

## ***Interactive comment on “Variability of annual CO<sub>2</sub> exchange from Dutch Grasslands” by C. M. J. Jacobs et al.***

**C. M. J. Jacobs et al.**

Received and published: 15 August 2007

Dear referees,

We would like to thank you for giving useful suggestions and advices with regard to the content of our manuscript. We have all your suggestions taken into account and we hope that our responses meet your suggestions and advices.

Best regards,

On behalf of all authors, Adrie Jacobs

Comments referee 1: This is a very nice summary paper of CO<sub>2</sub> exchange in Dutch grasslands. The sites have unique biological/management strategies that make them very interesting to study. For example, I have not come across a grassland where LAI is maintained (via grass cutting) through the growing season! Also, as would be

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

expected in Holland, some grasslands are essentially bogs. The net NEE, GPP, and Re data show interesting patterns based primarily on soil respiration being more variable between sites than GPP. This fact is consistent with some work summarizing CO<sub>2</sub> exchange over several European forests.

Some concerns: Four sites are grazed (and two have no mention if they are grazed) but there is no discussion of the removal of this biomass on CO<sub>2</sub> exchange. This is an important component which is not being discussed and will obviously have a big impact if the ecosystem is considered as a net source or net sink of carbon. Furthermore, grazing is going to have a significant impact both on GPP and Re, maybe more so than temperature. This issue should be addressed in the paper especially since there is discussion on the net CO<sub>2</sub> balance. I understand the fate of the grazed carbon is difficult to consider but this issue should be mentioned as a component of carbon exchange. For example, in the midwest USA, grasslands are occasionally burned and this management strategy is crucial in the overall CO<sub>2</sub> budget.

It is beyond doubt that carbon import and export and management effects need to be considered if one would like to assess the total carbon balance of any area. In that case, one would need to consider methane and nitrous oxide as well. However, it has not been the purpose of our paper to investigate such effects. We deliberately like to concentrate on emission, NEE, because it is the basic starting point of more sophisticated estimates, and because this matches best the data sets that we have available. For example, as explained in the introduction, only one number is available for all grassland areas in The Netherlands irrespective of location, use, management, meteorological conditions etc. We would like to contribute to an assessment of the uncertainty in this practice, by assessing NEE variability in space and time. This variability appears to be almost as large as the variability over the entire European region, even though climatic conditions for our sites are quite similar. However, our introduction may have been a bit misleading in this respect, promising too much regarding the data we have available for the analysis. Therefore, we reshuffled it and rewrote parts

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

of it to be more precise regarding our purposes: evaluate emission variability on a regional scale, with the aim to assess the starting point of the current reporting practice. In order to avoid confusion, we tried to avoid the terms 'sources'; and 'sinks'; as much as possible. We feel our analysis is a first, but crucial step in more refined analyses that will have to follow in future research. Such a follow-up research can then be targeted at a further analysis of the factors mentioned by the reviewers.

There are some other scientific issues I noted; Pg 1510 - may get better correlation with Ta because there is more diurnal variation with Ta compared to Ts. Do you have an idea of what fraction the night Re comes from vegetation and what comes from the soil? The diurnal variation in above ground Re may be greater than the soil Re so Ta has better correlation because of that also. We have found in our work also, Ta correlates better.

We agree that this may indeed be the case. In fact, in response to the comments of Rev. 2 we included a remark on this issue, that fits well into a response to the remark of Reviewer 1. Unfortunately, we have no data (yet) to prove that Ta may better correlated due to the contribution of aboveground Re.

Pg. 1510 No data during precip events - were the CO2 sensors open-path? It would be interesting to see if there was an increase in soil respiration following rains. Do you have any good quality data to suggest an increase?

Some measurements were carried out with open-path sensors, others were based on observations with closed-path sensors. For consistency, we treated all datasets in the same way. The effect of precipitation on respiration is a very interesting subject, indeed. However, our data and method of analysis do not allow such detailed investigation of the effect of precipitation, nor effects of possible freeze-thaw cycles etc. Moreover, we feel such an analysis is beyond the scope of the present paper.

