We are grateful for all the constructive comments from the Reviewers. We have addressed all the comments and questions. In our response the comments have been marked in black and our responses have been marked in blue.

Page and line numbers refers to the track changes version of the manuscript.

M. Schaap (Referee 2)

1. Modelling the reactive nitrogen budget is a challenging endeavor. A good representation of the ammonia emission fluxes and their variability is key to any modelling effort. The current practice of parameterizing the ammonia emission variability in regional chemistry transport models is very basic and needs detailing. Any improvement to the knowledge of the spatial and temporal distribution of NH_3 is relevant as there are large uncertainties in the emission and deposition budgets. The paper by Werner et al. describes an effort to apply the emission module developed by Skjoth et al. (2011) in WRF-CHEM. I agree with first reviewer and his or her motivation that this paper does little to advance our knowledge of the subject. Hence, I feel this paper is not publishable in its current form.

We put a lot of our effort to improve the paper and meet all the comments raised by the Reviewers. Below we listed all the changes we made in the manuscript.

I have a main comment on the Base-case emission profile as this is unfortunately not the profile used within LOTOS-EUROS as stated in this paper. A separate comment was written on this issue.

The reply is given to the separate comment given by the Reviewer (please see below).

2. The paper covers an improvement to the WRF-Chem by introducing a dynamical emission model to the WRF-chem model. The model results are then compared to a number of stations and a number of statistics are given. Figure 8 shows scatter plots of the modelled and observed NH_3 concentrations at a number of sites. The scatterplots show overestimations by the model, whereas I source areas most model tend to underestimate. For most sites the Dynamic simulations show a decrease in the correlations and an increase in the RMSE for most seasons besides the winter. This contradicts the claim that an improvement is made, can you further clarify? The authors try to show that statistically the model improves, while small changes can be observed for the at Harwell. When we check Table 4. the combination of stations shows a reduction in the correlation for the autumn, at the same time a range of statistical parameters are given but not explained or commented upon in the manuscript. Overall the paper does not convince me that the DYNAMIC approach is an improvement to the WRF-chem model.

We agree with the comment. To meet also the further Reviewer's comment on not enough number of stations taken to the evaluation process we took all the available EMEP-EBAS and RIVM stations and recalculated statistics. The new description, based on increased number of stations, was prepared with the clarification of the results. Details are given in reply to comment 3.

3. The ammonia budget is affected by many other parameters than the ammonia emissions. The loss of ammonia to particulate ammonium sulfate and nitrate can be large, typically 5-10% an hour, with higher values in areas with low ammonia levels. It would be helpful to see the evaluation of the aerosol components. In addition, the wet deposition fluxes can be evaluated to assess if this term is looking OK. The paper itself feels rushed with only the boundary layer height being discussed as a

possibility for the modelled ammonia concentrations being out of phase with the measured observations.

We agree that the discussion on disagreement between modelled and measured ammonia concentrations was not clarified duly. Now, we have used all available daily observations of NH_4^+, NO_3^- and SO_4^{2-} for 2012 from the EMEP-EBAS database to evaluate the model results. This included 24, 17 and 36 stations, respectively for NH_4^+, NO_3^- and SO_4^{2-}. The observations were compared with modelled hourly concentrations of aerosols aggregated to mean daily values. All the stations used are presented in Fig. 1S (Supplementary materials). The statistics are presented in Tables 4, 5 and 6. The Taylor plot is given in Fig. 8. Separately, we have summarized the model performance separately for the agricultural stations (source regions for ammonia emission), as suggested by the Reviewer in the point 2 above. We have also qualitatively compared and discussed modelled ammonia concentrations against a similar figure obtained by satellite observations for the year 2013.

Please see: p.15 line 5 – p.17 line 4; p. 23 lines 8-34

In the case of wet deposition – we used the chemical option with full wet deposition processes and reactions in aqueous phase but the wet deposition fields are not saved into the standard output. However, we believe that the expanded evaluation for both NH_3 and aerosols and literature review have clarified the results.

