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Biogeosciences Discussions

Interactive comment on "The exchange of carbon dioxide between wet arctic tundra and the atmosphere at the Lena River Delta, Northern Siberia" *by* L. Kutzbach et al.

L. Kutzbach et al.

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Cumulative final response to the two referee comments to the manuscript of Kutzbach et al.: bgd-2007-0078

Thank you very much for the constructive critique and the very useful hints to weak points and open questions in our study. In the following, we will answer all comments of the two reviewers cumulatively and propose changes to our manuscript which we think would improve it significantly. First, we will repeat the comment of the referee and then we will give the respective answers.

Anonymous Referee #1

(1.) Units. Throughout the manuscript I believe the unit for carbon flux is in CO2 rather than CO2-C. I guess this may be why the authors are not completing their units. But, while this reviewer appreciate the consistency I still think it is important all units are named properly. There is no such thing as g m-2! Only g CO2 m-2 or g CO2-C m-2 is a unit. Taken out of context (which often happens...) many sentences and figures in this paper are impossible to understand as they stand.

Answer: Since we also very appreciate consistency, we feel in this context to be in a dilemma: Should we try to be consistent with an admittedly large part of carbon flux publications or with the recommendations of the International Bureau of Weights and Measures (BIPM)? We are aware that the units “g C” or “g CO2” are often used in carbon flux studies to avoid misconceptions. However, we think that using this kind of units is against the recommendations of the International Bureau of Weights and Measures (BIPM). As we understand their brochure “The International System of Units 8th edition”, it is not recommended to include information about the quantity in the unit. This information should be given when explaining the quantity, not the unit. Here, we cite the respective chapter 5.3.2 of the brochure on page 132: “5.3.2 Quantity symbols and unit symbols Just as the quantity symbol should not imply any particular choice of unit, the unit symbol should not be used to provide specific information about the quantity, and should never be the sole source of information on the quantity. Units are never qualified by further information about the nature of the quantity; any extra information on the nature of the quantity should be attached to the quantity symbol and not to the unit symbol.” http://www.bipm.org/utils/common/pdf/si_brochure_8_en.pdf http://www.bipm.org/en/si/si_brochure/

Similar recommendations can be found in the Guide for the Use of the International System of Units (SI) published by US National Institute of Standards and Technology: “7.5 Unacceptability of mixing information with units When one gives the value of a quantity, any information concerning the quantity or its conditions of measurement must

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be presented in such a way as not to be associated with the unit. This means that quantities must be defined so that they can be expressed solely in acceptable units (including the unit one - see Sec. 7.10). Example: the Pb content is 5 ng/L but not: 5 ng Pb/L or 5 ng of lead/L" http://www.physics.nist.gov/Pubs/SP811/contents.html

This means that there is indeed such thing as "g m-2", and it is actually the only unit for a mass flow consistent with the SI system. We would prefer to be consistent with the SI system.

We introduced the quantity FCO2 as "carbon dioxide flux" in chapter 2.4: "Modelling of carbon dioxide fluxes". Thus, we think it should be clear that FCO2 is flux of carbon dioxide and not of carbon (CO2-C). Afterwards, we explain in Eq. (3) that FCO2 equals NEE and that NEE is the sum of Reco and Pgross. Thus, also these quantities must indicate fluxes of CO2 because of summation rules. As we see the problem of misconception, we would propose to add explaining sentences to chapter 2.4 like: "The quantities FCO2, NEE, Reco and Pgross indicate throughout this study fluxes of carbon dioxide (and not of carbon) and have the unit g h-1 m-2. This unit would equal in this context the unit g CO2 h-1 m-2 which has been often used in other carbon flux publications." Similar sentences could be added to the captions of the CO2 flux figures Fig.3, Fig. 5-9.

We think that this unit consistency problem is of general interest for the carbon flux community. The scientific journals have to decide which rules they recommend to follow. Concerning this study, we ask the responsible editor T. Laurila to decide which units we should use.

(2.) Page 1955. Line 26 ff. The claim that it may cause "major alterations".. and feedbacks in the global system. Can you really argue this convincingly if you look at possible changes in arctic terrestrial CO2 flux per se versus predicted anthropogenic emissions over the next 100 years? A paper by Zhuang et al (GRL, 2006) put some questionmark to this and I think it ought to be discussed when writing such a standard

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phrase.

Answer: In the text is written "...climatic changes will have a severe impact on the distribution, composition and functionality of plant and animal communities in the tundra (...). This will cause major alterations of the energy, water and carbon balance in the Arctic, which will feed back on the global atmospheric system (...)." Thus, we do not state that there will be "major alterations and feedbacks in the global system", and we don't see that we are in conflict with the paper of Zhuang et al. (2006). Zhuang et al. (2006) projected an increase of the carbon source of northern land ecosystems from currently 276 Tg yr-1 by up to 473 Tg yr-1 by the end of the 21st century. This would be indeed a major alteration of the carbon balance of these northern ecosystems (1.7-fold increase of C source). Nevertheless, to include the important conclusions of Zhuang et al., we propose to change the text as follows: "This will cause major alterations of the energy, water and carbon balance of the Arctic land surfaces, which will feed back on the atmospheric system of the Arctic (Zhuang et al., 2006; Sturm et al, 2001b; Chapin et al., 2005). While the carbon balance changes in northern ecosystems are estimated to have only minor feedback effects on the global climate in the context of the projected anthropogenic carbon emissions (Zhuang et al., 2006), the changes of the water and energy balance by hydrological and vegetation changes in the Arctic are expected to be significant also on the global scale (Bonan et al., 1995; Lafleur and Rouse, 1995; Pielke and Vidale, 1995; Bröcker, 1997; Beringer et al., 2001; Peterson et al., 2002;)."

The following articles have to be added to the references list: Broecker, W. S.: Thermohaline circulation, the Achilles heel of our climate system: Will man-made CO2 upset the current balance?, Science, 278, 1582-1588, 1997. Peterson, B. J., Holmes, R. M., McClelland, J. W., Vörösmarty, C. J., Lammers, R. B., Shiklomanov, A. I., Shiklomanov, I. A., and Rahmstorf, S.: Increasing river discharge to the Arctic Ocean, Science, 298, 2171-2173, 2002.

(3.) Page 1956. Line 21. What bias towards studies from the Canadian Arctic? There aren't too many published from that particular country.

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Answer: You are absolutely right. We propose to remove “the Canadian Arctic” from this sentence.

(4.) Results particularly Fig 7 and 8. I have problems with the visualization of the “2004/2003” season. The huge gap filling exersize is a bit nervewrecking but ok from the perspective that it is convincing that the fluxes are very small during the lacking period of measuring. But I see no need to artificially construct a seasonal pattern as is done in Figure 7 and maybe most importantly in Figure 8. Figure 8 neglects the possibility that there is an impact of particular climatic conditions (for example a very dry early summer or a very late snowmelt) early in the season on dynamics later in the season. It also confuses any comparison to be made with other sites and years where a full proper season has been measured. I see very little but rather confusing value added in Fig 8 relative to Fig 5.

Answer: Placing the two years in this order in Figure 7 and Figure 8 was done to visualise clearer the general seasonal pattern of CO2 fluxes in this Arctic region which we find of interest and would like to discuss. The figures should not neglect the possible impact of different climatic conditions in the two years. However, the data from the two years should be separated more clearly as it was also requested by the second reviewer (#3), and the figure will be changed accordingly. As the inter-annual variations of summer time NEE at Arctic sites were shown to be large (e.g. Groendahl et al., 2007), the combination of data from 2003 and 2004 for a synthetic growing season and the calculation of a seasonal CO2 balance does still deliver a meaningful result from our point of view. We do not want to state that the absolute values observed during our study are universally valid for all years. We think that we should address clearer the inter-annual variability in the Discussion as we did before (see also (7.)).

(5.) Page 1972. Line 22-23. Quote the original source rather than a synthesis paper when citing site-specific data. Here Nordstroem or Soegaard instead of Laurela.

Answer: We cannot find a paragraph about light response modelling and values of maximum seasonal canopy photosynthetic potential Pmax in the paper of Nordstroem et al. (2001) which deals with the 1997 data and is referenced in the Laurila et al. (2001) paper. Thus, we assume that the light response modelling in Laurila et al. (2001) is original work which we used to compare with our data and has to be referenced. In the article of Soegaard and Nordstroem (1999), a thorough light response modelling is performed for 1996 measurements. However, they used a different (more complicated) approach than our study. We followed the same analysis approach (same equations) as Laurila et al. (2001) allowing for a direct comparison. We propose to add the Nordstroem et al. (2001) reference to the sentence as: "(...) was substantially higher with -1.6 g h-1 m-2 (Laurila et al., 2001; Nordstroem et al., 2001)."

(6.) Page 1980. Line 17-18. Respiration continues even at lower temperatures. See recent paper by Panikov et al. in Soil Biology and Biochemistry (2006).

Answer: The text will be changed (and shortened considerably) to adjust it to the Panikov et al. (2006) study as follows: "Microbial respiration continues even in soils cooled down to -39 °C (Flanagan and Bunnell, 1980; Michaelson and Ping, 2003; Panikov et al., 2006)." Furthermore, CO2 which has been produced during autumn and trapped in the frozen ground can probably be released in winter via small cracks in the frozen ground. (Zimov et al., 1993; Oechel et al., 1997a). For instance, Corradi et al. (2005) reported substantial (...)" The following article has to be added to the references list: Panikov, N. S., Flanagan, P. W., Oechel, W. C., Mastepanov, M. A., and Christensen, T.: Microbial activity in soils frozen to below -39 °C, Soil Biol. Biochem., 38, 785-794, 2006.

(7.) Page 1983. Line 7. First line of the conclusions. I don't think this study have shown anything that "differs considerably" from other studies in other regions of the Arctic. Rather it has nicely confirmed a number of characteristics that has been documented through the Alaskan studies and maybe most comparable the work at Zackenberg in NE Greenland (Nordstroem and Soegaard et al). In this latter context a comparison

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may be appropriate also with the recent paper by Groendahl et al. ref below.

Answer: We propose to delete the first two sentences of the Conclusions as they appear to us somewhat exaggerated now, after considering your arguments. Furthermore, we propose to change all sentences of the Conclusions to Past Tense. We think now that the use of Present Tense appears to generalise our findings more than is appropriate. We propose to change the first sentence of the last paragraph of the Conclusions (p. 1985, row 6): Instead of "Under the current arctic climate (...) acts as a CO2 sink with (...)" it should be written: "Under the observed meteorological conditions (...) acted as a CO2 sink with (...)". Thank you for calling our attention to the Groendahl et al. (2007) paper. We will include the very interesting multi-annual results of Groendahl et al. (2007) into Table 1 and will discuss them in the respective parts of the discussion: The following sentence should be added to the Discussion, Chapter 4.3 (p. 1982, row 10): "The cumulative summer CO2 uptake at high-Arctic dry ecosystems was observed to be - though very variable over different years - substantially lower than at the studied polygonal tundra (Lloyd, 2001b, Groendahl et al., 2007)". Then, we would like to add and change some sentences in the discussion to point out clearer the high spatial and inter-annual variability of CO2 fluxes which make generalised conclusions for the tundra somewhat questionable: p. 1982, row 16: We propose to add the sentence: "Considering the high spatial variability of NEE between tundra ecosystems, generalised conclusions on the response of the biome tundra appear questionable (Oechel et al., 1993, 2000). p 1982, row17: The beginning of the paragraph should be changed to: "The function of tundra ecosystems as CO2 sources or sinks was found to fluctuate considerably on the inter-annual and decadal scales in response to fluctuating meteorological conditions and changing climate (Groendahl et al, 2007; Oechel et al., 1993, 2000). The only eddy covariance study on NEE performed for more than two growing seasons in the Arctic demonstrated large inter-annual differences of summer CO2 net ecosystem exchange at a high-Arctic heath tundra ranging over more than one order of magnitude, i.e. from -5 g m-2 to -85 g m-2 (Groendahl et al., 2007). This shows that CO2 flux results from one or two years as given by most of the published

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work including this study can not be generalised easily to be valid on the multi-annual time scale which is most relevant for regional and global carbon budgets as well as climate change projections."

