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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 #2

GENERAL COMMENTS: The main problem is that in my opinion the presented approach and data is not sufficient to derive a robust relationship between the satellite information and the ground-based GPP. The authors do not write very clearly about the experimental design, i.e. the number of closed chamber measurements of NEE, Reco and GPP over the day performed on the 11 measurement days. However, according to the df in Table 2, it appears that every plot was measured only once per day. I doubt that it is feasible to calculate a robust mean by averaging only 11 measurements per-

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formed on different days with probably different meteorological conditions and different phenological states at different hours of the day.

Response: We have gone through the section about the chamber measurements carefully, and it is now reorganized, we hope that the revised version is clearer and less confusing. In total, 55 measurement plots were randomly chosen within the different vegetation types; 15 plots for continuous fen, 10 plots, each for hummocky fen and grassland, and 5 plots, each for Cassiope heath, Dryas heath, Vaccinium heath and Salix snowbed. In these plots CO2 fluxes were measured using the closed chamber technique. Half of the plots were measured once one day, the second half were measured once the next day, and the instrument were used for other measurements the third day, thereafter the cycle was repeated. For each individual measurements net ecosystem exchange (NEE) was measured with a transparent chamber, and ecosystem dark respiration (ER) was thereafter measured on the same plot after covering the chamber with a lightproof hood. Between 51 and 154 measurements (for sample size see table below) were done for each vegetation type, distributed over 11 occasions between 30 June and 4 August 2007. Measurements at the same vegetation type were done at different times of the day (between 10 am and 6 pm), so that variation should not be caused by diurnal differences. Average values were estimated and presented in Table 2, just for giving values of the level of daytime GPP, ER and NEE and for giving different levels for the different vegetation types. The measurements are done at different days with different meteorological conditions. But all vegetation types were measured at during these conditions, and we assume that a realistic mean for the different vegetation types are given in table 2. The differences in daytime carbon fluxes are due to different vegetation types and not differences in meteorological conditions.

Sample size for the different vegetation types. Vegetation type Sample size Continuous fen 154 Hummocky fen 108 Grassland 110 Salix snowbed 51 Cassiope heath 54 Dryas heath 54 Vaccinium Heath 54

As the CO2 exchange at vegetated sites is characterised by a pronounced temporal

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variability, the likewise strongly varying environmental control variables (e.g., irradiance) must be accounted for which is typically done by developing empirical models which explain GPP in dependence of PAR, temperature and possibly other influencing variables and allow the modelling of CO2 exchange fluxes over time by using continuous datasets of control variables. The parameters of the models can be compared between vegetation types or be used to calculate weekly, daily or mean hourly GPP. Such GPP data would be more appropriate to be related to FAPAR or NDVI information for upscaling in space and time than the means of 11 chamber measurements used in the manuscript.

Response: It is true that the measurements are affected by the different meteorological conditions at different hours of the day and phenological states of the vegetation at different parts of the growing season. However, as at least 50 measurements were done per vegetation type, we assumed that the sample size was large enough to give a good average value of the daytime fluxes from the start up until the peak of the growing season. The data was presented to show how the different vegetation types affect fluxes. We have carefully edited the text and hope this is clearer in the revised version. Importantly, these measured average values were never used in the remote sensing part of the manuscript. Light use efficiency is different at different part of the growing season and we can hereby not use measurements taken up from different parts of the growing season. Therefore, we choose to only use the three measurement occasions closest in time to the Landsat image 29 July 2007. APAR is measured at the exact same time as the GPP and inside the same chamber. It is hereby possible to relate these two measurements directly. It does not matter if the measurements are done at different parts of the day or at different meteorological conditions, as the two measurements are conducted at the same time. It could be that LUE is different due to these different conditions, but as APAR and GPP are measured simultaneously, we get an estimate of LUE directly. One of our aims was to create a simple model and we consider it a strength that we can relate simultaneous measurements of GPP and APAR. We have thus chosen not model to GPP, otherwise this large advantage

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would be missed out. Additionally, there are already large uncertainties in the model (cf below) (due to for example the natural variability in LUE). Adding an extra model and relating this value to APAR would introduce even larger uncertainties in the model.

