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Interactive comment on "Tropospheric vertical column densities of NO2 over managed dryland ecosystems (Xinjiang, China): MAX-DOAS measurements vs. 3-D dispersion model simulations based on laboratory derived NO emission from soil samples" *by* B. Mamtimin et al.

Anonymous Referee #2

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This is a very good and interesting paper for those researchers interested in soil NOx emissions, how to quantify these better, and learn about useful approaches to scale up localized NOx emissions from land-use specific plots to a more heterogeneous, mixed-cover regional average. The paper is well written although makes for a quite technical read, and at times some more general lessons on what can be learned from the authors' findings are lacking. Nevertheless, the authors have clearly succeeded in testing model simulations of field conditions with relevant observations by up-scaling lab-based soil NO fluxes to a field-size area. I think the paper should be accepted after the comments and suggestions below, and those from the other reviewer have been addressed.

Major issues:

P19372, L5: it is not clear why the LASAT model is 'state-of-art'. From the description, it appears that chemistry is missing from the model, so that temporal evolution of the chemically active NO-species is difficult to track. Furthermore, it is unclear how pixel cross-talk, or advection, in the model (highly relevant with model resolution of 30 m) is described. The authors should improve the description of these issues.

Calculation of concentration of NO2 from the photochemical equilibrium between NO and O3 is in principle feasible. However, O3 concentration increase with altitude, and J(NO2) also has a vertical profile. It is unclear how these vertical distributions are taken into account? If they are neglected, which seems to be the case, the authors should estimate the error associated with these assumptions.

More attention should be paid at the local time at which measurements and model simulations have taken place. For instance, in Figure 4, it is unclear what the local time was for the MAX-DOAS measurements shown. This is important, in view of the diurnal cycle in soil NOx emissions (presumably higher at mid-day in response to higher soil temperatures) and the diurnal cycle in NO2 concentrations with a midday minimum in NO2 reflecting higher mid-day OH-levels (e.g. Fig. 7). Also, the paper would improve greatly if the authors could indicate whether their evaluation of the LASAT simulations with the MAX-DOAS observations is consistent with the parametrization of the diurnal cycle in soil NOx emissions that follows Eq. (10). The community would benefit from an evaluation of Eq. 10 with the laboratory net-derived potential net NO-fluxes. To my knowledge, such an evaluation with samples from the field has not yet been done.

In section 3.5, the authors state that 'there is remarkable good agreement' between measured and simulated data. Inspection of Figure 7 however shows that there is a discrepancy of 25-30% between the LASAT model and most reliable MAX-DOAS measurement (at 15 deg elevation), with LASAT being too high. While I agree that the authors have done an impressive job in describing the spatial and temporal detail of soil NOx emissions from the area, I think it is a bridge too far to claim that the agreement between simulated and measured NO2 is remarkable. I think the discrepancy needs more attention. It could be caused by the lack of chemistry in the LASAT simulation (NO2 too long-lived). It is also intriguing that the 2 and 4 deg elevation cases (for which the geometrical AMF will lead to errors) show better agreement than the 15 deg elevation case (for which the simple geometrical AMF works fine). These aspects should be discussed in more detail than just claiming 'remarkable agreement'.

Specific comments:

P19362, L25-26: I'm not sure if the assumption that free tropospheric NO2 advection is negligible holds. In the study-area, considerable contributions from lightning and soil (from other areas) resulting in summertime NO2 maxima have been reported (e.g. van der A et al., 2008; Miyazaki et al., ACP, 2012 – Figure 14).

P19363, L8: here 'NE' is mentioned, but in L11 'NW' is mentioned. Should it be NW everywhere? Please clarify if NW means 'North West'.

P19363, L19: please have the list of references preceded by e.g.

P19365, L5-6: strongly suggest to provide references that confirm that scattering may be neglected at elevation angles > 15 degrees.

P19365, L8: I don't think the abbreviation or meaning of LASAT has been introduced at this stage. Suggest to do so.

P19367, L4: the section title should read 'NO fluxes', not 'NO2 fluxes'.

P19368, L7: please clarify why the soil T variation between 20 and 30 degrees is 'desired'. Do ambient temperatures in July never drop below 20 C?

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P19368, L17: after ‘As shown during the last two decades’, a few citations would be appropriate.

P19371, L2: ‘methods’ should be ‘method’

P19371, L5: the closing bracket after plant cover is redundant.

P19372, L23: in terms of stability classes.

P19377, L9: 76% of total, 24-hour soil biogenic NO emissions? Please clarify.

P19378, L14-16: the resolution of Figure 6 is a bit low. I think the Figure is so nice that it would merit an improvement in resolution so the spatial detail can be better distinguished.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 19357, 2014.

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Interactive Comment