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Interactive comment on “Impact of meteorological anomalies in the 2003 summer on gross primary productivity in East Asia” by N. Saigusa et al.

N. Saigusa et al.

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We are grateful to two anonymous referees for their valuable comments and suggestions. The following is the list of the authors' reply to the interactive comments on “Impact of meteorological anomalies in the 2003 summer on gross primary productivity in East Asia” by N. Saigusa et al.

Anonymous Referee #1

[General comments]

This is a clear, well-written manuscript. It describes the use of satellite, flux tower, and modelling data to investigate regional (continental scale) driving forces in GPP. The tables and figures are complete and useful. I have seldom read such a manuscript

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that is essentially error-free. I do not have any major criticisms or comments on the manuscript. I think that it should be published with very minor changes.

[Reply]

Thank you for your positive comments about the manuscript.

[Specific comment]

Figures 5 and 6. The units on the y-axis should be consistent or clarified in the figure caption. For example, precipitation is given as mm (meaning mm/month) whereas GPP is given as g/m²/month. Similarly PPF_D is really mol/m²/month.

[Reply]

The units has been corrected from [mm] to [mm month⁻¹] for precipitation, and from [mol m⁻²] to [mol m⁻² month⁻¹] for PPF_D.

[Specific comment]

References: The authors need to check these thoroughly. For example: Yu et al 2005 in text but 2006 in reference list. Page 9. Cook 2004 should be Cook et al. There may be other minor errors.

[Reply]

We have carefully corrected the mistakes in the reference list and in the text.

Anonymous Referee #2

[General comment]

Overall this is a very interesting paper and subject of study, both in the evaluation of vegetation responses to a summer anomaly (extreme event?), as well as the idea of merging flux tower results with satellite remote sensing. The study, however, appears to fall short on scientific significance, and needs a more rigorous analyses of testing and answering the key questions posed on vegetation responses to the summer 2003

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anomaly. There also seems to be a serious weakness in the lack of adequate time series data for an anomaly-based study, as the datasets for all towers never straddle the 2003 anomaly and most sites consist of only 2 years of data.

With respect to a more rigorous analysis, although the anomaly is defined as 'significant', standardized anomalies are not presented nor evaluated in this study. It would be more useful to present the spatial distribution of anomalous vegetation responses that exceed ± 1 standard deviation from expected (or normal) interannual variations. This is particularly necessary given that the SVM model comparisons with 5 Asia flux tower sites (Fig. 3) resulted in an RMSE of 2.33 gC m⁻² day⁻¹ between the observed and satellite-based GPP, and this uncertainty exceeds by a factor of 2, the anomaly values of this study and presented in Fig. 7.

[Reply]

The description using 'standardized anomalies' is indeed more desirable rather than using 'absolute values of anomalies' if the accuracy of observed and estimated GPP is high. However, we used absolute values of anomalies in this study because of the following two reasons.

(1) The accuracy of GPP estimation is still pretty low compared with that of other meteorological variables as discussed in section 2.4 and Fig. 3. In this case, 'standardized anomalies' tend to include large errors in such areas as high latitudes and arid regions where GPP values are relatively low. Those errors might cause misinterpretations of spatial distribution in GPP anomalies. In order to avoid this problem, we focus only on the absolute values of anomalies in Fig. 7. To show the significance of absolute anomalies, we add the information of 95% confidence limits of 0.58 gC m⁻² day⁻¹ for 2-month average (July-August), which was estimated by the RMSE of 2.33 gC m⁻² day⁻¹.

(2) We believe that showing the absolute amplitude of GPP anomaly in Fig. 7 is more important than showing the 'standardized anomalies' in the viewpoint of showing year-

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to-year change in CO₂ uptake capacity of terrestrial ecosystems Our intention is to compare the importance of each ecosystem in the role of atmospheric CO₂ uptake.

[General comment]

A key objective of this study is to determine the controlling factors of vegetation responses to the anomaly, but the manuscript mostly focuses on PPFD and does not analyze the role of Temperature. Its not clear why the PPFD anomaly is presented but not temperature (as the rain belt caused cooler temperatures within and warmer temperatures to the south). Many of the controlling factors actually covary (light and temperature, light and rainfall), and this should either be acknowledged and discussed or better yet, statistically evaluated.

[Reply]

We add a figure (Fig. 1 attached in this reply) showing the temperature anomaly, and also add discussions on the importance of the role of temperature on GPP anomaly.