The presented means of GPP (Table 2) represent an average over the diurnal, seasonal, weather-related, and spatial variability, and it is quite unlikely that this average of 11 data point will represent the "true" mean GPP of the plots during the peak growing season.

Response: See response above.

Furthermore, it should be noted that just taking averages from chamber measurements introduces a significant underestimation bias to GPP estimates as PAR is always lower in the chamber compared to ambient, and this difference in PAR has to be accounted for by some model approach.

Response: Average GPP is not used in the relation to APAR. Both reflected and incoming PAR is measured inside the chamber, and it is measured at the same time as the GPP measurements. As both PAR and GPP is measured inside the chamber, they are both affected by the transparency of the chamber. This is now emphasized in the text.

The here discussed shortcomings are probably reflected in the poor validation skill of the model which can be seen in Figure 3. Although LUE-based and modelled GPP was highly correlated, this does not mean that the model skill is good.

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 version of the manuscript. This significantly improved the validation of the model (559 g CO2 m-2 h-1 and 553 g CO2 m-2 h-1, for average modeled and measured GPP, respectively).

The Pearsson correlation coefficient is a not very efficient statistical indicator in this context. When evaluating potential biases, the slope and intercept of a regression line would have been more meaningful.

Response: Regression analysis should only be done to test if one variable is depending on anther variable. In the case of evaluating a model against field measured data, we are not testing if LUE modeled GPP are depending on field measured GPP, but if it is correlated to the field measured GPP. This is the reason for doing a Pearson correlation. They are basically testing the same thing as the R2 value is the same for both methods. However, what a linear regression analysis would add to the result is to show that all the variance seen in the field is not completely covered by the model. This is added both in the results and the discussion sections.

I do not agree with the authors that the model only slightly overestimated GPP. About 30% deviation is a rather significant bias.

Response: True. We have completely redone the evaluation of the model and using PAR measured inside the chamber instead of PAR measured at the climate station considerably improved the performance of the model. See response above.

Also, the model has strongly differing performances for the three validation years. Actually the shown data suggest that there might be different NDVI-GPP relationships (different slopes and intercepts) for 1998 and 2007 compared to 2000 which is not a good sign for model stability. In my view, the manuscript presents valuable information about the greening at this high arctic site but fails to improve the quantitative assessment of the effect of the greening on the GPP.

Response: The different years are more similar after the changed model evaluation and 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.

III.) The closed-chamber approach to determine GPP is not described in sufficient detail. What was the chamber height? What was the collar area? What was the material of the transparent chamber walls? Did the chamber have a vent or not? What was the CO2 concentration measurement interval? Was the chamber headspace air cooled? Was the temperature increase inside the chamber monitored? Without these information, it is not possible to assess the probability of biases introduced by the applied chamber methodology.

Response: These questions are all addressed in the revised version of the manuscript (in the method and/or discussion).

IV.) Combining an empirical linear model connecting small-scale FAPAR with larger scale NDVI with an empirical model connecting FAPAR with GPP clearly demands a thorough uncertainty analysis to be able to evaluate the extrapolation results. However, such an uncertainty analysis was not provided at all.

Response: Both an error analysis and an uncertainty analysis are now done. 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 slope and intercept of the FAPAR_NDVI linear regression, and the LUE coefficient. 2000 of each parameter were taken randomly in a normal distribution around the average. The 2000 sets of parameters were used in the model together with the NDVI dataset 1992-2008 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 large (on average 1992-2008; 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. This reasoning and the error and uncertainty analyses are added to the revised manuscript.

V.) The language of the manuscript is not appropriate for publication at this point. I had to make quite many orthography comments; particularly concerning the correct placing of commas. Also the grammar and style has to be improved. Often, the wording is not precise enough for a scientific text. One example: page 1110, line 8: "using nearest neighbour, : : :" This is colloquial language; in a scientific text the full term should be used: "nearest-neighbour interpolation" or "nearest neighbour algorithm". I suggest that the authors go carefully through the long list of technical comments. Very likely, I have not found all mistakes. Also, the structure and clearness of the whole manuscript has to be improved. In my opinion, the authors should have proof-read this manuscript much more carefully before submitting it to a journal.

