

# Interactive comment on “Nitrogen oxides and ozone fluxes following organic and mineral fertilisation of a growing oilseed-rape” by Raffaella M. Vuolo et al.

## Answer to Anonymous Referee #1

Received and published: 3 August 2016

I thought this was a very well written article, summarising a study which used the eddy covariance method to monitor variation in  $\text{NO}_2 + \text{NO}$  ( $\text{NO}_x$ ) and  $\text{O}_3$  in a field of oilseed rape. As the authors point out, there have been few papers which have measured  $\text{NO}_x$  and  $\text{O}_3$  in the field using this technique and none which have measured them in an agricultural setting. One initial thought it that the article is fairly long, and the editor may feel that some information, particularly in the methods section, may be trimmed.

We thank referee #1 for this supporting comment. Regarding the methods section, we are open to editor's proposal to move part of it in a section of the supplementary material. We propose to suppress title in line 182 and merge with title in Line 174 as “*Spectral corrections and flux uncertainties*”. We further propose to move sections 2.6 and 2.7 in supplementary material sections S1 and S2 and write a condensed description of these sections below line 189:

### “2.5 Chemical reactions, time scales and flux divergence

*Chemical reactions between  $\text{NO}$ ,  $\text{NO}_2$  and  $\text{O}_3$  are important to consider when interpreting the measured fluxes as they can affect the fluxes above the ground. A common way to determine whether these reactions may indeed affect the flux is through comparison of chemical and transport time scales. Details of the reactions rates, times scales and flux divergence calculations are given in supplementary material sections S1-S3.”*

The results and discussion sections are comprehensive and insightful. I have a limited number of comments/questions listed below.

Line 30 –  $\text{NO}_x$  are toxic to humans above a certain critical limit (<http://ec.europa.eu/environment/air/quality/standards.htm>) and are also potentially damaging to ecological systems above a critical load (<http://www.apis.ac.uk/indicative-critical-load-values>). There are similar, but lower, values relevant to ozone. This is worth mentioning in the introduction. I think these discussions would help to put your findings in the context of air quality issues. These could be referred to again in the discussion. What do your findings mean in relation to pushing concentrations towards critical limits?

We thank referee #1 for this comment. We already mentioned toxicity of  $\text{NO}_x$  and  $\text{O}_3$  at lines 30 and 35. Indeed,  $\text{NO}_x$  and  $\text{O}_3$  are toxic to humans and animals. Similarly nitrogen deposition leads to serious adverse effects on vegetation (eutrophication, biodiversity erosion and acidification being the most serious ones), while  $\text{O}_3$  has a direct adverse effect on plant health through oxidation of photosynthesis pathways and direct tissue destruction above large thresholds. We propose to remove the following text lines 20-31 “ $\text{NO}_x$ , and especially  $\text{NO}_2$ , are toxic gases for humans (WHO, 2013) and national and international authorities regulate their levels”, and line 35 “*is a major tropospheric pollutant, harmful for humans and ecosystems, and*”, and add the following text after line 36:

*“ $\text{NO}_x$ , and especially  $\text{NO}_2$ , are toxic gases for humans, increasing risks for various respiratory diseases. World Health Organization gives guidelines for  $\text{NO}_2$  exposure limits, both annual means ( $40 \mu\text{g m}^{-3}$ ) and 1-hour mean ( $200 \mu\text{g/m}^3$ ) (WHO, 2005: Air quality guidelines for particulate matter, ozone, nitrogen dioxide and sulfur dioxide). For ozone, only a short-term threshold is given ( $100 \mu\text{g/m}^3$  for 8-hour mean) because there are fewer studies on long-term exposure. These thresholds are established both on epidemiological and toxicological studies on humans and animals. Similarly nitrogen deposition leads to serious adverse effects on vegetation (eutrophication, biodiversity erosion and acidification being the most serious ones), while  $\text{O}_3$  has a direct adverse effect on plant health through oxidation of photosynthesis pathways and direct tissue destruction above large thresholds. For nitrogen, the concept of critical load has been developed which gives the amount of nitrogen deposition above which an ecosystem is impacted. These critical loads range from  $5 \text{ kg N ha}^{-1} \text{ yr}^{-1}$  for sensitive habitats to  $20 \text{ kg N ha}^{-1} \text{ yr}^{-1}$  for less sensitive ones (APIS, 2016). For these reasons, national and international authorities regulate atmospheric levels of these pollutants.”*

We propose to also refer to it in the conclusion section as follows (added after line 500):

“Our findings show that NO emissions from agricultural soils are limited (0.27% of the N-NO applied over the 8 month period, which with a conservative estimation we can extend to a yearly amount). When extended to France with an average nitrogen fertiliser use of 80 kg N ha<sup>-1</sup> over a fertilised area of around 26 Mha, this would lead to emission of NO<sub>x</sub> of around ~5.6 t N-NO ha<sup>-1</sup>, which is negligible compared to transport and industry which is several hundreds of thousand larger (CITEPA, 2015). The seasonality of these emissions may however lead to air quality issues during spring and late summer-autumn which are the main fertiliser application periods. Indeed, most of the emission we measured occurred with a few weeks following fertilisation. In terms of ozone, our findings, and previous ones, show that ozone is efficiently deposited throughout the year. This means that crops are participating through this process in the reduction of the atmospheric oxidising capacity”

Answers to referee #2 provide more details on O<sub>3</sub> deposition analysis.

