Interactive comment on: "A new model of ragweed pollen release based on the analysis of meteorological conditions" by Menut et al. (acp-2014-184)

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. 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).

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.

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.

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).

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. The authors therefore need to provide more convincing arguments, why the selected sites are usable for this kind of study. It is

important to emphasise here that routine aerobiological sampling is performed at roof level. Therefore if 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?

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). 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).

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).

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.

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.

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 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.

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.

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.*

Page10897 Row5: “tqhree” should be “three”

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