Anonymous Referee #2

1. (p. 1985, ln. 8-12) Last sentence of the Conclusion considers the influence of CH4 on the carbon balance. This should really be presented in Discussion. Actually, it would be great to have here the actual CH4 emission rate instead of the expression "2-3% of carbon released by Reco'. At least it would be more informative to present the emission as percentage of the annual/seasonal NEE estimate.

Answer: We propose to add the following text to the end of the Discussion (page 1983, after row 5: "The carbon balance of the wet polygonal tundra is not complete with the study of CO2, because the polygonal tundra is also a source of methane (CH4). Methane is an important radiatively active trace gas, second only to CO2 in its cumulative effect on the additional anthropogenic greenhouse effect. The global warming potential (GWP) of CH4 for a 100-year time horizon is about 25 times the GWP of CO2 (IPCC, 2007). During the micrometeorological campaigns 2003 and 2004 described in this study, also the ecosystem exchange of CH4 was measured by the eddy covariance method (conditionally accepted manuscript by Wille et al., 20071). The cumulative CH4 emission over the combined measurement periods 2003 and 2004 was calculated by Wille et al. to be 2.4 g m-2 which corresponds to a carbon (C-CH4) release of 1.8 g m-2. The cumulative CO2 ecosystem exchange NEE over the combined measurement periods of -90 g m-2 calculated in this study corresponds to a carbon (C-CO2) uptake of 24.5 g m-2. Consequently, the C-CH4 release was about 7 % of the net C-CO2 uptake over the combined measurement period 2003 and 2004 (end of May to middle of October). The annual cumulative C-CH4 release of 2.25 g m-2 estimated by Wille et al. was 12 % of the annual cumulative C-CO2 uptake of 19.4 g m-2 estimated in this study. The ratio of the CH4 emission to the CO2 uptake was substantially lower at the studied Arctic polygonal tundra compared to other Siberian

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wetlands with higher CH4 emissions (Friborg et al., 2003; Corradi et al., 2005).The greenhouse gas balance expressed as mass flux of CO2 equivalents was calculated considering the 100-year GWP of CH4 of 25 (IPCC, 2007). It was around -60 g m-2 (uptake) for the combined measurement periods (end of May to middle of October) and not significantly different from zero (+ 4 g m-2) for the annual estimates of CO2 and CH4 exchange. Further discussions on the greenhouse gas balance can be found in Wille et al. (2007)1. Then, we propose to change the last sentence of the Conclusions as follows: "Since the carbon released as CH4 was observed to be about 7 % of the carbon uptake as CO2 over the combined measurement period 2003 and 2004 and estimated to be about 12 % of the carbon uptake as CO2 over the whole year (Wille et al., 20071), this will still result in a clear carbon sink function of the investigated wet arctic tundra, both for the vegetation period and the whole year. However, considering the high global warming potential of CH4, the annual greenhouse gas balance of the investigated Siberian tundra can be estimated to be near-neutral."

The following articles has to be added to the reference list: Friborg, T., Soegaard, H., Christensen, T. R., Lloyd, C. R., and Panikov, N. S.: Siberian wetlands: Where a sink is a source, Geophys. Res. Lett., 30(21), CLM 5-1, 2003. IPCC: Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change, edited by Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, M., Averyt, K. B., Tignor, M., and Miller, H. L., Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 996 pp, 2007.