Mention is made of ‘significant difference’ in the results section but I am unsure of the precise statistical methods used to derive these conclusions. Please add more detail.

We propose to explain what was meant by ‘significant’ each time the term is used in the text. The term was misused in line 336 where we propose to replace it by “The NO<sub>2</sub> flux daily pattern was different”. Otherwise, we used Student t-tests to check a difference in mean.

This field is surrounded by heavy trafficked roads. To what extent do these roads fall within the flux footprints? Vehicles are prolific producers of NO<sub>x</sub> and O<sub>3</sub>. The authors have referred to this in part in the ms, but is it possible to use statistics to unpick the contribution of the field and traffic to the different levels? Perhaps by correlation with traffic densities.

This is a good question indeed. The field is surrounded by heavy traffic roads. Unfortunately we do not have traffic statistics at that location for the given period. To answer this question, we propose to evaluate the footprint of the roads using the FIDES flux and concentration footprint model (Loubet et al., 2010), which is essentially similar to the Korman & Meixner model (Kormann and Meixner, 2001) but with a different treatment of the lateral dispersion. Overall the flux footprint from the nearby roads is smaller than 1% (which means that only 1% of the roads emission contributes to the flux at the mast) most of the time, but the concentration footprint reaches up to 10% during some episodes, with different roads contributing differently during different periods.

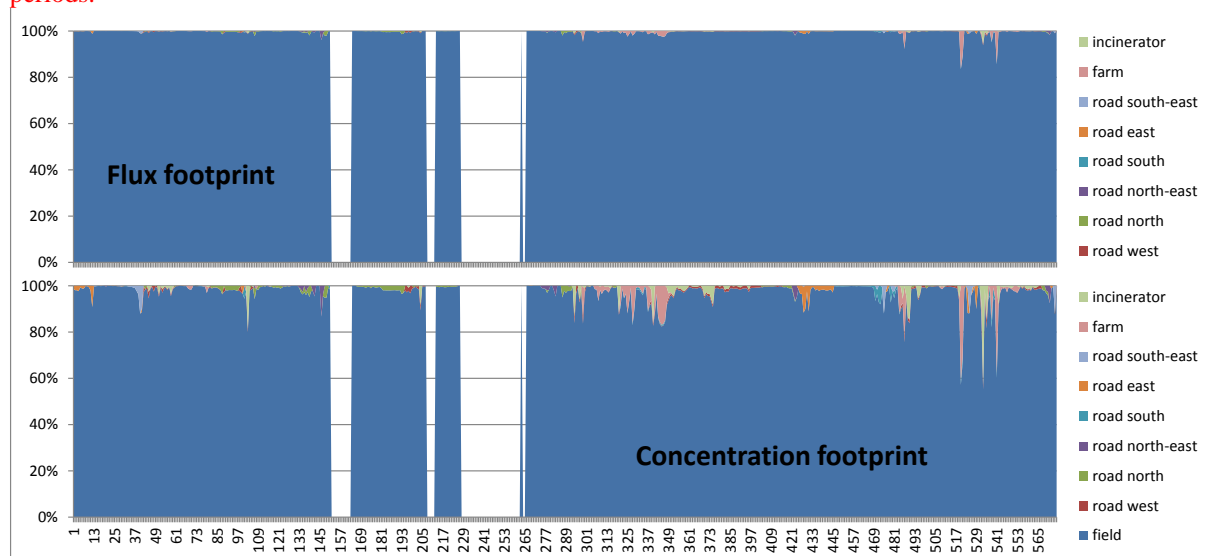


Figure R1. Flux and concentration footprints of the field and surrounding roads calculated with the FIDES model.

Using the flux and concentration footprint allows evaluating the contribution of traffic to the NO<sub>x</sub> concentration and fluxes. For that, a conservative emission of 250 mg km<sup>-1</sup> vehicle<sup>-1</sup> was considered. The average vehicle count per day ranges from 5000 to 13000 on the surrounding roads (2010 counts, “Statistiques du département des Yvelines pour 2010”). Using an average of 10000 vehicles per day, we can calculate that the flux due to the surrounding roads may be of magnitude 4% to 40% of the measured flux. However, the NO<sub>x</sub> vehicles emissions have a sporadic nature: indeed, 10000 vehicles per day means a maximum of 1 vehicle every ~2 second if we consider, conservatively, that most of the traffic is condensed during 9 hours only. This vehicle is moving at say 90 km h<sup>-1</sup> (25 m s<sup>-1</sup>) hence leading to a moving point source of NO<sub>x</sub>. We therefore expect that the signal of this moving and sporadic source is not captured by the eddy-covariance method, and would be filtered out by

despiking and flux calculations (Foken, 2008). From another perspective, we cannot exclude that we have not biased the flux by filtering out the flux coming from the roads as discussed by Mahrt (2010)

We propose to add the following discussion section after line 316:

*Using the FIDES flux and concentration footprint model (Loubet et al., 2010) we evaluated the footprint of nearby roads. Overall the flux footprint from the nearby roads was smaller than 1% (which means that only 1% of the roads emission contributes to the flux at the mast) most of the time, but the concentration footprint reaches up to 10% during some episodes, with different roads contributing differently during different periods (Figure S1). Assuming a conservative emission of  $250 \text{ mg km}^{-1} \text{ vehicle}^{-1}$  and an average vehicle count 10000 vehicles per day (2010 counts, "Statistiques du département des Yvelines pour 2010" shows range between 5000 and 15000), we evaluate a contribution from 4% to 40% of the road on the measured flux. However, since vehicles emissions of  $\text{NO}_x$  have a sporadic nature. Indeed 10000 vehicles per day means a maximum of 1 vehicle every ~2 second (if we consider, conservatively, that most of the traffic is condensed during 9 hours only). These vehicles are also moving at say  $90 \text{ km h}^{-1}$  ( $25 \text{ m s}^{-1}$ ) hence leading to a moving point source of  $\text{NO}_x$ . We therefore expect that the signal of this moving and sporadic source is not captured by the eddy-covariance method, and would be filtered out by despiking and flux calculations (Foken, 2008). From another perspective, we cannot exclude that we have not biased the flux by filtering out the flux coming from the roads as discussed by Mahrt (2010)."*

We further propose to include Figure R1 in Supplementary section S4 and add following text to introduce the figure:

#### ***"S4 Flux and concentration footprint***

*The flux and concentration footprint was roughly estimated for each of the major roads around the site. Each road was geo-localised and assumed 10 m width. The FIDES model was computed with field roughness ( $z_0$ ), friction velocity ( $u_*$ ) and Obukhov length (L)."*

Line 460 - Vehicles also emit VOC which also may affect your interpretation here.

This is true indeed, but the amount of VOC to  $\text{NO}_x$  emitted is small (below 1% for non methanic VOC, according to French national emissions inventory, CITEPA (2015), not considering CO) and hence the effect on  $\text{O}_3$  concentrations and flux is expected to be of second order. Moreover, we see increased  $\text{O}_3$  deposition on a period (August) which has the lowest traffic density throughout the year in this area, and we do not see increased deposition during high traffic load days throughout the year, which points towards a small effect of VOC emitted by vehicles on the  $\text{O}_3$  flux in August.

However, we have tempered our interpretation on the potential effect of VOC emitted by slurry on ozone flux since, as pointed out by the reviewer #2, the increase of ozone following fertilisation may have other physical and chemical explanations.

#### **Technical/minor corrections**

Line 10 – '7-months' should be '7 month' or 'over a period of 7 months'

Thanks for this suggestion. We propose to correct for it.

L104 – 'sheep' not 'sheeps'

Thanks for this suggestion. We propose to correct for it.

## Answer to Anonymous Referee #2

Received and published: 16 August 2016

The manuscript presents eddy covariance flux measurements for O<sub>3</sub>, NO and NO<sub>2</sub> over an arable field during 8 months between August and the following April. The flux data processing and quality assessment appears sound. The discussion of the NO fluxes (with influence of fertiliser application) and the chemical reactions of the NONO<sub>2</sub>-O<sub>3</sub> triad show that the related gas phase chemistry below the flux measurement height can have a considerable effect on the NO and NO<sub>2</sub> fluxes but only a very small relative effect on the O<sub>3</sub> flux. This finding is in agreement with previous similar studies (e.g. Stella et al., 2012).

However the discussion of the O<sub>3</sub> deposition (velocity) and its temporal variation is quite disappointing in its present state. In the discussion of the 8-month time series of ozone fluxes, the authors mainly concentrate on 2 weeks in August pointing out that the deposition velocity after slurry spreading (average midday values around 0.6 cm/s) was much larger than for the rest of the observation period (average midday values around 0.2 cm/s). They then search for processes that can explain the higher deposition velocity and identify reactive VOC compounds as the most likely cause. I see major shortcomings in this evaluation and interpretation that are detailed in the following comments.

We thank Referee #2 for this comment. We have put more emphasis on NO and NO<sub>2</sub> than O<sub>3</sub> in this manuscript because, as spotted by referee # 2, we have already published a lot of results on O<sub>3</sub> on this site while NO and NO<sub>2</sub> flux measurements are newer. We nevertheless propose to include a result and discussion section specifically on ozone fluxes and deposition velocity. We agree that we might have put too much emphasis on the potential reaction between VOC compounds and O<sub>3</sub> in the current manuscript and therefore propose to compare our O<sub>3</sub> deposition velocity with existing literature and especially with the parameterisation we had on bare soil in Stella et al. (2011). We also have more balance the discussion between the three main hypotheses that can explain the increase in ozone deposition following slurry incorporation: 1) soil surface increase due to tillage, 2) reaction with NO in the soil and 3) reaction with emitted VOCs. We agree that we can not conclude on which process (or combination of processes) may explain these observations.