4. The paper provides very little details on the implementation of the dynamic emission model. How was the allocation to agricultural subsectors done exactly? Were the emission totals per grid cell kept as a constant or allowed to be changed? The remark in the discussion that the emission distributions significantly changed suggest the latter. In other words, are the BASE and DYNAMIC emissions the same?

The emissions are distributed differently throughout the year, but the total emission per grid sell is kept equal to the original emissions.

The additional description has been added to the methods section (p. 7 lines 26-28; p. 8 lines 2-31).

A few other general comments/questions:

1. The dynamic model in itself is not new, only the introduction of the model to WRFChem is. As it is presented now it seems no modifications or improvements were made. It seems that the lessons learned from the former 3 papers (Skjoth 2004,2011 and Werner et al 2015) are not addressed or applied. Is that correct?

In our study the European dynamic ammonia emission model (Skjøth et al. 2011) was for the first time applied within a chemical transport model (CTM) for the entire Europe and evaluated for this region. The paper of Skjøth et al. (2011) provided the emission model code for Europe and included a test of the model over Northern Europe (Denmark, Germany, and part of surrounded countries) using the Danish Eulerian Hemispheric Model (DEHM). Two major aspects have previously prevented model calculations over all of Europe: 1) There was no suitable inventory outside the DEHM model domain. 2) The emission model did not provide sufficient accurate results in colder climates such as parts of Sweden or Russia or warmer climates such as Spain. Here, we wanted to: 1) evaluate the emission model for the entire Europe including the updates to the model code and the new underlying inventory; 2) check the capabilities of WRF-Chem for simulations of ammonia concentrations. Therefore this paper significantly extends the work presented earlier by Skjøth (2004, 2011) and Werner et al. (2015). 3) Explore maximum ammonia concentrations Europe-wide by comparison with satellite observations.

2. The claim that there is only hourly concentration data available for Harwell is not true. A fast check at the EMEP/EBAS website shows 190 datasets including time series of hourly data. Furthermore hourly data from the Dutch LML network is freely available. The UK has a large ammonia network which could be used for investigating seasonality. In short, a much more thorough evaluation is possible.

Thank you for the comment. The reason we used selected number of data from the EMEP/EBAS was that previously we focused only on stations in the agricultural area or in the close neighborhood of these regions, but we agree that we should have taken into account all available observations to make a complex evaluation of emission model and WRF-Chem. Thus now, we have used all the EMEP-EBAS stations available for the year 2012 with hourly, daily and monthly (here also 1 week, 2 week, 3 days were taken) ammonia concentrations. As suggested by the Reviewer, this was done to investigate seasonality. The statistics between the model and observations were calculated for 3 time resolutions: 1 hour, 1 day, and 1 month. In the case of daily values all daily stations were used and additionally all hourly aggregated into daily. The same procedure was used for monthly evaluation – here all stations were used (monthly, hourly and daily). Additionally all the Dutch stations, available through the RIVM web page were used. The stations are presented in Figure 1S (Supplementary materials). The simulations performance is shown in Tables 2 and 3. The new plots are provided: Fig. 2, Fig. 4, Fig. 8. The methodology (p. 9 line 9 – p. 10 line 22), results (p. 15 line 24 – p.16 line 16) and discussion (p.17 line 23 – p. 18 line 9) sections were modified and expanded.

3. A figure illustrating the difference in emissions between both models would be helpful.

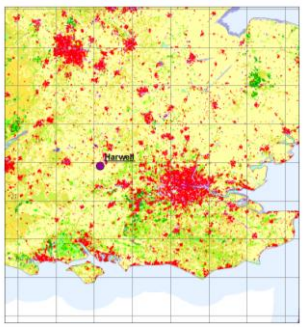
A barplot showing the monthly emissions for the BASE and DYNAMIC approach has been added. Please see Fig. 1.

4. The model has an increased number of layers with a thickness of only 20 meters, at the same it has a horizontal grid of 36x36km. As a reason behind this move the authors mention the importance of chemistry and the vertical distribution of ammonia in the boundary layer. Why was the vertical resolution increased and was a higher horizontal resolution not considered? I appreciate the large computational effort made in this study, but I feel that the use of a slightly simpler model targeting at least several years would have been a better choice to evaluate the dynamic emission model.

