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Discussions

Interactive comment on "Water use strategies and ecosystem-atmosphere exchange of CO_2 in two highly seasonal environments" *by* A. Arneth et al.

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The authors compared CO2 assimilation and respiration at few Siberian ecosystems and at a savanna woodland which have distinct active periods due to temperature and moisture limitations. They used micrometeorological eddy-covariance observations of CO2 and water vapour fluxes from which for example surface conductance and marginal water cost of plant carbon gain were calculated. Results showed that these ecosystems were highly adapted to environmental conditions. Growth started shortly after above zero temperatures at the Siberian sites and after first rains at the savanna site southern Africa. During the active season, gas exchange was regulated by stomatal conductance responding to dry and rainy conditions.

The CO2 flux data used in the study have been published earlier (Veenendaal et al., 2004; Röser et al., 2002; Lloyd et al., 2002; Arneth et al., 2002; Tchebakova et al., 2002). This fact raises doubts if the data and analysis are original. The rapid commencement of vegetation activity after cold/dry season have been observed by authors of the original articles and others. However, the comparison of assimilation and respiration processes between the sites is illustrative showing variations between different ecosystems and environmental conditions. Effect of surface conductance on CO2 fluxes have been earlier thoroughly analysed only for the Siberian pine site (Lloyd et al., 2002). In this article, authors centre on mountain birch and semi-arid woodland sites whose surface conductance - CO2 flux interactions have not been analysed earlier. Less attention is given to the northern wetland site because calculation of conductance is confounded by evaporation. As a conclusion, there were sufficiently new analyses and results to merit publication.

Mostly the article is well written. The results and discussion are given in the same chapter that make some statements unclear if they are based on these data or from other sources. For example, on the page 357 there is a discussion on the enhanced microbial respiration processes releasing nutrients to support growth during the early stages of growing season. This is generalised to be a common process in deciduous forests. This is based, however, on rather limited data and probably only micrometeorological measurements (NEE data) are not very convincing because the source of the efflux is not known. In the birch forest, there was most probably ground flora which is active before leaf out modifying the NEE observations. That was the case in mountain birch forest in Scandinavia (Aurela et al., 2001). No information is given on the ground vegetation. Same kind of springtime process producing elevated efflux is probably taking place in soils and litter layer in coniferous forests but micromet systems are not able to resolve it because uptake in the canopy starts already during the thaw. Cuvette measurements would be appropriate to study leaf growth, leaf litter and soil respiration.

On the page 360, the authors write that it is difficult to compare the study sites with other sites referring to Falge et al. (2002) which tabulates results differently. These

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sentences are unnecessary, because if the authors are really willing to compare these sites with others there are publications and data available. For example, Aurela et al. (2001) give daily values.

More symbols and colours in Figs 1 and 2 would enhance clarity.

In Fig. 7, it is unclear which symbols refer to which sites.

Minor point on p. 352, l. 21, ...night-time rates under unstable conditions... usually stratification is stable during the night

References: Aurela et al., 2001, Theoretical and applied climatology, 70, 135-148.

Arneth et al., 2002, Tellus, 54B, 514-530.

Lloyd et al., 2002, Tellus, 54B, 590-610.

Röser et al., 2002, Tellus, 54B, 642-654.

Tchebakova et al., 2002, Tellus, 54B, 537-551.

Veenendaal et al., 2004, Global Change Biology, 10, 318-328.

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