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Biogeosciences Discussions

Interactive comment on "The seasonal cycle of the greenhouse gas balance of a continental tundra site in the Indigirka lowlands, NE Siberia" *by* M. K. van der Molen et al.

M. K. van der Molen et al.

Received and published: 3 October 2007

General

All three referees comment that the dicussion paper would benefit from shortening. In response, we have taken out several pieces from the text, mainly from the discussion, and reformulated certain parts. A complete list of removed and shortened items is given at the end of this document (page 14).

The referees also comment that measurements were collected only during part of the year, and that making up annual balances is thus not feasible. Whereas we fully agree that winter flux measurements are not made and may be siginificant, we would also like to point to section 5.3, where we discuss the possible role of winter fluxes at the

site. There we conclude that winter fluxes may be considerable, but probably not large enough to significantly change our results. In response to the comments we have changed the title to *The growing season greenhouse gas balance of a continental tundra site in the Indigirka lowlands, NE Siberia*.

The referees are positive about the contribution of the paper to greenhouse gas research in arctic tundra.

We thank the referees for their time and useful comments. Below detailed responses are given to each of them. The referee comments are numbered and in italics, the author responses start with '»'.

Sincerely,

Michiel van der Molen, Ko van Huissteden, Frans-Jan Parmentier, Roxana Petrescu, Han Dolman, Trofim Maximov, Alexander Kononov, Serge Karsenaev, Dimitri Suzdalov

Anonymous Referee 1

Received and published: 12 September 2007

1. This article extends the knowledge of two important greenhouse gases in a critical region of the world. Inclusion of methane is rapidly becoming recognized for its importance to the issue of climate change, further demonstrated by these data.
» no comment

2. The estimations of flux should be kept to summer or seasonal as no data were collected during the winter period, through any means. Therefore it is impossible to estimate with any accuracy the annual flux values.
» We agree, and the title of the paper will be changed accordingly. See comment 2a, referee 3.

3. I am confused as to why the data collection frequency would be altered, even if data storage space was an issue. Numerous articles cite the importance of at a minimum,

10 hz data collection. Were the cospectra analyzed for 5 vs. 10 hz? I suspect significant data loss during periods of 5 hz data collection.
» We had the choice to collect data at high frequency during part of the periods between field visits or at lower frequency but without interruptions. We choose the latter, because we attached much value to covering temporal variability, i.e. determining the timing of the start of the growing season. In 2007, however, we installed a larger memory card, allowing 10Hz data collection at all times. Nevertheless, it is important to verify what the impact of the data collection frequency was on the resulting carbon dioxide fluxes. Therefore, we performed an experiment, where we used data collected at 10Hz. First we calculated the half-hourly fluxes just as normal. Second, we removed every second data point, thus numerically simulating data collection at 5 Hz, and calculated the half-hourly fluxes again. When plotting the 5Hz fluxes versus the 10Hz fluxes, the majority of the data points is close to the 1:1 line. A linear least-squares regression resulted in a relationship of f5Hz = (0.952 $\pm 0.006) \times f10Hz - (0.1 \pm 0.0), Sy = 1.1\mu$mol/m2/s, N = 1328. Thus we conclude that a reduction of sampling frequency does not significantly change the resulting fluxes.

4. 2003 is mentioned only briefly in the text, and in Figures 12 and 13. What were the general conditions? From the two figures, it seems to have the most consistent data with a clear seasonal pattern.
» The text the referee refers to is in section 2.1.1, which is now: 'On average, the summer months June, July, August receive about 260, 250 and 150 h of sun. The years 2004 and 2005 received considerably less and more than average (90 and 280 h, respectively).' We add: '2003 and 2006 had sunshine hours close to the long-term mean.'

Further towards the end, the river levels are described. We add (in italics) '*With respect to 2003, in 2004,* the river stage was relatively high after high snowmelt runoff. In 2005, the river stage was approximately 1.5-2m lower, as a result of a dry winter and spring; moreover, the air temperatures were as high as 30 °C during most of the field

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campaign. The river stage in 2006 was intermediate.' Because long-term river level data are not available, we cannot be more qualitative at this point.

5. This paper is long and has a lot in it. It may be useful to save some of it (ORCHIDEE part?) for a separate manuscript. The site description is way too long, as is the discussion. Many of the tables/figures are unnecessary and should be cut down.
» We agree that the paper is a bit lengthy. However, as explained below (referee 3) the site description can be shortened, but a slightly more extensive description is justified. The ORCHIDEE part is an integral part of the paper. We will try to condense the paper wherever possible.

