Answer to the comments from Anonymous Referee #1 received and published on 31 March 2011

We thank reviewer #1 for the very helpful comments that will certainly improve our manuscript. We clarified some of the points raised in the answer below and will include them in the revised manuscript. We will also include some more data that will improve the interpretation of the results. We are performing a more in depth analysis and will elaborate the discussion to more accurately represent the data presented.

The manuscript is focusing on a pulse emission event of N_2O from a poplar plantation following an unusual high rainfall event. Flux measurements were done with EC technique using a Los Gatos N_2O analyzer. Though the measurements itself and the data processing is convincing, the paper let one wondering if one can draw any conclusions from the presented dataset. The data show that N_2O fluxes increased largely following an intensive rainfall event (not new), which also led to a significant increase in the water table (not new too).

We appreciate that the reviewer valued our measurements and data processing and we realized that we did not properly highlighted the novelties of this study: the availability of landscape scale high temporal resolution (half hour) N_2O flux data (very difficult to collect) allowed modeling and quantifying the explanatory power of instantaneous environmental variables (including also the wind that cannot be modeled using chamber measurements) on N_2O fluxes. As highlithed by reviewer #2, this temporal resolution for the N_2O eddy flux measurements was not shown before. The reported diurnal pattern in N_2O loss in the first peak emission days in our study was also never reported before from eddy covariance measurements and could give important information on the instantaneous environmental controls on this emission. Previous eddy covariance measurements, for example Pihlatie et al., 2005 and Pihlatie et al., 2010 Biogeosciences, presented daily averaged N_2O fluxes to reduce noise. We assume that the large N_2O loss at our site during these peak emission days was responsible for the low noise of the N_2O flux data. Also, another important result that we did not properly highlight is that a poplar plantation just converted from agriculture could have peak emissions comparable to the extremely high ones of agricultural field right after fertilization.

We will modify the discussion to better highlight these novelties.

Due to correlations with temperature and somewhat with u* and windspeed the authors claim that transpiration fluxes was a major source for N_2O emissions. However, there is no experimental evidence for that, e.g. by additional chamber measurements for soil and plant fluxes. Also a rough calculation if indeed soluble N_2O in transpiration water would provide a meaningful quantity is not presented. Furthermore, existing literature on plant mediated N_2O fluxes is not explored at all. I also wonder that possible correlations to transpiration and CO_2 fluxes is not explored.

The reviewer highlighted very important points that we are planning to address in the revised manuscript. We are performing a more complete statistical analysis also adding CO_2 and H_2O fluxes, which will be included in the revised manuscript. As we did not measure N_2O concentration in the soil, we cannot provide an accurate estimate of the N_2O emitted from leaves. However, we will include a more in depth discussion of this part. A back of the envelope calculation of N_2O loss through the transpiration stream is provided in a following section.

In addition, the finding that N_2O emissions following a second rainfall event did not led to a new peak in N_2O emissions is not astonishing. Possibly one should consider the depletion of N (and C) substrates. Also the possibility that the production layer of N_2O shifted towards deeper soil laers is not discussed or experimentally explored.

These are very important points and we will include them in the discussion.

The provided hypothesis "The main objective of this study was to investigate the impact of soil hydrological changes (e.g. WFPS and water table change) on N_2O emission in a highdensity bioenergy poplar plantation, recently converted from cropland and pasture. We hypothesized that increases in water table and WFPS connected to rain events lead to increases in N_2O emissions. We also hypothesized that increases in soil temperature stimulate N_2O production and thus increase N_2O emissions if adequate water is available in the soil." was obviously formulated following the measurements. Otherwise, the authors would have performed some meaningful measurements of soil and environmental parameters such as changes in soil mineral N concentrations, redox potential, soil chamber and plant chamber measurements for elucidating N_2O emission pathways, microbial activity, soil gas concentrations etc. to strengthen their case.