Pg 1512 - may want to consider using a conversion between Rin and PAR (photosyn-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

thetically active radiation). You should be able to easily establish a conversion (generally  $R_{in} \approx 0.5 \cdot PAR$ ) or use one from the literature so that  $R_{in}$  would be comparable to many other light response studies in grasslands. Perhaps you could simply add a second y-axis in Fig 4a as  $R_{in}$  and distinguish it from  $R_{out}$  (as  $R_{in}$ ).

A good suggestion. We included a second y-axis in Figure 4a.

There were also some strictly editorial issues:

Introduction - don't want to end this section on a negative note (i.e., we know  $NO_2$  and  $CH_4$  exchange is important but we did not measure it). Move this paragraph somewhere else in this section but don't leave it at the end.

The phrase the reviewer is referring to was re-written, moved and put in a different context as well.

Pg 1511 - to stick to - this expression is far too colloquial. Use a more formal expression.

The sentence was rewritten as follows: Therefore, it was decided to restrict our analyses to periods of one entire year.

Pg 1514 - Line 18 - these two sites - it was not clear which two sites the authors are referring to.

Site names were added.

Pg 1516 - Line 29 should anti-correlated be negatively correlated?

Okay, changed.

Pg 1519 - Line 4-6 The text needs to be edited to make sense

We now write: The weighed mean for the Dutch grasslands is  $28 \pm 90 \text{ g C m}^{-2} \text{ a}^{-1}$ , indicating that, on average, the Dutch grasslands may behave like a sink for  $CO_2$ .

Pg 1523 - I did not see the Jacobs et al (2003b) reference in the text

It is referenced in the description of the Haarweg site.

Pg 1530 Should Fig 3 be Re on the y axis and not Ro?

Correct; changed in Fig. 3.

Section 3.3 Annual CO<sub>2</sub> Exchange and Fig. 6 and 7 should be Re instead of Rd?

Correct. All occurrences of Rd changed into Re.

Fig 1 Locations 5,6, and 7 need to have arrows pointing to the location - they numbers are on top of each other.

Okay, changed, by zooming into the area.

Comments referee 2: This is a very nice summary paper of CO<sub>2</sub> exchange in Dutch grasslands. The sites have unique biological/management strategies that make them very interesting to study. For example, I have not come across a grassland where LAI is maintained (via grass cutting) through the growing season! Also, as would be expected in Holland, some grasslands are essentially bogs. The net NEE, GPP, and Re data show interesting patterns based primarily on soil respiration being more variable between sites than GPP. This fact is consistent with some work summarizing CO<sub>2</sub> exchange over several European forests.

Some concerns: Four sites are grazed (and two have no mention if they are grazed) but there is no discussion of the removal of this biomass on CO<sub>2</sub> exchange. This is an important component which is not being discussed and will obviously have a big impact if the ecosystem is considered as a net source or net sink of carbon. Furthermore, grazing is going to have a significant impact both on GPP and Re, maybe more so than temperature. This issue should be addressed in the paper especially since there is discussion on the net CO<sub>2</sub> balance. I understand the fate of the grazed carbon is difficult to consider but this issue should be mentioned as a component of carbon

**BGD**

4, S1168–S1176, 2007

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

exchange. For example, in the midwest USA, grasslands are occasionally burned and this management strategy is crucial in the overall CO<sub>2</sub> budget.

It is beyond doubt that carbon import and export and management effects need to be considered if one would like to assess the total carbon balance of any area. In that case, one would need to consider methane and nitrous oxide as well. However, it has not been the purpose of our paper to investigate such effects. We deliberately like to concentrate on emission, NEE, because it is the basic starting point of more sophisticated estimates, and because this matches best the data sets that we have available. For example, as explained in the introduction, only one number is available for all grassland areas in The Netherlands irrespective of location, use, management, meteorological conditions etc. We would like to contribute to an assessment of the uncertainty in this practice, by assessing NEE variability in space and time. This variability appears to be almost as large as the variability over the entire European region, even though climatic conditions for our sites are quite similar. However, our introduction may have been a bit misleading in this respect, promising too much regarding the data we have available for the analysis. Therefore, we reshuffled it and rewrote parts of it to be more precise regarding our purposes: evaluate emission variability on a regional scale, with the aim to assess the starting point of the current reporting practice. In order to avoid confusion, we tried to avoid the terms  $\sum \text{sources}$ ; and  $\sum \text{sinks}$ ; as much as possible. We feel our analysis is a first, but crucial step in more refined analyses that will have to follow in future research. Such a follow-up research can then be targeted at a further analysis of the factors mentioned by the reviewers.