Response: We thank the reviewer for this careful editorial work and have gone through the manuscript carefully. All technical corrections given are corrected. Additionally, there have been substantial revisions in the new manuscript, and the structure and clearness are hopefully improved.

SPECIFIC COMMENTS. 1.) page 1102, line 13: Specify. On which spatial scale the GPP was modelled? Monthly GPP, annual GPP, average daytime GPP at the peak of the growing season?

Response: The spatial scale was a 1.4 km2 rectangle surrounding the fen, Rylekaerene. The GPP was modeled for noon the days of the satellite images for the peak of the growing season. It is now specified both in the abstract and in the text.

2.) Page 1102, lines 13-14: Here you should make a statement on the model perfor-

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mance (which was not so great in my opinion).

Response: This is now stated in the abstract. Additionally, the performance of the model has improved considerably, see above.

3.) Page 1102, line 16: "well correlated" is not sufficient when evaluating possible biases.

Response: See above.

4.) Page 1104, lines 9-10: Give already here an indication how LUE was "parameterised"?

Response: We added how LUE was parameterised to the final section of the introduction.

5.) Page 1104, lines 11-12: For the extrapolation approach, the correlation is not essential. It is the identification of the transfer function between NDVI and FAPAR. How this transfer function identification was done can be critical: OLS regression RMA regression? Errors-in-variables models? Both variables are noisy, and OLS regression may not be appropriate.

Response: We performed an ordinary least square regression. We changed the introduction, to clarify that a linear regression was fitted. The exact transfer function of the regression is described more in the method section. Additionally, we have now changed it to a major axis regression. Major axis was used instead of reduced major axis as the same quantity is measured in FAPAR and NDVI (radiation) and both are of the same scale.

6.) Page 1105, line 25: Write more specific. Is rho(NIR) is "reflectance in the near infrared (NIR) band) 7.) Page 1105, line 27 to Page 1106, line 1: This sentence is unclear, please rewrite.

Response: These things are clarified in the revised manuscript

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8.) Page 1106, lines 6-19: This paragraph about FIPAR and FAPAR is confusing for me. I would define FIPAR and FAPAR the other way round compared to the authors. To estimate FAPAR, you only need the incoming and the reflected PAR: FAPAR = (Incoming PAR – reflected PAR) / (incoming PAR). However, this is the equation which is used for the calculation of FIPAR in the manuscript (?!). I think that FIPAR = fraction of ground covered by photosynthetically active vegetation * FAPAR. In the manuscript it is the other way round. The fraction of absorbed radiation must always be higher than the fraction of radiation intercepted by the vegetation. Apparently, the authors have related FIPAR to NDVI and not FAPAR to NDVI as was intended to do. This should have a disturbing effect on the results if the vegetation cover is significantly less than 100%.

Response: It was intended to relate NDVI to the PAR intercepted by vegetation, and not total intercepted PAR. And it certainly has an effect as vegetation cover is less then 100%. According to several studies, FAPAR is defined as the PAR absorbed by the green vegetation and not by the total system (Huemmrich et al., 2010;Lagergren et al., 2005;Schubert et al., 2010;Goward and Huemmrich, 1992). In agreement with these studies we have chosen to define FIPAR as total intercepted PAR and FAPAR as PAR absorbed by green vegetation. We added the definitions and references to the revised version of the manuscript.

9.) Page 1106, lines 18-19: please indicate the threshold values of illumination angles which corresponds to these exclusion times.

Response: Good point, the threshold value of the illumination angle is set to 45o, instead of a certain hour of the day in the revised manuscript.

10.) Page 1107, line 3-4: As both NDVI and FAPAR are noisy variables, the transfer function could be biased. What happens if you take FAPAR as the explanatory xvariable and NDVI as the explained variable y ? The result for the transfer function between NDVI and FAPAR will be different. This potential bias has to be kept in mind for the uncertainty assessment.

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Response: We agree. It was wrong to use the ordinary least square regression, as there was noise in both variables. Both variables are in the same range and a major axis linear regression was therefore done instead. The uncertainty in the FAPAR_NDVI model is now considered in the uncertainty analysis, see above.

11.) Page 1107, lines 16-17: How often one plot was measured on one measurement day? See also GENERAL COMMENTS above.

Response: Each plot was measured once each measurement day, however several measurements were done at the same vegetation types distributed over the day. See above.

12.) Page 1108, lines 17-18: What were the percentiles defining the region where data points were considered to be outliers? Actually, I think that such an outlier exclusion is not appropriate when dealing with highly variable time series! Outliers cannot defined just in relation to the mean of all points!

Response: This sentence is now removed. We used the boxplot outlier function for having a method for removing the outliers. There was never any doubt of the data points being disturbed and we have instead written that disturbed data points were removed.

13.) Page 1108, lines 20-21: Taking the mean of only 3 of the only 11 measurement points in time is even more dangerous than taking the mean of 11 points. This approach is very questionable, please see paragraph II in General Comments

Response: As described above, GPP was never averaged for these three measurements. Light use efficiency in each plot was estimated at these three occasions by dividing GPP by APAR. Then an average of the Light use efficiency was estimated for each vegetation type. This is clarified in the revised manuscript.

14.) Page 1108, lines 20-26: See above: FIPAR and FAPAR were confused!

Response: See above. We have also included the different definition of FIPAR and

FAPAR in the revised text.

15.) Page 1109, line 15: How many days? +/- 7 shall indicate the uncertainty of this time span, right? But how long was the time span itself?

Response: The ±7 days shall not indicate the uncertainty of the time span. The time span is 14 days, 7 before the day of the satellite image and 7 after the day of the satellite image. This is clarified in the revised version of the manuscript.

16.) Page 1110, lines 1-2: What is the correction exactly for?

Response: The aim of correcting satellite information is to get appropriate values of earth surface reflectance. Reflectance measured by the satellites are affected by the atmosphere (e.g. aerosols, haze, clouds, cloud shadows and atmospheric water vapor), solar illumination angle, sensor viewing geometry and slope of terrain (e.g. different atmospheric depths, reflections from adjacent pixels and shadowing effects). This is added to the revised manuscript.

17.) Page 1111, lines 13-15: Pearssons's correlation is not the appropriate measure to evaluate biases. Slope and regression of regression lines would be more interesting.

Reponse: See response above.

18.) Page 1111, lines 18-20: R2 is of course significant. For an uncertainty analysis, one should have a look on the uncertainty of the parameters of the linear functions. It should also be considered that there could be a bias due to inappropriate use of OLS regression when x is noisy.

Response: An uncertainty analysis is included in the revised manuscript, see above.

19.) Page 1112, line 24: "soil temperature in 10 cm depth"

Response: This is taken care of.

20.) Page 1113, line 15: This is no slight overestimation but a significant overestimation

(30%).

Response: The performance is now better, see above.

21.) Page 1114, lines 9-10: It does not become really clear what the GPP values are actually standing for: Average GPP over daytime (10 a.m.-6 p.m.) during the peak of the growing season?

Response: We totally agree, and these values are now removed. It stood for the increase in GPP 1992-2008 for the different vegetation types.

22.) Page 1115, lines 9-17: Why did the earlier study found the opposite trend? Do you have an explanation for this discrepancy? Different sites?

Response: Ellebjerg et al. (2008) focused on the years 1999-2006, and NDVI in there study is 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 in the revised manuscript.

23.) Page 1116, lines 1-3: it is not possible to compare daily GPP values with GPP values restricted to some hours during the daytime. This is no issue which has to be discussed in the discussion. You can only compare your GPP data with other published daytime data.

Response: True, this is corrected.

24.) Page 1116, lines 21-24: This is not only a possible explanation, this is a fact. And this bias has to be accounted for by using empirical models between GPP and PAR.

Response: This is now considered in the evaluation of the model and this significantly improved the outcome of the model, see above.

25.) Page 1117, line 13-15. Pearsson's correlation coefficient is not appropriate to evaluate biases. In my opinion, the model showed in this validation test not a good skill. As also no uncertainty analysis was provided, it remains highly unclear how much

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we could trust the spatial and temporal up-scaling by the presented approach see also Genreal comments II.).

Response: The estimated uncertainty of the model is very large (Average standard deviation for all years is 488 mg CO2 m-2 h-1). We still claim that the increased GPP trend seen is robust, even if we see large uncertainties in the model. There are large uncertainties in the exact averaged modeled GPP value mainly due to large natural variation in field measured data, but the trend is still very clear. We therefore conclude that GPP increased 1992-2008. This reasoning is clarified in the revised manuscript.

