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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric ammonia variability and link with PM formation: a case study over the Paris area" *by* Camille Viatte et al.

Anonymous Referee #2

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General comments

In this study, Viatte et al. use satellite observations (CrIS, IASI) to a) characterize the spatial and inter annual variability of ammonia column over Western Europe and its drivers and b) examine the connection between NH3 and PM2.5 over Paris. The material presented is interesting and well suited for ACP However, I have some significant concerns regarding the robustness of some of the conclusions and the lack of connection between a) and b). These need to be addressed before I publication can be considered.

General Comments

a) there are places when the authors make fairly definitive claims with insufficient sup-

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Discussion paper

port/references. For instance Line 49: it is stated that N causes species/ecosystem extinction. A specific reference is needed. Line 341 and discussion above. This discussion is too speculative and needs to be much better supported. Was more corn planted in 2011 than in 2012? Were planting dates shifted earlier in 2011 relative to 2012? This is critical since the authors then state that they have shown that meteorology and farming practices account for the interannual variability in NH3 column. Line 374 It is stated that the correlation is "good" based on Fig. 7 (r2<0.3) . What is the p value, what is the uncertainty on the slopes given the large error bars shows in Fig. 7? In general, the authors need to be more quantitative when reporting statistics: always give p value for correlation (e.g ., line 331 and 333) and uncertainty for slopes.

b) there is very little connection between a) and b) in the current manuscript. In part b), the authors focus on the relationship between PM2.5 and NH3 in two (fairly similar) years (2014,2015). The main conclusion is that meteorology (temperature, local BL) probably controls whether NH3 contributes to PM2.5. This is interesting although very much expected from studies performed in other regions. From part a), I was instead expecting the authors to consider whether the considerable variability in NH3 sources over Belgium/Netherlands could impact PM2.5 over Paris. From part a), I was also expecting to have the authors show whether CHIMERE is able to capture the observed correlation between PM2.5 and NH3. This could help understand whether the observed PM2.5 enhancement results from production of ammonium nitrate in Ile de France or from transport of ammonium nitrate/sulfate or other aerosols from Belgium. I fully appreciate that such analysis will require significant work. However, without a significantly stronger connection between part a) and b), I would recommend the paper be split, with part a) being more readily publishable.

Technical comments

Section 2.3 the description of CHIMERE is far too short (especially with respect to the treatment of ammonia. For instance: -> how is dry deposition represented? Does it include the bidirectional exchange between land and atmosphere -> what is the temporal

resolution of the emissions? Does it include a diurnal cycle? It would be useful to show the seasonality of the emissions in a few regions, to help the reader better analyze Figs 2 and 3 -> how is the gas/aerosol partitioning of NH3 represented (ISORROPIA?) -> I assume that NH3/NH4/NH4NO3 in CHIMERE have been evaluated previously? Please provide reference for these studies at this stage. I also encourage the authors to show how the configuration of CHIMERE that is used here performs against surface observations (e.g., EMEP wet deposition/concentrations). This could be briefly discussed in the main text, with figures in the supplementary materials.

Section 3.1.1

It would be useful to include a map showing the distribution of livestock and major crops in Western Europe so that the reader can see the relationship between NH3 emissions and the different sources described by the authors.This would be especially helpful as some of the material the authors refer to is in French.

Fig. 5. This figures shows first and foremost that there is good correlation between skin temperature and precipitation at the regional level. I think it would be more relevant to show the relationship between temperature/precipitation and NH3 anomaly In addition, I assume that the precipitation/temperature anomalies exhibit some significant spatial variability? Do you weigh the anomaly by the average NH3 column? High NH3 columns only cover a small fraction of your domain and it's unclear to me why it would respond to the average temperature change (vs the local change)

Section 3.2

I am a little confused by the need for the standardization. CrIS and IASI seem reasonably close, so why not use the model absolute NH3 column. In addition, Fig. 6 only show one CHIMERE time series, shouldn't there be two, one for CHIMERE sampled at the IASI overpass time and one at the CrIS overpass time (with AK)..

Line 351 I am not sure I understand the motivation for picking this years. Why not use

the climatological seasonality? Why are these years more useful to benchmark the model? They look fairly similar as far as I can tell from the supporting material.

Technical comments

They are a few issues with language. It sometimes (rarely) makes it challenging to understand the manuscript line 28: regression slope. Remove slope line 63: related->relative Line 112: many of studies? Line 283: farming species? Do you mean livestock? Line 300. What are non-poultry granivorous (animals)?

Fig. 7 What do the error bars correspond to?

Fig. 9: Same than Fig.7 -> “Same as Fig. 8”

Fig. 12: Define IQR

Line 220: I don’t understand the distinction between inorganic, organic and natural aerosols?

Line 487. Why is the value given on line 476 different (mean/median?)

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