General Comments

This paper tackles the issue of insufficient spatial resolution of modeled pollutant concentrations over Europe. I think that this is an open issue and therefore, new research in this area is definitely welcome. The authors present a post-processing technique, that combines finely resolved emission maps and dispersion model simulations to downscale regional chemistry-transport simulations at finer scale. I appreciate the special effort that has been made to keep the application simple and generalizable to different case-studies. However, I think that with the simplifications the authors made in order to keep their application 'light' – namely the use of a meteorology that does not match the case study and the averaging over annual time scale – they shift the original question from the actual scientific issue of the unresolved sub grid scale variability to the very technical one "how to deal with European air-quality modeling since no high resolution inventory is available all over the continent". I wonder if the developed methodology is of any interest over areas where such high resolution bottom-up inventories already exist (e.g. the US), or in a future where such an inventory becomes available over Europe.

What strikes me most with the manuscript is the use of the term 'subgrid model'. The authors claim to develop a subgrid model to simulate pollutant concentration variability within regional CTM grid cells, but in my view, what they develop is a post-processing, downscaling technique to map regional scale simulations on finer resolution emission data using some parametrization to account for dispersion of these emissions. I think that referring to this technique as 'modeling the sub grid scale variability' is misleading because there is nothing in the formulation of the CTM that has been changed here to actually model the unresolved variability. If their model was actually a sub grid scale model I think that the authors should have refer in the introduction to the different approaches developed so far: Galmarini et al., 2008, Cassini et al., 2010, Korsakissok and Mallet 2010, Valari and Menut 2010 among others. Their references though, span the field of post-processing downscaling techniques rather than the issue of solving the unresolved sub grid variability.

Having said so, I think that seen as a post-processing, geostastical, downscaling technique that combines a fine resolution emission map as proxy with some assumptions on pollutant dispersion, the research presented in the manuscript is worth publishing. This would require significant changes in the formulations starting from the title and abstract. The introduction, discussion and conclusions should also change to put the work on this different framework.

Specific comments

For simplicity and to make their application easily reproduced, the authors chose to use a generic meteorology, not simultaneous with their case-study. The parametrization of emission dispersion does not include any sub grid process such as fast chemistry or deposition. What is more, the time scale of their application is much coarser than the CTM's (annual vs. several minutes though the time step of the EMEP model is not explicitly mentioned). Those choices, make it clear, in my opinion, that this effort is not meant to solve the subgrid variability. To do so, the effort would rather focus on high resolution meteorology and emissions on line with the regional CTM to capture the unresolved features of atmospheric chemistry and dispersion.

p.1 ln 25: I am not convinced that this method could be extended in shorter time scales and other pollutants. The meteorology does not match the case-study and I think that looking at hourly or even daily data the discrepancies due to this mismatch should become very large. Since there is no coupling with chemistry, this approach seems to me relevant only for chemically inert species (or at time scales where active species could be considered as inert).

p2. ln 20-25: If the mean value is correct it would be surprising that the urban background concentration is underestimated. It would make sense to say that near-sources concentration levels are under predicted but if the background value is off as well, I don't see how we could get the mean value right.

p4. ln 5: Wouldn't it make more sense to use the same meteorology as in the EMEP model at least for this sensitivity test?

p.5 ln 30: I am wondering what do emission sources as large as 1km2 could possibly represent. In my understanding, dispersion models are conceived to represent emission from point sources such as industrial stacks. Is this the right model to represent large area sources such as crops or residential emissions? Is this type of modeling adequate to represent dispersion around busy roads? Don't dispersion patterns depend on the emission sector?

p7 ln 10: I don't think it is appropriate to say that "the sub grid model preformed better than the EMEP model". It would be more fair to say that the downscaled version of the EMEP model compares better with observations. The same remark applies on many formulations throughout the results section.

p7 ln15: This makes me wonder how would results look like if no dispersion was taken into account and the same process was done only by using the 1km (or 7km or both) emission proxy.

p8 ln 5-10: I think it would be interesting to look at the effect of the meteorological dataset at a finer time-scale. Especially since the authors claim in their conclusions that this method is easily applicable at finer time scales.

p9 ln 20-25: I think that the comparison with Denby et al., 2011 study is off mainly because they worked on hourly data and not annual.

p9 ln 30: I think that the correlation in Schaap et al., 2015 is on time and not in space as in the present study.

p10 ln 10: The question inevitable arises of whether a direct EMEP run at 7km resolution with its corresponding meteorology would bring about the same improvement as the downscaling developed in the present study. And in this case the data would be directly at hourly resolution.