Review of the paper „Measuring dry deposition of …” by Daan Swart et al.

First I would openly state that I have a long lasting collaboration with some of the authors and I closely followed and discussed their progress. I cannot exclude that I am biased in this sense.

This paper presents an NH3 flux intercomparison with two novel open-path instruments. The major advantage of open-path instruments is the absence of an inlet line that is notoriously influencing high frequency concentration measurements needed to determine fluxes. The measurements took place at the Cabauw station located in the middle of the Netherland. It is an open landscape mostly used as intensive grassland. From a micrometeorological perspective it is good location as most of the time good turbulent conditions are present.

I am very pleased to see the great progress that was achieved regarding the accuracy and precision of the DOAS system. The systems have no reached a level that meaningful vertical gradient measurements are possible. Also the open path system from Healthy photons is precise and fast enough to perform reliable EC measurements.

Looking from a greater distance the overall results summarized with figure 11 are looking very nice. Both systems show similar NH3 fluxes and more important they show the same diurnal structure with identical changeover from deposition to emission. So it is very likely that both instruments do measure an existing mean flux over the surface determining the flux.

I could stop here and say publish as is with some minor corrections already addressed by the first reviewer. But this is not my understanding of a serious review and I will dig in the following a little bit deeper. But keep in mind that this is complaining at a high level.

Specific issues:

I think that the title is not appropriate. The work focus on an intercomparison of two approach to determine bidirectional NH3 exchange. The title should reflect this.

It would be important to describe shortly the “flux landscape” of the chosen site. It is located in a an area with intensive animal production associated with a high NH3 turnover. Generally small areas with high emission densities (stable, storage, manure fields) are surrounded with intensively managed field that are expected to show all the time changes from emissions to deposition mainly controlled by their actual N-status (see e.g. the year around measurements that we did at the Oensingen grassland site many years ago, Flechard et al., 2010 https://bg.copernicus.org/articles/7/537/2010/). Judging from a google maps picture from the area there are many small individual fields as well as stationary sources within the footprint area.

Deposition mainly occurs during night stable conditions when NH3 is concentrated in the boundary layer and emission occurs during instable conditions when the growing boundary layer leads to NH3 concentration below the “system compensation point”. Consequently an inverse relation between concentration and fluxes must be expected, even though at a first glance this seems contra intuitive. In this context I was very puzzled by the sentence in line 488 *“The highest concentrations were observed by both systems at noon when air temperature reached the highest level of the day (Figure 8)”.*

I am uncertain whether this is just a slip of the pen or a misunderstanding.

The evaluation of the EC data is state of the art. The most important correction is the high frequency damping in the order of 20 to 30%. I am aware that the empirical ogive method needs a lot of good data. I suggest to use only flux data with a good looking covariance function for both NH3 and temperature flux and not using daily mean values.

DOAS gradient measurements require a demanding procedure to be precise enough that includes measurements in cross position. I rate this as a very positive development of the DOAS approach that I was not expecting based on our own experiences. But I see a tendency to overestimate the precision of the DOAS measurements. E.g. in line 280 it is stated “*that differences can be measured well below our target precision of 0.1 µgm-3.”* In line 449 it is stated “*The random error of the miniDOAS NH3 concentration differences … was determined to be 0.088 µgm-3”.* This is below the target limit but not well below.

I also have an other interpretation of the cross position data. In the second period I judge that there is a systematic offset in the order of 0.14 µgm-3 as this is a consistent value for good wind conditions. In case the wind is coming from the red sector I don’t see a mechanism that could produce the measured difference between the two crossed paths, especially for the lower concentration range.

The obstacle upwind will affect mostly the turbulence and not the NH3 concentration. You could also check the EC-NH3 covariance function and time series to see whether there is a major disturbance.

As far as I know there are many influences that potentially influences concentration determination in the order of 0.1 to 0.2 µgm-3 especially under field operation. Judged from figure S5 such a correction could at least partly explain the higher NH3 emission of the AGM approach around noon.

The whole footprint discussion should be modified. In case of the EC approach the calculated fluxes are representative for a small volume at a height of 2.8m. In case of the AGM approach it is a mean vertical flux integrated over a 22m path between 0.76 and 2.26m assuming that the used transfer velocities correctly reflect the atmospheric turbulence. These fluxes need to be translated into exchange fluxes at the soil surface. The simplest approach and implicitly used in most cases is that the vertical flux is constant in all 3 dimension and consequently the measured fluxes are equal to the surface exchange flux. In such conditions footprint considerations are not an issue.

The footprint climatology shown in Figure 13 gives a good impression which area will on average determine the measured fluxes. But the analysis should be adapted to the selected data point (green and light green conditions).

NH3 exchange fluxes will most likely be different between all the small fields in the neighborhood of the measuring point within the footprint. Consequently EC and AGM approach will considerably differ as the footprint density function for the two approaches differ. Note also that this function will considerably differ for the lower and upper DOAS path.

I also suggest to separately calculated the footprint density function for stable and instable conditions. By the way the bls-R tool developed by Christoph Häni is appropriate to do so. It allows to calculate concentration and flux footprints µgm-3 (https://github.com/ChHaeni/bLSmodelR). It would interesting to overlay these footprint density function on a land use map. This would allow a plausibility control whether the recorded differences in Figure 11 reflect a topographical effect or whether systematic effects in the measurement systems are in the focus. But an in depth footprint analysis needs detailed information on the land use.

To summarize I suggest to do:

- Reconsider the title
- Add a paragraph regarding the expected flux landscape at Cabauw. This can be integrated to the discussion of the concentration characteristics.
- Down scale the footprint issue. The presented fluxes assume a homogeneous vertical flux and that the used set of turbulence parameter reflect the present meteorological conditions. A consideration of the footprint is for another paper.

For my curiosity I would like to see in a supplement or in the main paper a plot with the AGM concentration differences, the transfer velocities and the fluxes (extension of figure S5). Regarding the EC data it would be helpful to see NH3'w' covariance function as well as ogives for a few cases , both from the green and red sectors. (not only the best!).