General comments:

The highlight of this paper is a documentation of the sensitivity of NOx emission estimates on the quality of the meteorological fields and the winds in particular. As shown, these issues are especially critical when the resolution of the observations and the models advances to the km scale. This sensitivity is demonstrated in a clear way, and to my opinion is an important aspect to keep in mind for the development of future regional analysis systems. As such, I am still in favour of publishing these results in ACP.

The revised document has a completely new set of figures and new experiments, which provide more insight into the behaviour of the analysis results. This has improved the paper considerably compared to the ACPD version. Despite this, there are still aspects of the paper that can be improved, as documented below.

It would be good to check the English spelling throughout the document, especially for the parts which changed since the previous version of the document.

Generally the abstract is not well written and not well structured and should be revised. It should clearly state the aim of the study, the tools and simulations, and the conclusions. See my comments below for further details.

Specific comments:

- Abstract: The first 10 lines of the abstract would be better placed in the introduction, and may be largely removed.

- Abstract: "Accurate observation and modelling of these variations require spatial resolution of order 4 km." This fits well with TEMPO, but for me it is unclear what the quoted length scale of 4km is based on. It is not a result of the paper so I suggest to remove it from the abstract.

- Abstract: The aim of the study can be provided in more detail. In particular the investigation of the sensitivity of emission inversions on the accuracy of the wind analyses.

- Abstract: What is missing is a description of what is done (the OSSE experiments and tools). The ingredients of the OSSE setup should be explained in a few sentences.

- Abstract: "Last but not least our results show that the information of wind uncertainties is not important for NOx emission estimation." This is unclear. First it is claimed that it is essential that the wind RMSE should be below 1 m/s, which seems to contradict the statement that the wind uncertainty is not important !? See also below.

- Introduction: Is well written, clearly states the aim of the study and contains a good set of references. No further comments.

- Sec 2.1, p4, l12: "RADM2 predicts peak NO2 concentrations and the timing that the peak occurs very well when the mechanism is tested against the representative environmental chamber experiments". Is there a reference ?

- p5, l13: "In our next paper we will discuss the potential benefit from assimilations of chemical observations to improve the meteorological fields." I would suggest to remove this line: it is not interesting for the reader to know about future studies.

- Tables 2 and 3: For people not familiar with DART and WRFDA these tables are difficult to understand, e.g. terms like "cv_options" and "je_factor" is not meaningful. It would be good to explain these options in the text.

- Section 2.4: The description of the emission updates is described in a confusing way. First, time-evolving emissions are introduced. Then it is mentioned that "time-static" emissions are the aim of the study. After that, emission scaling factors are introduced, which change after each assimilation, which seems to contradict the "time-static" remark. Please re-write and explain more clearly how emissions are analysed.

- p7, l1: "We calculate a layer dependent Box-Air Mass Factor" This statement is a bit confusing as it suggests that a radiative transfer code is run. I understand that lookup tables from Bucsela et al are used. Please formulate more clearly.

- p7, l6: "We assume clear-sky conditions for all observing scenes." Why is this done? This will increase the number of observations artificially.

- Table 4: The normal distribution N(200%, 100%). What does this mean? Why are there two percentages? This seems to contradict the notation of normal distributions as given in p7, l32, where I would expect the first number to be the nature run value.

- p10, l9: "succeeds in recovering the true emissions (Figure 5)." I guess you mean Figure 6? The order of the figures should follow the order of the references to the figures in the text (5 <-> 6).

- p11, l5-10: The emission update scheme is explained here. The "scaling factor for t-1" seems to be inconsistent with the discussion in section 2.4, which only mentions updates for e_{i+1}. The two texts seem to be inconsistent. Please explain the emission update process more clearly.

- p13, l24: "Last but not least our results show that the information of wind uncertainties is not important for NOx emission estimation." I guess that the combined wind/NO2 analysis could also lead to adjustments of the winds which give a better match between observed NO2 concentrations and the location of the emission sources. The ensemble, with a spread equal to the wind uncertainty, would allow for such adjustments and could in principle outperform the system where the winds are prescribed. It seem the authors claim that such benefits are not significant?

- p13, l26: "Assimilations of NO2 only with the meteorology constrained from an hourly weather assimilation product would perform as well as the joint assimilation of meteorology and chemistry."
Do you suggest that a CTM with prescribed meteorological analyses will perform as well as a fully integrated chemistry-meteorology analysis system?

- p13, l34: "will fail" -> will also fail