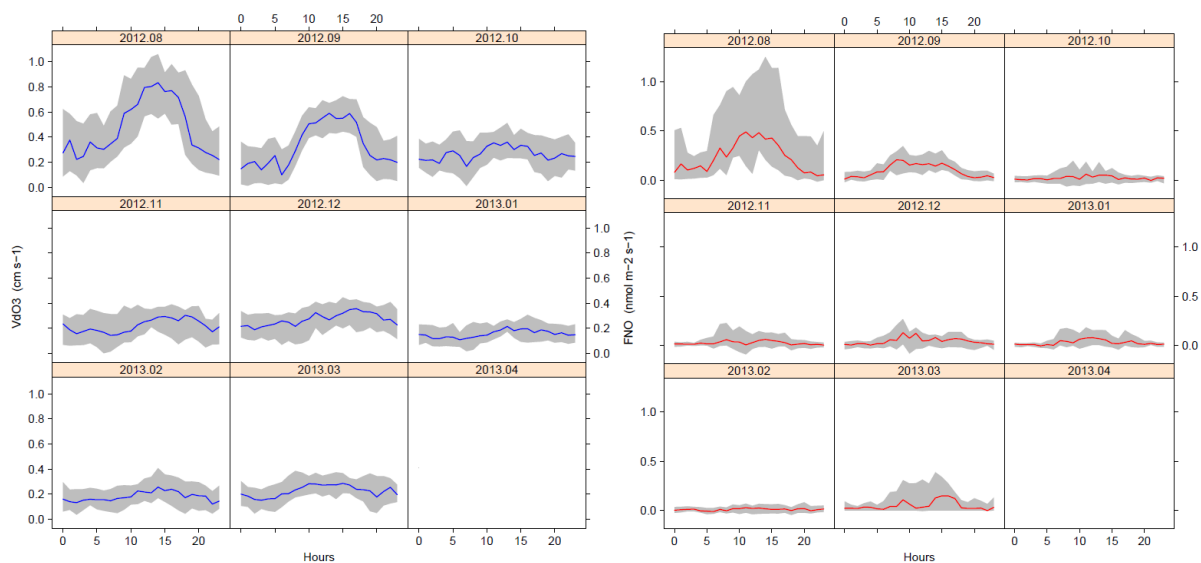
### MAJOR COMMENTS

1) A comparison of the observed O<sub>3</sub> deposition velocities with previously published data is largely lacking, although it would be very important to compare the magnitude of the values observed in this study with literature results.

This is indeed a good comment. We have included in the Figure R2 below the ozone deposition velocity during the period as averaged daily patterns. We propose to include Figure R2 and the following short paragraph in a supplementary material S6 section on ozone seasonal pattern:

#### *S6. Seasonal pattern of O<sub>3</sub> deposition velocity and NO fluxes*

*We found similar magnitude of ozone fluxes in August and September as those reported by Stella et al. (2013) over a meadow during the summer. We also found similar nocturnal O<sub>3</sub> deposition velocity as found by Stella et al. (2011) over bare soil during summer, but higher daily maximum (0.8 cm s<sup>-1</sup> instead of 0.5-0.6 cm s<sup>-1</sup>). We further find a similar midday magnitude as Stella et al. (2011) found in April with wetter soils. Night-time ozone deposition velocity does not go lower than around 0.2 cm s<sup>-1</sup> in our study, as also found by Zhu et al. (2015) over a growing wheat in China, Stella et al. (2011) over bare soil in summer, and Lamaud et al. (2009) over maize. These authors as well as Huang et al. (2016) clearly show that this is due to non-stomatal deposition being primarily driven by  $u_*$  which does not reach zero at night during these periods. We can hence conclude that we found consistent ozone deposition in August and September compared to other studies at that site or in other geographical areas. When compared to previous years on the same site the deposition velocity measured during the winter in this study was clearly smaller. We interpret this as being primarily due to  $u_*$  being smaller than winter compared to other winters as well as due to a slow growth of the winter crop due to soil drought in September (SWC =20% in the 15 cm horizon).*



**Figure R2. Seasonal changes of ozone deposition velocity  $V_{dO_3}$  and NO fluxes. Lines show median and grey area inter-quantiles.**

2) It is particularly astonishing that the authors do not compare their observations with results for the  $O_3$  deposition velocity by Stella et al. (2011) observed over bare soil at the same site. This comparison would reveal that the deposition velocities after slurry application presented here are very similar to previous results over bare soil up to 1 cm/s (also without slurry application). Thus the deposition velocity after slurry spreading is obviously not exceptionally high, but quite normal for bare soil. The need for a special additional chemical sink is therefore not so clear. It rather needs to be discussed why the deposition velocities in the other periods were exceptionally low in the present study, i.e. lower than in other studies over arable/wheat fields (see e.g. Potier et al., 2015, Agric. For. Meteorol.).

As just exposed in the answer to question 1) of Referee #2, we found similar night time deposition velocities as in Stella et al. (2011) but larger daytime values during the summer and similar as in April 2007, which showed also wetter soils. Regarding the other periods, the fact that we report smaller ozone deposition velocity than in Potier et al. (2015) is due to the periods which are spanned in this study: from August to March, which correspond to the period with the lowest stomatal component and also the smallest non-stomatal component because of the small leaf area index of the rapeseed. This winter also showed an especially low  $u_*$ .