6. In pages 2333-4, average hours of sun per month are given, then it is stated that the average in 2004 and 2005 are considerable different. What are the averages for 2004/2005 (90/280)? Annually? Monthly? How to interpret these is confusing.
» The average (1998 to 2006) number of sunhours in June, July and August is 275, 248 and 136 hours. In 2004, the number of sunhours in June, July and August was in total 384 hours less (128 hours per month) and in 2005 it was in total 186 hour more (62 hours per month). The average numbers have changed slightly because they accidentally did not yet include the 2006 data.

7. Page 2345 line 27 use IS LARGER not IS MORE LARGE.
» ok

8. It appears (Fig 9d) that the model cuts off GPP at 9 umol?
» No, not really. This becomes clear by looking at Fig. 9a. At high radiation levels, the model approaches the maximum photosynthesis rates at the prevailing temperature and vpd. There is some variability in the modeled GPP, but it is clear that radiation limits photosynthesis. The observed GPP shows a very similar behaviour, but with more variability. This may be explained by the variability in plant species, soil and hydrological conditions around the tower as well as measurement uncertainty. In Fig. 9d this shows as if the modeled GPP is cut off.

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9. Table 1 BASIC CLIMATOLOGY OF AS OBSERVED should read BASIC CLIMATOLOGY OF THE STUDY SITE AS OBSERVED.
» ok, thank you

10. Table 2 line 2 water table should be 5-20, not May-20? Table can be simplified.
» yes indeed. We removed the 'Floodplain/Terrace column' and indicated the difference by an extra line. Otherwise, we could not simplify the table without losing relevant data.

11. Table 4 can be included in text.
» ok.

12. In Fig. 1, remove panel C and detail panel B.
» we replaced this figure with a circum polar elevation map.

Anonymous Referee 2

Received and published: 30 August 2007

1. Paper is extensive and compiles plenty of descriptive information on the studied field site: soils, geology, climate, vegetation, etc. Another advantage is application of powerful measuring technique for gas fluxes: micromet tower, soil chambers and leaf cuvette.
» Thank you. No further comment

2. Unfortunately time series for gas fluxes were very short: during three years (2004-2006) chambers and towers were erected and run no longer than several days in Jul-Aug. It is understandable from logistic point of view: too risky to keep expensive instruments in the Arctic wilderness. But I would try to cover the warm and transient (spring, fall) periods at least with chambers. By the way, chambers were too small: 10 cm diam will not cover even medium size individual vascular plants (sedges, bushes) and metal frame should severely damage roots and soil resulting in abnormal flux. My personal experience is that the newly installed chambers always display 'atypical' and irregular behavior first 2-5 days, we prefer to discarded first data point in time series as artifacts,

but this work was entirely build on a single-term measurements! I would not take them seriously. They are unreliable and mostly meaningless.

» The referee may have misunderstood parts of the Instrumentation section. First the short period of the measurements: Only the chamber flux measurements of CO_2 respiration and CH_4-flux have been made in short periods of a few days of measurement. The tower CO_2 flux measurements operated throughout the summer season in all years, as has been clearly stated in section 2.2.1.

Second, the diameter of the chambers for CO_2 respiration measurement is indeed small, being 10 cm. Our standard equipment is very similar to the ones used in many other CO_2 respiration studies. In addition to the standard equipment, we inserted open metal rings into the soil in the year 2003. The closed chambers can be fit tightly on those rings during the measurements. The rings were left in the field to be used in subsequent years The rings can not hold plants larger than its 10 cm diameter, but it must be noted that the plants at the site are very small indeed. In many of the rings, the original vegetation is still growing. However, there may be a chance that respiration from larger plants is underestimated. From photographs, we estimate that this may have occurred at between 20 and 50 Third, for the CH_4- flux measurements a different size of chamber has been used with diameter of 30 cm. This has not been stated in the text of section 2.2.4, since for the methodology of the CH_4 flux measurements we refer to another paper. We have now added this information to the text in section 2.2.4. The referees' remarks on the short stabilization time between insertion of the chamber are only valid for the CH_4 flux measurements. However, we strongly disagree with his remarks that the results are therefore meaningless, although we understand that our approach needs discussion. Our method of taking single measurements over a large number of sites rather than taking time series on a few pre-installed sites is based on deliberate and well-founded choice. The logistics of equipment transport to our remote site did not allow to bring a large number of chambers or chamber frames. In such a case one can choose either to install two or three sites at a fixed location and return with only a few unrepresentative flux measurements, or try a different approach that at

