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Interactive comment on "Impact of agricultural emission reductions on fine particulate matter and public health" *by* Andrea Pozzer et al.

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We thank the referee #1 for the accurate review.

Main comments:

1. **Presenting the global perspective is interesting but I personally doubt that such work has any implications on regional policy as it entirely misses discussion of regionally specific aspect of mitigation opportunities analyzing rather unrealistic scenarios of agricultural emissions; additionally referring to 2010 levels while emissions from livestock and arable land production (fertilizer use) are likely to increase further in several regions, especially in Asia.**

This study presents the effect on atmospheric chemistry of hypothetical reductions of agricultural emissions. Although these reductions are hard to reach in reality, the model results allow us to understand the link between ammonia and $PM_{2.5}$ for different ammonia levels. Even though a 100% reduction of ammonia emissions cannot be achieved by policies, it is of scientific interest to study the impacts of different reduction scenarios. Further, the experiments show that, although a reduction of ammonia would indeed reduce the fine particle concentrations, the particle acidity would be strongly affected, and these effects must be taken in account when establishing emission reduction policies. In the manuscript we have listed a few of the numerous studies which analyzed technologies for ammonia emissions reduction from livestock production or manure/slurry application. A good overview is given by Webb et al. (2006), who reviewed different methodologies for ammonia emissions reduction. Webb et al. (2006) showed that different technologies could produce different emissions reduction, ranging from 20% to 80%. In the same study it is shown that for the United Kingdom a moderate reduction in ammonia emission is easily affordable, while the costs are likely to increase exponentially for reductions above 25%. The same control measures would become even more difficult to apply in developing countries or where strong ammonia emissions are present (such as South Asia), as these should be adopted massively. Further, as noticed by the referee, livestock production is projected to largely increase in the Asia region (Delgado et al., 2001). On the other side, the societal cost for hospitalization and/or premature mortality due to air pollution should be also considered, as the large expenses for ammonia emissions reduction could be compensate by a reduction in premature mortality. This is however beyond the present study and a further work in this direction is planned.

2. **Another aspect of this work that needs more clarity is the issue of temporal distribution of agricultural emissions used in the simulations. The authors**

make comments about the potential issue with temporal pattern of emissions but do not explain that any further and do not provide the actually used profile which makes it difficult to comment on that further.

We thank the referee for pointing this out. Indeed the description in the manuscript was not accurate enough. As shown by Pozzer et al. (2012) the comparison between observed and modelled NH_4^+ shows a good temporal distribution of ammonia emissions both for Europe and Asia (temporal correlations are above 0.7 and 0.5, respectively). On the other hand, the correlation on the East Coast of the USA is even below 0.2. As suggested by Pozzer et al. (2012), this is possibly due to the wrong seasonality, driven by an understimation of the livestock emissions, which have a maximum in summer and should account for 80% of the annual NH_3 emissions in the region (Battye et al., 2003). Differently, the agricultural emissions of ammonia in this region in the model reproduce mostly the fertilizer application as described by Goebes et al. (2003). We can therefore assume, based on the previous evaluation of the model, that this inaccurate seasonality does affect only the USA region. On the other side, despite the low seasonal correlation with the observations, it should still be mentioned that the overall results over the USA have good agreement with the observations, as the annual mean is well reproduced by the model (see Tab. 2 in the manuscript). We will extend the description in the model, mentioning this issue and that the seasonal results over the USA should be taken with caution.

3. **another issue is the spatial resolution of the modelled PM2.5 concentrations and its use for calculation of population exposure. While the dose-response functions are referred to, it is not clear how the 10x10km (or 0.1x0.1 degree) mortality map (Fig 7) is produced when the output of the model is 1.1x1.1 degree which would lead to underestimation of exposure in urban areas. A clear explanation and discussion of consequences for the results and conclusions would be important.**

We do agree with the referee that this point was not clear enough in the manuscript. The approached used in this manuscript is described in Lelieveld et al. (2015), where a detailed discussion on the uncertainties is included. Although it is expected to have an underestimation of exposure in urban areas, it is also true that model results with such resolution gives very similar outcome (for mortality) when compared to high resolution data as the one used by the Global Burden of Disease (Lim et al., 2013), indicating that the error introduced by the coarse resolution is only marginal. As discussed in the supplement of Lelieveld et al. (2015), $PM_{2.5}$ concentrations are not very sensitive to local emissions as the main $PM_{2.5}$ fraction is secondary in nature. This leads to small urban increments, as shown in many studies that have compared urban with sub-urban and rural $PM_{2.5}$ concentrations. This is particularly true for annual average concentrations, which are most relevant for long-term health impacts and mortality. Further, it has been shown by McKeen et al. (2007), in an intercomparison model exercise, that increasing the resolution of the model could degrade the correlation between model and observed fine particle and that the physic/chemistry package of the model is as important as the resolution. Additionally, a study by de Meij et al. (2007) showed that the mass fractions of SO_4^{2-}, NH_4^+, NO_3^- are not strongly resolution dependent. Finally, the comparison of the model with observations showed an agreement similar to regional model (see Table.1 and 2) and therefore we assume that the accuracy of our model is comparable to high resolution models. We acknowledge however the lack of discussion on this point in the manuscript and the text will be extended to include also a description of uncertainties sources. Following the work of Lelieveld et al. (2015), an uncertainties of 50% should be estimated for the mortality attributable to air pollution.

