have made revisions in our manuscript and took into account the reviewer's remarks. We emphasize that our goal was not here to surpass existing approaches but to propose a possible methodology, based on the identification of most robust statistical relations between daily weather and pollen counts of ragweed pollen, to simulate the daily variations of the release term.

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.

The goal is not here to construct a new regional model, this is the goal of the ATOPICA project. This goal will be reach step by step. The first step is to estimate the meteorological conditions conducive to daily ragweed pollen release variability. It was the first title of this paper and we agree with the reviewer to use this original title, to better match the paper content.

The title is thus changed and is now: "**Identification of meteorological conditions driving the daily ragweed pollen release variability**".

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.

This is indeed our goal and we hope it is now better explained in the introduction: the goal of this study is to better model the daily fluctuations of the pollen release. Modeling the other factors is beyond the scope of the article, and would require in particular a phenology/vegetation model.

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.

As emission flux measurements were not available, we developed here a methodology using an assumption that in most highly infested areas daily pollen counts should be roughly proportional to emissions within a corresponding spatial scale. The use of pollen counts as a proxy is not new, for instance a recent inventory of ragweed plants (Skjoth et al, 2014) used counts to spatialize the presence of ragweed (together with other informations). This is quite appropriate in absence of flux data as long as this assumption is clearly stated and kept in mind. We hope that the revised version better explains this assumption.

Finally, please note we are not comparing absolute values of pollen counts and modelled release **but only their day to day variabiility**.

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.

Yes, indeed, this is an important result. Because in previous papers, such as (Zink et al., 2013), the correlations are low and their statistical significance is not reached (as described in the paper itself). In this study, the correlations are higher and the statistical significance is correct. We can thus consider this study is an improvement compared to the previous studies.

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.

All calculations are now presented with the new scheme with the observed (E-OBS) and the modelled

(WRF) 2 m temperature. We showed that the differences are low between the two and thus our approach is robust.

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.

The fact that the Efstathiou model is considered by the reviewer as "poorly validated" is perhaps true. But this is the only model we found for daily calculation of ragweed pollen release calculations, based on meteorological variables and not on tuned parameters.

[Prank et al., 2012] and [Zink et al., 2013]) are using fixed parameters for this day to day variability: we do not criticize these approaches, but they just do not fit the goal of the study presented here.

Two ragweed emission models developed and evaluated for Europe have been ignored.

If a reviewer considers there is an important lack in the bibliography (i.e he knows exactely what study is missing), its role is to clearly cite the missing references (as done by the reviewer #1). This remark is perhaps true but difficult to use for us.

But, we updated the bibliography (including the remarks of reviewer#1) and, to our knowledge, the only new paper on this topic and over Europe was published by [Zink et al., 2013]. In this paper, they conclude that their scores are low and their statistics not significant. This is why this new scheme was not tested, giving no added value in this study.

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.

The reviewer can be assured that we read articles we cite and for which we use the parameterizations. The development in our paper is not for concentrations. The concentrations are only used to evaluate the release scheme to provide realistic daily variability, over several years and several sites.

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".

In this new version, we compared E-OBS 2m temperature time-series to the modelled values we used. We showed there is a very good agreement between the two, proving that the day to day variability of all modelled meteorological parameters is a good proxy to evaluate the real meteorological variability observed in the studied sites.

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.

All references we know about modelling of ragweed pollen release are now in the bibliography.

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.

Yes, we agree there is little similarity between birch and ragweed. The only similarities we pointed out are in the way to model the release term of these two species. See for example [Zink et al., 2013] using the same scheme but with different input parameters. The sentence was reworded to be more clear.

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

The schemes used in our study are also used for ragweed pollen emissions release calculations as shown with the references in the paper (see section 4.3).

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.

This is right that representativity depends on many meteorological (not constant), vegetation (not constant) and topographical (constant) parameters. This is why we only say: "a few hundred meters". In fact, this is more than this distance and the pollen measurements sites are chosen in order to have the largest representativity as possible. The reviewer 1 write "30km". As answered to the reviewer #1, and considering continental meteorological conditions, our model aims indeed at representing processes being representative of this spatial scale (0-100 km). The text was changed to better explain the "representativity" problem, always present in model studies when model results are compared to local measurements.

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.

Some details were added in the article about the measurements technique, as fully decribed in [Laaidi et al., 2003].

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.

Yes, this is true: high resolution meteorological models outputs exist or analyses, such as the SAFRAN data but for several parameters nothing guarantees absence of biases, and not all parameters studied here are available. But the high resolution is not the goal of our study. Our goal is precisely to quantify if we can simulate the pollen emissions with a model having the resolution of current regional climate models, or regional chemistry and transport models (such as in the MACC project). Since we want to analyze the correlations between modelled meteorology and measured pollen concentrations, it is important to use the current models configurations and no observations in the release calculation.

Also, direct measurement of fluxes are not available and instead pollen counts are used, which integrate emissions over a rather large scale. Therefore there is little hope that a parameterization based on high-resolution weather and pollen counts would provide more accurate numbers than those provided by low resolution weather data.

Equation 1 is a triviality and should be removed.

Yes, we can remove this equation. But in the previous "technical review", the reviewer #1 asked for details about the correlation equation used.

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.

There is no suggestion that the model is able to predict the total annual amount. l.5, we explain that this amount is estimated using measurements. In the conclusion, we explain that a vegetation model is necessary to model this quantity.

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

This is equation 3 and, Ok. this was changed accordingly.

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.

This is right and this is the best way to completely understand the scores of the release calculations.

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.

We agree with this remark. We already read the [Prank et al., 2013] and [Martin et al., 2010] papers but we were certainly wrong by estimating that the release term was calculated in the same way between birch and ragweed in the SILAM model. The [Prank et al., 2013] article is not really verbose about the methodology used for the release term calculation. After this reviewer remark, we have to conclude there is no really a release term calculated in this model, but only the use of a Gaussian function, such as the phenology function. The [Sofiev et al., 2013] scheme is for birch modelling only and the adaptation we made in this paper is indeed not relevant. Thus, we removed all results with this scheme in our study. Results are only compared to the [Efstathiou et al.] model.

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.

Yes, this is one of the assumptions made in this paper. We consider that ragweed emissions occur during the day and under unstable conditions.

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.

As previously answered in the technical review, the scheme was selected because it is the rare already existing and using explicitely meteorological variables.

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?

The fact that ragweed emissions are predominant during the morning is explained in (Holmes and Bassett, 1963). This reference is in the paper. Regarding the principle of pollen trap, the analysis frequency depends on the user choice and not on the instrument capabilities. In our case, the concentrations database provides daily values (see also answer below and the reference to [Laaidi et al., 2003])

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.

We agree with the reviewer and the Figure was removed, considering the text is enough.