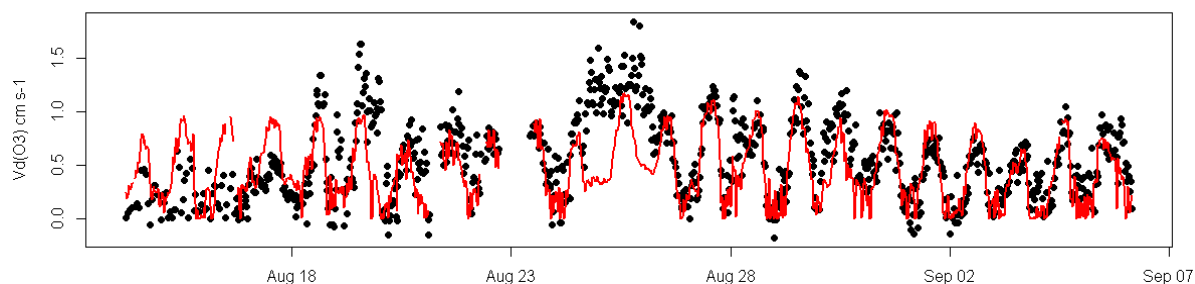
3) As shown by Stella et al. (2011) the  $O_3$  soil resistance, and thus the deposition velocity, for bare soil strongly depends on the soil surface temperature and/or on the relative surface humidity (for an overview of related processes, see Fowler et al., 2009, Atmospheric Environment). In Figure 8 (and 7a/b) it can be seen that the strong increase of the dep. velocity at slurry application coincides with a strong increase of the daytime surface temperature by about 15\_C (from <25\_C to >40\_C). I suggest that the authors apply the relationship for  $R_{soil}$  derived by Stella et al. (2011) to their own dataset, and discuss the deviation of the measurements from that relationship.

This is a very good suggestion which we followed. We propose to add Figure R3 and R4 together with the following paragraph in the supplementary material S7 section on ozone seasonal pattern:

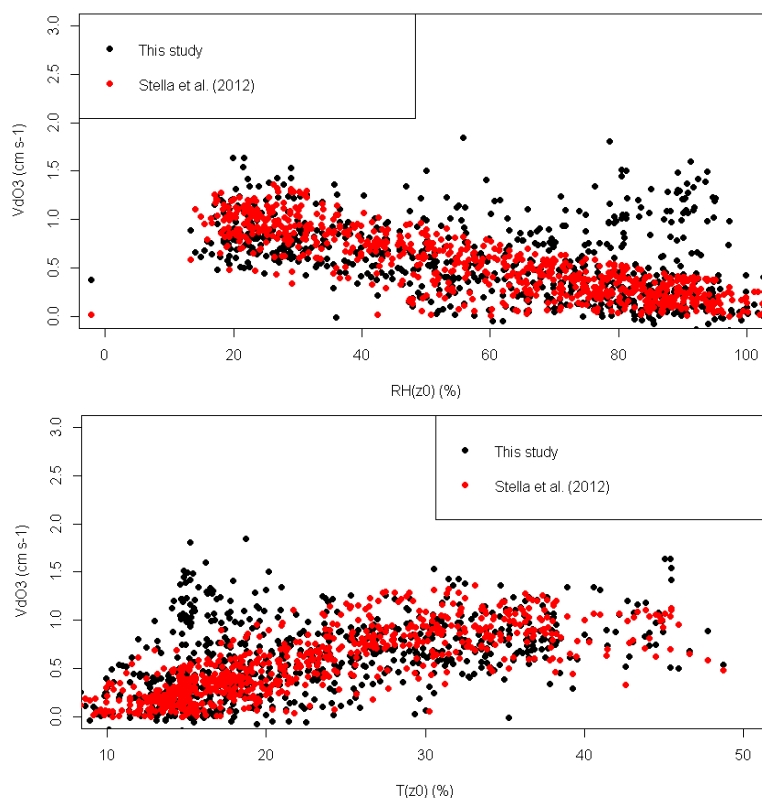
### **S7. Comparison of ozone fluxes to Stella et al. (2011)**

*In order to compare to previous studies of ozone deposition to bare soil on the same site, we have calculated the soil surface resistance as in Stella et al. (2011) and deduced the ozone deposition velocity as  $V_{dO_3} = (R_{soilO_3} + R_{bO_3} + R_a(z_{ref}))^{-1}$ . In this way, we can compare the two studies while excluding any confounding factors (roughness and turbulent exchange intensity). We can see in Figure (R3) that the measured ozone deposition velocity during August follows most of the time the parameterisation of Stella et al. (2011) except for some days including 18 and 19 August which corresponds to slurry application and 24, 25, 26 August, which follows a small rainfall. We also see an overestimation of the Stella parameterisation before the 18 August, which we interpret as being due to the straw and wheat residues laying on the ground before slurry incorporation. This comparison hence demonstrates that the ozone deposition was indeed increased slightly following slurry application and subsequently following rainfall. This may be either due to a physical reason (increased surface exchange on the soil due to tillage or humidity change due to slurry) or a chemical reason (surface reactivity*

changes due to added organic matter or VOC emissions from slurry). Figure R4 further shows that the main differences are observed for wet soils and relatively low temperatures (this is after rainfall) and to a lesser extent for dryer and hotter situation (following slurry spreading).



**Figure R3.** Comparison of ozone deposition velocity from this study (black dots), and from the parameterisation of Stella et al. (2011) (red line) based on surface temperature.



