Answer to Anonymous Referee #2 Received and published: 13 April 2011

We thank the reviewer for valuing our dataset and supporting a resubmission after major revision. We will certainly include the very helpful comments of reviewer 1 and 2, which will majorly improve our manuscript.

I basically follow the concerns of reviewer #1, that an interesting dataset is presented by this manuscripts, but major part of the discussions are too speculative. However, unlike reviewer #1, I think it has the potential to be published after major revisions. At the moment it does not fulfil the requirements of a "least publishable unit". I am not a friend of the reading, that more "papers" is better than "less papers" at all, but I am very aware that particularly PhD and postdocs are mainly assessed by the number of papers and the impact factor of the journals rather than by their real quality! Therefore I do understand the try or need to publish field data on N_2O of approximately two months which is per se not en vogue today.

We thank the reviewer for realizing how tough the life of post-docs is! ☺

However, I would like to highlight that papers on N_2O fluxes from eddy covariance typically present short datasets, mostly due to challenging operation of fast response N_2O analyzers; in fact many of these papers were focused on proving that fast response N_2O analyzers could be used for eddy covariance:

Pihlatie et al., 2005 Biogeosciences 2 May-5-June: a bit more than a month

Pihlatie et al., 2010 Biogeosciences 25 April to 27 June: about 2 months

Mammarella et al., 2009 two months

Nefter et al., 2007 two months

Nefter et al., 2009 about four periods of two weeks each (a total of about two months)

Eugster et al., 2007 almost one month

Wienhold et al., 1995 one month

Kroon et al., 2010 one year

Cheng-I Hsieh et al., 2005 one year

Wagner-Riddle et al., 2007 flux gradient was the only study carried out continuously for five years

We apologize if we missed some other studies. Also, sometimes papers with long datasets, even if certainly are more representative of a certain ecosystem not necessarily provide more information than papers with shorter datasets. We have now almost one year of data and our interpretation of the environmental drivers on the peak N_2O loss has not changed. We however realize that the short dataset is indeed a limitation and we will highlight this in the revised manuscript.

As already stated by reviewer #1 you should mainly skip the speculation on why u* and windspeed correlated for some days with N_2O fluxes (some literature can be included, a rough calculation on N_2O dissolved in water evaporated by the plants; but also wind may influence soil derived emissions). You must include data on H_2O and CO_2.

We agree with the reviewer on the need to present more data to improve the manuscript. We will include CO_2 and H_2O fluxes and implement the statistical analysis. We performed also a rough back-of-the-envelope calculation of the possible maximum N_2O release through the transpiration stream which supports our previous interpretation:

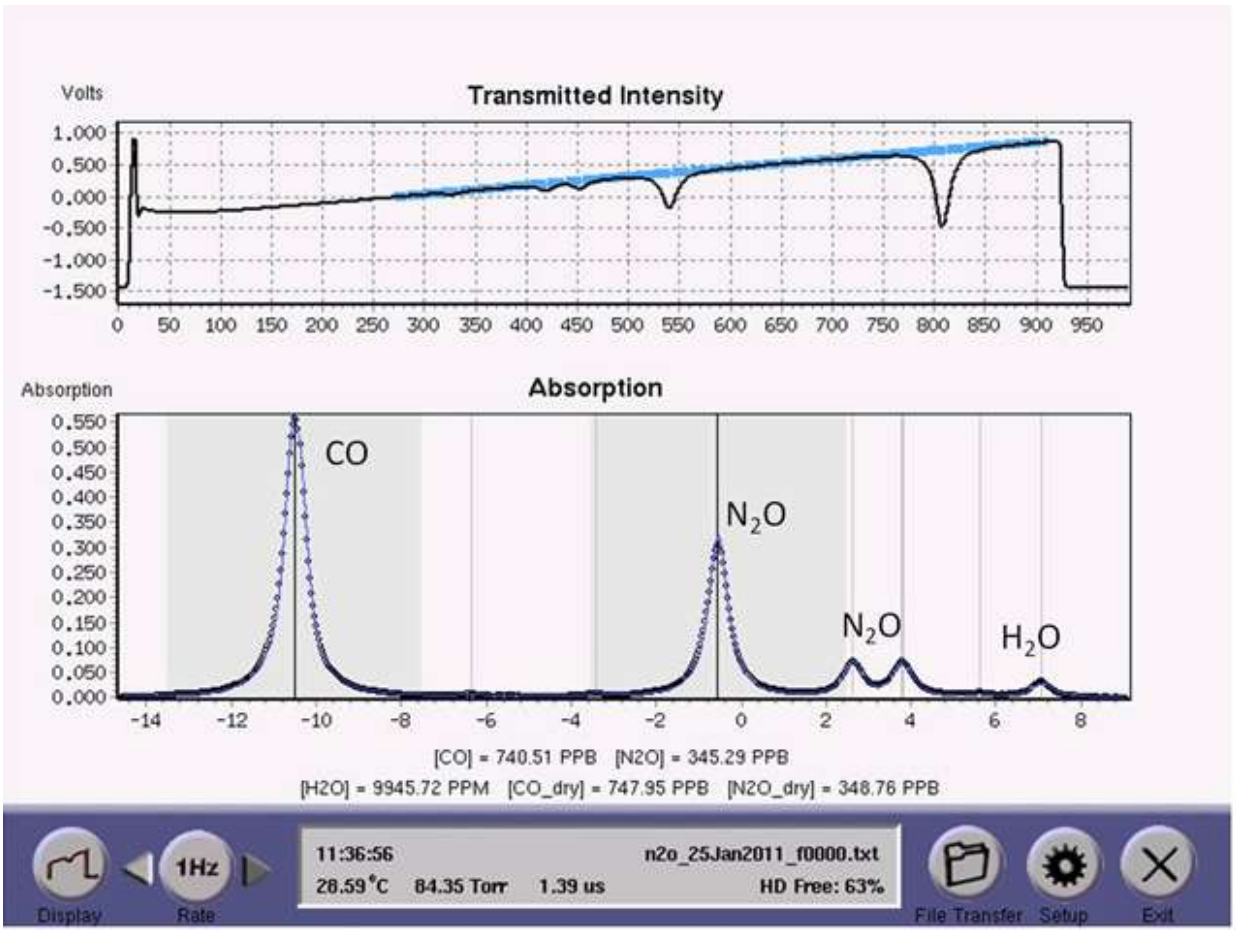
For an ideal back of the envelope calculation we would need N_2O concentration in the soil or in the leaves, which we unfortunately did not collect. We therefore performed a back-of the envelope calculation using the maximum N_2O concentration used in the root chamber experiment described in Pihlatie et al. (2005). We used ET from eddy covariance to estimate water loss through the stomata: for example for 19 August at 16:30 the ET was 99 Wm^{-2} which corresponds to 0.15 mm h^{-1} (using a latent heat of evaporation of 2445587.311 J Kg^{-1} and a temperature of 21°C) to 0.15 l m^{-2} h^{-1}. If the N_2O concentration in the soil was 10400 μgN_2O l^{-1} (or 3309.091 μgN_2O-N l^{-1}) and dissolved in the transpired water, this would result in 482.8 μgN_2O-N h^{-1} per m^2. The N_2O flux we measured from eddy covariance at that same time was 547.4 μgN_2O-N m^{-2} h^{-1}. We therefore assume that this process could represent an important pathway of N_2O loss. A similar estimate comes from the study of Chang et al. (1998): they estimated that for high transpiration rates (10 mm d^{-1} similar to the values observed in our site) the flux of N_2O transpired through crops could be as high as 100 g N_2O-N ha^{-1} d^{-1} which corresponds to 416.7 μgN_2O-N m^{-2} h^{-1} similar to values observed during the peak emission days at our site. We assume this result is connected to the much higher ET of poplar comparing to beech investigated in Pihlatie et al. (2005). We do, however, agree with the reviewer on the lack of strong evidence and we will tune down the discussion.