There are some other scientific issues I noted; Pg 1510 - may get better correlation with  $T_a$  because there is more diurnal variation with  $T_a$  compared to  $T_s$ . Do you have an idea of what fraction the night  $R_e$  comes from vegetation and what comes from the soil? The diurnal variation in above ground  $R_e$  may be greater than the soil  $R_e$  so  $T_a$  has better correlation because of that also. We have found in our work also,  $T_a$

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

correlates better.

We agree that this may indeed be the case. In fact, in response to the comments of Rev. 2 we included a remark on this issue, that fits well into a response to the remark of Reviewer 1. Unfortunately, we have no data (yet) to prove that Ta may better correlated due to the contribution of aboveground Re.

Pg. 1510 No data during precip events - were the CO2 sensors open-path? It would be interesting to see if there was an increase in soil respiration following rains. Do you have any good quality data to suggest an increase?

Some measurements were carried out with open-path sensors, others were based on observations with closed-path sensors. For consistency, we treated all datasets in the same way. The effect of precipitation on respiration is a very interesting subject, indeed. However, our data and method of analysis do not allow such detailed investigation of the effect of precipitation, nor effects of possible freeze-thaw cycles etc. Moreover, we feel such an analysis is beyond the scope of the present paper.

Pg 1512 - may want to consider using a conversion between Rin and PAR (photosynthetically active radiation). You should be able to easily establish a conversion (generally  $R_{in} \approx 0.5 \cdot PAR$ ) or use one from the literature so that  $R_{in}$  would be comparable to many other light response studies in grasslands. Perhaps you could simply add a second y-axis in Fig 4a as  $R_{in}$  and distinguish it from  $R_{i}$  (as  $R_{i}$ ).

A good suggestion. We included a second y-axis in Figure 4a.

There were also some strictly editorial issues:

Introduction - don't want to end this section on a negative note (i.e., we know NO2 and CH4 exchange is important but we did not measure it). Move this paragraph somewhere else in this section but don't leave it at the end.

The phrase the reviewer is referring to was re-written, moved and put in a different

**BGD**

4, S1168–S1176, 2007

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

context as well.

Pg 1511 - to stick to - this expression is far too colloquial. Use a more formal expression.

The sentence was rewritten as follows: Therefore, it was decided to restrict our analyses to periods of one entire year;

Pg 1514 - Line 18 - these two sites; - it was not clear which two sites the authors are referring to.

Site names were added.

Pg 1516 - Line 29 should anti-correlated be negatively correlated?

Okay, changed.

Pg 1519 - Line 4-6 The text needs to be edited to make sense

We now write: The weighed mean for the Dutch grasslands is  $28 \pm 90 \text{ g C m}^{-2} \text{ a}^{-1}$ , indicating that, on average, the Dutch grasslands may behave like a sink for CO<sub>2</sub>.

Pg 1523 - I did not see the Jacobs et al (2003b) reference in the text

It is referenced in the description of the Haarweg site.

Pg 1530 Should Fig 3 be Re on the y axis and not Ro?

Correct; changed in Fig. 3.

Section 3.3 Annual CO<sub>2</sub> Exchange and Fig. 6 and 7 should be Re instead of Rd?

Correct. All occurrences of Rd changed into Re.

Fig 1 Locations 5,6, and 7 need to have arrows pointing to the location - they numbers are on top of each other.

Okay, changed, by zooming into the area.

**BGD**

4, S1168–S1176, 2007

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper