

Dear editor

Below you can find authors response to reviewer's comments. Authors response to comments are represented in red and underlined:

## Reviewer 1:

### General remarks:

My main concern is that discussion of interannual variability overstretches the information contained in the data. Given the nature of such analysis, the time series is reduced to 3 data points, one for each year (figures 5 and 7). These points form the basis of subsequent statistical analysis. In my opinion the relevance of a regression based on three points is very limited especially in the absence of theoretical target values. If possible, the authors could add more years to the analysis. Although this would improve the credibility of the statistics to some extent, it would not overcome my concern unless 10 or more years of data are available. Therefore, I would like to encourage the authors to explore the intra-annual variability and then try to relate the latter with the interannual variability. In my opinion, the three presented years are suitable for detailed analysis.

Very good comment. Unfortunately our measurements only ran for three years so the possibility to add in more years is not an option. On the other hand we are very aware of the weakness of doing statistical analysis on only three years. Therefore we have chosen to change the title of the manuscript (taking the emphasis away from "annual variability") and putting the focus more on where in the ecosystem the changes took place during the measurement period, as well as highlighting more the seasonal variability than the annual one. Also, major changes were made in the Results and Discussions chapters to meet this comment.

Climatic similarities between the first and the last year allow to determine the magnitude of the effect on NEE due to ageing of the stand. The second year, with an anomalous spring could inform us about the effects of spring frost on GPP, Re and NEE. A possible question to address is: what happens to the photosynthetic and respiration parameters after a frost event (see for example Richardson et al. 2007 in Global Change Biology)? By using a simple photosynthetic and respiration model and fitting this model to different subsets of the data, one could quantify the effects of the frost event. If the focus is shifted towards the effects of the spring events rather than settling on hasty conclusion concerning inter annual variability, I'll be happy to recommend publication.

Also a very good comment – and we thank the reviewer for the support for making such an analysis. However, we had already considered it to be a special topic that should be published later in a separate paper. Further modeling analysis on the frost effect in the Vallanes data is underway, and is due to be published in a separate paper where the main goal will be to analyze and illustrate the spring recovery related to spring frost.

It was, however, not possible to leave it out of the present paper since the effect on annual C-balance was very obvious. But the main goal of the present paper is not to focus on the spring frost event, but to report the three year C-balance of

the afforestation site in Vallanes and where in the ecosystem those changes took place.

### **Specific remarks:**

Page 6607, line 16: concerning the energy balance closure. This could be improved if you would add an energy storage term. An example how to calculate this storage term can be found in:

Lamaud, E., Ogée, J., Brunet, Y., Berbigier, P., 2001. Validation of eddy flux measurements above the understorey of a pine forest. *Agricultural and Forest Meteorology* 106, 187–203.

We are grateful for this comment and added it to the discussion part of the paper and added the reference supplied by the reviewer. A lack of the key measurements (e.g. temperature of biomass and litter), however, hindered us in making the relevant calculations in the present manuscript.

Page 6612, line 27: maybe it is better to note NEE values as negative when you are talking about uptake.

Done

Page 6612, line 14-18: this belongs to discussion and not to results.

Done

Page 6617, line 7: “: : it was noteworthy that annual NEE was lowest: : :” Should be: uptake was lowest.

Done

Table 2: it is not necessary to give so many decimals for the estimated parameters

Done. Number of decimals has been reduced.

Figure 2: Units of ordinate are not correct.

Done

Figure 3: in the caption, CO<sub>2</sub> should be CO<sub>2</sub>

Done

Figure 4: why are there no statistics of these regressions?

The statistical outcome of these analyses is found in Table 2. We further added information on the r<sup>2</sup> and P values for each analysis into Table 2 to meet this comment.

## **Reviewer 2:**

### **General remarks:**

In general the underlying methodology seems sound and the paper is of interest to the readers of *Biogeosciences*. Description of experiments and calculations is complete

and precise and relevant studies are cited. However, the authors could better structure their work and add years 2007 and 2008 in the analysis if the measurements have been running. If the data is not available, the manuscript should be directed towards the seasonality and annual changes because as it is, the number of years is too low to draw any conclusions on the interannual variation.

Like mentioned above in comments to Reviewer I, measurements for the years 2007 and 2008 are not available. Therefore, authors have chosen to change the title of the manuscript and have the focus more on the seasonal variation and less on the annual variation. See earlier comment.

Also the introduction lacks the structure to deliver a clear and straightforward aim for the study and therefore the conclusions are rather vague. Hence, the manuscript would need some improvements before being acceptable for publication.

Done

The introduction is too wide and should be tied up. For example the third paragraph of the introduction is irrelevant for this study. As well some of the referred studies are not necessary to introduce the work. Some of them might be useful to bring up in the Discussion. Please perform some restructuring in this section.

Done

The description of seasonal and annual variation in physical factors (3.1 and 3.2) is otherwise sufficient but information is missing regarding snow. Were there a snow cover and was some of the precipitation in the form of snow?