TECHNICAL COMMENTS. 26.) page 1102, line 10: hyphenate: "field measured" 27.) page 1102, line 11: "was" instead of "were" 28.) page 1102, line 14: Insert comma before "and". 29.) Page 1102, line 16: Insert comma before "and". 30.) Page 1102, line 16: Insert something like "According to the results of this study,: : :" or "The results of this study suggest: : :.2 31.) Page 1102, line 18: Move "during this period" to the end of the sentence. 32.) Page 1102, line 20: Remove comma before "due". 33.) Page 1102, line 22: Hyphenate: "High-latitude" 34.) Page 1102, line 26: Remove comma before "since" 35.) Page 1102, line 26: Insert "soil": "cool soil conditions" 36.) Page 1103, line 1: Write more specific ": : :the rates of soil organic matter decomposition, : : :" 37.) Page 1103, line 6 : uptake and release of what? Write specific. 38.) Page 1103, line 7: "carbon sink" instead of only "sink"; "of" instead of "for" 39.) Page 1103, line 9: Write more specific: "remote sensing data form satellites" instead of only "satellites". 40.) Page 1103, line 14: Insert comma before "and very"; "however" instead of "and". 41.) Page 1103, line 15: comma before "high" 42.) Page 1103, line 18: More precise: "plant productivity" 43.) Page 1103, line 26: comma before "and" 44.) Page 1104, line 7: sentence structure awkward 45.) Page 1105, line 15: Hyphenate: "Non-vegetated" 46.) Page 1106, line 7: Remove comma before "and". 47.) Page 1106, line 27: Insert comma before "and". 48.) Page 1106, line more precise: "using the spectral information of a Landsat: : :." 49.) Page 1107, line 12: Insert comma after "plots". 50.) Page 1107, line 18: "chamber volume" instead of "chamber" 51.) Page 1107,

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line 19: Insert comma before “and” 52.) Page 1107, line 21: insert comma before “two” 53.) Page 1107, line 24-25: Revise sentence structure. 54.) Page 1108, line 16: “exchange fluxes “ instead of “effluxes” 55.) Page 1109, line 15: Remove comma before “that” 56.) Page 1110, line 8: “nearest neighbour interpolation”, insert “the “ before “average” 57.) Page 1110, line 12: Insert comma before “and” 58.) Page 1110, line 23: Remove comma before “since” 59.) Page 1111, lines 4-7: If 1999 was not used, it is noz needed to write about it. 60.) Page 1111, line21: Remove “at” 61.) Page 112, line 6: “values” instead of “value” 62.) Page 1112, line 14: Remove comma after “Although” 63.) Page 1112, line 15: Insert comma before “it”. 64.) Page 1112, lines 19-22: Awkward sentence, please revise. 65.) Page 1112, line 22: Insert comma before “there” 66.) Page 1112, line 27: Revise sentence structure. 67.) Page 1113, line 5: insert comma before “and” 68.) Page 1113, line 7: insert comma after “variation” 69.) Page 1113, line 21: remove “up” 70.) Page 1114, lines 2-6: This should be written more scientifically. 71.) Page 1114, line 13: Insert comma after “temperatures” 72.) Page 1114, line 14: Hyphenate: “temperature-limited” 73.) Page 1114, line 17: Insert comma before “and” 74.) Page 1114, line 20: Insert comma before “and” 75.) Page 1114, line 23: Hyphenate: “warming-induced” 76.) Page 1114, line 24: “decomposition of soil organic matter” 77.) Page 1114, line 25: Hyphenate: “nutrient-limited”, change to “leading to a scarce plant cover. 78.) Page 1115, line 9: Remove “up” 79.) Page 1117, line 4: Insert comma after “period” 80.) Page 1117, line 5: Hyphenate: “global change-induced” 81.) Page 1117, lines 7-8: Insert comma before “and” 82.) Page 1117, lines 11-13: Hyphenate: “high-arctic” (2x) 83.) Page 1117, lines 16-17: Insert comma before “and”

Response: These technical corrections have all been addressed.

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,

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