This is a fair criticism: we should clarify that the primary goal of this project is to calculate a full greenhouse gas balance of a bio-energy plantation. However, after the first year of this project in occurrence of this large rainfall event we became very interested in the effect of this extreme water table change on N_2O fluxes and we started investigating the environmental drivers leading to this emission. We will better clarify this in the revised manuscript.

In conclusion, I liked the dataset, but I found that the discussion is mostly speculative and not confounded by measurements.

We are glad the reviewer values our dataset and we will certainly improve the discussion according to these very helpful comments.

Furthermore, the paper has severe shortcomings with regard to a) a full dataset exploration (not shown: CO_2 and water fluxes!), b) necessary measurements (e.g. changes in soil N concentration, microbial activity), c) exploration of existing literature and d) the interpretation of cited literature (e.g. Boeckx and van Cleemput). I am somewhat doubtful if these short comes can be solved in a revision.

We can certainly address in a revision point a) including CO_2 and water fluxes, point c) better exploring the existing literature and including the important papers we missed (we apologize for overlooking important papers), and d) revising errors (we apologize for the error in the estimates of N_2O emission). As we did not collect N concentration before and during the rain event but only afterwards, we will highlight this shortcoming and focus the discussion on the available dataset. We feel this dataset provides important information and with the addition of the suggested datasets, a more comprehensive data analysis, and less speculative discussion, the revised manuscript could be able to meet the publication standard of Biogeosciences.

Page 2072, line 1 The average N2O emission from arable land in Europe was 5.6 kg N2O-N ha-1 cultivated land per year in the study of Boeckx and Cleemput (2001) and not approx. 15!

We apologize for this mistake. We will correct this in the revised manuscript.

Page 2072, line 5 following Pure speculation not substantiated by any measurements

We will remove this part

Page 2073, line 22 follow. I do not see any evidence that N_2O emission via transpiration was a significant N_2O emission pathway. Just provide a back to the envelope calculation using measured transpiration rates and maximum N_2O solubility in water. Also check existing literature on N_2O emissions via plant transpiration (e.g Pihlatie et al., 2005, New Phytologist). I am doubtful that transpiration N_2O fluxes are indeed significant. The entire discussion here is speculation (wind pumping effect, a more aerobic layer in 20-40 cm). Where is experimental evidence?

We thank the reviewer for pointing out the work of Pihlatie et al. (2005) that we overlooked, and that will be cited and referred to in the revised version of our manuscript. 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 21ºC) 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.

Page 2075 conclusions I do not see any compelling evidence for pressure pumping or increasing gas flow through the soil.

As described in Gu et al., 2005 and reported in Rogie et al., 2001 “atmospheric pressure fluctuations had a strong impact on air exchange near the soil surface. These researchers found correlated, coherent structures in the time series of CO_2 efflux, atmospheric pressure, and wind speed”. We investigated the relationship between nighttime CO_2 efflux, u* and wind speed, and atmospheric pressure and found significant correlation particularly in the period 23-25 August 2010. As Gu et al. 2005 reported, “pressure pumping effect is the primary factor influencing the flux – u* relationship at the high end of the u*”. On the other hand they highlighted that this assumption could be an oversimplification of the reality. We agree with the reviewer on the lack of more experimental evidence and we will implement the discussion for this part.

What is clearly missing is measurements of auxiliary data such as changes in soil mineral N concentrations, redox potential, soil chamber and plant chamber measurements for elucidating N_2O emission pathways, microbial activity, soil gas concentrations ..

We realize that this 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 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 performed chamber measurements but not during that peak event; so, we were not able to tease apart the release pathways mentioned by the reviewer. A similar problem was encountered by Pihlatie et al., 2010, Biogeosciences, that were not able to capture the N_2O emission peaks. On the other hand we should have recorded soil mineral N concentration and soil gas concentration and we will highlight this limitation in the revised manuscript.

Also I cannot understand why relationships between CO_2, H_2O and N_2O fluxes are not explored or data is not shown

We are performing further statistical analysis and will include these data in the revised manuscript.