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least results in a representative overview of the spatial variation, as we did. We agree with the referee that the insertion of a chamber rather shortly before measurement could cause unstable measurements from ebullition of methane from oversaturated soils. However, in the first place this ebullition can be detected by careful inspection of the individual concentration measurements on which the flux measurement is based, as outlined by van Huissteden et al. (2005). Measurements that show anomalous starting concentrations or rates of rise were discarded. Second, we tested several times repeated measurements at one or two day intervals at the same sites, for the sites most vulnerable to ebullition. In al cases these showed reproducable results (Van Huissteden, 2005). Third, even with pre-installed frames and boardwalks installed ebullition cannot be excluded at these sites. We added more explanation on this topic in section 2.2.4.

3. It does not mean that first sketchy data should not be published. Yes, publish and discuss them in appropriate careful way! The main my disappointment was caused by dreadful disparity between the modest amount of available observational data (several days per year during three year) and grand scale of global/regional extrapolation. Such extrapolation as well as brave attempts to simulate mathematically and find mechanistic interpretation for each obtained number is wasting of time!

» With respect to the remark on our large scale extrapolation, we would first like to refer to the fact that we have more than a few days of measurement for CO_2 respiration, which the referee apparently did not understand (see 1). Moreover, CO_2 respiration is also derived from partitioning the eddy covariance data. So, for CO_2 an extrapolation over the whole growing season is not unjustified. To compare our CH_4 results with the whole-season CO_2 results we have indeed extrapolated the CH_4 from the short measurement period to the growing seasing by using a well-established process model. This is not a waste of time, but making optimal use of the data we have, of course, with understanding of all the restrictions that apply to such a procedure. A short discussion of these uncertainties have been added to the Discussion section.

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4. Specific comment about soil biology. Discussion on some biological issues (photosynthesis - root exudation - aerobic and anaerobic microbial activity at above and below zero temperatures) was done incorrectly. For instance, Ryvkina et al, 2000 and Panikov, Sizova, 2006 did not study methanogens. I strongly recommend to read papers (at least summary) before citing.
» Indeed, Rivkina et al. 2000 and Panikov Sizova, 2006 did not study methanogens but soil bacteria from permafrost in general, which should have been mentioned at citation of these papers. The text has been adapted in page 2348, line19 and page 2351, line 12-14.

Anonymous Referee 3

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1. General comments The manuscript extends the poor quantitative knowledge on carbon dioxide and methane fluxes in the Russian Arctic, showing exceptionally high annual NEE. However, given the low seasonal coverage and missing winter/spring measurements, extended long-term observations have to confirm the exceptionally large annual NEE. The preliminary character of the results should be clearly mentioned in the manuscript.
» We are not sure why the referee states that the measured NEE are preliminary. We present four years of eddy covariance data, supported by independent CO_2 respiration data, and a model. The methane observations cover 3 years and many replications, and are interpreted with a model as well. Section 5.6 and Table 7 show that the NEE we observed is in range with other arctic tundra studies. We relate high CO_2 sink to the more continental climate at our site, than at many other sites. Section 5.3 gives an overview of winter fluxes observed in other stations and argues that the impact on the NEE observed at our site may be relatively small. As such, we do not consider our results as preliminary.

2. Specific comments *a. Title should reflect the fact that no winter measurements are*

available.
» We propose to change the title into "The growing season greenhouse gas balance of a tundra site in the Indigirka lowlands, NE Siberia"

b. Generally the text contains too much qualitative information in the introduction/ site description. Shorten the general site description, while referring to the concise description by van Huisteden 2005. Figure 1 is very poor, climate data covered also by Table 1.
» We agree that the site description could be made somewhat shorter - there is some redundant information there. However, this paper is the first one presenting the full data for four years of measurements on the site, more papers are to come since the research on the site will continue for several years. Therefore it is justified to give a more extensive description of the site. The description by Van Huissteden et al (2005) is very concise, and needs to be expanded for a more general paper like this one. Referee 2 (comment 1) considers the amount of information in the site description an advantage.