The main aim of the paper was twofold – first, we wanted to apply the dynamic emission model to the on-line integrated chemical transport model (CTM) for the entire Europe, which has not been done before. Second, we wanted to evaluate the WRF-Chem model capabilities for modelling of ammonia concentrations. We agree that it would be useful to run WRF-Chem at a higher spatial resolution, however we would suggest it for further application in nested domain over certain regions/countries. We hope that the results from our study will be a step forward for running the dynamic emission model and CTM at a high spatial resolution. As mentioned by the Reviewer, the computational effort behind running the WRF-Chem model for the entire Europe is significant, especially if the model is run several times for the entire year. We have shown some drawbacks behind the application of this model fed by the dynamic emission of ammonia. This has not been done before. It is likely, that we could obtain better model-measurements agreement if the model is applied for the same area with higher spatial resolution and with better separation of the source regions for ammonia, and this is what we would like to try in the future, with larger computational resources.

5. What are the major sources near the Harwell site? Is Harwell representative for the gridcel it is in? If the site is located in the left corner of an WRF-CHEM cell this would mean it is in the same cell as part of London, and thus all industry near it.

The major sources near Harwell is agricultural. In fact the area is dominated by traditional agricultural production such as meat production (pig and cattle), milk production and crops. The attached figure shows the location of London, the grid cell definition in WRF-Chem and the location of the Harwell site.



For further analysis, for all ammonia stations we have specified if the station is in the agricultural area or not. We used the following methodology: for each station we calculated a buffer with a diameter of 2 km and then check the dominant land use according to Corine Land Cover 2006 (CLC). If the prevailing land use was agricultural then the station was classified as "agricultural". When, another type of land use was dominant the station was treated as "non-agricultural". Using this approach, the Harwell station was classified as agricultural. This is described in methodology section (p. 9 lines 21-26) and also used for calculation of statistics (Table 2 and table 3) and for plots (Fig. 2 and Fig. 4)

5. Most of the figures could use some titles indicating the seasons/data. Figures should be understandable without the text.

The figures were improved to be understandable without the text.

Referee 2a

Reading the paper by Werner et al. I notice that the reference or BASE simulation setting refers to the emission profile used in LOTOS-EUROS. I am afraid this is not the case in reality. There appears to be confusion about the temporal profiles used in LOTOS-EUROS for ammonia emissions from agriculture which is probably based on a copy paste error in an old report referred to in this paper. The report provides a constant diurnal cycle in agricultural emissions but we this is not according to our specifications in the model. The error in the 2005 report is not present in any of the more recent reference guides on LOTOS-EUROS (available on the website). I regret to see this unfortunate situation.
There are two sets of profiles used at TNO. The first is a set of functions delivered along with e.g. AQMEII and EURODELTA projects. This set refers back to data used in the early stages of EURODELTA and CITYDELTA and derives largely from the GENESIS –project. These are the profiles as for instance used in AQMEII and shown in van Damme et al. (2014). This version includes a diurnal cycle of the emissions ranging between 0.6 at night and 1.7 during the day. Spring time emissions starts to be increased in February. As the Danish team has participated in EURODELTA and AQMEII I would have expected that these profiles would have been used as a starting point.
In LOTOS-EUROS we normally use a seasonal variability as described in Schaap et al. (2004), which was assumed before European model intercomparisons such as EURODELTA and CITYDELTA even started. This function has most emissions in March and April and a lower contrast during the seasons. It uses the same diurnal as above. It has been reported in several other publications (e.g. Banzhaf et al., 2013). This is the profile we define as STATIC in our efforts to improve the ammonia emission variability (Hendriks et al., 2015).
In any case, we never use emission profiles without a significant diurnal cycle as seems to be the case in the BASE simulation by Werner et al.. In a sensitivity study to the diurnal cycle by Schaap et al. (2003, p 106) the largest impact is observed for winter, with much higher concentrations when neglecting the diurnal cycle.

We would like to ask the authors to contact us directly next time they use our settings as a reference. We would be happy to contribute and make sure the correct and latest information is used.