**Figure R4.** Response of ozone deposition velocity to surface humidity  $RH(z_0)$  and surface temperature  $T(z_0)$ . Shown are data from this study and from the parameterisation of Stella et al. (2011). Period from 14 August to 6 September which is before and after slurry spreading and corresponds to Figure R3

4) Figures 5 and 6 are mentioned only shortly in the text and do not provide much additional insight. They could either be omitted or at least reduced (Fig. 5 to one season/whole experiment; Fig. 6 to flux histograms).

*That sounds like a fair suggestion. However we feel that these are still important and therefore propose to put these two figures in a supplementary material section S5, and refer to in the text. "S5 Wind roses and histograms of NO, NO2 and O3 concentration at the site."*

5) Title: I am not sure if the title is really appropriate. It makes not much sense to focus on the comparison of "fluxes following an organic and a mineral fertilization" if the first was in high summer and the second in deep



winter. It would be more appropriate to mention the observed seasonal/management cycle for the winter crop in the title, and also to discuss it more profoundly.

This is a sound suggestion. We propose to change the title to: "Nitrogen oxides and ozone fluxes for an oilseed-rape management cycle: influence of organic fertilisation"

#### MINOR COMMENTS

6) Line 160: Please explain what "filtered for outliers" means here. I guess it was a kind of smoothing and gap filling procedure?

Points away from median lag  $\pm$  standard deviation were considered as outliers for the lag.

7) Line 165: This equation is only valid, if the raw ozone signal has no offset. Why did you not use an analyser sensitivity here (like in the following equations)?

Thank you for this comment. Indeed, the ratio method as described in Muller et al. (2010) and as applied here, considers that the signal has no offset. The alternative methods, computation of the flux by use of sensitivity and offset values, fixes this problem but, on the other hand, present shortcoming in terms of offset and sensitivity determination. Indeed, this kind of fast-sensor make use of cumarine disks whose sensitivity decrease on a weekly timescale, but this is not taken into account in the sensitivity estimation by signal regression or by disk calibration, that assumes sensitivity to be constant for each disk. We chose the ratio method that neglects offset but takes into account the sensitivity variation along the lifetime of the cumarine disks. An example of regression between fast- and slow- sensors signals for a three-day period is given in fig. R5. With a negative offset of 0.04V, the relative error on the flux, that can be estimated as the ratio between the offset and the raw signal, would range between about 4% to 20% (for ambient concentration of 80 and 20ppb respectively).

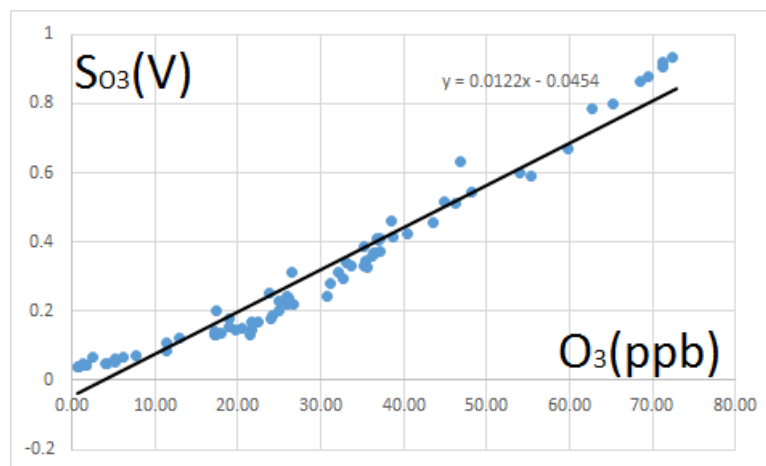


Figure R5. O3 fast sensor signal versus slow-response analyser signal. Half-hour means for a period of 3 days.

8) Line 166/167: It is unnecessarily confusing to use "s" in these equations twice for two different quantities (in size scaled equations the uppercase and lowercase "s" are often difficult to distinguish). I would recommend to use two different symbols instead for the raw signal and the analyser sensitivity.

We propose to use now the name of the compound instead of s for signals.

9) In Eq. 3 the left side should read "F\_NO2" (the subscript 2 is missing in my copy)

Thanks for spotting this. We propose to change for  $F_{NO_2}$ .

10) Line 169: It is not fully appropriate to talk of a "NO2 sensor signal" because there was no NO2 sensor. It would be more correct to name it "NOx sensor signal" or "BLCsignal", from which the NO2 flux was derived as a difference to NO (acc. to Eq. 3).

Thanks for this suggestion. We propose to use "NO<sub>x</sub> sensor signal".

11) Line 171: It is quite confusing to use the same (or a very similar) symbol for the molar volume and for the deposition velocity in this manuscript. Volumes are generally represented by an uppercase "V". I would suggest to use here the symbol "V<sub>dry</sub>".