c. More emphasis on the specific site description, regarding the footprint partitioning of the eddy covariance tower (e.g., species composition, soil moisture) and its representativeness for up-scaling to a larger area (e.g., river versus lake emissions, active layer depth, soil moisture). This is a prerequisite for any quantitative up-scaling study as the site and larger area are reported to be heterogeneous.
» Based on available remote sensing data and terrain visits the site is representative for moist tundra and floodplain environments in the Indigirka lowlands above the tree line. On the Circumpolar Arctic Vegetation Map, the area classifies as G4 (Tussock-sedge, dwarf-shrub, moss tundra) while the Berelekh floodplain classifies as S2 (Low-shrub tundra). However, these classes are broad and not very useful generalizations. Within the site and its wide surroundings large tracts of W2 (Sedge, moss, dwarf-shrub wetland) occur and may even dominate. Quantification of these vegetation types requires at least a remote sensing study, that has been planned but not carried out yet. In the text we will make reference to the units of the Circumpolar Arctic Vegetation Map.

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d. It remains unclear to what area the areal fraction numbers in Table 2 refer (e.g., how big is the absolute area, and absolute area = tower footprint?).
» The areal fractions in table 2 represent fractions within a 150 m radius circle around the eddy correlation tower, based on 8 radial transect counts at 5 m distance between points. The fractions on the floodplain have been derived from two cross transect counts of in total 520 m. This information will be added tot the caption of table 2.

e. Vegetation description would benefit from a general tag (e.g., bioclimatic zone or assign a class of the legend of the Circumpolar Arctic Vegetation Map giving a hint to how representative the measured vegetation type is for the Arctic).
» see comment c.

f. The respiration measurement and parameterization description (sect. 2.2.2 and 3.3) need clarification in terms of no. and year of measurements, soil versus ecosystem respiration, flux variability for a single location throughout 1 experimental cycle (i.e., quality control) and choice of the model used to explain the data (the poor fit does not support it). Remove first sentence of Sect. 3.3 as eddy cov. fluxes are not used in this section. Corresponding Fig 6: Ecosystem or soil respiration (text says ecosystem; figure caption soil respiration)? Does it summarize data of several years? The parameterization of the chamber respiration data remains poor and thus further use is questionable. Include parameters directly in the text rather than separate table. Soil moisture is probably one of the most important factors contributing to the seasonal pattern/ variability between years and should thus be observed additionally to the water table.
» We report about CO_2 respiration measurements collected on 27 July to 1 August 2004 (Fig. 7a) and on 26 to 29 June 2005 (Fig. 7b). In 2004 189 individual measurements passed quality control and in 2005 249. These represent roughly three quarters of the total number of measurements. We aimed to observe ecosystem respiration. A more extensive discussion about this is given in the reply to comment 2 of referee 2. Quality control was carried out by checking the linearity of the increase of CO_2 con-

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centration in the chamber. The first sentence of section 3.3 is correct. We include 'Continuous time series of ...' at the beginning of this sentence to clarify it. This continuous time series, derived from the eddy flux data is later compared with the chamber flux measurements. We add a remark about near the end of this section.

We use a Q10-function to describe the temperature dependency, which is a generally accepted method. We describe the base respiration (Ro) as a function of time of the year. Section 3.3 explains that time of year is an empirical proxy for changes in LAI, active layer depth, biomass and substrate. It may also contain soil moisture information. We collected soil moisture data with a probe, but the data proved unreliable. Therefore soil moisture data related to the CO_2 respiration data are lacking. However, the soils were generally very wet and never drying out. Because the CO_2 respiration is much less dependent on water table or soil moisture than CH_4 respiration, and because time of year proves a relatively good descriptor of the remaining variability, we think we can neglect the influence of soil moisture for now. However, this will be checked during coming field experiments. The referee states that Fig. 6 does not support the model chosen, but we do not agree on this. Indeed, figure 6a shows considerable variation about the mean temperature response, but the color index shows that most of the variation is explained by the 'time of year'. Indeed, Fig. 6b shows quite a good model performance.

g. Not having access to the article by v.d. Molen et al., in review, the extrapolation to annual cumulative values remains questionable, and corresponding results should be regarded as preliminary. Also, the speculations onto longer time spans should be excluded, unless being based on climatological data/ model analysis.
» We removed the reference to that paper, as it may not be that important in this paper. We explain the integration method somewhat more detailed. The longer-time span discussion is not a speculation, but a calculation of the radiative forcing of greenhouse gas concentrations which are changing in time (Frolking et al., 2007).

h. Discussion is somewhat lengthy.