Schaap, M.: On the importance of aerosol nitrate in Europe, PhD thesis, University of Utrecht,

Utrecht, The Netherlands, 2003, http://dspace.library.uu.nl/handle/1874/708

Schaap, M., van Loon, M., ten Brink, H.M., Dentener, F.J., Builtjes, P.J.H., 2004. Secondary inorganic aerosol simulations for europe with special attention to nitrate. Atmospheric Chemistry and Physics 4, 857-874.

Van Damme, M., R. J. Wichink Kruit, M. Schaap, L. Clarisse, C. Clerbaux, P.-F. Coheur, E. Dammers, A. J. Dolman, and J. W. Erisman (2014), Evaluating 4 years of atmospheric ammonia (NH3) over Europe using IASI satellite observations and LOTOS-EUROS model results, J. Geophys. Res. Atmos., 119, doi:10.1002/2014JD021911.

Hendriks, C., Kranenburg, R., Kuenen, J.J.P., van den Bril, B., Verguts, V., Schaap, M., 2015. Modelling ammonia distributions across north western Europe using emission time profiles based on manure transport data, submitted to atmospheric environment

Banzhaf, S., Schaap, M., Wichink Kruit, R.J., Denier Van Der Gon, H.A.C., Stern, R., Builtjes, P.J.H., 2013. Impact of emission changes on secondary inorganic aerosol episodes across Germany. Atmospheric Chemistry and Physics 13, 11675-11693.

We are sorry we did use the old citation and we did not contact the authors to clarify the issue. Thank you for the clarification of the emission profile used in the LOTOS –EUROS. The incorrect statements were removed from the manuscript.

Anonymous Referee #1

This paper deals with an important topic, the spatial, temporal and vertical distribution of NH3 in Europe, and the impacts of dynamic approaches. Although I agree with the comment by Dr. A. Dore that this subject is important, I think this paper does little to advance our knowledge of the subject. The authors claim that the aim is to improve the basic understanding of ammonia in the atmosphere, but essentially all they show is that WRF-Chem performs poorly for hourly NH3 when used with static emissions, and still performs poorly when used with dynamic emissions. No real attempt to solve the problem with this model is presented, and no attempt is made to show if the problem is general for other models and locations.

We put a lot of our effort to improve the paper and meet all the comments raised by the Reviewer. We have applied the model for the entire Europe and now we have compared the results with significantly larger number of stations and we discuss the results with more studies focused on ammonia modelling for this region. All the changes are provided below as well as in the reply to Reviewer 2.

Detailed Comments

1. The use of dynamic emissions compared to static emissions has already been shown by Skjoth et al (2004, 2011) and Werner, et al. 2015. The main new thing in this paper is that the dynamic emissions still result in poor reproduction of the diurnal cycle. This would have been worth exploring, but essentially no

exploration is done. The authors did no sensitivity tests of their own, and seem unaware of the much more detailed work done on this subject by other workers.

We agree with the comment that the dynamic emission model has been used before. However, in our study the European dynamic ammonia emission model (Skjøth et al. 2011) was for the first time applied within a chemical transport model (CTM) for the entire Europe and evaluated for this region. The paper of Skjøth et al. (2011) provided the emission model code for Europe and included a test of the model over Northern Europe (Denmark, Germany, and part of surrounded countries) using the Danish Eulerian Hemispheric Model (DEHM). Two major aspects have previously prevented model calculations over all of Europe: 1) There was no suitable inventory outside the DEHM model domain. 2) The emission model did not provide sufficient accurate results in colder climates such as parts of Sweden or Russia or warmer climates such as Spain. Here, we wanted to: 1) evaluate the emission model for the entire Europe including the updates to the model code and the new underlying inventory; 2) check the capabilities of WRF-Chem for simulations of ammonia concentrations. Therefore this paper significantly extends the work presented earlier by Skjøth (2004, 2011) and Werner et al. (2015). 3) Explore maximum ammonia concentrations Europe-wide by comparison with satellite observations.

