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Biogeosciences Discussions

Interactive comment on "Satellites reveal an increase in gross primary production in a greenlandic high arctic fen 1992–2008" *by* T. Tagesson et al.

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Response to anonymous Referee #1

General comments The discussion is confusing and should be reorganized. It is not clear what the point of the authors is: they reported an increase in GPP but then they discussed a decrease in NDVI by a previous study in the same location (Ellebjerg et al., 2008). They did not clearly explain the difference between their study and this previous study.

Response: We have revised the discussion carefully, and we hope that it is clearer and less confusing. Ellebjerg et al. (2008) focused on the years 1999-2006, and NDVI in

their study was averaged over the entire growing season whereas we are only looking at peak season NDVI. They did not see any significant changes in max NDVI 1999-2006. This paragraph in the discussion is completely reorganized.

They should probably also show NDVI (it should be added in Fig.4).

Response: In the first version we omitted NDVI as the GPP model is linearly related to NDVI. However, we agree with the referee and NDVI is added to Fig. 4..

They discuss the importance of water (and probably effect of drought stress on vegetation) but they never present any data on PPT-PET. Their discussion should be bold to the data they present. They should probably include some PPT and PET data to support the importance of water stress on GPP.

Response: The discussion about the water stress is mainly an explanation for the differences in GPP between different years. We have found precipitation data 1997-2005 from a publication by Hansen et al. (2008), and modeled evapotranspiration 1998-2004 from a publication by Hasholt et al. (2008), which are now included in the discussion. These studies support the drying trend between 2000 and 2005.

Most importantly the error of their model is sometimes higher then the reported increase in GPP. An error analysis should be included to the paper to allow estimating the uncertainties of their model. Fig.3 shows a large overestimation of the model compared to the observation (the y-intercept is higher than 300 mg CO2 m-2 h-1).

Response: We changed the evaluation of the model in several ways. Firstly, PAR measured inside the chamber is used instead of PAR measured at the climate station. Secondly, in the evaluation we compared each point where GPP is measured with modeled GPP, instead of estimating an average for each NDVI pixel, which were done in the first submission. This significantly improved the validation of the model (564 g CO2 m-2 h-1 and 553 g CO2 m-2 h-1, for average modeled and measured GPP, respectively). Both an error analysis and an uncertainty analysis are now in-

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cluded in the revised version. The root mean square error of the model was 223 mg CO2 m-2 h-1. The uncertainty analysis was done by adopting a Monte Carlo sampling approach by sampling 2000 sets of model parameters. The parameters were the slope and the intercept of the FAPAR_NDVI linear regression, and the LUE coefficient. 2000 of each parameter were randomly spread in a normal distribution around the average. The 2000 sets of parameters were used in the model together with the NDVI dataset 1992-2008 and together with average incoming PAR (1106 μmol m-2 sec-1, (average from noon at the days of satellite images 1996-2008)). Average values and standard deviation were estimated from the 2000 model runs, where standard deviation gives the model uncertainty. The estimated uncertainty of the model is (average standard deviation; 488 mg CO2 m-2 h-1). We however, still claim that the uncertainty is restricted to the magnitude of the GPP trend and not to the existence of such a trend. A higher intercept (between modeled and measured GPP) means that the model is overestimating low GPP and underestimating high GPP. An improvement of the model would thus increase the trend, since low values estimated for the early years before 2000 would be lowered and the high values after 2000 would increase. Consequently, although there are large uncertainties in the exact averaged modeled GPP we maintain that peak season GPP increased 1992-2008.

The offset of the model should be similar in different years to prove that the model consistently overestimates the observations and could be used to accurately describe a temporal trend in GPP. From Fig. 3 it seems that in 2000 the offset was far larger than in 1998 and 2007 combined. If the slope of the regression is statistically different depending on the year (maybe 2000 compared to 1998 and 2007 combined), the LUE model could not be used to prove an increase in GPP, as the error of the model would be dependent on the year and higher than the increase in GPP shown in Fig.4.

Response: The different years are more similar after the changed model evaluation and the use of PAR inside the chamber (see above). However, both offset and slope are still different for the 1998 data compared to 2007 and 2000, which are more similar. This

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is most likely a result of that average PAR 10:00-14:00 (although the measurements were distributed over this time period) had to be used to all model values. Additionally, several of the measurements were done within the same NDVI pixel. Consequently, this affects the slope and lowers the variance in the modeled data in comparison to the measured data. A low variance gives a smaller slope and a higher intercept for 1998. This reasoning is added to and clarified in the revised manuscript.

Finally, careful editing and grammar review is needed to improve the clarity of the paper. Several sentences are wordy and confusing; their structure should be simplified, especially in the discussion.

Response: All technical corrections given are corrected. The manuscript is carefully edited and hopefully the grammar and the sentence structure are now improved.

Specific comments Page 1104 line 5 Shaver and other researchers in his group studied this relationship for decades. Please include a more accurate literature review. Boelman, N., M. Stieglitz, K. Griffin, and G. Shaver (2005), Inter-annual variability of NDVI in response to longterm warming and fertilization in wet sedge and tussock tundra, Oecologia, 143(4), 588-597. Page 1104 lines 20-21: in which way the climate deviates from the high Arctic, please specify. Page 1105 line 27 and Page 1106 line 1: not clear, clarify, is FAPAR unique because independent of the vegetation types? Or the opposite? Page 1106 lines 15-16: not clear specify the height in less compact soil. Page 1106 line 16: how many sensors?

Response: All comments suggestions are added to the revised manuscript.

Page 1106 lines 24-26: not clear what the authors refer to as peak season, the time range they refer to (25 June to 5 August) is basically the entire growing season in the high Arctic, they should be more specific when they refer to peak season (probably July?)

Response: The time range referred to was 25 July to 5 August; this is clarified in the

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manuscript.

Page 1107 line 15: which measurements? Specify NEE? GPP? Probably line 26 should proceed this line. Page 1107 lines 17-18: not clear, “each individual plot was measured at different times of day (between 10 a.m. and 6 p.m)” does this mean each plot was measured multiple times during the day or just once? Specify. Page 1108 line 17: which disturbance? Specify. Page 1109 lines 13-16: awkward, rewrite

Response: These parts are rewritten and clarified in the new manuscript.

Page 1109 lines 18-19: if 10 cm of snow are still present that date cannot be the snow melt date, how long before the snow is completely melted?

Response: The DOY of 10cm snow depth was used instead of DOY of snow melt as this was the only data that we had access to. We added that DOY of 10cm snow depth was used as a proxy for DOY of snowmelt.

Page 1110 line 19: how much these estimates varied across these years? Probably a standard deviation (as % of the mean) should be added.

Response: This is added to the revised manuscript.

Page 1113 lines 14-15: 928.2– 720.5 mgCO2 m-2 h-1 is more the 20% difference, not really a “slight difference”, but a fairly significant difference. This difference should be discussed and compared with the results of other models used to estimate GPP.

Response: See response above. Other models are discussed as well in the revised manuscript.

Page 1113 lines 18-19: why the NDVI data are not shown? They should be an important addition to Fig.4

Response: NDVI is added to figure 4.

Fig. 4 what is the deep in 2005 due to? The temperature is fairly high? Was it due to

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drought?

Response: The year 2005 was the year with the earliest snowmelt (DOY 158), which reduced water availability during the peak of the growing season. It was also the year with highest average annual temperature, which indicates a high evapotranspiration. We do not have any estimates of evapotranspiration for 2005, but Hasholt et al. (2008) showed an increase in evapotranspiration 1998-2004, due to increased temperature. Additionally, 2005 was the third year in a row with low precipitation (Hansen et al., 2008), indicating a water limitation in the system. The temperature was especially high during wintertime, and there were low snow-cover and several thaw events both during winter and spring. We hypothesize that these events disturbed the vegetation and that it affected GPP during the growing season. The discussion is revised in the new manuscript

Page 1114 lines 9-10: the reported increase is some of times lower than the error of the model, a more accurate discussion should be added and an error analysis should be included before concluding that this increase is significant.

Response: See response above.

Page 1114 lines 13-15: this discussion should be expanded to include the effect of temperature on respiration.

Response: We agree with the reviewer that temperature effects on respiration are important to the ecosystem CO2 flux. However, we decided not to include this in the manuscript as we chose to focus solely on GPP. We do not have any data indicating increased respiration in the area 1992-2008. However, a section is included discussing the effect of increased respiration on potential increases in nutrient supply, which in turn would have positive effects on GPP. We acknowledge the importance of respiration for ecosystem processes and consider it a important aspect of future studies in the area.