[General comment]

The number of study sites is very low, 6 in total, and only 2 are actually within the "rain band" from 30-40 deg latitude, and 1 clearly north and only 1 clearly south of the rain band. The subtropical site is also somewhat artificial in that it is Planted Pine, which is not representative of southeast China (or is it?). Perhaps these limitations should be more seriously considered in the interpretation and discussion of results.

[Reply]

We are aware that the datasets which are open to the public are still quite limited in Asia, and we need more efforts to increase the number of available datasets as much as possible. Those efforts will be continued as a part of AsiaFlux network activities. A preliminary result of the integrated analysis based on AsiaFlux is presented in this manuscript, and more rigorous analyses based on sufficient numbers of sites and years will be reported in future studies. The vegetation of southeast China (Figs. 1 and 7)

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consists of evergreen coniferous forests (mainly artificial) and evergreen broadleaved forests. The authors consider that the vegetation at QYZ was one of typical artificial forests in this region. As the referee mentioned, there is a limitation in this manuscript that the study did not include any evergreen broadleaf forest site. We mention this limitation in the text.

[General comment]

The SVM model and parameters used are basically an Ameriflux-based result (or heavily influenced), causing some issues in using SVM to drive an East Asia vegetation GPP study. In fact, this study could primarily be a satellite study as the role of the 6 flux tower sites has become lost and unknown. Satellite estimates of GPP are available? and the 6 flux tower sites were only partly used to develop the SVM-based GPP model.

[Reply]

The number of AsiaFlux sites was still quite limited, and many of training datasets for the SVM were provided by AmeriFlux. The underestimation of SVM-based GPP (Fig. 3) might relate to the limited number of AsiaFlux datasets. It is definitely important to explain the cause of the systematic difference in GPP between AmeriFlux and AsiaFlux, and to reduce the error by increasing the number of AsiaFlux datasets. However, clarifying the cause of the systematic difference is beyond the purpose of this study. We consider this problem as the next target to be solved in future studies. Even though the problem has not been solved yet, there is a particular importance in this manuscript, such that the present study merged the flux datasets to satellite observation and empirical models to analyze various phenomena in East Asia for the first time.

[Other comments regarding the paper include]

- there are large sections in the Results that belong in the Introduction or Discussion, such as the teleconnections discussion.

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[Reply]

We reduce the discussion on the teleconnections in the Results.

[Other comment]

- in the Methods section, there is some very elaborate processing not provided, for example, "Small gaps (<2–3 h) were linearly interpolated, and larger gaps were filled using empirical functions." This may be acceptable, however, it also would prevent anyone from replicating this study. Also, since the respiration component was handled differently at one site (QYZ), the issue of consistency in processing across sites should be discussed.

[Reply]

We provide the method of data processing more in detail.

[Other comment]

- it is not clear why multiple satellites were used (MODIS, SeaWiFs, GLI) when either of the first two, encompassed the study period and provided the geographic coverage?

[Reply]

Two sensors (MODIS and SeaWiFS) were used in this study, and GLI was used in a previous paper (Frouin and Murakami, 2007). The study period was from January 2001 to December 2006, and the MODIS products were used to estimate PPFd satisfactorily only after July 2002. Therefore we used SeaWiFS from January 2001 to June 2002, and MODIS from July 2002 to December 2006.

[Other comment]

- why was monthly land-surface reflectance derived with an assumed tropospheric aerosol model and an empirical NDVI relationship, when, for example, the MODIS sensor provides a real-time optical thickness product? This may be of concern, given

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the anomalous weather conditions, making it less likely that an assumed aerosol model would be valid.

[Reply]

We have two reasons to use an assumed aerosol model and an empirical NDVI relationship instead of using the MODIS optical thickness products.

(1) Our study period was from January 2001 to December 2006. The Aqua MODIS products were not available from 2001 to early 2002. In order to increase the length of study period and to reduce discrepancy between different algorithms, we used a common method (an aerosol model and an empirical NDVI relationship) for the whole study period instead of using the MODIS products.

(2) The diffuse (diffuse + direct) transmittance, which corresponds to PPFD, is generally not so sensitive to the aerosol types than the direct transmittance (i.e., optical thickness). And the surface reflectance is not so important for the PPFD estimation in cloudy or vegetation (dark) areas. We assumed that a consistent and simple estimation of the seasonal change of the land-surface reflectance was more important than the precise aerosol retrievals.