We also agree, that we should have made more effort to study the results of other works. According to the Reviewer's suggestions, we studied relevant papers as well as made more effort to analyze out results in details (e.g. we used all available European stations of NH_3 concentrations and used aerosol measurements). The details are given in the reply to comment 3 and 4.

2. Hourly measurements are presented for just one station (Harwell, UK), and only NH_3 concentrations are presented. As discussed below, hourly data are available for other stations in Europe, and for at least some of the other key compounds which one would normally look at when trying to further understanding of NH_3 in the atmosphere (e.g. HNO_3, sulfate).

We agree with the comment and have largely expanded the evaluation of the results. We used all available for 2012 EMEP-EBAS stations with hourly, daily and monthly (here also 1 week, 2 week, 3 days were taken) ammonia concentrations. The statistics between the model and observations were calculated for 3 time resolutions: 1 hour, 1 day, and 1 month. In the case of daily values all daily stations were used and additionally all hourly aggregated into daily. The same procedure was used for monthly evaluation – here all stations were used (monthly, hourly and daily). Additionally all the Dutch stations, available by the RIVM web page were taken. For further analysis, for all ammonia stations we have specified if the station is in the agricultural area or not. We used the following methodology: for each station we calculated a buffer with a diameter of 2 km and then check the dominant land use according to Corine Land Cover 2006 (CLC). If the prevailing land use was agricultural then the station was classified as "agricultural". When, another type of land use was dominant the station was treated as "non-agricultural". We also used all available daily EMEP-EBAS concentrations of NH_4^+, NO_3^- and SO_4^{2-} to evaluate the results.

All the stations used are presented in Figure 1S (Supplementary material). The model performance is given in Tables 2 and 3 for ammonia concentrations, in Tables 4, 5, 6 for aerosols concentrations. New plots are presented in Fig. 2, 4, and 8. The methodology (p. 9 line 9 – p. 10 line 22), results (p. 15 line 24 – p.16 line 16) and discussion (p.17 line 23 – p. 18 line 9) sections were modified and expanded.

3. Indeed, the authors seem unaware of many of the studies done in Europe to highlight problems in the understanding of NH3 diurnal cycles, or that other models do not show such poor performance for hourly data. Work with the LOTOS-EUROS

model in particular has extensively looked into model comparisons against hourly data in Europe, and these studies did a much better job of analyzing the reasons for any discrepancies and of testing alternative model. Some examples:

• Aan de Brugh, JMJ et al. Modelling the partitioning of ammonium nitrate in the convective boundary layer Atmos. Chem. Physics, 2012, 12, 3005-3023
- investigate hourly data and partitioning of NH3-NH+4 at Cabauw, including a number of model tests with ISORROPIA to explain the observed diurnal variations. This paper has a much more thorough analysis of both the diurnal cycle and vertical profiles (and sensitivity analysis) than the submitted manuscript.

Schaap, M. et al., Illustrating the benefit of using hourly monitoring data on secondary inorganic aerosol and its precursors for model evaluation. Atmos. Chem. Phys., 11, 11041-11053, doi:10.5194/acp-11-11041-2011, 2011
- one of the first papers to show comparisons of modeled versus observed diurnal cycles of the key components, and with an extensive discussion of equilibrium issues. The model used showed better results that those of WRF-Chem in the submitted manuscript.
• Wichink Kruit, R. et al., Improving the understanding of the secondary inorganic aerosol distribution over the Netherlands, TNO report TNO-060-UT-2012-00334, 2012 (available online),
- again, more examples of more successful evaluation and testing of models against hourly data at Cabauw.
The Cabauw site has seen a large number of measurements over the years, including vertical profiles, that would be very relevant to this investigation. For example,
• Kulmala, M. et al., General overview: European Integrated project on Aerosol Cloud Climate and Air Quality interactions (EUCAARI) – integrating aerosol research from nano to global scales, Atmos. Chem. Phys., 11, 13061-13143, 2011,
- shows vertical profiles of sulfate, nitrate and ammonium at Cabauw
• Aas, W et al., Lessons learnt from the first EMEP intensive measurement periods, Atmos. Chem. Physics, 2012, 2, 8073.8094,
- show hourly NH3, HNO3, NH+4 and NO−3 for the EMEP sites Harwell, Ispra, and Cabauw, and data for Auchencorth Moss are said to be available.
This paper also showed that different sites had rather different diurnal cycles (e.g. some had peak NH3 in daytime, others at night), which implies that a model cannot be evaluated by comparison with one site alone. This Aas paper spends more time discussing the reasons and caveats of diurnal cycles than the submitted manuscript.
In fact, both the LOTOS-EUROS and EMEP models seems to capture the diurnal pattern of NH3 quite well in many cases, although sometimes with significant bias. Why do these models perform better than WRF-Chem with either static or dynamic NH3 emissions? If other pollutants and sites had been considered we might have learned how well or badly WRF-Chem performs in general before trying to draw conclusions for just one pollutant.