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» We will check the discussion for possible removal of redundant text. However, since the paper contains a synthesis of a large amount of data a proper discussion of our results is justified.

i. Given the vicinity of the methane measurements to the river/floodplain, a careful interpretation of the air/soil temperature/active layer depth versus water table/soil moisture (drying/rewetting) effect would be appropriate when concluding on the prospective arctic situation (e.g., concluding sentence).
» The referee asks for a careful interpretation of the 'air/soil temperature/active layer depth versus water table/soil moisture (drying/rewetting) effect' in view of our concluding remark that precipitation increases in the Arctic may have a stronger effect than increase of temperature. The arguments already have been given in the discussion. The correlation of the measured methane fluxes with soil temperature is poor, and that with water level prominent. The active layer thickness, that also correlates positively with methane fluxes, also tends to correlate with water table rather than soil temperature. Next, the river floodplain - highly susceptible to flooding and high water tables at higher precipitation - is the most productive area of the site. These findings point all in one direction - the higher the precipitation surplus, the higher the water table during the growing season, the stronger the methane fluxes will be. As yet, the effect is difficult to quantify with respect to the effect of soil warming, but it is likely to be substantial. We will adapt our conclusion on the methane fluxes to reflect the quantitative uncertainty but prefer to adhere to the importance of this precipitation effect of the relation methane fluxes - climate change.

j. Generally, the manuscript would profit from considerable shortening.
» see comment h.

3. Technical corrections and comments *a. A more general reference for the evidence of climate change in arctic regions (instead of Chapin, 2005) would be Hinzman et al., Climatic Change (2005) 72: 251-298.*
» we added a reference to this paper.

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b. Introduce abbreviations and variables in the text/figure captions when used the first time (e.g., WNF, TW, TD, fCH_4).
» ok

c. Leaf onset (instead of leave onset)
» ok

d. Fig. 6 - not well readable. What data are averaged for the means and standard deviations plotted?
» we do not quite understand why the referee thinks this figure is not well readable. The means and standard deviations represent those of the individual data points in the bins.

e. Section 3.5, add year(s) of modeling exercise.
» The ORCHIDEE model was applied for 4 years, i.e. for each point in time that meteorological data are available. The PEATLAND-VU model was applied for 3 years, i.e. the years when methane flux observations were collected.

f. Caption Fig. 9 - revisit last sentence.
» we add: 'to exclude data when the vegetation was not completely grown.'

g. Caption Fig 11 - last sentence fluxes instead of fluxed.
» ok

h. Fig 12 - NEE, Re and GPP all eddy covariance based?
» yes, except for those times that one of the components was missing and the ORCHIDEE or CO_2 respiration model could be used to gap fill.

i. Fig 13 not readable, especially lower panel
» We added a third panel for the GHG balance. The zoom function in the pdf reader may be used to take a closer look at the differences.

General comments from all three referees

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All three referees suggest that the paper should benefit from shortening.

» section 2.1.2, last sentence: condensed this sentence.
section 2.2.2 last sentence: we removed 'Because respiration 8230; as opposed to methane fluxes'
section 5.4: we removed large parts dealing about the long-term sensitivity.
Section 5.3 and 5.5: We have merged these sections into a new section called 'Temporal and spatial upscaling'. We removed fig. 15 and its explanation. Particularly the spatial upscaling part has been reduced in size, in order to reduce the length of the paper.

Furthermore, we do not know how to shorten the paper without taking important information away. The abstract is quite concise. The introduction is of medium length, because some space is taken to position our study site amongst other sites. The site description may indeed be a bit lengthy, but we think this is justified, because new, related research is being initiated, so that more publications may be expected to appear in the coming years and the current paper may serve as a basic reference paper. The instrumentation section is not too long,considering that we use many different types of equipment. Section 2 is well structured, and even if much information is given, the readers may decide for themselves which parts need to be read or not. Section 3 is very much to the point and taking away parts would result in loss of necessary information. Section 4 Results is not long, considering that the carbon dioxide, methane and the resulting ghg balance need to be presented together. Section 5 was a bit long, but we have now shortened it considerably.

Interactive comment on Biogeosciences Discuss., 4, 2329, 2007.