

Interactive comment on “Impact of cloudiness on net ecosystem exchange of carbon dioxide in different types of forest ecosystems in China” by M. Zhang et al.

Anonymous Referee #2

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The followings were some of the comments made previously.

- (1) First of all, MS needs editing for English wording. → The English wording in the revised manuscript has improved but insufficient, requiring more thorough editing.
- (2) Important issue, great, valuable data yet with no clear scientific question. The objective was "to reveal the effect of changes in cloudiness on NEE of two different ecosystems." However, there was no a priori hypothesis to prove/disprove to answer the question. → Unfortunately, this is still the major drawback of this paper and its consequences are manifested in the results, discussion and conclusions of the revised manuscript.

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- (3) Literature review was poorly done. There were many and enough references cited but mostly without insight and summary. → Literature review has been improved.
- (4) The method section should be improved with more practical details instead of just referring other studies (e.g., calculation of diffuse PAR). → Incorporated.
- (5) Figure quality is generally poor. → They are better.
- (6) Trend analyses, regressions and comparisons should be supported by the results of significance test with a pre-determined alpha value. → See the comments.
- (7) The descriptive interpretations of some of the results from figures/tables are either incorrect or misleading. → See the comments.
- (8) Need more description and analysis on the comparison and contrast of canopy structure (LAI profile, leaf angle distribution, etc) between the two ecosystems, in addition to LAI values. → See the comments

Overall comments:

Considering the value of the data presented and the importance of the issues addressed, the paper can be published but only after a substantial revision. At present, it suffers from (1) a poor description in English wording, (2) an equivocal thinking, and (3) inaccurate interpretations of the data. Please, consider the following concerns.

Specific comments:

Abstract.

1st sentence: (1) Clouds affect not only 'carbon uptake' but also 'carbon release'. (2) The term in bold and italics in "... the solar radiation on the ground ..." was used throughout the manuscript. However, this is not an accurate description because the solar radiation on the ground does not affect canopy photosynthesis very much. Perhaps, the correct way to describe is "... the (quantity and quality of) solar radiation intercepted by forest canopy ..." L.10-14: (1) Please, consider rewriting the current

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sentence (in passive form) into active form. (2) "... on cloudy skies ..." → "... under cloudy skies (or conditions) ...". L.20: Delete "rather" (because 'rather than' means 'instead of') L.24-25: Why is the phenomenon important only for temperate forests? A brief explanation would make the ending strong.

Introduction

P.8217, L.1-3: It would be better to cite some original papers. L.6: If the authors want to emphasize the term "on the ground", then consider adding "within-canopy" in front of "temperature". L.15: Need a reference for changes in aerosol content by air pollution. P.8218, L.15, 17, 19: "on the ground" → "by forest canopy" L.23-29: The authors' deduction may be misleading. In my opinion, it was the environmental conditions that were different and not the ecosystems' responses. In other words, the response mechanism was consistent between the two forests but the different environmental conditions resulted in different results. P.8219, L.18-20: The study objective sounds okay. Without employing the modeling approach, however, it would be very difficult to separate the effect of cloudiness from those of other environmental factors that are concurrently changing. What do we expect to learn from the findings such as "different effects under different conditions in different forests at different locations"? L.23: "... to carbon budget ..." → "... on carbon budget ...".

Methods

P.8221, L.14-16: "Based on the clear sky conditions, ... analyzed ... PAR under clear and cloudy sky, ..." Probably the authors meant, "Based on the clearness index, ..." Otherwise, what is the meaning of this sentence? L.15: PAR has been defined already. No need to spell it out again. L.23: "data was ..." → "data were ..." L.24: "... the storage 25 below EC height ..." Delete "25".

P.8222, L.19-22: Please, rewrite this part in a simple sentence with no additional explanation. Otherwise, divide this long sentence into two (and be careful with punctuation, also). L.20, 21: "... closing to ..." → "... close to ..." P.8223, L.1: New paragraph

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usually would not start with "Because." Instead, consider "The rainy season in CBS and DHS was from June to August, and thus days with no clouds were rare during this period." L.8: "showed" → "shows" L.15-17: Changes in quality and quantity of solar radiation with changes in elevation angle is a known fact. Hence, use the 'simple present tense' throughout the sentence.

P.8224, L.11: No need to redefine PAR. L.12-13: This sentence is not needed. L.17: "Statistical ..." → "Statistically ...".