Done-Information on this has been added in the manuscript

The discussion on the reasons behind the abrupt switch from a net source to a net sink is a little unconvincing. First, it seems to me that the figure 1 shows that the soil has thawed already earlier than the stand becomes a net sink, at least in 1cm depth. In the case of water-logging as the authors suggest (p 6612 l. 14-18), the figure 3 should indicate depression in soil respiration at the date of thaw before the rapid change in NEE. However, the figure indicates that the level of respiration has been rather invariable during the early phase of the year. Nevertheless, the comparison is rather difficult from the separate figures without any additional information on the dates of thaw etc. Please clarify your thoughts or give more details.

Major changes were made in the results and discussion chapters to meet this comment. The authors have moved a part of this section away from the Results chapter, much reducing the speculative nature of the former results paragraph. Furthermore the authors have tried to clarify their thoughts in the relevant sections of the manuscript.

It is not clear, how you have partitioned heterotrophic and autotrophic respiration (p. 6612 l. 25-27). Some of the discussion is mentioned to be speculative but not all. In general, it would be very informative if the authors would present the temporal profiles of GPP and Re as they have presented the daily course of NEE. Also, it would be great benefit if the authors could further fractionate the different components e.g. to heterotrophic and autotrophic respiration. However as I understand, the authors do not have additional chamber measurements from soil, for example, and therefore the partitioning is pretty impossible without additional models and assumptions.

Nevertheless, I think that the data still has value as it is now but the discussion on the annual variation in ecosystem respiration (3.6.), for example, is thin and too simplified and does not discuss true causes and consequences.

The reviewer is correct, the authors don't have the means to divide the respiration flux, which is derived from nighttime flux measurements, into heterotrophic and autotrophic fluxes. We, however, tried to further improve our discussion based on these valuable comments.

Each year was divided into four intervals: winter, spring, : : : These seem rather artificial and at least the figure 4 gives an impression that the seasons are not constant because the Re in the autumn of 2006 has "summer-kind" of response to temperature. I suggest that the different periods would be somehow connected to biological or physical changes in the environment. The figure would need r2 values too.

More information about the statistical analysis in Figure 4 was added to Table 2 (see earlier comment by Ref I). It was a deliberate chose to use fixed dates when seasonal changes between years were studied in figure 4; only by doing that it was possible to detect which seasons/years were much different from others (e.g. autumn of 2006 in Fig. 4). Elsewhere (e.g in fig 2) we defind the biological growing season based on the ecosystem function. We understand and appreciate the comment made the Revier II, but still want to keep the original division between seasons in fig 4.

At the study site, LAI increased from 1.34 to 3.37 from 2004-2006 as well as the irradiation, soil and air temperatures and soil water potential changed from year to year. Also a frost event occurred in 2005. These all have direct and indirect, partly complex effects on CO2 exchange. Therefore I find that the three data points in the figures 5 and 7 do not show any credible results on the responses of GPP and Re. I would suggest that you remove these or justify these more carefully in the text. Moving the emphasis more on the seasonal and instantaneous responses would be appropriate too.

We appreciate the Revier's comment and have changed the emphasis more on the seasonal responses, as suggested (see answers to first comments made by Reviewer I and Reviewer II).

## **Specific remarks:**

p. 6603 l.10: Could you be more detailed with the reference (IPCC, 2007).

Done

p. 6603 l.12: Remove "a" or add the additional reference it in the list (IPCCa, 2007)

Done

p. 6606 l.21-24 Please make the sentences more fluent by removing the repetition e.g. "Eddy covariance measurements at the experimental site Vallanes, started in late 2003 as described by Bjarnadottir et al. (2007a). This paper presents continuous data for three years, 2004–2006. The eddy covariance system was an open path: : :"

Done

p. 6607 l.7-10 Please clarify the sentence ("The flux data: : :.")

Done

p. 6610 l.17-18 The mean annual air temperature was 6.7 and 6.8 for years 2004 and 2006 indicating that 2004 was slightly warmer than 2006?? Please clarify.

Done

p. 6613 l. 4-5 Subscripts are missing (E0). I guess all the used parameters should be italics throughout the whole manuscript including equations.

Done

P. 6613 l 11-20 The paragraph needs fluency. It is also quite speculative.

Done

Table 1 Shortest distance to edge? Does this mean the distance between the ec tower and stand edge? Please clarify

Done

Table 2 Please give r2 values

Done, see earlier answers to Ref I and Ref II.

Fig. 3 Please check the units of the y-axis

Done – we did not detect any mistakes there.

Fig. 4 Time units are missing from the y-axis title. Please add the depth of soil temp Measurements

Done - the correct time unit has been added; the measurement depth of soil temperature was added to the figure legend.

Fig. 6 Time units are missing from the y-axis title. Please give r2 values or confidence intervals.

Done. R<sup>2</sup> values and P values were added as well as the time unit for the y-axis.