Minor comments:

ABSTRACT: I am not sure if the last sentence about the impact of 100% reduc-

tion is of any significance; such reductions are not even theoretically possible.
We do fully agree with the referee. However, this is a theoretical study, and it is scientifically interesting to check the effect on aerosol acidity for such large decrease of agricultural emissions. Although this reduction is not achievable, we believe to be an important exercise to understand the effect of ammonia on the chemical properties of the atmosphere.

Page 1, Line 23: One could add there a reference to the EU policy which includes now targets for NH_3 emissions within the revised air quality legislation. The authors include a reference to that later in the paper.
We will move the reference to this location.

Page 2, Line 4: not clear what is meant by 'manure processing' , suggest replacing with 'manure storage and on field application'
We will reformulate the sentence.

Page 2, line 4: suggest add 'N' or 'nitrogen' before "fertilizer"
The change will be implemented.

Page 2, line 12: maybe 'leads' should be replaced with 'would lead' or 'could lead' as this is a modelling study rather than impact observed anywhere.
We agree with the referee and the change will be implemented

Page 2, line 18: 'by agriculture' should be replaced with 'from agriculture' and 'resulted' can be possibly modified to 'would or could result'
The changes will be implemented.

Page 2, last paragraph: As before, suggest adding a reference to the recent European air quality policy and possibly underlying analysis.

A good review of air quality policy both for Europe and US is presented in Kuklinska et al. (2015). We will refer to this publication in the revised manuscript, as well mentioning the official web site reviewing the European air quality policy.

Page 3, from line 21: The emissions are for the year 2010 but the references are for data sets until 2005. Few words of explanation?

The references are indeed based on the year 2005, although they are for the same model set-up and emissions database. However, an additional evaluation is present in the manuscript, both with $PM_{2.5}$ (climatology) and with aerosol inorganic components (for the year 2010). This corroborates that the model performances have not been deteriorated with respect to the previous evaluation when simulating a different year.

Page 4, Figure 2: A bit small, hard to read the axis

The axis label will be increased.

Page 5, last paragraph: I believe it would be beneficial to put these assumptions in perspective of what has been discussed as feasible since the reductions given here, even the lowest level, are in most regions perceived as either infeasible or close to maximum reduction potential unless dietary changes are considered reducing meat demand. Beyond that, the realistic potential varies strongly between the regions which could be at least mentioned. It would be also advisable to add a clear statement which agricultural sources are included, eg., livestock manure, N mineral fertilizers, open burning of agricultural residues.

As pointed out also by referee #2, a more detailed discussion on how feasible such reductions are is needed in the manuscript. We will extend the text at page 7, lines 16-20. In fact, although the technology to reduce ammonia from livestock production (the largest source of European anthropogenic emissions of ammonia) by 80% does exist, it is unclear about the costs of such abatement methods.

Webb et al. (2006) suggested that "[...]While there is scope for reduction in NH_3 emissions at moderate cost, to achieve large ($> 25\%$) reductions costs are likely to increase exponentially."

Page 6; first paragraph: Presumably the first sentence refers to agricultural burning and so it could be moved to the end of this paragraph where combustion emissions are mentioned. In general this paragraph should be clear as to which sources are meant next to specific pollutants.
We will reformulate the text to make it clearer, moving the emissions of Black Carbon and Organic Carbon at the end of the paragraph.

Page 6; line 17: The 20-90% reduction refers to single measures and not to the overall mitigation potential and so nowhere 90% can be achieved for the whole agriculture. The potential is typically between 20-45% with some exceptions where structure is different, [..]
We fully agree with the referee and this chapter will be extended based on the previous comments (see also reply to referee #2).

Page 8, para from line 15: Total emissions in winter are not higher than in summer! NH_3 emissions are increasing with temperature and also organic fertilizers are applied in Spring, Summer, Autumn, just as the mineral fertilizers.
The referee is correct, and the text was simply wrong. The average emissions in the Northern Hemisphere show clearly a minimum during winter and a maximum in the spring season, with high emissions until late autumn. Here we were referring to the emissions in the USA, which do not present a correct seasonality. As showed by Pozzer et al. (2012), the temporal correlation between model results and observations over the USA for NH_4^+ is not fully satisfactory. The emissions of NH_3 in this region probably represent fertilizer application (Goebes et al., 2003), underestimating the importance of livestock emissions which have a maximum in summer and should account for 80% of the NH3 emissions in the region (Battye

et al., 2003). This point will be clarified in the text.

Figure 7: here the resolution for the mortality attributable to $PM_{2.5}$ is indicated as 10x10km. An explanation what data are used to develop that is needed. In general some discussion related to how coarse resolution concentration fields are used in health impact assessment would be useful [. . .]

The data at 1×1 degree resolution have been interpolated to the 0.1×0.1 degree resolution of the population map. As suggested by the referee earlier, this could cause smoothing of the $PM_{2.5}$ values, underestimating the exposure over urban areas. Although this is in principle correct, we have shown in Lelieveld et al. (2015) that this approach gives very similar results to other approaches using very high resolution datasets for $PM_{2.5}$. We therefore consider our results robust in the statistical sense, although an uncertainties of 50% must be considered for our results, in light of such assumptions.

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