The manuscript "*A dynamic ammonia emission model and the online coupling with WRF-Chem (WRF-SoilN-Chem v1.0): development and evaluation*" by Ren et al. describes an ammonia emission model coupled with WRF-Chem that incorporates impacts of meteorological and soil conditions on emission factors (EFs), which aims to provide better estimates of NH3 emissions on both spatial and temporal scales. The modelling results (including fluxes and concentrations) were compared to multiple observations and measurements in China, with evident improvements that can be seen. This study addresses the lack of consideration of environmental impacts on NH3 emissions in current emission inventories. The main assets of this study include: 1) Very simple correction factors were used to represent the climatic dependences of NH3 emissions but covered important processes. 2) The emission model was performed on high spatial and temporal resolution and was coupled to an online climate-chemistry model, which is an advantage for predicting NH3, a short-lived species whose sources can be greatly influenced by meteorological factors. 3) Improvements in estimating NH3 emissions and simulating atmospheric chemistry were made by implementing this emission model. 4) The operation of coupling the emission model with the parent WRF-Chem model looks user-friendly according to the description in the manuscript. Overall, the manuscript is clearly structured and well written. The manuscript should be published in GMD after the authors address the following questions and comments.

Major comments

1. As mentioned, the emission model is simple but it accounts for important factors that influence NH_3 emissions. Therefore, it is crucial to justify why to use the parameterizations shown in the manuscript. The model focuses on China geographically, and most of the corrections for EFs are empirical equations. To what extent these parameterizations can be used globally or in other regions to give reasonable estimates remains unclear. Is it only applicable to China? How well is the model performance in other regions/countries if applying the correction factors in the same way? A useful method would be performing sensitivity tests of some selective parameters in the equations to justify which are the most important ones impacting emissions and what is the relative importance of each parameter. The method used in this study is modifying existing EFs from emission inventories by including a set of environmental dependencies. Theoretically, this can be done for regions like Europe and US, which could be a future direction.

2. The description of running the emission model and the WRF-Chem model in the manuscript is unclear. Did you average the monthly (or annually) basic emission data to obtain the emission with the temporal resolution that is required in the WRF-Chem model? In addition, how did you run the coupled model for China? I assume that you ran nested simulations (I could be wrong). If so, what were the boundary conditions and what emissions did you use for the global run? It is useful to add a section or a paragraph to clarify.
3. Following the above points, a potential weakness I am concerned about is the overall integrity of the emission model. The emission is not calculated from the sources such as the amount of nitrogen in the fertilizers or manure but is derived from the given EFs. However, the EFs are related to the sources such as how much fertilizer nitrogen is applied on land. If you calculated the online EFs at each time step (incorporating the correction factors into the basic emission), the hidden philosophy of the model is that there is a source at every time step which is problematic. Meanwhile, the model might overestimate the emission because the nitrogen reservoirs are depleting. If the basic EFs were assumed to be the same throughout a period (e.g., a month), this does not reflect the tendency of a decreasing emission as there is less and less nitrogen to be emitted. This should be discussed in the manuscript either as an uncertainty evaluation or how the model deals with such a problem.
4. There are some inconsistencies and ambiguities in the overall design of the experimental simulations. It is important to clarify in the manuscript why the year 2019 was chosen to run simulations (rather than a year between 2010 – 2015) and for each comparison what meteorology was used. In the evaluation section, both the annual NH_3 concentrations over eastern China by NNDMN (Fig.4) and the site measurements (Fig.8b and Fig.9b) used for model comparisons were not from the year 2019. This raises the question of whether the comparisons are representative enough when using different meteorology for the simulations given that NH_3 is strongly influenced by climatic conditions.

   It is stated in Section 3.1 that "…experiments were run for the entire year of 2019 …", but it is not clear which year's meteorology was used for the comparisons with satellite (Section 3.3; Fig.5) and field measurements of NH_3 fluxes (Section 3.5; Fig.7). Also in Section 3.5, for comparing to the field measurements of NH_3 flux, did you use the corresponding meteorological in 2012?