[Other comment]

- the evaluation and validation of the PPFD satellite results is incomplete. As the satellites would give an instantaneous measure of PPFD, how exactly were satellite and tower measures validated to the monthly average PPFD (how was the flux tower data aggregated?).

[Reply]

The PPFD estimated by satellite was converted to the daily value by using calculated diurnal cycle of the solar zenith angle and the satellite-observed instantaneous atmospheric conditions. The satellite PPFD had been validated by flux tower data for daily-base, 8-day-base, and monthly-base, respectively. We add a figure (Fig. 2 attached in

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this reply) showing the relationship between ground observations and satellite estimations for daily-base. According to the validation, we can show that the PPFd satellite results satisfactorily estimated daily PPFd at least in the mid-latitudes, even though the satellites would give an instantaneous measure of parameters. More validations are necessary in the low- and high-latitudes where there may be other sources of error in the PPFd estimations; such as cloud, mixture of vegetation and snow cover, and timing of precipitation (bias behavior in the morning and in the afternoon, etc.).

Interactive comment on Biogeosciences Discuss., 6, 8883, 2009.

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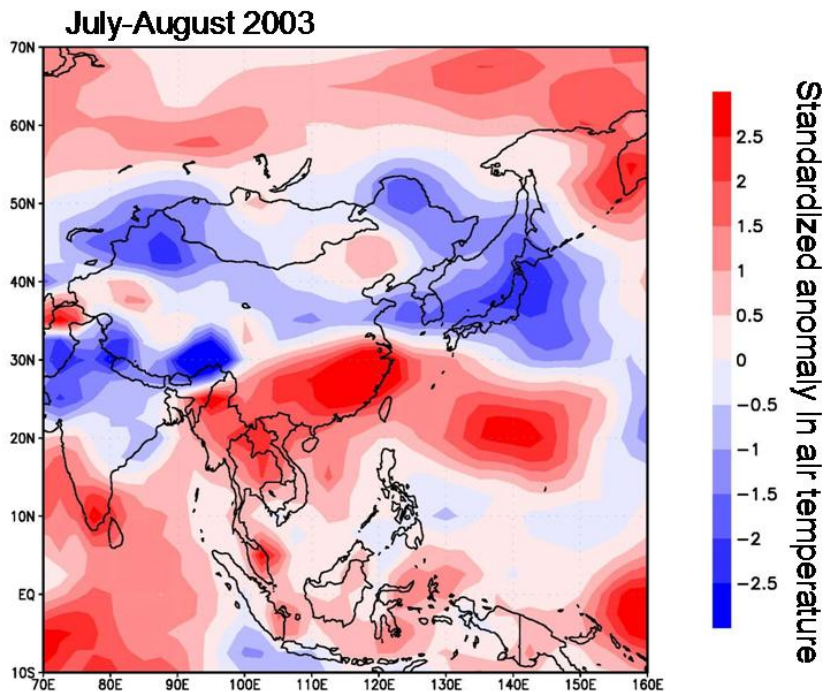


Fig. 1. Spatial distribution of the standardized anomaly in the summer air temperature (2-month average of July-August) in 2003 (base period 2001-2006).

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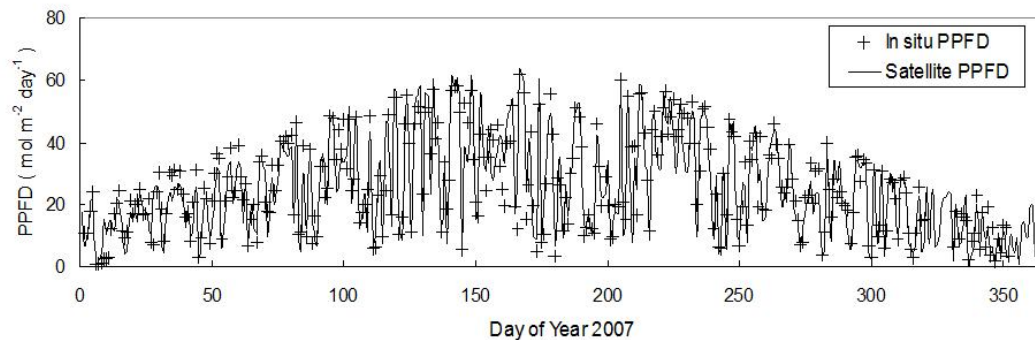
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Fig. 2. The comparison between in situ (tower-based) PPFD and satellite-based PPFD for daily-base in 2007 at TKY.

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