

The manuscript titled 'Nitrogen oxides and ozone fluxes for an oilseed-rape management: influence of organic fertilisation' by Vuolo et al. discusses fluxes of NO, NO₂ and O₃ measured using the eddy covariance method. The paper is well written and structured and describes an interesting dataset which I believe the flux community will like to see. I recommend that the paper should be published subject to some small edits which I leave to the editors and author's discretion. I hope that my comments aid the authors and look forward to seeing the paper published.

Comments:

It may be prudent to change the title to 'Nitrogen oxides and ozone fluxes from an oilseed-rape management cycle: the influence of cattle slurry application'. The term organic fertilisation covers a wide variety of possibilities and as the authors point out, much of the chemistry occurring can be dominated by VOC emissions which will vary widely depending on fertiliser type and consistency.

As mentioned in the previous review of the paper, it is a fairly long submission. The author's attempts to shorten the paper by converting some of the methodology section to supplementary material do help with this. I don't believe that shortening the paper further would improve its readability and would only serve to damage its scientific value.

The soil pH is relatively high (7.6). Is this normal for the field or due to recent liming? There is little mention of this on nitrification rates.

There is mention of the FIDES footprint analysis which shows that the effect of pollution from the cars would be minimal, but no graphical representation of this. Would it be possible to include a rough sketch of the field site and location of the roads with a representation of the footprint contribution during the measurement period?

What percentage of eddy covariance measurements passed QC steps for each compound? What is the total time coverage for each?

Which version of eddy pro was used? Were any other settings changed in eddypro outside of the carboEurope settings to accommodate NO, NO₂, O₃. (i.e. spike removal, outliers etc...) If so please include a brief summary.

L190: It would be useful to give the reader a range of Reynolds number that would be in the turbulent range for the site in brackets. i.e (xxxx to xxxx)

I would like to see a bit more detail in how the fluxes were quality controlled. What were the cut-off values for outliers and why? Was u^* limited used as a cut-off? If so, what limit was chosen and why? If not, why not?

L213: At what point would the authors deem the eddy covariance method unusable?

Section 3.3.1: These observations seem worrying when applying eddy covariance to such reactive compounds which are constantly changing as they disperse from sources. Later in Section 3.7 an estimate of 4 to 40% of contribution to fluxes is described based solely on estimated stats. Horizontal transfer of the species being measured in and out of the storage

area of the fetch and the resultant advection effect seems to be a very significant source of uncertainty and one which cannot be fully accounted for in this study even with de-spiking etc... It is understandable that no field site is perfect, but as one of the aims of this study is to assess if eddy covariance is suitable for the measurements it seems odd to accept such a large source of error as a given. Perhaps some of the more negative aspects of the methodology should be embraced as a discussion point for future studies?

Is it possible to report a detection limit for each of the measured fluxes?

Perhaps this comment is beyond the scope of the paper and I do not expect the authors to amend the manuscript. Was short term changes in PAR (i.e. the effect of clouds) compared to the random error of Ozone fluxes? Is this not an issue when looking at fluxes over a 30 min period when UV exposure can change so dramatically over very short time periods? If fluxes were calculated over a 60 min period instead of 30 minutes are the same fluxes and correlations observed? Would it reduce cumulative flux uncertainty at the cost of data points and the observation of diurnal patterns?

Section 3.6: How were cumulative totals estimated? Linear regression between points or using the diurnal cycles to gap fill? With such consistent patterns and correlations it seems like gap filling could be modelled relatively well?

Axis Text on Fig 7 has overlapped in places

Optional:

I don't like the phrase 'changed sign' referring to fluxes switching between emission and uptake at different levels. L23 & L 526. If possible please re-word.

L21: replace 'at all times during' with 'constantly throughout'

L 37: replace 'increasing risks for' with 'exposure to which increases risk to'

L48: replace 'mostly due to' with 'primarily the by-products of'

L136: replace '~monthly' with 'approximately once a month'

L193*This is assumed to be "white noise" and.....

L241: replace 'on' with 'over'

L246: replace 'strongest' with 'highest'

L258: ...it can be deduced that deposition velocities were around...

L337: probably similar to those measured in September

Technical:

Indents throughout the manuscript are inconsistent. Editorial team will correct?

Dates are presented inconsistently throughout. Choose either (18th of February) or (18/02/16) format and stick with it.

L131: *December

L124 * performed on

L223: *7 month period

L25: replace 'than' with 'of'

L297: 0.27 or 0.25? See line295

L330*24th

L397: delete 'of'