Results

L.20-21: Again, the paragraph does not start with "Since," so please rewrite it. This statement is only partially correct because the authors showed the results from other years in many figures. On the other hand, if the statement is indeed true, then this is an important result. In other words, the consistency and repeatability of the results for four years in two ecosystems should be highlighted. P.8226, L.5-10: The description in these three sentences are very subjective and not convincing. The similarity in the patterns of Ta and P at DHS is as good as that at CBS. By the way, what is the point of saying about the agreement between the patterns of Ta and P? L.20: The sentence should be deleted. L.21: "... PAR of clear ..." → "... PAR under clear ..." L.22-24: Either delete this sentence or move it to "Discussion." L.25-27: (1) Response of NEE to cloudiness can be different depending on the individual response of GPP and RE to cloudiness. Unless the authors examine these components, the analysis with NEE is less meaningful. (2) The authors stressed that the difference was not consistent between the two forests. I am afraid that the opposite is true. Such different responses in fact show the very consistency of the two ecosystems in terms of their responses to environmental changes including cloudiness. L.27: "... than that clear ..." → "... than that under clear ..." P.8227, L.2: No need re-define $P_{ec,max}$. Delete "the light-saturated maximum photosynthetic rate" L.2-4: Earlier, the authors pointed out that only 2005 results were presented because the results from 2003 to 2006 were similar. The results shown here, however, demonstrate relatively large variations from year to

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year. L.5-9: This also demonstrates differences in the results from different years. L.8-9: The authors' statement mentioned here (i.e., clear conditions were more favorable to increase the net carbon uptake at DHS) is not acceptable. Based on the results presented in Table 2, (1) the results from individual years are inconsistent to support the authors' argument and (2) the results for α and $P_{ec,max}$ are inconsistent to support the authors' argument. For example, except for 2003, α ($P_{ec,max}$) decreased (increased) from cloudy to clear conditions in 2004, 2005 and 2006. The reverse was the case in 2003. (In fact, such an inconsistency was also noted at CBS between α and $P_{ec,max}$ and between different years.) L.20: "It meant ..." → "It means ..." L.21-23: Either delete "Although" or complete the sentence. L.23-24: The statement is not correct. In Fig. 5, the NEE-kt relationships were not much different for different β intervals at DHS. L.24-28: Again, the statement is partial and biased. (Furthermore, the description is backward. "NEE increased" actually means NEE became more negative. Either say "the magnitude of NEE" or reverse the description.) The authors pointed that only at high β , the magnitude of NEE decreased at DHS. In Fig. 5, however, such a decrease was observed in all other intervals of β at DHS. L.29: The authors' conclusion is not acceptable. The clearer sky conditions restrained net carbon uptake at DHS, also.

Discussion

P.8228. L.14-15: As already pointed out, the statement is not true and thus should be deleted. Although the sensitivity was relatively lower, the general relationship between NEE and kt at DHS was similar to that at CBS. L.16-20: According to the above-mentioned points, this paragraph regarding the different responses of NEE is no longer needed. Besides, the paragraph is grammatically and logically erroneous, thus equivocal. This paragraph may be divided into two sentences and be rewritten as: "(1) Inconsistent responses of environmental factors to cloudiness resulted in different responses of NEE to cloudiness between the two ecosystems. (2) The control of environmental factors on carbon exchange processes are not different between the two ecosystems." Then, these two statements may be summarized as, "The carbon

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exchange mechanisms in two ecosystems are the same but their environmental factors were different." Is this the point of the authors'? In view of the above points, the subtitle of this section 4.1 needs modification. P.8229, L.6: Delete the comma, ",". L.3-10: This paragraph demonstrates the consistency in the responses of the two ecosystems. L.11: "...linearly with kt ..." → "... linearly with decreasing kt ..." L.12-15: The argument here is based on a weak and ambiguous analysis. The authors should clarify the details of the air temperature measurement used in Fig. 6 such as the measurement height. In Fig. 6 (and 7), explain why a different range of β was used for CBS and DHS for the analysis. Furthermore, a significance test is required for the difference between the r^2 values from the two ecosystems to support the authors' argument. Figure caption should be corrected such as: Fig. 6. Changes of diffuse PAR (PAR_{diff}) at (a) CBS, (b) DHS, and (c) air temperature (T_a) with the clearness index (kt) for selected intervals of solar elevation angles from June to August in 2005. L.18-22: This equivocal paragraph is contradictory to the authors' argument in the previous section. L. 24-27: Please, improve the sentence, which is grammatically wrong and has a typo. P.8230-8231: Most of these paragraphs are reiterations and thus should be reduced and combined with the Results section. L.15: "shade" → "shaded" L.20-21: The authors should highlight this point with more insightful analysis and discussion. For example, the LAI at CBS was higher than at DHS, which is further substantiated by 2.5 times higher biomass (i.e., much higher tree density) at CBS (Table 1). No wonder that more diffuse radiation was more beneficial at CBS due to denser canopy compared to DHS where more diffuse radiation was not appreciated much by its thinner canopy. Provide more detailed descriptions on clumping index, structure of the canopy, understory vegetation, for instance. L.22-26: The relationship between R_e and T_a should be analyzed based on Eq. (12) (not with a linear regression). The comparison should be based on a factor such as Q_{10} . P.8231, L.10-13: Again, this is a self-contradictory statement. L.13-16: It is not convincing and hard to justify.

Conclusions

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L.21-27: These conclusions are conditional depending on the canopy structure and plant density. P.8232, L.4: Delete “certainly”. Also, “could enhance” → “enhanced”
L.5-6: After all these measurements and analyses, this is an inadequate and weak ending.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/6/C3607/2009/bgd-6-C3607-2009-supplement.pdf>

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