

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

### **Anonymous Referee #1**

Received and published: 11 April 2010

Journal: BG Title: Satellites reveal an increase in gross primary production in a Greenlandic high arctic fen area 1992-2008 Author(s): T. Tagesson et al. MS No.: bg-2010-14

### General comments

The paper presents interesting data on an increase GPP in high latitude tundra ecosystem. The contrasting findings of different studies (some showing increase, some decrease in NDVI and productivity in the Arctic) makes it very important in the current discussion on climate change impact on tundra ecosystems. However some major flaws prevent me from recommend it for publication. The discussion is confusing and should be reorganized. It is not clear what the point of the authors is: they reported

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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. They should probably also show NDVI (it should be added in 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. Most importantly the error of their model is sometimes higher than 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 CO<sub>2</sub> m<sup>-2</sup> h<sup>-1</sup>). 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. 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. I would recommend including these changes and I encourage a resubmission of the revised manuscript.

### 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 long-term warming and fertilization in wet sedge and tussock tundra, *Oecologia*, 143(4), 588-597.

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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?

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?)

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

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?

Page 1110 line 19: how much these estimates varied across these years? Probably a standard deviation (as % of the mean) should be added. Page 1113 lines 14-15: 928.2 – 720.5 mgCO<sub>2</sub> m<sup>-2</sup> h<sup>-1</sup> 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.

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

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

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.

Page 1114 lines 13-15: this discussion should be expanded to include the effect of temperature on respiration. Plus I would guess Chapin is not the first to report increase in photosynthesis with temperature. Please include previous studies.

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.

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

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

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

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what the authors mean: did the collars shade the plot? What was the field of view of the PAR sensors?

Technical corrections

Probably "arctic" should be capitalized ("Arctic")

Check equation 1: the expression should be  $NPP = \epsilon APAR$  (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".

Page 1116 line 7: 0.6 what? add units

Page 1117 lines 4-6: awkward, rephrase.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/7/C474/2010/bgd-7-C474-2010-supplement.pdf>

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