2. (p. 1973, ln. 4-5) Leaf area index (LAI) for the site is presented in Discussion. However, as LAI is such an important parameter describing the ecosystem in question, I would rather see it in the site description. Is the presented LAI total or projected? The authors also present LAI for mosses. Does that mean the moss coverage with a maximum value of 1 or is it something more complicated?

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Answer: The presented estimated leaf area index would equal projected green LAI at the maximum of the leaf development. However, the presented values are only coarse estimations (by eye and thumb) of the fraction of the basal area that were covered by the green leaves and stems of the respective species (projected) as it is commonly used in phyto-sociological research. This estimation was done separately for the forbs and moss layers. Correspondingly, moss leaf area index is the coverage of the green mosses with a maximum of 1, nothing more complicated. We propose to change the words “leaf area index” to “maximum summer leaf coverage”. Also, we will add this information about the “maximum summer leaf coverage” to the site description (Chapter 2.1, page 1959, row 6) like: “(...) Timmia austriaca) dominate. The maximum summer leave coverage of the vascular plants was estimated to be about 0.3, both at the polygon centres and the polygon rims (Kutzbach, 2000). The leave coverage of the mosses was estimated to be about 0.95 at the polygon centres and rims.” The discussion text on p. 1973 shall be changed to “(...)low coverage of vascular plants in the investigated wet polygonal tundra (maximum summer leaf coverage about 0.3). Mosses, which have a high leaf coverage of about 0.95 at the study site, have a much lower photosynthetic capacity than vascular plants. (...)”

3. Presenting the years 2003 and 2004 in reverse order in Figures 7 and 8 is well justified in order to present the whole growing season cycle. However, it would be fair to mark the change of the year somehow in the figure (for example thin/ dashed line). This could also be stated more clearly in the figure legend in order to understand it without reading the whole paper.

Answer: Placing the two years in this order in Figure 7 and Figure 8 was done to visualise more clearly the general seasonal pattern of CO2 fluxes in this Arctic region which we find of interest and would like to discuss. However, the data from the two years should be separated more clearly as you requested, and the figure will be changed accordingly. We do not want to state that the absolute values observed during our study are universally valid for all years. We think that we should address clearer

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the inter-annual variability in the Discussion as we did before (see 7. comment of first reviewer).

4. (p. 1969, ln.11) 'During this time, due to the release of large amounts of latent heat, the soil temperatures remained for a long time at 0degC, whereas Reco degreased steadily. This indicates the importance of the contribution of above-surface biomass to overall ecosystem respiration at the tundra site.' I can't see straight away sufficient ground for this conclusion. 1. Reco is the modeled respiration and on the short term it mostly indicates the changes in surface temperature, not the actual respiration rate. Is this conclusion based on modeled Reco or observed NEE? 2. Does the 'contribution of above-surface biomass' mean decrease in the autotrophic or in the heterotrophic respiration (of the new litter)? 3. Isn't it possible that this decrease is due to gradual decrease in the activity in deeper soil layers - even though the soil temperatures are close to 0 degC.

Answers: 1. This conclusion was based on measured NEE observed during October 2003. During this time the measured NEE equals Reco as Pgross apparently had ceased. During this period, the temperatures in the complete soil profile stagnated around zero due to the large amounts of latent contained in the liquid soil water ("zero curtain effect"). We draw our conclusion due to the clear decrease of Reco while soil temperatures did not change but surface and air temperatures changed. 2. We assume that heterotrophic respiration has no significant contribution to the above ground respiration, because there is not much litter above the ground surface, which would mean on the dense moss layer or between the living moss stems. During senescence, the leaves and stems of the dominant Carex plants just become brown and dry but don't fall down on the ground forming a litter layer. The few deciduous shrubs (e.g. Salix) produce so few leaves that also no continuous litter layer can be observed. 3. Yes, we agree with you. There could also be a gradual decrease of soil respiration during the period of the "zero curtain effect" which could have led to the decrease in Reco. We propose to retract this conclusion at this place as it is anyway not appropriate in

