**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "Drivers of interannual variability in Net Ecosystem Exchange in a semi-arid savanna ecosystem, South Africa" *by* S. Archibald et al.

G. Wohlfahrt (Editor)

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Editor decision:

The manuscript bgd-2008-0100 (Archibald et al.) investigates the interannual variability of the net ecosystem CO_2 exchange (NEE) and its drivers of a savanna ecosystem in South Africa. To this end five yours of eddy covariance flux, meteorological and remote sensing (fPAR) are used. A long-term time series of drivers is then used to model NEE for 1981-2004. In order to conduct their analysis the authors develop an approach for calculating daytime ecosystem respiration from nighttime flux data, a prerequisite for calculating gross primary production. The manuscript has received

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generally favourable comments by the two reviewers - one recommended minor, the other major revisions. I think the reviewers have provided useful (and somewhat different) suggestions for improving the manuscript, which I too believe will require major modifications before becoming acceptable for publication. The manuscript is though a very valuable contribution and I thus suggest the authors carefully consider all of the comments by the two reviewers, as well as mine, and resubmit a revision of their manuscript in due course.

Editor comments: (1) While I fully understand that a purely temperature-driven algorithm for modelling ecosystem respiration must fail in this system, I have difficulties imagining that modifications of such a model which take into account soil water availability and possibly assimilate supply, would not represent a (at least somewhat) more process-oriented approach than the one chosen by the authors. For example, it has been suggested by Reichstein et al. (2002 ?) for a Mediterranean ecosystem to modify the base respiration rate and sensitivity of respiration to temperature with soil water content – in principle such an approach should be able to produce the optimum-like shapes shown in Fig. B1 and B3. Such an approach would have seemed more logical too me, but maybe the authors can prove that this would not work in this situation.

(2) I agree with one of the reviewers that more information is needed regarding the footprint issue of the two contrasting soil/vegetation types. My questions are: what is the relative contributions of the two soil/vegetation types, is there a pattern with regard to day/night and seasons ? Without this information it is difficult to judge whether part of the variability is not simply due to variability in the flux footprint. Likewise, I would expect post-rainfall flux patterns to differ between the two soil type because of differences in soil physical properties.

(3) Finally, I agree with that same reviewer in that we will need error bars on the annual sums reflecting the systematic (and random) uncertainty of these numbers.

(4) The authors show that fPAR is a better predictor than precip and conclude that precip drives fPAR; is there statistical evidence supporting this conclusion (see also comment by one of the reviewers).

(5) Fig. 3: here I'd appreciate to see the daily sum of NEE (gC/m2d) instead of the average; this number will be more familiar to most readers.

(6) Fig. 4: is the data basis the same for both panels ? if not, use the same data to calculate statistics.

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