Thank you for the comment and giving suggestions for other publications. According to the Reviewers' comments we have expanded the evaluations (details in reply to comment 2) and discussed results of other works (please see discussion section, p. 18 line 18 – p. 19 line 14; p. 23

lines 8 – 34; p. 24 line 31 – p. 25 line 13). We have included the papers of: Aan and Brugh (2012), Schaap et al. (2011), Kulmala et al. (2011), Aas et al. (2012), Damme et al. (2015).

4. The paper actually claims that hourly NH_3 measurements are only available at one site in Europe, Harwell. This is clearly not true, as the above studies testify.
We agree. We have extensively extended the evaluation and used all available hourly, daily and monthly stations from EMEP-EBAS as well as from RIVM. The details are given above (point 2).

5. The paper stresses some points which are obvious and well known from even decades old studies, in particular that the concentrations of a pollutant released at ground level are inversely correlated with PBL height. This is basic air-pollution meteorology.
We agree with the comment. We have removed the plot showing the dependence between NH_3 concentrations and PBL height. However, we have decided to keep the part of the Discussion section on PBLH and the WRF-Chem model.

6. The authors try to make the point that dissociation of NH4NO3 is not a strong source of NH3. This would have been interesting to quantify, but instead the authors simply cite that fact that there is a phase-shift between the NH3 and NH+4 concentrations
We have focused on the extensive evaluation of the modelled ammonia and aerosol concentrations with observations and spatial comparison of the modelled ammonia concentrations with satellite product (please see reply to the comment 2 and 3). The aerosol measurements we used here were available only at daily temporal resolution. We have not expanded our study towards quantifying of the dissociation process.

7. The paper is also careless in many places, for example the lack of proper labeling on Figures and use of citations that aren't appropriate, e.g. the Sutton et al. paper given as a reference for ECLAIRE doesn't mention ECLAIRE.
We have improved the paper in this context and corrected the figures carefully. The inappropriate citation has been removed.

8. The authors claim that they have analyzed the vertical distribution of NHx, but they haven't. They have simply illustrated this, without comparison to measurements or even earlier studies that have done this before in a more thorough way.
We agree with the comment, it was too strong saying that we analyzed the vertical distribution. We have removed this statement. We did not have the measurements of the vertical distribution. We have compared our results with the paper of Kulmala et al. (2011), (p. 19, lines 6-14).

9. The authors concentrate on WRF's bias with respect to temperature, but what about wind-speed, or even friction velocity? The paper does cite other studies (e.g. Jimenez and Dudhia, 2013) but since these were done by other groups in different areas and likely with other WRF settings, those studies are not necessarily relevant for the European area or Harwell.
The WRF model has been extensively evaluated for wind speed for the area of Europe, showing small bias (usually close to 0.5 m s^{-1}, e.g. Kioutsioukis et al. 2013, Santos-Alamillos et al. 2015, Kryza et al. 2015, Vieno et al. 2010) and good agreement with the measurements. The model tends to overestimate the observed wind speed. Because of good agreement of the WRF modelled wind speed we paid less attention for this meteorological parameter, if compared to air temperature.
We have specified this in the WRF-Chem model section (p. 6 line 26 – p.7 line 2)

