

[Interactive  
Comment](#)

# ***Interactive comment on “Assessing the ammonium nitrate formation regime in the Paris megacity and its representation in the CHIMERE model” by H. Petetin et al.***

## **Anonymous Referee #3**

Received and published: 9 October 2015

This manuscript evaluates several aspects of nitrate formation in Paris, including an analysis of ammonia and nitric acid observations from several sites, along with a corresponding analysis of the CHIMERE modeled concentrations for the same locations and time periods. Observations of ammonia and nitric acid are analyzed to determine regions of origin (through back trajectory analyses). CHIMERE model estimates are analyzed and compared to observed concentrations and a number of statistical metrics are calculated to attempt to determine how well the model estimates both the model concentrations of various species and the conditions under which those species pollutants are formed.

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)

While I think the scientific methods presented in the manuscript are sound, the manuscript itself would benefit substantially from a great deal of editorial review. I found I often had difficulty understanding what point the authors were trying to make, and had to re-read sentences or entire paragraphs to finally understand the point being made. In addition, the use of chemical abbreviations is lacking, with the authors often switching back-and-forth between chemical names and chemical abbreviations. This also applies to meteorological abbreviations, such as relative humidity (RH). Also, there are a number of spelling errors in the manuscript (e.g. traffic) that need to be addressed. All these issues detract from the message trying to be conveyed in the manuscript. I would like to see these issues addressed before the manuscript is accepted. I also have a number of specific comments/corrections as well.

General suggestions:

It might be useful to look at an inert gas species (e.g. CO) that is well captured in the emission inventory as a indicator of the quality of meteorological simulation, in particular the evolution of the boundary layer throughout the day. The authors do discuss the meteorological performance to some degree, but this type of analysis might be enlightening to the reader, and presumably measurements of CO are available at a number of within the modeling domain.

I'd like to see better support of some of statements in the manuscript. Often the authors will point to an aspect of the meteorology, emissions or model as the cause of a particular deficiency in the model performance, but without what I feel is adequate support or a reference. One example is the last sentence of section 4.3.1, stating "Such a pattern may be due to high measurement uncertainties occurring for low TNO3 concentrations". I don't recall any discussion of this uncertainty, and there is no reference provided to support this claim. Also, the authors point to errors in agriculture emissions as a large source of error in the model results, however, I don't think this claim is well supported in the manuscript (and if the authors think it is, it should probably have its own section detailing why those emissions are in error).

Specific comments:

23736, 13: Define EMEP.

23738, 21: Define PRIMEQUAL FRANCIPOL.

23741, 11: What is an "air quality survey"?

23741, 14: Define MELCHIOR2.

23742, 17: MM5 is quite old now. Why was a more up-to-date meteorological model, such as WRF, not used?

23743, 13: It would be good to explain to the reader here why the MOD-noddep simulation is being performed.

23743, 19: Change "field" to "the observed".

23745, 18: Explain here why the S ratio is being calculated.

23746, 13: 4 ppb is not in the middle of the range between 0.4 and 63.6 ppb.

23747: It would be good to briefly explain here how exactly temperature and relative humidity affect the formation of NH<sub>3</sub>.

23749, 26: Change kerbside to curbside (the more common form of this word).

23751/23752: How exactly were NH<sub>3</sub> emissions from traffic added to the simulation?

23754, 1: I don't feel like the statement "in particular the lack of dynamical treatment of agricultural emissions as a function of environmental factors" is well supported. Please add additional support for that statement.

23754, 26: How do you know that high winds are the reason that HNO<sub>3</sub> is not higher for that period?

23757, 8: Why is dry deposition of HNO<sub>3</sub> in the model thought to be too low? Is there a reference to support this assertion?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



23763, 16: Since the observations of NH<sub>3</sub> and NH<sub>4</sub> (and HNO<sub>3</sub> and NO<sub>3</sub>) are not measured at the same sites, it would be useful to provide some discussion of the heterogeneity of those species in space.

Table 1: Not sure this table is really necessary since it conveys minimal information.

Figure 1: It would be useful to provide the different domain resolutions on the figure.

Figure 3: It's difficult to pick out the different lines since all the colors are similar. Please change to more contrasting colors.

Figure 6: Same suggestion as for figure 3.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 23731, 2015.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