Thanks for this suggestion. We propose to use  $V_{dry}$  instead. Overall we propose to change text and equations from lines 165 to 173 as:.

$$F_{O_3} = \frac{\overline{\chi_{O_3}} \overline{w' O_3'}}{V_{dry} \overline{O_3}} \quad (1)$$

$$F_{NO} = \frac{\overline{w' NO'}}{S_{NO} V_{dry}} \quad (2)$$

$$F_{NO} = \frac{1}{\alpha V_{dry}} \left( \frac{\overline{w' NO_x'}}{S_{NO_2}} - \frac{\overline{w' NO'}}{S_{NO}} \right) \quad (3)$$

where  $O_3$  (in mV),  $NO$  and  $NO_x$  (in counts  $s^{-1}$ ) are the uncalibrated fast signals,  $\chi_{O_3}$  is the 30 min average of the slow-sensor reference  $O_3$  mixing ratio (in ppb), while  $S_{NO}$  and  $S_{NO_2}$  are the sensitivity of the analysers (in counts  $s^{-1}$  ppb $^{-1}$ ).  $\alpha$  is the blue light converter conversion efficiency, and  $V_{dry}$  is the molar volume of dry air (in  $m^3$  mol $^{-1}$ ). All fluxes (momentum, heat,  $CO_2$ ,  $H_2O$ ,  $NO$ ,  $NO_2$ ,  $O_3$ ) were computed by the Eddypro software and final flux data were averaged for 30 min intervals. Evaluation methodologies from the CarboEurope project were applied - see (Aubinet et al., 2000 ; Loubet et al., 2011).

12) Line 226: This formulation is confusing. Were there two high frequency losses? I think this should be rephrased to "The first main uncertainty was : : :"

Indeed this formulation was misleading. We propose to rephrase with "The largest uncertainty ...". We then rephrase Line 220 as "The second largest uncertainty ..."

13) Line 235f.: How was this high uncertainty effect for  $NO_2$  quantified in the uncertainty calculation?

Actually, we have mistaken here  $NO_2$  for  $NO_x$ . As the uncertainty analysis was performed on the  $NO_x$  channel. Hence we have changed the text accordingly: "A higher random uncertainty was found for  $NO_x$  fluxes which were smaller than  $NO$  fluxes and with a relatively low conversion ratio from  $NO_2$  to  $NO$  (30%)"

14) Line 298: "which was probably due to a dryer soil in this study." It is not clear which study is meant here. Please make a clearer distinction between "this/these" and "that/those".

We propose to change for: ". Stella et al. (2012) measured larger peak of  $NO$  emissions following slurry spreading, but only lasting two to three days, which was probably due to a dryer soil in our study compared to Stella et al. (2012). "

15) Lines 346-356: this paragraph is oddly placed here. It should be combined with the text in chapter 3.7.2.

This is a sound suggestion we propose to move and cut this paragraph to include section 3.7.2 as follows:

"Reactive VOCs such as sesquiterpenes and monoterpenes were previously found to be emitted from soils (Horvath et al., 2012; Penuelas et al., 2014), and some of these sesquiterpenes species react with  $O_3$  in the order of a few seconds. The reactions of  $O_3$  with larger terpenes are important sources of  $OH$ , as well as the ozonolysis of simpler unsaturated compounds. (Donahue et al., 2005). "

16) Line 401-417: This paragraph with Eq. 10 to 12 should be moved from the discussion to the method section, since this calculation already has been applied previously for the same site (e.g. Stella et al., 2012).

This is indeed a good suggestion. We propose to move lines 402-417 to a supplementary section after the chemical time scales (actual sections 2.7):

### S3 Evaluating the flux difference between ground and the reference height

When chemical timescale is shorter than transport timescale, chemical reactions affect concentrations and fluxes, resulting in flux divergence. This causes the flux at the measurement point to be different from the surface flux. The flux difference can be evaluated with a method developed by Duyzer et al. (1995) based on the early developments of Lenschow (1982) and Lenschow and Delany (1987). This method assumes a logarithmic profile of the flux divergence and depends on measured mixing ratios, stability function and friction velocity:

$$(\partial F / \partial z)_z = a \ln z + b \quad (10)$$

$$\alpha_{NO_2} = -\alpha_{NO} = -\alpha_{O_3} = -\Phi_H / k u_* \left[ k_r \left( \overline{[NO]} F_{O_3, z_{ref}} + \overline{[O_3]} F_{NO, z_{ref}} \right) - j_{NO_2} F_{NO_2, z_{ref}} \right] \quad (11)$$

Here  $\overline{[NO]}$  and  $\overline{[O_3]}$  are mixing ratios which should ideally refer to the geometric mean height of the profile measurements but was taken from the measurement height in our study,  $z_{ref}$  is the measurement height and  $\Phi_H$  is the stability correction function for heat estimated at  $z_{ref}$  (Dyer and Hicks, 1970). Coefficient  $b$  of Eq. 10 can be computed as  $b = -a \ln(z_2)$  where  $z_2$  is the height above which the flux divergence is zero. Duyzer et al. (1995) showed with numerical simulations that the  $NO_x$  flux divergence could be approximated by Eq. 10 below a height of 4m, while it was negligible above. We refer to 4 m as the reference height  $z_2$  at which we assume the



flux divergence to be zero. Equation 10 can be integrated from measurement height to any height, for each compound giving:

$$F(z_{z0}) = F(z_{ref}) + a(1 + \ln(z_2))(z_{ref} - z_0) - a[z_{ref}\ln(z_{ref}) - z_0\ln(z_0)] \quad (12)$$

We propose to replace "Mainly due" at start of Line 420 by We quantified this variation by numerically solving Eq. 12, based on the model of Duyzer et al. (1995). Due

17) Line 420: "Mainly" can be omitted here.