Kioutsioukis I., de Meij A., Jakobs H., Katragkou E., Vinuesa J.-F., Kazantzidis A., 2016, High resolution WRF ensamble forecasting for irrigation" multi-variable evaluation, Atmospheric Reseach 167, 156-174.
Santos-Alamillos F.J., Pozo-Vazquez D., Ruiz-Arias J.A., Tovar_pescador J., 2015, Influence of land-use misrepresentation on the accuracy of WRF wind estimates: Evaluation of GLCC and CORINE land-use maps in southern Spain, Atmospheric Research 157, 17-28
Vieno, M., Dore, A. J., Stevenson, D. S., Doherty, R., Heal, M. R., Reis, S., Hallsworth, S., Tarrason, L., Wind, P., Fowler, D., Simpson, D. and Sutton, M. A.: Modelling surface ozone during the 2003 heat-wave in the UK, Atmos. Chem. Phys., 10(16), 7963–7978, doi:10.5194/acp-10-7963-2010, 2010.
Kryza, M., Wałaszek, K., Ojrzyńska, H., Szymanowski, M., Werner, M. and Dore, A. J.: High resolution dynamical downscaling of ERA-Interim using the WRF regional climate model (Part 1) – model configuration and statistical evaluation for the 1981-2010 period, Pure Appl. Geophys., In review, 2015.

10. The authors compare a BASE case with a DYNAMIC case, but nowhere do we see the annual time-series compared with each other.
The annual time series have been added. Please see Fig. 4 (time series based on hourly values) and Fig. 2 (time series based on monthly values).

11. Page 22937, Line 15. Are not hourly measurement of ammonia rare because of their expense and complexity?
We agree with the comment. The sentence was modified (p. 3 lines 10-14).

12. Page 22938, Line 21. It is claimed that the work of Werner et al., 2015 shows significant improvements when dynamic approaches are used, which is a slight exaggeration. Werner only examined few locations, and found worse results for some statistics in some seasons.
We agree. We have removed this sentence.
Simultaneously, we have emphasized that in this paper we present for the first time the dynamic emission (Skjøth et al. 2011) implemented into a chemical transport model and verified for the entire Europe (please see reply to comment 1).

13. Page 22939, Line 5. What is FP7?
FP7 is the Seventh Framework Programme for Research and Technological Development. We have modified the sentence as given below:
Addressing this knowledge gap is one of the objectives in the ECLAIRE (Effects of Climate Change on Air Pollution and Response Strategies for European Ecosystems), which is a project founded by the Seventh Framework Programme for Research and Technological Development.

14. Page 22940, last paragraph. This is very hand-waving. The model performs 'well', there were 'biases', the 'biases are significant', and I have no idea what any of that means. Quantify.
The paragraph has been modified to be more precise in terms of quantification of the model performance:

The WRF-Chem model has been extensively used and evaluated for both meteorological and air quality studies in Europe. The model performance for meteorology affects both the air quality results and the calculated emissions. Several studies, focused on the entire Europe, report biases for both air temperature and precipitation, e.g. Miglietta et al. (2012), Katragkou et al. (2015), Wałaszek et al. (2014), Kim et al. (2013), Warrach-Sagi et al. (2013). Recent findings provided by Skjøth et al. (2015)

show that the bias in air temperature at 2 m varies spatially and seasonally. These biases are significant (up to +2.0 K in eastern Europe during summer and autumn and -2.0 K in southern Europe in winter) and might affect e.g. online calculated emissions and the processes in vegetation models. Similar findings are reported by Kryza et al. (2015) for the area of Poland, where the air temperature bias is low in winter (mean bias -0.6 K), but summer temperatures are significantly overestimated (up to +1.0 K). A bias in WRF calculated air temperatures were also reported by Mooney et al. (2013) and Miglietta et al. (2012). The model performs well at simulating wind speed for Europe, with mean bias not exceeding 0.5 m s^{-1} (Jiménez and Dudhia, 2013; Miglietta et al., 2012; Santos-Alamillos et al., 2013; Vieno et al., 2010) which is the second variable affecting ammonia emission in this study.

15. Page 22941, Line 6. Are the Schaap 2005 profiles commonly used? Which models use them?

The second Reviewer explained us this issue. The sentence was removed from the paper.