5. In Section 3.2, it is unclear whether the description/discussion is for basic NH_3 emissions or online NH_3 emissions. It is useful to include a map for the online NH_3 emissions in Figure 2. Besides, it can be helpful to see the difference between the two.
6. In Section 3.4, since the model results can be extracted at site locals, such as Nanjing and Beijing, can you present more comparisons in the same way for other sites (as there are over 10 more monitoring sites shown in the boxes in Figure 2)? This can be put into Supplementary materials. Or it is worth adding a paragraph or a few sentences to clarify why only these two sites were selected for comparisons, i.e., availability, data quality etc.
7. In Section 3.5, I was wondering how you ran the model to get the emissions. First, as mentioned, which year's meteorology was used? Second, did you run the model at the site scale or did you just extract results from the regional simulations for China? How you calculated the basic EFs from the nitrogen application rates given by the field study, i.e, EF_{0i}, $CF_{pH,}$ CF_{method}, and CF_{rate} in Equation 2 are not well described. Details can be put in Supplementary Materials. Third, the field measurement of NH_3 emissions shows relatively comparable magnitudes in terms of daily emissions, i.e., the general daily trend of NH_3 emissions is quite "flat" rather than gradually decreasing over 15 days. The field emissions after fertilization usually reach the maximum within a few days and then decline due to less nitrogen being available for emitting. It is unknown if the model is capable of capturing such features. In other words, the impressive agreement between the modelled emissions and measurements becomes a bit less convincing. It might be due to averaging the monthly emission factor giving the same hourly emission factors, which results in comparable daily sums. Nevertheless, the model reproduces diurnal variations in emissions very well and captures the decreasing feature of NH_3 during a rainy day.
8. There is a lack of discussion on uncertainty from the new emission model. This can be either some text descriptions discussing various uncertainties, or any back-of-envelope calculations derived from the potential sensitivity tests.
9. Although WRF-Chem can be run globally, the study focuses on China. I would suggest revising the title of the manuscript to be more specific on its spatial coverage. For example: "*A dynamic ammonia emission model and the online coupling with WRF-Chem (WRF-SoilN-Chem v1.0): development and regional evaluation in China*" or "*A dynamic ammonia emission model for China and the online coupling with WRF-Chem (WRF-SoilN-Chem v1.0): development and evaluation*".

10. I would personally suggest putting Section 3.5 in front of Section 3.3. Section 3.5 directly compared modelled NH_3 emissions to measured emissions, while Sections 3.3 and 3.4 are comparisons between modelled and observed/measured atmospheric NH_3 concentrations. By doing that, the evaluation is in the order of "emission, NH_3 concentrations, and aerosol concentrations". This point is optional.

Specific comments

P6L138: This is not entirely correct because soil pH can increase after urea application. Urea hydrolysis consumes H^+ ions so it tends to lead to more NH_3 emissions. The extent of pH increase is dependent on the soil's pH buffering capacity. Although this is complex, it should be specifically clarified that the model assumes constant soil pH. Please modify "In the fertilizer application section, soil pH … are relatively stable in a short time."

P6L164-165: Same comments as mentioned above and in the 3rd points from Major comments. Ammoniacal N concentrations can vary greatly throughout the application period. Ammoniacal N is lost from the soils through various pathways such as volatilization, nitrification, and physical transport like runoff and diffusions, which can affect the concentration of ammoniacal N. Ammoniacal N also exists in different phases, e.g., can be adsorbed on soil particles, which depends upon soil moisture and other factors. Since the model uses simple correction factors and does not include detailed soil processes, it should clearly state the assumptions to avoid misleading. Please consider rephrasing into "…ammoniacal N concentration and soil pH **<u>are assumed to be</u>** stable in short time …".

P7L177: Is there any statistical or activity data for China supporting that "the handle of excrement is usually settled in closed containers"?

P7L192: Please explain why soil moisture correction is applied for modelling emissions from animal houses. There are houses with concrete floors or slatted floors. Or is this equation only applied to specific management?

P8L211-216: It is useful to include more details for the experimental study carried out by your research group which is used for deriving soil temperature correction factor, i.e., what method was used? How was the study designed? Any reference?

P8L225-229: It is worth mentioning in the manuscript that emissions can be different from soils with the same water content but different porosity (soil water content at saturation), which is not considered in the model.

P9L230-235: I was wondering what is the underlying mechanism that rainfall affects NH_3 emissions in the model. Is it because of infiltration, runoff, or changes in soil moisture? Is there a double-counting issue here between the rainfall and soil moisture correction?

P11L268: Again, since urea is the most widely used fertilizer in China, a discussion of the uncertainty caused by not including soil pH change is missing.

P13L293-294: Tibet has very little NH_3 emissions as shown in Figure 2, while there are some hot spots in Xinjiang province. Meanwhile, sheep are not a significant contributor to NH_3 emissions especially when grazing is dominant. What does the model tell? You should be able to diagnose the sectoral emissions from the model.

P14L337: Then why not simulate 2010-2015?

P19L431-432: As stated in the manuscript, there are no diurnal variations in the inventory. However, some diurnal variation can be seen as shown in Figure 7b from the base run (e.g., 10:00 to 18:00 has higher emission than other times), which is confusing.

Other comments

P2L63: "environment elements" to "environmental elements".

P5L124: "agriculture soil" to "agricultural soil".

P9L235: Numbering of the equation is missing.

P18: Labelling of Figure 7 is missing.

P18: Consider using grams or kilograms rather than mol.