One thing really missing that cannot be regained is additional chamber measurements for soil and plant fluxes (a) to check the rather novel method of the Los Gatos N_2O Analyzer and (b) to potentially separate soil fluxes from plant mediated fluxes. Even though it is highly likely that the Los Gatos Analyzer is measuring N_2O correctly it is always wise to compare with established methods. Mid-infrared based devices are always prone to "water vapour" errors. That is not only because condensation may happen in the device but also absorption of H_2O and N_2O may occur at very similar wave lengths. Therefore a very broad H_2O peak can overlap a small N_2O peak.

We realize that the lack of chamber measurements during this peak emission is a shortcoming of our study, and it should be better addressed in the revised version of our manuscript. However, we observed this sudden and very important N_2O release, and want to report on it as it considerably affects greenhouse gas flux measurements. But the sudden nature of the N_2O release makes it very challenging (if not impossible) to systematically capture these peak release events. We did performe soil chamber measurements but not during that peak event; so, we were not able to tease apart the release pathways mentioned by the reviewer. We will highlight this limitation in the revised manuscript.

Concerning the instrument:

As far as interference, the cell pressure is ~80 torr, which narrows the spectral width of the absorptions. There is absolutely no spectral interference between the water and the N_2O. As shown in the manual (see figure below), the water peak is not substantially broader than the N_2O peak. They have very similar spectral widths, (see figure below from the Los Gatos manual).



Concerning possible condensation according to Robert Provencal, the senior physicist of Los Gatos Research that regularly monitored the performance of the instrument from august 2010 until a couple of days ago, “the analyzer was still reading ~ 320 ppb for ambient. This means that the optical pathlength in the cell had not changed appreciably. If there had been any condensation in the cell, then the cavity transmission and the reading for N_2O would have decreased dramatically”.

I am always alerted if one needs to correct for dry air (as is done by the Los Gatos device) and use algorithms to produce raw data. You stated you have calibrated, but have you done it with wet air (calibration gas is dry!) also?

We only calibrate with dry air from regular gas tanks. We indeed applied the water vapor density correction term of the WPL correction to our flux data. We also applied a point-to-point conversion from mole fraction to mixing ratio using software (Eco2s) according to Ibrom et al., 2007, and found that the differences between these approaches were negligible. Of course we used data from the Li-7000 for this water vapor correction and we highlighted this limitation in the paper.

Assuming that you get another chance and of course cannot change the things that cannot be redone you should focus on what you can declare which is the importance of high temporal variability of N_2O fluxes. Again reviewer #1 is right it is not new (rainfall event, and water table) but (a) I think we cannot have enough papers on that point and we need spatial repetition in environmental science and (b) I have not seen it in that temporal resolution.

We appreciate that the reviewer valued our dataset and we will improve the discussion to better highlight the strength and the novelties of this study.

Another point is the fact only really named in the title “bio-energy” poplar plantation – could these fluxes “offset” the aimed climate neutrality of such bioenergy attempts? At least it makes CO_2 equivalent flux calculations very difficult (Los Gatos devices cannot be used for too many bio-energy fields to test that) particularly in the light of a changing more extreme climate expecting more dry periods accompanied by heavy single rainfall events.

This is an extremely important issue, and in fact it represents one of the main goals of this project:

http://webh01.ua.ac.be/popfull/

It is indeed very important to assess all greenhouse gases (not only CO_2). Once an estimate of the total greenhouse budget over the year and/or over a rotation cycle will be available, we could certainly answer some of these concerns. Of course a better spatial sampling would be needed to compare the estimates from this plantation to other plantations in different parts of the world, but at least this study would provide a first answer.

So rather use your conclusions for your discussions. Do not formulate hypotheses, as you could not have expected such an event, but state what were the objectives after having the chance of having measuring during such an event.

We agree with the reviewer and will implement this part.