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the results chapter. We propose to change and shorten the text as follows: "The low performance of the exponential fits of Reco and soil temperature data was primarily caused by the different trends of the data series during the refreeze of soils (Figs. 4 and 5). During this time, due to the release of large amounts of latent heat, the soil temperatures remained for a long time at 0 °C ("zero curtain effect"), whereas Reco decreased steadily." Furthermore, we propose to change the text in Chapter 4.2.2 which deals with the discussion of the temperature control on Reco and restrain our interpretation somewhat: p. 1979, row 5: "(...) temperature can be explained (...) instead of "(...) temperature is explained (...)" Then, we propose to add the following sentences at the end of chapter 4.2.2. (p. 1979, row 17): "However, heterotrophic respiration can not be assumed to have a significant contribution to the above ground respiration, because there is not much litter above the ground surface, which would mean on the dense moss layer or between the living moss stems. An additional explanation for the poor fits of Reco to the soil temperatures compared to the surface temperature is that the soil respiration probably declined gradually during the refreeze period in response to the continuously low temperatures while the temperatures itself stayed stable due to the "zero curtain effect".

5. References: In text, the citations to Lloyd (2001) and Sturm et al (2001) are missing the a/b -extension (p. 1963, ln. 12; p. 1980, ln. 20; p. 1983, ln. 2)

Answer: The extensions will be added. On page 1963, the reference will be "Lloyd, 2001a". On page 1980, the reference "Sturm et al., 2001b" will be deleted as the text will be changed in reply to reviewer 1. On page 1983, the reference will be "Sturm et al., 2001b".

6. (p. 1976, ln. 19-22) '...phenological events such as bud break in spring...are controlled by...also the photoperiod...'. Does the photoperiod have a significant influence on bud break or other phenological events in spring/ early summer? Is there a reference for that?

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Answer: To make it more meaningful, we propose to extend the text and add several references as follows: “When projecting future changes of CO2 assimilation due to climate change, it has to be kept in mind that phenological events such as bud break, culm elongation and flower development in spring and summer as well as senescence and bud dormancy initiation in autumn are controlled by a complex suite of environmental and plant-internal variables including not only temperature but also photoperiod or species internal periodicity (Heide, 1990, 1997, 2005; Oberbauer et al., 1998, Keller and Körner, 2003). In the Arctic, however, the timing of snowmelt and soil thaw has to be considered as the key factor in determining the start of the growing season (Pop et al., 2000; Chapin et al., 2005). The photoperiod would only limit significantly the plant development in spring if the snowmelt would be drastically earlier (for example April) because the timing of snowmelt in the Arctic under the actual climate occurs during all-day-light conditions near the summer solstice. On the other hand, the triggering of the end of the growing season is less well understood and probably the result of several interacting factors (Oberbauer et al., 1998; Van Wijk et al., 2003). In the long-term, an extension of the growing season will favour species which are able to adapt to and benefit from a longer growing season enhancing thus vegetation community changes in the Arctic (Chapin et al., 2005).” The following articles have to be added to the reference list: Heide, O. M.: Dual floral induction requirements in Phleum alpinum, Ann. Bot., 66, 687-694, 1990. Heide, O. M.:. Environmental control of flowering in some northern Carex species, Ann. Bot., 79, 319-327, 1997. Heide, O. M.: Ecotypic variation among European arctic and alpine populations of Oxyria digyna, Arct. Antarct. Alp. Res., 37(2), 233-238, 2005. Keller, F. and Körner, C.: The role of photoperiodism in alpine plant development, Arct. Antarct. Alp. Res., 35(3), 361-368, 2003. Oberbauer, S. F., Starr, G., and Pop, E. W.: Effects of extended growing season and soil warming on carbon dioxide and methane exchange of tussock tundra in Alaska, J. Geophys. Res., 103, 29075-29082, 1998. Van Wijk, M. T., Williams, M., Laundre J. A., and Shaver, G. R.: Interannual variability of plant phenology in tussock tundra: modelling interactions of plant productivity, plant phenology, snowmelt and soil thaw, Global Change Biol., 9,

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