We propose to delete.

18) Line 448: "is constant" should be omitted here.

We propose to delete.

19) Line 481f.: If it is assumed that very reactive VOCs significantly contribute to the gas phase destruction of O<sub>3</sub> it should also be discussed what the effect of these VOCs on NO could be.

This is a sound remark indeed. VOCs are known to form intermediates R $\dot{O}_2$  and HO<sub>2</sub> radicals which react with NO, converting NO to NO<sub>2</sub> (Atkinson, 2000). Once VOCs are emitted, they are broken down chemically into free radicals. The degradation reactions of VOCs lead to the formation of intermediate RO<sub>2</sub> and HO<sub>2</sub> radicals that can further react with NO, converting NO to NO<sub>2</sub> which, after photolysis, form O<sub>3</sub>.



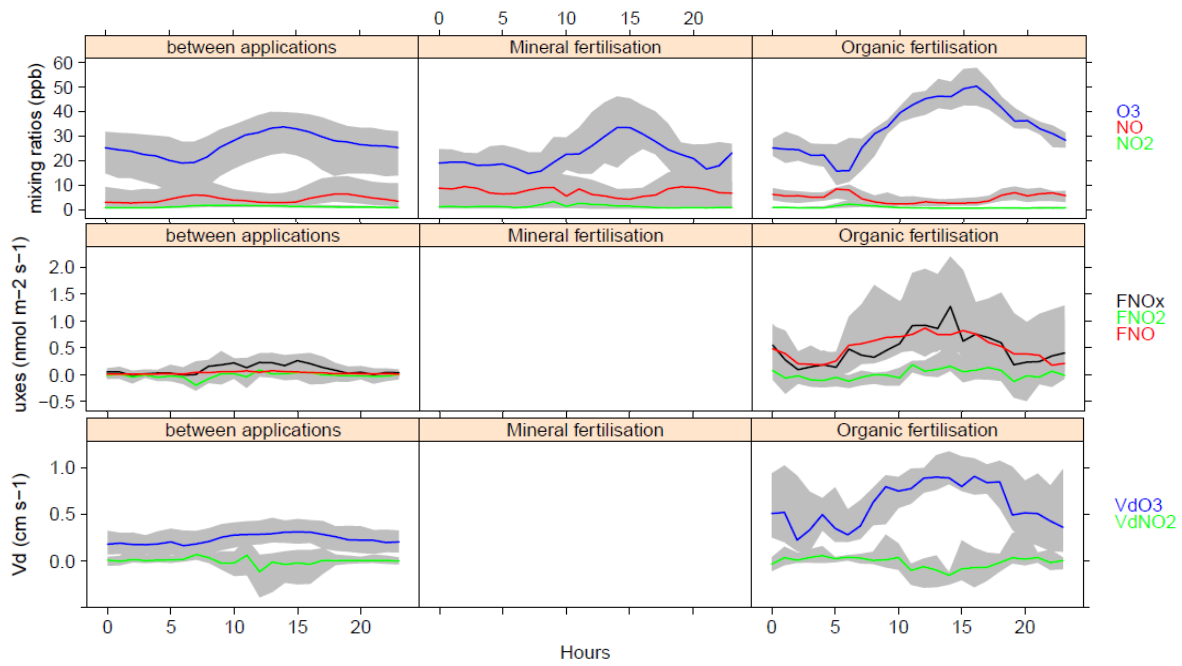
The ozone formation chain is then determined by the competition between the peroxy radicals (HO<sub>2</sub>) reactions and NO and the peroxy radical termination reactions. The factors that affect the number of NO molecules converted to NO<sub>2</sub> will also affect the rate of O<sub>3</sub> formation. These factors include radical sources and sinks, NO<sub>x</sub> sinks and reaction pathways of NO molecules converted to NO<sub>2</sub> in the VOC's degradation mechanism. The photochemical formation of O<sub>3</sub> vs. photochemical loss of O<sub>3</sub> in the troposphere depends therefore on the NO concentration and is determined by the rate of the reaction of the HO<sub>2</sub> radicals with NO. (Atkinson, 2007; Monks, 2005).

20) Line 770: The unit "N L min<sup>-1</sup>" is very uncommon. Better use standard liter "sL min<sup>-1</sup>".

We have several example of NL min<sup>-1</sup>. However if editor suggest better using sL we propose to change for sL.

21) Figs. 4, 7, 8, 9: It would be very useful for the reader to use consistent color coding for NO, NO<sub>2</sub> and O<sub>3</sub> throughout the figures.

We propose to set all colors as blue for O<sub>3</sub>, green for NO<sub>2</sub>, and red for NO. We have hence changed figure 7b to the following:



**Figure 7b.** Diurnal cycles of NO, NO<sub>2</sub> and O<sub>3</sub> mixing ratios and fluxes as well as the deposition velocities of NO<sub>2</sub> and O<sub>3</sub>, averaged over the three periods of interest at the Grignon field site. The shaded areas represent the interquartile range.

22) Fig. 10: The axis labelling is not complete.

Indeed, although Da and PSS has no units, Q units are missing. We propose to add labels (ppb s<sup>-1</sup>). The new Figure 10 looks like this:



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