Plus I would guess Chapin is not the first to report increase in photosynthesis with

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temperature. Please include previous studies.

Response: This paragraph is completely rewritten and this part is omitted.

Page 1114 line 26: this sentence is not connected to the data shown. There is not data on soil nutrient presented in this paper. The authors should rephrase and expand the discussion starting from the data presented. They should discuss the overall effect that a temperature increase has on different ecosystems functioning and soil respiration, water, etc.

Response: The entire discussion is now rewritten and is more firmly based in the data we present in the results. We, however, refer to other studies showing increases in nutrient supply with a changing climate, and discuss the possibility of increased nutrient availability as one of the actors behind the modeled increased GPP.

Page 1115 lines 9-10: actually Fig. 4 shows a GPP increase until 2000, then a stable GPP, then a decrease in 2005, followed by another increase. These different periods should be discussed.

Response: We fully agree with the reviewer and now discuss the reasons for the differences between these time periods in more detail.

Page 1115 lines 22-23: this statement implies that you should present and compare year-round data to summer data for each year, or al least discuss more in depth when it is necessary to present both.

Response: We still decided to omit the summer temperatures in the revised version of the manuscript. Firstly, we do not have summer temperature data for the years before 1996, which was the reason for omitting it from the first version of the manuscript. However, after 1996 the same trend is seen for the annual average values as for the summer temperatures. The discussion is reorganized and this section is removed.

Page 1116 lines 1-3: describe more in details difference in the calculation of GPP in this paper and in previous studies.

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Response: The methods used for measuring GPP are added for the different studies.

Page 1116 lines 21-26: how much the PAR decreases inside the chamber? The difference between these two PAR estimates should be included in the paper if it is believed to be important in explaining the difference in GPP. For the collar effect it is not clear what the authors mean: did the collars shade the plot? What was the field of view of the PAR sensors?

Response: The model evaluation is now changed so that PAR measured inside the chamber is used as incoming radiation instead of PAR measured at the climate station, see above. The PAR sensors were hemispherical and it could be that the PAR estimate was affected by the chamber. This discussion is clarified and reorganized.

Technical corrections Probably “arctic” should be capitalized (“Arctic”) Check equation 1: the expression should be NPP = " APAR (APAR = absorbed photosynthetically active radiation) Page 1110 line 23: replace “since it is a small are” with “due to its limited spatial extent” Page 1114 line 13: add “in” before 1992-2008. Page 1114 lines 22-24: this sentence is too long, rephrase, or split in two sentences. Page 1115 lines 8-9: the sentence starts with the past and then the present; be consistent. Page 1115 line 12: replace “elevation” with “increase” or”rise” Page 1115 lines 13-15: not clear, rewrite. Page 1116 line 4: this sentence is not correct: or you say NDVI and FAPAR are correlated, or you say that the linear relationship is “significant”.

Response: We have addressed all these technical errors in the revised manuscript.

Page 1116 line 7: 0.6 what? add units

Response: FAPAR is the fraction of PAR absorbed by the vegetation. This is clarified in the revised manuscript.

Page 1117 lines 4-6: awkward, rephrase.

Response: It is taken care of.

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References: Ellebjerg, S. M., Tamstorf, M. P., Illeris, L., Michelsen, A., and Hansen, B. U.: Inter-Annual Variability and Controls of Plant Phenology and Productivity at Zackenberg, in: Advances in Ecological Research, edited by: Meltofte, H., Christensen, T. R., Elberling, B., Forchhammer, M. C., and Rasch, M., Academic Press, 249-273, 2008. Hansen, B. U., Sigsgaard, C., Rasmussen, L., Cappelen, J., Hinkler, J., Mernild, S. H., Petersen, D., Tamstorf, M. P., Rasch, M., and Hasholt, B.: Present-Day Climate at Zackenberg, in: Advances in Ecological Research- High-Arctic Ecosystem Dynamics in a Changing Climate, edited by: Meltofte, H., Christensen, T. R., Elberling, B., Forchhammer, M. C., and Rasch, M., Academic Press, 111-149, 2008. Hasholt, B., Mernild, S. H., and Sigsgaard, C.: Hydrology and Transport of Sediment and Solutes at Zackenberg, Advances in Ecological Research, 40, 197-222, 2008.

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